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EFFECTS OF MASS TRANSIT SUBSIDIES BY SOURCE AND USE

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Federal, state, and local governments have subsidized mass transit since the 1960s. A body of work stretching back 30 years has established that public money decreases incentives for cost control and reduces technical efficiency. Only recently, however, have researchers examined the effect of these subsidies on transit output, and few authors since 1976 have studied absolute levels of ridership. Papers in the field have typically aggregated funding by source or use. Using a panel-corrected standard errors model, I find evidence that transit budgets should be disaggregated into six source-use pairs to accurately measure the effects of subsidies.

JEL Classification: H40, R42, H540

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I. Foundations of Transit Subsidies

Since 1964, the federal government has subsidized the recapitalization (Smerk, 1965) and, eventually, operations of mass transit agencies. Following World War II, rising incomes and the national highway system (Sauter, 1967) facilitated the rapid growth of suburbs. Mass transit firms faced large operating deficits, and most were purchased by city governments using federal money following the Urban Mass Transportation Act of 1964. The entire legislative history of transit policy could be briefly summed up as a slow shift towards more discretionary power at the local level and thus, more impetus for operations as opposed to capital funding (Dilger, 1998). Further, the pairing of highway and mass transit funding during the Reagan administration created a powerful legislative coalition that virtually guarantees the survival of the transit program (Smerk, 1983).

There are myriad justifications for this subsidy. Mohring (1972) theorizes that urban bus transportation experiences increasing returns to scale. Public economic theory maintains that such a good requires subsidy for its price to equal the marginal cost. Jackson (1975) presents another familiar argument, that traffic congestion is caused by inefficient (indeed, generally non-existent) pricing of freeway use and necessitates the subsidization of an alternative to correct the externality. These two core arguments, increasing returns to scale and externalities, sit at the heart of a sprawling field of potential benefits. Beyond the basic economic arguments, the justifications include reduced noise and visual intrusion, mobility for the poor and elderly (Bly and Oldfield, 1986), reduced pollution (Parry et al, 2007), stronger central business districts, and reduced urban sprawl (Cervero, 1984). There are other distortions, like free parking (Segelhorst and Kirkus, 1973) that further lower the price of freeway travel below the marginal cost.

While these theoretical benefits of mass transit are numerous, the actualization of these benefits depends on the effectiveness of the subsidy program at increasing ridership. There is a long literature investigating the effects of public subsidy on the technical efficiency and output of transit agencies. Many papers examine the effects of subsidies from different levels of government; a few focus on the effects of earmarking funding for operations or recapitalization. This study mixes the two approaches to measure the combined effect of source and use of a subsidy on transit ridership.

II. Transit Subsidy Literature

Economists and transportation researchers have thoroughly analyzed the impact of subsidies at all levels of government on mass transit provision. Parshigian (1976) performed the first quantitative analysis of the subsidy program. Parshigian is chiefly interested in assessing three theories explaining the socialization of transit service in the 1960s. He treats an agency's profit margin as a measure of the extent of public ownership. Firms with larger profit margins indicate they were able to cut unprofitable routes and reduce service in line with the general trend towards suburbanization and high car ownership. Low or negative margins indicate that the firm is providing unprofitable service ostensibly in the public interest. The "regulation theory" posits that firms in cities with large populations of transit users will go public faster and be more strictly regulated (i.e. provide less profitable service). The competing theories, which he calls the "externality" and "declining industry" theories, essentially claim the opposite. These predict that firms go public in order to achieve economies of scale in the face of declining ridership.

Parshigian regresses each agency's profit margin on dummy variables describing their regulatory status. He finds that agencies regulated by jurisdictions with fewer transit users, like state utility commissions, are more profitable than those regulated at the city level. This result is consistent with the regulation theory. It is important not for the specific conclusion that state-level regulators are less favorable to transit riders, but for the general idea that transit operators respond to political incentives based on the makeup of their constituency. Parshigian also regresses transit ridership on these dummy variables along with controls for heterogeneity in demand factors, like income and population. Using national data for 1960-1970, he finds no significant difference in ridership between public and private systems. This is consistent with Savage (2004), who characterizes the early days of the subsidy programs as the worst in terms of efficiency, as the ridership incentives were not introduced until the Surface Transportation Act of 1982 (Smerk, 1983).

Parshigian asks a question that the transit literature will not answer adequately for 35 years: “By how much do transit subsidies increase ridership?” Answering this question accurately is essential to evaluating the usefulness of transit subsidy programs. All of the work following Parshigian, however, focuses on measuring the impact of subsidies on the technical efficiency of transit firms. Researchers in the field typically find that subsidies introduce some amount of inefficiency. But without examining the effect of the subsidies on ridership, these researchers cannot adequately answer whether the subsidy program achieves its goal of reducing car travel and its externalities, as Obeng (2011) points out.

Here the transit literature has little to say. But the focus on technical efficiency is useful in providing a theoretical and methodological grounding for an empirical model. Pickrell (1985) sets out to explain the dramatic growth in the size of the transit subsidy program in the 1970s. He regresses the total subsidy amount a transit firm receives on a variety of supply factors including wages, fuel costs, average age and speed of the fleet, peaking ratio (a measure of how concentrated travel is during rush hour), and amount of dedicated local funding (Pickrell, 1985).

More peaked travel patterns mean that fleets and driver pools have to be larger to service rush hour demand. The marginal cost of a rush hour rider is high because of this effect, but popular flat fare structures prevented transit firms from adjusting. Pickrell finds that the primary driver of rapidly increased subsidies 1970-1982 was driver wage. Pickrell declines to make conclusions regarding the direction of causality. He states only that subsidies are being used for higher wages. It is a more difficult to say how much of those increases are endogenous to the firm's funding sources and management.

Bly and Oldfield (1986) offer three explanations for the association between rising subsidies and costs: exogenous changes in input prices are financed by subsidies; subsidies encourage the provision of more costly services; or the receipt of subsidies weakens incentives for cost control. In order to isolate this relationship, the authors lag subsidy variables using a dataset for 117 systems in 11 European and North American countries from 1965-1982. Their coefficients are largely insignificant.

Pucher and Markstedt (1983) make a strong and more nuanced theoretical argument for causality running from subsidy to wages. They develop a case study of four transit systems with varying ownership and urban characteristics, using them as a theoretical basis for a regression analysis (Pucher et al, 1983). The authors note that from 1970-1980, driver wages (the largest component of a transit agency's costs) rose faster in public agencies like the Chicago Transit Authority than in private ones like Portland's Tri-Met or New Orleans Public Service (Pucher and Markstedt, 1983).

The authors offer several explanations for variation in costs. They posit that, generally, state and federal funding separates the sources of funding from local actors, reducing incentives for cost control through tougher wage negotiations and reductions in unprofitable service.

Additionally, federal funding must be spent in compliance with federal regulation regarding accessibility, sustainability, and domestic sourcing (Pucher and Markstedt, 1983). Smerk (1983) describes the Carter administration's Rehabilitation Act of 1974 as requiring "economically heroic" efforts from local agencies to make vehicles handicap accessible. The elimination of these rules was part of the Reagan adminsitration's strategy for enticing transit agencies to accept funding cuts. Ultimately, accessibility requirements would remain in place and new rules on domestic sourcing were added, further increasing costs (Smerk, 1983). These arguments would suggest that federal and state subsidies increases unit costs more than local funding.

Not all local funding is created equal, however. Cross-subsidies from utility revenues and revenue-inelastic, highly visible taxes increase the financial and political incentives for cost control while dedicated funding sources and less visible taxes decrease these incentives (Pucher and Markstedt, 1983). Finally, regional taxes incentivize transit firms to provide unprofitable service to outlying suburbs (Pucher and Markstedt, 1983). This has interesting implications when contrasted with Parshigian's regulation theory, where transit funded by geographically dispersed jurisdictions tends to be more profitable as rural and suburban non-users benefit less from lower fares. In this case, dispersed taxation creates a constituency in outlying suburbs that demands costly, inefficient service.

Using pooled cross sectional data for 1979 and 1980, Pucher et al (1983) regress hourly bus operating costs on the amount of federal and state subsidies, along with a dummy variable for dedicated local funding and controls for wages, productivity, and fleet characteristics. Because subsidies act on unit costs through higher wages and reduced productivity, the authors instrument for these labor characteristics using subsidy levels and fleet characteristics. The estimates of wages and productivity are included in a new model in order to filter out multicollinearity. That is, the coefficients of the estimated variables reflect their effect independent of the variance caused by differing subsidy levels. The net effect of subsidies on operating costs is larger after this treatment, but their relative effects are the same. They find that dedicated local subsidies increase costs by $2.30 for every dollar in spending, making them roughly four times more inflationary than federal subsidies and eight times as inflationary as state subsidies. This evidence supports the notion that as the "fiscal distance" (Shughart and Kimyeni, 1991) between operator and benefactor increases, the less efficiently funds are spent.

Shughart and Kimyeni also find evidence for this theory of fiscal distance. They estimate a regression model for 118 systems from 1984-1986 using data from the Urban Mass Transportation Association's (now Federal Transit Administration) Section 15 reporting program. The authors find that each dollar of local assistance increases costs by 66 cents. A dollar of state funding increases costs by $1.38. The model controls for a variety of demographic and system characteristics. Novel variables include dummies for the use of private contractors and the operation of multiple modes of transportation.

Cervero (1984) creates a dataset for 17 California transit agencies from 1971-1981 and regresses various measures of operating efficiency on the proportion of funding by source. He finds that local operating assistance has a negative effect on efficiency, counterbalanced by efficiency gains from federal funding, contrary to the fiscal distance theory. Cervero suggests that limiting his study to Caltrans properties controls for heterogeneity. However, as Pucher et al shows, factors like the age and size of the vehicle fleet (which could easily vary from city to city) significantly impact efficiency. Essentially, Cervero's conclusion rests on the extent to which these 17 cities in a very large state are homogeneous.

Karlaftis and McCarthy (1998) regress performance measures on various demographic and system characteristics and the proportion of subsidy from each level of government using a fixed-effects model. The authors use data for 38 transit agencies in Indiana from 1983-1994. They find that by interacting relative subsidy with a dummy variable for the size of the system (small, medium, or large) that subsidies have different effect on different sized systems. Specifically, subsidies appear to improve performance in small and medium systems while having no effect on large system performance. The authors note that a fixed-effects model is necessary to control for unobserved cross-panel heterogeneity. They also correct for an AR-1 process in their panel data and caution that many other studies have not performed similar tests or corrections.

To address the possibility of endogeneity bias due to reverse causality, the authors perform Granger causality tests between the performance indicators and the level of subsidy for each class of system. In the large systems, subsidies were found to Granger cause performance. In the small systems, subsidies and performance were found to Granger cause each other, evidence of an endogeneity problem. That is, at least in small systems, subsidies are both a response to and a cause of changes in performance.

Savage (2004) models the Chicago Transit Authorty from 1948-1997. He separates exogenous and endogenous changes in input prices by creating an index for national labor and fuel prices for the period of the study and comparing that to the wages and fuel prices experienced by the CTA. Savage attributes most of the decline of transit to exogenous factors like population dispersion, but does show subsidies increased unit costs. Subsidies had the most deleterious effects in the 1970s before the program was reformed. Savage shows the negative effects of subsidies on efficiency declining in the 1980s, after the performance-based incentive tier was added to the federal subsidy program in the 1982 (Smerk, 1983).

While the literature does not provide a conclusive comparison of the effects of spending at each level of government, there is a great deal of evidence to support the inclusion of the source of subsidy in any measurement of a transit agency's performance. There is also evidence that the use of subsidy between operations and recapitalization is important as well. Most studies have examined either the aggregate impact of spending or the impact of operating assistance alone. Cromwell (1991) focuses on capital subsidies. He argues that if the purpose of maintenance is to defer recapitalization, the presence of federal capital subsidies (which cover 80% of costs) can significantly alter a firm's level of maintenance spending. By regressing maintenance spending on a variety of urban and fleet characteristics like the type and mean age of the bus fleet, he is able to determine the effect of private ownership. Cromwell finds that private agencies spend 17% more on maintenance than private agencies, and attributes this difference to the capital subsidies these public agencies receive. Cromwell is unable to estimate a significant coefficient for the actual level of subsidy, however. Because most states cover none, 10%, or 20% of the remaining cost of a vehicle, there is not enough variation to get a significant result.

Obeng (2011) is the most recent addition to the transit subsidy literature. He argues that while investigations of technical efficiency are useful for informing policy, they can only provide a partial answer to the question of whether the subsidy program as a whole is effective. The policy can be successful given its inefficiencies if subsidies generate an increase an output that outweighs the losses higher input prices. He regresses several output measures like passenger and vehicle miles on several ostensibly exogenous supply factors like the price of fuel, size and age of the fleet, and service area, along with dummies for receipt of subsidy from different levels of government and the absolute amount of operating and capital funding the system uses. He finds small increases in output due to subsidies.

III. Related Literature

The transit subsidy literature is set within a larger body of work concerning various policies and best practices for the provision of mass transit, some of which informs studies on subsidy impacts. Giuliano (1980) is one of the earliest to recognize the need to control for cross-system heterogeneity. She lays out a detailed explanation of the classes of variables that impact the performance of mass transit. She divides these variables broadly into institutional (those under the operator's control) and environmental (those exogenous to the operator's decisions) factors. Environmental factors are relatively straightforward: demand is determined by the overall need for transportation and the cost of alternatives, chiefly car travel. The provision of transit service is also affected by the nature of the city's demographics and traffic congestion.

Guiliano uses a fixed-effects model to regress several measures of efficiency and output on these factors. She uses revenue vehicle hours (RVH) per employee, RVM per maximum vehicle hours, and operating expense (OE) per RVM as her dependent variables. Using data from California, she finds that OE per RVM is increased by the presence of a union, the age of the firm, the size of the fleet, and the "peakness" (extent to which service is clustered at rush hour) of the system. Guiliano repeats this exercise for the other measures, but this regression is a useful example. She demonstrates that factors under a firm's control like the distribution of service and the size of the fleet impact costs, alongside exogenous factors like the bargaining power of workers. In other regressions, she finds smaller service areas make agencies more efficiently employ labor.

Wunsch (1996) is notable for recognizing the presence of heteroskedasticity in datasets that include multiple modes of transportation. Wunsch assembled several databases on European transit agencies. The study analyzed 178 firms in 12 countries and was intended to find what the author called economies of density, capacity, and speed. More relevant to this paper, Wunsch also finds that systems financed through fares tend to control costs more effectively.

IV. Methodology

I assemble panel data from two sources: the Federal Transit Administration's National Transit Database and Texas A&M's Urban Mobility Report. As part of its section 15 reporting program, the FTA requires all transit agencies to submit data on system financing and performance. The database tracks 885 agencies from 1991-2011. Due to inconsistencies in reporting and waivers for small systems, data for all 21 years is only available for 48 systems. Texas A&M's Transportation Institute (TTI) maintains data on traffic congestion as part of their Urban Mobility project. The data is collected through electronic monitoring of roadways and includes 498 cities in the United States from 1982-2011.

Variable	Description	Source
Federal Capital State Capital Local Capital	Funds used for construction of facilities and purchase of new or replacement vehicles, disaggregated by source	NTD
Federal Operations State Operations Local Operations	Funds used for purchase of non-capital inputs (primarily fuel and labor), disaggregated by source	NTD
Traffic Delay	Average number of hours an auto commuter is delayed by traffic annually	TTI
Population	Number of people living in a city (rounded to the nearest hundred thousand)	TTI
Gasoline	Average annual statewide price of gasoline	TTI
Value of Time	Estimated value of time based on a combination of vehicle choice, driving speed, and lifetime earnings	TTI

Table 1: Variable List

I regress passenger miles travelled on the agency's budget decomposed into its component sources and several other environmental variables that impact either supply or demand. Theoretically, this assumes each operator uses its budget to hire inputs which translates into a level of service, given certain environmental characteristics. Passenger miles travelled is computed as the number of unlinked passenger trips (transit data is blind to whether a passenger uses multiple routes to reach a single destination) times the average trip length in miles. An implication of this specification is that the institutional factors affecting transit output (as defined by Giuliano) are totally determined by the basket of subsidies the operator receives. This is by design; federal legislative formulas distribute funds based on mode as well as population and density (Schmidt, 2001), and Cromwell (1991) shows that vehicle life decreases under public ownership. Because operator decisions are impacted by these incentives, I do not control for them so that the full effect of subsidies can be observed, contra Obeng (2011).

A second important implication is that there is an interaction between the source and use of a subsidy. Nearly every author in the field aggregates funding by either source or use. Only Obeng (2011) includes both source and use, but treats them as separate variables. There is a substantial theoretical argument that the political incentives facing the various levels of government makes them more effective at some types of projects than others.

Obeng (2011) suggests that part of the reason the transit subsidy literature focuses on efficiency is that there is no consensus on the appropriate measure of output. PMT does not measure output in purely economic terms. Consider two agencies, one with higher PMT and the other with higher RVM. The second agency is strictly more productive because it generates more bus-miles from a fixed amount of inputs. It is easy to imagine a situation where a firm is technically efficient but totally useless, squeezing extra productivity out of its factors by never making any stops. This exaggeration illustrates why PMT is the appropriate measure of transit output when assessing the effectiveness of subsidies. If the goal of mass transit is to alleviate externalities caused by car travel, it fulfills that goal by enticing drivers to substitute bus and train transport for cars. PMT is a measure, then, of the miles of car travel not driven due to transit ridership, and thus a measure of the system's success. As a robustness check, I specify an alternative model with passenger trips as a dependent variable.

Most models of transit choice treat the demand for transportation as exogenously determined: people generally have little autonomy over whether they go to work or buy food. Commuters of transportation, then, are cost minimizers, finding the cheapest way to satisfy their implacable demand for travel. Longer or more expensive modes are less desirable than faster or cheaper ones. For this reason I include several proxies for the costs of individual and mass transportation. Higher gas prices and more traffic makes driving more costly to the auto commuter, increasing demand for alternatives. On the other hand, higher valuations of time will drive commuters to abandon mass transit and drive, even though it costs more.

Population density would have been included in the model as it is repeatedly found to be a significant driver of transit ridership. Denser cities have larger concentrations of destinations, making cost-efficient route planning easier. Contrary to my expectations, density had a negative sign in early regressions and maintains it even after corrections are applied. This result can be attributed to a mismatch between my datasets. I computed density using the NTD's measurements of the land area in square miles of the "UZA" or urbanized area and TTI's population estimates. UZAs in the NTD sometimes contain multiple cities, and so do not necessarily correspond to the urban environment that transit operators have to plan for. More seriously, densities could not be computed correctly because of this mismatch. It is not apparent whether UZAs always correspond to metropolitan statistical areas as defined by the Bureau of Economic Analysis. For these reasons I do not use density in my regression.

A fixed or random-effects regression is likely necessary to adequately control for cross-sectional heterogeneity. Despite controlling for some factors affecting demand for public and private transportation, there may be time trends or unobserved heterogeneity between cities that must be addressed. To verify this assumption, I use a Breusch-Pagan test and reject the null hypothesis that there are no unobserved panel effects (1A).

Fixed-effects is a relatively clear choice from a theoretical perspective as the heterogeneity between panels should be time-invariant. The built environment of a city: its street grid, parks, and neighborhoods, are not totally unshakeable. But the makeup of cities, and thus the desirability of mass transit or car travel, is based on large amounts of fixed capital like roads and apartment complexes. For this reason I expect fixed panel effects to be the appropriate method. Typically, a Hausman test can determine whether fixed or random effects is appropriate, but it is not robust to data anomalies, which prevent it from working correctly for my model. I run an overidentification test in order to verify that fixed-effects is appropriate (1A). I reject the null hypothesis and demonstrate that fixed effects is the correct model to use. I also test to verify that year dummies are necessary to control for unobserved heterogeneity and find that they are (1A). These tests show that a two-way fixed-effects model is both necessary to control for unobserved heterogeneity and superior to random-effects in this respect.

The next step is to test the model for anomalies. Panel data between 20 and 30 years typically exhibits autocorrelation. Additionally, there are strong theoretical grounds to suspect spatial correlation between panels. Transportation is linked to economic activity so nationwide events like recessions will affect ridership in all panels simultaneously. I test for autocorrelation, spatial correlation, and heteroskedasticity and find all three (1B), which biases my standard errors. This complicates my procedure somewhat as standard fixed-effects regressions in Stata do not have options for spatial correlation. To compensate for these anomalies, I estimate a panel-corrected standard errors regression with the constant suppressed and dummy variables added for each transit agency and year to simulate a fixed-effects regression.

The results of this model may be biased by endogeneity. Fixed effects controls for unobserved heterogeneity, but there may be reverse causality running from the performance of the system to the level of subsidy. Consider a transit agency that suffers some exogenous negative shock to ridership. Farebox revenues will decrease, and the agency may petition state and federal officials for additional subsidies to fund the deficit. This scenario would make subsidies appear to cause low ridership, when in reality subsidies are sent to places specifically because they have low ridership.

Papers in the field do not typically address reverse causality as such, but some mention that larger systems receive more of their funding from state and local sources while smaller systems get more from the federal government. If smaller systems in less densely populated areas chronically underperform, this could bias the coefficients of federal spending down. However, complete data is not available for very small systems as they benefit from a reporting waiver. It is these very small systems that, as Karlaftis and McCarthy (1998) and Shughart and Kimyeni (1991) suggest, chronically under-perform and require large subsidies. So the availability of data also inadvertently mitigates the effect of endogeneity bias.

Instrumenting for the amount of subsidy is difficult for several reasons. The first is that unlike other studies, I disaggregate subsidies by their use and source. While this allows me to unmask some subtleties in the effectiveness of various types of funding, it also means that there are potentially six endogenous variables. Second, the variable most theoretically susceptible to reverse causality, federal operations spending, is determined by a legislative formula that exclusively uses variables which determine transit ridership, including ridership itself.

To address potential endogeneity, I estimate a generalized method of moments with systemic panel data estimation. GMM uses lagged first difference and absolute variables to instrument endogenous covariates. Its robustness makes it ideal for dealing with multiple potentially endogenous variables. Tests for instrument validity and other model assumptions are included in the appendix (1C).

V. Results

Variable	Passenger-Miles	Passengers
Federal Capital	0.074 (0.086)	0.002 (0.018)
State Capital	-0.419 (0.143)***	-0.066 (0.027)**
Local Capital	-0.001 (0.105)	0.002 (0.017)
Federal Operations	0.249 (0.205)	0.005 (0.045)
State Operations	0.641 (0.124)***	0.087 (0.029)***
Local Operations	0.592 (0.136)***	0.073 (0.023)***
Traffic Delay	1003467 (426392)**	1065.607 (72853.09)
Population	1.654 (0.291)***	0.151 (0.046)***
Value of Time	-10100000 (2614287)***	375743.3 (374013.1)
Gasoline	31500000 (17000000)*	1238479 (2589887)
	R^2 = 0.973	R^2 = 0.987

Table 2: Preis-Winsten Regression with Panel Corrected Standard Errors (48 panels, 21 observations/panel)

These results are encouraging as coefficients of the all the controls in the PMT model have the reasonable, hypothesized signs and are significant at various alphas. More traffic, more people, and more expensive fuel increases transit ridership while richer residents are willing to spend more to travel faster. In the model using UPT as the dependent variable only one control is significant. The coefficients on the budget components compare relatively well between the models.

The theory underlying the choice to separate funding into six source-use pairs is that some levels of government may be more or less effective when charged with different sorts of projects. Aggregating these differences, as most other studies in the transit literature do, may mask interesting effects. For instance, if state governments are very good at providing operating funding but very bad at funding capital projects, aggregating funding by source would result in the effects cancelling each other out.

Operations spending pretty closely follows the fiscal distance theory demonstrated by Pucher et al and Kimyeni and Shughart. This is reasonable as operations budgets are typically dominated by labor expenses (Pickrell, 1985), so it is here that we should expect incentives for cost control and public monitoring of expenditures to similarly dominate the effectiveness of the spending. While fiscal distance theory predicts that local spending should be more effective, these studies were also able to control for the particular tax arrangements that fund transit. I expect that if similar controls were introduced into this model, transparent, inelastic tax arrangements lead to higher performing transit systems than less visible ones. Additionally, state governments represent a more suburban, auto-dependent population than city governments. So these results are also consistent with Parshigian, who predicts that state regulators will be the most effective at controlling labor costs and maintaining system solvency. Federal operations spending is insignificant, again consistent with the fiscal distance theory.

The results for capital spending do not follow the fiscal distance theory as closely. Federal capital expenditure has an insignificant effect on transit output. As per Smerk (1983) and Pucher and Markstedt (1983), stipulations attached to federal construction funds require domestic purchases, payment of prevailing union wages, and compliance with safety, accessibility, and sustainability regulations. These increased costs may offset the benefits of new construction or rolling stock and perhaps explain the insignificant effect.

This second set of results is also interesting as state capital spending has a negative effect on transit ridership. This can be explained by considering the constituency of a state government compared to a city government. Parshigian (1976) theorizes that transit agencies run by state governments will be more profitable because those governments represent a broader population that includes rural and suburban voters who do not use mass transit and do not want to pay for it. Pucher and Markstedt (1983) argue along similar lines, but introduce the idea of regional tax arrangements that tend to correlate with little-used, inefficient suburban service. Some combination of these effects probably explains the negative coefficient on state capital spending. State governments, representing both urban transit-users and rural non-users may select projects or include stipulations that extend service far beyond city limits, stretching the firm's resources and generating little additional ridership.

The GMM results (1C) are relatively similar to the ones obtained in the fixed-effects model. All capital spending is no longer significant, but the signs are the same save for local capital spending. State and local operation spending are significant at the 0.01 and 0.10 levels, respectively. State operations spending remains more effective than local spending, and both have more of an impact than insignificant federal operations spending. The controls take their expected signs, although traffic delay is no longer significant.

From a methodological perspective, these results are important as studies in the field typically aggregate budget information by either the source of funding (Pucher et al, Parshigian, Shughart and Kimyeni, Karlaftis & McCarthy) or the use of funding (Obeng and Sakano). The most comprehensive approach is found in Obeng (2011), who includes separate variables for source and use. This assumes, however, that source and use have independent effects on output. That is, the additional dollar of, for instance, state funding has the same impact on ridership regardless of its use. There are strong theoretical grounds to believe this is not the case, as state governments have distinct motivations like cost control that make them more effective at some types of spending than others. This project now offers some empirical evidence against this assumption as well.

VI. Conclusion

Any discussion of the results should be tempered with the acknowledgement of potential endogeneity bias, which has gone, for the most part, unaddressed in this field so far. It is a shortcoming of the approach developed in this paper that in order to achieve the level of detail the model does, it leaves little room for addressing endogeneity bias. Assuming the results are trustworthy, they suggest that delegating more operational funding to budget-constrained local sources could increase ridership, while state governments effectively control operating costs but demand route extensions that tax system resources. The federal government does neither of these things particularly well relative to its political subunits. These findings are consistent with much of the work done to date. This has contemporary policy implications as transportation funding legislation frequently features battles between states and cities over control of block grants. But ultimately, these results show that a carefully managed transit subsidy program can, under some circumstances, generate additional ridership.

These results are particularly interesting as subsidies have always been aggregated by source or use. The opposite coefficients on state capital and operations funding indicate that this is not appropriate. Disaggregating an agency's budget into source-use pairs, then, may be necessary to observe additional gradients of performance.

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Appendix 1A: Tests for Model Seletion

Breusch and Pagan Lagrangian multiplier test for random effects

```
pmt[systemid,t] = Xb + u[systemid] + e[systemid,t]

Estimated results:
                 Var     sd = sqrt(Var)
        ---------------------------------
         pmt |  2.36e+17      4.86e+08
           e |  4.01e+15      6.34e+07
           u |  4.64e+15      6.81e+07

Test:   Var(u) = 0
chi2(1) =  2053.05
Prob > chi2 =     0.0000
```

Test of overidentifying restrictions: fixed vs random effects
Cross-section time-series model: xtreg re robust cluster(systemid)
Sargan-Hansen statistic 239.044 Chi-sq(10) P-value = 0.0000

Time Effects:
chi2(20) = 166.58
Prob > chi2 = 0.0000

Appendix 1B: Tests for Anomalies

Spatial Correlation:
Pesaran's test of cross sectional independence = 7.709, Pr = 0.0000
Average absolute value of the off-diagonal elements = 0.385

Heteroskedasticity:
Modified Wald test for groupwise heteroskedasticity in fixed effect regression model
H0: sigma(i)^2 = sigma^2 for all i
chi2 (48) = 2.7e+06
Prob>chi2 = 0.0000

Autocorrelation:
Wooldridge test for autocorrelation in panel data
H0: no first-order autocorrelation

```
   F(  1,      47) =    427.777
           Prob > F =      0.0000
```

Appendix 1C: GMM Results and Instrument Validity

Variable	GMM
Federal Capital	0.066 (0.09)
State Capital	-0.064 (0.08)
Local Capital	-0.007 (0.021)
Federal Operations	0.019 (0.104)
State Operations	0.168 (0.056)***
Local Operations	0.111 (0.059)*
Traffic Delay	348475.9 (322413.1)
Population	0.132 (0.051)**
Value of Time	-17700000 (10500000)*
Gasoline	46200000 (25300000)*

Table 3: GMM System Dynamic Panel-Data Estimation

Ar-2 processes will invalidate the instruments used by GMM as they are lagged over multiple periods. The test below shows no second order autocorrelation.

Arellano-Bond test for zero autocorrelation in first-differenced errors

Order | z Prob > z

1 |-1.3567 0.1749
2 | -.4397 0.6602

H0: no autocorrelation

Test for instrument validity:
Sargan test of overidentifying restrictions
H0: overidentifying restrictions are valid

chi2(884) = 13.74987
Prob > chi2 = 1.0000