Econometric Evidence on the Benefits of Infrastructure Investment: an Australian Perspective

Working Paper

This Working Paper forms part of a research project investigating into certain issues concerned with measuring the benefits of investment in transport infrastructure. The focus of the project is on possible benefits from increased employment; and benefits often claimed to be significant but understated by benefit-cost analyses, especially; cost savings from business logistic responses to improvements in infrastructure (for example, substitution of transport for inventory); rural regional development benefits; and the indirect benefits that an item of transport infrastructure provides to non-users of that infrastructure.

Bureau of Transport and Communications Economics

WORKING PAPER 25

ECONOMETRIC EVIDENCE ON THE BENEFITS OF INFRASTRUCTURE INVESTMENT: AN AUSTRALIAN TRANSPORT PERSPECTIVE

ISSN 1036-739X
ISBN 0 642 25254 8

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Printed by the Department of Transport and Regional Development, Canberra

FOREWORD

This working paper forms part of a research project being conducted by the Bureau of Transport and Communications Economics (BTCE). The project is an investigation into certain issues in measuring the benefits of investment in transport infrastructure. The focus of the project is on:

- possible benefits from increased employment; and
- benefits often claimed to be significant but understated by benefit–cost analyses, especially;
  - cost savings from business logistic responses to improvements in infrastructure (for example, substitution of transport for inventory);
  - rural regional development benefits; and
  - the indirect benefits that an item of transport infrastructure provides to non-users of that infrastructure.

To determine the adequacy of current methods for measuring these benefits, and to set directions for improvements, BTCE has been conducting a literature review as well as case studies of infrastructure investments. The final report on the project is scheduled for release this year.

The case studies are mainly concerned with the regional development effects. The pilot case study examined the effects of two highway bypasses in rural New South Wales; a second study examined a proposed inland railway between Brisbane and Melbourne (BTCE 1994a, 1996a). (For brief descriptions of these studies see BTCE 1994b and 1996b.)

The literature under review includes studies using system models of economies to examine the effects of transport investments. These system (or 'general equilibrium') models indicate the effects on other sectors of the economy besides transport, whereas benefit–cost analyses tend to focus on outcomes within the transport sector. Studies using the ORANI model of the Australian economy to evaluate transport investments were reviewed in BTCE (1995). Following that review, BTCE used the ORANI model in analysing the employment effects of road construction activity (1996c).

In reviewing another strand of the relevant literature, this paper focuses on evidence most relevant to Australia, and caters to lay readers interested in infrastructure policy as well as to economists. (Technical details go in the appendix.) Data issues are explored in greater depth than in other reviews of the macro-econometric literature on infrastructure.

David Luskin prepared this paper and is leading the broader project to which it contributes. Other BTCE staff provided some research assistance (Emil Elinon) and editorial input (Maureen Wright). Glenn Otto, an economist at the University of New South Wales, kindly provided comments on a draft of this paper.

David Luck
Research Manager
Land Branch

Bureau of Transport and Communications Economics
Canberra

May 1996

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ABSTRACT

This paper considers the macro-econometric evidence on benefits from infrastructure investments, particularly as it relates to Australian transport. It argues that macro-econometric analysis is a very low-powered tool for estimating these benefits. Data problems are examined here in greater depth than in previous reviews of the evidence and are shown to be serious. The generalisations about infrastructure that come from the macro-econometric studies are mostly too broad to be useful for policy, and the findings are very sensitive to changes in data and model specification. Proponents of infrastructure spending have focused on findings of large returns, but studies have also produced findings of small and even negative returns. The contribution of further macro-econometric research to an evaluation of Australia's infrastructure needs is doubtful; far more could be learned from benefit–cost analyses of individual investments.

INTRODUCTION

The econometric studies reviewed in this paper attempt quantitative generalisations about the effects of infrastructure on the economy. They examine the role of roads and other infrastructure in facilitating production by providing essential inputs. In other words, they focus on the supply-side effects of having infrastructure in place. The possibility of a demand stimulus to the economy from infrastructure construction activity is not the subject of these studies.[1]

THE NATURE OF THESE STUDIES

Econometric analysis can easily mystify the lay reader, so we start with a simple description of the studies to be reviewed.

Infrastructure measured by public capital

'Infrastructure' lacks precise definition. Basically, it refers to types of capital inputs which are considered essential to society and which are often provided by governments. In the econometric literature under review, it normally refers to government-owned fixed capital (equipment, buildings and structures). Military capital is excluded. For reasons to be discussed, the definition also sometimes excludes the capital of government business enterprises (GBEs) like public utilities. This leaves the fixed capital of the 'general government' sector, which provides largely unpriced services like roads, education and regulation. Data limitations normally prevent the inclusion of private capital which has infrastructure characteristics.

Focus on private sector output

Measurement problems also explain why many of the studies consider the contribution of infrastructure to private sector output, but not public output. The output of the general government sector is mostly non-priced and intangible. How does one measure the output of government regulatory

1. At least, the possible demand stimulus is not what the studies attempt to measure. In practice, estimates of infrastructure payoffs from some of these studies might reflect such a stimulus, as recognised by Gramlich (1995). For an analysis of the employment effects of road construction activity, see BTCE (1996c).

agencies, for example? The national accounts measure the output of general government services by the expenditures on inputs used to produce them, but this does not permit measurement of productivity gains. Better roads may reduce business travel time for government safety inspectors. While a given workforce of inspectors could then achieve more, this increase in output from their services would not show up in the national accounts as an increase in public output.

Findings are broad generalisations

The econometric studies reviewed in this paper aspire to rather high-order generalisations. These may be 'rules of thumb' about how much private sector output will increase for an additional $1 billion worth of infrastructure. The lower-order generalisations in these studies distinguish broad categories of infrastructure or industries within the private sector, but are fairly sweeping nonetheless. Otto and Voss (1993), in their analysis of Australian data, distinguished only between 'road' and 'other' infrastructure. Because of their focus on the 'big picture', we refer to the studies under review as 'macro-econometric'. The studies mostly examine the effects of infrastructure on output, though some examine the effects on production costs or profits. This distinction need not concern us here, so we refer to the estimated effects of infrastructure as output gains.

How generalisations are reached

The other defining characteristic of the studies under review is their short-cut path to generalisations. A review of case study evidence on individual infrastructure investments might seem the natural path to generalisations, but this is not what the econometric studies do. Instead, they apply statistical tools to fairly aggregated data. Suppose the researcher is examining variations in private sector output between states, as has been done in many US studies. The dataset includes a valuation of the total stock of infrastructure in each state. However, simply comparing output with levels of infrastructure will mislead, if only because many factors other than infrastructure provision determine the variation between states in private sector output. So the researcher will add other explanatory variables to the analysis, such as the amounts of private sector labour and capital in each state, and use statistical tools to try to disentangle the separate influence of each variable. Using these tools is rather like doing a large cross-tabulation, but with simplifying assumptions to make the analysis more tractable.

Value of the macro-econometric approach for evaluating transport investments

Macro-econometric studies of infrastructure payoffs could, in concept, help evaluate proposals for transport infrastructure investment. Such proposals are often not accompanied by an analysis of their economic effects, even when the investments are large. Moreover, when such analyses are performed, they often attract the criticisms that benefits have been either exaggerated or understated. For both these reasons, the econometric studies might provide a useful check on the available evidence from evaluations of transport investments. Econometric evidence on the returns to non-transport infrastructure might also help evaluate transport infrastructure investments, since alternative investments compete for limited government funds.

Datasets

The dataset for a macro-econometric analysis of infrastructure payoffs should contain ample variation in infrastructure levels and the other explanatory variables, if reliable findings are to be obtained. Datasets showing variations over time only, and not across place, were used in Aschauer (1989) and many related studies, including all the econometric studies of infrastructure payoffs conducted thus far with Australian data. These purely time-series datasets might conceivably permit an analysis of the effects of infrastructure 'in general', but they usually lack sufficient variation to permit reliable estimates for specific categories of infrastructure, as has been conceded by Aschauer (1992).

Estimation possibilities improve when the dataset adds variation over place to variation over time, as in several American studies of infrastructure payoffs that combine time-series for different states. The addition of cross-state variation creates its own problems, though, particularly with capturing the benefits from one state's infrastructure that accrue to other states. Cross-country variation has also been used in estimating infrastructure payoffs, with attendant problems in data comparability.

Tests of statistical significance

Statistical tests of significance aim to distinguish real patterns from chance variation. A finding is 'significant' when the test indicates a sufficiently small probability that the finding arises from chance variation alone. Usually, 'sufficiently small' means less than 5 per cent, though other levels of significance are also used.

WHAT DO THE MACRO-ECONOMETRIC STUDIES FIND?

Aschauer (1989) launched the recent spate of econometric research on infrastructure, with his analysis of US data that indicated huge gains in private output from additional public infrastructure. For 1991, his findings imply that if $1 billion more infrastructure had been in place, private sector output would have been at least $940 million higher than it actually was. (Infrastructure in his study appears to include GBE capital.) Many subsequent studies have produced similar findings for the US and some other countries, including Australia. (Dowrick 1994 surveys the Australian evidence, to which Kearney 1995 has recently added.) The more recent of these studies have used more sophisticated modelling techniques and have produced estimates of infrastructure payoffs smaller than what Aschauer found—only about half as large according to one review—but fairly large nonetheless (see Otto & Voss 1995). Such findings are all the more remarkable considering that they reflect benefits in private sector productivity only. Benefits from increased public sector productivity would be additional, as would be benefits accruing directly to consumers (for example, savings in travel time and vehicle operating costs for non-business travel).

On a closer look, however, the macro-econometric literature also includes careful studies finding small payoffs to infrastructure. Kelejian and Robinson (forthcoming) stands out for its relatively thorough sensitivity testing. Analysing US data on the 48 continental states from 1972 to 1985, the authors could not confirm that infrastructure has positive effects on private output, let alone that the effects are large, despite their attempt to capture spillover effects across states. Only their basic model specification produced significantly positive estimates of infrastructure payoffs. Specifications that incorporated complications ignored in the basic model produced corresponding estimates that were mostly negative and significantly so in some cases. The complications considered would be familiar to economists: fixed state-level effects, autocorrelation; heteroskedasticity; endogeneity of some explanatory variables (but not the infrastructure variables). None of these complications are spurious ones designed to produce a certain result. (Aschauer argues, with some justification, that researchers have resorted to odd specifications of econometric models in their efforts to debunk his findings; see Aschauer 1993.)

Findings of insignificant infrastructure payoffs have also emerged from some analyses using national time-series. These studies concentrate on the possibility that the estimated relationship between infrastructure and output is coincidental. Examples abound of tight correlations between economic variables that have little logical connection. Aschauer (1992) refers to findings of such a correlation between productivity of the US private economy and other variables—the dollar/yen exchange rate and the proportion of the population aged five to fifteen. Neither of these correlations make much sense and are almost surely coincidental. Although Aschauer rightly objects to including nonsensical variables in an econometric model, the good statistical performance of some such variables adds to suspicions that the good performance of theoretically sounder variables may also partly reflect coincidence. Econometric studies attempting to adjust for coincidental correlations through the use of special techniques (differencing) have produced mixed findings about infrastructure payoffs. Otto and Voss (1994a) estimated the payoffs to be still statistically significant, but 'fairly modest', with annual private output in Australia rising by only $40 million for an additional $1 billion worth of infrastructure. Harmatuck (1996) obtained a similar estimate with US data. Ford and Poret (1991) found no significant evidence of infrastructure payoffs in Australia and in five of the other ten OECD countries they considered. Gramlich (1995), in his excellent review of the econometric literature on infrastructure, cites US time-series analyses that find insignificant payoffs after adjustments for coincidental correlation.

Findings on transport infrastructure

Australian evidence

Otto and Voss (1993) conducted an econometric analysis of the payoffs to road capital in Australia, as an input to a report for the Australian Automobile Association (see Allen Consulting 1993). They conclude that increases in the public capital stock substantially boost private sector output, particularly when the additional capital is in the form of roads. For 1991–92, the implied output gain from having an additional $1 billion of road capital in place is $940 million, a first-year rate of return of over 90 per cent. For an extra $1 billion of non-road public capital, the corresponding implied output gain is $350 million. (All these figures are in 1989–90$, and the public capital stock excludes GBE capital.) Otto and Voss suggest on the basis of their findings that reallocating public investment toward roads would boost private sector output.

The Otto and Voss findings about road capital rest on rather shaky statistical foundations and, in the BTCE's view, do not establish anything about the effects of reallocating public investment. The finding that road capital produces larger private output gains than does other public capital follows automatically from a fairly arbitrary assumption. The authors assume, roughly speaking, that a one per cent increase in road capital will have the same effect on private output as a one per cent increase in other public capital—in technical parlance, that the *elasticities* are equal. Now, the stock of other public capital is much larger than the stock of road capital (in 1991–92, almost three times larger). Hence, the assumption of equal elasticities automatically produces the authors' finding—that the output gain from an increase in road capital will exceed that from the same *dollar* increase in other public capital.

So what is the basis for the Otto and Voss assumption of equal elasticities? The assumption passed a statistical test which Otto and Voss conducted, but the test was a weak one. Passing indicates a lack of evidence that the assumption is extremely unlikely to hold in reality. An assumption that passes a test of extreme unlikelihood is not necessarily a good one. Given that the assumption in question is so central to the study's results, its adoption should have sound theoretical justification. The authors provide no such justification and none is apparent. Rather, the justification offered is pragmatic: that the stocks of road and non-road public capital move in so similar a way over time as to preclude estimation of their separate effects in a time-series dataset. This is the common problem of *multicollinearity* between explanatory variables, for which the best remedy is finding a better dataset. The authors' way around this problem—assuming equal elasticities for road and non-road public capital—is more disguise than solution.[2] When the authors estimate their model without this dubious assumption, the estimated effects of road and non-road public capital are both insignificant. Another ground for treating cautiously the authors' findings of large returns to road capital is that, in the study being discussed, they could not employ techniques to deal with coincidental correlation or with the problem of reverse causality discussed below. (The sample was too small.)

Basically, the results of the Otto and Voss study on road capital confirm Aschauer's point: time-series data alone provide a weak basis for estimating the payoffs to different types of infrastructure. There do not appear to be any studies that have used cross-state or other cross-sectional variation within Australia to derive generalisations about infrastructure payoffs. Such analyses have been performed for other countries, however.

Overseas evidence

Macro-econometric studies using overseas data have produced very mixed findings about payoffs to transport infrastructure. Diversity exists even among studies using similar data. FHWA (1992) provides a good, but slightly dated, review of econometric evidence on returns to road capital in the US.

Estimates of returns to US road capital come partly from studies combining time-series for the 48 continental states. McGuire (1992) finds that states increasing their road capital stock realise significant gains in private sector output, whereas Kelejian and Robinson (forthcoming) find no evidence of such payoffs.

Other econometric estimates of the returns to road capital in the US come from time-series analyses at the industry level. JFA (1994) measured the returns in cost savings to manufacturing and arrived at the 'highly tentative' result that new highway investments were earning real rates of return of about one per cent post-1980, down considerably from the study's estimates of 6.7 per cent pre-1978 (see Holleyman 1996 for a summary of the analysis). Nadiri and

2. Moreover, it is not the only possible expedient. Alternatively, one might assume equal elasticities for private capital and public non-road capital. This assumption might also pass a statistical test of extreme unlikelihood, and is not obviously less plausible than the assumption of equal elasticities for the two types of public capital. Indeed, when Otto and Voss estimate their model with no such assumptions, the elasticity for non-road public capital is much closer to the elasticity for private capital than to that for road capital.

Mamuneas (forthcoming) have conducted a similar analysis with US time-series data on a broader range of industries. Neither study estimates the effect of public infrastructure other than roads.

Cross-national datasets have also underpinned some econometric estimates of transport infrastructure payoffs. Canning and Fay (1993) analysed data on 96 countries under several model specifications, featuring alternative assumptions about how returns to infrastructure investment vary over the payback period. Returns were measured by gains in real GDP. Only some of the model specifications yielded statistically significant evidence of positive returns, and the authors did not test whether these specifications fit the data better than the other specifications. (As they explain it, their database contained insufficient observations over time to conduct such tests.) Based on one specification only, they estimate that in high-income countries, additional road infrastructure earns an annual rate of return of around 10–20 per cent.[3] A similar study by Easterly and Rebelo (1994) reported finding 'supernormal' returns to transport and communications infrastructure.

LIMITATIONS OF THE ECONOMETRIC STUDIES

The econometric literature under review has attracted a barrage of criticisms. A principal complaint is that findings vary too much to provide any useful conclusions. As we have seen, some studies produce large estimates of infrastructure payoffs, while others fail to confirm any payoffs, even where the same country and category of infrastructure are concerned. Jorgenson (1991) has gone so far as to suggest that macro-econometric analysis of infrastructure payoffs cannot provide evidence clear enough to be useful for policy purposes, owing to limitations of this approach.

The limitations of the macro-econometric analyses can relate to the model specification or to the database. One source of misspecification is the omission of some variable that belongs in the model—the price of oil, perhaps. (Whether the oil belongs or not has sparked some debate; see Aschauer 1992 and Kearney

3. This specification assumes, in essence, that the annual returns to an infrastructure investment constantly increase over the payback period. This could make sense in an economy with sources of growth other than the additional infrastructure. Economic growth can increase the returns by increasing the usage of the infrastructure. For example, a growing labour force could result in more workers travelling the roads on business and, hence, in returns to a road investment increasing over time (at least before congestion sets in). But the specification favoured by Canning and Faye seems to embody the extreme assumption that the returns would go on increasing even in the absence of economic growth due to other factors. For this situation, a more realistic assumption is that returns stabilise after people have adjusted to having the new infrastructure in place; this is the assumption embodied in alternative model specifications which Canning and Faye used, and some of these specifications provide no significant evidence of transport infrastructure payoffs. The lack of realism in the specification favoured by Canning and Faye carries over to more normal scenarios where infrastructure investment is one of many sources of economic growth.

1995). Data problems exist even in a properly specified model, since the data on most variables will be flawed. Reviews of the macro-econometric literature on infrastructure payoffs have tended to focus on specifications issues, so data problems, which can be equally important, receive more attention than usual in the following discussion.[4]

Specification problems

Reverse causation

An increase in national or state output can place greater demands on existing infrastructure. Businesses may need more inputs of infrastructure services to achieve the increase in output; and consumers, with their incomes boosted by the output gains, are likely to increase their use of infrastructure, as by travelling more. The greater demands placed on infrastructure may, in turn, cause governments to respond by increasing infrastructure investment. The causal chain just sketched, running from more output to more infrastructure, is the reverse of the causal relationship that has chiefly preoccupied the macro-econometric studies on infrastructure. Reflecting this preoccupation, we have presented the findings thus far as estimates of the effects of infrastructure on output, which is what the studies were basically trying to estimate. Nevertheless, many of the findings could be overestimates of these effects, since a positive relationship between output and infrastructure may partly reflect reverse causation. (Another possibility, already mentioned, is that the relationships are partly coincidental.)

Some macro-econometric studies have attempted to deal with reverse causation through special statistical techniques and have still found large returns to infrastructure (see again Gramlich's 1995 review). Otto and Voss (1994a), while finding no evidence of reverse causation being a problem in their dataset, cautioned in a later publication that 'more work on this issue is clearly required' (1995). Likewise, Roy (1994) reports a consensus at a recent OECD conference that doubt remains about the direction of causality in the macro-econometric findings on infrastructure and output.[5]

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4. Gramlich (1995) is probably the best for a concise and balanced review of the international literature; also worth a look are Gillen (1996) and Nadiri and Mamuneas (forthcoming). Dowrick (1994) and Kearney (1995) review the literature from an Australian perspective.

5. Dealing with reverse causation appears to be difficult. The use of instrumental variable techniques, for example, requires data on one or more variables that can be assumed to affect output only through their effect on the level of infrastructure investment. A variable does not qualify as an instrument if it has some effect on output over and above its effect on the level of infrastructure investment. It is a matter of judgement which variables qualify as instruments, and some studies, like Easterly and Rebelo (1993), provide no theoretical justification for their choice. Moreover, some variables that do qualify may be rather poor instruments because they are weakly related to the level of infrastructure investment. In this case, using these variables as instruments may give less reliable results than simply ignoring

Omitted variables

Whether an econometric analysis indicates large or small output gains from infrastructure can depend on which other variables are recognised in the model as determinants of output. Standard inclusions in these models are variables for inputs of labour and of non-infrastructure capital. Other variables such as business cycle indicators and the price of oil also sometimes appear. (The oil price shock of the 1970s may have contributed to a productivity slow-down in some countries.) However, data constraints prevent the inclusion of all variables that are theoretically relevant. Data may simply be unavailable for some variables. Also, the number of observations in the dataset places a maximum on the number of explanatory variables. With observations for only twenty years, an econometric analysis could incorporate at most nineteen variables explaining the economy's output. Even below that maximum, attempting to disentangle the effects of very many explanatory variables would likely result in a statistical muddle. This practical constraint often forces the omission of some explanatory variables that could be quite relevant. Since there is an element of arbitrariness in deciding which ones to omit, a prudent course is to experiment with alternative sets of explanatory variables and to compare the findings. Sensitivity testing of this sort normally features in macro-econometric studies, but usually not enough to allay suspicions that the findings have been manipulated.

The potential for manipulation of findings emerges clearly from Levine and Renelt (1992), who analysed cross-country variation in growth rates of national output (real GDP). The authors identified a large set of explanatory variables that looked like important determinants of a country's growth rate, based on past studies and economic theory.[6] When their model of economic growth included many of these explanatory variables, hardly any variable was estimated to influence economic growth significantly. For models containing fewer variables, they undertook fairly comprehensive sensitivity testing with respect to the choice of variables, as recommended by Leamer (1983). In most cases, a variable that appeared statistically significant in one model lost its significance after the model was varied to include a few extra variables. The estimated effect of the infrastructure variable—the ratio of government capital formation to GDP—failed to be consistently positive and significant.

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the problem of reverse causation. Canning and Faye (1993) report this lack of success in their use of instrumental variables.

6. The core variables were measures of investment, education, population growth, and the initial level of real GDP per capita. The rationale for the latter variable is that initially poorer countries are technologically backward, and tend to grow more rapidly than other countries as their technology catches up. Other variables with which Levine and Renelt experimented were monetary and political indicators; measures of international trade patterns and policies; and measures of government fiscal-expenditure patterns.

To claim that sensitivity testing can destroy any macro-econometric finding would, however, exaggerate. There were a few points of stability in the Levine and Renelt analysis, including a positive correlation between output growth and the ratio of total investment to GDP. The lesson is that points of stability can emerge only through comprehensive sensitivity testing.

Lagged effects

The question of how the output gains from an infrastructure investment vary over the payback period has received little serious attention in macro-econometric studies. In reality, the output gains may lag the construction of the infrastructure to some extent. The gains from a new road may emerge gradually as producers relocate and find other ways to take advantage of the road. Investment in educational infrastructure raises productivity by improving the skills of the workforce, but usually only some years after the investment is made. The macro-econometric studies tend to assume that payoffs are immediate because the data they use are inadequate for properly analysing lagged effects.[7]

Choice of infrastructure variables

Non-capital inputs: Public provision of infrastructure services requires inputs like labour and fuel, in addition to capital (such as the railway lines and power plants). Most macro-econometric studies ignore these other inputs, and this may distort the findings for infrastructure capital. Otto and Voss (1994a), in their analysis of Australian data, included government consumption expenditure to allow for non-capital infrastructure inputs, and found this variable to have little explanatory power.

GBE capital: The rationale for including GBE capital in infrastructure capital has received scant attention in the macro-econometric studies choosing to do this. Otto and Voss included GBE capital in one study (1994b) and excluded it in another (1994a) without explanation. Dowrick (1994) observes that GBE capital can affect private sector productivity differently from general government capital, because of the greater role of user charges in financing the former. If only for this reason, GBE capital should enter the macro-econometric models separately from general government capital, if it is entered at all.

Composition of infrastructure: The shortage of detail on the composition of infrastructure, indeed the absence of any detail in many of the macro-econometric studies, limits the policy relevance of the findings. Table 1 provides the finest breakdown by purpose category available from the ABS.

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7. Otto and Voss (1994a), using quarterly data on the Australian economy, allow for lagged effects, but only up to about a year.

TABLE 1 GENERAL GOVERNMENT NET CAPITAL STOCKS, AUSTRALIA, 1994[a]

Purpose of use	*Millions of current dollars*	*Per cent of total*
Roads	41.9	27.7
Economic services[b]	26.0	17.2
Education	29.6	19.6
General public service	13.3	8.8
Health	12.8	8.5
Law and order	6.3	4.2
Social security and welfare	1.9	1.3
Other purposes[c]	19.3	12.8
Total	151.2	100.0

a Non-dwelling and non-military fixed capital; end of financial year values of capital stocks (30 June).

b Fuel and energy; transport and communications; mining, manufacturing and construction; agriculture, forestry, fishing and hunting; and other economic affairs.

c Includes housing and community amenities, recreation, culture and religion, and other functions.

Note The figures in this table do not include the capital of government business enterprises. Figures by purpose category do not sum to totals because of rounding.

Source Unpublished ABS data.

Variation in returns to infrastructure

The returns to an infrastructure investment will depend on when the investment is undertaken because of factors such as labour force growth, changes in the managerial efficiency in infrastructure agencies, and technological progress. The macro-econometric studies make only crude allowance for the influence of such factors due to data limitations.[8] Estimates of returns to investment in a particular year must therefore be treated with caution. (This is perhaps especially so for years toward the start or end of the database period, which will be technologically least representative.)

Aggregation biases

The macro-econometric studies use data that are aggregated over many producers—for example, all producers in the private sector of the Australian economy. The resulting loss of micro-level information will generally bias estimates of infrastructure payoffs.

8. The studies usually assume the Cobb–Douglas form of the production function with a linear time trend. The trend variable is a catch-all for technological progress, changes in managerial efficiency and all other factors not explicitly modelled.

Data problems

Findings sensitive to small changes in data

Data for the macro-econometric models come mainly from government statistical agencies, which sometimes revise their data to incorporate new methods of estimation or new information about the past. Re-estimating the models with revised data can greatly change the findings about infrastructure. Aschauer (1989) used data on the US private sector from 1949 through 1985, and found the output gains to be twice as large for a dollar spent on 'core' infrastructure as for a dollar spent on other infrastructure. Core infrastructure was defined to include four categories of the nine categories of public capital used in government statistics: highways, sewers, water supply and 'other structures'. (The latter category includes airports, aviation, transit systems and electric and gas utilities). One would expect much of the non-core infrastructure, such as hospital buildings and park facilities, to have a much weaker link to private sector productivity than does core infrastructure, and this is exactly what Aschauer found. However, re-estimation of Aschauer's model using subsequently revised data for the same period totally reversed this finding: non-core infrastructure now appeared to be twice as productive as core infrastructure. This underlies the previously noted admission by Aschauer (1992) that macro time-series data cannot, by themselves, yield reliable estimates of the effects of individual categories of infrastructure.

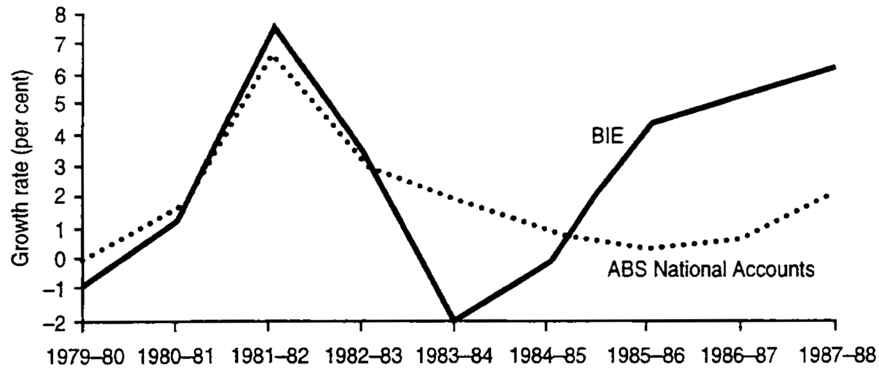
The problem, in fact, goes deeper: even without splitting infrastructure into categories, macro-econometric analysis of time-series can give findings for infrastructure that are very unstable across original and revised datasets. Ratner (1983) obtained an estimate of 0.06 for the elasticity of US private sector output with respect to infrastructure, meaning roughly that a ten per cent increase in infrastructure raises output by 0.6 per cent; Tatom (1991) re-estimated Ratner's model using revised data for the same sample period, and obtained a much larger elasticity estimate, 0.28.

Also worrying is the apparent sensitivity of time-series findings to the addition of data for only a few years. Aschauer (1989) estimated the elasticity of US private sector output with respect to infrastructure, using annual data from 1949 through 1985. Nienhaus (1991) found that adding data for the next two years, 1986 and 1987, substantially reduced the elasticity estimate based on Aschauer's model, from 0.39 to 0.24. (This finding is as reported in FHWA 1992).

Measurement of capital stocks

The sensitivity of macro-econometric findings to changes in data would be less disturbing were the data not so flawed. Lattimore (1990) explored, in the Australian manufacturing context, the problems in measuring stocks of infrastructure and other capital. Since a capital stock is an accumulation of past

FIGURE 1 GROWTH RATES OF NET CAPITAL STOCK OF PLANT AND EQUIPMENT IN AUSTRALIAN MANUFACTURING, 1978–79 TO 1987–88: ABS NATIONAL ACCOUNTS AND BIE ESTIMATES



Note The series in the above graph are derived from different investment data. The ABS National Accounts series uses mainly information collected by the Australian Taxation Office from business tax returns. The BIE series, derived by Lattimore (1990), uses information from the ABS capital expenditure survey after 1984–85 and information from the ABS manufacturing census before that date.

Source Lattimore (1990).

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investments, he investigated the historical series on manufacturing investment in the ABS national accounts. Finding some shortcomings, he constructed an alternative and seemingly equally defensible series, using other published ABS data. Over the period common to the two series, 1978–79 through 1987–88, there were significant disparities for some years. As a result, the series create quite different impressions of manufacturing capital growth after 1982–83 (figure 1).

Capital stock series also depend on assumptions about asset lives, and Lattimore found only slender justification for the assumptions used in the Australian national accounts. Although plausible changes to these assumptions had, in his analysis, smaller effects on the capital stock series than did the use of alternative investment data, some of these effects could be sufficient to noticeably alter the findings of a macro-econometric analysis. For illustration, Lattimore estimated a production function for Australian manufacturing using time-series data on capital and labour inputs. When he assumed an average asset life of 18 rather than 13 years, the elasticity of output with respect to capital declined from 0.90 to 0.72.

Changes in product quality

Changes in product quality require careful attention in estimating the output benefits from infrastructure. If a transport investment allows farm produce to reach consumers faster, one should estimate the value to consumers of fresher produce. Equally, one must account for changes in the quality of the infrastructure and other capital being produced.

The national accounts, from which the macro-econometric studies take most of their data, make some allowance for changes in product quality. For some industries, the series on real output reflects, in concept, changes in both quality and quantity.

Take the simple case where an industry makes two products with constant characteristics, and the price of neither changes between two years. The change in the real output would then be measured in the national accounts by the change in revenue. A compositional shift in the industry's output toward the higher-priced (and presumably higher-quality) product would contribute positively to the change in revenue. In this sense, the series on real output would reflect quality improvements. The national accounts also provide series on real capital stocks that similarly reflect changes in quality and quantity.

The national accounts nevertheless tend to understate the real growth in output and capital stocks, and sometimes quite significantly. The measurement problems are greatest for products like computer equipment, which undergo rapid and revolutionary changes in quality. In the US, the adoption of a more accurate method of allowing for quality improvements in computer manufacturing—the so-called 'hedonic' method—produced sharp upward revisions in the national accounts estimates of that industry's real output growth. Productivity growth in US manufacturing looked much stronger as a result, with one-third of the total growth during the 1980s due to this adjustment alone (Gordon 1993). This has been the only hedonic adjustment for quality change in the US national accounts, according to Griliches (1994), despite similar, if less extreme, problems existing for products other than computers. The same situation prevails in Australia, where the less accurate but easier method of quality adjustment—the 'matched model' method—is applied to non-computer products. For the US, there are indications that use of hedonic methods for all producer durable equipment (not just computers) would substantially raise the estimated growth rate of the private sector capital stock. For the period 1947–1983, the estimated growth rate would increase from 3.51 per cent per year to 5.11 per cent, according to estimates in Gordon (1990).[9]

For road construction and other components of the ABS category 'engineering construction', the extreme diversity of construction projects compounds the problems of measurement. This diversity makes it very difficult to monitor price changes through the matched model method. The ABS therefore focuses on the prices of inputs to engineering construction rather than the prices of outputs. Say that the prices of inputs to road construction increase overall by 5 per cent between two years, while government expenditure on road

9. For discussion of methods of quality adjustment and their limitations, see the excellent paper by Griliches (1994). Methods used in the Australian national accounts are discussed in ABS (1990). Also recommended are Wyckoff (1993), and Jorgenson and Stiroh (1993), who focus on computers.

construction increases by 7 per cent. In that case, real government investment expenditure has increased by 2 per cent, according to the national accounts. By ignoring productivity improvements in engineering construction, this method tends to understate growth in capital stocks. The ABS (1990) considers that the bias would be slight, but provides no evidence. For the US, the evidence suggests that for construction as a whole, the national accounts underestimated output growth by at least 0.5 per cent per year from 1963 through 1982 (Pieper 1990).

Measurement of real gross product

Most of the studies under review examine the variation in the real gross product of the private sector, sometimes at an industry level. Gross product is simply another term for value added (that is, the value of output after deducting the cost of goods and service used up in current production but before deducting depreciation costs). Procedures for converting nominal to real gross product vary, though many countries have adopted the most widely approved method, known as double deflation. The ABS will soon rely on this method for most industries, now that input–output tables will be prepared annually. Currently, the ABS double deflates gross product only for agriculture and mining, and uses generally less accurate methods for other industries for which data are more limited.

The method of measuring real gross product can affect the interpretation of macro-econometric estimates of infrastructure payoffs. Appendix I contains a preliminary exploration of this hitherto neglected issue, which bears most closely on the estimated effect of GBE capital. The analysis reinforces our earlier caution that GBE capital should be modelled separately from general government capital. It also shows that the estimated effects of GBE capital can be a poor indication of the payoffs from investing in GBE capital.

Implausible findings

The macro-econometric studies often produce implausible findings, as the many flaws in data and model specification would lead one to expect. It is hard to believe that additional infrastructure would reduce private sector productivity, as indicated by some of the estimates in Kelejian and Robinson (forthcoming), or that it produces benefits in the stratospheric range estimated by Aschauer (1989). For private sector inputs too, the macro-econometric studies have produced odd estimates. In Otto and Voss (1993), for example, labour's estimated contribution to private sector output is too small to accord with its share of private sector production costs. Better highways increase inventory costs in US manufacturing according to findings in Holleyman (1996), which, while theoretically possible, runs counter to the weight of case study evidence.

ASSESSMENT

Macro-econometric analysis has so far failed to realise its theoretical potential for contributing to evaluations of infrastructure investments. The findings from such analysis simply vary too much to support any conclusions about the magnitudes of infrastructure payoffs. For that matter, the findings conflict too much even to confirm that additional infrastructure facilitates production, something that can scarcely be denied. For categories of infrastructure like roads, the picture that emerges is no clearer.

Our reading of the evidence is similar to that in numerous other reviews of this literature, including those conducted by the World Bank (1994) and the US Congressional Budget Office (1991). In Australia, the National Transport Planning Taskforce commissioned a literature review from Kinhill Economics (1994), from which it concluded that the macro-econometric evidence 'does not provide a sound basis for policy formulation on infrastructure investment' (NTPT 1994). The Australian Automobile Association, on the other hand, saw the macro-econometric evidence as corroborating other indications of large returns to additional infrastructure investment and of the need for Australia to substantially increase its spending on transport infrastructure (AAA 1995). However, as we have seen, the macro-econometric literature includes findings that do not support these assertions. A case for a substantially increasing transport infrastructure spending must therefore rest on other evidence. (As a point of information, the Taskforce judged the other evidence to be insufficient to make this case.)

BTCE doubts the value of further macro-econometric research on infrastructure payoffs. Aggregation bias aside, such research has serious limitations arising from the nature of the data used. Macro-econometric analysts are not in the position of some natural scientists, who can conduct a vast number of experiments and vary one thing at a time to examine its effect. Rather, they must make do with such variation as has arisen, which will often be insufficient to reliably estimate the effects of interest, even when time-series are combined with cross-sections. Large errors in the available data, such as underallowances for improvements in product quality, make the prospects for analysis bleaker. Overall, BTCE considers that further benefit–cost analysis of individual investments would advance our understanding of infrastructure payoffs far more than the macro-econometric studies. As Gramlich observes, these studies have already commanded resources 'way out of proportion' to whatever might be learned from them.

Future macro-econometric research on infrastructure payoffs—and there will undoubtedly be some in Australia and elsewhere—should pay more attention to data issues and to sensitivity testing. Although many data problems are beyond the control of the analyst, more thorough consideration of these problems can aid the interpretation of results and the specifications of the models. More thorough sensitivity testing is essential if, to borrow Leamer's phrase, we are to take the 'con' out of econometrics.

APPENDIX I MEASUREMENT OF REAL GROSS PRODUCT

The econometric literature on infrastructure payoffs has paid little attention to the implications of different methods of measuring real gross product. This appendix discusses the implications for the interpretation of the estimated effects of GBE capital. The discussion is very much exploratory and is aimed at stimulating further debate.

DOUBLE-DEFLATION METHOD

The ABS provides estimates of annual gross product for broad industry sectors, like mining and manufacturing, both in nominal and real terms. As mentioned before, gross product is another term for value added: that is, the value of gross output minus the cost of material and service inputs used in current production (with no deduction for depreciation costs). Real gross product equals gross product measured in base-year prices.

To fix ideas, suppose that some industry produces a single output and uses a single intermediate input. The nominal and real gross products of that industry, V and V^r, can then be expressed by the following equations:

$$V = pQ - p_m M \qquad \text{(I.1)}$$

$$V^r = p^o Q - p_m^o M \qquad \text{(I.2)}$$

where p and p_m are the current prices for output and the intermediate input, Q and M are the corresponding quantities, and the superscript ^{o} denotes base-year values.

The double-deflation method of measuring real gross product adheres most closely to the definition of real gross product. It uses price deflators for both outputs and intermediate inputs to convert from current to base-year prices (hence, 'double deflation'). To explore how an increase in GBE capital affects real gross product, suppose that the industry under consideration is entirely private, perfectly competitive, and the sole purchaser of some GBE service. Imagine further that the GBE service is the only intermediate input used by the industry, so that M and p_m represent the price and quantity of this service. To complete the illustrative scenario, we assume away two of the problems with the macro-econometric studies. The increase in GBE capital is assumed to occur exogenously, rather than in response to changes in demand conditions (no reverse causation). In addition, the quantity of GBE services is assumed to be directly proportional to the stock of GBE capital, so we refer to the services and capital synonymously.

As in the macro-econometric studies, our interest is in the marginal effect of GBE capital on real gross product, holding constant the amounts of private inputs of capital and labour. This effect is expressed in equation I.3, where F is the aggregate production function for the hypothetical private sector industry, and L and K are the industry's input quantities of labour and capital.

$$\frac{\partial V^r}{\partial M} = p^o \frac{\partial Q}{\partial M} - p_m^o = p^o \frac{\partial F(L,K,M)}{\partial M} - p_m^o \qquad \text{(I.3)}$$

Assuming profit maximisation, ignoring aggregation problems (as macro-econometric studies do), and also ignoring the possibility of externalities, we can rewrite the above equation as follows:

$$\frac{\partial V^r}{\partial M} = p^o \left(\frac{p_m}{p} \right) - p_m^o \qquad \text{(I.4)}$$

The partial derivative in the above equation depends on the change between the current and base years in the price of intermediate input (the GBE service) relative to the price of output. Since the price relativity could just as well move either way, we proceed on the assumption of no change. The partial derivative then becomes zero: this means that the contribution of additional GBE services to the industry's gross output (Q) equals what the industry pays for these additional services.

If there are external benefits from GBE capital, then the marginal effect of GBE services will be understated in equation I.4. External benefits could push $\partial F / \partial M$ above p_m / p; hence, the marginal effect of GBE capital on real gross product will be positive. It follows that under the double-deflation method, a positive estimate of this marginal effect might be seen as an indication of external benefits.[10]

10. Of course, the interpretation of such findings must also have regard for evidence from other sources. Although recent theories of economic growth have emphasised external benefits from investment in physical capital, supporting evidence from case studies is rather thin (see Pack 1994). External benefits from infrastructure certainly exist—an architect walks past a railway station and finds some inspiration in its design—but whether they contribute much to the overall benefit of GBE investments is doubtful. Hence, with double deflation, a large positive estimate of the effect of GBE capital on real industry gross product might indicate serious modelling errors rather than large external benefits.

GROSS OUTPUT METHOD

For most industries, the ABS now uses the gross output method of measuring real gross product. The assumption is that within each subindustry comprising an industry, real gross product grows at the same rate as real gross output (which is more easily measured). This assumed equivalence in growth rates does not extend to the industry level, since subindustries differ in their growth rates and in their base-year ratios of gross product to gross output. For example, if the faster-growing subindustries have relatively high base-year ratios, then the measured real growth rates for the industry will be higher for gross product than for gross output. For the sake of argument, however, suppose that these compositional effects do not exist (the subindustries have identical growth rates or identical base-year ratios of gross product to gross output). In that case, the series on real gross product would be identical to the series on real gross output apart from a constant ratio difference. An econometric analysis of either series would thus give the same indications of infrastructure payoffs. The estimated effect of GBE capital would reflect the gain in the industry's real gross output resulting from the associated increase in GBE services. This effect should be positive, even in the absence of externalities ($\partial F / \partial M > 0$). The difference in interpretation from double deflation is worth noting. A small positive estimate suggests in one case only that the external benefits are small (double deflation), and in the other case that the contribution to real gross output is small (gross output method). Admitting compositional effects would complicate our argument, but preserve our conclusion that the interpretation of the macro-econometric findings must have regard for how real gross product is measured.

OTHER METHODS

Researchers sometimes fashion their own series on real gross product using national accounts data. Otto and Voss (1994b) devised a series on real gross product of the Australian private sector using a single-deflation method: they summed the private sector components of GDP (measured on the income side) and divided by the implicit GDP price deflator. The use of a single-deflation method can significantly change the interpretation of econometric findings for GBE capital. Reverting to our hypothetical industry example, suppose that nominal gross product is divided by a deflator for the output price. Measured real gross product can then be expressed as follows:

$$V^r = \frac{(pQ - p_m M)}{\left(\frac{p}{p^o}\right)} = p^o\left(Q - \frac{p_m}{p} M\right) \qquad \text{(I.5)}$$

The marginal effect of GBE services then becomes:

$$\frac{\partial V^r}{\partial M} = p^o\left(\frac{\partial F(L,K,M)}{\partial M} - \frac{p_m}{p}\right) - \frac{\partial\left(\frac{p_m}{p}\right)}{\partial M}M \qquad \text{(I.6)}$$

which, assuming profit maximisation and no externalities, reduces to:

$$\frac{\partial V^r}{\partial M} = -\frac{\partial\left(\frac{p_m}{p}\right)}{\partial M}M > 0 \qquad \text{(I.7)}$$

The partial derivative on the right-hand side indicates that an exogenous increase in the supply of GBE services reduces the relative price of these services. This benefits the industry purchasing the GBE services, as shown by the increase in its single-deflated gross product. Indeed, the estimated marginal effect of GBE services on single-deflated gross product should reflect only this relative price effect, according to the above equation. Thus, a large and positive estimate does not imply that GBE capital has a large payoff in private sector output or productivity. Such payoffs could be small at the same time that the price decline is large. Otto and Voss (1994b) include GBE capital in their infrastructure variable and interpret its estimated marginal effect as a marginal product, the conventional interpretation in Aschauer-type studies. However, their use of a single-deflated measure of real gross product throws some doubt on this interpretation.

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Abbreviations

AAA	Australian Automobile Association
ABS	Australian Bureau of Statistics
BIE	Bureau of Industry Economics
BTCE	Bureau of Transport and Communications Economics
ECIS	European Centre for Infrastructure Studies
EPAC	Economic Planning Advisory Commission
FHWA	Federal Highway Administration
IRU	International Road Transport Union
JFA	Jack Faucett Associates
MTRU	Metropolitan Transport Research Unit
NTPT	National Transport Planning Taskforce
OECD	Organisation for Economic Co-operation and Development
RTA	Roads and Traffic Authority (New South Wales)
SACTRA	Standing Advisory Committee on Trunk Road Assessment

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