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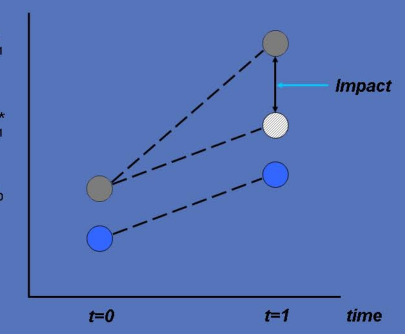

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Introduction

This paper summarizes methodological approaches for evaluating the role of urban transportation projects in poverty alleviation. Poverty impact evaluations are becoming more common within the World Bank. See, for example, the handbook developed by Baker (2000). Yet impact evaluations of urban transportation projects are still rare, especially in developing countries. Impact evaluation can illuminate causal sequences and links to poverty alleviation in ways that go beyond the insights from a standard cost-benefit analysis. Impact evaluation applied to urban transport has much promise, but the literature provides few examples of transport impact evaluation in the developing world. The purpose of this paper is to summarize the literature, often from developed countries, and to interpret that literature in ways that provides methodological guidance for pursuing poverty impact evaluations of urban transport in developing countries.

Impact evaluations of urban transportation projects present special challenges, including interactions with other markets and potential selection biases. To start, consider the endogeneity problem associated with urban transportation projects. Urban economic theory holds that city dwellers will adjust their location choices and travel patterns in response to urban transportation projects. Some of these readjustments may be at least in part the intended goal of the project. Firms, businesses, and non-profit or government organizations may also change their locations in response to changes in accessibility. As an example, imagine the construction of a transit line that improves access to employment centers. After the new line opens, motivated job seekers might move near the line to be closer to employment opportunities. After construction of this hypothetical transit line, when one observes that persons living near the line have higher employment rates, a researcher would need to adjust for the possibility that persons moved near the line in part based on their attachment to the labor market. Employable persons might have chosen to move near the new transit stations, as distinct from any effect the new transit line might have had on the employment prospects of persons who lived near the line before the project was built. More generally, urban transportation influences the land market and the market for travel in ways that are endogenous to the transportation intervention, and evaluations must deal with that endogeneity.

Complicating matters further, poverty impacts are usually an indirect result of an urban transportation project, not a direct result. Transportation projects are pursued for mobility goals, such as reduced travel times. These reductions in travel times may have impacts on poverty. For example, commuters may take more distant jobs if commute times are reduced, which could reduce poverty for residents who live far from job centers. Yet the data typically used to evaluate the mobility goals of an urban transportation project will not be sufficient to assess the poverty impact. The transportation outcome variable – a change in mobility, access, or travel time – will often be an independent variable in a study of a poverty related outcome. Hence in addition to the endogeneity and selection problems mentioned above, poverty evaluations for urban transportation projects can bring data collection requirements that go beyond the usual data collected in that sector.

Lastly, poverty impact evaluations in the urban transport sector present theoretical challenges. A longstanding tenet of transportation economics is that the benefits of transportation projects can be completely measured by the benefits that appear in the market for transportation. Hence reduced travel time, reductions in vehicle operating costs, and reductions in transportation-related accidents are considered to be complete measures of project benefits in the absence of externalities or market imperfections. Transportation economists have long cautioned against adding benefits in other markets to mobility benefits, as that would "double count" the benefits of a transportation project. For discussions, see e.g. Boarnet (1997, p. 477) Mohring (1961), Mohring and Harwitz (1962) and Wheaton (1977). Poverty impact evaluations typically examine other markets; for example, a poverty impact evaluation might examine how labor market outcomes are related to an urban transportation project. While such an evaluation will illuminate how the transportation project relates to the World Bank's core mission of poverty alleviation, partial equilibrium analysis of poverty impacts is not the same as a welfare measure, and the difference between the two can be large. Double counting project benefits by, for example, adding impact measures to benefits measured in the transport market will inappropriately inflate the benefits of the project.

These challenges can often be overcome. Rigorous assessment of the poverty impact of urban transportation projects is possible. This paper provides a guide to implementing evaluations of the poverty alleviation impact of urban transportation. That guide begins, in Section I, with some background on the relationship between traditional transportation cost-benefit analysis and poverty impact evaluations. Section II provides background on recent World Bank urban poverty projects. Section III briefly discusses classical impact evaluation approaches, and particular methodological challenges inherent in applying those approaches to urban transportation projects. Section IV discusses types of impacts that would be typical foci for evaluation studies related to urban transport. Section V discusses methods for assessing the impact of urban transport projects on labor market outcomes of the poor. Section VI examines impact evaluation related to firm location. Section VII discusses evaluation methods to evaluate the impact of transport projects on access to services. Section VIII discusses methods to evaluate impacts on land prices, and the final section concludes.

I. Cost-Benefit Analysis and Impact Evaluation for Urban Transport Projects

Methods for cost-benefit analysis of transportation projects are well established. Costs are typically determined by actual (discounted) project costs, or the opportunity costs of resources devoted to the project, in a manner consistent with general cost-benefit analysis principles. See, e.g., Gramlich (1997) for a discussion. Benefits are typically divided into three parts – reductions in travel times, reductions in vehicle operating costs, and reductions in accidents (e.g. AASHTO, 1977; Forkenbrock and Foster, 1990). All benefits are monetized; for a discussion, see, e.g., Small (1992).

Wheaton (1977) shows that, absent externalities or imperfect competition, the benefits of urban transportation investments can be obtained from an appropriate surplus measure derived from a well-specified travel demand function. This is consistent with focusing only on the mobility or transportation benefits of urban transportation projects, as per the common practice described above. Other benefits, such as land price changes or reductions in shipping costs, are not distinct benefits but simply reflections of transport benefits in other markets. As such, it is not appropriate to count both transport market benefits and benefits in other markets that flow strictly from changes in the transport market; counting both would be "double counting" the benefits of the transportation investment. Consider the below examples.

Transportation projects that improve access from suburban land to more central employment centers will often increase the value of suburban land. For example, highways near the fringe of a metropolitan area might increase the value of nearby land. Yet counting both the monetized value of travel time reductions and the value of land price increases would double count the benefits of the transportation project. The land price increases are due to the travel time reductions from the transportation project. Classic urban land use models show that, under some assumptions, the land price changes are strictly due to changes in travel times (see, e.g., Alonso, 1964 or Alonso, 1972.)

Or consider a highway project that reduces the cost of shipping produce from rural farms to an urban market. Reduced shipping costs would lead to either higher profits for produce sellers, lower consumer prices, or both, depending on the incidence of the reduction in shipping cost. Yet adding lower consumer costs or higher agricultural profits to the monetized value of the reduced shipping costs would double count benefits, since the change in consumer prices or grower profits is due to the change in shipping costs.

For these reasons, transportation analysts have long accepted the idea that urban transport project benefits should be measured only in one market. The benefits in the transport market can be reflected in many other markets, but those benefits are not distinct from the transportation benefits (e.g. Mohring, 1961; Mohring and Harwitz 1962; Wheaton, 1977).

Given the double counting critique, why pursue poverty impact evaluations? There are several reasons. First, the double counting critique applies to markets without externalities or imperfect competition (see, e.g., Wheaton, 1977 or Jara-Diaz, 1986). In the presence of externalities or imperfect competition, transport project benefits may not equal the mobility benefits typically measured by changes in travel times, vehicle operating costs, and accidents. While this is an important theoretical caution, conventional wisdom holds that the distortions from imperfect competition or externalities are typically smaller than errors induced by double counting. Thus concern about imperfect competition will rarely be solid grounds for summing benefits in several markets, as the errors created by doing so might be larger than the errors avoided. More generally, the preferred solution to external impacts would be to model and measure those impacts explicitly within the context of a cost-benefit analysis focused on transportation market benefits. Hence the motivation for poverty impact analysis will often be practical, rather than theoretical.

Impact analysis should be viewed as a complement to traditional cost-benefit analysis, not a substitute for cost-benefit analysis, and care should be taken to avoid double counting benefits. Impact analysis can often better illuminate the distributional properties of project benefits, especially as that distribution relates to poverty alleviation. This might occur both in cases where transport projects have important external impacts and in cases where those benefits flow directly from mobility benefits but are more easily observed in other markets. As an example of external impacts, consider projects that require the relocation or resettlement of slum dwellers. In those cases, the impact of resettlement, including any resettlement program or compensation, will often be better understood through impact evaluation than through changes in mobility characteristics. As an example of cases where impact analysis can provide a more direct window into poverty alleviation, consider projects that change commuting times and hence access to jobs. Impact evaluations that measure changes in labor market outcomes might be a better guide to poverty impacts than studies that focus only on commuting behavior, even if the commuting behavior reflects changes in labor market outcomes. In cases such as the labor market example, where analysts choose a market for convenience in measuring and understanding impacts, particular care must be taken to avoid double counting the overall benefits of transportation investments. In short, impact assessment should be viewed as an alternative window into transport project benefits, rather than a measure of new and previously unmeasured project benefits.

One way to view the difference between impact evaluation and benefit analysis is to consider the scale of the project and to ask when a measure of transport user benefits will or will not give a full measure of project benefits. Here the distinction between a partial equilibrium and general equilibrium analysis is helpful. Partial equilibrium analysis looks at one market, while general equilibrium analyses incorporate linked changes in multiple markets. For transportation projects, general equilibrium effects include locational changes; persons and firms will move (and hence locationally sort) based on the effect of a transportation project.

Consider how this effect could depend on project scale. Small projects might change only the cost of transport, without inducing any locational changes.[2] In such cases, measuring benefits that accrue to transport users, based on an appropriately specified transport demand curve or the methods of transport cost-benefit analysis, will be sufficient. Note though that we have assumed both that the effect of the project is small enough so that no resident or firm changes location and that no externalities are present. Hence size and externalities, both individually and taken together, matter. If a

[2] "Small" here refers to the size of the transportation impact of the project, measured as the change in transport cost for users.

project is large enough to induce locational changes, a model that incorporates an analysis of locational equilibrium will be necessary to get accurate user benefit measures. See, for example, Sieg, Smith, Banzhaf and Walsh (2000) for a comparison of partial and general equilibrium estimates of air quality improvements and for evidence that partial equilibrium benefit measures that do not take account of locational sorting by households give estimates of benefits that are very different from general equilibrium techniques. Furthermore, if externalities exist, a complete benefit measure cannot be obtained by only looking at transportation user benefits. Examples include projects that change environmental externalities associated with transportation or projects that might influence pre-existing distortions in related markets, such as the labor market.

Where does this leave us? Several points are important. First, impact analysis is not a benefit analysis, and adding measures of impact analyses to other impact measures or benefit measures should be avoided, as doing so would generally be double counting. Second, location choice is important. Transportation projects can cause persons and firms to sort over the landscape.[3] Third, project size can provide some rough guidelines about the need to account for location changes; small projects are less likely to induce location changes, and thus measures that ignore location choice are appropriate for those projects. Fourth, for large projects, both residents and firms will likely change their location. The techniques discussed in this report give some guidance for incorporating location choice into impact assessment, although such techniques are not full general equilibrium measures of the sort outlined in Sieg, Smith, Banzhaf, and Walsh (2000). The focus here is not on general equilibrium benefit assessment, but on measuring impacts in ways that account for location choice. Fifth, externalities and market failures can drive a wedge between project benefits and benefit measures derived from transport demand curves. In such cases, impact assessment can at times give insights, but it is not appropriate to simply sum several partial equilibrium impact assessments, as in general a full benefit measure would require a full treatment of both the general equilibrium nature of the problem and externalities.

II. World Bank Urban Transport Projects

World Bank urban transport projects are largely of three types – infrastructure investment, economic or regulatory reform, and traffic or travel demand management. Examples of each type of project are discussed below.

Infrastructure investment ranges from large-scale road or passenger rail projects to efforts to provide maintenance or refurbishment for existing infrastructure. Examples include a metro rail extension in Sao Paulo (Barone and Rebelo, 2003), a bus rapid transit

[3] Such sorting could be within cities or across cities, depending on whether the effect of the transportation project is primarily to change the relative attractiveness of locations within cities or, for larger and more economically important projects, to change the attractiveness of one city relative to other cities.

system in Bogota (World Bank, Project Appraisal Document, Report No. 28926-CO, 2004), and a rural road rehabilitation project in Vietnam (van de Walle and Cratty, 2002). In some cases, infrastructure investment includes a prominent (or exclusive) role for maintenance. The Vietnam rural road rehabilitation program did not construct new roadways, but in some cases portions of the rehabilitated roads were not passable for some or all of the year (van de Walle and Cratty, 2002, p. 8). In the Kyrgyz Republic, World Bank credit leveraged a government road maintenance fund to assist cities with arterial road maintenance and rehabilitation ("Cities on the Move," World Bank, 2002, p. 78).

The Line 4 rail transit line in Sao Paulo is an example of a large infrastructure investment. Line 4 is a new metro line, being built with World Bank assistance. The project will be a major extension of the urban rail transit infrastructure in Brazil's largest city. Line 4 will improve system-wide connectivity, providing direct rail transit service from the south and southwest of Sao Paulo into the business and financial centers downtown and providing connections between the city's commuter rail system and the metro system. Because of the system-wide nature of the Line 4 link, 70 percent of the poor in the Sao Paulo metropolitan area reside within the Line 4 catchment area (Barone and Rebelo, 2003), and lower income neighborhoods to the east of the metropolitan area will be better connected to growing financial and employment centers to the southwest of downtown (Barone and Rebelo, 2003).

While infrastructure projects provide new capacity, economic or regulatory reform projects typically aim to improve the efficiency of existing capacity. Economic or regulatory reform projects often aim to introduce elements of private sector participation and competition into the market for urban transportation. This can involve concessioning publicly owned transit or transport operations to the private sector, or other interventions that create competition in or for the market to introduce efficiencies into the urban transport system ("Cities on the Move", pp. 14-15 and pp. 117-121). Examples from the past decade include the concession of the Buenos Aires metro system (Shaw, Gwilliam, and Thompson, 1996; Mitric, 1997), the concession of the Rio de Janiero metro and commuter rail systems (Rebelo, 1999), and the concession of the Brazilian freight railway system (World Bank, "Brazil: Multimodal Freight Transport: Selected Regulatory Issues, 1997, pp. 13-14; World Bank, "Staff Appraisal Report, Brazil: Federal Railways Restructuring and Privatization Project," 1996). In other instances, market reforms are introduced that retain public sector provision of services while allowing or encouraging private competition. An example is the reform of the state bus enterprises and associated efforts to allow private provision of bus service in Uzbekistan (Gwilliam, Meakin, and Kumar, 2000). Efforts at market reform involve substantial regulatory components, which often include establishing regulatory bodies, concessioning or contracting practices, and (ideally) methods for dispute resolution. Hence building institutional capacity can be an important focus of these projects.

When the intervention is an infrastructure investment, once the project is built, an analyst can have some assurance that the intervention will remain in place during the evaluation period. Economic or regulatory efforts, by their nature, can be more easily reversed. The potential impermanence of regulatory reforms can complicate impact evaluation, although in many cases regulatory reform efforts have had enough consistency to allow an evaluation.

For example, in Brazil in the late 1990s, the federal and state governments pursued an aggressive program of concessioning public sector highways to the private sector, with the goal of attracting private investment to rehabilitate roads in exchange for allowing private tolling. Once the concessions were granted, the overwhelming majority remained in place as of a 2002 World Bank assessment (Reja and Boarnet, 2002, draft), but some instances of policy reversion illustrate the potential to roll back market reforms.

In Santa Catarina state, the state government did not allow the concessionaire to charge tolls after the initial emergency rehabilitation was completed. In Paraná state, the state government reduced tolls to one-half of the contractually allowed level before an election, but toll levels have since been allowed to return to the levels allowed by the contract. In Rio Grande do Sul state, two concessioned highways were returned to the federal government after a new state government that came to power failed to support the concession program. The return of the delegated highways to federal control did not alter the concession agreement—a hopeful sign for the stability of the Brazilian concessioning program.

Overall, the caution is that market reforms can be reversed more completely than building projects that leave infrastructure on the ground. Yet in the Brazilian case, the bulk of the concessions, once approved, have proceeded forward as planned (Reja and Boarnet, 2002, draft), and those concessions are associated with a program of infrastructure investment – both are indications that impact evaluations can be conducted in the context of market and regulatory reform.

Traffic and travel demand management projects include traffic regulation and enforcement, parking controls, traffic control and restraints (ranging from one-way directional flows to cordon pricing or restraints in heavily congested areas), efforts to balance access and safety for non-motorized modes with motorized traffic, strategic traffic planning, and pricing. A broad range of techniques, and their relationship to urban transport in developing countries, is described in the World Bank's urban transport strategy review, "Cities on the Move" (2002, e.g. pp. 65-92). Many traffic management techniques are policies, such as enforcement or travel restrictions, and so can potentially be modified or reversed and, like economic reforms, lack the permanence of infrastructure investments. Yet like economic reforms, careful traffic management programs can provide a level of consistency that makes impact evaluation possible.

III. Impact Evaluation and Methodological Issues Related to Urban Transport

Impact evaluation is based on classic tenets of research design (e.g. Cook and Campbell (1979). A key insight is to construct evaluations that mimic, as best as possible, random assignment to control and experimental groups. While random assignment is sometimes possible in social programs, it is often not possible, especially in the case of urban transportation projects.[4] A critical issue for evaluations, then, is to develop a "control group" that will allow the researcher to identify the counterfactual – what would have happened in the absence of the intervention. Much impact evaluation is focused on obtaining control groups that identify the counter-factual without statistical bias. For a discussion of research design in the context of development projects, see Baker (2000).

An especially powerful and recently popular approach to impact evaluations combines differences-in-differences estimation with a carefully selected control population. Differences-in-differences (DID) estimation compares outcome variables before and after the program intervention (the treatment) for two groups of persons – those who received the treatment, and those who did not. In the context of urban transportation, such an estimator might compare a labor market outcome (such as employment or income) for two groups of persons – those living near a transportation improvement and those distant from the improvement. As an example, see Holzer, Quigley, and Raphael (2003). A general form for a DID estimator, drawn from Smith and Todd (2005, p. 312), for an outcome variable Y is shown below:

$$Y_{it} - Y_{it'} = \varphi(X_{it}) - \varphi(X_{it'}) + D_i\alpha + (U_{it} - U_{it'}) \qquad (1)$$

where Y = the outcome variable

X = a vector of observable characteristics

D = a dummy variable, equal to 1 for individuals affected by the project (the treatment group, with the control group having values of D = 0)

U = error terms

i and t subscripts indicate individuals and time periods, with the subscript *it* indicating before the project and the subscript *it'* indicating after the project

Note that the basic research design divides the subjects into two groups, the treatment and control group, and two time periods, before and after the intervention. The general formula in equation (1) above allows for time series data at multiple time periods before and after the project. In the special case where there are data at only one time period before and one time period after the project, and if the control and experimental subjects were randomly assigned such that variation in individual characteristics is subsumed in the error terms, U, then a simple differences in means test would suffice. The test, in that special case, would examine changes in the outcome variable from the "before" to the "after" period, and test for a significant difference in the change in Y

[4] Random assignment for social program evaluation typically has involved randomly enrolling eligible persons into the social program, while randomly assigning other eligible participants to a control program. Transportation infrastructure projects would not be able to use this technique, as the infrastructure is built based on geographic concerns, and persons near the infrastructure cannot be excluded from the accessibility benefit in the way that eligible social program participants can be assigned to "control" programs. Transportation programs that offer individual benefits, such as transit subsidies to individuals, could pursue a pure random assignment framework.

across the control and experimental group. This double difference approach motivates the "differences in differences" name.

As with all research designs, constructing a valid control group is vital. There are several approaches, including gathering data on individuals in the control and treatment groups so that differences in characteristics across the two groups can be controlled in a regression framework, as in equation (1) above. Recent studies have popularized using propensity score matching techniques to select a control population (e.g. Rosenbaum and Rubin, 1983 and 1985; Dehejia and Wahba, 1999 and 2002). See Baker, 2000, pp. 48-51 for a discussion of propensity score matching in the context of an evaluation of a development project.

Propensity score matching typically involves two steps. First, a discrete choice regression is run to predict program participation based on sample characteristics. In a social program such as labor market training, the discrete choice regression would predict the likelihood that an individual participates in the training program. Second, for each program participant (member of the treatment group), the non-participant with the likelihood of participating that is closest to each participant is matched to that participant. Those pair-wise matches form the control group. For a discussion, see Baker (2000, pp. 48-51). Several alternatives to pair-wise matching have also been proposed and used. Smith and Todd (2005) examine various alternative matching techniques. One insight from Smith and Todd (2005) is that matching techniques that have a high likelihood of yielding control groups with characteristics closely similar to the experimental group are preferred. The practical insight is to draw the control group from a population that is as similar as possible to the experimental (using propensity score techniques to match individual experimental and control subjects) and to have a rich set of individual characteristics with which to form the propensity score match.

Note that the DID technique assumes that the effect of the "treatment on the treated" (TT) is the same as the "average treatment effect" (ATE) measured by the DID estimator shown in equation (1). This might not be the case. If persons are heterogeneous, and if the most recently "treated" persons differ from the persons who are first treated, the marginal treatment effect will differ from the average treatment effect. Suppose, for example, that a three-mile rail extension passes through a neighborhood of relatively high income persons, and the effect of the extension on labor market success is measured with DID, as shown in equation (1). Suppose that an additional three-mile extension would go through a lower income neighborhood. The marginal effect of the project on labor market outcomes might differ substantially, as the persons who live near the second extension differ from the persons who live near the first extension.[5] More generally, marginal and average treatment effects will differ when persons are heterogeneous and when expansions of the project influence different types of persons (in ways that cause differences in treatment effects) or when persons can sort into treatment groups.

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[5] I am grateful to an anonymous reviewer for suggesting this point and the example.

When persons are heterogeneous and project impact might vary across individuals, predicting marginal effects of a project will require that the DID estimation be adjusted to measure impacts for persons who will most likely be affected by the project. For transport projects, predicting which "marginal group" will be affected by a project will sometimes be clear based on project geography. Yet, even then analysts should take care to be sure that they have adjusted for residential selection, possibly by selecting a "control group" before a project is announced or explicitly modeling location choice, project location, or other factors that can influence access. See the discussion in Section IV for further elaboration on these themes.

DID and propensity score matching techniques can be adapted to urban transport evaluation, but urban transport projects bring some particular challenges. First, urban transport projects affect many markets. The location of residences, firms, and service providers can change in response to major urban transport interventions. In a market economy, changes in accessibility will change land values and induce changes in location. Commuting behavior and the pattern of activities that individuals pursue can also change in response to the accessibility changes created by urban transport interventions. Hence the pattern of accessibility is endogenous due to selection. For example, persons who prefer to work in finance jobs might choose to locate near financial centers, or persons who are full-time students might choose to live near school. Urban transport evaluations must control for these selection effects, and the fact that many markets are involved requires a careful approach.

Second, the effect of urban transport projects is not the same throughout the urban area. Locations near a transport improvement will generally have a larger improvement in accessibility than areas distant from the project. Such spatial variation in the effect of urban transport projects often provides a tool for choosing experimental groups (affected by the project) and control groups (typically distant from the project and not affected). Note, though, that the spatial variation in the effect of the project needs to be understood if that spatial variation is exploited for evaluation purposes. Network analyses of accessibility would be an ideal way to understand what locations are and are not influenced by a transport project. Less precise approaches might also work, such as choosing areas far from a project but within the same urban area based on local knowledge of the pattern of transportation accessibility.

Third, a long timeframe is needed to observe some transport project impacts. In a market economy with well functioning land markets, land prices would likely adjust quickly. (For evidence from the United States, see Boarnet and Chalermpong, 2001.) Commuting patterns might also adjust quickly. Due to the durability of urban structures and delays associated with moving, the location of residences, firms, and service providers would not adjust so quickly. Evidence from lagged adjustment models can be used to infer how quickly the pattern of population and employment location adjusts to an equilibrium. These estimates of the "speed of adjustment," from studies that use intra-metropolitan data for the United States, suggests that urban settlement patterns in that country typically close from less than 10 percent to 100 percent of the gap between actual and equilibrium population and employment locations in a decade. Many estimates of the adjustment speed show adjustments of around 30 to 40 percent of the gap between equilibrium and actual values closed in a decade. This implies that urban form, in the United States, adjusts to new locational equilibria over a period of from one to three decades. (For a summary of these studies, see Boarnet, Chalermpong, and Geho 2005, especially Table 1.) There is little available evidence on adjustment speeds in developing countries, but note that the U.S. evidence suggests that the locational effects of urban transport projects are long-run, not short-run, phenomena. The same is almost certainly true of developing countries, as persons and firms both adjust to changes in the geography of amenities with lags.

All of these factors combine to make determining the counter-factual difficult in the case of urban transport projects. Project evaluation should take care to consider whether the impacts being studied are short-run or long-run phenomena. Care should also be taken to control for selection through location and commuting choices. These cautions apply to investment projects. Similar cautions would apply to economic or regulatory reforms or traffic management projects that, by changing the efficiency of the transport system, change the spatial pattern of accessibility. Economic or regulatory reforms or traffic management projects also bring the added complication that those reforms may not be permanent, and could change during the period of the evaluation. Lastly, the scale of the urban transport project is important. Large projects with a large impact on accessibility will induce the largest changes in location choices and travel patterns, and so selection issues associated with location choices, in particular, are more severe for larger projects.

The rest of this paper is organized around specific impacts of urban transport projects. Those specific impacts are:

- The impact of urban transport on labor market access
- The impact of urban transport on the location of firms and hence the location of employment
- The impact of urban transport on access to services
- The impact of urban transport on land prices

Each of these impacts is described below, and methods for evaluating each type of urban transport project are also described.

IV. Evaluating Labor Market Impacts of Urban Transport Projects

A. Background

The idea that transportation access is linked to labor market success has been most vigorously researched in the United States. As examples of hypotheses related to this literature, improved transportation access might increase the likelihood that individuals are employed, or allow already employed persons to access better jobs, or simply reduce commute times and possibly increase individual welfare. Of those topics, the first question – whether there is a link between access to jobs and employment – has been called the "spatial mismatch" literature, using a phrase coined by John Kain (1968). For a summary of the U.S. literature, see, e.g., Ihlanfeldt and Sjoquist (1998).

The causal stories hypothesized above include possible market failures and factors that are likely not market failures. Examples of possible market failures include the following: (1) Persons may not have information about job openings due to poor transportation access (i.e. they may not come in contact with persons from inaccessible labor markets and so lack important information about those labor markets), or (2) If information flows are not impeded, persons may not take jobs due to transportation barriers and thus fail to accumulate human capital that would prepare that individual for further labor market success. Alternatively, in the absence of information or human capital externalities, individuals might simply not take job opportunities because the commute cost is too high. In that case, the effect would not be a market failure, but distributional concerns would remain. Also, if land prices adjust to perfectly compensate for transportation access to employment, long commutes might not be associated with lower individual welfare if employment disincentive effects do not lead to long-term reductions in human capital accumulation that bring negative labor market impacts. Such land price adjustment to reflect commuting costs is likely in developed countries, but less likely in developing economies with weak or incomplete land markets.

Second, the data recommended below will typically not be sufficient to disentangle information, human capital, reservation wage, or other explanations about why transportation access might influence labor market impacts. As Ihlanfeldt and Sjoquist (1998) note, the state-of-the-art in this research involves testing the hypothesis that transportation access influences labor market outcomes; few studies further explain the factors that cause transportation access to be linked to individual labor market outcomes. As such, it will likely not be possible for World Bank analyses to determine whether a link between transportation access and labor market outcomes reflects, for example, an underlying market failure or simply a distributional concern. For that reason, impact analysis in this area is best viewed as a window into poverty alleviation, and hence distribution, and questions about efficiency should be addressed through a detailed cost-benefit analysis of transport projects, as was suggested in Section I of this paper.

B. Examples that Suggest a Link Between Transportation Access and Labor Market Success

Experience in developing countries suggests that access to jobs can limit the labor market success of the poor. A survey of households in Mumbai, India found that among the poor (households earnings less that 5,000 rupees per month), those living in outlying zones had longer commutes than those living in inner zones. Commutes averaged 6.2 kilometers for the poor in the most distant zone versus 2.3 kilometer in the zone closest to the center of the city (Baker, Basu, Cropper, Lall, and Takeuchi, 2004).

Yet commute length, by itself, is not a meaningful indicator of a link to labor market success. The Mumbai study found that the high income group, households earning more than 20,000 rupees per month, also had longer commutes in the outlying zone – an average 10.4 kilometer commute in the outlying zone versus an average 4.6 kilometer commute in the most central zone (Baker, Basu, Cropper, Lall, and Takeuchi, 2004).

The Mumbai data suggest that the wealthy may be able to access more distant jobs than the poor. A long commute may not be a sign of disadvantage, but possibly the converse, as the poor might not be able to learn about or access distant jobs. The Mumbai study provides evidence that is consistent with this hypothesis, finding that 60.8 percent of the survey respondents in the poorest group (income less than 5,000 rupees per month) walked to work (Baker, Basu, Cropper, Lall, and Takeuchi, 2004).

Further evidence that transportation access limits the ability of the poor to find or hold jobs comes from a qualitative study in Wuhan, China ("A Lifetime of Walking," World Bank, December 15, 2003). That focus group research included the following quotes from study subjects:

> "I could have got a job delivering newspaper at Yangchahu that paid Y600. But with transit costs of Y3-4 a day and no lunch provided, it's hardly worth it." Unemployed man, Wuzhong area, Wuchang district.
>
> "My daughter was given a job at the Wuhan Plaza department store in Hankow after graduation; the job paid her Y600. There was not much left once she paid for her meals and the bus. So she just gave up." Resident of Kangyuan area, Qingshan district.

The above examples, coupled with the large literature from the U.S. (Ihlanfeldt and Sjoquist, 1998), suggest that transport projects can have important impacts on labor market outcomes and, through that, poverty alleviation. Yet there are few careful impact evaluations of this hypothesis in developing country contexts. Below we discuss the data and methodological approaches that are appropriate to study this hypothesis in developing countries, with a description of example studies.

C. Variables

The dependent variable in evaluations of the impact of transportation on labor market access is often a measure of individual labor market outcomes. In the spatial mismatch literature from the United States, employment is the most common outcome variable. Other typical labor market outcomes, such as wages or income, can also be used. In a developing country context, consumption (as measured by a household expenditure survey) will often be preferred over formal employment measures due to the importance of seasonal, informal, and non-cash employment (World Bank, "Cities on the Move," 2002, p. 26).

Independent variables typically include a measure on transportation access to employment. This requires two types of data – information on the spatial distribution of employment and a measure of a study subject's accessibility to that employment. The data should include the residence location for study subjects, since access to employment should be measured from a subject's place of residence. Job accessibility measures can be constructed based on employment within zones. Changes in accessibility would be measured by changes in travel time or travel distance to job centers. Geographic Information System data and road network analysis can be used to calculate travel distances and times to job centers. (Travel time is theoretically preferred, if travel time data can be accurately obtained.) Job centers could be defined based on local knowledge of the location about important job centers or data on employment by geographic zone. A potential variable, which weights employment in zones based on travel time or distance, provides a generalized measure of access to employment throughout the urban area.[6] An example of a potential variable is shown below:

$$JACCi = \sum_{j=1}^{Z} \frac{EMP_j}{\mathrm{Im}_{ij}{}^{\alpha}} \qquad (2)$$

Where $JACC_i$ = potential measure of job accessibility from a household's residence location "i"

EMP_j = the number of jobs in zone "j"

Z = total number of geographic zones

IM_{ij} = a measure of travel impedance (or cost) from a household's residence location "i" to geographic zone "j". The measure of impedance can be travel distances or times obtained from network analysis of GIS programs, and can include information about out-of-pocket travel costs.

α = damping parameter, which can either be estimated from travel models, obtained from transportation agencies, or chosen from values over a pre-determined range (e.g. ½ to 2)

The potential variable for accessibility is a weighted sum of employment in different zones, with the weights corresponding to travel cost from the household's residence location "i" to zone "j".

An alternative measure of transportation access would be individual commute times taken from survey data. If residence and job locations are unchanged before and after the transport project is built, then changes in commute times reflect changes in an individual's job. Yet both residence and firm locations could change, creating the possibility that commute times would reflect not only a direct change in access due to the project, but also some behavioral response. For that reason, measures of access linked to the geography of the urban area (such as potential measures of access to jobs) would be

[6] The name potential variable, when applied to weight sums of one variable that are damped by an inverse distance measure, refers by analogy to terminology from the law of gravitation. Similarly, weighted sums of the product of two variables whose interaction is damped by distance are called gravity variables.

preferred.[7] If the access measure is based on commute time from individual surveys, care should be taken to ask the individual about job locations, residence locations, and whether the individual changed job or residence locations in the study period, and if that individual regards those changes as motivated in part by the transportation project.

D. Data Sources

To recap, there are two key types of variables – measures of individual labor market success and measures of the change in accessibility to employment. A large number of additional control variables will be needed; briefly, other determinants of both individual labor market success and, potentially, the determinants of transportation investment, should be gathered. Here we describe the data sources for these variables.

Data on individual labor market success require a household survey. Transport surveys can be conducted independently, or coupled with the World Bank's Living Standards Measurement Study (LSMS) household surveys by adding an additional module to an LSMS survey. Baker and Denning (2005, p. 2) describe the pros and cons of both approaches. Briefly, the LSMS surveys have evolved into sophisticated national probability samples, such that opportunities to add a transport module to such a survey can provide benefits in getting detailed transportation information for countries. If the focus is on a specific project, as will be the case in impact evaluations, the alternative of unique surveys, that combine transportation questions with questions on household living standards or labor market outcomes, might be preferred.

Such unique surveys can augment standard modules from the LSMS surveys, as suggested by Baker and Denning (2005). Ideally, the LSMS consumption module would be used to measure household consumption, since consumption will often be the preferred indicator of both labor market success and household welfare in countries with a significant amount of informal, non-cash, or seasonal work. For urban transport, augmenting a consumption measure with questions that assess the individual's labor market activity, including employment status and wages, would be useful. In cases where non-cash or seasonal work is important, researchers would have to use the local context and some care to determine how to incorporate data on consumption, employment, and wages, especially if employment and wage data likely refer to formal, market economy jobs. If seasonality is an important factor, time series surveys, as would be implemented in a "before and after" study, should be administered at the same time during the year to control for seasonality.

Travel diaries can also be included in the household survey. For an example, see Baker and Denning (2005, pp. 20-21). If the focus of the impact analysis is limited to the

[7] Yet even accessibility measures based on the distribution of employment could reflect locational adjustment resulting from the transport project. Except for very large projects, potential measures based on job locations are less likely to reflect locational adjustment than individual commute times. This would be especially true in early years after project completion, before changes in urban form are realized.

link between labor market success and transportation access to employment, a full travel diary typically will not be needed. Instead, questions can be limited to work-based (commute) travel.

The key independent variable, change in access to employment, can be obtained in one of two ways: spatial measures of transportation access to employment, or individual self reports of commute times, distances, or commuting origins and destinations. Data that measures spatial access to employment would be obtained from geographic data on job locations and measures of access to job locations. Data on commute times, distances, or individual commute origins and destinations could be obtained from household surveys. As was mentioned in part C, individual commuting behavior could be endogenous to labor market success, and so data on the spatial pattern of access is the preferred independent variable.

Spatial access requires some information about the distribution of employment opportunities and the travel cost (time and money cost) of getting to those opportunities. Detailed data on jobs in different zones of the city are sometimes available from metropolitan transportation planning surveys. If such data are available, one could construct potential variables that measure accessibility to employment. The potential variable should be calculated for each household, and requires zonal data on job locations, network measures of travel impedance or cost, or effective proxies for such data. In the absence of detailed zonal data, travel times to a dominant single employment center (i.e. the downtown) can be used to measure transportation access to employment.

In the absence of good data on the spatial distribution of employment, and hence the spatial pattern of job accessibility, individual responses to household surveys about commute distances or times might be used. Individual commute distances and times are endogenous to labor market and residential and job location choices, and so such data can be a substantially inferior alternative to data that measure the spatial distribution of employment.

Both the dependent variable (household consumption of labor market success) and the independent variable (transportation access to employment) should ideally be measured before and after the transport project is built. A suggested methodology is outlined below.

E. Methodological Approaches and Challenges

In this sub-section we first describe two important methodological issues related to evaluations of the labor market impact of transport projects, and then we outline the steps needed for a differences-in-differences (DID) evaluation technique. The DID technique, with careful matching of experimental and control subjects, will often be the preferred evaluation method. [8]

The two methodological issues related to labor market impacts involve (1) linking high transportation costs to individual welfare, and (2) controlling for endogenous residential location. First we discuss some cautions in linking transportation costs to welfare.

A naïve view of transportation costs would be that high transportation cost, in and of itself, is a sign that persons are disadvantaged (i.e. have lower utility). In this view, evidence that persons have costly commutes, either in terms of time, inconvenience, or out-of-pocket cost, is *a priori* evidence that those persons are disadvantaged. The difficulty with this reasoning is that well functioning land markets will adjust land prices, and hence housing prices, to compensate for commute costs. In the classic monocentric urban model, the compensation is one-for-one – land prices adjust in response to commuting costs such that persons at all locations, and with all commuting costs, have the same utility (e.g. Alonso, 1964). Thus persons with high commuting costs have lower land (and hence housing) costs. This is an important caution. High transportation costs are not necessarily a sign that persons are at lower utility levels. The motivation for labor market impact studies should not be that commuting costs lower individual welfare.

Instead, the motivation should be that high commuting costs can be a barrier to labor market success, and hence pose an equity issue or, equivalently stated, a barrier to poverty alleviation. Where land markets are not competitive and hence where land prices will not compensate for long commutes, labor market impact studies should still focus on transportation costs as a barrier to labor market success as opposed to a focus on the commute cost, even though in the absence of a well functioning land market high commute costs will not be compensated by lower land prices.

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[8] More generally, the behavioral context is two-fold, and includes the need to understand the location of transportation investments and the need to understand the determinants of individual labor market success. For a discussion, see "Socioeconomic Impact Assessment of Rural Roads: Methodology and Questionnaires," World Bank, July 30, 2003. The units of observation and behavior theory differ for both questions. When looking at individual labor market success, one might view transportation access as exogenous. The assumption of exogenous transportation access can be flawed, as individuals can move in ways that influence their access to jobs. At the community level, spatial patterns of political influence, organization, or social, economic or political resources across geographic units can explain the location of transportation investments. Whether and how this matters for impact evaluation depends on the evaluation methods used. If the researcher has an opportunity to use a DID estimator, with data on the same individuals before the transportation improvement is announced and after the transportation investment is completed, and if a control group of individuals is chosen from geographic areas that, before the transport investment was announced, were otherwise similar, the question of endogenous transport investment might be effectively controlled through careful selection of first control areas and then control groups. If, on the other hand, data are only available after the transport investment, but if a control group is chosen from among areas that are similar to those close to the transport investment, it may be necessary to control for the location of the transport investment. Our emphasis here is on understanding labor market impacts, as opposed to the determinants of transportation investment.

Turning to studies that attempt to link transportation costs, and hence accessibility, to labor market outcomes, a primary methodological issue is the endogeneity of residential location choices. This issue is commonly discussed in the U.S. literature on spatial mismatch (e.g. Ellwood, 1986; Ihlanfeldt and Sjoquist, 1990; Ihlanfeldt and Sjoquist, 1998; Holzer, Quigley, and Raphael, 2003). Persons who are motivated to seek employment might choose to live near job centers, creating a correlation between transportation access to employment and labor market success. Stated more formally, in a regression that predicts an indicator of labor market success (i.e. employment or, in developing countries, consumption), unobservable variables that predict labor market success (i.e. motivation, unobserved ability, reservation wage) could be correlated with measures of transportation access to employment, biasing cross-sectional estimates of the effect of accessibility on labor market success. To clarify this concept, an example regression based only on one cross-section of data is shown below.

$$L_i = \alpha + X_i\beta + \gamma ACC_i + u_i \qquad (3)$$

Where L = a measure of labor market success

X = control variables that are correlates of labor market success

ACC = measure of accessibility to employment (possibly a potential variable, as discussed in Section C)

u = error term

"i" indexes individuals

In the above regression, unobservable variables that are correlated with both labor market success and accessibility will induce correlation between the variable ACC and the error term, u. One can think of this as a selection problem; through residential location choices, individuals can "select" into the "treatment" – in this case, choosing to live in places with better transportation access to employment. Alternatively, it is an omitted variable problem. A long econometric tradition examines sample selection problems as omitted variable problems. See, e.g. Heckman (1979). One solution, applied in other problem areas but not commonly used in relation to this topic, would be to model residential location choices to control for the selection problem. In concept, this would be an acceptable approach, but in practice collecting the data needed to model residential location choices can create additional costs and econometrically identifying the system (finding variables that are exogenous to individual labor market choices) can be difficult. For that reason, the solutions used in the past have been of two types: (1) studying persons who are mobility constrained, and so who cannot select into the treatment group, or (2) if one can assume that the unobservables that correlate with "u" in the regression above do not vary over time, using research design, through for example a DID estimator with a carefully chosen control group, to identify the effect of transportation access on labor market success.

As an example of studying the mobility constrained, Ellwood (1986) studied teens, who typically live with a parent and so for whom job accessibility might be exogenous. In developing countries, if one can argue that the poor are mobility constrained, accessibility to employment may be exogenous to the individual.

Either argument has pitfalls. If mobility constraints limit the residential location choices of the poor while still leaving some poor persons with an ability to select residences based on job access, job access can be endogenous to labor market success. If unobservable factors are transmitted from parents to children (i.e. in the form of inter-generational transmission of attitudes or ability), the labor market success of teens can still be influenced by unobservable factors that, through parental location choice decisions, might be correlated with job accessibility. In short, cross-sectional studies are not ideally suited to establishing the causal impact of employment accessibility on individual labor market success, and recent research has turned to methods that apply DID estimation in the context of quasi-controlled experiments.

A quasi-experimental study, set up for DID estimation, would follow the steps, described below.

1. *Identify the "natural experiment":* Quasi-experimental research, unlike cross-sectional studies, identifies planned transportation improvements for "before and after" study. Major investments that will importantly improve transportation accessibility for some portion of an urban area's population are the best candidates. Smaller projects, with limited impacts on travel times, are less likely to impact labor market success.

2. *Choose the treatment and control areas:* These areas can be selected based on the geography of the transportation improvement and the metropolitan area. The treatment area would be the area where transportation access to employment is most improved. This might be the corridor on either side of a new bus rapid transit system or the area around a new rail station. The size of the area should be based on the typical mode used to access the new transportation project. As an example, in many developing countries, the urban poor access public transit by walking, so the size of the treatment area should correspond to typical walking distances in the metropolitan area. The control area should be distant enough from the new project that the transportation investment will not affect travel times in the control region. On other characteristics that would influence labor market success – income levels, education levels, and (as appropriate) race, ethnicity, or class – the treatment and control areas should be as similar as possible. Researchers might wish to first choose a control region, and then use statistical matching techniques to find zones within the control region that most closely match the characteristics of the zones in the treatment area, thus adding a level of statistical rigor to the selection of the control area. See van de Walle and Cratty (2002) for an example.

3. *Choose treatment and control subjects:* Now it is time to choose individual study subjects from each of the treatment and control areas. Once a sample of individuals in the treatment area has been drawn, the researcher could use a propensity score matching technique to choose individual subjects in the control area who most closely match the characteristics of the treatment subjects. This match should be based on data collection before the transportation improvement has had an impact. The data needed for this match will often have to

be collected from surveys, as described in Step 4, below. This means that samples in the treatment and control area need to be drawn first, and then a pair-wise propensity score matching method can be used to form closest matches between treatment and control subjects. At the researcher's discretion, treatment subjects without a close control match might be excluded from the analysis.

4. *Use a household survey to collect data on individuals in both the treatment and control areas:* The survey should include questions about labor market success (including consumption if that is judged an important indicator of labor market success), as discussed in Sections C and D above.

5. *Obtain accessibility data for both before and after the transportation improvement was built:* Travel times and costs from the treatment area to job centers should be collected before and after the transportation project opens. As mentioned in Section C, travel times based on network analysis or verified agency data are preferred over self-reports of commute times and costs, as individual commuting behavior can be endogenous to the labor market outcomes being studied. Employment data needed to construct accessibility measures, through, e.g., potential variables, should also be collected. In some cases the researcher might judge that the spatial distribution of employment hardly changed between the "before" and "after" observation periods, and so data on travel times and costs might be the only "after" information needed.[9] Also, the researcher might judge that transportation accessibility was unchanged in the control area, and so could collect data on accessibility changes in the treatment area only. Yet if there is any doubt about whether accessibility in the control area also changed, accessibility data should also be collected for the "before" and "after" case in the control area. One of the strengths of DID estimation is being able to compare *changes* in the outcome variable to *changes* in an independent variable. For that reason, collecting "before" and "after" accessibility data in both the treatment and the control areas is recommended.

F. Examples of Labor Market Impact Evaluation

Holzer, Quigley, and Raphael (2003) examined the impact of an extension of the Bay Area Rapid Transit System (BART) on hiring practices of firms in the San Francisco Bay Area. While that study looked at firms, rather than individuals, the research approach and methods are a good example of the DID techniques described above.

The BART is a heavy rail urban transit system that connects downtown San Francisco to the suburbs to the east across San Francisco Bay. The rail system is oriented radially inbound, and was originally designed to bring workers from East Bay suburbs

[9] Such a judgment becomes less valid as the time period from project completion to "after" data collection grows longer, allowing time for firms to change their locations. This likely is not to be an important issue for follow-ups that are on the order of a year or less after the completion of the transportation project.

through Oakland and across (actually under) the bay to San Francisco. In 1997, the 13.5 mile Dublin-Pleasanton extension opened. The extension included two new stations, one in Oakland and the other in job-rich suburbs to the east. This extension connected the low-income, minority communities in Oakland to job-rich suburbs to the east, facilitating "reverse commuting" from the more centrally located Oakland to employment opportunities in growing suburban office centers. (The Dublin-Pleasanton line is similar to Sao Paulo's Line 4, which will connect the central area of the city to job growth areas to the southwest.)

Holzer, Quigley, and Raphael (2003) tested the hypothesis that the enhanced accessibility provided by the Dublin-Pleasanton line would increase the hiring of minorities into low-skilled jobs offered by firms in the area near the eastern terminus of the line, at the Dublin-Pleasanton station. This hypothesis reflects the history of spatial mismatch studies in the United States. The spatial mismatch hypothesis, as originally proposed by Kain (1968), suggested that segregation in the housing market in the United States limits the labor market opportunities of blacks by limiting their spatial access to employment centers. Against that backdrop, Holzer, Quigley, and Raphael (2003) were specifically interested in whether a transportation improvement that linked low-income minority residences to job centers would increase the likelihood that firms hire minorities. While the interest in hiring a particular racial or ethnic group may not apply to some settings in developing countries, the Holzer, Quigley, and Raphael study gives insight into whether transportation access is linked to labor market outcomes.

Holzer, Quigley, and Raphael (2003) first document that the Dublin-Pleasanton line improved transportation access to outlying jobs for residents in the Oakland area. Public transit in-vehicle travel times dropped from 20-22 minutes (Oakland to Dublin-Pleasanton station) before the new line opened to 15 minutes after the BART extension was built.[10] Headways were reduced from 20-40 minutes peak, 60 minutes off-peak before the new line opened to 15 minutes peak, 20 minutes off-peak after the new line. Service hours were lengthened when the new line opened, and transfers were reduced. The out-of-pocket cost to travel from Oakland to Dublin-Pleasanton was not increased when the BART extension opened (Holzer, Quigley, and Raphael, 2003, Appendix 1).

Holzer, Quigley, and Raphael implemented a DID estimation technique by examining hiring practices in firms within 6 miles of the new Dublin-Pleasanton station (the experimental or treatment group) and firms from 6 to 12 miles from the new station (the control group) before and after the station opened. The researchers administered a survey to firms in the two study areas four weeks before the new station opened, and again a year after the initial survey. They asked firms about the race and ethnicity of the establishment's most recent hire. Using responses to that question as a measure of propensity to hire minorities, Holzer, Quigley, and Raphael (2003) formed the difference in the fraction of firms who said the last hire was a minority in the near group (within 6 miles from the station) minus the distant group (6-12 miles from the station). The authors

[10] Public transit travel time before the opening of the Dublin-Pleasanton BART line was via BART Express Shuttle Buses.

then subtracted the "before construction" value for that "near minus far" difference in minority hiring propensity from the "after construction" value for the same "near minus far" difference. This DID estimator showed that firms within 6 miles of the new station increased their hiring of Latinos, compared to the firms in the 6 to 12 mile range. The increase was statistically significant. To illustrate the implementation of a DID estimator, results from Holzer, Quigley, and Raphael (2003, Table 3, partial) are shown below. The bottom row shows before-after change in the probability that the last hire was Latino (a difference), and the right-most cell in the bottom row shows how that before-after change differs between near and far firms (the differences in differences, or DID, estimator).

Table 1: Illustration of Differences in Differences Estimation: Probability that Last Hire is Latino, Based on Proximity to BART Station, before and after new station

	Near Station (within 6 miles)	Far from Station (beyond 6 miles)	Difference = (Near – Far)
Before	0.194 (0.041)	0.316 (0.054)	-0.122 (0.067)
After	0.301 (0.048)	0.211 (0.047)	0.091 (0.068)
Difference (After – Before)	0.108 (0.064)	-0.105 (0.066)	0.213 (0.093)

Note: Standard errors are in parentheses. Results are from 188 completed surveys of firms.
Source: Holzer, Quigley, and Raphael (2003) Table 3, Panel C, Dublin/Pleasanton Station Only.

Regression analyses that controlled for the percentage of the firm's workforce that was unionized, firm size, industry classification, the race and ethnicity of the hiring official, and recruitment methods confirmed the result. Firms near the Dublin-Pleasanton station increased their hiring of Latinos after the station was opened compared to firms 6-12 miles from the new station (Holzer, Quigley, and Raphael, 2003).

G. Special Methodological Considerations for Labor Market Impact Evaluation in Developing Countries

The literature on labor market outcomes and transportation access is mostly from developed countries. In adapting that research to cities in the developing world, some special considerations arise. Here we list four key issues, and discuss how those issues will influence research design and data collection.

First, kinship networks are an important part of an individual's labor market success in many parts of the developing world. Anecdotal evidence raises the possibility that such informal networks, based on family ties or ethnic group membership, could be important sources of information about job opportunities. Given that, how would labor outcome evaluations be modified to account for this?

In a DID study with matched control and experimental groups, if the kinship ties or group memberships are fully controlled by the matching criteria, there would be no problem. In this framework, the match would perfectly randomize the associational ties across control and experimental groups. Certainly that is the goal of a careful DID study, but researchers should be alert to cases where either that is not possible or where associational ties will be correlated with patterns of pre-project or post-project transportation access. Researchers should consider the labor market context for their evaluation, and assess whether kinship or group ties vary in ways that would correlate with the patterns of transportation access being studied. If so, the evaluation should control for those kinship or group ties. When primary data are collected, this would include surveying individuals about their membership in groups that might be relevant for labor market success, and ideally using group membership as a variable in a propensity score match.

Second, transportation projects might influence the choice of whether to participate in the formal or informal economy. This is a choice margin that is typically not reflected in the research from the developed world. Because of the importance of the informal economy, surveys that gather data for impact evaluation should include questions about whether the subject participates in the formal or informal economy. Once those data are gathered, the choice margin between the formal and informal economy can be treated both as an outcome variable (examining whether a transport project influence the choice of formal or informal work) and an independent variable (examining whether consumption or income varies with the choice of formal or informal work.) Studies examining the distinction between formal and informal work should make a distinction between short-run and long-run effects. A transport project that influences the choice of work in the formal or informal economy could lead to differences in individual consumption or income over both timeframes. In the short-run, the study should collect data that would allow researchers to determine whether any impact on the choice of formal or informal work lead directly to differences in individual well being, which will typically be measured by consumption. Any short-run differences might vary over the long-run. As an example, income growth might differ in the formal and informal economies, or one sector might be more vulnerable to macroeconomic cycles and shocks. Thus when transport projects would likely importantly affect the choice between informal and formal labor, studies should look not only for labor market impacts in the short-run, but should also be able to track individuals to examine how the changed choice margin does or does not lead to differences in labor market outcomes over the longer term.

Third, transportation access to jobs might have importance distance threshold effects in the developing world that would not be evident in U.S. studies. The importance of walking as a commute mode in developing cities suggests that the margin between motorized and non-motorized access to employment could be substantially more important in the developing world. Transport access improvements could thus work with important threshold effects. As an example, transit extensions could allow a person who previously walked to work to access distant employment. For that reason, measures of improved accessibility might search for impacts across reasonable motorized/non-motorized thresholds. The implication is that simply gravity variables of transportation access might need to be adapted to allow for threshold effects. As an example, a project that allows persons to switch from walking to transit commutes might use a "before project" gravity measure of employment access based on reasonable walking distances, and an "after project" gravity measure of employment access based on transit commute costs (where cost would reflect both out-of-pocket and time cost). The walking gravity variable would not include jobs outside of the catchment area for pedestrian commutes, while the transit-based gravity variable would have a considerably larger catchment area.

Fourth, some analysts have speculated that endogenous residential location choice might be less of an issue in cities with low residential mobility. There is no doubt that the problem of residential selection (based on residential relocation) is linked to the ease of residential mobility, and in societies where persons are relatively immobile endogenous residential location choice might not be as serious a methodological issue as in the U.S. Yet the degree of residential mobility is an empirical question, and even in cities with formal barriers to mobility, persons often move despite the formal barriers. For that reason, and because residential location choice is a key source of endogeneity bias in evaluations of transportation and labor market outcomes, analysts are advised to generally proceed as if residential selection will be an important methodological issue.

Note that for many of these issues, qualitative research can provide initial insights into the magnitude of the methodological problem. In particular, there might initially be limited data on the role of kinship or group membership, the choice of formal or informal labor markets, or the importance of walking as a commute mode, but all three are key considerations for labor market evaluations of transport projects in developing cities. In all three cases, preliminary qualitative studies can shed light on both the magnitude and the nature of the effects. This might be particularly useful in understanding the importance of kinship networks or group membership. Such qualitative studies can lay the groundwork for later survey research that would support a quantitative impact evaluation, controlling for factors that the qualitative research suggested would be important.

V. Evaluating the Impact of Transport Projects on Economic Opportunities

A. Transportation and Economic Opportunities

Transportation can relate to poverty alleviation through economic growth that increases labor market opportunities for the poor. This is the common reasoning for pursuing transportation projects as part of a poverty alleviation program. Transportation accessibility is important for economic competitiveness, and hence economic development (e.g. Llewelyn-Davies, Banister, and Hall, 2004; Cities on the Move, esp. Chapter 2). Yet with the exception of recent research there have been few evaluations that examine whether and how transport projects influence economic development in ways that relate to poverty alleviation.

The focus of this section is on studies that can illuminate how transport projects impact a metropolitan economy. There are two broad classes of studies in this area: (1) studies of firm location and (2) students of agglomeration benefits linked to transport investment or system efficiency. These two mechanisms are distinct from the job-matching effects that were implicit in the labor market impact evaluations discussed in Section V. While one might imagine that the two effects could be intertwined – an increase in the number of jobs might improve the prospects for an individual's labor market success through either a higher probability of employment or a better job match – here we discuss labor demand and labor supply as distinct phenomena.[11]

Those two phenomena – firm location and agglomeration economies – require distinct impact evaluation methods. Studies of firm location can be within metropolitan areas, often focused on a particular transportation corridor or policy. Those studies will either implicitly or explicitly focus not only on firm location and spatial patterns of employment growth, but also on questions that relate to possible shifts of employment growth within a metropolitan area. Agglomeration studies, while they can focus on shifts in the location of economic activity (i.e. Boarnet, 1998; Lall and Shalizi, 2003), are typically inter-metropolitan in focus. The questions asked by agglomeration studies often require that several regions or metropolitan areas be compared. While recent research has linked transportation to firm-level productivity benefits and hence to patterns of agglomeration (e.g. Lall, Funderburg, and Yepes, 2004), the focus here will be on firm location studies that can be more directly linked to the poverty alleviation impact of specific projects.

Here we analyze firm location as a distinct phenomenon from an individual's access to jobs, as the behavioral actors, firms and residents, differ. Yet note that the tie from firm location to poverty alleviation is the same as the link from an individual's improved job access to poverty alleviation. Firm location patterns can increase or reduce the access of the urban poor to jobs, and hence have an impact on poverty through labor market outcomes of the poor. A full evaluation of the poverty impact of firm location could combine the methods described here with methods for studying the impact of job access on labor market outcomes, described in the previous section.

B. Variables

For firm location, the firm is the unit of analysis. Dependent variables measure firm activity. Examples of dependent variables include counts of firms or establishments, firm or establishment births and deaths, and employment levels and changes in those

[11] Of course, one must control for the relation between the two in impact evaluations. For example, an increase in labor demand (number of jobs) could have an impact on labor supply, and vice versa. Later in this section we focus on two methods for controlling for the general equilibrium interaction between the labor market and economic opportunity effects discussed in Sections VI and VII. One method is through careful quasi-experimental research design (e.g. Chalermpong, 2004). The other method explicitly controls for factors such as labor supply and local market demand while examining questions of firm location or employment growth (e.g. Boarnet, 1994).

levels. Employment levels and changes in those levels give the best information about job opportunities, and hence potential impacts on poverty. Yet firm or establishment births or other information about locations can also give insight into the spatial pattern of economic activity.

Importantly, the variables mentioned above must be linked to geography. Transport's influence on economic activity, especially within an urban area, is inherently a question about the location (or spatial pattern) of economic activity. Thus the variables mentioned above should have address information or coordinates that allow geocoding the data to a GIS map, or relating the data to reasonably fine geographic areas or zones. Analysts will typically use one of three geographic techniques to ask and answer the explicitly spatial question, "How do transportation projects influence the location of economic activity?" Those geographic techniques are: (1) matching firm or establishment data to catchment areas or corridors around the transport project, (2) using firm or establishment data matched to zonal boundaries to provide geographic detail throughout the metropolitan area, or (3) measuring the distance (straight-line or network, possibly adjusted for travel times) from firms or establishments to transport projects.

Analysts should understand the difference between firms and establishments and the difference between employment-at-place (jobs) and employed residents. Firms can have multiple locations, with each location called an establishment. Establishments are a preferred unit of observation for data collection. In particular, care should be taken to ensure that jobs are geographically linked to the establishment, rather than having employment for several different establishments reported at one location – e.g. the location where a multi-establishment firm keeps its human resources department. Census data based on households will sometimes give information on where employed residents live, but given commuting this can be quite distinct from the spatial pattern of employment-at-place (or employment) that is an object of study in this section.

The key independent variable, as with labor market studies, is a measure of transportation access. Unlike labor market studies, it is more difficult to understand what firms would like to access. Firms might desire access to various things – shipping nodes, customers, labor supply pool, resource inputs, agglomeration benefits (i.e. knowledge pools), or crucial infrastructure. Different firms will no doubt value accessibility to different things. Given the difficulty in making *a priori* judgments about what firms might desire to access, a common strategy in firm location studies is to simply measure accessibility to important transportation improvements. Distance from a major new roadway or rail node is often used as the accessibility variable in firm location or employment growth studies, for example.

Researchers will have to control for other non-transportation factors that can influence firm location and growth. Large literatures in related fields can provide guidance. Firm location has been studied in literatures that include transportation (i.e. Holl, 2004a and 2004b), economic development (i.e. Fisher and Peters, 1997; Gabe and Kraybill, 2002), and state or local tax and fiscal policy (Fox, 1986; Papke, 1991). Independent variables typically include measures of local agglomeration benefits, market demand, labor supply, locational characteristics such as major business centers, universities, or government centers, and the location of major public infrastructure investments. Agglomeration benefits are often proxied by measuring the number and industrial mix of jobs near a firm, sometimes using a potential function (as described in Section V) to allow the effect of agglomeration benefits to damp with distance. Market demand and labor supply are typically measured by population, again damping by distance. For a discussion or specific implementations and examples, see, e.g., Boarnet (1994) and Boarnet, Chalermpong, and Geho (2005).

C. Data Sources

Data for firms, establishments, or employment are most typically available from administrative sources. Importantly, censuses of households or residents will not give information on employment-at-place, and instead researchers will need to turn to censuses of firms. Most such censuses track establishments (plants, branch locations, and the like), since firms with multiple locations can have many different establishments. As an example, Holl (2004b), whose study of Portugal is described in sub-section E below, used data on firms and plants (or establishments) from annual surveys conducted by the Portuguese Ministry of Employment and Social Security. Most typically national or sub-national employment agencies are tasked with tracking data on firms and establishments, and researchers should turn to those agencies when looking for data on this topic. For large metropolitan areas, a regional or metropolitan planning agency might also have data on firms, establishments, and employment. In some cases, transportation planning agencies might have collected firm information as part of their travel modeling functions.

The key independent variable, access to transport projects or improvements, can usually be easily measured with a GIS. Straight-line or network distances from business establishments to transport improvements are typically used to measure access.

D. Methodological Approaches and Challenges

There are three methodological challenges in studying the impact of transport projects on firm location – (1) controlling for factors other than transportation access, (2) controlling for the interaction between labor demand and labor supply within metropolitan areas, and (3) allowing a sufficiently long timeframe to observe an impact.

The variables that influence firm location and employment growth at intra-metropolitan scales are well known, and fall largely into three classes – fiscal policy variables, agglomeration variables, and other location specific amenities (e.g. crime rates, labor quality). Most studies of transport projects and firm location control for these other factors. For examples that give specific variables, see, e.g. Holl (2004b) and Bollinger and Ihlanfeldt (1997). The literature on firm location, more generally, can also give insights into specific control variables. See, e.g., Bollinger and Ihlanfeldt (2003), Edmiston (2004), Gabe and Bell (2004), and Guimaraes, Figueiredo, and Woodward (2004).

Transport projects can influence the location of both labor demand and labor supply by influencing the location of firms and residents. Researchers have typically controlled for this in one of two ways. Some studies of firm, establishment, or employment location include measures of labor supply as independent variables. This is possible if the labor supply measures are exogenous to the firm, establishment, or employment dependent variable, as might be the case when data on individual firms or establishments is the dependent variable and labor supply is measured for surrounding areas that are large enough that the labor supply area is unaffected by one firm. More rigorously, some studies have treated both labor supply and labor demand as endogenous variables. See, e.g., the simultaneous population-employment regression model used in Boarnet (1994a and 1994b) and Bollinger and Ihlandfeldt (1997).

Changes in business location are not instantaneous. Most studies of transport and firm location have examined time periods that range from ten to twenty years. Recent research has found impacts as quickly as four years after new transport projects have opened (Chalermpong, 2004), although impacts over such timeframes are likely not the full long-term impact.

E. Examples of Evaluations of Firm Location and Transport Investment

An example of a regression study of firm location is Holl's (2004b) study of the impact of motorway improvements on the spatial pattern of plant births in Portugal. From 1980 to 1998, Portugal's motorway (highway) network increased from approximately 200 kilometers concentrated around Lisbon and Porto, the two largest cities, to a nationwide network of 1,300 kilometers. Holl (2004b) uses data on all plant (or establishment) births from 1986 through 1997 to study the impact of these new highways.

Holl's (2004b) data are for 275 municipalities – geographic zones that completely cover Portugal's geography. Holl (2004b) estimates a negative binomial fixed effects regression for plant births by zone from 1986 through 1997. The negative binomial specification is appropriate when the dependent variable is count data (i.e. non-negative integers) and when many cells have zero values. The negative binomial is an extension of Poisson regression that relaxes the Poisson assumption that the mean and standard deviation of the distribution are equal (Hausman, Hall, and Griliches, 1984 or Cameron and Trivedi, 1998). With annual data, each municipality is given a fixed effect to control for time-invariant unobservables unique to the municipality.

The dependent variable is the number of plant births, by year, in the municipality. The transportation access variables are a series of dummy variables that describe the distance from the municipality to the nearest motorway in ten-kilometer bands, from 0-10 kilometers to greater than 50 kilometers. Other independent variables measure market access, two agglomeration economy variables (a measure of specialization and a measure of diversity following Glaeser, Kallal, Scheinkman, and Shleifer, 1992), and three labor force characteristics (wage costs, education and experience levels of the labor force in a municipality.) The effect of infrastructure, represented by the coefficients on the motorway distance dummy variables, is estimated while controlling for these variables, which include a measure of market access. The market access measure was a potential variable, similar to the potential variable described in Section V. To illustrate some of the common specification choices made when constructing potential variables, the form of the potential variable in Holl (2004b) is shown below.

$$ACC_j = \sum_k W_k \exp(-bc_{jk}) \qquad (4)$$

Where ACC_j = market accessibility measure for municipality "j"
W_k = a measure of the market size in municipality "k"
c_{jk} = a measure of the impedance (travel cost) from municipality "j" to "k"
b = a damping parameter.

Holl (2004b) fit separate regressions for 22 different industries. Her results provided evidence that municipalities closer to a motorway had, ceteris paribus, more new plant births, although the magnitude and significance of this effect varied by industry.

An example of a quasi-experimental methodology applied to a similar question is Chalermpong's (2004) analysis of the link between a new highway in Los Angeles and employment growth in surrounding census tracts. The 17 mile long Century Freeway (Interstate 105) opened in 1993 and transects lower income and middle income blue collar neighborhoods in and near South Central Los Angeles. Chalermpong (2004) examined how employment growth is related to the opening of the freeway by conducting a quasi-experimental study that mimics the logic of DID estimation.

Chalermpong (2004) analyzed employment growth in census tracts, choosing the 63 tracts within a mile on either side of the Century Freeway as the experimental group. Chalermpong chose control tracts from the 219 tracts within South Central Los Angeles that were more than two miles from the Century Freeway. Chalermpong (2004) used a matching technique to select pair-wise controls – one control for each experimental tract – based on population and employment levels and growth rates, industrial composition, land use mix (such as amount of land in residential and other uses), median property value, housing age, distance from the coast, and presence of a nearby freeway (other than the Century Freeway). The match was based on characteristics in the 1980 – 1990 time period, before the Century Freeway opened. Following a DID logic, experimental and control tracts were matched to be similar in the 1980 – 1990 time period, before the new highway opened, and employment growth in the experimental and control tracts was compared for the 1990 – 1997 time period, which included the first years after the Century Freeway opened.

Chalermpong (2004) did not use propensity score matching techniques to choose control tracts. Instead, he used a technique that follows Rephann and Isserman (1994), choosing control tracts that minimize the matrix distance (for the multiple matching characteristics used) between an experimental and its control tract. See Chalermpong (2004) or Rephann and Isserman (1994) for a discussion. While the matrix-distance minimization technique provides a data-based method for choosing similar control tracts, propensity score matching is easier to implement when one can specify a logit regression for membership in the experimental category.

Chalermpong (2004) compared employment growth rates in the experimental and control census tracts in two time periods, 1980 – 1990 (before the opening of the Century Freeway) and 1990 – 1997 (after the opening of the Century Freeway). In the "before" period, 1980 – 1990, employment growth rates were not statistically significantly different across the experimental and control tracts and, in terms of magnitudes, employment growth rates across experimental and control tracts did not differ by more than five percentage points in the 1980 – 1990 time period. After construction of the Century Freeway, the 1990 – 1997 time period, all of Chalermpong's (2004) preferred matching specifications showed a statistically significant difference in employment growth rates between experimental and control tracts. The experimental tracts (within a mile of the new highway) had employment growth rates that exceed the control tracts by between 129 percent to 215 percent, suggesting increased employment growth in the corridor surrounding the new highway.

Chalermpong (2004) extended his analysis to examine whether the employment growth near the Century Freeway was net growth or whether it was in part a redistribution of economic activity that, absent the new highway, would have located elsewhere in the metropolitan area.[12] Chalermpong (2004) used matching techniques to develope a second control group, chosen from among the 333 census tracts in the San Fernando and San Gabriel Valleys, over 10 miles from the freeway. Chalermpong compared employment growth rates in experimental tracts (within a mile of the new highway) to the first control group (matched tracts within South Central Los Angeles and farther than two miles from the highway) and to the second control group (matched tracts more distant from the highway than were the South Central Los Angeles control group tracts). If employment growth near the Century Freeway in the 1990s was a redistribution of jobs that, absent the highway, would have located elsewhere in South Central Los Angeles, Chalermpong (2004) argued that employment growth rates in the experimental tracts would exceed growth rates in the South Central control group and employment growth rates in the South Central control group would be lower than employment growth rates in the more distant San Fernando – San Gabriel control groups. Chalermpong (2004) found some support for this redistribution hypothesis.

Both the studies by Holl (2004b) and Chalermpong (2004) used two techniques that lessened the need to give explicit attention to the general equilibrium interaction between labor demand (firm growth or employment growth) and labor supply (growth in employed residents). Holl (2004b) and Chalermpong (2004) both examined natural

[12] As background on the question of whether economic growth near highways is net growth or shifts of economic activity across the landscape, with an empirical test using California data, see Boarnet (1998).

experiments – the construction of new highways – that provided some ability to identify the effect of transportation infrastructure. Both also adopted research methods that control, at least in part, for changes in labor supply. Holl (2004b) included characteristics of the labor force in her regression analysis, but notably labor supply was not explicitly included in the regression. Chalermpong (2004) used a quasi-experimental technique that matched experimental census tracts, near the new highway, with control tracts based in part on census tract population levels and growth rates. This can help control for labor supply to the extent that the quasi-experimental technique obtains a good match.

More generally, studies of the effect of transport projects on firm location should take care to identify the effect of the transport project while controlling for changes in labor supply. One way to do this would be to adopt the two-equation population and employment growth model popularized by Carlino and Mills (1987) and extended to intra-metropolitan settings by Boarnet (1994a and 1994b). Such a model can be used to explain, for example, census tract population and employment as a function of several variables, including transportation access. Because labor demand (employment) and labor supply (population or, in more refined cases, measures of employed residents) are endogenous to the model, such regressions can yield estimates of the impact of transport projects on labor demand while controlling for changes in labor supply. For an example, see Bollinger and Ihlanfeldt (1997), who use a two-equation simultaneous regression system for population and employment growth to examine the impact of the Atlanta rail rapid transit system.

VI. Evaluating the Impact of Urban Transport Projects on Access to and Use of Services

A. Background

Just as improved transportation access might allow persons to participate more often or more regularly in the labor market, improved access could lead to increases in the consumption of services that improve the well being of the poor. In an impact evaluation of road improvements in Morocco, a World Bank study found that improved transport access led to a significantly higher enrollment in primary education. For girls, the impact was especially large, with enrollment in primary schools more than tripling over the period of the road improvements. The study also found that persons living near the improved roads increased their use of health care. The Morocco study compared persons living near improved roads with those living near unimproved roads, and then implemented an approach similar to DID by examining individual behavior before and after the road improvement. In the Morocco study, the choice of “control groups” (unimproved roads) was not based on formal matching or propensity score techniques, but instead on researcher judgment. Still, the study gives important information about how transport improvements can not only increase participation in education, but can also impact other social goals, such as gender imbalances in education or other services.[13]

The evidence from Morocco is from a rural road program. One might hypothesize that access to schools and health care is better in urban areas than in rural areas, and in turn one might hypothesize that improvements in transportation access in urban areas, if access to services is already good, might have smaller impacts than in rural areas where access is poor. While this is a caution in applying rural examples to an urban setting, there are likely many urban settings in the developing world where access to basic services could be substantially improved by transport projects, and in those instances the insights from the Morocco study might be applied to formal evaluations of the link between transportation access and the use of basic services.

B. Variables

The variables needed for an evaluation of urban transport projects and consumption of services are similar to the variables needed for a study of the link between transport access and labor market success. The independent variable, transportation access, could be measured in ways described in Section V. For that reason, the focus here will be on describing ways to measure the dependent variable.

To ground the discussion, this section will be based on two examples – education and health care. The dependent variables for an impact evaluation would be measures of individual consumption of education or health care. Access to other basic needs, such as clean water, could also be studied using domain-specific knowledge to form an appropriate measure of the dependent variable.

A dependent variable measuring education could take three forms: (1) a measure of distance or travel time from school(s), (2) a measure of consumption of education, such as the frequency or regularity of schooling, and (3) a measure of completed education milestones, such as years of schooling obtained, enrollment, or dropping out.

Distance or time measures could be obtained from the sample questions listed in the appendix of Baker and Denning (2005). Those sample questions ask, for example, how far an individual's home is from key services and how long it takes to travel to those services. Note though that this is more a measure of access to the services – the independent variable – than a measure of education obtained, and so this type of measure should be used to measure access rather than educational outcomes.

[13] The information in this paragraph is drawn from "Socioeconomic Impact Assessment of Rural Roads: Methodology and Questionnaires," World Bank, July 30, 2003, draft, pp. 38 – 40. That report summarized the evaluation of the Morocco rural roads program, "Impact Evaluation Report – Socioeconomic Influence of Rural Roads (Fourth Highway Project, Loan 2254-MOR), Operations Evaluation Department, World Bank, Washington, D.C., June 1996, Report No. 15808-MOR.

Consumption of education is often measured based on the completion of milestones, such as years of schooling completed. Such data would typically come from special individual surveys. As an intermediate step, one might also survey respondents about the frequency and regularity of school attendance. Travel diary data could be used to infer some information about school attendance. As an example, one could measure the proportion of all trips or the number of trips devoted to education. The difficulty with this is twofold. First, travel diary surveys typically cover only a small time period, often one or two days, and as such unmodified travel diaries would be poorly suited to give information about the regularity of school attendance. Second, most education studies would care primarily about the completion of milestones and measures that give information about both the quantity and quality of education. Neither could be inferred from an unmodified travel diary survey, which typically has information only about trip-making. Yet travel surveys or associated surveys could be modified to add questions about the completion of schooling milestones, enrollment, and the like.

For health care, a researcher could more feasibly use an unmodified travel diary survey to form a dependent variable that measures health care consumption. The number of trips to health clinics or for health care purposes could be a measure of consumption of health care. As with schooling, though, travel diaries that only cover one or two days would not give good information on the consumption of health care, since such trips are not typically daily activities. Instead one would desire a longer time window. For that reason, special survey questions that ask respondents about their health care visits for the past month might be added to the survey. Also, researchers might wish to measure specific health care outcomes, such as immunizations. Depending on the reliability of self-reported data, such specific outcomes could be measured with individual surveys.

C. Data Sources

The data sources for these evaluations would be similar to data sources described in Section V, with the exception of the need to do special surveys to study domain-specific dependent variables (i.e. education, health care, or similar services or basic needs). As an alternative to survey data, administrative data might be available in some cases. As an example, some developing nations might have education data (enrollments, completion rates, or similar data.) Such data might be available aggregated by geographic area; one would be less likely to find administrative data that could be used at the individual level. Aggregate data were not recommended for labor market studies, because of endogeneity problems that require individual data. In the case of consumption of services, aggregate data could be more useful, even if not ideal. This is discussed more in the sub-section on methodology, below.

D. Methodological Approaches and Challenges

As with labor market studies, an ideal methodology would be to use DID estimation with a carefully and systematically chosen control group. Some studies, such as the evaluation of the social impact of road projects in Morocco, have used such techniques. The use of experimental (treatment) and control groups chosen before the urban transport project is implemented can help control for possible endogeneity. The primary source of endogeneity in urban transport evaluations is that persons might choose their location, and hence their accessibility, based in part on desired activities or consumption patterns.

The analogy, in the case of education or health care, is that persons might locate in part to be accessible to schools or health clinics. Studies in the United States, for example, show that school characteristics are an important determinant of house price variation within urban areas (e.g. Black, 1999; Bogart and Cromwell, 1997; Bogart and Cromwell, 2000), and by extension unless home prices adjust to fully compensate for differences in school quality as perceived by all persons (an unlikely occurrence) school quality is a determinant of intra-metropolitan residential location choices in the United States. Yet in developing countries, where residential mobility is somewhat lower than in the United States, one might expect to find a smaller link between residential location choice and school quality. One might argue that persons would be even less likely to move in relation to their desired access to health care.

Without firm evidence on the determinants of residential location choice, one is left to conjecture how important factors like education and health care access are for individual location choices in the developing world. If individual location decisions rest largely on other factors, the endogeneity problem in the context of transportation access and services might be less severe than in the context of transportation access and labor market success. For that reason, while DID estimation with a control group is still the preferred approach, research designs that use cross-sectional data with simple correlations between access and service consumption might be acceptable in this domain. Such research designs might also make use of aggregate data on schooling (e.g. relationships between school attendance rates and distance from schools for geographic areas) in cases were more ideal individual data are not available.

E. Examples of Studies of Transportation and Access to Services

A recent World Bank sponsored study of the economic and social impacts of a rural road program in Peru provides an example of an impact evaluation in this area, albeit in a rural rather than an urban setting (*The First Phase of the Rural Roads Program: An Economic and Social Impact Assessment*, no date). The program rehabilitated about 8,800 kilometers or rural road network in twelve rural districts in Peru in the late 1990s. In 2000, an evaluation examined the impact of the rural road program by implementing an experimental control group study. The experimental group was the area around 74 rehabilitated roads, with a similar 74 non-rehabilitated roads chosen for comparability. Surveys were distributed to about 2,000 households in the twelve districts. Respondents were asked questions that elicited information about their schooling and health care usage. The rural road rehabilitation program had been completed by the time the survey was administered, but retrospective questions were used to elicit information about baseline, or pre-rehabilitation, conditions. Thus a DID research design was implemented.

Retrospective questions are not as ideal as surveys administered before and after the transport project is built. Similarly, it is unclear how sophisticated the matching techniques for the control group were. Yet the Peru study illustrates that even when an opportunity for baseline data is missed, retrospective questions can provide some insight into pre-project conditions. The quality of retrospective questioning in a DID framework hinges importantly on whether memories about baseline conditions would differ across the experimental and control groups.

The Peru study did not show any impact on education, when measured by attendance, drop-out rates, or enrollment (*The First Phase of the Rural Roads Program: An Economic and Social Impact Assessment*, no date, p. 111). Qualitative interviews administered as part of the study did reveal, though, that study subjects perceived benefits in the form of greater safety for the trip to school and increased punctuality among students and teachers after the rural road program was completed (*The First Phase of the Rural Roads Program: An Economic and Social Impact Assessment*, no date, p. 111). This reveals the importance of qualitative data that can elicit information about service quality in addition to quantitative measures of consumption.

In another example, Wright et al. (1984) simulated the impact of transport uncertainties on health care operations in Ghana. Wright et al. (1984) cite relationships, developed by the Ghana Ministry of Health, that conclude that persons will not travel further than 8 kilometers for most health care. Using those relationships, Wright et al. (1984) simulate the operation of a tiered health care system that provides primary care at clinics within 1.6 kilometers of all rural residents, larger centers that provide, e.g., immunization and follow up for tuberculosis and leprosy within 8 kilometers of all rural residents, and hospitals which will supervise the smaller centers and clinics within 40 kilometers of all rural residents. Wright et al. (1984) focus on the logistics of sending supplies and personnel between the larger and smaller centers, and the authors simulate the impact of random transport delays, caused by, e.g., vehicle breakdowns, on the operation of such a health care system. This approach, while useful, is not likely to be the common application of evaluation studies. Instead, evaluation researchers might use data from health care utilization studies to infer relationships between health care visits and transport cost, likely measured by travel time. Such information could be used prospectively before projects are implemented to assess how improvements in transportation access might increase health care usage. Note, also, that the methods in Wright et al. (1984) were an ex ante simulation of the impact of policy change, not an ex post evaluation of a policy change as has been discussed in this paper. If one shifts attention to ex ante predictions of policy changes, simulations could play a larger role.

VII. Evaluating the Impact of Urban Transport Projects on Land Prices

A. Background

Urban transport projects influence land prices and, through the impact on land prices, location choices. Standard urban economic theory (e.g. Alonso, 1964; Mills, 1972) holds that the impact of transport on location choices is due to the impact of transport on land prices. Thus land price impacts could be viewed as intermediary impacts. Yet there are some advantages to studying land prices directly. Land price impacts will likely be realized more quickly than impacts that require changes in locations. Land price impacts have also been used as welfare measures, although here the "double counting" critique is fundamental – with competitive markets welfare impacts will be reflected in changes in land prices or changes in travel demand, but counting both would "double count" the benefits of the transport project.

The most common method of studying land price impacts is hedonic analysis of housing prices. See, e.g. Rosen (1974), the review in Huang (1994), or examples such as Haurin and Brasington (1996), Kockelman and ten Siethoff (2002), Li and Brown (1980), or Zabel and Kiel (2000). House prices are typically the dependent variable, and the characteristics of the structure are controlled through the hedonic regression. The regression typically also includes a variable that measures the location-specific amenity of interest. In the case of transport projects, a variable measuring transportation access would be included among other independent variables to measure the impact of transport on house prices and, by extension, land prices.

B. Variables

The dependent variable in a hedonic regression analysis is individual house prices. Sales prices are the preferred sources of data, and sales are typically selected to reflect arms-length market transactions. Transactions between family members or sales involving non-profit organizations are sometimes excluded from hedonic price analyses, on the assumption that such sales are not arms-length, market-based transactions. Independent variables are typically of three types: (1) characteristics of the structure, (2) location-specific characteristics that would be expected to influence house prices, and (3) controls for year and, as applicable, month or quarter of sale to control for time trends and seasonality in housing markets. A typical hedonic regression is shown below.

$$P = \alpha + S\beta + A\lambda + T\delta + u \qquad (5)$$

where P = house sale price

S = a vector of structure specific characteristics (typically the size of the house in square feet, lot size, number of bedrooms, number of bathrooms, and age of the

structure, which might be broadened to include factors such as toilet type, type of access to potable water, or roofing material in developing countries)
A = a vector of location-specific amenities, such as school quality, crime rates, nearby natural or cultural amenities, and transportation access
T = a vector of time dummy variables to control for time of sale
u = error term

A IS A SCALAR AND B, Γ, Δ ARE COLUMN VECTORS OF PARAMETERS TO BE ESTIMATED

The hedonic regression can be estimated in linear form, log-log form, or semi-log form with only house prices expressed as logs. Typically measures of fit and analysis of the data and regression residuals are used to choose the specification. See Boarnet and Chalermpong (2001) for a discussion.

In addition to house sale prices, data on the time of sale and the characteristics of the structure and neighborhood are necessary. Most hedonic regressions control for the size of the home, size of the lot, number of bedrooms, number of bathrooms, age of the structure, and neighborhood characteristics that, in developed countries, most commonly include school quality and crime rates. In developed countries, the independent variables in the hedonic regression should also reflect characteristics that, while ubiquitous in the developed world, would reflect important quality variation in developing countries. For example, toilet type, access to running water, or roofing material might be included in the S vector. Based on the local context, researchers should take care to identify local characteristics that might be correlated with transportation access, and so if omitted from a hedonic regression would bias the estimate of the effect of transportation access on house prices.

The researcher might measure transportation access using a gravity measure of access to employment. The difficulty with such a measure is that it ignores the extent to which homeowners might value accessibility to things other than employment. For that reason, measuring transportation access as distance to the transportation improvement will often be preferred.

House price studies can examine the impact of new transport projects by looking at changes in house prices before and after the project was built. In such cases, the transportation access variable would measure the change in access, either by measuring travel times to particular locations (before and after project construction) or by allowing the effect of access to vary before and after project construction. An example of the latter technique, drawn from Boarnet and Chalermpong (2001), is shown below. In the below regression, Boarnet and Chalermpong (2001) measure the effect of the construction of a new toll road in Orange County, California on house prices using data on home sales from 1988 through early 2000.

$$P = \alpha_0 + \alpha_1 SQFT + \alpha_2 Bedroom + \alpha_3 Bath + \alpha_4 Lotsize + \alpha_5 Age + \alpha_6 SATscore + \alpha_7 CrimeRate + a_8 DtrBefore + \alpha_9 DtrAfter \sum_{i=1}^{12} \beta_i YEAR_i + \varepsilon \quad (6)$$

Where P = home sales price

SQFT = size of house, in square feet

Bedroom = number of bedrooms in house

Bath = number of bathrooms in house

Lotsize = size of lot, in square feet

Age = number of years since house was constructed

SATscore = average SAT scores for the school district that contains the home

CrimeRate = total violent and property crimes per 1,000 residents in the municipality where home is located

$YEAR_i$ = Dummy variable for year of sale, ranging from 1988 (index "i" = 1) to 1999 (index "i" = 12); 2000 is the omitted year

DtrBefore = Dtr*(1 – ThresholdDummy)

DtrAfter = Dtr*ThresholdDummy

Where Dtr = straight-line distance from each house to the nearest toll road on-ramp

ThresholdDummy = 0 for all home sales that occur before the threshold year; 1 for sales in the threshold year and in subsequent years. Threshold years were chosen by examining several possible threshold years.

The coefficient on DtrBefore measures the house price gradient from the toll road before toll road construction, and the coefficient on DtrAfter gives an estimate of the house price gradient after toll road construction. By comparing the two gradients, one could estimate how the housing market values the new toll road.[14]

C. Data Sources

In developed countries, house sales price data are often available from offices that record property sales for purposes of administering property tax systems. In many states in the U.S. such data are public records and can be obtained from agencies. In the U.S., commercial vendors compile house sales price data, often from administrative agencies, and sell those data to professionals in the real estate industry. Such commercial vendors are typically the best source for high quality house sale price data. The data sets usually include home sale prices, the date of sale, and basic structural characteristics needed for a hedonic regression. In the past several years, such data have been increasingly used both

[14] Boarnet and Chalermpong (2001) were interested in testing the hypothesis that the toll road influenced land prices, and so did not assess the magnitude of the differences in gradients before and after toll road construction. In concept, such a comparison of magnitudes could give information about the "housing market value" of the new road.

for real estate analyses and in hedonic studies of a broad range of amenities and disamenities.

Such data might be less commonly available in developing countries. In cases where data are collected administratively, for example to support property tax systems, researchers might have to work with cumbersome administrative records where real estate support vendors are not well established. In other cases, researchers might conduct surveys to assess house price sales. The survey strategy is not promising, though. Self-assessments of home values might be less reliable market signals than actual sales, and so surveys would want to capture recent sales. Absent administrative systems that record sales, several homeowners would have to be surveyed to find persons who recently bought or sold a home. Self-reported estimates of home values might be more useful in settings where data on actual sales are difficult to obtain or in cases where many transactions are not "arms length", but instead involve non-market sales between family members. Research would be necessary to study whether self-reported assessments correspond to market values in developing countries.

D. Methodological Approaches and Challenges

Some of the common methodological challenges in hedonic house price analysis were mentioned above. The characteristics of the structure and measures of non-transportation amenities or disamenities that might be correlated with transportation access should be included in the regression. The choice of functional form (linear, log, or semi-log) would be based on diagnostic tests and analysis of the data. In addition to these issues, researchers should take care to apply hedonic regression analysis within uniform market areas. Research suggests that housing sub-markets are smaller than metropolitan areas, and that hedonic regressions perform best when fit on uniform submarkets that are subsets of market areas (e.g. Mayer, 1993).

E. Examples of Studies of Transportation Access and House Prices

There is a large literature examining the relationship between housing prices and transportation access in the U.S. Below we describe three examples. The first study, by Baum-Snow and Kahn (2000), highlights the possibilities for using aggregate data rather than micro-data on home sales. While much of the discussion above emphasized the advantages of micro-data on individual home sales, such data might be difficult to obtain in developing countries. In that context, the Baum-Snow and Kahn (2000) study provides insight into what is possible with more aggregated data. The second example, from Bowes and Ihlanfeldt (2001), uses micro-data in a cross-sectional setting, taking advantage of differences in access to transportation infrastructure and econometric controls to identify the effect of access on house prices. The third, example, from Boarnet and Chalermpong (2001), uses data from before and after the construction of new highways to detect changes in house price gradients. Taken collectively, these examples provide insights into varying approaches that can be used.

Baum-Snow and Kahn (2000) used the construction of new rail transit lines in five U.S. cities to examine the effect of a change in rail transit access on house prices. The study cities – Atlanta, Boston, Chicago, Portland, and Washington D.C. – all opened new rail lines or new rail transit systems during the 1980's. Baum-Snow and Kahn (2000) used data from the 1980 and 1990 census of population and housing to estimate the regression for median home price, by census tract, shown below.

$$\Delta price_{i,j} = city_j + \psi CC_{i,j} + \beta \Delta X_{i,j} + \phi \Delta dist_{i,j} + \Delta u_{i,j} \quad (7)$$

where price = median home price (median rental and median home value were used)

city = city fixed effects

CC = distance to central business district

X = a vector of control characteristics that measured average income, racial composition, and education levels of census tract residents

dist = distance from the census tract to the nearest rail transit line

u = error term

Δ indicates change as the difference between 1980 and 1990 values

"i" subscript indicates census tract, "j" subscript indicates city

Baum-Snow and Kahn (2000) note that as new rail lines were built in the study cities, some census tracts had sizeable changes in the distance to the nearest rail line, while tracts in other areas of the city had little if any change in the distance to the nearest rail line. This variation allows a regression specification that mimics some of the qualities of a DID specification – the change in the distance to the nearest rail line identifies the effect of access on median home prices.

Baum-Snow and Kahn (2000) found that the change in distance to the nearest rail transit line is significantly negatively associated with home prices. Tracts that got closer to rail transit, due to nearby rail transit expansions, experienced increases in median home prices, *ceteris paribus*. A change in the distance to the nearest rail line from 3 kilometers to 1 kilometer was associated with an increase of $14 per month in median rents and an increase in median house prices of $4,972.

Bowes and Ihlanfeldt (2001) used data on single-family home sales in Atlanta for the years 1991-1994. The authors had data on 22,388 sales of single family homes in the city of Atlanta and the inner suburb of Dekalb County – a region that contained 31 of the 33 stations of the Atlanta MARTA rail transit system. Bowes and Ihlanfeldt (2001) used a cross-sectional approach; the sales from the three years were pooled in the regressions.

Bowes and Ihlanfeldt (2001) noted that access to rail transit could influence home prices in direct and direct ways. The direct effect is the value of rail transit access. Bowes and Ihlanfeldt (2001) also sought to measure two indirect impacts: (1) the link between rail stations and local crime rates, and through that a link from rail access to house values, and (2) the link between rail stations and neighborhood retail activity, and through that the link from rail access to house values. In short, Bowes and Ihlanfeldt (2001) noted that home buyers might be attracted to rail stations for the retail opportunities that are often clustered near stations, and that home buyers might also be influenced by crime rates, which could be higher or lower near rail stations depending on the particulars of the station and neighborhood, and that both of those effects could be distinct from the value of transportation access. The regression specification is shown below.

$$P = \alpha_o + \alpha_Z Z + \beta_L L + \beta_C C + \beta_R R + \gamma_S S + \varepsilon \qquad (8)$$

where P = home sale price

Z = a vector of house characteristics, including number of bedrooms, number of bathrooms, size of the lot, age of the house, and variables indicating whether the home had a basement or fireplace

L = location characteristics other than crime and retail activity

C = crime rate in the census tract that contains the house

R = retail activity in the census tract

S = distance to the nearest rail transit station

ε = error term

α, β, and γ are coefficients

In the primary regression, access to rail transit was measured by the distance from the house to the nearest rail station in quarter-mile increments up to a half-mile distance and then half-mile increments up to three miles distance from the nearest rail station. Because these were dummy variables, the reference (omitted) category is homes further than three miles from the nearest rail transit station. Bowes and Ihlanfeldt (2001) also estimated two auxiliary regressions to explain census tract crime rates and census tract retail opportunities as a function of local characteristics that included access to the nearest rail transit station. Using the coefficients from the auxiliary regressions, Bowes and Ihlanfeldt (2001) decomposed the full effect of access as shown below.

$$\frac{\partial P}{\partial S} = \gamma_S + \beta_C \frac{\partial C}{\partial S} + \beta_R \frac{\partial R}{\partial S} \qquad (9)$$

The terms on the right-hand side of the above equation show, respectively, the direct effect of rail transit access on home prices (the portion that one could infer is due to the value of transportation access), the indirect price effect flowing from rail transit's influence on crime rates, and the indirect price effect flowing from rail transit's influence on retail shopping opportunities.

Bowes and Ihlanfeldt (2001) found that homes within a quarter-mile of a rail station sold for 19 percent less than homes farther than three miles from a station, indicating that negative externalities (such as noise, congestion, crime) might dominate the advantages of rail access at close distances. Bowes and Ihlanfeldt (2001) also found that homes at distances from one to three miles from a station sold at an average 4 percent price premium compared to homes farther than three miles from a station. Decomposing the price effect into direct and indirect effects, Bowes and Ihlanfeldt

(2001) found that, on average, a home one half mile from a rail station, 12 miles from the central business district, in an upper income census tract (median income of $50,000) had a price premium of $29,923, of which $25,571 is the direct effect. Bowes and Ihlanfeldt (2001) found that the crime and retail opportunity effects on home prices were generally negative for lower income neighborhoods near the central business district and generally positive for higher income neighborhoods further from the central business district, suggesting the importance of examining how those indirect effects play out differently in different neighborhoods. In the one-half to one-mile ring, the direct effect of access was typically larger than either indirect effect, but the indirect effects had magnitudes that were sometimes larger than the direct effect at shorter (i.e. one-quarter to one-half mile) and longer (i.e. two to three mile) distances.

Boarnet and Chalermpong (2001) implemented a "before and after" test by using the construction of new toll roads in Orange County, California, as a natural experiment. Using the regression specification in equation (6) above, Boarnet and Chalermpong (2001) test the hypothesis that the new roads altered home prices. The authors used data on all home sales within three miles of the new toll roads in 1988 through early 2000 – a period that spans the opening of the two new toll roads from 1993 through 1996. The specification in (1) allows Boarnet and Chalermpong (2001) to test for a negative price gradient (i.e. a negative price premium associated with distance from the nearest toll road entry ramp) associated with a threshold year. By varying the threshold year, Boarnet and Chalermpong (2001) test for both anticipation effects (the appearance of a price gradient before the road opens) and lag effects (the appearance of a price gradient after the road opens). They find robust evidence that the toll road is associated with the appearance of a negative price gradient, and the price gradient first appears roughly when construction began for one of the two toll roads or, for the other road, when the road cleared the last legal hurdle in a highly publicized litigation process.

F. Caution: House Price Analysis in Weak or Incomplete Land Markets

Hedonic price analysis relies on well functioning markets. Land markets in some developing countries can be characterized by informal legal tenure, command and control as opposed to market-based allocation, or barriers to relocation or mobility that would be large enough to call into question the ability of land (and hence housing) prices to reflect location characteristics. This raises important cautions both in regard to impact evaluation techniques and policy responses.

Methodologically, hedonic analysis works if the property market allows buyers to bid on properties, effectively ensuring the "many buyers – many sellers" assumption of competitive markets. The departures from competitive markets noted above (informal tenure, non-market allocation of land, or high barriers to residential relocation) could all reduce the extent to which land prices reflect location premia. For an example, based on an analysis of the dynamic adjustment of the Krakow, Poland, land market in the transition from a command-and-control to a market economy, see Redfearn (2005).

The lesson is that hedonic analysis is a useful impact evaluation tool only in well functioning property markets. In cities without well functioning land markets, the analysis of transport projects should focus on analyses of the market for travel and evaluations of other impacts, such as labor market access or access to services. Because land prices are an intermediate impact, it is not necessary to measure land prices to understand the impacts of transport projects, and in general land price analysis should only be pursued where land markets are well established and operate relatively free of market failures.

Turning to policy implications, land (and hence housing) prices often adjust to compensate for location specific advantages. In some models, that compensation can be one-for-one. Thus, persons have argued that individuals who live far from the central business district are compensated for their long commutes by lower land prices. Similar arguments hold for other positive or negative location amenities. One policy interpretation is that changes to transportation systems can influence land values. In some cases, such changes might be considered windfalls. For example, land owners might have purchased land with no expectation that access would be improved, and if transportation access is later improved and the value of the land rises, that gain to the landowner would be unexpected and, rather than compensating, would reinforce the benefit or improved access. On the other hand, persons who buy near disamenities (i.e. near noisy airports) might pay less for the land, and in equilibrium the lower price compensates for the noise disamenity. Timing matters, as unanticipated changes in access or amenities constitute windfall gains or losses, while expected changes or amenities that are evident at the time of sale can influence prices in ways that compensate the land owner.

The matter of compensation, though, is more complex. As an example, consider a hypothetical case of low income renters living near a transportation improvement. If the improvement is substantial, the value of land will increase and rents for properties near the improvement will rise. Property owners might raise rents to a level that low income persons will need to move to other locations. The benefit of the improvement is incident on land owners, not renters.

Furthermore, if land tenure is not assigned (as would be the case in squatter slums), the land price adjustment will not be a mechanism for compensation. Transport improvements can raise the value of nearby land, but if the residents on that land do not have legal tenure, they will not have an ability to reap that benefit (e.g. in the form of higher sale prices.) If the residents of the informal settlements can stay on their land, they would at least reap the benefit of improved transportation access, but one of the characteristics of informal land tenure systems is that the ability to reside permanently on particular plots of land is dependent on political rather than market processes. In cities with sizeable informal settlements, analysts should be aware that land price changes will not provide compensation for location amenities. Such cases require careful attention to the distributional properties of transportation improvements and to the impact of political responses.

VIII. Impact Evaluation for Regulatory Reform Projects

Many World Bank transport projects are economic or regulatory reforms, rather than simply public sector infrastructure investment. As mentioned earlier, impact evaluations in the context of regulatory reforms can be hampered by the possibility of policy reversion; regulatory policies are, by their nature, less permanent than fixed infrastructure. Having said that, experience suggests that many regulatory reform projects are stable, and many such projects are also associated with a program of infrastructure investment. In this section, we discuss impact evaluation for economic or regulatory reforms in the urban transport sector.

Economic or regulatory reforms typically involve market adjustment and private sector participation in the pursuit of two possible goals: increases in efficiency and productivity or financing new infrastructure. Some reforms bring major infrastructure investment programs, such as Brazil's highway concession program, which included a front-loaded schedule of maintenance and infrastructure investment as part of the concession agreement. In those cases, impact evaluations can focus on the infrastructure investment, and the methods discussed earlier can be adapted directly. For example, in Brazil the highway concession program resulted in maintenance and investment in specific roadways. An evaluation of specific poverty related impacts, such as labor market outcomes or access to services, could proceed from a measure of reductions in travel times to job centers or service locations. In this case, the key independent variable, change in transportation access, would be measured based on reductions in travel times made possible by the infrastructure investment, and whether that the investment is funded by private or public funds would not matter for an impact evaluation focused only on specific outcome variables.

In other cases, the regulatory reform is fundamental to an evaluation of the project. Consider the distinction from Section I between the scale of a project and whether the project either has external impacts or impacts in other markets. Some regulatory reform projects might logically be viewed as small interventions, with impacts predominantly in the transport sector. Examples might include transit market reform that lowers the cost to consumers or traffic management programs that reduce travel times slightly along key routes. In such cases, the transport market would include all information needed to understand impacts, and measuring surplus with a valid welfare measure based on travel demand curves would be sufficient. For large projects that could have effects in many markets, impact evaluation can be used to examine impacts on those ancillary markets.

An example is regulatory reforms that include large staff retrenchments as part of a program to increase labor productivity. The freight rail concession program in Brazil in the 1990s was pursued in large part to allow reductions in excess staff in the state-owned freight rail line. When Brazil's federal railway (RFFSA) was concessioned in the mid-1990s, the goal was to reduce the staff by approximately 18,000 persons. Staff members were given incentives to separate voluntarily, but many involuntary layoffs were also necessary to meet the goal. The RFFSA concession included an ambitious program of labor market training for the separated employees, described in Estache et al. (no date). Separated employees were surveyed to gather information about their participation in the job training program and their post-program employment rates. In concept, a full DID impact evaluation could have been used to match separated and non-separated employees, and the methodological techniques described in Section III were at times developed and applied in the context of labor market training programs. In practice, the separated employees likely were systematically different from the employees retained by the private railway concessionaires, complicating any type of "experimental/control" impact evaluation, although researchers might attempt to identify persons close to the "separation" margin, finding those who were the last to be laid off and choosing a control group from persons who might have lost their jobs had more separations been required.

The example of the labor retrenchment and retraining program associated with Brazil's freight rail concession highlights that transport reforms sometimes have large impacts that are not related to the market for transportation. In such cases, evaluations that examine those non-transport impacts are vital to understanding the full scope of the project.

More broadly, impact evaluation of economic or regulatory reform should be guided by the following points: (1) The context of the reform will determine the impact to be evaluated. (2) For projects with substantial infrastructure investment, the investment can potentially be evaluated separately from the economic or regulatory reform, unless elements of the regulatory environment are vital for understanding the impact of the investment. (3) Even if a project is a bundled package of reform and investment, the investment component, once completed, is fixed, although evaluations should take care to document changes in the regulatory environment that would influence the outcome being studied by the evaluation. (4) Impact evaluation is especially well suited to examine the role of projects that intentionally bring large impacts outside of the transportation market, such as labor retrenchment programs associated with privatization of public transport utilities.

Conclusions

Impact evaluations can illuminate the role of urban transport in poverty alleviation. This can be especially useful in understanding impacts that are outside of the transport market, as would be appropriate when projects have large impacts in ancillary markets. Examples include measuring how transport improvements help link persons to jobs, and hence help reduce poverty. Other transport interventions bring ancillary goals that work not through the transport reform *per se*, but are a more direct result of the project. As an example, consider staff retrenchment programs associated with privatizing overstaffed state-owned transport utilities or resettlement programs needed to acquire land to build transport projects. In those cases, the retrenchment or resettlement are a side effect of the program, and evaluation studies can give information about the impact of the retrenchment or resettlement program on the affected populations. A similar point can be made in relation to environmental externalities associated with transport projects.

Impact evaluation should be viewed as an adjunct to traditional cost-benefit analysis, not a replacement for welfare studies. Transportation theory cautions against double counting benefits, and that caution is especially important in the context of impact evaluation. For projects with no externalities, the welfare effect can be measured in the market for travel. Impact evaluations, while certainly not a replacement for welfare analysis, are useful for several reasons. For some projects, impact evaluation will measure the same benefit that could be measured in the market for travel, but an impact evaluation gives a more direct link to poverty alleviation. Studies of labor market success from improved access would fall into this category if the labor market in the city in question is competitive and free of market failures. In such cases, the impact evaluation does not measure additional benefits beyond traditional transport cost-benefit analysis, but the link to poverty alleviation can be clearer. With externalities, evaluation studies can illuminate impacts that are outside of the transport market and would not be captured by traditional cost-benefit studies. Applying impact analysis to measure the role of staff retrenchment or resettlement programs on the affected individuals would be an example.

The key methodological issue in impact evaluation for urban transport is endogeneity. For individual actors, travel time, location choice (and hence access), and bid prices for land are all endogenous. Thus key independent variables that one might naively think measure the impact of the transport project, including travel times and measures of access, are endogenous, and persons or firms can select their level of access – if not in the short-run, then in the long-run. This paper suggested methods to cope with such endogeneity. A careful DID research design, using matching techniques to select a control group, can establish the counter-factual of what would have happened absent the transport project. Such techniques can be applied in many urban transport projects, but two complications need attention. First, researchers should be aware of the possibility that persons or firms might select into evaluation groups based on characteristics that are unobserved to the researcher. Equivalently stated, are the control and experimental groups in a DID evaluation really identical on all characteristics other than the impact of the transport project? This requires some care, and issues of particular concern will depend on the context of the project. Second, the average effect measured in a DID study might not be the marginal impact – an issue of particular importance if a researcher wants to forecast the impact of extending the transport intervention to new locations and hence new groups of persons or firms.

As part of impact evaluation techniques – either in conjunction with DID studies or in place of such studies – researchers should seed survey instruments with questions that illuminate changes in the travel time and location choices of behavior actors. In concept, such survey data could allow full modeling of the endogenous choice variables, and hence offer an alternative to DID approaches.

Of the four outcome variables discussed in this paper – labor market success, firm location, consumption of services, and land prices – the link from transportation access to labor market success is likely the most fundamental for understanding poverty impacts.

There is a large U.S. literature examining the link from transportation access to labor market success, but little corresponding research exists in the developing world. This knowledge gap should be addressed. One priority area for future research should be evaluations that shed light on the role of urban transport in the labor market outcomes of the poor in developing cities. The literature from the U.S. gives reason to believe such a link exists, and that such a link can be an important channel through which transportation access can alleviate poverty. Access to services should also be a priority area for future research, as service consumption, including health care and education, might be linked to patterns of transportation access, and such linkages might be more important in developing cities than in the developed world, where transportation access is more ubiquitous.

At one level, evaluating the impact of urban transport projects on poverty is straightforward. Sound research design, with careful attention to determining the counter-factual, should guide the researcher. Yet given the interaction of urban transport projects and the markets for land and location, special care is necessary. In particular, residential location is endogenous, and failing to account for that can bias evaluations of the impact of urban transport. More broadly, the nature of the evaluation should be tailored to the impact being examined. Also, impact evaluation is not a substitute for cost-benefit analysis, and analysts should be careful not to double count benefits that are reflected in multiple markets. Having said all that, impact evaluation is a powerful tool to understand the impact of urban transport on poverty alleviation. Given that many transport projects are pursued primarily for mobility reasons, understanding the impact on poverty alleviation is especially helpful in illuminating the role of urban transport in the World Bank's mission.

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