Objective analysis. Constructive dialogue. Innovative ideas.

Economic Returns to Rural Infrastructure Investment

Farm Foundation®

1301 West 22nd Street
Suite 906
Oak Brook, IL 60523
Tel 630.571.9393
Fax 630.571.9580
farmfoundation.org

facebook.com/thefarmfoundation
twitter.com/farmfoundation
AgChallenge2050.org
youtube.com/thefarmfoundation

Economic Impacts, Costs and Benefits of Infrastructure Investment—Review of the Literature

This paper is one of six commissioned as part of the workshop, *Economic Returns to Rural Infrastructure Investment*, organized by Farm Foundation and USDA's Economic Research Service (ERS). The workshop took place April 10–11, 2018, in Washington, D.C. A seventh paper, which had already been published, was also presented at the workshop because of its high relevance to the topic.

Authors John Pender, Ph.D., of USDA's Economic Research Service, and Maximo Torero, Ph.D., of the World Bank, review literature on the literature on the impacts, costs and benefits of infrastructure in the United States and developing countries, focusing on studies published since the early 1990s. The review also helped to identify areas where additional research is needed.

To read the complete paper, or any of the other six papers, visit the Farm Foundation website, https://farmfoundation.org.

Farm Foundation gratefully acknowledges BNSF Railway for its support of the commissioned papers. The findings and opinions expressed in this paper are those of the authors and do not represent those of Farm Foundation, ERS, USDA or BNSF.

Economic Impacts, Costs, and Benefits of Infrastructure Investment – Review of the Literature

John Pender and Maximo Torero*

Abstract

This paper reviews literature on the impacts, costs, and benefits of infrastructure in the United States and developing countries, focusing on studies published since the early 1990s. A review of 28 econometric studies of productivity impacts of public capital in the United States found a wide range of estimates of the output elasticity of public capital (a measure of the percent increase in the value of output associated with a one percent increase in the value of the public capital stock) – ranging from -0.49 to +0.56, with a mean value of 0.12. The range of estimates depends on the unit of analysis, the type of public capital, and the method of analysis. Generally larger productivity impacts were found in national than in state-level studies and for water and sewer capital than for highway capital. Smaller impacts were found in studies that controlled for state-level fixed factors that affect productivity. These estimates imply an even wider range of estimates of the marginal rate of return to public capital stocks, ranging from close to zero for highway stocks to nearly 90 percent for water and sewer capital. Similarly large ranges of rates of return were estimated by studies investigating impacts of public capital on the costs or profits of firms. A few studies estimated the benefits of public capital stocks in U.S. cities including amenity benefits, and found that such benefits can be larger than the productivity benefits. The benefit-to-cost ratio (BCR) of public capital stocks estimated in these studies ranged from about 0.3 to greater than 2.0, depending on the assumptions of the econometric framework. Many econometric studies have investigated impacts of particular types of infrastructure in the U.S. and in developing countries, though few have estimated rates of return implied by the estimates. Model-based estimates of BCRs of infrastructure investments by the U.S. Army Corps of Engineers and the Federal Highway Administration suggest that BCRs greater than 1.0 are common for water and highway infrastructure projects, but no evidence was found in the literature reviewed that these have been validated using econometric approaches. Rigorous econometric impact evaluation methods to assess the causal impacts of infrastructure investments have been used by the Millennium Challenge Corporation and multilateral development banks to validate and improve the results of predictive models in some developing country contexts and have found some statistically significant impacts on railroads, roads, rural electrification, water and information and communication technologies (ICTs). Such an approach could be useful to apply in more contexts.

**U.S. Department of Agriculture (USDA) Economic Research Service (ERS) and World Bank, respectively.*

The findings and conclusions in this preliminary publication have not been formally disseminated by the U. S. Department of Agriculture and should not be construed to represent any Agency determination or policy. This research was supported in part by the intramural research program of the U.S. Department of Agriculture, Economic Research Service. The findings and conclusions of this publication also do not represent the views of the World Bank.

Contents

Summary

In this paper, we review literature on the impacts, costs, and benefits of infrastructure investments in the United States and selected developing countries. A large literature on the productivity impacts of infrastructure investments in the United States and other countries has developed since the seminal work of Aschauer was published in 1989. Much of this literature has focused on estimating the output elasticity of public capital – the percentage increase in GDP or other measures of the value of production resulting from a 1 percent increase in the value of the public capital stock. A review of 28 published studies that estimated this parameter for the United States for different time periods, different levels of analysis, different types of public capital, and using different econometric specifications and methods found a wide range of estimates – ranging from -0.49 to +0.56 – with a mean value of 0.12. This variation in elasticity estimates results in part from variations in the study focus and methods; for example, studies that estimate national level impacts generally find larger output elasticities of public capital than studies that estimate elasticities for states or regions and studies that account for unobserved fixed factors that affect output generally find smaller elasticities.

The literature often finds large differences in the output elasticities for different types of public capital – e.g., total public capital vs. highways or water and sewer capital. Since the marginal annual return to public capital stock from increased productivity is equal to the output elasticity multiplied by the output/capital stock ratio (which can vary greatly across types of public capital), even larger variations are found in the marginal returns to different types of public capital. The mean annual rate of return to highway capital across state-level studies was close to zero, while the mean for water and sewer capital was nearly 90 percent.

The marginal rates of return to public capital stocks are conceptually not the same as internal rates of return or benefit-cost ratios to federal investments in public capital. One dollar in federal investment may be offset by a decrease in public investment by state and local governments and to a lesser extent by private entities, so that the internal rate of return to federal investment may be less than marginal rate of return to an increase in public capital stock. Furthermore, the marginal rates of return implied by most econometric studies do not account for depreciation of infrastructure capital, the opportunity costs of the funds necessary to finance public capital investments, or the timing of costs and benefits, which affect internal rates of return and benefit-cost ratios.

A shortcoming of productivity studies is that they do not account for the amenity benefits that people may receive directly from access to infrastructure and that are not reflected in measures of productivity. A few studies have estimated these benefits using spatial equilibrium theory to assess the benefits reflected in interurban variations in wages and rents or housing values. One prominent study by Haughwout (2002) estimated the benefits of infrastructure in 33 large cities and found that amenities account for most of the value, which was estimated to be in the range of \$1.4 billion to \$2.8 billion (in 1990 dollars), substantially less than the cost of the infrastructure (\$4.6 billion). A recent study by Albouy and Farahani (2017) updated and extended Haughwout's approach, allowing for the effects of non-traded production, Federal taxes, and imperfect mobility of households. Albouy and Farahani (2017) estimated that the benefits-to-cost

ratio (BCR) of infrastructure in the cities studied by Haughwout was in the range of 0.70 to 1.35; more than twice the BCR range found by Haughwout (2002). No studies were found that used this approach to estimate the value of infrastructure investments in rural areas.

A review of estimates of expected benefits and costs of water resources infrastructure investments, which are conducted by the U.S. Army Corps of Engineers (USACE), found a wide range of BCR estimates resulting from project feasibility studies – typically well over 1.0 and often greater than 3.0. Similarly, benefits and costs of potential highway investments are regularly estimated by the Federal Highway Administration (FHWA) and show BCR estimates greater than 1.0 for a wide range of scenarios. Strengths and limitations of the approaches used to generate these estimates are discussed, as is the need for retrospective studies evaluating the benefits and costs of these investments after implementation.

A review of econometric studies of impacts of particular types of infrastructure in the United States – focusing on telecommunications (mainly broadband) infrastructure, water and power systems, and electricity systems – found many studies investigating impacts of broadband or broadband programs and few studies on the impacts of other types of infrastructure. Several broadband studies investigated impacts on labor market outcomes, such as employment, earnings, and wage levels and many find positive impacts of broadband access or adoption on such outcomes. A few studies investigated impacts of broadband access on housing sales values, finding that broadband access can increase house values by up to 7 percent, depending on the available speed.

A review of studies of impacts of particular types of infrastructure in developing countries – focusing on roads, rural electrification, and information and telecommunications technologies (ICTs) – found a large number of studies focused on the impacts of investments in these forms of infrastructure on a wide array of economic and social outcomes. Road investments in developing countries have in many cases been found to have strong effects on productivity in general and on agricultural productivity, transportation costs, commodity prices, nonfarm economic activity, employment, rural household incomes and poverty, household consumption, property values, access to health and education services, and others. Some studies have found that the impacts of road development are greater for poor people. Positive impacts of roads are not universally found, however, and in some studies displacement of economic activities across locations has been observed. Rural electrification and ICT investments are also found to have positive impacts on outcomes reflecting rural people's economic activity, income, and welfare in numerous studies.

Ex ante studies of the costs and benefits of infrastructure investments are often required by donor agencies, but ex post studies are rare. The Millennium Challenge Corporation appears to be an exception in promoting retrospective estimation of costs and benefits of investments based on *ex post* impact evaluations and on impact evaluations conceived since the design of the project. The latter are complex because the nature of infrastructure projects makes it extremely difficult to build appropriate counterfactuals, is costly to implement experimental or quasi-experimental designs and in most cases the impacts could take several years requiring very costly data collection.

Introduction

In February, President Trump proposed investing $200 billion of Federal funds with a goal of stimulating at least $1.5 trillion in infrastructure investment with State, local, Tribal, and private partners.[1] Of the direct Federal funding, $50 billion is proposed to be for rural infrastructure investments. The recommendation of the Interagency Task Force on Agriculture and Rural Prosperity (2018) included a strong emphasis on promoting investments in rural infrastructure – especially in rural broadband e-connectivity – but also in other forms of infrastructure, such as improving water and sewer systems, smart electric grid systems, transportation infrastructure improvements, and others.

Several studies point to a need for increased infrastructure investment in the United States. For example, the American Society of Civil Engineers' (ASCE) 2017 Infrastructure Report Card gave U.S. infrastructure a D+ grade. The ASCE estimated that the U.S. needs to spend some $4.6 trillion by 2025 to improve the state of the country's infrastructure to a state of good repair; representing a funding gap of about $2.1 trillion above anticipated total infrastructure spending over this period.

Despite such estimates and the increased attention being paid to infrastructure issues in policy circles, the economic impacts of and returns to infrastructure investment are not fully clear and are hotly debated in the economics literature. If such a large increase in funding for infrastructure projects is enacted into law, many decisions about how and where to allocate the investments will be required. Evidence on the economic impacts of past infrastructure investments and their benefits and costs can help inform such decisions. Furthermore, a review of the literature can demonstrate approaches to learning about the impacts of and returns to future infrastructure investments.

The main objectives of this paper are to review existing literature on the economic impacts, costs, benefits, and rates of return to infrastructure investments; draw lessons relevant to current and forthcoming policy debates and decisions about where to invest in infrastructure; and identify gaps and opportunities for future research relevant to infrastructure policies and investments. We have reviewed the U.S. and developing country literature related to these issues,[2] though placed greater emphasis on findings potentially relevant to decision makers responsible for infrastructure investments in rural areas of the United States or on demonstration of methods that could be used for assessment of infrastructure investments in the rural United States. Our review emphasizes mainly published empirical findings since the late 1980s, when the seminal works of Aschauer (1989) and Munnell (1990a; 1990b) were published, but we incorporate methodological insights from much older literature and the latest work on rigorous ex post impact evaluations using experimental (RCTs) and quasi-experimental methods

[1] https://www.whitehouse.gov/briefings-statements/building-stronger-america-president-donald-j-trumps-american-infrastructure-initiative
[2] Time and space constraints prevented us from including a review of literature in other developed countries besides the United States.

(difference-in-difference, IV methods, and regression discontinuity methods) with appropriate baselines and control groups as well.

Economic Impacts, Benefits, and Costs of Infrastructure in the United States

Input-Output (IO) Model-Based Literature

A common modeling approach used to analyze the regional or national impacts of many types of interventions in the United States – especially smaller interventions that are not expected to affect prices in the economy – is an input-output (IO) model. IO models estimate the *multiplier impacts* of an intervention (usually on employment, output, and/or income), accounting for the inter-industry demands generated by the initial increase in demand. These models start with the *direct effects* of an intervention on a set of industries, then add *indirect effects* generated by the demand for outputs from other industries as a result of the additional production required to meet the direct demands. For example, a project to install broadband infrastructure could generate demand for firms that lay fiber optic cable, which would result in additional employment in those firms and income of the workers and owners of such firms (direct effects). Those firms would generate demand for other firms and industries that produce goods and services necessary for their work, such as the firms that produce fiber optic cable, construction equipment, etc. (indirect effects). In addition, these models can account for the increase in employment, output, or income resulting from the increased income circulating in the economy due to the direct and indirect effects (*induced effects*), if the IO model is embedded in a social accounting matrix (SAM) that accounts for the flows of income between households, governments, and other consumers in the economy and the industries represented in the IO model.

IO and SAM models for the United States as a whole or subregions of the United States are generally based on the input-output model for the U.S. economy generated by the Bureau of Economic Analysis (BEA). These models have the advantages of being readily available, fairly easy to implement, able to account for the multiplier effects of infrastructure investments as well as direct effects, and yield generally consistent predictions of impacts for similar situations. They also provide predictions of impacts *ex ante*, which can be very useful in analyzing impacts of possible future policies and investments. The main drawback of these models for measuring impacts of infrastructure is that they don't measure the full set of impacts or the benefits resulting from the use of infrastructure – e.g., impacts on the costs and productivity of businesses, economic growth, or the value of infrastructure to consumers' welfare. They also are subject to several restrictive assumptions, which may not be valid in real situations; especially when large investments are involved that may be affected by supply constraints and/or lead to changes in input or output prices.[3]

[3] The assumptions of IO and SAM models include assumptions that supplies of factors of production (land, labor, capital) do not constrain production and are available in unlimited supply at a fixed price, that factors of production and inputs purchased from other industries are used in fixed proportions in each subsector, and that production is constant returns to scale. These assumptions imply that output, income, and employment are determined by demand conditions, and are more likely to be valid approximations in a recession when there is excess labor and capital, for

The IO model literature yields fairly consistent predictions about the employment impacts of infrastructure investment, with national total employment impacts generally in the range of 14,000 to 28,000 jobs per $1 billion ($1B) invested. Employment multipliers vary by type of infrastructure and region and tend to be smaller in smaller regions because of greater dependence of smaller regions on imports of goods and services from outside the region.[4]

Several IO studies estimated impacts of infrastructure investment in the context of discussions of the American Recovery and Reinvestment Act (ARRA) of 2009. Heintz et al. (2009) used an IO model to estimate the national direct and indirect employment impacts of a baseline infrastructure investment scenario (averaging $87B per year over five years) and a high investment scenario ($148B per year) and combined that analysis with econometric estimation of a consumption function (to estimate how workers/consumers respond to increased income resulting from the direct and indirect effects) to estimate the induced effects. They estimated that each $1B in infrastructure investment generates about 18,000 jobs on average, due to direct, indirect, and induced employment effects. This impact is 22 percent larger than the estimated impact of an equivalent tax cut; mainly because a larger fraction of the tax cut would be spent by consumers on imports. Heintz et al. predicted impacts of investments in several different types of projects, including energy infrastructure (gas, electricity, solar, wind), transportation infrastructure (roads, bridges, rail, mass transit, aviation, inland waterways, levees), and water infrastructure (dams, drinking water, waste water systems). Across infrastructure types, the predicted employment impacts would be greatest for investments in inland waterways, levees, and dams (about 24,000 jobs per $1B) and smallest for rail, electricity, and wind energy investments (about 15,000 jobs per $1B) (Table 1). Differences in employment impacts across types of infrastructure are mainly due to differences in the imports (from outside the United States) required to build the infrastructure and differences in the labor intensity of industries providing inputs for the investment.

DeVol and Wong (2010) estimated the impacts of $426B invested in a variety of infrastructure projects (highway and transit, broadband, oil and natural gas facilities, water and waste water systems, smart grid electricity transmission, nuclear energy, renewable energy, and air transportation). Across all investments, they estimated a total impact on employment of 10.7 million jobs (average employment multiplier of about 25,000 jobs per $1B investment) and an impact on output of $1.4 trillion (average output multiplier of 3.3).[5] They estimated the highest employment and output multipliers for investments in highways and transit systems and water and waste water systems (27,500 jobs per $1B and $3.45B per $1B, respectively) (Table 2). The

small regions that can readily import labor and capital at a fixed price, or for small interventions that do not change prices in the region.

[4] Here, "imports" refers to goods and services purchased from businesses in other regions, not necessarily imports from other countries.

[5] An output multiplier of an investment measures the dollar value of total output generated from one dollar of investment. "Output" here refers to the total value of production in the economy, including outputs of one industry that are intermediate inputs to other industries and not purchased directly by final consumers (e.g., automobile parts sold to automobile manufacturers). This concept is different than value-added, which subtracts out the value of intermediate inputs used in each industry, and is the basis for Gross Domestic Product (GDP). Output multipliers are thus generally larger than GDP/value-added multipliers.

lowest employment multipliers were found for investments in air traffic control systems (17,500 jobs per $1B) and the lowest output multipliers were found for investments in broadband infrastructure (2.88). One reason for the larger employment multipliers estimated by DeVol and Wong (2010) than by Heintz et al. (2009) may be that DeVol and Wong used the consumption function implied by the BEA IO tables (though this isn't clear in the report), which Heintz et al. argued results in unrealistically large estimates of induced impacts.

The other two studies discussed in this section are IO studies of impacts of particular types of infrastructure investments. Berkman et al. (2010) estimated the impacts of investments in water-related projects such as dams in California. They estimated statewide and local employment multipliers for projects in eight different sub-state regions and found these to range from 10,400 to 13,200 jobs per $1B for statewide impacts and from 8,500 to 13,000 jobs per $1B for local within-region impacts. The fact that smaller employment multipliers were estimated in this study than by Heintz et al. (2009) or DeVol and Wong (2010) is probably because state and local multipliers were estimated. Multiplier estimates are generally smaller for smaller geographic regions because a larger share of spending tends to be spent on imports in smaller regions.

Kuttner (2016) estimated the impacts of the rural broadband industry in the U.S. as a whole and by state. He estimated that the rural broadband industry directly accounted for $17.2B in value added in 2015 and that indirect and induced impacts led to an additional $6.9B in value added, for a total impact of $24.1B (value-added multiplier = 1.40). Overall, he estimated that this industry contributed to 69,600 jobs, considering all impacts. The largest impacts (both in employment and value added) were found in Texas, Florida, and North Carolina.

These studies are just a selection of studies of impacts of infrastructure using IO and SAM models that can be found in the unpublished "grey" literature. As noted earlier, these studies offer only predictions of multiplier impacts and do not account for impacts of infrastructure investments on the productivity and growth of businesses or on consumer welfare. Such impacts can be addressed using econometric methods, such as in the studies reviewed next.

Econometric Literature

Productivity Impacts Estimated Using a Production Function

A large number of studies have estimated the impacts of infrastructure investments on productivity by estimating a production function, taking output to be a function of labor, private capital, public capital, and selected other factors. The seminal study by Aschauer (1989), using national data for 1949 to 1985, estimated the elasticity of total national output (measured by aggregate output of the private sector) with respect to the total value of the stock of nondefense public capital to be 0.39.[6] Aschauer (1990, p. 25) estimated that his elasticity estimates imply a rate of return to an increase in the value of the public capital stock in the range of 50 to 60

[6] The elasticity of an outcome variable with respect to an explanatory variable is the percentage change in the outcome variable resulting from a 1 percent increase in the explanatory variable, holding other factors constant. In this case, the elasticity estimated by Aschauer indicates that a 1 percent increase in the value of the public nondefense capital stock would result in a predicted 0.39 percent increase in the annual value of private national output.

percent.[7] Aschauer also estimated separate elasticities of output to "core" public capital (including highways, mass transit systems, airports, electrical and gas facilities, and water and sewer systems) (estimated elasticity of 0.24), hospitals (0.06), educational buildings (-0.01), other buildings (0.04), and conservation and development structures (0.02). He did not estimate the marginal rate of return to each of these types of public capital, but based on the share of the total nondefense public capital accounted for by core infrastructure in 1985 (53 percent)[8], Aschauer's elasticity estimate of 0.24 for core infrastructure implies a marginal rate of return of 64 percent for core infrastructure.[9]

Aschauer's estimates of the impacts and returns to infrastructure attracted a great deal of attention and stimulated a large number of studies seeking to estimate the output elasticity of public capital using different time periods, units of analysis (whether the data are for the national, state, county, or other regional level), for different countries and regions, and using different econometric methods and regression specifications. A recent review of studies on this topic found around 1,500 published articles on the topic and conducted a meta-analysis of a sample of nearly 2,000 estimated values of the output elasticity of public capital drawn from 145 articles (Núñez-Serrano and Velázquez 2017). Across studies, the average estimated elasticity was 0.13 for short-term impacts and 0.16 for long-term impacts, substantially lower than Aschauer's estimate but still positive on average and in many studies.[10] An earlier review by Bom and Ligthart (2014) analyzed 578 estimates from 68 studies and found that the average output elasticity was 0.08 for short-term impacts and 0.12 for long-term impacts. In both cases, many of the studies and estimates were for countries other than the United States.[11]

For this review, we focus on a sample of 28 studies that provide estimates of the output elasticity of public capital in the United States, listed in Table 3.[12] These studies include 235 elasticity estimates, ranging from a minimum of -0.49 to a maximum of 0.56, with a mean value of 0.12. This mean is slightly larger than the mean value found by Bom and Ligthart (2014) and somewhat smaller than the mean found by Núñez-Serrano and Velázquez (2017).Twelve of the

[7] Aschauer (1990) did not provide the details of this estimate, but presumably he multiplied his elasticity estimate by the ratio of private output to the value of the nondefense public capital stock over his study period. Given the estimate of 0.39, this ratio must have been in the range of 1.28 (0.50/0.39) to 1.54 (0.60/0.39). We use the midpoint value of this ratio (1.41) to estimate marginal returns to public capital based on national level studies. For estimating marginal returns to public capital based on state level studies, which are usually based on Gross State Product (GSP), we use the ratio of GDP to the value of public capital at the national level.

[8] Based on Bureau of Economic Analysis data on the Fixed Reproducible Tangible Wealth of the United States in 1929 to 1995, available at https://www.bea.gov/scb/account_articles/national/0597niw/table1.htm.

[9] This estimate is based on Aschauer's estimated elasticity of core infrastructure (0.24) multiplied by the output/total nondefense public capital stock ratio (1.41, as explained in footnote 7), divided by the core share of the nondefense public capital stock in 1985 (0.53).

[10] Long-term impacts refers to impacts estimated by using "long-differencing" of the data – subtracting the values of variables in an initial period from the values at least several years later – or by conducting time series analysis (in national level studies) to test for a "co-integrating" (long-run) relationship between the public capital stock and output variables (Bom and Ligthart 2014). See the discussion of methods of statistical measurement in the Appendix for more information on these and other methods discussed in this report.

[11] Bom and Ligthart (2014) was based almost entirely on studies in OECD countries. Núñez-Serrano and Velázquez (2017) included more studies in developing countries.

[12] Our selection of these 28 studies was based on the U.S.-focused studies reviewed by Bom and Ligthart (2014) and Núñez-Serrano and Velázquez (2017).

studies reviewed used national level data, similar to Aschauer (1989), while 15 used state level data, and one used county data for California (Boarnet 1998). All of these studies estimated a Cobb-Douglas production function, taking output to be a function of labor, private capital, public capital, and other variables that varied across studies.[13] Several of the earlier studies (Aschauer 1989; Ram and Ramsey 1989; Munnell 1990a; Eisner 1994) estimated the production function using ordinary least squares (OLS) regressions with national data in levels.[14] The minimum output elasticity of public capital estimated in these studies was 0.21.

Aschauer's approach was criticized by several authors (e.g., Aaron 1990; Hulten and Schwab 1991; Jorgenson 1991; Tatom 1991) for not accounting for the nonstationary nature of the data, which could have caused spurious results.[15, 16] These critics argued that the regressions should be estimated in first differences rather than levels to remove common time trends.[17] Three national level studies estimated the model using OLS in first differences (Tatom 1991; Sturm and de Haan 1995; and Delorme et al. 1999) and found a wide range of elasticities, from small and statistically insignificant elasticities found by Tatom (1991) to much larger and statistically significant elasticities found by Sturm and de Haan (1995) and Delorme et al. (1999). Two other national level studies used standard methods of time series analysis to address the nonstationarity issue and also estimated fairly large and statistically significant positive elasticities (Crowder and Himarios 1997; Nourzad 1998). Vijverberg et al. (1997) estimated a production function model similar to Aschauer's and tested for nonstationarity and rejected that assumption. Their estimated output elasticity of public capital was even larger than Aschauer's estimate (at least 0.465 for state-owned capital). Thus, the large output elasticity of public capital estimated by the earlier

[13] The Cobb-Douglas production function assumes that the logarithm of output is a linear function of the logarithms of the input variables, which implies that the elasticities of output with respect to each input are constant, and assumes a constant elasticity of substitution between the inputs. A translog production function, which relaxes these assumptions, was estimated by some authors. The output elasticity of public capital (estimated at the mean of the data) resulting from the translog model was little different from the Cobb-Douglas results in studies that compared these results. We focus our analysis on the Cobb-Douglas results. Other variables that were included in some studies included measures of capacity utilization, unemployment, energy use or energy prices, government expenditures, and others.

[14] Ordinary least squares (OLS) regression is a statistical technique to find the parameters of a predictive model of a dependent variable (e.g., production) based on a set of explanatory variables (e.g., labor and capital stocks) by minimizing the sum of squared differences (residuals) between the values of the dependent variable and the predicted values of the dependent variable. A linear prediction model is most commonly used in OLS regression (i.e., the prediction is a linear sum of parameters multiplied by values of the explanatory variables), but nonlinear least squares regression models are also used. Under certain assumptions, OLS yields the best (lowest variance) unbiased estimates (expected value of the estimate equals the true value) of the parameters of the prediction model.

[15] The problem of spurious correlations can arise in time series data when variables in the regression are all tending to increase (or decrease) over time. Correlations among such nonstationary variables can arise simply because the variables are correlated with time, and not due to any causal relationship among them.

[16] To address productivity trends, both Aschauer (1989) and Ram and Ramsey (1989) included time trends in their regressions, while Munnell (1990a) and Eisner (1994) included an index of multifactor productivity. Inclusion of these variables may have reduced problems of nonstationarity of the data and spurious correlations in these studies, though these studies did not reports tests of this.

[17]Munnell (1992) countered that using first differences would eliminate any long run relationships between public capital and output, and that one would not expect short term changes (e.g., within one year) in the public stock to greatly affect short term changes in output.

national level studies appears not to be explainable by spurious correlations resulting from nonstationary time series.

Another criticism of these national level studies is that they could be affected by many sources of omitted variable bias.[18] Other variables that were correlated with economic growth and economic cycles could have been correlated with changes in the public capital stock. Various studies attempted to address such issues by including additional variables such as capacity utilization, unemployment, energy quantity or price, public expenditures, and others, and the results appear to be robust to inclusion or exclusion of such variables. Nevertheless, this does not guarantee that the estimates are not affected by other omitted variables; but this can never be guaranteed in a regression analysis.

Probably a more serious concern is the problem of reverse causality – i.e., changes in public capital stocks may have been caused by changes in output (e.g., increased output leads to increased tax revenues which facilitates public investment in infrastructure), rather than or in addition to the reverse. Aschauer (1989) addressed this issue using a two-stage least squares (2SLS) instrumental variables model,[19] using lagged values of government capital stocks as instrumental variables and found that his estimate of the output elasticity was not greatly affected. Finn (1993) used a generalized methods of moments (GMM) estimator[20] and estimated a substantially smaller output elasticity of public capital than Aschauer (0.158), though still statistically significant. None of the other national level studies addressed the reverse causality issue.

A related concern with national level time series studies is that they lack a counterfactual comparison group, generally regarded as essential in impact evaluation studies. By their nature,

[18] Omitted variable bias refers to a bias in the estimated parameters of an OLS (or other) regression model that can result from exclusion of explanatory variables that affect the outcome variable and that are correlated with one or more of the included explanatory variables. For example, if private capital stocks were excluded from a regression model predicting output based on public capital stocks and labor, the estimated coefficients for public capital stocks (and labor) could be biased. If private capital stocks tend to be greater where there are more public capital stocks and private capital stocks contribute to greater output, the coefficient of public capital stocks would reflect to some extent the effect of greater private capital stocks in places having greater public capital stocks, and thus overestimate the effect of public capital stocks on output.

[19] Two-stage least squares (2SLS) regression addresses the problem of reverse causality and the more general problem of "endogenous" explanatory variables (explanatory variables that are affected by unobserved factors that affect the dependent variable) by using a predicted value for the endogenous explanatory variable. The variables predicting the endogenous explanatory variable must satisfy several properties to produce valid unbiased estimates of the true causal relationship from the explanatory variable to the dependent variable: (i) at least one of the variables used to predict the endogenous explanatory variable must affect the outcome variable only by affecting the endogenous explanatory variable and can therefore be excluded from the regression without causing omitted variable bias (such an excludable variable from the regression model is called an "instrumental variable"); (ii) the excluded instrumental variable(s) should be strong predictor(s) of the endogenous explanatory variable; and (iii) the instrumental variable(s) (as well as the other explanatory variables in the regression) should themselves be exogenous (i.e., not correlated with the error term in the regression). If these conditions are satisfied, the predicted rather than actual values of the endogenous explanatory variable will not be correlated with the error term in the regression, thus eliminating the bias.

[20] Generalized methods of moments (GMM) is an instrumental variables method that is a generalization of 2SLS.

national level studies must rely only on before-after comparisons to identify impacts, which is problematic because of omitted variables and reverse causality concerns.

Some of these issues are more readily addressed using state level data. For example, the effects of fixed omitted variables can be removed from the regression by using fixed effects or first difference estimation with state level panel data, as most of the state level studies have done (Eisner 1991; Evans and Karras 1994; Holtz-Eakin 1994; Baltagi and Pinnoi 1995; Holtz-Eakin and Schwartz 1995a, 1995b; Garcia-Mila et al. 1996; Holtz-Eakin and Lovely 1996; Kelejian and Robinson 1997).[21] Munnell's (1992) concern about first difference estimation applies to these studies as well. This concern was addressed in several studies that used "long differences" (differences in variables at least several years apart) in the estimation (Holtz-Eakin 1994; Baltagi and Pinnoi 1995; Holtz-Eakin and Schwartz 1995b; Garcia-Mila et al. 1996); most of these studies found that using long differences did not change their estimates substantially.[22]

In general, the state level studies that used fixed effects or first difference methods find smaller output elasticities of public capital than national level studies. The average elasticity estimated by these studies is 0.003, with a range of -0.49 to 0.38, and few of the elasticities estimated in these studies are statistically significant. By contrast, the six state level studies that did not use fixed effects or first difference estimation (Munnell 1990b; Garcia-Mila and McGuire 1992; Munnell 1993; Andrews and Swanson 1995; Berechman et al. 2006; Cohen 2010) estimated larger elasticities on average than the other state level studies but smaller than the national studies. The average elasticity estimated in these studies is 0.090, with a range of -0.01 to 0.38. The set of state level studies that do not incorporate fixed effects or first differences may be affected by omitted variable bias due to unobserved fixed differences across states in productivity that are correlated with the level of public capital stock. Thus, the range of estimates found in the first nine state level studies may be a more reliable indicator of the potential state level productivity impacts of public capital stocks.

To investigate this further, it is important to account for the fact that the type of public capital stocks considered varies across studies. In some studies, the elasticity is for total public capital within a state and in others it is for particular types of public capital, such as highways, water and sewer systems, or other public capital. Table 4 presents the averages of selected elasticities

[21] Panel data refers to data having multiple observations for particular units of observation at different points in time. State level panel data has observations for states from multiple years. A fixed effects panel regression subtracts the mean over time for each unit of observation from the value of each observation, for all variables in the regression. In a linear model, such differencing removes the effects of any fixed factors (which are differenced out), eliminating any omitted variable bias that could result from such fixed factors (e.g., differences across states in their natural endowments such as climate that may affect productivity and may be correlated with infrastructure stocks or other variables of interest). Taking differences over time for observations within a panel data set, such as in first difference or long difference models, also removes the effects of fixed factors.

[22] Another advantage of use of long differences is that it should reduce the effects of measurement errors in the explanatory variables (Baltagi and Pinnoi 1995; Garcia-Mila et al. 1996). Consistency of results between models using short and long differences thus helps to allay concerns about this source of potential bias. However, Baltagi and Pinnoi (1995) found that the output elasticity of public capital was positive and statistically significant in their first difference 2SLS model but negative and statistically significant in their fourth difference 2SLS model, a difference they attributed to the effects of measurement errors.

estimated by different studies at the national and state level.[23] As shown, nine studies estimated the output elasticity of total nondefense public capital at the national level and eight studies estimated this elasticity at the state level. Only two national studies (Aschauer 1989 and Munnell 1990a) estimated the output elasticity of core public capital. One national study (Finn 1993) and ten state level studies estimated the output elasticity of highway capital, while seven studies estimated the output elasticities of water and sewer capital and of other state and local capital.

The results reported in Table 4 support the view that national level estimates of the output elasticity of public capital are larger than the state level estimates, after controlling for the type of capital stock being considered.[24] It also supports the view that studies that used fixed effects or first differences to control for omitted fixed factors estimated lower output elasticities for each type of public capital.[25] For example, the average output elasticity for total state level public capital was 0.139 for the three studies that did not use a fixed effects or first difference estimator, compared to -0.024 for the five studies that did. Similar differences by type of study were observed for output elasticities of the specific types of state and local capital.

A few of the state level studies used 2SLS or GMM estimation (using lagged values of variables as instrumental variables) to address endogeneity or measurement errors in the public capital stock and other variables (Holtz-Eakin 1994; Baltagi and Pinnoi 1995). Both of these studies found that the output elasticity of total public capital was statistically insignificant or significant and negative in their preferred specifications. However, Baltagi and Pinnoi (1995) also found positive and statistically significant output elasticities for highway capital and water and sewer capital in several of their fixed effects and first difference regressions (with or without using instrumental variables). The estimated output elasticity for highway capital in these models ranged from 0.002 (and statistically insignificant) to 0.10 (and statistically significant); the output elasticity for water and sewer capital ranged from 0.05 to 0.22 (statistically significant in all models). By contrast, they found that the output elasticity of other state and local capital was either statistically insignificant or negative (minimum value of -0.20) across their specifications.

These results and others reported in Table 4 suggest that the productivity impact of water and sewer capital is greater than that of either highway capital or other state and local capital. They also suggest that the effect of total public capital on productivity may involve a mix of positive and negative effects of different types of capital, leading to relatively small effects overall at the state level.

[23] In cases where a study estimated multiple values of the same elasticity, we selected a single estimate for the analysis based on our judgment of which estimates were the most reliable or representative, given the analysis presented in the study. For example, if statistical tests showed that an instrumental variables or fixed effects estimator was superior to an OLS estimator, the results from the preferred estimator was used. In some cases, authors were fairly clear about which estimates they preferred and why; though this was not always the case.

[24] This finding of larger output elasticities of public capital using national vs. regional level data is consistent with findings of the meta-analyses conducted by Bom and Ligthart (2014) andNúñez-Serrano and Velázquez (2017)

[25] This finding is not consistent with the findings of the meta-analysis by Bom and Ligthart (2014) – who found no statistically significant difference in elasticities between studies that controlled for fixed effects and those that didn't – or Núñez-Serrano and Velázquez (2017) – who found that studies that controlled for fixed effects had somewhat smaller elasticities. Our finding appears to be particular to studies of the United States, but is based upon a small number of studies that used state-level data.

Accounting for Spillover Effects of Infrastructure

A concern with state level estimates of productivity impacts of public capital is the possibility for network effects and spillovers of impacts across state boundaries, which are not reflected in analyses focusing only on within state impacts, such as all of the studies using fixed effects or first difference panel estimators.[26] This concern has been argued as a reason why state level studies typically estimate smaller productivity impacts than national ones (Munnell 1992). Several studies addressed this spillover issue, mainly for highway infrastructure (Holtz-Eakin 1994; Holtz-Eakin and Schwartz 1995b; Kelejian and Robinson 1997; Boarnet 1998; Berechman et al. 2006; Cohen 2010).

Holtz-Eakin (1994) addressed the issue of spillover effects by estimating a version of his model for eight multi-state regions, as well as for states, and found little difference between the output elasticity estimated using region level data vs. state level data. He concluded that the effect of spillovers within multi-state regions is roughly zero. Holtz-Eakin and Schwartz (1995b) investigated the spillover effects of state highways using a spatial econometric model incorporating the effects of highway stocks in nearby states as well as own-state highways in the production function. They found that a state's own highway stock has a small and statistically insignificant effect in all regressions (output elasticity estimate ranging from -0.007 to 0.016) and that spillover effects were either negative or statistically insignificant in all regressions.

Kelejian and Robinson (1997) estimated an array of spatial econometric models to address issues arising from the spillover effects of public capital and productivity in neighboring states and serial correlation and spatial autocorrelation in the error term.[27] They estimated a negative elasticity of own-state public capital in all models that incorporated fixed effects, although this estimate was statistically insignificant in models that accounted for spatial autocorrelation. The estimated effect of neighboring states' total public capital was positive but statistically insignificant in almost all of their specifications. In models investigating impacts of specific types of state and local public capital, they found that a state's own highway capital has a negative output elasticity, but was statistically insignificant in their preferred specification. Neighboring states' highway capital had a positive elasticity in all specifications, but was also statistically insignificant in their preferred specification. Water and sewer public capital and other state and local public capital – both in the own state and in neighboring states – had statistically insignificant productivity impacts in almost all of their specifications, including the preferred one.

[26] In these panel data studies, the effects of infrastructure are estimated based only on variation within states over time, since fixed differences across states are netted out.

[27] Serial correlation refers to correlations between the error terms of a regression for the same observational unit across time. Spatial autocorrelation refers to correlation in the error terms across units at the same point in time. In regression models without endogenous explanatory variables, correlations in the error term across observations affect estimation of standard errors and the size of confidence intervals, but do not bias the coefficient estimates. But such correlations may also indicate the presence of relevant omitted variables that may cause biases. Accounting for such correlations in the estimation therefore is important for obtaining valid statistical inferences.

Boarnet (1998) investigated spillovers of county and state-owned street and highway capital on productivity in California counties, using a model with spatial lags[28] of street and highway capital and long differences. He estimated a positive and statistically significant output elasticity of a county's own street and highway capital – ranging from 0.24 to 0.30 across specifications[29]– and a negative output elasticity of neighboring counties' street and highway capital in most specifications (ranging from -0.81 to 0.12).[30] A negative elasticity of neighboring counties' public capital suggests a competition effect of infrastructure investment in one county on neighboring counties. Boarnet also estimated the output elasticity of state-owned highway capital in one model and found a positive elasticity for both own-county and neighboring-county highways (0.065 for the own county elasticity and 0.047 for the neighboring county elasticity). Thus, the competition effect was evident only for investment in county-owned streets and highways, whereas state-owned highways showed positive spillover effects.

Berechman et al. (2006) estimated models of spillover effects of highway capital at the state level for the 48 contiguous states and at the county and municipality levels for the 18 counties and 389 municipalities in the New York/New Jersey metropolitan area. They used data on public expenditures by the Federal Highway Administration in a given county to represent the highway capital "stock" (though expenditures are a flow, not a capital stock) and estimated the effects of the region's (whether state, county, or municipality) own highway capital stock and neighboring regions' highway capital stock on regional output.[31] They estimated that for states, the output elasticity was 0.035 for own highway capital and 0.021 for neighboring states' highway capital. For counties, the elasticity estimates were 0.042 for own county highway "capital" and 0.022 for neighboring counties' highway "capital", and for municipalities, the elasticities were -0.009 for own municipality and 0.01 for neighboring municipalities highway "capital". The models estimated did not include fixed effects or use differencing to address unobserved fixed factors, so the results are subject to the same criticism as earlier studies that used a similar approach. Besides not accounting for fixed effects, their estimates at the county and municipality level are suspect because of the use of a flow measure to represent highway capital stocks.

Cohen (2010) estimated the elasticity of manufacturing output with respect to a state's own highway stocks, accounting for the effects of manufacturing output in neighboring states on a state's own manufacturing output. He did not investigate the effects of highway stocks in neighboring states on a state's manufacturing output, so this study did not directly address the spillover effects of infrastructure investment. He estimated an elasticity of manufacturing output to state highway stock of 0.119, accounting for the "spatial multiplier effect" of the

[28] "Spatial lags" refers to including the average value of variables in neighboring units (counties in Boarnet (1998)) as explanatory variables to investigate spatial spillover effects.
[29] The different specifications estimated by Boarnet (1998) corresponded to different weighting schemes for measuring the weighted average of neighboring counties' street and highway capital.
[30] This estimate was statistically significant in only two of Boarnet's five specifications.
[31] Most other studies of productivity impacts of public capital use a measure of the stock of public capital, following Aschauer (1989) and Munnell (1990). For their state level analysis, Berechman et al. (2006) also used Munnell's data on highway capital stocks. However, in their analysis for municipalities, they apportioned the county-level FHWA expenditure flows based on personal income.

interdependence of manufacturing output among states.[32] As with several other state level studies, Cohen (2010) did not include fixed effects or first differences, which may have resulted in a biased estimate of the output elasticity.

Overall, the studies that addressed spatial spillovers provide little evidence to overturn the main findings of the set of studies that did not address this issue. When fixed effects or first difference estimation is used with state data as in Holtz-Eakin (1994), Holtz-Eakin and Schwartz (1995b), and Kelejian and Robinson (1997), small and usually statistically insignificant output elasticities of own-state and neighboring states' public (usually highway) capital stocks are found. The state level findings of Berechman et al. (2006) and Cohen (2010) are broadly consistent with other studies that found positive elasticities of state capital stocks when not accounting for fixed effects, and are suspect for that reason. The findings of Berechman et al.'s (2006) county and municipal level analyses are also suspect because a current expenditure variable was used to represent highway capital stocks. Boarnet's (1998) estimation approach for California counties is more credible, but yields mixed and mostly statistically insignificant results, and is representative of only one state. Nevertheless, the potential for competition effects of infrastructure investments across local jurisdictions is an interesting finding worthy of further exploration in other contexts.

Marginal Rates of Return Implied by Output Elasticity Estimates

As noted in the discussion of Aschauer's (1989) paper, the output elasticity estimates have implications for the marginal returns to public capital stocks. The general formula is:

Marginal rate of return = Output elasticity of capital stock x (Output/Capital stock ratio)

Unfortunately, few studies besides Aschauer (1990) estimated this parameter, which can inform policy debates about the returns to infrastructure investment. Table 5 provides our rough estimates of the marginal rate of return implied by the mean output elasticities reported in Table 4. A key point to notice from comparing the estimated marginal returns in Table 5 to the elasticities in Table 4 is that the marginal returns are larger in magnitude and have larger variation that the elasticities. That is because the output/public capital stock ratio is greater than 1.0, even when considering all nondefense public capital, and can be much greater than 1.0 for specific types of capital. The marginal returns based on state level studies range from -0.91 to 1.328, while the elasticities from those studies range from -0.049 to 0.139. It is thus an error to view a "small" output elasticity as implying low marginal returns to public capital, without considering the output/capital ratio. For example, the "small" mean output elasticity of water and sewer capital (0.075) implies an annual marginal return of such capital of 88 percent.

The main substantive points suggested by the results in Table 5 are that the marginal return (in terms of productivity impacts) of core infrastructure appears to be greater than the return to all infrastructure and that water and sewer capital appear to have much higher marginal returns than

[32] If \$1 of increased output in a state increases output in neighboring states by \$$\rho$, the spatial multiplier effect of an increase in public capital stock in a state is $1/(1-\rho)$ times the direct impact. In Cohen's study, the direct manufacturing output elasticity of highway capital was estimated to be 0.106 and ρ was estimated to be 0.112; resulting in a total output elasticity of 0.119 (= 0.106/(1-0.112)).

other types of state and local capital. These points are based on a small number of studies, however; hence may not be robust.

It is important to note that the marginal rates of return to public capital stocks estimated in Table 5 are conceptually not the same as internal rates of return or benefit-cost ratios to federal investments in public capital. As argued by the Congressional Budget Office (CBO 2016, p. 4), $1 in federal investment may be offset by a decrease in public investment by state and local governments and to a lesser extent by private entities. CBO (2016) estimates that “on average, each dollar of federal investment increases total public investment by only two-thirds of a dollar”. There is debate in the literature, however, concerning the extent to which federal investment displaces or attracts investment by other levels of government and the answer probably depends on the context and the nature of the investment or the program promoting the investment.[33]

In addition to such potential displacement effects, investments in public capital do not necessarily increase the value of public capital because of depreciation. And because it takes time (often many years) to complete infrastructure investment projects and for the full benefits of an investment to be realized, the internal rate of return to infrastructure investment (which is reduced by delays in realizing the full benefits of an investment) can be much lower than the steady state marginal rate of return to an increase in the infrastructure stock (even aside from the effects of depreciation), due to discounting of future benefits because of the opportunity cost of capital. Such opportunity costs can include not only the financial cost of borrowing, if investments are financed by borrowing, but also losses in the economy resulting from taxation, which is ultimately necessary to pay for investments, even if initially financed by bonds. Such “user costs of public capital” may be quite substantial. For example, Morrison and Schwartz (1996) estimated the user cost of public capital in 1985 to be at least 20 percent, considering only depreciation and financial costs, but as high as 35 percent considering the economic costs of taxation. Bom and Ligthart (2014) estimated a lower user cost of public capital of 14 percent, considering only depreciation and financial costs, and based on more recent real interest rates, which have declined since the 1980s.

These factors suggest why a marginal rate of return to public capital even as high as 50 to 60 percent, as estimated by Aschauer (1990) and criticized by several authors, may not be inconsistent with the claim by Holtz-Eakin and Schwartz (1995b, p. 459) that “traditional, project-based analyses of benefits and costs [of public infrastructure projects] typically do not find large [internal] rates of return for new projects”.[34]

[33] For example Knight (2002) found that increases in federal highway grants are almost fully offset by reduced state spending on highways, with little or no net increase in spending. The Government Accountability Office (GAO 2004) estimated that about half of increases in federal highway spending were offset by lower state and local highway spending. By contrast, Leduc and Wilson (2015) found that highway grants resulting from the American Recovery and Reinvestment Act (ARRA) of 2009 led to higher spending by state governments.

[34] Holtz-Eakin and Schwartz (1995b) did not provide any evidence to support this claim.

Productivity Impacts Estimated Using Vector Autoregressions

Pereira (2000) used vector autoregressions (VAR) to investigate the dynamic relationships among public investments in different types of public capital, private investment, private employment, and output (GDP), using national level data for 1956 to 1997.[35] Allowing for intertemporal causal linkages in both directions among these variables (under the key assumption that current output does not affect current public investment, but may affect future public investment), Pereira estimated positive long-term elasticities of output with respect to investment in different types of infrastructure. The estimated elasticities imply a 7.8 percent rate of return on investments in public capital overall, including a 3.4 percent rate for highways and streets, 7.2 percent for conservation and development structures, 8.9 percent for buildings, 9.7 percent for water and sewer systems, and 16.1 percent for electric and gas facilities, transit systems, and airports.

This study takes a substantial step forward from the production function studies by allowing for dynamic feedback effects among public capital investments, output, and other production inputs. In this type of analysis, rather than being just a confounding factor, reverse causality is part of the mechanism of impacts of public investment. For example, public investment may stimulate increased private investment and use of labor, resulting in greater output and income, which in turn may facilitate further public investment and impacts over time. One limitation of this study (like other national level time series studies) is the limited number of observations and the absence of data on a counterfactual, which may limit the robustness of the results. The potential for omitted variables appears to be substantial, given that only a few variables are considered in the VAR system and capital stocks are not accounted for (only investment flows).

Cost and Profit Impacts

Productivity impacts of infrastructure can be measured by the impacts on firms' costs or profits as well as by measuring impacts on the value of production. Some studies have used this so called "dual approach" to estimate such impacts (e.g., Morrison and Schwartz 1996; Vijverberg et al. 1997; Cohen and Morrison Paul 2003). Morrison and Schwartz (1996) estimated the value of additional state level infrastructure in reducing costs of manufacturing firms by estimating a cost function and system of input demand functions.[36] The impact of an increase in infrastructure on manufacturing firms' cost – the marginal value of infrastructure – was estimated for four subnational regions and by year from 1972 to 1987. The marginal impact was found to be negative and statistically significant for all regions and years, with values ranging from -0.056 (representing a \$0.056 cost reduction per additional \$1.00 of the value of state level infrastructure) to -0.349. These returns generally increased over time in all regions and were largest in the South, especially in later years; apparently due in part to a declining ratio of public

[35] Vector autoregression (VAR) is a statistical technique to investigate the concurrent and intertemporal relationships among a system of variables over time. VAR analysis allows simulation of impacts of a shock in one variable on the evolution of the system of variables, based on the estimated relationships.

[36] Cost functions and input demand functions assume that costs and input demands are a function of the quantity of output and the prices of inputs, based on the theory of a cost-minimizing firm. Morrison and Schwartz's economic model included state fixed effects.

to private capital, which was greatest in the South. In all regions except the South, the marginal value of infrastructure was estimated to be less than the marginal value of private capital, estimated to range 0.149 and 0.686 in the other regions. In the South, the estimated marginal value of private capital ranged from 0.102 to 0.141 and was less than the marginal value of infrastructure in most years. Comparing the estimated marginal values of infrastructure to alternative estimates of the user cost of public capital – which ranged from 0.055 to 0.392 over the estimators and time periods – the net benefits of state infrastructure investment on reducing costs in the manufacturing sector were positive in most years and regions without adjusting for the costs of taxation. Including the costs of taxation, the net benefits for the manufacturing sector appear to have been negative in most years outside of the South, but close to zero or positive in many years in the South. Considering that infrastructure likely has positive benefits other than its impacts on the costs of manufacturing firms, these estimates suggest that there was underinvestment in infrastructure in the South. The implications for other regions are less clear.

In addition to estimating productivity impacts of public capital using a production function approach, Vijverberg et al. (1997) also estimated impacts of public capital on the costs and profits of the nonfinancial corporate sector, using national level data. Using cost data, they estimated that the marginal return to state public capital (at the means of the data) is 0.144 in the most plausible model, while the estimated marginal return to federal capital in the most plausible model is 1.222 – more than 100 percent rate of return. Their preferred version of the estimated profit model predicts a marginal return to federal capital on corporate profits of 0.593 (i.e., a $0.59 increase in annual profits for each $1.00 of increased value of federal capital) and a marginal return to state capital of 0.139.

Cohen and Morrison Paul (2003) estimated the impacts of own state and other state airport capital on manufacturing sector costs using a spatial econometric model to account for spillovers of airport impacts among states and spatial autocorrelation in the error term. They estimated that for states with large hubs, the manufacturing cost elasticity of own state airport capital was -0.113, meaning a 1 percent increase in airport capital within the state reduces costs of manufacturing firms by 0.113 percent. The cost elasticity of other state airport capital for states with large hubs was similar: -0.116. For states without a large hub, the cost elasticity of own state airport capital was -0.056, while the cost elasticity of other state airport capital was -0.188. These results indicate the relatively greater importance of investments in airports in other states for states without a major hub. The authors did not provide estimates of the marginal returns to airport capital implied by these elasticity estimates. Unlike Morrison and Schwartz's (1996) model, Cohen and Morrison Paul (2003) did not include state fixed effects in their regression model, which could have resulted in biased estimates.

Estimating Amenity and Productivity Impacts Using Spatial Equilibrium Theory

Studies focused only on the productivity impacts of infrastructure neglect the possibility that infrastructure may also have value as a *consumer amenity*;[37] that is, it may have direct value to

[37] Amenities are local characteristics (such as the climate, scenery, or access to infrastructure) that are of value to people but are not directly purchased in markets.

consumers even if it doesn't affect the productivity, costs, or profits of businesses. Dalenberg and Partridge (1997) considered this possibility in estimating the impact of public capital stocks on wages using state-level data. Drawing on the spatial equilibrium theory of Roback (1982), they argued that public capital stocks can affect wages either by affecting the productivity of firms or because of their amenity value to workers. If productivity effects dominate, increases in public capital stocks should increase wages; but if amenity effects dominate, increases in public capital stocks should reduce wages (because workers will be willing to accept lower wages to live in places where they enjoy greater nonmarket amenities, including public capital). They found that highways and water and sewer systems were associated with lower average earnings, suggesting that amenity effects of these types of capital outweigh their productivity effects. Other types of public capital increased average earnings, suggesting productivity effects are greater for those types.

This is the first study among those reviewed to point out that infrastructure can have amenity effects as well as productivity effects, which is an important issue to consider when interpreting results of other studies that investigate impacts on output (which is strongly correlated with average earnings and income). Perhaps state level studies that find an insignificant or negative effect of infrastructure on output are reflecting in part the amenity value of infrastructure tending to reduce workers' wages and incomes in states having greater stocks of public capital; if infrastructure has value as an amenity.[38] This study also addressed endogeneity of public infrastructure (using 2SLS estimation), serial correlation, and spatial autocorrelation and the results were robust. The main drawback to their approach is that the underlying spatial equilibrium assumptions guiding the interpretation of the results were not tested and cannot be tested with the data used. According to Roback's theory, wages can either go up or down or stay unchanged if public capital stocks are increased, so any result for wages is consistent with the theory.

To estimate the value of infrastructure with a spatial equilibrium model, impacts on rents or property values must also be taken into account. Haughwout (2002) applied Roback's spatial equilibrium approach to estimate the value of public capital stocks in 33 large cities using household level data on both wages and housing prices. Based on estimated wage and housing price functions, Haughwout estimated the aggregate willingness to pay for a one standard deviation increase in the public capital stock (costing $4.64B in 1990 dollars) in the cities studied, which he found was in the range of $1.4B to $2.8B (i.e., a benefit-cost ratio (BCR) of 0.30 to 0.60). Based on the fact that the estimated BCR of infrastructure investment was less than 1.0, Haughwout argued that there was an oversupply of infrastructure in these cities. In all of his econometric specifications, Haughwout found that the amenity value of infrastructure to households was much greater than the productivity value to firms; implying that estimates based only on productivity impacts may seriously underestimate the value of infrastructure.

[38] This effect is relevant to studies using state-level or other subnational regional level data, but not to national level studies, since the issue of mobility of firms and workers affecting relative wage levels in different regions does not arise in national level studies.

A recent working paper by Albouy and Farahani (2017) updated and extended the estimation approach of Haughwout with methodological advancements and more recent data for 55 cities. Their methodological advances included allowing for the effects of non-traded production, Federal taxes, and imperfect mobility of households. One major difference between Albouy and Farahani's approach and Haughwout's is to treat housing as produced by a non-tradable sector whose productivity can be affected by infrastructure, rather than implicitly assuming that variations in housing values reflect only variations in land values. They also accounted for the effects of Federal taxes on the spatial equilibrium conditions. They showed that accounting for these effects (assuming initially that productivity in the housing sector is equal everywhere) more than doubles the estimated BCR of infrastructure investments in the cities studied by Haughwout, to a range of 0.70 to 1.35. They also pointed out that these estimates exclude the spillover benefits of urban infrastructure on surrounding suburban areas and could also be downward biased as a result of errors in measuring infrastructure stocks. Their results replicated Haughwout's finding that amenity benefits dominate productivity benefits in determining the value of infrastructure investments. Albouy and Farahani then extended the analysis using a longer panel of data – 1974 to 2011 – for more cities (55) and using additional data sources for housing prices and wages. In this extended analysis, they found a smaller BCR range for infrastructure investments in the 55 cities studied – BCR range from 0.62 to 0.73 – which is closer to Haughwout's original estimates. But Albouy and Farahani's revised estimates show a greater effect of productivity relative to the consumer amenity effects of infrastructure. Finally, Albouy and Farahani extended their model to allow for the effect of infrastructure on productivity in the non-tradable housing sector and for either perfect or imperfect mobility of households and find that their BCR estimates for infrastructure investment are increased to a range of 1.17 to 2.12. Albouy and Farahani's results demonstrate the sensitivity of estimated benefits of infrastructure to the assumptions of the estimation model and that many reasonable adjustments to Haughwout's (2002) model imply higher BCR's than previously estimated.

Unfortunately, neither Haughwout (2002) nor Albouy and Farahani (2017) estimated the value of increases in different types of infrastructure or the value of infrastructure in rural areas. Further research is needed to shed light on these issues using Roback's spatial equilibrium approach and extensions of it. However, there are hedonic studies that investigate the impacts on property values of particular types of infrastructure; some of these are discussed below.

Impacts of Broadband

A fairly large number of studies on the economic impacts of broadband were found in the literature. The econometric literature on economic impacts of broadband reveals a wide range of estimates of impacts. Most studies reviewed found positive impacts of broadband availability or adoption on the level or growth of employment, while impacts on the value of output, earnings, or income were more mixed and often statistically insignificant across studies and contexts. Several studies found that broadband availability was associated in some contexts with lower (level of or growth in) wages or income (e.g., Kolko (2012), Mack and Faggian (2013), Whitacre et al. (2014a)). This result is consistent with the Roback spatial equilibrium theory discussed above and suggests the need to investigate impacts of broadband on rents or property values as well as on labor market outcomes to better understand the value of broadband as a consumer amenity, in addition to its productive impacts for businesses.

Only three of the studies reviewed investigated impacts of broadband on rents or property values – Lehr et al. (2006), Molnar et al. (2015), and Render (2016). Lehr et al. (2006) found conflicting results of the impact on median rents within zip codes depending on the method used (OLS regression or matching) and that study has several methodological problems.

Molnar et al. (2015) is a stronger study, based on hedonic regressions of house sale prices from over 500,000 sales and using detailed data on broadband availability at the neighborhood (census block group) level from the National Broadband Map. That study found that access to fiberoptic capability increases house values by 1.3 percent on average, that faster maximum available download speeds increase house prices by up to an additional 6.0 percent (for 1 Gbps compared to 25 mbps maximum), and that a larger number of local non-fiber Internet service providers (ISPs) also increases house prices. However, Molnar et al. (2015) did not fully address the potential endogeneity of broadband (e.g., broadband firms may be attracted to neighborhoods with greater property values and more ability to pay for broadband)[39] and did not investigate impacts of broadband in different contexts (such as in rural vs. urban areas). Further studies along similar lines would be valuable, addressing the endogeneity issue and investigating the heterogeneity of impacts of broadband on property values across the rural-urban spectrum or by varying educational and socioeconomic characteristics.

Render (2016) estimated the impact of access to fiber optic broadband on the value of multi-dwelling units (MDUs) (apartments and condominiums) using a survey of over 2,000 MDU residents in the United States and Canada. The survey asked respondents what discount would be required for them to purchase or rent an otherwise equivalent condominium (assumed to be worth $300,000 with fiber to the home) or apartment (worth $1,000 monthly rent with fiber to the home) without fiber to the home. For a condominium, the mean discount required was 2.8 percent ($8,528) and for an apartment, the mean discount was 8.0 percent ($80 per month). Render (2016) reported that a parallel study for respondents considering purchase of a single family home worth $300,000 found the mean discount to be 3.2 percent ($9,734). He interpreted the higher percent discount required by apartment renters to be due to a shorter time horizon for renters. These estimated percentage discounts for access to high speed fiber are of the same order of magnitude as the housing value impacts estimated by Molnar et al. (2015).

Kim and Orazem (2017) investigated the impacts of broadband availability on the location decisions of new businesses in rural Iowa and North Carolina, using a difference-in-difference estimation approach to correct for selection bias.[40] Kim and Orazem found that that new

[39] Molnar et al. (2015) used a control function approach to correct for any bias caused by the endogeneity of broadband. However, as they acknowledged, this approach did not use any excluded instrumental variables in the control function, implying that their results were identified based on the assumed nonlinearity of the control function. They indicated their lack of confidence in the control function results and intention to pursue this issue further in subsequent research.

[40] Difference-in-difference (DD) estimation involves estimating the impact of an intervention by estimating the mean difference between the change in outcomes from before to after the intervention for observational units that are affected by the intervention (the "treatment group") and units that are not affected (the "control group"). In a regression context, the effects of differences in observed control variables on the changes in outcomes are accounted

businesses are 60 percent to 101 percent more likely to locate in rural zip codes with broadband availability, while using a more traditional fixed effects approach, they found that new businesses were only 3 percent more likely to local in rural zip codes with broadband availability. They argued that the fixed effects estimate represents a lower bound of the true broadband effect.

Two studies investigated impacts of the USDA Rural Utility Service's (RUS) Broadband Loan Program (BBLP). Kandilov and Renkow (2010) found that the pilot BBLP had positive impacts on several outcomes – including employment, payroll, and number of establishments – but found little evidence that the regular BBLP had positive impacts. Kandilov et al. (2017) found that the pilot and regular BBLP had positive impacts on farm sales, expenditures, and net revenues, though the impacts of the pilot program were larger. Annual farm net revenues were on average $24,000 greater in a county that received a pilot BBLP loan and $9,000 greater in a county that received a regular BBLP loan.

Some of the studies reviewed investigated heterogeneous impacts of broadband, with multiple studies finding that broadband has more positive impacts where people are more educated and skilled (Forman et al. 2012; Kolko 2012; Atasoy 2013; Mack and Faggian 2013). However, studies reach conflicting findings regarding whether broadband has more positive impacts in more rural areas. Atasoy (2013) found a more positive impact of broadband access on employment in more rural areas and Kolko (2012) found larger employment impacts in less densely populated zip codes, but also found larger impacts in more populated counties. Forman et al. (2012) found very little impact of adoption of advanced Internet technologies on employment and wage growth outside of 163 initially advantaged counties with high population, high education, high income, and high IT-intensity. Kim and Orazem (2017) found that the positive impact of rural broadband on business location decisions was larger in more populated rural zip codes and in metro adjacent areas. Kandilov and Renkow (2010) found that the zip code level impacts of the pilot BBLP on employment and payroll were positive only for metro counties, while the impact on the number of establishments was largest in nonmetro counties that are not adjacent to a metro county. Kandilov et al. (2017) found that the positive impact of the pilot BBLP on farm sales was confined to metro adjacent counties. Thus, it appears that the impacts of broadband are highly outcome-dependent and context-dependent.

No peer reviewed studies were found that estimated the benefits of rural broadband relative to the costs. Estimates that have been developed are only rough and are subject to considerable

---

for. The DD estimator assumes that mean changes in the observed outcomes of the control group, after accounting for the effects of observed control variables, are equal to the mean changes that would have occurred for the treatment group in the absence of the intervention (the unobservable *counterfactual* outcomes). *Selection bias* results if the mean outcomes for the control group are not equal to the mean counterfactual outcomes of the treatment group after controlling for observed differences between the groups (Heckman et al. 1998). Kim and Orazem (2017) addressed the potential for selection bias by including the intervention indicator as an explanatory variable for the pre-intervention outcomes of the control observations as well as for the treatment observations. If there is selection bias, it is reflected by the "effect" of the intervention on the control observations (if there were no selection bias, this "effect" should be zero), and this bias is also added to the estimated effect of the intervention on the treatment group. So the DD estimator, which computes the differences in outcomes between the treatment and control groups, subtracts out the effect of this bias.

uncertainties concerning both the estimated benefits and costs, but illustrate the potential value of rural broadband and the need for more rigorous research on this topic. For example, in a recent article in the online magazine, the *Daily Yonder*, Gallardo and Rembert (2017) estimated that the economic benefits of providing access to broadband (defined as meeting the FCC's current definition of 25 mbps download/3 mbps upload speed) to all unserved households would be a maximum of $22.5B per year or $219B over 15 years (discounted at a 7 percent discount rate) if all unserved households adopted broadband; including $106B in metro areas and $113B in nonmetro areas. If only a percentage of unserved households adopted broadband, the benefits would be proportionally smaller (e.g., the benefits would be 20 percent as large in the case of 20 percent adoption). Based on an estimate from a report by the Microsoft Corporation of the cost to provide broadband to all rural residents ($10B), Gallardo argued that the benefits of such an investment would greatly exceed the costs, even if only 20 percent of the unserved rural population adopted broadband.[41]

Impacts of Other Types of Infrastructure

A smaller number of peer reviewed econometric studies on impacts of other types of infrastructure were reviewed. One study investigated impacts of USDA's water and sewer infrastructure grants and loans on a variety of outcomes in rural Oklahoma communities (Janeski and Whitacre, 2014) and two other studies investigated impacts of water quality on property values in specific contexts, with potential implications for the benefits of investments in water and sewer infrastructure (Leggett and Bockstael 2000; Guignet et al. 2016). All of these studies found that investments in better water quality are associated with higher property values. Janeski and Whitacre (2014) estimated that the USDA water and sewer grants and loans increased median house values by as much as 13 percent in Oklahoma. Leggett and Bockstael (2000) estimated that improving water quality in the Chesapeake Bay to the state health standard for fecal coliform bacteria would increase the value of 494 affected waterfront properties in Anne Arundel County by up to $12 million. Guignet et al. (2016) estimated that nitrate and nitrite well-water pollution levels reduced affected residential property values in Lake County, FL by as much as 15 percent. However, limitations of these studies limit the ability to be sure of causal relationships. In addition, the generalizability of these findings to other contexts not studied is not clear.

[41] Gallardo and Rembert's (2017) estimates of the benefits of broadband adoption were based on an estimate of the average consumer surplus for broadband of $1,850 per year per subscriber provided by Rembert et al. (2017). Consumer surplus is defined as the difference between the maximum amount consumers are willing (and able) to pay to purchase a good and what they actually pay and is usually estimated for a normal private good as the area under an inverse demand curve that is greater than the price paid by consumers. Gallardo and Rembert's (2017) estimate was the average of two estimates for the consumer surplus value of broadband cited by Rembert et al. (2017) - $1,500 per year (Greenstein and McDevitt 2012) and $2,200 per year (Nevo et al. 2016). Rembert et al. (2017) argued that $1,850 per year is a conservative estimate of the average value of broadband services today, given advancements in Internet services and broadband quality and the increasing integration of the Internet in people's lives, which suggest that the average benefits of broadband are increasing over time. On the other hand, this estimate is much larger than the mean discount that Render (2016) found that MDU renters indicated would be necessary to forgo access to high speed fiber service ($80 per month, or $960 per year).

One study investigated the impacts of wind power development on growth in incomes and employment in a 12 state region of high wind potential (mainly in the Great Plains) (Brown et al. 2012). That study estimated that wind power development increased personal income by about $11,000 per installed MW of wind generation capacity and increased employment by about 0.5 jobs per MW. Those impact estimates are within the range of impacts estimated by IO and Social Accounting Matrix (SAM) models for the operating period impacts (IO and SAM models predict much larger impacts during the construction phase).

Cost-Benefit Studies

Cost-benefit analysis (CBA) originated in the 19th century, stimulated by the work of French engineer Jules Dupuit on the value of public works (Prest and Turvey 1965). In the 20th century, CBA was first adopted and institutionalized by the U.S. Army Corps of Engineers (USACE) to evaluate flood control and other civil works projects related to managing and developing water resources. The USACE began developing and using methods of measuring tangible costs and benefits of water projects in the early part of the century and was subsequently required to do so by the Flood Control Act of 1936, which authorized Federal participation in flood control projects "if the benefits to whomever they may accrue are in excess of the estimated costs".

Many of the methods of modern CBA were developed by 1950, when the U.S. Federal Inter-Agency River Basin Committee published the first set of principles, standards, and procedures for evaluating costs and benefits of river basin projects ("Green Book" or "Principles and Guidelines") (Ward 2012). The Principles and Guidelines have been updated several times since 1958, most recently in 2014 by the Council on Environmental Quality (CEQ 2014). The 2014 CEQ guidelines apply to all Federal agencies implementing projects affecting water resources, including the USACE, the Departments of the Interior (DOI), Agriculture (USDA), and Commerce (DOC), the Environmental Protection Agency (EPA), the Federal Emergency Management Agency (FEMA), and the Tennessee Valley Authority (TVA). Prior to the new Principles and Guidelines (P&G) issued in 2013 and 2014, the agencies using the P&G were limited to the USACE, the Bureau of Reclamation (in DOI), the TVA, and the Natural Resources Conservation Service (in USDA).

There is still limited experience with the conduct of CBA under the new P&G, so we do not review that experience. Under the older guidelines, a large number of CBA studies have been conducted over the years by the USACE and a requirement enforced for certain types of projects (e.g., those intended for National Economic Development) that such projects must have a benefit-cost ratio (BCR) of at least 1.0 and may be required to be substantially higher than 1.0.[42] For example, the FY 2019 USACE Civil Works Budget reports estimated BCRs for five

[42] A benefit-cost ratio (BCR) is the ratio of the value of estimated benefits of an intervention to the estimated costs. For benefits and costs that accrue over time, the present value (PV) of current and future benefits and costs are estimated, which discounts future benefits and costs by a discount rate that reflects the opportunity cost of capital (see Appendix for a more complete explanation). Methods of cost-benefit analysis (CBA) have been developed to address issues of uncertainty, nonmonetary benefits and costs such as environmental and social benefits and costs, and external (international for U.S. level analyses) benefits and costs, and others. Such methods had not been greatly used by the USACE and other agencies that were required to use CBA prior to implementation of the new P&G, however (CEQ 2014).

navigation projects and five flood and coastal storm damage control projects, with BCRs ranging from 1.10 to 4.50. The guidelines accompanying the budget document stipulate that projects funded on the basis of their economic return must have a BCR of 2.5 or higher. Other projects, such as nearly completed projects or those using non-structural means to prevent flood damage, are eligible if their BCR is at least 1.0. Other projects addressing environmental, health, or safety concerns are eligible without consideration of the BCR.

Although the BCR is important, it is not the only factor affecting whether a water resources project is proposed by USACE or selected for funding. A review of projects considered by USACE between 1973 and 1983 found that the estimated net benefits of a proposed project was a strong predictor of whether the project was proposed by USACE to the Office of Management and Budget (OMB) and an even stronger predictor of whether the project was proposed in the President's budget or approved by Congress (Hird 1991). But many other factors also influenced these decisions in Hird's analysis, including political factors, such as the numbers of congressional representatives and senators from the directly affected congressional districts and states in House or Senate leadership positions or on key committees and the number of congressional districts and states directly affected by the project. These findings reflect the complexity of decision-making regarding large infrastructure projects, as well as the importance of both economic and political factors. Hird (1991) estimated that the efficiency cost of use of other factors besides the BCR in selecting projects reduced the expected net benefits of the projects by $3.6 billion (in 1986 dollars), a 19 percent reduction in net benefits compared to the net benefits expected if the most efficient set of projects had been selected.

Based on its highway engineering models, FWHA (2015) estimated the BCRs of six alternative investment scenarios for Federal-Aid Highways, finding average BCRs (for a 20 year planning horizon) ranging from 2.28 for the highest investment scenario to 2.93 for the lowest investment scenario (FWHA 2015, p. 7-12). The marginal BCR (the lowest BCR for any project implemented under the scenario) ranged from 1.80 for the highest investment scenario to 1.95 for the lowest investment scenario. These results suggest the availability of many economically beneficial highway investments and the declining marginal BCR that occurs as the amount of highway investment increases. Alternative investment scenarios for the National Highway System and the Interstate Highway System were also modeled, showing a similar pattern of declining marginal BCRs with greater levels of investment. However, the marginal BCRs for these systems were generally lower for the scenarios considered than the marginal BCRs for the Federal-Aid Highway scenarios, ranging from 1.0 to 1.66.

Economic Impacts of Infrastructure in Developing Countries

In this section we focus on the evidence of the impact of infrastructure investments in developing countries because in the last decade most of the infrastructure spending, as a percentage of national GDP, has occurred in developing countries and therefore much important work has been done to assess infrastructure investment impacts in developing countries. (For example, as reported in the Global Infrastructure Outlook 2016, between 2007 and 2015, 20 percent of total fixed investment in Africa and 20 percent in Asia is dedicated to infrastructure, compared to nine percent in the Americas).

The aggregate-level links between physical infrastructure and the long-term production and income levels of an economy have been demonstrated in both the macroeconomic endogenous growth literature and in empirical studies. Moreover, a number of micro studies have shown that development of infrastructure is one of the indispensable components for poverty reduction, including Lipton and Ravallion (1995), Jimenez (1995), and van de Walle (1996). More broadly, the literature discusses many different dimensions of the impacts of infrastructure, including the role of rural roads, telephones, or access to electricity on poverty alleviation (Howe and Richards 1984; Binswanger, Khandker, and Rosenzweig 1993; Jacoby 1998; and Lebo and Schelling 2001, among others).

More recently, Renkow, Hallstrom, and Karanja (2004) estimated the fixed transaction costs (those not dependent on commercialized volume) that impede access to product markets by subsistence farmers in Kenya. The authors estimated that high transaction costs are equivalent to a value-added tax of approximately 15 percent, illustrating opportunities to raise producer welfare with effective infrastructure investments. Similarly, Smith et al. (2001) showed that in Uganda, the rehabilitation of roads increases labor opportunities in the service sector.

Based on an infrastructure index that includes road, rail, and telecommunications density, Limão and Venables (1999) found that infrastructure is a significant and quantitatively important determinant of bilateral trade flows. Improving destination infrastructure by one standard deviation reduces transport costs by an amount equivalent to a reduction of 6,500 km of sea travel or 1,000 km of overland travel. According to their findings, most of Africa’s poor trade performance can be attributed to poor infrastructure.

To further analyze the effects of public infrastructure on rural development and rural poverty, it is necessary to distinguish between direct and indirect effects. *Direct effects* occur when an increase in public infrastructure is accompanied by an increase in production, shifting the production frontier and marginal cost curve and increasing the rate of return for private investment in rural activities. *Indirect effects* take place as the access to public infrastructure permits a reduction in the transaction costs that small producers face when they integrate into supply and factor markets.

These lower transaction costs significantly change the structure of relative prices for the producer, stimulating changes in methods of cultivation and breeding, and possibly reallocating labor from the agricultural to the nonagricultural sector. Lanjouw, Quizon, and Sparrow (2001) found that there was a significant increase in nonagricultural activities as a consequence of better roads in Tanzania. This diversification could have arisen out of the necessity to hedge against unanticipated risks in a context where credit and insurance markets malfunction or are nonexistent (Zimmerman and Carter 2003; Ellis, Kutengule, and Nyasulu 2003). Alternately, it could be because people lack access to more profitable labor markets because of insufficient public or private assets (Reardon, Berdegué, and Escobar 2001). In either case, access to public infrastructure could play a direct or indirect role in increasing the income-generating opportunities for the poorest rural populations.

In summary, most developing country studies recognize that infrastructure investment has a strong impact on rural incomes, especially for smallholder farmers. However, this literature has not completely assessed the benefits and costs of alternative infrastructure investment options or the causality of relations that generate higher rural incomes due to better infrastructure services.[43] This lack of knowledge regarding causal relationships between investment in infrastructural services and the increase in income-generating opportunities and welfare benefits to rural populations limits the development of specific policy recommendations, resulting in policies that are directed toward a general increase in public infrastructure investment but lack information about appropriate intervention strategies for specific contexts.

Impacts of Roads

Early literature on roads attempted to establish a relationship between the stock of public infrastructure and productivity. In rural India, Antle (1984) found that roads, telecommunications infrastructure, and human capital have a positive impact on agricultural productivity. However, the study fails to account for possible reverse causality between output and capital. Also, common trends in infrastructure and output may reflect a spurious correlation that is related to the underlying time trend. Binswanger, Khandker, and Rosenzweig (1993) corrected for reverse causality by using a fixed-effects model with time-trend variables on a panel of 85 districts in 13 states in India. They found that areas with favorable agroclimatic conditions attract roads and financial institutions, ultimately resulting in higher investment and agricultural productivity. These authors were among the first to model the endogenous processes through which roads may lead to higher output. Zhang and Fan (2004) applied the generalized method of moments (GMM) in India to account for reverse causality and found that road density and irrigation have significant positive effects on agricultural total factor productivity.

The majority of impact evaluations on road paving or rural road construction have found beneficial effects across a wide array of outcome measures, including property values (Gonzalez-Navarro and Quintana-Domeque (forthcoming); Jacoby 2000), transport costs (Jacoby and Minten 2009), agricultural productivity (Dong 2000), crop prices (Khandker, Bakht, and Koolwal 2009; Casaburi, Glennerster, and Suri 2013), income and nonfarm employment (Rand 2011; Jacoby and Minten 2009; Gachassin, Najman and Raballand 2010), consumption (Jalan and Ravallion 2002; Gibson and Rozelle 2003), specialization (Qin and Zhang 2012), and access to health and education services (Valdivia 2009).

The distribution of the benefits of road improvement has tended to favor men, mainly because men's and women's gender-defined roles and responsibilities lead to different patterns of transport access, needs, and use (World Bank 2012). Women in developing countires are less likely to own motorized transportation and more likely to walk (Peters 2001). In addition, fewer women work in transport-related jobs (Duchene 2011). Despite the growing recognition that women have different transport needs, however, few studies have taken gender into account when assessing the impact of roads. Khandker, Bakht, and Koolwal (2009) examined the effect of paving feeder roads and upgrading market infrastructure in rural Bangladesh on men's and women's agricultural and nonagricultural labor supply and found that the number of days

[43] The studies carried out by Fan and Hazell (1999); Zhang and Fan (2000); Fan, Hazell, and Haque (2000); Fan, Hazell, and Thorat (2000); and Fan, Zhang, and Zhang (2002) in India and China are among the few that have looked into the relationships among investment in infrastructure, rural growth, poverty alleviation, and the role of a complementarity of investments.

worked during the previous month increased for men and decreased for women. Valdivia (2009) analyzed a road maintenance program in Peru and found that women reduced their participation as unpaid workers on the family farm in favor of outside agricultural work; on the other hand, men appeared to have better access to nonagricultural wage work.

There is mixed evidence regarding whether wealthier households are better positioned to benefit from road improvement. Lokshin and Yemtsov (2005) evaluated rehabilitation of schools, roads and bridges, and water systems in Georgia. Dividing their sample into poor and nonpoor households, they found that off-farm employment improved solely for nonpoor households; however, their results were not statistically significant. Khandker, Bakht, and Koolwal (2009) estimated the effects of feeder roads on different parts of the income distribution with a quantile regression, finding that the program increased household per capita expenditure and that these effects were larger in poor communities. However, Khandker and Koolwal (2011) estimated the long-term effects of road construction and found that its pro-poor benefits diminish over time. These studies underscore the point that impacts may take a while to emerge and may be different over time, as do the studies of Mu and van de Walle (2011) and van de Walle (2009).

Van de Walle (2009) also highlighted the fact that people do not derive utility from roads themselves, but rather through the opportunities for extra consumption that the roads facilitate. Thus, the impact of roads is dependent on other investments, infrastructure, and community characteristics. Gachassin, Najman and Raballand (2010, p. 28) strongly advocated against "investing uniformly for roads in Africa" and emphasized that roads are effective only insofar as they take into account the needs of road users. Raballand, Macchi, and Petracco (2010), drawing on case studies in Burkina Faso, Cameroon, and Uganda, challenged the assumption that the presence of high-quality roads will increase mobility and allow farmers to truck their produce to market, on the grounds that farmers may not have adequate surplus or there may be collusion in the trucking industry.

The literature acknowledges that roads may not be sufficient to ensure poverty reduction and that their impact may depend on access to other assets. Yamauchi and others (2011) examined survey data on village road quality in rural Indonesia and found that in areas that received road improvements, post-primary education significantly increased the number of days worked in nonagricultural labor, as well as nonagricultural income growth. Escobal and Torero (2005) estimated the interaction effects between traditional infrastructure, such as roads, electricity, and sewers, with "human capital–generating" public services such as education, access to healthcare, and access to communication infrastructure. Using a simulation based on survey data from Peru, they found that investment in a combination of roads, telecommunication infrastructure, and schools leads to a higher expected increase in expenditures among the poor than the sum of the individual effects of these investments alone.

Road placement is not random and factors linked to the decision to build a road are likely correlated with outcome variables. For example, if an area is selected to receive a road because of its high agricultural potential, then estimates of the impacts of the road will be upwardly biased. Furthermore, there may be unobserved individual characteristics, such as those affecting a household's decision to locate near a road, that are likely correlated with program placement.

Gonzalez-Navarro and Quintana-Domeque (forthcoming) ran an experiment to evaluate the effects of asphalting roads by randomly selecting a first-time asphalting of residential non-arterial streets in a peri-urban setting in Mexico. They found that two years post-intervention, households that had received the treatment increased their use of collateralized credit, leading to higher consumption of consumer durables and automobiles. However, the authors were unable to determine whether this increase was the result of an increase in the demand for or the supply of credit. They also found that road paving did not significantly increase consumption of nondurables, labor supplied, income, school attendance, or self-reported health.

Other evaluations of road construction employ quasi-experimental techniques to deal with endogeneity. Rand (2011) implemented a matched double-difference approach that controlled for factors influencing the placement of roads and subsequent employment growth rates to evaluate the effects of construction of tertiary roads in Nicaragua. He estimated that hours worked per week increased by between 9.5 and 12.3 in communities that received roads, relative to control communities. Escobal and Ponce (2002) used propensity score matching at the town level to evaluate a rehabilitation program in Peru and found that the program increased income through access to wage opportunities. However, consumption did not increase because the road improvement was "perceived as transitory" (Escobal and Ponce 2002, p.5). Banerjee, Duflo, and Qian (2012) used historical data from cities and counties in the People's Republic of China on transportation networks to estimate the effect of access to transportation networks on regional economic outcomes in the country over a 20-year period of rapid income growth. This paper addressed the problem of the endogenous placement of networks by exploiting the fact that these networks tend to connect historical cities, showing that proximity to transportation networks have a moderate positive causal effect on per capita GDP levels across sectors, but no effect on per capita GDP growth. Based on a simple theory, the authors argued that their results are consistent with factor mobility playing an important role in determining the economic benefits of infrastructure development.

Casaburi, Glennerster and Suri (2013) evaluated the paving of feeder roads in Sierra Leone using a regression discontinuity approach that created cutoff points with the exact methodology and data that the managing consultant used to prioritize which roads would be built first. Paving a road reduced both transport costs and market prices of rice and cassava. The authors then tested alternative theoretical models to explain their results, finding that results were most consistent with the search-cost framework developed by Mortensen (2003). In this framework, higher transportation costs stemming from being far away from a city lower the net price available to traders, which leads to fewer traders entering the market and increases traders' monopsony power. Road construction decreases these high costs, with larger effects in the most remote markets.

Instrumental variables methods have also been used to evaluate the effect of roads in the absence of an intervention or new construction. Dercon et al. (2008) used a generalized method of moments (GMM) instrumental variable estimator with household fixed effects to account for endogeneity in a growth model. Their sample consisted of survey data from 15 Ethiopian villages whose residents had access to roads of different quality. Using as instruments the area of fertile landholdings, the number of adults, and the number of livestock holdings, they found that access to all-weather roads reduces poverty by 7.6 percent and increases consumption growth by 16.3 percent.

Evaluations of highway construction have used instrumental variables for road placement based on the timing of construction or project-specific features. Gibson and Rozelle (2003) used the year that a district receives a national highway as an instrument to explain variation in travel time to roads in Papua New Guinea. They found that cutting the time to the nearest road to three hours would reduce the percentage of people living below the poverty line by 5.36 percent. Faber (2014) used least-cost path-spanning networks as an instrument to evaluate highways intended to connect provincial capitals with cities of more than 500,000 people in China. He found that the project reduced interregional trade costs, which led to a decrease in GDP growth in non-targeted rural counties. In the study, road infrastructure led to a reduction in industrial growth in non-connected areas relative to connected ones.

Finally, Torero et. al (2017) used two methods to assess the impact of major highway investment in the northern part of El Salvador (NTH). First, they took advantage of the timing of construction of different segments of the highway to implement a regression discontinuity approach. The authors assumed that households in each segment just happen to be divided by an engineering discontinuity that determines the timing in which they benefit from the NTH construction. Thus, they use the segments as a quasi-random assignment of households into treatment and control groups over time. Their second method uses a continuous treatment approach. After determining a set of relevant destinations in a given area, they estimate each household's travel time using raster analysis. In this analysis, the time each household takes to access a menu of markets is estimated considering the quality, size, and geographic characteristics of the area, rivers, bridges, etc., using GIS data and considering the cost and benefits of accessing different markets due to differences in prices and the size of the market. Using these times to market as a baseline they re-estimate the times to this menu of markets varying the proportion of the segment that was constructed each year and altering the impedance factor of the segment of the NTH that has been improved (which captures road enhancement and higher speeds of transit) – households' "optimal" travel time under these new conditions was estimated. Variation in travel time experienced by each household was used in a regression setting to determine the effect of highway construction on households' time use, income, and other welfare indicators. Their main results suggest that, if it had any impacts, the NTH in El Salvador had modest impacts on household welfare. While they find some increases in labor supply (especially for women), there were no significant changes in agricultural sales, harvests, income, or expenditure.

Impacts on Railroads

Very little evidence on experimental estimates exists on the impacts of railroads. The only paper where a clear identification strategy is implemented is the paper of Donaldson (2018). Donaldson employed a general equilibrium trade model and archival data from colonial India to investigate the impact of India's vast railroad network. The study found that railroad infrastructure reduced trade costs and interregional price gaps; increased interregional and international trade; increased real income levels;

and generated substantial gains from trade. Specifically, his estimates of outcome elasticity with respect the access to railroads is between 0.157 and 0.188.

Impacts of Rural Electrification

According to the International Energy Agency's *World Energy Outlook* (OECD/IEA 2013), more than 1.2 billion people worldwide did not have access to electricity in 2011, almost all in developing countries (accounting for 1.257 billion of the total 1.258 billion). The electrification rate in Africa south of the Sahara was no higher than 32 percent and these figures were even more alarming in rural areas – only 65.1 percent of developing country rural areas had access to electricity in 2011.

In theory, access to electricity can improve socioeconomic conditions in developing countries by improving health, education, income, and the environment (Kanagawa and Nakata 2008; Barron and Torero 2018; and Torero et.al 2017). Chaurey, Ranganathan, and Mohanty (2004) argued that a strong correlation exists between rural poverty and access to electricity because electricity is a prerequisite for productive activities. In addition to providing access to more efficient means of production, access to an electrical grid and better electric services could also result in household time savings, allowing households to work more hours by increasing their access to markets (Bernard and Torero 2011). Impact evaluations of rural electrification programs can help identify the causal link between the intervention's activities and these socioeconomic outcomes. Several impact estimations on various economic development measures have been conducted, reaching various conclusions.

Many articles focus on electrification in South Africa. The keen interest in this particular country can be explained by the rollout of grid infrastructure in South Africa and the provision of electricity to households, both of which provide a very good opportunity for impact evaluation. Davis (1998) focused on changes in rural South African households' energy consumption patterns following electrification. The author used data from a household survey and described the evolution of energy expenditures and fuel use, concluding that an energy transition did appear in rural households, but keeping the role of access to electricity in perspective. According to Davis, only weak evidence suggests that electrification accelerated the energy transition. Dinkelman (2011) used panel data, instrumental variables, and a fixed effects approach to find that electrification has a positive effect on female employment. She also found that the new infrastructure seems to increase hours of work for both men and women and that while women's wages tend to decrease if they are released from home production, men appear to earn more money under the same circumstances. Dinkelman estimated the outcome elasticity with respect to access to rural electrification to be between 0.30 and 0.35.

The literature also looks at other countries. Khandker et al. (2009) analyzed the welfare impacts of rural electrification in Vietnam, basing their analysis on panel surveys from 2002 and 2005. Their econometric framework included difference-in-difference (DD), DD with fixed-effects regression, and propensity score matching with DD estimation. The authors found significant grid electrification to have positive impacts on households' cash income, expenditures, and educational outcomes. They also stressed that a saturation point is reached after prolonged exposure to electricity.

Focusing on India, Bhattacharyya claimed that "rural electrification alone is unlikely to resolve the energy access problem because of low penetration of electricity in the energy mix of the poor" (2006, p. 387). More recently, however, van de Walle et al. (2013) found that rural electrification has positive effects on consumption and earnings, as well as on schooling for girls. Bernard (2012) explored the impacts of rural electrification projects in Africa south of the Sahara and gave a very interesting review of trends in electrification programs over the past 30 years in the region. While the author argued in favor of the importance of rural electrification, he also pointed out that its impacts on development components, such as health or education, are "largely undocumented" (Bernard 2012, p.33).

Torero et.al (2017) and Barron and Torero (2018), using a similar methodology of random encouragement as Bernard and Torero (2011) and a fixed-effects estimation, that exploits the longitudinal nature of the data and uses within-household variation in electrification status to estimate the effects of rural electrification for rural northern El Salvador. The authors found an increase in adoption of electricity by reducing connection costs, ameliorating credit constraints, providing incentives not to procrastinate in the decision to connect to the grid, and perhaps even increasing awareness about the benefits of electrification. Second, spillover effects seem to play an important role in adoption and their effects do not reduce with time. An additional connection within 100 meters of a household increases the probability of that household connecting formally to the grid by 10 percentage points, roughly the same increase generated by vouchers. Third, the authors found that electrification increases investment in education among school-age children and participation in income-generating activities among adult women. The increases in educational investment materialize through an increase in participation in educational activities. Electrification increases the probability of studying at home by 54 percent and of performing other school-related activities (time in school, time commuting between school and home) by 84 percent. Fourth, a robust result was found that adult females increase their participation in income-generating activities as a result of electrification. Electrification increased the probability of operating a home business by 12 percent. This is more than a 150 percent increase compared to the control group. Fifth, the experimental estimates of the effect of an electrical connection on income suggest that electrification increased annual household income by around $1,600 per year. This is the first time not only short-term effects, but medium-term effects, have been identified. Finally, the evidence presented also shows that electrification leads to reductions in indoor air pollution, which reduced the incidence of acute respiratory infections among children and lowered exposure to pollutants among adult household members (Barron and Torero 2017).

Impacts of Information and Communication Technologies

An increasing body of evidence highlights the potential for information and communication technologies (ICTs) to improve the lives of the poor. ICTs can make poor populations more resilient in several ways. First, access to technology can increase the amount, timeliness, and quality of information available to the poor. Preliminary research suggests that this, in turn, can translate into better job opportunities (as the poor establish better contacts) and higher crop yields (as they get access to timelier and better-quality information on products and inputs, environmental conditions, and market conditions) (Klonner and Nolen 2010). Second, ICTs may promote learning, which itself can enhance technology adoption among farmers (Bandiera and Rasul 2006). Last, although no evidence is available as yet, it is conceivable that improved access to health and nutritional information through ICTs can help reduce the prevalence of hunger among the poor.

There are many reasons to believe that ICTs may have a large impact on agricultural markets. ICTs can allow different market agents to communicate with each other more efficiently, thus enhancing information flows. This can be critically important for rural areas in developing countries, where inadequate infrastructure tends to make markets less integrated than elsewhere. Mobile phones are particularly good at spreading information. As of October 2013, 98 mobile phone projects were being implemented in the agricultural sector of developing countries, as compiled by the 2013 Global System for Mobile Communications Mobile and Development Intelligence project.[44] Delivery is done mainly through short message service (SMS or text message), although voice messages, interactive voice response systems, and mobile applications (apps) are also used. Most projects deliver information regarding market prices (48 percent) and agricultural extension (39 percent), combined with weather advisory information in a number of important cases.

Increased information in agricultural markets can improve market efficiency.[45] With increased access to mobile phones, farmers can better plan how much to plant each season and how much and what type of investments could be profitable based on demand and supply fundamentals.[46] They can also gather information from extended networks and cooperatives regarding market conditions and quality standards in higher-end markets.

There is also anecdotal evidence suggesting that ICTs might affect transportation costs for both inputs and crops. A farmer in India stated, “I was in process to transport my produce [approximately 1,000 boxes in two trucks] to Delhi when I got an SMS through RML [Reuters Market Light, a mobile phone-based information service] that the freight rate from Kotgarh to Delhi is Rs [rupees] 41.07 per box. I showed this message to the truck operator, who till then was citing a rate of Rs 44 per box. Following this I was able to settle the transporting deal at Rs 41.07, finally saving around 3,000 rupees” (Murali 2011).

ICTs can also be used to reduce price variability. In a context of limited information – and thus limited arbitrage – prices tend to vary based on the current local supply. However, as information flows improve, more opportunities for arbitrage emerge, effectively limiting the influence of local fluctuations and more closely relating market prices to less-volatile aggregate supply. Finally, improved information can teach households about more profitable crops or previously unknown agricultural techniques, thus potentially influencing production patterns in the long term.

Though far from conclusive or uniform, some studies have provided a range of estimates for some of the hypothesized effects of ICT information flows on smallholders’ sales prices and

[44] See www.mobiledevelopmentintelligence.com.
[45] For a wider list of gains see Jensen (2010).
[46] See Abraham (2007), Jensen (2007), Aker (2008a, 2008b, 2010), and Muto and Yamano (2009).

profits. Investigating the impact of price dissemination through radio, for example, Svensson and Yanagizawa (2009) found large increases (around 15 percent) in farmgate prices for maize in Uganda. Preliminary research in Peru and the Philippines suggests similarly large effects.[47] A more thorough list of such studies is presented in Table 6.

ICTs can also play a role in reducing the three main constraints faced by traditional extension services in developing countries (Cole and Fernando 2012). First, poor infrastructure makes it difficult and costly to visit remote areas. Second, traditional extension programs usually provide only one-time information to farmers; the lack of follow-up information and feedback can restrict the information's long-term benefits. Finally, traditional extension is plagued by principal-agent and institutional problems, including a lack of accountability among extension agents. ICTs can overcome these problems by reducing the cost of extension visits, enabling more frequent two-way communication between farmers and agents, and improving the accountability of agents. By increasing communication among farmers, extension agents, and research centers, ICTs can facilitate coordination of relevant content among all three groups.

Our analysis of the existing research takes into account (1) the level of mobile phone penetration in the country when the interventions in the studies detailed in Table 6 were implemented, (2) the specific characteristics of the commodity in terms of its market value, (3) the specificity or quality of the content being provided to farmers (that is, whether price information is general or specific to the commodity and the markets relevant for the farmer), and (4) the statistical significance of the interventions' impacts. The literature is not conclusive given the small number of existing studies and the preliminary nature of several of them; however, several patterns suggest hypotheses to be further researched.

First, we find that the lower the mobile phone penetration at the time of implementation, the more significant the intervention's impact on farmers, especially for medium- and high-value commodities. This result can be partially explained by the fact that low penetration can be directly related to a significant difference in knowledge about prices (or information asymmetry) among farmers; as ICT penetration increases, all farmers might be better able to access the same price information, which has the potential to significantly impact farmers' marketing decisions (such as whether to invest in medium- and high-value crops). Thus, an intervention that increases ICT penetration has the potential to significantly affect agricultural markets.

Second, as penetration, and therefore access to information increases, the specific content of the information (that is, the usefulness of the information to the farmer) comes to matter significantly. We find that the impact of information seems significant only when that information provides specific price information regarding high-value commodities. Fafchamps and Minten (2012) assessed the impact of information in regions of India where mobile phone penetration was higher than 40 percent, but where only generic information was provided; they found no significant results stemming from that information. On the other hand, other studies have shown significant results when the information provided was customized to the specific

[47]For Peru, see Chong, Galdo, and Torero (2005) and Beuermann, McKelvey, and Vakis (2012). For the Philippines, see Labonne and Chase (2009).

high-value commodities and varieties produced by the farmers studied.[48] Nakasone (2013) also suggested that increased information, no matter how specific, for low-value and less perishable commodities is not significant.

Impacts on Irrigation

As a notable example of impact evaluation of infrastructure using a quasi-experimental method, Duflo and Pande (2007) performed impact evaluation of dams in India on poverty reduction, using river gradient variables as instrumental variables for placements of dams for engineering reasons. Using district-level data from India, they found that in districts located downstream from a dam, agricultural production increases, rural poverty and vulnerability to rainfall shocks decline, agricultural production shows an insignificant increase, and poverty increases in the district where the dam is located, but its volatility increases. These results suggest that neither markets nor state institutions have alleviated the adverse distributional impacts of dam construction

Impacts of Water and Sanitation

The role of clean water and adequate sanitation in development has long been recognized; from the effects on child mortality to school attendance and work productivity gains, water and sanitation can improve the well-being of people throughout their life span (WHO and UNICEF 2005). Lack of access to water and sanitation not only exposes people to infectious waterborne diseases that decrease the probability of survival at both young and old ages, but it also imposes a burden on their economic life by increasing time spent out of productive activities due to illness, time spent fetching water, and water storage and treatment costs.

The health impacts of water and sanitation programs have been studied frequently (see Fewtrell et al. 2005; Pattanayak et al. 2008, 2010; Newman et al. 2002; Galiani, Gertler, and Schargrodsky 2005; Galiani, Gonzalez-Rozada, and Schargrodsky 2009; Devoto et al. 2012; Jalan and Ravallion 2003; and Gamper-Rabindran, Khan, and Timmins 2010). However, very few studies have measured other important outcomes, such as changes in households' costs of collecting, storing, and treating water or income losses due to waterborne and water-washed illnesses (Pattanayak et al. 2008). Furthermore, there are few rigorous impact evaluations that have measured water and sanitation interventions' education, gender, and poverty reduction impacts. Bosch et al. (2000) categorized water and sanitation impacts into four groups: health improvement, education, gender and social inclusion, and income and consumption.

On the health side, impact evaluations have focused on child mortality, given that diarrheal disease is the second leading cause of death in children under five years old and a leading cause of malnutrition (WHO 2013). Newman et al. (2002) evaluated small water and sanitation projects in Bolivia and found that community-level training was needed to impact water quality. They also found effects on infant mortality, bringing forward the importance of coupling "hardware" interventions with "software" interventions to achieve development goals. Galiani, Gertler, and Schargrodsky (2005) found that child mortality in Argentina fell by 8 percent due to increased access to water (through privatization), with the poorest benefiting the most. Kremer et

[48] See Nakasone (2013), Courtois and Subervie(2013), and Nyarko et.al. (2013).

al. (2011) found positive effects on child health from a randomized experiment in Kenya that protected water sources, while Devoto et al. (2012) found effects on child health from an intervention that provided tap connections to an urban sample in Morocco. These two studies highlight the importance of distinguishing between increases in water quantity and quality. Kremer et al. (2011) evaluated the impact of an increase in the water quality available to the treatment group by protecting the water sources, while Devoto et al. (2012) evaluated the impact of increasing the quantity of water available to households in an urban area; no changes in quality are expected in this latter setting, since these households were already obtaining water from this network before the intervention.

Other studies have explored the link between water and child health. Jalan and Ravallion (2003) found that piped water lowered the prevalence and duration of diarrhea among children under five in rural India. On the other side of the spectrum, Klasen et al. (2011) evaluated the impact of increased access to piped water supply in Yemen and found that frequent water rationing worsens health outcomes, likely due to pollution in the network. Fan and Mahal (2011) found non-robust positive effects of water and sanitation on dysentery and significant reductions in diarrhea among children under five due to hygienic practices (hand washing).

One important issue to keep in mind is the complementarities of water and sanitation projects. For example, Esrey (1996) found that improved water quality can improve child health if sanitation is also provided. Some rigorous evaluations, like those of Pattanayak et al. 2008, 2009), found that a community demand–driven water and sanitation intervention in India had positive effects on the level of access to piped water and sanitary services, but no discernible health or education impacts.

The evidence on the effects of water and sanitation on income and consumption is limited,[49] as is evidence of the effects of water and sanitation on gender and social inclusion. Impacts on gender and social inclusion refer to the extent that minorities, the poor, or other vulnerable populations benefit from the water and sanitation interventions. The effects might be larger for some of these populations because of nonlinear treatment effects; for example, if women disproportionately participate in fetching water, they would see larger benefits from a project that provides tap water (Koolwal and van de Walle 2013). On the other hand, if the cost of connecting to a tap is high, poorer households might not be able to afford the connection and thus will not benefit from the project even if they were the targeted population (Zwane and Kremer 2007).

Few studies quantify the impact of water access on productivity in either agriculture or the labor market and, to date, no discernible effects have been found. For example, Devoto et al. (2012) found no changes in the time allocated to productive activities and Koolwal and van de Walle (2013) did not find that access to water leads to more off-farm work for women.

[49]Some studies have found limited effects on these outcomes (Chase 2002; Lokshin and Yemtsov 2005; Kremer et al. 2011; Pattanayak et al. 2008, 2010; and Devoto et al. 2012).

Conclusions

Summary of Findings

A large literature on the productivity impacts of infrastructure investments estimates the output elasticity of public capital – the percentage increase in the value of production resulting from a 1 percent increase in the value of the public capital stock. A review of 28 published studies for the United States found a wide range of estimates of this parameter – ranging from -0.49 to +0.56 – with a mean value of 0.12. The range of marginal returns to public capital implied by these estimates is even larger because some estimates apply to different types of public capital, with different output/capital ratios. In general, larger marginal returns have been found for investments in water and sewer systems than for highways or for other state and local infrastructure, larger marginal returns have been found in national level studies than in state level studies; among state level studies, smaller marginal returns were found in studies that controlled for unobserved fixed factors using fixed effects or first difference estimation. It is not clear whether the larger returns in national level studies are due to positive spillover effects of infrastructure across state boundaries, as argued by some authors, though the studies that investigated spillover effects cast doubt on this. An alternative possibility that has not been explored is that unmeasured differences across states in the amenity value of infrastructure, which may tend to reduce wages and incomes in states with more infrastructure stocks, may account for some of the difference between the apparent returns in national and state level studies.

A few U.S. studies estimated the impacts of infrastructure on the costs or profits of businesses and found significant cost reducing or profit increasing impacts. These studies imply positive marginal returns to public capital, though not always or everywhere sufficient to cover the user cost of capital.

Production, cost, and profit impact studies do not account for the amenity benefits of infrastructure. Two studies estimated these benefits for cities using interurban variations in wages and housing values. Haughwout (2002) estimated the benefits of infrastructure in 33 U.S. cities and found that amenities account for most of the value, estimated to be in the range of $1.4B to $2.8B (in 1990 dollars), substantially less than the cost of the infrastructure ($4.6B), implying a benefit-to-cost ratio (BCR) of less than 1.0. A recent study by Albouy and Farahani (2017) updated and extended Haughwout’s approach, estimating that the BCR of infrastructure was more than twice the range found by Haughwout (2002) and even larger BCRs when imperfect mobility of workers and impacts of infrastructure on productivity in the housing production sector are considered.

The U.S. Army Corps of Engineers estimates the benefits and costs expected from water resources infrastructure investments – with BCRs of approved projects usually greater than 1.0 and in many cases greater than 3.0. The benefits and costs of potential highway investments estimated by the Federal Highway Administration show BCRs greater than 1.0 for a wide range of scenarios. Investments in Federal Aid Highways appear to offer greater BCRs than investments in the National Highway System or in Interstate highways.

A review of econometric studies of impacts of particular types of infrastructure in the United States – focusing on broadband, water and sewer systems, and electricity systems – found many studies on impacts of broadband but few on the other types of infrastructure considered. Several broadband studies found positive impacts on labor market outcomes, though the impacts are highly context dependent. One study of broadband impacts on housing sales values found that broadband access increases house values by up to 7 percent, depending on the available speed.

Studies of impacts of railroads, roads, rural electrification, dams, ICTs, and water and sanitation in developing countries were also reviewed to bring some evidence on the proper identification of the causal effectiveness of infrastructure in increasing incomes and reducing poverty and the broader complementarities between market, state, and community mechanisms. Railroads and road investments in developing countries were found to have significant effects in many cases on a range of outcomes, including agricultural productivity, transportation costs, commodity prices, nonfarm economic activity, employment, rural household incomes, poverty, property values, access to health and education services, and others. Road development has tended to benefit men more than women and some studies have found that the impacts of road development are greater for poor people. Rural electrification and ICT investments (Nakasone and Torero, 2016) are also found to have positive impacts on outcomes reflecting rural people's economic activity, income, and welfare in many studies.

Rural electrification in developing countries has been found to significantly increase investment in education among school-age children and participation in income-generating activities among adult women. In addition to providing access to more efficient means of production, access to an electrical grid and better electric services also result in household time savings, allowing households to work more hours by increasing their access to markets (Bernard and Torero 2011). Moreover, adult females increase their participation in income-generating activities because electrification increased the probability of operating a home business (Torero et.al 2017).

The health effects of investments in water and sanitation in developing countries are well documented. The evidence on effects of water and sanitation on income and consumption is limited and very few studies have looked at the effect on productivity or the labor market. So far, no discernible effects on these outcomes have been found.

Beneficial impacts of infrastructure are not always found across the nearly 200 developing country papers reviewed. *Ex ante* studies of costs and benefits of infrastructure investments are often required by donor agencies, but *ex post* cost-benefit studies or impact evaluations conceived from the design phase of the project are relatively rare. This is in part because of the nature of infrastructure projects, which can make it extremely difficult to form an appropriate counterfactual, given that sometimes the project is just for a single new port, a new airport, or a new powerplant in a small country.

Research Implications

One of the key lessons of this review for researchers is the need to estimate rates of return to infrastructure, when feasible with the data used. Despite the large number of published studies on the productivity impacts of public capital and despite the fact that it requires only a straightforward calculation to estimate the marginal rate of return to public capital, given the estimated output elasticity and the data used to estimate it, only a few of the studies reviewed estimated the marginal rate of return of public capital. Having this additional estimate would make the literature more useful to policy discussions (as policymakers may have less use for elasticity estimates, but could possibly make good use of rate of return estimates), and could also improve researchers' insights into the issue and future research designs. For example, as noted in the discussion related to the marginal returns estimated in Table 5, even a quite small elasticity estimate can imply a large marginal rate of return if the output/capital ratio is large. Noticing this might encourage researchers to question their econometric assumptions and test alternative specifications that don't yield such large and difficult to believe estimates of marginal returns for some types of capital. If such large marginal returns are found to be robust to the econometric specification, they may be found to be credible, which would call for future research into why such large rates of return exist.

A second lesson from this review is the overemphasis in the literature on the productivity impacts of infrastructure. Following Aschauer's (1989) seminal work, a cottage industry of studies investigating the productivity impacts of infrastructure in different countries and regions arose, with 145 such studies included in the recent meta-analysis by Núñez-Serrano and Velázquez (2017). Yet Haughwout (2002) demonstrated that the amenity value of infrastructure investments in U.S. cities may greatly exceed their productivity value. The same may be true of infrastructure investments in rural areas; particularly investments in water and sewer systems or in broadband that likely have substantial amenity values. The amenity value of broadband investments could help explain some of the insignificant or mixed findings in the literature on impacts of broadband on local wages, per capita earnings, and income. Future research on impacts of broadband and other types of infrastructure could usefully apply this insight. More generally, more research applying the spatial equilibrium framework of Roback (1982), or variants of it such as developed by Albouy and Farahani (2017), to value different types of infrastructure and infrastructure in different contexts (e.g., rural vs. urban) could be valuable.

A third lesson is that even the best studies of impacts of infrastructure do not necessarily yield insights about the aggregate benefits, let alone the costs or rate of return to infrastructure investments. Impacts on local employment or wages do not obviously translate into measures of economic benefits, especially because positive impacts in some localities may simply reflect a transfer of employment and wages from locations without adequate infrastructure to places with better infrastructure. Such transfers, while important to understand as a distributional impact of infrastructure, do not represent aggregate benefits to the national economy as a whole, or necessarily even to subnational regions such as rural regions. Further research is needed to identify how much of the impacts of infrastructure investments represent aggregate benefits vs. distributional transfers among regions and populations. Beyond this, data on the economic, environmental, and social costs of infrastructure investments are needed if a full assessment of the benefits, costs, and economic and social rate of return is to be possible.

A fourth lesson from the econometric impacts literature is that the estimated impacts of infrastructure are quite sensitive to the data and analytical methods used, the temporal and spatial context of the study, the factors controlled for and interactions investigated, and the outcomes investigated. In part, this reflects the context-dependence and heterogeneity of infrastructure impacts, which several studies of broadband impacts in the United States and numerous studies in developing countries have demonstrated. Clearly, there is never going to be one simple answer to the question of what the impacts of or the rate of return to infrastructure investment are. More research illuminating on what the impacts and rates of return depend on will make infrastructure research more useful to policy makers and program managers trying to decide what, where, and when infrastructure investments can be expected to be most beneficial.

A final lesson is the need for peer-reviewed research predicting *ex ante* the benefits, costs, and rate of return to alternative infrastructure investments using rigorous methods and using *ex post* data and econometric methods to validate and improve such predictions. One of the most commonly used predictive modeling approaches in the United States is input-output modeling, which, as is argued in this paper, does not actually estimate the full impacts or benefits of infrastructure investments, such as improvements in productivity, reduced transaction costs, or increased consumer amenities and welfare. The popularity of such models may be due to their ready availability and ease of use, rather than their suitability to the problem of evaluating potential infrastructure investments. Other modeling approaches more appropriate to the task – such as using partial or general equilibrium models to predict impacts of infrastructure investments on producer and consumer surplus or other welfare measures – can and have been used to estimate benefits of some types of infrastructure investments. Beyond demonstrating how to conduct rigorous *ex ante* CBAs, future research could also seek to demonstrate how the results of *ex post* impact studies can help to validate and improve assumptions made in *ex ante* CBAs. The Millennium Challenge Corporation appears to be one of the few organizations attempting to link *ex post* impact studies with *ex post* or *ex ante* BCAs.

Overall, despite the existence of a large number of published studies on the impacts of infrastructure investment in the United States and elsewhere, much remains to be learned about how the impacts of such investments depend on the context; how to use such impact estimates in estimating the benefits, costs, and rate of return to infrastructure investments; and how research can contribute to a learning cycle in which the knowledge gained from rigorous research is incorporated into appraisals of proposed new infrastructure projects and evaluation of ongoing or completed projects.

Table 1. Estimated employment multiplier impacts of infrastructure spending

Type of Infrastructure	Jobs per $1 billion	
	Direct & Indirect	Total (with induced impacts)
Energy	**11705**	**16763**
Gas	15976	21888
Electricity generation, transmission, distribution	9819	14515
Solar	10951	15767
Wind	10076	14880
Transportation	**13829**	**18930**
Average for roads and bridges	13714	18894
Roads and bridges: new	12638	17472
Roads and bridges: repair	14790	20317
Rail	9932	14747
Mass transit	17784	22849
Aviation	14002	19266
Inland waterways/levees	17416	23784
School buildings	**14029**	**19262**
New institutional construction	14291	19637
Repair of non-residential buildings	13768	18886
Water	**14342**	**19769**
Dams	17416	23784
Drinking water	12805	17761
Waste water	12805	17761

Source: Heintz et al (2009)

Table 2. Estimated economic impacts by type of infrastructure project

Type of Infrastructure	Invest-ment amount ($B)	Direct employ-ment impact (# jobs)	Total employ-ment impact (# jobs)	Total output impact ($B)	Total jobs/ Investment (jobs/$B)	Total output/ Investment ($B/$B)
Highway and transit system	225.0	2106914	6189480	775.4	27509	3.45
Broadband infrastructure	55.0	293736	1048064	158.3	19056	2.88
Onshore exploration &devt./offshore drilling	46.5	194844	896185	145.0	19273	3.12
Drinking water and wastewater infrastructure	30.0	280922	825264	103.4	27509	3.45
Smart grid	24.0	219578	649627	82.0	27068	3.42
Nuclear energy	15.0	139145	397271	48.7	26485	3.25
Renewables (solar, wind, biofuels)	14.5	115874	337558	44.3	23280	3.06
NextGen air traffic control	10.4	30631	181921	32.1	17492	3.09
Inland waterways	2.6	32951	67100	8.1	25808	3.12
Clean coal technology	2.6	24018	66127	7.9	25932	3.10

Source: Based on DeVol and Wong (2010)

Table 3. Studies that estimated the output elasticity of public capital in the United States

Author(s)	Units of analysis	Regression method (preferred spec)	Output elasticity of public capital			
			N	Min	Max	Mean
Aschauer (1989a)	National	OLS - levels	20	0.24	0.56	0.379
Ram and Ramsey (1989)	National	OLS - levels	2	0.191	0.24	0.216
Munnell (1990a)	National	OLS - levels	10	0.21	0.49	0.367
Munnell (1990b)	States	OLS - levels	9	0.06	0.36	0.128
Eisner (1991)	States	OLS - state fixed effects	19	−0.491	0.383	0.048
Tatom (1991)	National	OLS - first diff.	2	−0.075	0.042	−0.017
Garcia- Milà and McGuire (1992)	States	OLS – levels	2	0.044	0.045	0.045
Finn (1993)	National	GMM – levels	1	0.158	0.158	0.158
Munnell (1993)	States	OLS – levels	19	−0.004	0.38	0.111
Eisner (1994)	National	OLS – levels	1	0.27	0.27	0.27
Evans and Karras (1994a)	States	AR1 - first diff.	12	−0.110	0.102	−0.023
Holtz-Eakin (1994)	States	Dynamic GMM - first diff.	15	−0.130	0.203	−0.027
Andrews and Swanson (1995)	States	OLS - state random effects	4	0.01	0.13	0.073
Baltagi and Pinnoi (1995)	States	IV - first diff.	18	−0.080	0.39	0.071
Holtz-Eakin and Schwartz (1995a)	States	OLS - first diff.	5	−0.038	0.112	0.039
Holtz-Eakin and Schwartz (1995b)	States	ML - long differences	14	−0.022	0.054	0.009
Sturm and De Haan (1995)	National	OLS - first differences	6	0.26	0.71	0.488
Garcia-Milà, et al. (1996)	States	OLS - first diff. with fixed effects	6	−0.058	0.37	0.088
Holtz-Eakin and Lovely (1996)	States	OLS - fixed effects	2	−0.144	−0.132	−0.138
Crowder and Himarios (1997)	National	OLS - removed stochastic trend	12	0.065	0.382	0.248
Kelejian and Robinson (1997)	States	GMM - spatial model with state fixed effects	26	−0.193	0.146	−0.066
Vijverberg, et al. (1997)	National	2SLS AR1 model	4	0.465	0.55	0.496
Boarnet (1998)	California counties	GLS long differences with spatial lags	6	0.065	0.3	0.225
Erenburg (1998)	National	OLS model with short and long run effects	5	0.24	0.5	0.342
Nourzad (1998)	National		1	0.34	0.34	0.34

Author(s)	Units of analysis	Regression method (preferred spec)	Output elasticity of public capital			
			N	Min	Max	Mean
Delorme, et al. (1999)	National	OLS - first differences	3	0.176	0.276	0.222
Berechman, et al. (2006)	States, plus counties and municipalities in NY/NJ metro area	OLS - basic model, state level analysis	9	−0.009	0.047	0.027
Cohen (2010)	States	2SLS spatial lag model	2	0.062	0.106	0.084

Note: All studies and their reported minimum, maximum, and mean elasticities based on Bom and Ligthart (2014)

Table 4. Estimated elasticities of output with respect to different types of public capital

Elasticity of output with respect to	National		State	
	N	Mean (std. error)	N	Mean (std. error)
Total nondefense public capital	9	0.265 (0.039)	8	0.033 (0.031)
- With state fixed effects or first differences			5	-0.024 (0.013)
- Without state fixed effects or first differences			3	0.139 (0.008)
Core public capital (highways, mass transit, airports, electrical and gas facilities, water and sewer)	2	0.365 (0.125)	0	
Highway capital	1	0.158	10	0.006 (0.017)
- With state fixed effects or first differences			6	-0.018 (0.024)
- Without state fixed effects or first differences			4	0.046 (0.005)
Water and sewer capital	0		7	0.075 (0.032)
- With state fixed effects or first differences			5	0.071 (0.043)
- Without state fixed effects or first differences			2	0.113 (0.005)
Other state and capital (buildings, other structures, equipment)	0		7	-0.036 (0.029)
- With state fixed effects or first differences			5	-0.049 (0.043)
- Without state fixed effects or first differences			2	0.006 (0.003)

Source: ERS analysis, based on studies listed in Table 3

Table 5. Estimated marginal returns to different types of public capital based on the mean output elasticities in Table 4

Marginal return to	National	State
Total nondefense public capital	0.374	0.053
- With state fixed effects or first differences		-0.038
- Without state fixed effects or first differences		0.223
Core public capital (highways, mass transit, airports, electrical and gas facilities, water and sewer)	0.971	
Highway capital	0.675	0.026
- With state fixed effects or first differences		-0.079
- Without state fixed effects or first differences		0.203
Water and sewer capital		0.881
- With state fixed effects or first differences		0.834
- Without state fixed effects or first differences		1.328
Other state and capital (buildings, other structures, equipment)		-0.668
- With state fixed effects or first differences		-0.909
- Without state fixed effects or first differences		0.111

Source: ERS analysis, based on mean output elasticities in Table 2 and Bureau of Economic Analysis, Fixed Reproducible Tangible Wealth, 1929 – 1995 (https://www.bea.gov/scb/account_articles/national/0597niw/table1.htm)

Notes: Estimates based on the formula: Marginal return = output elasticity x (ratio of private sector output to nondefense public capital stock)/(ratio of capital stock considered to nondefense public capital stock in 1985). Ratio of private sector output to nondefense public capital stock assumed to be 1.41, based on the midrange of estimates of marginal returns reported in Aschauer (1990). Ratio of core public capital stock to nondefense public capital stock in 1985 = 0.53; ratio of total highway capital stock to nondefense public capital stock in 1985 = 0.33; ratio of state and local highway stock to nondefense public capital stock in 1985 = 0.32; ratio of water and sewer capital stock to nondefense public capital stock in 1985 = 0.12, ratio of other state and local capital stock to nondefense public capital stock in 1985 = 0.076.

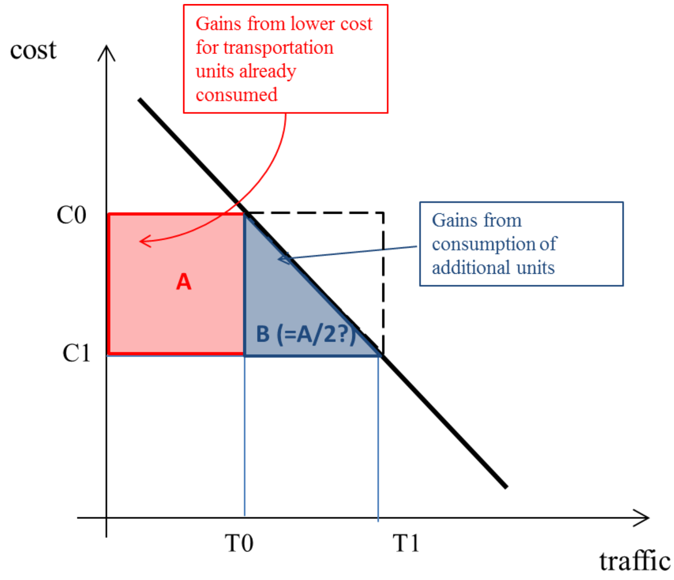
Table 6. Studies on the impacts of ICTs

Technology	Location/product	Effect (and outcome)	Study
Latin America			
Public pay phones	Peru, various crops	+16% on prices	Beuermann(2011)
Public phones	Peru, various enterprises	+13% on farm income	Chong, Galdo, and Torero (2005)
Cell Phones	Peru, various crops	+11% household consumption	Beuermann, McKelvey, and Vakis (2012)
Cell phones	Peru, various crops	+11 to 14% on average prices	Nakasone (2016)
SMS	Colombia, various crops	No significant effect	Camacho and Conover (2011)
Africa			
Radio	Uganda, maize	+15% on prices	Svensson and Yanagizawa (2009)
Mobile phone coverage	Uganda, banana and maize	Somewhat positive relationship, but depends on distance to district center No effect for maize	Muto and Yamano (2009)
Grameen /MTN village phones	Rwanda, various products	No significant effect	Futch and McIntosh (2009)
Cell phones	Niger, cowpeas	No significant effect	Aker and Fafchamps (2010)
SMS	Ghana, maize and groundnuts	Price increases for maize (12.7%) and groundnuts (9.7%)	Curtois and Subervie (2015)
SMS	Ghana, various crops	7% price increase for yams. No effect for maize, cassava and gari	Nyarko et al. (2015)
Asia			
Cell phones	Philippines, various crops	+11 to 17% on the growth rate of per capita consumption	Labonne and Chase (2009)

Technology	**Location/product**	**Effect (and outcome)**	**Study**
Cell phones	Kerala, India, fisheries	+8% in fishers' profits	Jensen (2010)
eChoupal	Madhya Pradesh, India, soybeans	+1 to 3% (average: 1.6%) on prices	Goyal (2010)
SMS	West Bengal, India, potatoes	No significant effect	Mitra, Mookherjee, Torero, and Visara (2011, 2018)
SMS	Maharashtra, India, various products	No significant effect	Fafchamps and Minten (2012)

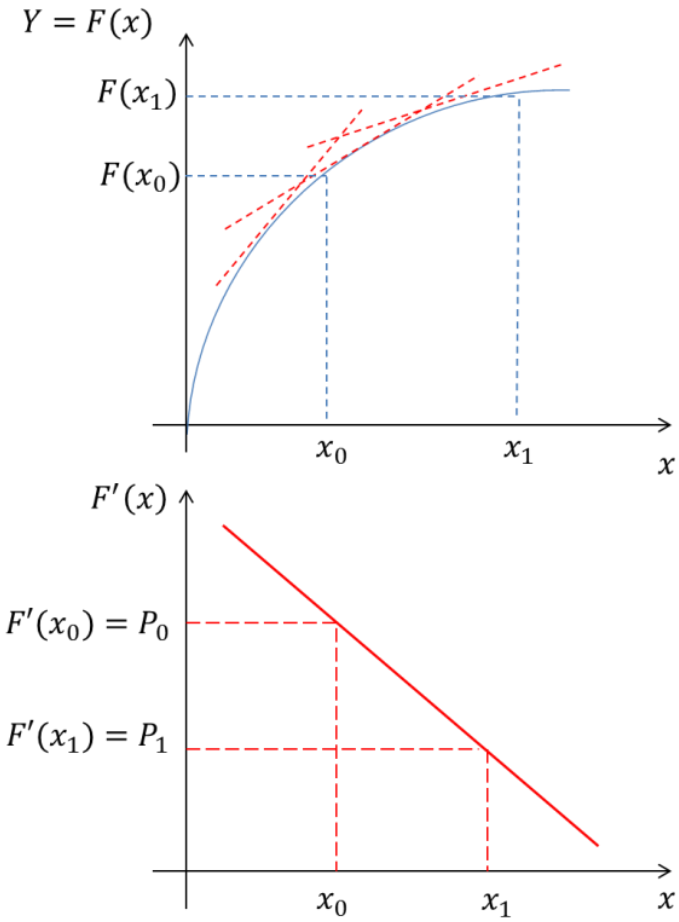
Source: Nakasone and Torero (2017)

Figure 1. The Surplus Approach



Source: Authors

Figure 2 The Surplus and Production Approaches



Source: Authors

References

Abraham, R. 2007. “Mobile Phones and Economic Development: Evidence from the Fishing Industry in India.” *Information Technologies and International Development* 4 (1): 5–17.

Africon. 2008. *Unit Costs of Infrastructure Projects in Sub-Saharan Africa*. Africa Infrastructure Country Diagnostic Background Paper. Washington, DC: World Bank.

Aker, J.C. 2008a. *“Can You Hear Me Now?” How Cell Phones Are Transforming Markets in Sub-Saharan Africa.* Washington, DC: Center for Global Development.

———. 2008b. *Does Digital Divide or Provide? The Impact of Cell Phones on Grain Markets in Niger*. Working Paper 154. Washington, DC: Center for Global Development.

———. 2010. “Information from Markets Near and Far: Mobile Phones and Agricultural Markets in Niger.” *American Economic Journal: Applied Economics* 2 (3): 46–59.

Aker, J.C. and Fafchamps, M. 2010. How does Mobile Phones Coverage Affect Farm-Gate Prices? Evidence from West Africa. Presented at the Allied Social Science Association Meeting 2011.

———. 1984. “Human Capital, Infrastructure, and the Productivity of Indian Rice Farmers.” *Journal of Development Economics* 14:163–181.

Albouy, D. and Farahani, A. 2017. Valuing Public Goods More Generally: The Case of Infrastructure. Upjohn Institute working paper; 17-272. Available at SSRN: https://ssrn.com/abstract=2981752 or http://dx.doi.org/10.2139/ssrn.2981752

Aschauer, D.A. 1989. Is public expenditure productive? *Journal of Monetary Economics*, *23*(2), pp.177-200.

Aschauer, D.A. 1990. Public investment and private sector growth. *Economic Policy Institute, Washington, DC.*

Atasoy, H. 2013. The effects of broadband internet expansion on labor market outcomes. *Industrial & Labor Relations Review*, *66*(2), pp.315-345.

Bagi, F.S. 2002. Economic impact of water/sewer facilities on rural and urban communities. *Rural America*, *17*(4), pp.44-49.

Bandiera, O. and Rasul, I. 2006. “Social Networks and Technology Adoption in Northern Mozambique.” *Economic Journal* 116 (514): 869–902.

Barron, M. and Torero, M. 2017. "Household electrification and indoor air pollution," Journal of Environmental Economics and Management, Elsevier, vol. 86(C), pages 81-92.

Bayes, A. 2001. Infrastructure and rural development: Insights from a Grameen Bank village phone initiative in Bangladesh. Agricultural Economics 25: 261–272.

Banerjee, A., Duflo, E., and Qian, N. 2012. On the Road: Access to Transportation Infrastructure and Economic Growth in China. NBER Working Papers 17897. Cambridge, MA: National Bureau of Economic Research.

Berkman, M., Sunding, D., and Tran, M. 2010. Technical memorandum on recommended methodology for estimating employment impacts of water related infrastructure projects. Berkeley Economic Consulting, June 8. Available at: www.cuwa.org/pubs/CUWA_Method_Memo_Final.pdf (accessed on June 1, 2017).

Bernard, T. 2012. "Impact Analysis of Rural Electrification Projects in Sub-Saharan Africa." *World Bank Research Observer* 27 (1): 33–51.

———. 2011. *Randomizing the "Last Mile": A Methodological Note on Using a Voucher-Based Approach to Assess the Impact of Infrastructure Projects*. Discussion Paper 1078. Washington, DC: International Food Policy Research Institute.

Beuerman, D.W. (2011). Telecommunication Technologies, Agricultural Productivity and Child Labor in Rural Peru. University of Maryland, Unpublished Manuscript.

Beuermann, D.W., McKelvey, C., and Vakis, R. 2012. "Mobile Phones and Economic Development in Rural Peru." *Journal of Development Studies* 48 (11): 1–12.

Bhattacharyya, S.C. 2006. "Energy Access Problem of the Poor in India: Is Rural Electrification a Remedy?" *Energy Policy* 34 (18): 3387–3397.

Binswanger, H.P., Khandker, S.R., and Rosenzweig, M.P. 1993. "How Infrastructure and Financial Institutions Affect Agricultural Output and Investment in India." *Journal of Development Economics* 41 (2): 337–366.

Bosch, C., Hommann, K., Rubio, G., Sadoff, C., and Travers, L. 2000. "Water and Sanitation." In *A Sourcebook for Poverty Reduction Strategies*, edited by J. Klugman, 371–404. Washington, DC: World Bank.

Boserup, E. 1981. *Population and Technology*. Oxford, UK: Basil Blackwell.

Briceño-Garmendia, C., Smits, K., and Foster, V. 2008. *Financing Public Infrastructure in Sub-Saharan Africa: Patterns, Issues, and Options*. Africa Infrastructure Country Diagnostic Background Paper. Washington, DC: World Bank.

Brown, J.P., Pender, J., Wiser, R., Lantz, E. and Hoen, B. 2012. Ex post analysis of economic impacts from wind power development in U.S. counties. *Energy Economics*, *34*(6), pp.1743-1754.

Burchell, R., Dolphin, W., Shad, N., Zakrewsky, A., Pignataro, L., Griffis, F., Epling, J., Haughwout, A., Varady, D., and Looye, J. 1997. *Public Works Program–Performance Evaluation: Final Report*, prepared for U.S. Department of Commerce, Economic Development Administration, Office of Program Research and Evaluation (EDA Project No. 99-06-07415).

Casaburi, L., Glennerster, R., and Suri, T. 2013. *Rural Roads and Intermediated Trade: Regression Discontinuity Evidence from Sierra Leone*. Working Paper. London: UK Department for International Development.

Chase, R.S. 2002. “Supporting Communities in Transition: The Impact of the Armenian Social Investment Fund.” *World Bank Economic Review* 16 (2): 219–240.

Chaurey, A., Ranganathan, M., and Mohanty, P. 2004. “Electricity Access for Geographically Disadvantaged Rural Communities: Technology and Policy Insights.” *Energy Policy* 32 (15): 1693–1705.

Chong, A., Galdo, V., and Torero, M. 2005. *Does Privatization Deliver? Access to Telephone and Household Income in Poor Rural Areas Using a Quasi-natural Experiment in Peru.* Washington, DC: Inter-American Development Bank.

Cole, S.A. and Fernando, A.N. 2012. *The Value of Advice: Evidence from Mobile Phone–Based Agricultural Extension*. Working Paper 13-047. Boston, MA, US: Harvard Business School.

Courtois, P. and Subervie, J. 2013. “Farmer Bargaining Power and Market Information Services.” Paper presented at Centre for the Study of African Economies conference, Oxford, UK, March 17–19.

Crandall, R.W., Lehr, W., and Litan, R.E., 2007. *The effects of broadband deployment on output and employment: A cross-sectional analysis of US data*. Brookings Institution, Washington, DC.

Dalenberg, D.R. and Partridge, M.D., 1997. Public infrastructure and wages: Public capital's role as a productive input and household amenity. *Land Economics*, *73*(2), pp.268-284.

Davis, M. 1998. “Rural Household Energy Consumption: The Effects of Access to Electricity – Evidence from South Africa.” *Energy Policy* 26 (3): 207–217.

Dercon, S., Gilligan, D., Hoddinott, J., and Woldehanna, T. 2008. *The Impact of Agricultural Extension and Roads on Poverty and Consumption Growth in Fifteen Ethiopian Villages.* Discussion Paper 840. Washington, DC: International Food Policy Research Institute.

DeVol, R. and Wong, P. 2010. Jobs for America: Investments and policies for economic growth and competitiveness. *Milken Institute, January*.

Devoto, F., Duflo, E., Dupas, P., Parient, W., and Pons, V. 2012. “Happiness on Tap: Piped Water Adoption in Urban Morocco.” *American Economic Journal: Economic Policy* 4 (4): 68–99.

Dinkelman, T. 2011. “The Effects of Rural Electrification on Employment: New Evidence from South Africa.” *American Economic Review* 101 (7): 3078–3108.

Donaldson, D. 2018. Railroads of the Raj: Estimating the Impact of Transportation Infrastructure. American Economic Review, Vol 108, No. 4-5, pp.899-934.

Dong, X. 2000. “Public Investment, Social Services and Productivity of Chinese Household Farms.” *The Journal of Development Studies* 36 (3): 100–122.

Duchene, C. 2011. *Gender and Transport*. International Transport Forum Discussion Paper 2011-11. Paris: International Transport Forum.

Duflo, E. and Pande, R. 2007. Dams. Quarterly Journal of Economics 122(2): 601– 646.

Ellis, F., Kutengule, M., and Nyasulu, A. 2003. “Livelihoods and Rural Poverty Reduction in Malawi.” *World Development* 31 (9): 1495–1510.

Escobal, J. and Torero, M. 2005. “Measuring the Impact of Asset Complementarities: The Case of Rural Peru.” *Latin American Journal of Economics* (formerly *Cuadernos de Economía*) 42 (125): 137–164.

Esrey, S.A. 1996. “Water, Waste, and Well-Being: A Multicountry Study.” *American Journal of Epidemiology* 143 (6): 608–623.

Estache, A. and Wodon, Q. 2014. *Infrastructure and Poverty in Sub-Saharan Africa.* New York: Palgrave Macmillan.

Evans, P. and Karras, G. 1994. Are government activities productive? Evidence from a panel of U.S. states. *Review of Economics and Statistics*, 76(1), pp. 1-11.

Faber, B. 2014. “Trade Integration, Market Size, and Industrialization: Evidence from China’s National Trunk Highway System.” *Review of Economic Studies*, forthcoming.

Fafchamps, M. and Minten, B. 2012. “The Impact of SMS-Based Agricultural Information on Indian Farmers.” *World Bank Economic Review* 26 (3): 383–414.

Fan, S. and Hazell, P. 1999. *Are Returns to Public Investment Lower in Less-Favored Rural Areas? An Empirical Analysis of India.* Environment and Production Technology Division Discussion Paper 43. Washington, DC: International Food Policy Research Institute.

Fan, S., Hazell, P., and Haque, T. 2000. “Targeting Public Investments by Agro-ecological Zone to Achieve Growth and Poverty Alleviation Goals in Rural India.” *Food Policy* 25 (4): 411–428.

Fan, S., Hazell, P., and Thorat, S. 2000. “Government Spending, Growth and Poverty in Rural India.” *American Journal of Agricultural Economics* 82 (4): 1038–1051.

Fan, S., Zhang, L., and Zhang, X. 2002. *Growth, Inequality, and Poverty in Rural China: The Role of Public Investments*. Research Report 125. Washington, DC: International Food Policy Research Institute.

Fan, V. Y.-M. and Mahal, A. 2011. “What Prevents Child Diarrhoea? The Impacts of Water Supply, Toilets, and Hand-Washing in Rural India.” *Journal of Development*

Fewtrell, L., Kaufmann, R.B., Kay, D., Enanoria, W., Haller, L., and Colford, Jr, J.M.. 2005. "Water, Sanitation, and Hygiene Interventions to Reduce Diarrhoea in Less Developed Countries: A Systematic Review and Meta-analysis." *The Lancet Infectious Diseases* 5 (1): 42–52.

Forman, C., Goldfarb, A., and Greenstein, S. 2012. The Internet and local wages: A puzzle. *The American Economic Review*, *102*(1), pp.556-575.

Foster, V. 2008. *Overhauling the Engine of Growth: Infrastructure in Africa.* Washington, DC: Africa Infrastructure Country Diagnostic, World Bank.

Garcia-Mila, T., McGuire, T.J., and Porter, R.H. 1996. The effect of public capital in state-level production functions reconsidered. *Review of Economics and Statistics*, *78*(1), pp.177-180.

Gachassin, M., Najman, B., and Raballand, G. 2010. *The Impact of Roads on Poverty Reduction: A Case Study of Cameroon*. World Bank Policy Research Working Paper 5209. Washington, DC: World Bank.

———. 2005. "Water for Life: The Impact of the Privatization of Water Services on Child Mortality." *Journal of Political Economy* 113 (1): 83–120.

Galiani, S., Gonzalez-Rozada, M., and Schargrodsky, E. 2009. "Water Expansions in Shantytowns: Health and Savings." *Economica* 76 (304): 607–622.

Gallardo, R. and Rembert, M. 2017. Broadband economic benefits: Why invest in broadband infrastructure and adoption? (http://www.dailyyonder.com/broadband-economic-benefits-invest-broadband-infrastructure-adoption/2017/08/07/20695/; accessed May 14, 2018).

Gamper-Rabindran, S., Khan, S., and Timmins, C. 2010. "The Impact of Piped Water Provision on Infant Mortality in Brazil: A Quantile Panel Data Approach." *Journal of Development Economics* 92 (2): 188–200.

Gibson, J. and Rozelle, S. 2003. "Poverty and Access to Roads in Papua New Guinea." *Economic Development and Cultural Change* 52 (1): 159–185.

Global Infrastructure Outlook 2016. Infrastructure investment needs 50 countries, 7 sectors to 2040. Oxford Economics. A G20 Initiative.

Gonzalez-Navarro, M. and Quintana-Domeque, C. Forthcoming. "Paving Streets for the Poor: Experimental Analysis of Infrastructure Effects." *Review of Economics and Statistics.*

Goyal, A. 2010. "Information, Direct Access to Farmers, and Rural Market Performance in Central India." *American Economic Journal: Applied Economics* 2 (3): 22–45.

Greenstein, S. and McDevitt, R.. 2012. Measuring the broadband bonus in thirty OECD countries. OECD Digital Economy Papers, No. 197, OECD Publishing.

Guignet, D., Walsh, P.J., and Northcutt, R. 2016. Impacts of Ground Water Contamination on Property Values: Agricultural Run-off and Private Wells. *Agricultural and Resource Economics Review*, *45*(02), pp.293-318.

Hanemann, M. 1991. Willingness to pay and willingness to accept: How much can they differ? *American Economic Review*, 81(3), pp. 635-647.

Haughwout, A.F. 2002. Public infrastructure investments, productivity and welfare in fixed geographic areas. *Journal of Public Economics*, *83*(3), pp.405-428.

Heckman, J., Ichimura, H., Smith, J., and Todd, P. 1998. Characterizing selection bias using experimental data. *Econometrica*, *66*(5), pp.1017-1098.

Heintz, J., Pollin, R. and Garrett-Peltier, H. 2009. How infrastructure investments support the US economy: employment, productivity and growth. *Political Economy Research Institute (PERI), University of Massachusetts Amherst*.

Homan, A. 2014. Role of BCA in TIGER grant reviews: common errors and influence on the selection process. *Journal of Benefit-Cost Analysis,* 5(1), pp. 111-135.

Horowitz, J. and McConnell, K. 2002. A review of WTA/WTP studies. *Journal of Environmental Economics and Management*, 44, pp. 426-447.

Howe, J. and Richards, P., eds. 1984. *Rural Roads and Poverty Alleviation: A Study Prepared for the International Labour Office within the Framework of the World Employment Programme*. London: Intermediate Technology Publications.

Imbens, G. and Wooldridge, J. 2009. Recent development in the econometrics of program evaluation. *Journal of Economic Literature*, 47(1), pp. 5-86.

Islam, N. and Saha, J.K. 2011. Country Paper on Household ICT Access and Individual ICT Usage in Bangladesh. Mimeo, Bangladesh Bureau of Statistics.

Jacoby, H.G. 1998. *Access to Markets and the Benefits of Rural Roads: A Nonparametric Approach*. Washington, DC: Development Research Group, Rural Development, World Bank.

———. 2000. "Access to Markets and the Benefits of Rural Roads." *Economic Journal* 110 (465): 713–737. Jacoby, H. G., and B. Minten. 2009. "On Measuring the Benefits of Lower Transport Costs." *Journal of Development Economics* 89:28–38.

Jalan, J. and Ravallion, M. 2002. "Geographic Poverty Traps? A Micro Model of Consumption Growth in Rural China." *Journal of Applied Econometrics* 17 (4): 329–346.

———. 2003. "Does Piped Water Reduce Diarrhea for Children in Rural India?" *Journal of Econometrics* 112 (1): 153–173.

Janeski, I. and Whitacre, B.E. 2014. Long-Term Economic Impacts of USDA Water and Sewer Infrastructure Investments in Oklahoma. *Journal of Agricultural and Applied Economics*, *46*(1), p.21.

Jensen, R. 2007. "The Digital Provide: Information (Technology), Market Performance, and Welfare in the South Indian Fisheries Sector." *Quarterly Journal of Economics* 122 (3): 879–924.

———. 2010. "Information, Efficiency and Welfare in Agricultural Markets." *Agricultural Economics* 41 (S1): 203–216.

Jimenez, E. 1995. "Human and Physical Infrastructure: Public Investment and Pricing Policies in Developing Countries." In *Handbook of Development Economics*. Vol. 3, edited by J. Behrman and T. N. Srinivasan, 2773–2843. Amsterdam: North-Holland.

Kanagawa, M. and Nakata, T. 2008. "Assessment of Access to Electricity and the Socio-economic Impacts in Rural Areas of Developing Countries." *Energy Policy* 36 (6): 2016–2029.

Kandilov, A.M.G., Kandilov, I.T., Liu, X., and Renkow, M. 2017. The Impact of Broadband on U.S. Agriculture: An Evaluation of the USDA Broadband Loan Program, *Applied Economic Perspectives and Policy*, *39*(4), pp. 635–661.

Kandilov, I.T. and Renkow, M. 2010. Infrastructure investment and rural economic development: An evaluation of USDA's broadband loan program. *Growth and Change*, *41*(2), pp.165-191.

Khandker, B. and Koolwal, G. 2011. *Estimating the Long-Term Impacts of Rural Roads: A Dynamic Panel Approach*. Policy Research Working Paper 5867. Washington, DC: World Bank.

Khandker, S., Bakht, A., and Koolwal, G. 2009. "The Poverty Impact of Rural Roads: Evidence from Bangladesh." *Economic Development and Cultural Change* 57 (4): 685–722.

Khandker, S.R., Barnes, D.F., and Samad, H.A. 2009. *Welfare Impacts of Rural Electrification: A Case Study from Bangladesh.* Policy Research Working Paper 4859. Washington, DC: World Bank.

Khandker, S.R., Barnes, D.F., and Samad, H.A., and Minh, N.H. 2009. *Welfare Impacts of Rural Electrification: Evidence from Vietnam*. Policy Research Working Paper 5057. Washington, DC: World Bank.

Kim, Y. and Orazem, P.F. 2017. Broadband Internet and New Firm Location Decisions in Rural Areas, *American Journal of Agricultural Economics*, *99*(1), pp. 285–302.

Klasen, S., Lechtenfeld, T., Meier, K., and Rieckmann, J. 2011. *Impact Evaluation Report: Water Supply and Sanitation in Provincial Towns in Yemen*. Göttingen, Germany: Courant Research Centre, Poverty, Equity and Growth.

Klonner, S. and Nolen, P.J. 2010. *Cell Phones and Rural Labor Markets: Evidence from South Africa.* Proceedings of the German Development Economics Conference 56. Frankfurt, Germany: VereinfürSocialpolitik.

Kolko, J. 2012. Broadband and local growth. *Journal of Urban Economics*, *71*(1), pp.100-113.

Koolwal, G. and van de Walle, D. 2013. "Access to Water, Women's Work, and Child Outcomes." *Economic Development and Cultural Change* 61 (2): 369–405.

Kremer, M., Leino, J., Miguel, E., and Zwane, A.P. 2011. "Spring Cleaning: Rural Water Impacts, Valuation, and Property Rights Institutions." *Quarterly Journal of Economics* 126 (1): 145–205.

Kuttner, H. 2016. The economic impact of rural broadband. Hudson Institute, Arlington, VA.

Labonne, J. and Chase, R.S. 2009. *The Power of Information: The Impact of Mobile Phones on Farmers' Welfare in the Philippines.* Working Paper 4996. Washington, DC: World Bank.

Lanjouw, P., Quizon, J., and Sparrow, R. 2001. "Non-agricultural Earnings in Peri-urban Areas of Tanzania: Evidence from Household Survey Data." *Food Policy* 26 (4): 385–403.

Lebo, J. and Schelling, D. 2001. *Design and Appraisal of Rural Transport Infrastructure: Ensuring Basic Access for Rural Communities*. Technical Paper 496. Washington, DC: World Bank.

Leggett, C.G. and Bockstael, N.E. 2000. Evidence of the effects of water quality on residential land prices. *Journal of Environmental Economics and Management*, *39*(2), pp.121-144.

Lehr, W.H., Osorio, C., Gillett, S.E., and Sirbu, M.A. 2006. Measuring broadband's economic impact. MIT Engineering Systems Division, ESD-WP-2006-2, Cambridge, MA.

Limão, N. and Venables, A.J. 1999. *Infrastructure, Geographical Disadvantage, and Transport Costs*. Policy Research Working Paper 2257. Washington, DC: World Bank.

Lipton, M. and Ravallion, M. 1995. "Poverty and Policy." In *Handbook of Development Economics.* Vol. 3, edited by J. Behrman and T. N. Srinivasan, 2551–2657. Amsterdam: North-Holland.

Lokshin, M. and Yemtsov, R. 2005. "Has Rural Infrastructure Rehabilitation in Georgia Helped the Poor?" *World Bank Economic Review* 19 (2): 311–333.

Mack, E. and Faggian, A. 2013. Productivity and broadband: The human factor. *International Regional Science Review*, *36*(3), pp.392-423.

Milner, C., Morrissey, O., and Rudaheranwa, N. 2000. "Policy and Non-policy Barriers to Trade and Implicit Taxation of Exports in Uganda." *Journal of Development Studies* 37 (2): 67–90.

Mitra, S., Mookherjee, D., Torero, M., and Visara, S. 2012. *Asymmetric Information and Middleman Margins: An Experiment with Potato Farmers*. Boston, MA, US: Boston University Press.

Mitra, S., Mookherjee, D., Torero, M., and Visaria, S. 2018. "Asymmetric Information and Middleman Margins: An Experiment with Indian Potato Farmers," The Review of Economics and Statistics, MIT Press, vol. 100(1), pages 1-13, March.

Molnar, G., Savage, S.J., and Sicker, D.C. 2015. Reevaluating the broadband bonus: Evidence from neighborhood access to fiber and United States housing price. University of

Colorado, Boulder. Available at: http://www.lightwaveonline.com/content/dam/lw/documents/FTTH_Report_06_26_2015.pdf (accessed 6/2/2017).

Mortensen, D.T. 2003. *Wage Dispersion: Why Are Similar Workers Paid Differently?* Cambridge, MA: MIT Press.

Mu, R. and van de Walle, D. 2011. "Rural Roads and Local Market Development in Vietnam." *Journal of Development Studies* 47 (5): 709–734.

Munnell, A.H. 1990. How does public infrastructure affect regional economic performance? *New England Economic Review*, (Sep), pp.11-33.

Murali, D. 2011. "Call of the Small Towns and Villages." The Hindu Business Line, October 12. www.thehindubusinessline.com/catalyst/article2531326.ece.

Muto, M. and Yamano, T. 2009. "The Impact of Mobile Phone Coverage Expansion on Market Participation: Panel Data Evidence from Uganda." *World Development* 37 (12): 1887–1896.

Nakasone, E. 2013. "The Role of Price Information in Agricultural Markets: Experimental Evidence from Rural Peru." Unpublished, International Food Policy Research Institute, Washington, DC; University of Maryland, College Park, MD, US.

Nakasone, E., Torero, M., and Minten, B. 2014. "The Power of Information: The ICT Revolution in Agricultural Development." *Annual Review of Resource Economics* 6:533–550.

Nakasone, E. and Torero, M. 2016. A Text Message Away: ICTs as a Tool to Improve Food Security. Agricultural Economics. Agricultural Economics, International Association of Agricultural Economists, vol. 47(S1), pages 49-59, November.

Nevo, A., Turner, J.L., and Williams, J.W. 2016. Usage-based pricing and demand for residential broadband. *Econometrica*, 84(2), pp. 411-443.

Newman, J., Pradhan, M., Rawlings, L.B., Ridder, G., Coa, R., and Evia, J.L. 2002. "An Impact Evaluation of Education, Health, and Water Supply Investments by the Bolivian Social Investment Fund." *World Bank Economic Review* 16 (2): 241–274.

Núñez-Serrano, J.A. and Velázquez, F.J. 2017. Is Public Capital Productive? Evidence from a Meta-analysis. *Applied Economic Perspectives and Policy*, *39*(2), pp.313-345.

Nyarko, Y., Hildebrandt, N., Romagnoli, G., and Soldani, E. 2013. *Market Information Systems for Rural Farmers Evaluation of Esoko MIS—Year 1 Results*. Abu Dhabi: New York University Center for Technology and Economic Development. www.nyucted.org/archives/1108.

OECD/IEA (Organisation for Economic Co-operation and Development/International Energy Agency). 2013. *World Energy Outlook 2013*. Paris: IEA Publications.

OED (Operations Evaluation Department). 1995. *Improving African Transport Corridors*. OED Précis 84. Washington, DC: World Bank. http://www-wds.worldbank.org/external/default/WDSContentServer/WDSP/IB/2004/04/16/000011823_20040416155316/Rendered/PDF/28510.pdf.

Office of Management and Budget (OMB). 1992. Circular A-94: Guidelines and Discount Rates for Benefit-Cost Analysis of Federal Programs. Washington D.C.

Pattanayak, S.K., Poulos, C., Yang, J.-C., and Patil, S. 2010. "How Valuable Are Environmental Health Interventions? Evaluation of Water and Sanitation Programmes in India." *Bulletin of the World Health Organization* (88):535–542.

Pattanayak, S.K., Poulos, C., Yang, J.-C., Patil, S., and Wendland, K.J. 2009. "Of Taps and Toilets: Quasi-experimental Protocol for Evaluating Community-Demand-Driven Projects." *Journal of Water and Health* 7 (3): 434–451.

Pattanayak, S.K., Yang, J.-C., Poulos, C., Patil, S.R., Arnold, B., and Colford, J.M. 2008. *Three Years Later: Environmental Health Impacts of a Community-Demand-Driven Water and Sanitation Program in Rural Maharashtra, India.* Working Paper 08-01. Research Triangle Park, NC, US: RTI International.

Pereira, A.M. 2000. Is all public capital created equal? *Review of Economics and Statistics*, *82*(3), pp.513-518.

Peters, D. 2001. *Gender and Transport in Less Developed Countries*. London: United Nations Environment and Development Forum.

Platteau, J.-P. 1996. "Physical Infrastructure as a Constraint on Agricultural Growth: The Case of Sub-Saharan Africa." *Oxford Development Studies* 24 (3): 189–219. Qin, Y., and X. Zhang. 2012. *Road to Specialization in Agricultural Production: Evidence from Rural China*. Discussion Paper 01221. Washington, DC: International Food Policy Research Institute.

Raballand, G., Macchi, P., and Petracco, C. 2010. *Rural Road Investment Efficiency*. Washington, DC: World Bank.

Rand, J. 2011. "Evaluating the Employment-Generating Impact of Rural Roads in Nicaragua." *Journal of Development Effectiveness* 3 (1): 28–43.

Reardon, T., Berdegué, J., and Escobar, G. 2001. "Rural Nonfarm Employment and Incomes in Latin America: Overview and Policy Implications." *World Development* 29 (3): 395–409.

Reinikka, R. and Svensson, J. 1999. *How Inadequate Provision of Public Infrastructure and Services Affects Private Investment.* Working Paper 2262. Washington, DC: World Bank.

Rembert, M., Feng, B., and Partridge, M. 2017. Connecting the dots of Ohio's broadband policy. Ohio State University. (https://aede.osu.edu/sites/aede/files/publication_files/Connecting%20the%20Dots%20of%20Ohio%20Broadband_0.pdf; accessed May 14, 2018).

Render, M. 2016. The tangible value of advanced broadband to MDUs. RVA Market Research and Consulting, LLC. (http://glenechogroup.isebox.net/ftthconnect?default=tXExg6Xo, accessed May 14, 2018).

Renkow, M., Hallstrom, D.G., and Karanja, D.D. 2004. "Rural Infrastructure, Transactions Costs and Market Participation in Kenya." *Journal of Development Economics*, 73:349–367.

Roback, J. 1982. Wages, rents, and the quality of life. *Journal of Political Economy*, *90*(6), pp.1257-1278.

Romer, C. and Bernstein, J. 2009. The job impact of the American recovery and reinvestment plan.

Shideler, D., Badasyan, N., and Taylor, L. 2007. The economic impact of broadband deployment in Kentucky. *Regional Economic Development 3*(2), pp. 88-118. Federal Reserve Bank of St. Louis.

Smith, D.R., Gordon, A., Meadows, K., and Zwick, K. 2001. "Livelihood Diversification in Uganda: Patterns and Determinants of Change across Two Rural Districts." *Food Policy* 26:421–435.

———. 2014. "Targeting Investments to Link Farmers to Markets: A Framework for Capturing the Heterogeneity of Smallholder Farmers." In *New Directions for Smallholder Agriculture*, edited by P. Hazell and A. Rahman, 155–183. Oxford, UK: Oxford University Press.

Stenberg, P., Morehart, M., Vogel, S., Cromartie, J., Breneman, V., and Brown, D. 2009. *Broadband Internet's Value for Rural America.* Washington, DC: US Department of Agriculture. Economic Research Service, Economic Research Report (78).

Torero, M. and S. Chowdhury. 2005. *Increasing Access to Infrastructure for Africa's Rural Poor.* Issues Brief, 2020 Vision. Washington, DC: International Food Policy Research Institute.

Torero, M, Almanzar, M., and Nakasone, E. 2016. Impact Evaluation of MCC Connectivity Project in El Salvador. The Northern Transnational Highway. Impacts estimates using Panel Data from 2009 to 2014. Millennium Challenge Corporation (MCC). Washington. https://data.mcc.gov/evaluations/index.php/catalog/135.

Torero, M. and Barron, M. 2016. Impact Evaluation of MCC Compact inEl Salvador. The Rural Electrification Sub-Activity. Impacts estimates using Panel Data from 2009 to 2014. Millennium Challenge Corporation (MCC). Washington. https://data.mcc.gov/evaluations/index.php/catalog/111

Valdivia, M. 2009. *Contracting the Road to Development: Early Impacts of a Rural Roads Program*. Working Paper 2009/10. Caracas: CAF—Development Bank of Latin America.

van de Walle, D. 1996. *Infrastructure and Poverty in Viet Nam*. Washington, DC: World Bank.

———. 2009. "Impact Evaluation of Rural Road Projects." *Journal of Development Effectiveness* 1 (1): 15–36.

WDI, World Development Indicators (2003-2014). http://data.worldbank.org/data-catalog/world-development-indicators

Whitacre, B., Gallardo, R., and Strover, S. 2014a. Broadband׳ s contribution to economic growth in rural areas: Moving towards a causal relationship. *Telecommunications Policy*, *38*(11), pp.1011-1023.

Whitacre, B., Gallardo, R., and Strover, S. 2014b. Does rural broadband impact jobs and income? Evidence from spatial and first-differenced regressions. *The Annals of Regional Science*, *53*(3), pp.649-670.

WHO (World Health Organization). 2013. "Diarrhoeal Disease." Fact Sheet 330. www.who.int/mediacentre/factsheets/fs330/en/.

WHO and UNICEF. 2005. *Water for Life: Making It Happen*. Geneva. www.who.int/water_sanitation_health/waterforlife.pdf.

Willig, R. 1976. Consumer's surplus without apology. *American Economic Review*, 66(4), pp. 589-597.

Wood, A. 2002. "Could Africa Be Like America?" Unpublished, UK Department for International Development, London.

———. 2012. "Morocco—Socioeconomic Influence of Rural Roads: Fourth Highway Project." http://lnweb18.worldbank.org/oed/oeddoclib.nsf/DocUNIDViewForJavaSearch/7BEC60EE0D81629F852567F5005D3597?opendocument.

Yamauchi, F., Muto, M., Chowdhury, S., and Dewina, R. 2011. "Are Schooling and Roads Complementary? Evidence from Income Dynamics in Rural Indonesia." *World Development* 39 (12): 2232–2244.

You, L., Ringler, C., Nelson, G., Wood-Sichra, U., Robertson, R., Wood, S., Guo, Z., Zhu, T., and Sun, Y.. 2010. *What is the Irrigation Potential for Africa? A Combined Biophysical and Socioeconomic Approach*. IFPRI Discussion Paper 993. Washington, DC: International Food Policy Research Institute.

Zhang, X. and Fan, S. 2004. "How Productive is Infrastructure? A New Approach and Evidence from Rural India." *American Journal of Agricultural Economics* 86 (2): 492–501.

———. 2000. *Public Investment and Regional Inequality in Rural China.* Environment and Production Technology Division Discussion Paper 71. Washington, DC: International Food Policy Research Institute.

Zimmerman, F.J. and Carter, M.R. 2003. "Asset Smoothing, Consumption Smoothing and the Reproduction of Inequality under Risk and Subsistence Constraints." *Journal of Development Economics* 71 (2): 233–260.

Zwane, A.P. and Kremer, M. 2007. *What Works in Fighting Diarrheal Diseases in Developing Countries?* Working Paper 12987. Cambridge, MA, US: National Bureau of Economic Research.

Appendix. Concepts and Methods for Estimating Impacts, Costs, and Benefits

Concepts

Economic Impacts

The term "impacts" is used in many ways in the literature, in policy discussions, and in common parlance. To avoid confusion, we define the *impact of an intervention* as the difference between what occurred with the intervention and what would have occurred without the intervention. This definition focuses on the causal effects of an intervention, and is consistent with most of the modern literature on economic impact evaluation (e.g., see Imbens and Wooldridge 2009). This definition assumes that the intervention already occurred (e.g., if one is conducting an *ex post* study of the impacts of an intervention). If the intervention has not occurred and one wishes to know the impact if it were to occur (e.g., if one is conducting an *ex ante* appraisal of potential impact of an intervention) one could define the *potential impact* of an intervention as the difference between what would occur with the intervention and what would occur without the intervention. In both of these cases, there is a basic problem of unobservable outcomes. In the case of the impact of an intervention that has already occurred, the outcomes if the intervention had not occurred (called the *counterfactual*) are not observable. The principal challenge for estimating causal impacts is the unobservable nature of the counterfactual outcomes. As a result, such counterfactual outcomes must be estimated. We discuss approaches to doing this below in the section on Methods.

Our reference to "economic" impacts means that our main focus in this review is on outcomes that are commonly viewed as economic outcomes, such as GDP, employment and unemployment, wages and other earnings, income, wealth, and poverty. That does not preclude investigation of impacts on outcomes not commonly seen as "economic", such as on population change, health, clean air and water, or other indicators considered to affect the quality of life and the environment. Indeed, as will be discussed in the literature review, many such quality of life outcomes can affect economic ones, such as levels of wages and property values or land rents. We did not exhaustively seek or review literature on such a broad range of impacts, however.

Costs

We use the standard economics textbook definition of opportunity costs, defined as the value of resources used in an activity if they were employed in their highest value alternative use. For example, in a competitive labor market, the opportunity cost of the labor used to complete an infrastructure project would often be reflected by the wages that were paid to the workers, which should reflect wages that the workers would earn and their productivity in the best alternative use of their labor. However, if unemployed workers who are unable to find alternative work at the market wage rate were employed in an infrastructure project, or if the project paid higher than the competitive market wage for workers, the opportunity cost of the workers could be less than the wages paid. Similar considerations apply to the cost of other investment inputs, such as the cost of land, buildings, and equipment.

Benefits

Benefits, if measured in value terms, are usually defined as the *willingness to pay* (WTP) of those receiving the benefit, or the *willingness to accept* (WTA) payment in lieu of receiving the benefit. Like the definition of opportunity costs, this definition of benefits emphasizes the value of what is foregone to receive or not receive the benefit. According to standard economic theory, for small changes in one's situation, WTP and WTA should be close to equal, while for large changes in welfare, WTA should generally be greater than WTP (Hanemann 1991). The divergence between WTP and WTA theoretically will also be larger for valuing benefits of a good or service that does not have close substitutes (e.g., Yosemite National Park) (Hanemann 1991) and this is supported by empirical research (Horowitz and McConnell 2002). As an extreme example of this, consider what someone would be willing to pay for a medical intervention that would certainly save his life, vs. what that person would be willing to accept in lieu of the lifesaving intervention. In the first case, the person's WTP is limited by his ability to pay. In the second case, there may be no amount that the person would accept in lieu of receiving the treatment, depending on his aversion to death and associated pain, bequest motives, etc. However, even in cases involving small changes in welfare and substitutable goods, WTA is often found to be larger than WTP in empirical studies, though the differences are smaller for such cases (Horowitz and McConnell 2002). Considering both measures may be helpful to place bounds on the estimated benefits of an investment. However, for U.S. Federal Government-supported projects, the Office of Management and Budget (OMB 1992) identifies WTP as the concept to use in estimating the value of benefits.

Consumer and Producer Surplus

For goods and services provided in competitive markets, consumers' total willingness to pay for a given amount of a good or service is represented by the area under the market demand curve; i.e., the demand curve represents the price some consumer is willing to pay for each unit of the good or service consumed, arrayed from the most to the least valued units.[50] The difference between consumers' willingness to pay for an amount of the good or service and the price paid for that good or service is called *consumer surplus*. Similarly, *producer surplus* is the difference between the price producers receive for the good or service and their cost of producing and selling it. If there are no transaction costs, taxes or tariffs inducing a difference between the price

[50] The notion of WTP discussed above in defining benefits is also called *compensating variation* in economics textbooks and is based on the concept of a *compensated or Hicksian demand function* (i.e., a function indicating the amount a consumer is willing to pay for a given quantity of a good or service, holding the person's welfare constant), while WTA is called *equivalent variation*, and is also based on compensated demand. By contrast, the concept of consumer surplus is based on the concept of an *uncompensated or Marshallian demand* function (the amount the consumer is willing to pay holding his or her income, but not welfare, constant). For estimating the welfare effects of a change in prices, Willig (1976) proved that the effect on consumer surplus of the change is between the compensating (WTP) and equivalent variation (WTA), and demonstrated that under reasonable assumptions, the difference between the change in consumer surplus and either WTP or WTA should be fairly small. Hanemann (1991) extended Willig's analysis to include the effects of changes in the quantity of non-marketed amenities or public goods and demonstrated that a generalized concept of consumer surplus for this case is between the WTP and WTA, but also showed that the differences between WTP and WTA may be very large, depending on the substitutability of market goods for the amenity or public good.

paid by consumers and the price received by producers and no externalities, the sum of consumer and producer surplus represents the net benefit to the economy of the use of the good or service. With transaction costs, the price paid by consumers and the price received by producers may differ (with the latter less than the former), but the net benefit to the economy is still the sum of consumer and producer surplus. If the difference between consumer and producer prices is due in part to taxes or tariffs, the net benefit to society includes the value of the tax or tariff collected.

Private vs. Social Costs and Benefits

The nature of a cost-benefit analysis (CBA) is defined by whose costs and benefits are being considered. For a private individual or firm, the relevant costs are those paid by that individual or firm and the relevant benefits are the benefits received by that individual or firm. An analysis of the costs and benefits to a private individual or firm is considered a *private* CBA. If the costs and benefits of everyone in a particular community (whether the community is a town, state, nation, everyone in the world, or some other defined group of people) is considered, it may be considered a *social* CBA. The differences between the costs and benefits considered in a private CBA and a social CBA are called *externalities*; i.e., costs or benefits that are external to the private investor but are borne or received by other members of the community considered. For analyses that focus on less than the entire affected population (e.g., a national level social CBA for a project that causes international costs and benefits), there may be externalities that occur to other communities that aren't considered, even in a social CBA.

Net Present Value, Benefit-Cost Ratio, and Internal Rate of Return

If the costs and benefits of an intervention occur over time, rather than all in one-time period (as will be the case for long-lived infrastructure investments), it is necessary to discount future costs and benefits to account for people's value of time or the opportunity cost of capital. Under conditions of perfect capital markets, these concepts should be equal to the market rate of interest (Pender 1996). For example, if one can borrow or save without constraint at a risk free real interest rate of r%, $100 received in one year (adjusted for inflation) should be worth $100/(1+r) at present to a borrower or saver. CBA's therefore compute the *present value* of future costs and benefits, usually discounting the real (inflation adjusted) values of costs and benefits by a measure of the real market interest rate for a time period commensurate with the time frame over which the analysis is focused. The difference between discounted benefits and costs is the *net present value* (NPV) of the intervention and is a measure of the net benefit. The *benefit-cost ratio* (BCR), which is the ratio of the present value of benefits to the present value of costs, can also be calculated using the discounted values of benefits and costs.

Complications arise for estimating the NPV or BCR when the simplifying assumptions of a perfect capital market with a single risk free rate of interest do not hold. In reality, not all borrowers are able to borrow unlimited amounts at a single rate of interest and transaction costs in capital markets can result in differences between the rate of interest for borrowers and savers. The relevant discount rate to use in a private CBA should take the investor's financial situation into account; for example, a credit constrained investor or one forced to borrow from high cost lenders would discount his future costs and benefits at a higher rate than one who is not so constrained or who is saving at a low rate of interest (Pender 1996). From a larger national point of view, however, the opportunity cost of capital to the Treasury of an infrastructure investment may be much lower than the cost to private investors and may be the appropriate discount rate for an investment financed by the Federal Government. This is the basis of the guidance provided by OMB for selecting the discount rate to use in appraisals of projects to be supported by the Federal Government (OMB 1994).

Given uncertainty about the appropriate discount rate to use, analysts sometimes prefer to estimate and use the *internal rate of return* (IRR), which is the discount rate that sets the net present value of the project equal to zero. Under normal circumstances (in which a single IRR achieves this equality),[51] a decision rule of investing in a project with a positive NPV (for some pre-specified discount rate r) is equivalent to a rule of investing in the project if its IRR is greater than r. However, differences between NPV and IRR decision rules in selecting projects can arise if there are multiple projects to choose from with different flows of costs and benefits over time. Maximizing NPV is the appropriate decision rule for such a case, assuming that the discount rate used reflects the marginal cost of capital and that capital is not rationed (Dudley 1972). Nevertheless, the IRR may still provide useful information.[52] If capital is constrained, a simple decision rule is not possible (Dudley 1972).[53]

Transfers

In CBA, it is important to distinguish between net benefits of an intervention and distributional *transfers* caused by the intervention. In a social CBA, the main goal is usually to determine the *net social benefits* – the difference between aggregate social benefits and social costs for the community (however defined) as a whole – or some alternative related measures, such as the social BCR or the social IRR. Many of the impacts of an intervention may involve transfers of costs and benefits between members of the community and hence net out of the net benefits measured for the community as a whole. For example, in a national CBA of investment in some type of infrastructure, some of the positive impacts on employment and income in one location may come at the expense of reduced employment and income in other locations. Impact studies, even if they validly estimate the causal impact of the investment on beneficiary regions, may overstate the aggregate net benefit of the investment if they ignore the negative impacts in other regions. This poses a considerable challenge for estimating aggregate net benefits based on impact estimates, since impact studies typically measure impacts in regions more directly affected by an intervention compared to more distant and presumably less affected regions. If the impacts in the more distant regions are sufficiently small and diffuse, it may be possible to obtain a valid estimate of impact in the directly affected regions but not in the more distant

[51] The IRR may have multiple values if the stream of net benefits in each time period alternatives between negative and positive values more than once (OMB 1994).
[52] OMB (1994, p. 9) argues that "While the internal rate of return does not generally provide an acceptable decision criterion, it does provide useful information, particularly when budgets are constrained or there is uncertainty about the appropriate discount rate".
[53] One possible approach is to adjust the discount rate upward to account for the effect of the constraint on the shadow price of capital, but this requires considering all future investment possibilities, and could result in varying discount rates over time as the constraint becomes more or less binding over time.

regions. Assuming that the aggregate impacts in more distant regions is zero, simply because the impacts on any particular location are too small to measure, may lead to invalid conclusions about aggregate costs and benefits of the intervention.

Understanding the magnitude of transfers among different regions and groups of people can be important in its own right, even if they do not affect the aggregate net benefits of an intervention. The distributional impacts of an intervention are a separate consideration that many decision makers – particularly those representing affected regions and groups – will care about.

Methods

Statistical Approaches to Estimating Impacts

Statistical approaches to *ex post* estimation of impacts include use of randomized control trials (RCTs), quasi-experimental designs (QEDs) that mimic the treatment-control paradigm of RCTs, various econometric methods of estimating impacts, and model-based approaches. We discuss each of these briefly.

Randomized Control Trials

One of the most widely accepted methods of estimating impacts of interventions, when it is feasible to use, is to use randomized assignment of the intervention to treatment and control groups. Randomization can be used for determining who will be exposed to the intervention, the level or timing of the exposure, or other aspects of the intervention. The advantage of randomization is that it assures that treatment and control groups (or groups assigned to different levels, timing, or characteristics of treatment) are statistically similar in both observable and unobservable characteristics prior to the treatment.[54] Under certain conditions, this assures that systematic (not random) differences in outcomes detected between the groups after the intervention are due to the intervention (subject to a measurable degree of uncertainty resulting from random variations) and are not the result of *selection bias*. Essentially, randomized assignment permits estimation of the counterfactual situation (e.g., what outcomes for the treated group would have been without the treatment) by observing the post-treatment outcomes for a randomly selected control group, which can be compared to the post-treatment outcomes of the randomly selected treatment group. Systematic differences between the distributions of outcomes of these groups are evidence of the causal impact of the intervention.[55]

[54] By "statistically similar", we mean that no systematic differences in the distribution of characteristics between the groups exists; i.e., the only differences are due to random variation.
[55] Typically, RCTs estimate the difference in the mean values of outcomes between treatment and control groups (called the average effect of the treatment on the treated, or ATT) and the standard error of this difference, which is used to estimate the degree of confidence that the observed difference is means is systematic and not due to random variations. Since randomization assures that the entire distributions of characteristics of the treatment and control groups, and not just the mean, should be statistically similar at the time of random assignment, systematic differences in other characteristics of the distribution of outcomes (e.g., medians, percentiles) can also be tested and attributed to the intervention with a RCT.

Despite their advantages for estimating impacts, RCTs face several limitations and disadvantages for assessing impacts, especially of infrastructure investments. Randomization is not always feasible to use. For example, they are not feasible for investments that have already occurred. Furthermore, the location and affected population of an infrastructure investment such as a dam, highway, or bridge may be determined by geographic characteristics and cannot or should not be randomly determined.[56] Using randomization may change the nature of the intervention, the pool of beneficiaries, or their behavior; hence may not result in a valid assessment of the impacts of the intended intervention, even when it is feasible to use.[57] This is called *randomization bias* (Heckman and Smith 1995). Another source of bias is if close substitutes for the intervention exist and if members of the control group pursue such substitutes as a result of being involved in the RCT; this is called *substitution bias* (Heckman and Smith 1995).[58]

One cause of substitution bias can be if governments or others seek to provide other benefits to people who aren't eligible for the particular intervention being assessed (Ravallion 2009). This relates to an assumption underlying both experimental and quasi-experimental evaluations that the program does not affect the control observations. This assumption may hold for a small project operating in a few locations, but may be unlikely to hold for a large infrastructure investment due to market level impacts. And even for a small project, spillovers of knowledge or local market effects may affect the members of the control group, if they are from the same or nearby communities. Selecting control observations from more distant communities can help to address this concern and is often used in RCTs and QEDs, but such observations may systematically differ in characteristics related to the local community and economy, contributing to selection bias.

Another assumption underlying both experimental and quasi-experimental methods is that the treated and control populations are statistically comparable prior to the treatment and remain comparable over time. Even if these populations were randomly assigned in an RCT, this only assures comparability if everyone assigned to these groups agrees to participate in the experiment and continues to participate throughout the time frame of the evaluation. Usually some members of each group will refuse to participate initially or drop out over time and this problem is often greater for members of the control group if they are offered no or smaller benefits of participating in the study. If there is a significant amount of non-participation or attrition from the study, the treatment and control groups may no longer be comparable. Even when random assignment is used initially, econometric statistical methods and additional data may be needed to test and correct for selection biases that result.[59]

---

[56] If access to the infrastructure is controlled and subject to fees, such as in the case of electricity, water systems, or a toll road, random allocation of vouchers subsidizing the cost of access can be used in an impact evaluation. An example of this approach is provided in Torero and Barron (2016).

[57] An example of randomization bias would be what would result if a selective university decided to randomly accept applicants rather than using a rigorous selection process and then compared the educational and career outcomes of the randomly selected and not selected groups. The results of such a RCT would not represent the impacts of the university as it normally operates, because it would have changed the nature of the university's selection process and the pool of students that normally could be accepted.

[58] This situation can occur in clinical trials of medicines and is the reason such trials often try to prevent study subjects from knowing whether they are in the treatment or control group by providing a placebo to the controls. Addressing this problem in evaluating an infrastructure investment presents obvious difficulties.

[59] Randomization can be used in combination with econometric approaches discussed below to produce more valid estimates of impact. For example, the random assignment can be used as an *instrumental variable* to predict

Quasi-Experimental Designs

Quasi-experimental designs (QEDs) involve comparing outcomes for the beneficiaries of an intervention to a similar group of non-beneficiaries. This assumes that no other differences between the beneficiaries and non-beneficiaries besides the intervention could be responsible for the difference in outcomes. As discussed earlier, violation of this assumption is called selection bias and the ability to avoid this bias is the main advantage offered by RCTs. This assumption may be violated in QEDs because of targeting of interventions to certain types of beneficiaries and because of decisions made by people to access the benefits of interventions. QED methods have been developed to try to minimize the selection bias problem by ensuring that the comparison groups are as similar as possible prior to the intervention in characteristics that affect the outcomes being considered.

Matching estimators. One approach is to match observations in the beneficiary group to observations in the non-beneficiary group using pre-intervention observable characteristics that are hypothesized to be associated with both the incidence of the intervention and with the outcomes, and then compare differences in average outcomes for the matched groups. Under the assumption that the outcome variable is independent of being in one of the two groups, conditional upon the observable variables used for matching, matching yields an unbiased estimate of the impact of the intervention (Rosenbaum and Rubin 1983).[60] This is a strong assumption, however.

Difference-in-difference estimation. If outcome data are available on beneficiaries and a comparison group both before and after implementation of the program, selection bias can also be addressed using simple difference-in-difference (DD) estimation. DD estimation compares the pre- to post-program mean changes in outcomes between the beneficiary and comparison group. If the factors that affect outcomes and differ between the two groups are fixed over time and are additive in their impact on outcomes, the DD estimator subtracts out the effect of such differences, resulting in valid impact estimates. If outcome data are available for both groups for multiple periods prior to the intervention, it is possible to test the validity of this estimator by investigating whether the groups of participants and controls exhibited parallel trends in outcomes prior to the intervention (Imbens and Wooldridge 2009). Failure to reject this assumption using pre-intervention data strengthens confidence that the DD estimation yields a valid estimate of impact, although it doesn't prove it. Low statistical power could be an alternative reason that parallel prior trends is not rejected or differences in time-varying factors could be affecting the two groups differently after the intervention.

participation in a program, and predicted participation used as the indicator of participation, rather than actual participation. This two-stage regression approach eliminates the problem of *endogenous* program participation, which can lead to biased results because factors affecting actual program participation may also affect the outcomes being measured. If program participation is predicted by the random assignment of *intent to treat*, then predicted program participation will not be correlated with factors that affect outcomes other than the intent to treat. Thus randomization can be valuable for producing valid estimates of impact even when problems such as non-random participation decisions or attrition of treated and control groups occurs.

[60] The validity of matching results also depends upon how well the matching performs in achieving balance of the characteristics used to match the samples, which can be tested.

Regression Methods

Another general approach to estimating impacts of an intervention is to estimate a multiple regression model, in which measures of the presence, proximity, and/or intensity of the intervention are included in the model to predict outcomes. Under strong assumptions, the *ordinary least squares (OLS) regression model* can provide an unbiased estimate of the impact of the intervention. The most important assumption for this result is that the variables representing the intervention are not correlated with the error term in the regression model. Failure of this assumption could occur because other factors that affect the outcome and are associated with the intervention are not accounted for in the regression (called *omitted variable bias* or *unobserved heterogeneity*), because of errors in measuring the variables representing the intervention or because the intervention variables themselves are affected by the outcome variable or unobserved variables that are correlated with the outcome (called *selection bias, reverse causality, endogeneity or simultaneity bias*). Various regression methods have been developed and used to address these potential sources of bias.

Fixed effects and difference estimators. If data are available for observational units over multiple time periods (i.e., the data are *panel data*), the linear effects of unobserved fixed factors on outcomes can be eliminated by differencing each variable value from its mean value (*fixed effects estimator*) or by differencing the value of each variable in each year from the prior year (*first difference estimator*). This addresses one of the main reasons for omitted variable bias or selection bias, as long as the omitted variables responsible for the bias are fixed during the time frame of the data and have a linear impact on the outcome. Note that the intervention variables must not be fixed for all observations, since if they are, their impact will not be estimable (since they will be differenced out). If the intervention variable is a simple indicator for the presence or absence of the intervention, the data must include some pre-intervention observations as well as post-intervention observations and some observations with and without the intervention. With only two time periods of data (one pre-intervention and one post-intervention) and two comparison groups (with vs. without intervention), the fixed effects and first difference estimators are equivalent, and are a DD estimator (Wooldridge 2010).[61] This estimator may still produce biased estimates if time-varying unobserved factors affecting the outcome are associated with intervention variables; e.g., if faster growth in the outcome is occurring for places close to an intervention than further from the intervention for reasons other than the intervention and not accounted for by the regression model. This concern can be addressed to some extent by using a *second difference estimator* if the panel includes observations for at least three time periods for each observational unit, which uses first differences of all first differenced variables in the regression. This estimator differences out not only the effects of fixed factors, but also the effects of linear time trends in outcomes that differ across observational units. While such first and second difference approaches can be effective in removing biases caused by unobserved heterogeneity, they may also reduce the ability of the analysis to detect longer run impacts of an intervention that may be reflected in different levels or time trends of outcomes across observational units (Munnell 1992).

Selection-bias corrected regression model. Another approach to address selection bias is to incorporate the effect of selection bias explicitly into a regression model, allowing estimation of

[61] The only difference between this regression-based DD estimator and the simple DD estimator discussed above is that the regression-based DD estimator can include other control variables in the model.

the impact of the program after correcting for the bias (Maddala 1983).[62] This addresses the problem of selection bias, but relies on assumptions about the distribution of the regression error term (such as assuming normally distributed errors), as well as other parametric assumptions of the regression model.[63] Furthermore, the selection bias corrected regression may produce imprecise estimates of the effect of the treatment unless some observed variables are good predictors of participation in the program but do not affect outcomes directly.[64] Such variables are called *instrumental variables*; identifying such variables depends on the assumption that these variables do not affect outcomes directly.

Instrumental variables regression. Instrumental variables (IV) regression involves using instrumental variables that are assumed to be uncorrelated with the error term in the regression to predict the value of possibly endogenous explanatory variables in the regression (i.e., variables that may be correlated with the error term). In linear regression models, some of these variables must be excludable as explanatory variables in the regression to be able to estimate the regression model.[65] IV estimation is said to be "as good as randomization" if such variables are available, because, like a random assignment, they are uncorrelated with unobserved factors affecting outcomes. An example of a valid instrumental variable could be an arbitrary condition limiting eligibility of participants in a program, such as selecting the eligible population based on the alphabetic position of their names. As long as the rule used to determine eligibility does not affect outcomes (other than by affecting eligibility), it is valid to exclude from the regression model that determines outcomes. If valid instrumental variables are available, it may be feasible to estimate the program impact without any parametric assumptions about the regression error term (Heckman and Robb 1985). However, unless the instrumental variables are strong predictors of program participation, IV estimation can yield more biased estimates than OLS regression (Bound et al. 1995). The strength of the instrumental variables in predicting participation can be tested and the results of such tests used to determine the potential size of bias caused by weak instruments. If more instrumental variables are available than the number of endogenous variables in the regression, it is possible to test the assumed lack of correlation of the instrumental variables with the error term in the regression with an *overidentification test* (Wooldridge 2010).[66]

Generalized method of moments estimators. A generalization of instrumental variables estimation is generalized method of moments (GMM) estimation, which treats the assumed zero

---

[62] This approach is called the "control function" approach in some literature.

[63] Distributional assumptions in these models can be tested, however, and more generalized distributions can be used (Maddala 1983).

[64] In the absence of such instrumental variables, estimation of the impact parameter relies solely on assumptions about the distribution of the error term (Heckman and Robb 1985). Furthermore, without instrumental variables, the estimation may be imprecise even if identification is technically feasible based on distributional assumptions, because the selection correction term is predicted by the same variables included in the regression equation, often causing a high degree of multicollinearity.

[65] Intuitively, if all of the instrumental variables used to predict the endogenous explanatory variables are also included as separate explanatory variables in the regression model, there will not be independent variation of the predicted endogenous explanatory variables from the other explanatory variables in the model, and thus no way to identify the independent effects of the predicted endogenous explanatory variables.

[66] Intuitively, an overidentification test tests how different the coefficients in the IV regression model are if different sets of instruments are used to predict the endogenous explanatory variables. If the instrumental variables are all valid (i.e., not correlated with the error in the regression), using different combinations of the instruments should have little effect on the estimated coefficients.

correlation between each instrumental variable and the error term as a moment condition to attempt to satisfy in the observed data (Wooldridge 2010). With panel data having a sufficient number of time periods for each observational unit, a large number of potential instrumental variables and moment conditions may be available using sufficiently lagged values of the variables, as long as serial correlation in the error term (i.e., the correlation of errors over time) is zero or of low order (i.e., if serial correlation occurs, it only is present for a few time periods) (Arellano and Bond 1991).[67] Arellano and Bond (1991) provided a test for serial correlation in such panel data models, which if found to be of low order can result in valid GMM estimates of impacts. Other concerns for IV regressions are also present for GMM regressions, such as the strength of the instruments and consistency of the moment restrictions.[68]

Regression discontinuity designs. One of the strongest econometric approaches, in terms of minimal reliance on untestable assumptions, is the regression discontinuity (RD) design. An RD design is appropriate when a threshold value of some continuous variable is used to determine eligibility for a program; e.g., if all applicants for a program are ranked according to some relatively continuous measure (for example the credit scores of applicants for a loan program), with all applicants having a value of the measure above some threshold being eligible and all those with ranks below the threshold being ineligible. The RD approach involves looking for an abrupt change (discontinuity) in the outcome variable in the vicinity of the threshold ranking, considering both eligible and ineligible applicants. Assuming that in the absence of the program there would be a continuous relationship between the measure used for the selection and expected outcomes in the absence of the program, a discontinuity at the threshold provides evidence of impacts of the program. This design has advantages over other QED and regression methods, since it does not rely on assuming that the unobserved factors affecting outcomes are fixed or that instrumental variables are available that are not correlated with those factors. But it does assume that the program reliably applies the selection rule. Some deviations from the decision rule may not prevent valid estimation of impacts, provided that a sufficient number of observations following the rule are found in the vicinity of the threshold value.

Combinations of Methods

Some of these methods can be combined to address weaknesses present in different estimators. For example, DD estimation can be combined with multiple regression or matching methods to help assure that the beneficiary and comparison groups are similar in observed characteristics and outcomes prior to the program intervention, while subtracting out the effects of unobserved fixed factors. Some combined approaches such as matched DD estimation can yield estimates that are comparable to results of RCTs (Heckman et al. 1998).

Statistical Approaches to Estimating Benefits and Costs

Many studies have estimated the productivity impacts of the public capital stock in the United States and elsewhere by estimating an aggregate production function at a national or regional level (typically state level in the United States), including the value of the stock of public capital

[67] Intuitively, if there is serial correlation in the error term, the lags of variables may be correlated with the current value of the error term, and thus are not valid instrumental variables.

[68] Unfortunately, in practice the strength of the set of instruments used in a panel GMM estimation or the results of overidentification tests are often not reported, limiting readers' ability to gauge the validity of the GMM model.

as an argument of the production function.[69] These studies usually estimate the *output elasticity of public capital* – defined as the percentage increase in output resulting from a 1 percent increase in the capital stock, controlling for other factors affecting production. These elasticity estimates can be used to estimate the marginal rate of return on the public capital stock, by multiplying the output elasticity by the ratio of output to the public capital stock. Although this is a simple calculation, unfortunately few studies have provided estimates of the marginal return to public capital. In a later section, we provide estimates of this rate of return based on the mean values of the output elasticities estimated by U.S. studies for different types of public capital, separately for studies conducted using national vs. state level data (since these studies have large differences in estimated output elasticities).

A few studies reviewed estimated the impacts of the value of infrastructure on the costs or profits of businesses (Morrison and Schwartz 1996; Vijverberg et al. 1997; Cohen and Morrison 2003). These estimates can be used to estimate the marginal return to infrastructure investments in terms of reduced costs or increased profits. The estimates reported by the authors for this parameter are reported in the results section below.

A third econometric approach used to estimate the productivity impacts of public capital stocks is to use a vector autoregression (VAR) to explore intertemporal relationships among output, public capital investment, private capital investment, and employment. One study was found that used this approach for the United States (Pereira 2000) and is discussed in this paper.

A fourth approach to estimating the benefits of infrastructure stocks is based on the spatial equilibrium theory of Roback (1982), which predicts that if workers and firms can move freely, the value of infrastructure will be reflected in interregional variations in wages and land rents (or property values). This approach has an advantage over the previous approaches, in that it values non-marketed amenity benefits as well as productivity benefits of infrastructure and can determine how much of the total benefit is due to each type of benefit. Two studies discussed in this paper (Haughwout 2002; Albouy and Farahani 2017) used this approach to value infrastructure stocks; the second of which relaxes the assumption of free mobility of workers and some other common assumptions of the spatial equilibrium model.

The hedonic spatial equilibrium approach can be applied to valuing individual types of infrastructure as well. However, our review of U.S. studies focused on broadband, electricity systems, and water and sewer systems found only a few studies that estimated impacts of infrastructure on property value or on wages and no studies that combined estimated impacts on both wages and property values to estimate the benefits of infrastructure.

[69] For example, Aschauer (1989); Ram and Ramsey (1989); Munnell (1990a; 1990b); Duffy-Deno and Eberts (1991); Eisner (1991); Tatom (1991); Garcia-Mila and McGuire (1992); Finn (1993); Munnell (1993); Eisner (1994); Evans and Karras (1994); Holtz-Eakin (1994); Ai and Cassou (1995); Andrews and Swanson (1995); Baltagi and Pinnoi (1995); Holtz-Eakin and Schwartz (1995a, 1995b); Garcia-Mila et al. (1996); Holtz-Eakin and Lovely (1996); Crowder and Himarios (1997); Kelejian and Robinson (1997); Vijverberg et al. (1997); Erenburg (1998); Boarnet (1998); Nourzad (1998); Delorme et al. (1999); Yamarik (2000); Berechman et al. (2006); Sloboda and Yao (2008); and Cohen (2010) for the United States. See the recent meta-analyses by Bom and Ligthart (2014) and Núñez-Serrano and Velázquez (2017) for references to studies of other countries and cross country studies.

Estimating Consumer Surplus

One of the most common methods used to estimate a program's benefits is a surplus approach (for example, used by the Millennium Challenge Corporation in their evaluation of road rehabilitation projects in Armenia and Burkina Faso – see the section on developing country impacts). Figure 1 illustrates the basic idea behind this calculation.

Figure 1 shows the demand curve for transportation, which relates how many "units of transportation" would be "consumed" with different unit-prices of transportation. With an initial cost C0 of transportation, T0 units are consumed. If the transportation price drops from C0 to C1, the accompanying shift in traffic would be T1-T0. Then the surplus generated by the project would be comprised of two areas. The first area is A (in red) and represents the gains from existing traffic (i.e., each of the units already consumed valued at the price differential). The second area is B (in blue) and represents the gains from new "generated" traffic: each of the additional units of traffic that were not consumed before the project (and are consumed after the project) appraised at the unit value determined by the demand schedule.

The implementation of this methodology requires collecting (or assuming) data on traffic for the rehabilitated road. This data allows us to determine the characteristics of the vehicle fleet that usually travels that road. Each type of vehicle (e.g., truck, automobile, motorcycle, etc.) is assumed to have a certain type of motor and tires. Fuel consumption and occupancy are also assumed for each type. The reduction in travel cost (the difference between C0 and C1) is then estimated using engineering models (such as the Highway Development and Management Model, or HDM) that are based on parameters for reduced vehicle depreciation rates (motor and tires), decreases in fuel consumption, and time savings (i.e., travel time reductions multiplied by average hourly wages). All of these components provide the cost reduction per travel unit.

We can readily apply these cost reductions to existing traffic levels to estimate the red rectangle (A) in Figure 1. However, it is more difficult to estimate the blue triangle (B). For this, we would need to know the level of additional traffic that would be generated by the road improvement. Some studies estimate B to be half of A as a rule-of-thumb; however, this approximation overstates the true impact when travel demand is somewhat elastic and understates it when demand is inelastic. The real size of this triangle is hard to calculate without the demand curve (of which little is typically known).

Another approach is to collect traffic data before and after the project is completed; the difference in traffic can be used to estimate the distance T1-T0 and the demand slope can be determined by extrapolating the points (T1, C1) and (T0, C0). While certainly more rigorous than the previous rule-of-thumb, this methodology has disadvantages of its own. Measurements of traffic before and after the project do not necessarily provide an accurate measure of traffic generated by the road itself. For example, if there are any other factors affecting traffic other than road construction (i.e., simplified customs for imported cars, increases in income that allow more families to own cars, etc.), the difference cannot be wholly attributed to the project.

Using Household Production Functions to Estimate Consumer Surplus

Most of the recent impact evaluation studies of infrastructure in developing countries use a different approach to estimate consumer surplus. Instead of trying to estimate transportation demand from the changes in the number of trips and the changes in the road quality, they treat it as an input in the production function of rural households. In this light, assume that x is transportation and $F(x)$ determines the level of household production Y that corresponds to each level of this input[70]. The demand for factor x is then determined by its marginal productivity (i.e. $F'(x)$); the person's willingness to pay for an additional unit of x is precisely what this additional unit would produce.

Figure 2 depicts hypothetical schedules for a production function $F(x)$ and the input demand for x. The input demand is determined by the slope of $F(x)$ throughout the range of x: $F'(x)$. When the price of factor x is P_0, the household demands units of x until $F'(x_0) = P_0$ (analogously, when the price reduces $F'(x_1) = P_1$). Rather than estimating the demand curve (or making any assumptions) for transportation, one can estimate the difference between $Y_1 = F(x_1)$ and $Y_0 = F(x_0)$. Because the demand curve for x is its marginal productivity, the area under $F'(x)$ between x_0 and x_1 is equivalent to $Y_1 - Y_0$: $\int_0^{x_1} F'(x)dx - \int_0^{x_0} F'(x)dx = F(x_1) - F(x_0)$.

Thus, these methodologies rely on directly measuring the change in household production or income derived from the transportation project. This approach has several advantages. First, it allows one to gauge the benefits of the project from observed changes in income, which does not require any assumptions about the input demand function or the production function. Second, it does not need to rely on assumptions regarding depreciation factors or data to measure households' time savings. Third, instead of capturing benefits from traffic flows as in the HDM models (which include foreign companies, large firms in the cities, etc.), it can restrict the analysis to the population of intended beneficiaries of the project. Finally, using micro data enables one to capture heterogeneous treatment effects, which allows analysis of the distributional impacts of the project.

Model-Based Approaches

An alternative or supplement to statistical methods of estimating impacts, costs, and benefits of infrastructure investment is to use engineering, economic, and/or other types of models to predict these types of measures. An example of an engineering model is the Highway Development and Management Model mentioned previously, developed by the World Bank to estimate reductions in travel costs resulting from highway investments.

The Federal Highway Administration (FHWA) of the U.S. Department of Transportation conducts CBAs of potential alternative investment scenarios in highways using an engineering/economic model – the Highway Economic Requirements System (HERS) – that is based on data on pavement conditions, geometry, traffic volumes, vehicle mix, and other measured highway characteristics for individual highway sections, and models of predicted

[70] As usual, we assume that the production function is increasing and concave in x. We normalize the output price to 1 so that $F(x)$ is also a revenue function. However, assuming any other output price does not affect this idea.

changes in pavement conditions, travel time savings and their value, changes in vehicle capital costs, and changes in costs of air pollution predicted to result from alternative investment options (FHWA 2015). FWHA uses the National Bridge Investment Analysis System (NBIAS) – a mathematical programming model – to analyze impacts of alternative bridge maintenance policies and improvement investments and their benefits and costs. These models are focused on the economic and air pollution costs and benefits of highway construction, and do not estimate benefits or costs associated with various environmental, health, or community impacts of highway or bridge maintenance or construction. The models are deterministic, but the effects of uncertainty are addressed using sensitivity analysis.

HERS has been linked to the United States Applied General Equilibrium (USAGE) model developed at Monash University to enable analysis of the impacts of highway investment on U.S. macroeconomic performance. Computable general equilibrium (CGE) models, such as USAGE, estimate impacts in an economy resulting from some change, in principle accounting for all changes in quantities and prices of goods, services, and factors of production (land, labor, and capital) and in income flows resulting from an initial change; based on assumptions of equilibrium in most or all markets (i.e., prices adjust to equate supply and demand). These models are most useful for analyzing impacts of large changes that cause changes in prices and income flows in the economy. They are a generalization of partial equilibrium modeling approaches – such as the consumer surplus model discussed above – which focus on changes in one or a set of markets without investigating economic impacts on the entire set of markets in the economy.