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Impacts of Highway Bypasses on Kansas Towns

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Prepared for
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Executive Summary

Purpose

This report was prepared by the Institute for Public Policy and Business Research for the Kansas Department of Transportation. It provides literature reviews, empirical findings, and policy analysis related to some of the effects of building a highway bypass around a small town in Kansas.

Modeling and Data Gathering

This report describes three types of models:

1. An origin-destination model of Kansas was developed showing the number of trips that take place between each town and city in Kansas. This model was used to estimate the amount of local traffic and through traffic in every town and city in Kansas.

2. A model was developed and data were gathered to estimate the value of the time-saving generated for through traffic by bypasses in Kansas.

3. A variety of economic impact models were developed and estimated using regression analysis of economic data from several sources. These models were used to quantify the effects of bypasses on business activity in bypassed towns.

The basic findings from these models are straightforward and consistent with previous research in other states.

Reliability of Results

The findings in this report represent an accurate description of the bare facts, of what has happened in certain Kansas towns that were bypassed. But they also attempt to provide something more: an interpretation of causal relationships, intended as a guide to what is likely to happen in the future. In that broader role, these findings must be viewed with some care. First, for some of the research questions the sample of bypasses was rather small. That leads to possible statistical sampling errors. These potential errors are addressed more carefully in the body of the report than in this summary. Second, empirical measurements of causality are very sensitive to modeling assumptions. In the body of the report, some effort is made to control this problem by using sensitivity analysis, yet some uncertainty will always remain. For these reasons, the following findings should be viewed as probabilistic, even though for convenience they are stated as if they were simple facts.

Findings

Long-term effects on counties and towns

In the long term, bypasses in Kansas typically have not had significant negative effects on the local economy. In fact, many counties and towns have enjoyed some long-term benefits from the construction of bypasses. The major part of this benefit consisted of an encouragement of basic industries due to the improved transportation system. Growth in basic industry then had second-round effects on local retailing and services.

Short-term average effects on towns and firms

In the first two or three years during and after construction, Kansas bypasses typically have *not* had negative effects on the bypassed town as a whole. Bypasses *have* had transitory negative impacts on selected firms. The negatively-impacted firms tend to be concentrated in travel-related businesses, including restaurants, bars, motels, and service stations. However, not all travel-related firms in a bypassed town were negatively impacted in the short term.

Variation across towns and firms

There is much background variation in the experience of individual towns and individual firms. The average effects of bypasses are generally small in comparison to these background effects. However, individual towns and firms could be affected by bypasses in ways that differ considerably from the average effects. In particular, it is possible that some towns suffered permanent gains or losses due to bypasses. Also, some individual firms may have chosen to go out of business rather than adjust to changed circumstances caused by the bypass. Those firms typically were replaced by other firms.

Background effects

The size of the background variation implies that many factors other than bypasses affect the economy of small towns and of individual firms, and these various factors together are substantially more important than bypasses. In particular towns, these factors could either offset or reinforce the effects of bypasses. Two important factors touched on directly in this report are the short-term effects of recessions and the long-term health of small towns in Kansas. The 1990-91 recession had a substantially negative effect on the growth of travel-related firms in small towns, as compared to its effect in the rest of the county. The growth rates for all types of business in small towns were found to be less than the corresponding growth rates in the rest of the county, both before and after the bypass was built.

Benefits of time-saving

Bypasses around small towns in Kansas have been highly beneficial to through traffic. Bypasses of 21 small towns in Kansas generated average time-savings for through traffic that are conservatively valued at upwards of $1 million per year (in 1994 dollars). With a discount rate of 10%, an average bypass would be justified on a benefit-cost test if the present value of all costs was less than $10 million. Assuming that the initial costs of land acquisition and construction constitute at least half of the social cost, then such a bypass would be justified on a benefit-cost test if those initial costs were less than $5 million. However, this figure would vary between different bypasses, depending on the amount of time-saving and the amount of through traffic. Larger initial costs could almost certainly be justified, but doing so would require a more detailed analysis than this report can provide.

Economic impact factors in policies for building bypasses

This report considered whether an objective method is available to take economic impacts into account when deciding whether to build a particular bypass. The report suggests the following policy considerations:

1. While long-term effects may differ between bypasses, it is not feasible at present to predict these differences in an objective manner. Since long-term effects are more likely to be positive than negative, they can reasonably be ignored.

2. For similar reasons, it is reasonable to ignore any short-term effects outside the retail and travel-related sectors.

3. In the case of retailing and travel-related services, policy-makers might want to take short-term adjustment costs into account in the decision to build a bypass. However, developing systematic policies for doing this would require additional research, which is beyond the scope of this study.

Future Research

This report suggests that additional research in several areas could be of use.

1. Predicting and managing effects of highway construction on individual firms. At present, no models exist that can help an individual firm predict the effects of highway construction on that particular firm. Such models may be feasible.

2. Developing policy guidelines for including adjustment costs of travel-related firms in the bypass decision. The main problem is to develop acceptable and routine ways for measuring adjustment costs.

3. The value of time for automobile travel in Kansas. At present there is no Kansas-specific knowledge on the value that automobile drivers and passengers place on time-saving.

4. Predictive Origin-Destination models of Kansas. It would be possible to extend the O-D model developed in this study so that it can be used to make predictions over a ten year horizon.

5. Effects of highways on growth and development in Kansas. The models developed in this study could be used to quantify various effects of highways on economic development in Kansas.

1 Introduction

This report examines the economic impacts of building a highway bypass around small towns and cities in Kansas. The report has several particular goals, which are described below; but the overriding purpose of the report is to support policy making on decisions to build a highway bypass in Kansas. Some of the conclusions are also likely to be useful in other states.

The Policy Issues: An Overview

A construction project for a highway bypass is likely to rank among the more controversial activities undertaken by highway authorities, for several reasons. First, the dollar costs involved are quite substantial, leading to heightened scrutiny by taxpayers and legislators. Second, a bypass project is usually "indivisible" -- there is no easy way to compromise on a lesser project because, practically speaking, there is rarely such a thing as half of a bypass. Third, the project is concentrated in one place, so that persons most affected by the project have a pretty clear idea of who they are, and they can easily form an opinion -- whether accurate or inaccurate -- of how the project will affect them. Finally, the project imposes both benefits and costs on particular individuals which can be perceived to be, and in some cases actually are, rather substantial.

The most important benefit of a bypass typically consists in improved travel time for vehicles that do not plan to stop in the town being bypassed. Another important benefit consists in the reduction in congestion and traffic hazard inside the town. The bypass may also assist economic development and be useful to local traffic traveling from one side of town to the other. These benefits are substantial in total quantity, but often they are dispersed across many individuals.

The most important cost of a bypass is often perceived by planners to be the dollar cost of land acquisition and construction of the facility. However, local residents may be more concerned with possible adverse effects on local businesses. Local retail businesses, in particular, have legitimate concerns about the impacts of reduced downtown traffic on their businesses. These impacts have several dimensions. Some impacts may be temporary, while others may be long-lasting. Some impacts may be in the form of gains in income and revenue, others may be in the form of losses. Some impacts may show up as aggregate losses or gains; other impacts may net-out across firms. For example, new business developing at the interchanges may cancel out old business lost downtown. However, if the new business is served by establishments with different owners than the old business, then there can be a significant economic impact on individuals which does not show up in an analysis of aggregate data.

Therefore, the decision to build (or to not build) a highway bypass around an existing town or city can be difficult to make. In the first place, it can be rather hard to quantify these various types of benefits and costs. However, the extent of each of these benefits and costs can generally be estimated with the aid of measurement methods and models drawn from regional economics and transportation planning.

In the second place, we need some way to make comparisons between different types of benefits and costs. For example, we need some way to balance improved travel time against dollar cost to the taxpayer. This is commonly done by placing a dollar value on each benefit and each cost. Various models can be used to translate non-monetary benefits into dollar values. For example, the value of time saved by transportation improvements (e.g. the time-saving from driving on a bypass instead of through town) can be estimated by observing the trade-offs that people make when they have alternative travel modes that differ in time and in monetary cost.

Within such a benefit-cost analysis framework, a given project is viewed as justifiable if and only if its dollar-valued benefits outweigh its dollar-valued costs. Of course, even if the project has been justified, it still has to compete with other projects for priority before it can be funded. In the typical benefit-cost framework, those projects with the highest ratio of benefits to costs are recommended with the highest priority.

However, the policy question is not simply a benefit-cost question, which could be settled by weighing the total costs against the total benefits of having a bypass. There is also a question of the *distribution* of costs and benefits. If the benefits of the project are widely dispersed, while the costs are disproportionately borne by a particular group, then policy-makers may perceive the project as unfair, and may be unwilling to proceed even when dollar values of benefits outweigh the costs. This is particularly true if the group bearing disproportionate costs is perceived to be a disadvantaged group.

Persons living in small rural towns in Kansas, in particular, are often perceived as economically disadvantaged, and there is some factual basis for this perception. A large majority of the rural counties in Kansas have been losing population fairly steadily ever since their populations peaked around the turn of the century. This loss reflects a steady long-term erosion of the largely agricultural economic base that supported life in those counties. Wage rates in these economically disadvantaged counties are distinctly lower than wage rates in the urban areas of Kansas.[1] Moreover, the Kansas legislature has a tradition of showing special concern for impacted rural areas. At the same time, many relatively rural areas in Kansas are doing quite well. For that and other reasons, answers to these distributional questions need to be based on individualized judgements.

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[1] This remains true even after we control for the lower cost of living, especially the lower cost of housing, in these rural areas. It is especially true if we take into account the lower levels of many public amenities in rural areas.

Therefore, policy-makers are likely to take seriously any concerns expressed by local business leaders whose town is a proposed location for a highway bypass. Local leaders formulate their concerns, in part, based on available information about other towns that experienced economic impacts from building a bypass. Moreover, state policy-makers are likely to take comparative information into account when evaluating the concerns being expressed.

For those reasons it is important to have objective information about the real economic impacts to be expected from building a bypass in Kansas. This report seeks to provide that kind of information.

Goals of This Report

This report contains information on the single largest benefit of building a highway bypass. That benefit consists in the improved travel time for vehicles that no longer need to enter the town. The report also contains a qualitative discussion of other types of costs and benefits that result from building a bypass.

However, this report is not a benefit-cost analysis of bypasses in general, or of any particular bypass. It does contain information that could be important in completing future benefit-cost analyses of highway bypasses in Kansas, but a complete analysis would also depend on substantial amounts of information obtained from other sources.

Instead, the major part of this report is directed towards the economic impacts of a bypass, and especially the effects of the bypass on local businesses and workers. Economic impact studies are not concerned with the total amount of economic effects, but rather with the *distribution* of effects. In traditional benefit-cost studies, economic impacts at the local level are *not* viewed as either costs or benefits from the point of view of the state as a whole. The reason is that any jobs or businesses lost at the local level are likely to be made up by gains somewhere else in Kansas. For example, even if the existence of a bypass discourages automobile traffic from stopping for food in a given small town, those automobiles are likely to stop somewhere down the road because the travelers still have to eat.

As a necessary side issue, this report also discusses methods for estimating what portion of the traffic on a given segment of a state route is local traffic and what portion of the traffic is through traffic. More generally, the report discusses results from a model which categorizes average daily traffic observed at discrete points into numbers of trips from each possible origin to each possible destination in the state. This origin-destination information could be useful for state-wide transportation planning, even though that was not a major goal of this research.

In summary, this report uses Kansas data sources to provide information on:

1. the economic effects of a new highway bypass on a typical rural community;

2. the time and cost savings for through traffic resulting from a typical rural bypass;

3. a technique for predicting the economic impacts of a bypass;

4. a technique for predicting through traffic in small communities from existing data on average daily traffic; and

5. some implications for public policy regarding the decision to build a bypass.

Empirical Issues

This report addresses empirical questions using a combination of direct methods and applied theoretical models.

Statistical and modeling methods

Two major modeling methods were employed in this report. First, a detailed origin-destination model of Kansas was designed and implemented on mainframe and PC computers. Its parameters were determined by nonlinear least squares methods. Second, many different multiple regression models were specified and estimated. Other methods of statistical analysis were also used as needed. Details of these methods are given in the individual chapters.

Literature reviews

This report draws on much previous work by many other authors. The report contains several literature reviews of that previous work. The individual reviews are organized by topic and are contained within the relevant chapters.

Data sources

This project used primary (original) data from four important sources. Each of these data sets is described in more detail in subsequent chapters.

1. Traffic counts: KDOT provided data on average daily traffic counts on each major segment of each state and US highway in Kansas for even numbered years.

2. Construction years: KDOT provided data on the history of bypass construction in Kansas.

3. Taxable sales: data on sales-taxable retail sales by county by year were obtained from

the Kansas Department of Revenue.

4. UI payroll and UI employment: the Kansas Department of Human Resources provided records on individual retail firms for several years from unemployment compensation insurance (UI) records, showing taxable payroll and number of employees.

In addition, other primary data on county and city populations were obtained from US Census reports, and data on distances along highway segments were obtained from official KDOT maps. The project also used some secondary data derived from published studies that addressed the values of travel times and the costs of operating commercial vehicles.

The following secondary data set used in the project was generated within the project:

5. Through-traffic: estimates of the amount of through traffic on each state highway segment in Kansas in each of several years, estimated by IPPBR.

In addition, IPPBR gathered some original data, most importantly:

6. Travel time savings: direct measurements were made of the time-saving that results from using various bypasses in Kansas.

A Roadmap of the Report

This report actually contains five separate studies, each of them relatively self-contained. The first study, contained in Chapter 2, describes an origin-destination model of Kansas. This model utilized the traffic count data set to produce data regarding the amount of through traffic versus local traffic and was used to generate through-traffic data used in two subsequent studies.

The second study, contained in Chapter 3, evaluates the time-saving that resulted from a sample of bypasses in Kansas and also estimates the total dollar value of the time saved for through traffic during each year. This study uses the though-traffic data developed in Chapter 2, and also used original data developed by IPPBR by timing actual trips on and off a selection of bypasses. This chapter also provides models which can be used to predict the value of time-saving for future bypasses.

Chapters 2 and 3 contain individual literature reviews related to their respective studies. Chapter 4 consists of a combined literature review related to issues that cut across the final three studies. This review discusses economic impact analysis in general, discusses previous literature on the impacts of bypasses in particular, and summarizes some of the statistical and conceptual issues involved in impact modeling.

Chapter 5 contains the third study, which describes the long-term impact of bypasses on counties in Kansas. This study uses the taxable sales and the bypass years data sets.

Chapter 6 contains the fourth study, which describes the long-term impact of bypasses on towns in Kansas. This study uses bypass history data, aggregated UI payroll data, and through-traffic data.

Chapters 7 and 8 contain the fifth study, which describes the short-term impact of bypasses on towns and individual firms in Kansas. This study uses UI payroll and UI employment data at the level of individual firms.

Chapter 9 draws together an analysis of the results, together with a discussion of the policy issues involved in the decision to build a highway bypass. It also makes concluding remarks and suggests some direction for further research.

Separate technical appendices to this report give mathematical specifications for the origin-destination model and provide additional data and results of the regression analyses.

2 Through Traffic Estimates

Introduction

Later chapters of this report make use of estimated through traffic data, i.e. data on the amount of highway traffic that passes through or by each small town in Kansas. These data consist of the through-traffic portion of traffic counts; they omit trips which have an origin or destination in the given town or in the same county. Subsequent chapters use these estimates in two ways: first, to assist in a calculation of the total value of time saved by creation of actual bypasses in Kansas; and second, as explanatory variables in a study of the impact of bypasses on payrolls and employment. This chapter explains how the study team arrived at the through traffic estimates.

Traffic counts are available on official KDOT traffic flow maps. However, the measured traffic counts do not distinguish local traffic from through traffic. To separate out the though traffic, we used a complex model of the state which analyzed all long-distance trips in the state by their origin, destination, and route.

This model consisted of several components. In the first stage, we calculated a most probable route between each origin and destination in the state using a minimum travel time or "shortest path" algorithm. In the second stage, we estimated the parameters of a "gravity model," which explained the observed traffic counts by assuming that trips between pairs of cities depend on the population of each city and on the travel time between them. In the third stage we used these parameters to calculate the expected amount of through traffic on each highway segment.

The origin-destination model described here may have potential applications that go beyond this study. We return to those potential applications in the final section of this chapter and in the concluding chapter.

The Traffic Count Data Set

Highway traffic counts are collected by KDOT on a routine basis, and are published every other year on traffic flow maps (e.g. Bureau of Transportation Planning [1990]). Data are gathered from total vehicle flow and for heavy commercial vehicle flow. Data are gathered at one or more points on every segment between intersections of state and US highways. These data are collected by mechanical recorders over a 24 or 48 hour period, and then corrected for axle factor, day-of-the-week and seasonal variations using a model based on data gathered from continuous recorders at a smaller number of sites. These data were used in two ways in our through traffic model: first, to find a best fit for the origin-destination model; and second, to correct the estimates of through traffic and local traffic so that they add up to the total traffic observed on each highway segment.

The data set included 796 segments in Kansas, plus 54 segments that exit the state. There are 610 towns, cities, and rural nodes in Kansas, and 54 terminal nodes on exiting segments. There are a total of 664x663/2 = 220,116 origin-destination pairs.

Literature Summary: Long Distance Origin-destination Models

Origin-destination highway models, also called O-D models, come in two general types: urban models of short-distance traffic flows, and rural or long-distance models.

Urban O-D models concentrate on the effects that congestion has on the choice of routes. That is, at time of peak load, all major arteries in an urban area are likely to be sufficiently busy so that traffic is delayed or slowed down. Congestion tends to be highest on the "best" (most direct, highest speed, limited access) roads. As a result of congestion, the most direct or most limited access route between two points may not be the fastest route. But traffic loads respond to congestion, because drivers learn to seek alternate routes that are faster. Consequently, traffic loads on different road segments are determined by a kind of simultaneous process with feedback effects between alternate routes. This produces ripple-effects which can extend all over the urban area.

In rural areas, however, major highways are usually not congested to the point that traffic chooses alternate routes. Traffic loads on a multi-lane rural highway in Kansas is usually low enough so that travel time for one vehicle is nearly independent of the number of other vehicles. Travel time on a two-lane highway, however, is affected by the traffic load: as traffic increases, velocities fall and travel time rises toward that of the slowest vehicle on the road, because of the increasing difficulty of passing. Yet in most cases this "slowest vehicle" effect does not affect the choice of routes, for two reasons. First, highway routes between major economic centers are designed to follow relatively direct routes; therefore, alternative long-distance routes are likely to be substantially longer than the direct route. Second, if congestion does become perennial on a given long-distance route, then highway authorities are likely to improve that route by adding multiple lanes.

Therefore, in long-distance O-D models, congestion is not an important internal variable. Some of the models of this type, reviewed by Cascetta and Nguyen [1988], simply find the O-D matrix that "best" fits the traffic count data, using some criteria but no behavioral theory. Other models focus on the factors that generate traffic between two locations. In the simplest such model, traffic depends on just two variables. It depends directly on the product of the quantities of economic activity at the two locations (usually measured by population); and it depends inversely on the travel time between the two locations. A model of this type is called a "gravity" model. (For an introduction, see Isard [1990].) A log-linear functional form is often assumed (equivalently, an exponential form). In exponential form, the model is

(1) $T_{ij} = \alpha(P_iP_j)^{\beta}/D_{ij}^{\gamma}$, where

T_{ij} is the number of trips per unit time period between location "i" and location "j";
P_i is the population of location "i";
D_{ij} is distance or the travel time between the two locations; and
α, β, and γ are parameters to be determined.

The general economic intuition behind this model is straight-forward. First, travel between places falls off with travel time or distance. This occurs because the cost of travel is roughly proportional to travel time. People and businesses instigate less travel to distant cities than to nearby cities because travel to distant cities costs more per trip. Second, travel between places increases with population of the origin or destination because (for household travel) there are more households choosing to travel and (for business transportation) there is more business need for people or things to be transported.

However, these general considerations do not constitute a tight model. For a specific derivation of a gravity model from these considerations using an argument from statistical mechanics, see Wilson [1967].

Economic models can also say something about the plausible values of the parameters. In particular, 0.5 is at least a reasonable guess for β. The argument is as follows: one expects the number of trips per person to be approximately constant, and approximately independent of the size of city he or she lives in. Therefore, if we double the population of *every* city, we expect that the total number of trips will approximately double and hence the number of trips between any pair of cities will approximately double. If so, β must be 0.5 from equation (1). If, on the other hand, actors in larger cities have fewer interactions that cross city boundaries than actors in small towns, then β would be less than 0.5. One study that found β=0.5 in an intercity model is Mylroie [1955; cited in Isard, 1990]. In fact, some of our own empirical fits did lead to β not statistically distinguishable from 0.5. However, the final model used in this report had a estimated value for β somewhat lower than 0.5.

In empirical applications, gravity models have been found to perform reasonably well. Examples include Mylroie [1955] and other citations in Isard [1990]; and Högberg[1976].

A previous O-D model of southeast Kansas is described in HNTB [1991]. This model differs from the present model in a number of respects. The HNTB model is a forecasting model that uses a variety of survey data sources. O-D trips which could not be determined from available survey data were inferred from traffic count data using a minimum information algorithm by Willumsen described in van Zuylen and Willumsen [1980]. The Willumsen model is similar to ours in that it uses traffic count data and assumes knowledge of routes for each trip; however, it does not make use of population and distance data and contains no behavioral model. The Willumsen approach is more appropriate for small regions where much of the traffic originates outside the study area, and where it is important to infer idiosyncratic features of the O-D structure from local variations in traffic counts. The gravity model is more appropriate for large regions where the main goal is to model flows between observed towns and cities.

The Shortest Path Model

A key assumption of our origin-destination model is that traffic takes the fastest available route from its origin to its destination. Therefore, to implement the model, we first needed to calculate the fastest route for every possible trip (i.e., for every possible origin-destination pair) in our model of the state of Kansas (including exit and entrance routes from the state). Since there are around half a million distinct possible trips in our model, this task had be done by a computer program.

Several types of data were needed for this task. First, based on the traffic count maps, IPPBR created a dataset that identifies each state route segment in the state. A segment is identified by its two nodes (i.e., its two endpoints). The dataset gives each node a unique identifier (usually the name of the town or city at that location). Consequently, the dataset can be used to locate all the segments that start or end at each node.

Next, using a KDOT highway map, IPPBR entered data showing the length of each segment in miles. We also entered data on the type of road. We then calculated an estimated travel time on each segment, assuming that traffic travels at 65 mph on limited access divided highways, and at 55 mph on other highways.

Finally, a computer program was written which checks every possible route for each trip and finds the fastest route. (The program was based on a particular algorithm by Dijkstra [1959] which is computationally efficient. No description of the algorithm will be given in this report because the task is fundamentally simple.)

After running the program, we checked some of the major routes selected by the program. All of the routes we checked made sense. That is, paths between cities near an Interstate Highway follow the Interstate Highway. Other major routes generally follow US highways. Routes near a highway bypass do use the bypass. There could still be some ill-chosen routes in the dataset that we did not detect, but our impression is that any errors of this type are likely to be on small-volume trips that do not have much effect on results of the subsequent analyses.

The Gravity Model

We used the traffic count data for 1990, together with population and travel time data to fit the parameters of a gravity model of Kansas trips by origin and destination. Travel time data were the same as those used in the shortest path model. Populations of towns and cities were taken from the 1990 Census of Population. The population of those persons in a county that didn't reside at identified nodes (i.e. that didn't live in towns or cities) was distributed across all the nodes of the highway map in that county, in proportion to the total traffic count of all segments touching a given node.

The model was largely based on equation (1). Equation (1) by itself is log-linear (after you take logarithms of both sides), but the fit model is non-linear. There are two factors that introduce non-linearity into the process of fitting the best parameters to that model:

1. Equation (1) predicts O-D counts on the shortest route from city I to city j. Since we know what segments lie on the shortest path, it also predicts counts on each segment resulting from each individual O-D pair. But our raw data consist of *total* traffic counts on a segment, and not individual O-D counts. To get total traffic counts, we must add up the counts of all the individual O-Ds that use that segment. The adding-up equation is nonlinear relative to equation (1).

2. Many of the highway segments exit the state. For these segments each trip depends on populations and distances to a location not contained in our data set. We introduced new parameters into the model to handle traffic on these existing segments.

In order to fit the parameters of the non-linear model, we used a modified Gauss-Newton method[2] in the NLIN procedure of the SAS statistical programming language. The goal of the best-fit program was to locate parameter values which minimized the sum of the squared errors between predicted traffic counts and actual traffic counts. The program was somewhat complicated and used various data processing tricks to manage the very large data set being fit. In an initial exploration, we searched for best fits for the three parameters α, β, and γ. This procedure used up the better part of a week of computer time on a main frame or on a Pentium PC in order to find a single solution, and even then was not able to locate a distinct minimum point. We therefore wrote an internal linear minimization routine which calculated α from β and γ and the given data. This change dropped the time required for a fit to around a day. With this new algorithm, we were able to show that the problem did have a unique point with minimum least squares.

The procedure so far had omitted traffic on the segments that exit the state of Kansas. Next, we introduced new parameters to account for the traffic that exited the state. The final fit used in this report explained about 51% of the variance in traffic counts, and used the following parameter values:

$\alpha = 0.002457$ trips per day per (person$^{2\beta}$) per ((hour of travel)$^{-\gamma}$).
$\beta = 0.4358$
$\gamma = 2.176$.

[2] Known variously as the multivariate secant method, the method of false positions, the DUD or Doesn't Use Derivatives method.

The Through Traffic Model

Through traffic and local traffic are used as explanatory variables in subsequent chapters of this report. Estimates of these variables were calculated for each segment of the highway map using the results of the O-D model. Through traffic on a highway segment was defined as traffic with its origin and/or destination in a different county from that of the given segment.[3] Local traffic on a segment was defined as all other traffic. The calculation logic for 1990 was straight forward: first, raw estimates were formed by adding up all traffic predicted on that segment from all O-D pairs in the model, while classifying each contribution as "through" or "local." Second, these raw totals were adjusted so that they added up to the traffic count observed on that segment. For years other than 1990, this procedure was then adjusted for changes in the populations of origins and destinations.

Chapter 3 contains some additional discussion of the through traffic model and provides results for through traffic on bypasses.

Potential Uses of the Origin/Destination Model

In this report, the O-D model was used only for generating a set of through traffic and local traffic estimates. However, it has other potential uses in statewide transportation planning for making two kinds of predictions.

First, the model can be used to predict the traffic load on new or modified long-distance routes, assuming that existing demand conditions do not change. That kind of prediction would be a rather straightforward extension of the existing model. Second, the model could also be used to predict future traffic loads on existing routes. This kind of prediction would depend on some additional development work. In particular, we might construct a time series of past models and then study how the parameters of the model respond to changes in economic conditions.

In either case, the model is suitable only for studying rural and long distance routes. Even though a few urban routes are included in the model, it is not very suitable for studying them because the model does not account for the effects of urban congestion.

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[3] For segments that cross a county boundary, "through traffic" was defined as traffic with an origin and/or destination in some third county.

3 The Value of Travel Time Savings

Introduction

This chapter estimates the amount of travel time saved annually by use of selected bypasses in Kansas and also estimates the aggregate dollar value of that time. Data on travel time are provided for a sample of 21 bypasses in Kansas. Valuations of travel time savings are based on secondary sources.

In the case of commercial transportation, the concept of the dollar value of time-saving is well defined and easy to measure, at least approximately. The concept could be defined as the reduction in costs to consumers of transportation that results from the time-saving. In a competitive market such as trucking, that cost should equal the reduction in cost to the trucking firm (with normal return on capital understood to be a normal part of cost). These costs can be determined from published data on the cost of operating trucks, divided by the number of hours they are operated. Good data on costs exist because commercial trucks operate in well-defined markets, which generate financial records as a normal part of business. Reasonably good data on hours of operation also exist.

In the case of personal transportation, the concept of the value of time can also be given a clear definition, but measurement of that concept is not so straight forward. Conceptually, the value of time-saving is the amount that an individual would be willing to pay in order to obtain it. In terms of measurement, there are no markets where individuals actually and directly buy time-saving; therefore, we need to use indirect means of valuing time. And, in addition to value of personal time of the person being transported, we need to consider the dollar cost of operation of the vehicle per time.

The Value of Time for Private Auto Traffic (literature review)

One indirect means of valuing time is simply to ask people what they would pay in order to save an hour on a long trip. But it is known that surveys of this type are subject to various biases. For example, since there are no markets for time saved, people do not think very carefully about what they should be willing to pay for time-saving. Also, people might be tempted to overstate or understate the stated value, depending on how they think the survey data might be used. Therefore surveys need to be supplemented with other indirect means of measurement.

Economists have used many different models to derive a value of time in given situations. There is enough commonality of results to support general conclusions about the value of travel time. Though Kansas has not been studied directly, their results can be applied to Kansas in at least a general way. The most widely and recently published expert in the field is David A. Hensher, an associate professor of economics at Macquarie

University in Sydney, Australia [Hensher, 1976, 1978, 1984; Hensher and Hotchkiss, 1974; Hensher and Truong, 1985; Hensher *et al.*, 1990; Truong and Hensher, 1987]. However, nearly all the economists contributing to the literature, whether they were conducting an econometric study, a direct survey, or a mathematical model, relied on the technological constraint formulas provided by DeSerpa in 1971. DeSerpa's model is a generalization of a model by Becker [1965].

The general setup in these studies starts with a data set showing choices that individuals have made on various trips, together with the constraints they faced. The main choice is the transportation mode (e.g. auto, bus, train). The main constraints are of two types: travel time and money costs for each mode. The goal is to infer the trade-off rate that people will accept between time and money.

The basic assumptions of Becker's models are these:

1. Individuals prefer to save money and also to save time.

2. Their trade-off rate between money and time is relatively constant.

3. Other than time and money costs, individuals are indifferent between transportation modes.

DeSerpa generalized the model so that:

1. different modes of travel have innately different levels of utility per time associated with them; e.g. auto travel may be more "comfortable" than public transit; and

2. within a given mode for a given trip, travel time cannot be reduced continuously by spending more money; i.e. there is a "technological constraint" which relates travel time and distance to cost.

DeSerpa also made some technical assumptions that allow the model to be solved.

Using models such as these, a range of estimates for the personal time-value of the driver is presented in the literature. These range from around $3.50/hour to $32.50/hour in constant 1994 US dollars. It is possible to limit the consideration further. First, studies of dense urban roads often resulted in a much higher value of time than rural studies. Since we are considering travel on open Kansas roads, it would seem logical to use an estimate based on those studies that found lower values. Second, the value of time was generally found to depend on wage rates. Since we are looking for an average result (and since Kansas has a relatively average distribution of income), we should use results for a moderate level of income. Third, since median US wage rates have remained roughly constant since about 1970, we should emphasize results based on post-1970 data. Table 3.1

below shows the results of empirical studies in the United States. Although the Hensher studies took place in Sydney, they are included as well due to Hensher's importance in the literature and because his studies are denominated in US dollars.

Based on these considerations, the estimated value could be based on the mean of the lower bound estimates of the studies, or about $9/person-hour (1994 dollars). The estimated error range could be taken as plus or minus the standard deviation of the lower bounds, which is about $4/person-hour. The estimated range is therefore $5 to $13 per hour.

Another way to estimate it is using a rule-of-thumb assumption appearing in the literature that time-value for the driver is around 40-60% of the local wage rate. Kansas annual wage and salary levels averaged around $23,000 in 1994 for full and part-time workers combined [BLS, 1995]. If the average worker worked around 1,650 hours in 1994, then hourly rates were in the neighborhood of $14/hour (net of any fringe benefits). 40-60% of that amount would be $5.50 to $8.50/person-hour, which is contained within the previous range of figures.

The value of time for the vehicle as a whole is greater than the personal value of time for the driver. In transportation studies it is conventionally assumed that the time value is less for passengers than for the driver; and there are two persons per automobile on average on rural highways. We also need to take into the account the automobile itself, which has a marginal cost of operation around $5 per hour. Assuming that the passenger's time value is 50% of the driver's time value on average, we would arrive at a total time value of $13 to $23 per vehicle-hour. For calculating the value of time-saving, we will use the estimate of $18/vehicle-hour.

These results are reasonably consistent with a recent review by Waters [1991; cited in Morrall and Abdelwahab, 1993], who found a value of around $13.43 (1990 $CDN) per vehicle-hour. Two corrections to Waters' result, one for inflation during 1990-94 and the other for differences in purchasing power parity between Canada and the US, would roughly cancel out.

The Value of Time for Truck Traffic

We will estimate the cost of operation of trucks in two steps. First, we will estimate the average number of operating hours per driver per year. Second, we use this and other data to estimate the cost per hour.

Driver hours per year

Data for inferring driver-hours per year are based on two models, which lead to similar answers. The first model for annual driver-hours consists of average industry annual wages for drivers (from Table 3.2) divided by an average hourly wage rate. From total wages and total employees in table 3.2 we can infer an average annual wage of $31,000. A wage rate of $10.80∓.80/hour is given for heavy truck drivers in an extensive Kansas survey of wages by occupation in the Fall of 1992 [Kansas Department of Human Resources, 1993]. However, this rate should be translated into a 1993 US estimate, using two correction factors: the ratio of Kansas wage rates to US wage rates in the transportation industry in 1992; and the ratio of the US wage rates in transportation industry in 1992 to those in 1993 [BLS, cited above]. The result of that calculation is an estimated national 1993 wage rate of about $12/hour. Dividing that wage rate into the average annual wage implies that drivers worked about 2600 hours in 1993.

The second model for annual driver-hours is based on informal interviews with two truck company operators [private communication]. They stated that their drivers typically work the USDOT (Federal Highway Administration) daily maximum of 10 hours, and they average about 5.5 days of work per week. If drivers work 50 weeks of the year, that would amount to 2750 driver hours per year.

Since they depend on models, these data on work hours would be expected to introduce some error. However, this error is probably not too large, in light of the reasonable agreement of the two independent models. Additional considerations constrain the result. On the one hand, USDOT regulations prevent a driver from operating more than 60 hours per week. On the other hand, the relatively high cost of trucking equipment and the relatively high time-value for getting the freight to its destination both place strong pressures on drivers to keep the trucks moving as many hours as possible. The modeled data imply that drivers are operating at around 90% of the USDOT limit over the course of the year, with the other 10% consisting of days off. It seems unlikely that the industrial average for reported hours could be much higher than 95% of the USDOT limit, given the practicalities (e.g. health and willingness to work) that would restrain a typical driver from operating at the limit. It also seems unlikely that firms operating much below 80% could remain competitive at the same wage rates with firms able to operate at 90% (because their unit equipment costs would be more than 10% higher). Therefore this deviation introduces an error in our measured cost per hour that is probably less than 10%.

The logic in the previous paragraph assumes, however, that operating hours are reported accurately by trucking firms. There is informal evidence that this is not the case. That is, individual drivers often have an incentive to work hours that exceed USDOT regulations and then falsify the records of hours worked; and some of them probably do underreport their hours. This practice could lead to an upward bias in our estimated cost per hour under the second model. However, "off-book" hours should show up in the first model used above. That is, drivers have a strong incentive to make sure they are paid for the hours they actually work; some drivers, for example, might use two sets of books and report falsified extra hours on days they actually worked less than 10 hours, which compensate for hours not officially reported. The reported total wage bill for drivers is likely to be accurately reported, because wages are an income tax deduction for the firm. Also, the modeled hours per driver are already very high compared to hours worked in most other occupations; it seems implausible to assume that adding the unreported hours could drastically increase this amount.

Cost per vehicle-hour

If we assume that trucks rarely carry two paid drivers at the same time, then driver-hours per year should be multiplied by the ratio of drivers to trucks (1.08, based on data in Table 3.2) in order to translate into truck-hours per year. Adjusting for drivers/truck yields about 2800 operating hours per truck-year in the first model, and about 3000 operating hours per truck-year in the second model.

Table 3.2 shows data on the number of trucks, miles driven and operating costs incurred for US trucking firms in 1993. These data imply an annual cost of about $83,800 to operate an average tractor-trailer truck in 1993. The cost concept includes variable costs, i.e. costs that depend on the miles driven or hours operated, including wages, fuel, maintenance, and normal returns on the capital invested in trucks. However, it omits fixed costs, i.e. costs that depend on the number of transactions, including costs of terminals, dispatching, and advertising (but these fixed costs are only a few percent of total costs). Data on costs, numbers of drivers and numbers of trucks are taken from American Trucking Association [1993, pp. 4-5] and are probably fairly accurate.

To translate annual cost per truck into a cost per truck-hour, we need to divide by the number of operating hours per truck, estimated at around 2900 hours per year. From this we infer it costs about $29 per hour to operate a tractor-trailer truck in the US in 1993. Using US annual wages for transportation, communication and utilities as an inflation index [BLS, computer-readable, 1993, 1994], we can estimate 1994 costs at about $30/hour.

The figure of $30/truck-hour was based on US data. Conditions could differ somewhat in Kansas, mainly because of differences in the wage rates of drivers. As we have already seen, that difference is relatively small. Also, many trucks operating on Kansas highways originate in other parts of the US.

In light of the estimated errors in driver-hours per year, ∓10% may be a reasonable estimate of the error rate for the estimated cost per hour for trucks.

Time Saved by Kansas Bypasses

In the Spring of 1995, IPPBR personnel measured the time saved by traveling on a bypass rather than through the city for a sample of 21 bypassed towns and cities in Kansas.

The methodology was as follows: in most cities, an IPPBR driver operated an automobile between intersection points of the bypass with the city route, once on the bypass and once through the city, and timed the two trips. However, in some cases, the current city route was a spur or in other respects did not correspond to the old city route that was in place before the bypass was constructed. In those cases, the driver timed two trips of approximately equal distance, one along the bypass and the other along the old city route.

Each trip took place during the work day (roughly 9:00 AM to 5:00 PM) on a weekday in midweek. The driver drove at the rate of the stream of traffic, which in a majority of cases exceeded the posted speed limit. Time-saving per trip was estimated as the difference in the two travel times. These results are reported in Column 2 of Table 3.3. (More complete detail showing all of the raw data is contained in Technical Appendix 3.1.) Note that these results should be interpreted as estimates of a lower bound for time-saving, for the following reason: if the bypass had not been built, then congestion through the town would have been higher in all cases, and substantially higher in some cases. With higher congestion, the trip through town would have taken longer on average. The true time-saving from the bypass is the difference between the actual time to traverse the bypass, and the (hypothetical) time it *would* have taken to traverse the town if the bypass had not been built.

Column 3 of Table 3.3 gives the length of the bypass route or city route in miles. It was measured by the IPPBR driver using the automobile odometer. Column 4 gives the average speed driven on the bypass. This was calculated as the ratio of distance to travel time. The data on speed and length are used below in statistical models for predicting per-trip time-saving on future bypasses.

Traffic Counts and Total Time-Saving

To calculate a total amount of time saved by the bypass, we need to multiply the per-trip time-saving by the number of trips being executed. Daily traffic counts are available from the KDOT traffic count map. However, these numbers need to be adjusted to account for local traffic, e.g. that portion of the traffic that is using the bypass to get from one side of town to the other. Local traffic of this type does not enjoy the full time-saving afforded by the bypass, because it typically starts and ends inside the through-city route. That is, in the absence of the bypass, the vehicle would have traveled less distance inside the city than through traffic would have traveled; and also, when taking the bypass, it travels a greater distance than through traffic now travels on the bypass. Therefore we omitted local traffic from our estimate of aggregate time-savings. Note that we are again using a lower bound estimate, since local traffic typically does enjoy *some* time-saving from the bypass (otherwise it wouldn't use the bypass.)

We assumed that all heavy vehicle traffic on the bypass was through traffic. Truck traffic on the bypass is reported in Column 3 of Table 3.4; it comes directly from KDOT traffic count data. We also estimated through automobile traffic on the bypass using a model, as reported in Column 4 of Table 3.4.

The model of auto through traffic was based on the origin-destination model described in Chapter 2. Using this model, we estimated numbers of trips (both auto and truck) between all cities in the model that were expected to use the bypassed highway. We divided these trips into two kinds: trips with origins and/or destinations within the same county (including trips to or from the bypassed town), and trips between origins and destinations both lying outside the county. This division is once again based on a lower bound approach: traffic within the county may have an independent reason to stop in or near the town, and therefore either would not use the bypass, or would use it but not enjoy the full time-saving. These predicted counts of local traffic and through traffic were then adjusted to match the observed total traffic count. Finally, from the adjusted counts of predicted total through traffic, we deducted the observed truck traffic to yield an estimate of auto through traffic. (None of the estimates of auto through traffic were negative, which could have happened had our model of total through traffic been too conservative.)

The table also reports the predicted aggregate time-saving per year for truck traffic and auto through traffic, calculated according to the formula:

(3.1) Annual time-saving =

$$\text{(time-saving per trip)} \times \text{(through traffic counts per day)} \times \text{(365 days/year)}.$$

Aggregate time-saving for truck traffic is reported in Column 5, and for through auto traffic in Column 6. Column 7 gives a total time-saving from both kinds of through traffic.

The Aggregate Dollar Value of Time-Savings

Aggregate value of time-saving is defined as

(3.2) $$\text{(Aggregate value of time-saving by type)} = \text{(annual time-saving)} \times \text{(value of time)}.$$

Estimates of the aggregated dollar value of time-saving are shown in Table 3.5 for each bypass. Column 2 shows an estimated value for trucks, Column 3 shows an estimated value for automobiles. Column 4 gives an estimated total for trucks and cars. These aggregate values simply represent the aggregate time-saving, multiplied by the values of time derived from the literature reviews.

Note that the last rows of the table show total and mean values across all bypasses. The total value of time-saving for the 21 bypasses is greater than $21M per year. The mean value per bypass exceeds $1M per year in 1994 dollars.

Analysis of Sources of Error

These measures of aggregate dollar value of time-saving are subject to uncertainties that derive from several sources. This section discusses the approximate size of these errors. Each error will be assumed to be measured as a root-mean-square (RMS) percent error rate. Therefore, for independent errors in terms that multiply together, the error rate for the product is the root of the sum of the squared percent errors of the individual multiplicands. For each source of error, we will estimate two error rates: a most likely error rate, and an extreme outer bound error rate.

Traffic count data

The raw traffic count data are collected mechanically by KDOT and can be presumed to be very accurate for the specific time period they were gathered (usually a 24 hour period), but counts during that particular time period might differ from the average over the course of the year. However, the data are corrected for day-of-week and seasonal variation using data from some 100 recorders across the state that operate continuously. No firm information is available on the expected error rate, but the procedures imply that the error rate is small, probably no more than a few percent. I will somewhat arbitrarily estimate the most likely error rate as less than 5%.

We will form an extreme outer bound measure of the error rate as follows: based on the fact that a simple gravity model was able to fit up to 60% of the variance in these data using only three parameters[4], not more than 40% of the variance in these numbers is due to measurement error. Most likely, however, given the small number of parameters, most of this variance is due to modeling error, and the measurement error in the underlying data is much smaller than 40%. Assuming that modeling error is substantially larger than measurement error, then 15% might be an extreme upper bound on measurement error.

Through traffic models

Technical Appendix 3.2 develops an independent model of through traffic for certain bypasses. It is evident from data in a separate Technical Appendix (Table A3.2) that the two models of through traffic can differ from each other by an average of around 20% on

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[4] This was the case for an O-D model which excluded highway routes that exited the states. The more general model that included exit routes had a less successful fit, but that may only reflect the lesser amount of time devoted the latter problem.

a given route. Since both of these models are reconciled with the traffic count data, these errors are in addition to errors in the traffic count data. Since the two models are otherwise independent of each other, the 20% figure may be a reasonable upper bound on the overall error in either model. If the error rate is equal between the two models, then each model would have about 14% error.[5]

Bypass time-saving measurements

Random sampling error could be important in the measures of time-saving due to the bypass. Repeated measurements of time-saving on some of the routes (reported in Technical Appendix 3.1) found a variation of around 10%. However, these repeated measurements took place on the same day. It is at least conceivable that time-saving varies drastically between different days of the year.

Data in Table 3.3 cast some additional light on this source of error. If we were to make an absolutely worse-case assumption, then *all* of the variation in measured speed across different bypasses would be attributed to random sampling errors and none would be assumed to reflect actual differences between bypasses. Under this assumption, observed variations in speed calculated from these data (and reported in Technical Appendix 3.1) imply that individual time measurements have a random root-mean-square (RMS) error of the order of 15%. Time-saving is measured as the difference between two times (on and off the bypass). The difference is typically about 1/3 of the average time. Under these worst-case assumptions it can be shown that the RMS error for measurements of time-saving would be around 50%.[6]

Estimated unit values for time

Judging from variations across studies in the measured values of automobile travel time (described above), it appears that the value of time for auto traffic is known only within some 30% or worse. The value of time for truck traffic is probably much more accurate, being based on market data, so that errors in the value of auto time are likely to dominate errors in the value of truck time. Since auto traffic receives about half of the value of time-saving, the average error in the unit value of time may average about 15%.

Total error rates

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[5] Because $\sqrt{[14\%)^2+(14\%)^2]} \approx 20\%$.

[6] More precisely, it would be $\sqrt{[(15\%)^2+(15\%/3)^2]}/(1/3) \approx 47\%$. A cross-check show that this figure is truly an upper bound: if the error were much higher, then with a sample of 21 bypasses, with a high probability one or more bypass would have shown a negative measured time-saving. But no such event was observed.

Under absolutely worst case assumptions, the RMS error from all of these sources together in the aggregate value of time-saving for any one bypass could approach 50%.[7] Assuming these errors for individual bypasses were independent, the error rate for the aggregate across all 21 bypasses would be around 10%. However, the fourth source of error (i.e. error in estimated value of a minute saved) is not independent across bypasses. Taking that into account, the error rate for the aggregate value across the 21 bypasses would be around 15%.

However, a much more reasonable estimate of the error per bypass, using moderate assumptions, is around 20%.[8] Under these assumptions, the error in the value of a minute would once again dominate other errors in the aggregate across bypasses, leading to a net error rate for the aggregate of around 15%. This error is mainly caused by uncertainty about the value of time for automobile travel.

Predicting the Per-Trip Time-Saving for Future Kansas Bypasses

The next three sections deal with the problem of predicting the value of time-saving for a planned new bypass. We begin with a method of predicting the time-saving for a single trip on the bypass.

Time-saving for a single trip on a bypass increases with the length of the bypass. A simple linear regression model on the sample data given in Table 3.3 yields the following predictive equation:

$$(3.3)\quad \text{(Time-saving per trip (minutes))} = \underset{(0.98)}{(1.74)} + \underset{(0.083)}{(0.202)}\text{(length of the bypass (miles))}.$$

(The numbers below the coefficients are standard errors. The coefficient of determination or R^2 was 0.24 with N=21.) Because of the nature of the sample, this equation would apply mainly to bypasses around small towns.

Note that this predictive model does not use any data about traffic speeds or delays when driving through the particular town in question. Given such data from engineering studies, an alternative estimate of the time-saving could be formed by predicting travel time on the bypass and comparing it to measured travel time through the city. Time-saving would then be estimated as measured travel time through the town less predicted travel time on the bypass. Using the average speed on bypasses taken from Table 3.3, travel time on the bypass could be estimated as

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[7] Because $\sqrt{[(15\%)^2+(40\%)^2+(10\%)^2+(15\%)^2]} \approx 46\%$.

[8] Because $\sqrt{[(5\%)^2+(10\%)^2+(10\%)^2+(15\%)^2]} \approx 20\%$.

(3.4) (travel time) = (length of the bypass)/(58 mph).

The estimated average speed of 58 mph on bypasses has a standard error of estimate of about 2 mph. However, this error refers to accuracy with which we know the average across all bypasses. Individual bypasses can vary from the group average with a standard deviation that may be as large as 9 mph. Consequently, for a particular bypass the predictor (3.4) has an innate error of perhaps 15%.

Any prediction of time-saving in the future is necessarily rather sensitive to assumptions about speed limits. That is, these predictive equations assume that the typical speeds of traffic will not change from those observed. However, in the intervening time Kansas has increased from the then current 55 to 60, 65, or 70 (depending on conditions) while keeping speed limits inside the small towns unchanged. We would expect increases in speeds on bypasses with increased speed limits, which would lead to increases in the time saved per trip.

Predicting Through Traffic Counts

Through traffic in a town can be measured directly, by stopping traffic and asking the driver about the origin and destination of the trip. Less accurate predictions could be formed in the same way that we formed the through traffic model used in Table 3.4. A simpler approximation could be formed using the average ratio of through traffic to total traffic from Table 3.4. That leads to the predictor:

(3.5) Through traffic per day = (0.82)(total traffic per day).

However, the standard deviation corresponding to the coefficient in this estimator is rather high, around 0.22. That is, this estimator will have 25% error or more in particular cases.

Predicting the Aggregate Dollar Value of Time-Saving for Future Kansas Bypasses

After predicting the time-saving and the through traffic, the aggregate value of time-saving for a bypass can be predicted using equation (3.1) and (3.2). The main remaining problem is to update the unit value of time for truck and auto travel. The value of time for truck travel could be updated simply by assuming it moves in proportion to a national price index for truck tariffs. The value of time for auto travel could be updated in proportion to a median annual salary for Kansas. Both types of information are provided by the Bureau of Labor Statistics, USDOL.

Conclusion

For this sample of 21 bypasses, the average annual saving in travel time for through traffic is conservatively valued at some $21M per year, or some $1M per bypass. The sample is by no means random, however, but focused mainly on small towns and ignored Interstate Highway bypasses. The aggregate value of the time saved by a bypass increases, of course, with the amount of through traffic, and with the time it takes to drive through the town; therefore savings are likely to be larger on major highways than minor highways, and larger near larger towns than near smaller towns. Therefore the estimated aggregate savings for all bypasses in Kansas (including large cities and Interstate bypasses) would be several times larger than the figure cited above.

It may be desirable in future research to try to improve on the accuracy of the aggregate value of time-saving. Based on the analysis of sources of errors given above, the factor that is most critical to over-all accuracy is the value of travel time savings for automobiles. Future work might usefully seek either direct new measurements of the value of travel time in rural Kansas or better ways to extrapolate existing time-value measurements between different places and times.

Table 3.1
Summary of Literature: Results of United States and Australian Studies

Author/ Publication	Data Year	Data Location	Type of Study	Value of Time (Nominal, cents/minute)	Value of Time (Nominal, dollars/hr.)	Value of Time (Real, dollars/hr.)[2]
Hensher and Truong [1985]	1981, 1982	Sydney	econometric model	16.25 - 36.55, based on varied techno-logical constraints (1.1531-4.8458)[1]	9.75 - 21.93	14.45 - 32.51
Larson [1993]	Summer 1980	Alaska	econometric model	3.73 - 7.86, based on various levels of household income (less than $20,000 /year - over $60,000/year)	2.24 - 7.86	3.87 - 13.61
Hensher, Milthorpe, Smith, and Barnard [1990]	1987	Sydney	survey/ model combination	4.82 - 16.82, based on varied types of travellers[1]	2.89 - 10.09	3.59 - 12.53
Kraft and Kraft, *Journal of Econo-metrics*	1961	New England	econometric model	3.083 - 7.217, based on different travel modes	1.85 - 4.33	8.74 - 20.46
Hensher and Truong [1985]	1982	Sydney	survey/ model combination	9.3 - 16.3, based on an exclusion of individuals with negative values on one or more time dimensions[1]	5.58 - 9.78	8.27 - 14.50
Shuldiner [1982]	1981	Cambridge	survey	8	4.80	7.11
Lisco [1967]	1964	Chicago	econometric model	4.22 - 6.12, based on different car mileage costs	2.53 - 3.67	11.38 - 16.50
Thomas [1967]	1965/66	USA	survey	6.37	3.82	16.12
Thomas [1967]	1965/66	USA	econometric model	3.03	1.82	7.68
Guttman [1975]	1969	USA	survey	5.08	3.05	11.34
Guttman [1975]	1969	USA	survey	4.92	2.95	10.97

[1] The Hensher studies were calculated in American dollars.
[2] Real values were calculated in 1994 dollars using GDP deflators.

Table 3.2
Costs of Operating Tractor-Trailers

Item	Cost
Wages and miscellaneous	$ 6,000,664,000
Total fringes	317,346,900
Total operating supplies and expenditure	3,317,501,000
Total general supplies and expenditure	1,177,384,000
Total operating tax and licence	1,003,504,400
Total insurance	874,374,000
Total communications and utilities	384,701,000
Total depreciation and amortization	1,210,246,000
Profit on variable assets	683,028,300
Total variable costs	$14,968,749,600
Number of drivers	193,108
Number of trucks	178,570
Variable costs per truck	$83,826

Source: American Trucking Association [1993].

Table 3.3
Bypass and Local Travel Times

City/Highway	Time Saved	Distance	Mph	City/Highway	Time Saved	Distance	Mph
Spring Hill		3.6	78.6	McPherson		6.2	59.9
US - 169	00:03:13	3.3	33.8	I - 135	00:01:46	5.1	33.9
Louisburg		8.2	61.8	McPherson		3.0	60.6
US - 69	00:01:30	8.2	52.3	K - 61	00:04:19	3.5	29.4
Paola		7.0	61.9	Eudora		5.9	68.2
US - 169	00:03:07	6.8	41.7	K - 10	00:02:23	5.8	46.9
Osawatomie		6.7	60.1	Perry		3.8	61.7
US - 169	00:04:21	7.7	42.5	US - 24	00:01:45	3.4	37.9
Pleasanton		2.3	55.7	Valley Falls		2.0	53.3
US - 69	00:02:35	2.7	32.6	K - 4	00:01:59	2.7	38.9
Humboldt		10.7	62.5	Hiawatha		3.4	60.1
US - 169	00:04:32	11.7	47.9	US - 36	00:03:20	3.6	32.7
Chanute		7.7	55.5	Highland		2.9	61.9
US - 169	00:03:36	7.1	36.1	US - 36	00:03:34	4.1	39.3
Earlton		5.1	70.8	Troy		2.6	51.1
US - 169	00:02:46	5.5	47.2	US - 36	00:02:19	3.0	34.6
Peru		0.2	37.3	Natoma		2.0	34.4
US - 166	00:01:19	0.7	26.4	K - 18	00:00:52	2.2	48.8
Severy		2	53.3	Hays		4.7	36.7
K - 99	00:03:16	2.9	32.2	K - 183-183A	00:04:00	4.9	47.9
Peabody		1.1	67.3				
US - 50	00:00:54	1.0	32.5				

Note: for each city, the first line gives distance and speed on the bypass; the second line gives distance and speed through town.

Source: IPPBR

Table 3.4
Bypass Traffic and Aggregate Travel-Time Savings

Bypass	*Daily traffic estimates* **Total**	**Truck**	**Through Auto**	*Annual time-saving estimates (hours)* **Truck**	**Through Auto**	**Total**
Chanute US-169	2,285	595	1,690	13,004	36,935	49,939
Earlton US-169	2,130	590	1,540	9,910	25,866	35,776
Eudora K-10	15,245	530	12,183	7,669	176,273	183,942
Hays US-183	3,435	350	3,085	8,145	71,793	79,938
Hiawatha US-36	1,720	365	1,355	7,386	27,420	34,806
Highland US-36	1,725	330	672	7,145	14,558	21,703
Humboldt US-169	2,330	410	1,920	11,284	52,841	64,125
Louisburg US-69	1,550	155	1,395	1,411	12,703	14,115
McPherson K-61	4,040	475	3,565	5,094	38,235	43,330
McPherson I-135	8,040	2015	6,025	52,805	157,891	210,696
Natoma K-18	520	110	275	579	1,448	2,027
Osawatomie US-169	3,030	467	1498	12,333	39,563	51,895
Paola US-169	5,850	710	1,347	13,434	25,480	38,914
Peabody US-50	3,435	1,240	1,553	6,775	8,487	15,262
Perry US-24	3,590	460	630	4887	6,692	11,579
Peru US-166	2,005	340	1,661	2,718	13,276	15,994
Pleasanton US-69	3,660	660	2,839	10,351	44,522	54,873
Severy K-99	885	158	727	3,133	14,418	17,551
Spring Hill US-169	6,485	605	3,073	11,814	60,008	71,823
Troy US-36	2,565	415	1,197	5,837	16,838	22,674
Valley Falls K-4	2,940	240	1,066	2,890	12,830	15,720
Total	77,465	11,220	55,509	198,604	858,077	1,056,682
Mean	3,689	534	2,313	9,457	40,861	50,318

Source: KDOT; IPPBR (see text)

Table 3.5
Dollar Values of Time-Saving

Bypass	Truck	Through Auto	Total
	Millions of $ per year ($1994)		
Chanute US-169	0.390	0.665	1.055
Earlton US-169	0.297	0.466	0.763
Eudora K-10	0.230	3.173	3.403
Hays US-183	0.244	1.292	1.537
Hiawatha US-36	0.222	0.494	0.715
Highland US-36	0.214	0.262	0.476
Humboldt US-169	0.339	0.951	1.290
Louisburg US-69	0.042	0.229	0.271
McPherson K-61	0.153	0.688	0.841
McPherson I-135	1.584	2.842	4.426
Natoma K-18	0.017	0.026	0.043
Osawatomie US-169	0.370	0.712	1.082
Paola US-169	0.403	0.459	0.862
Peabody US-50	0.203	0.153	0.356
Perry US-24	0.147	0.120	0.267
Peru US-166	0.082	0.239	0.320
Pleasanton US-69	0.311	0.801	1.112
Severy K-99	0.094	0.260	0.354
Spring Hill US-169	0.354	1.080	1.435
Troy US-36	0.175	0.303	0.478
Valley Falls K-4	0.087	0.231	0.318
Total	5.958	15.445	21.403
Mean	0.284	0.735	1.019

Source: calculated by IPPBR (see text)

4 Economic Impacts I: Introduction

Introduction

This chapter serves as an introduction to the following four chapters. This chapter provides a general review of the literature on impact modeling, especially with respect to highways and bypasses, and sets up the general conceptual model that is employed in the subsequent chapters. Each of the subsequent chapters uses a different approach and measures a different aspect of the economic impact of building a bypass. There are multiple aspects because impacts can be examined from several points of view along several dimensions. Three dimensions are especially significant.

The first dimension is time. Impacts can be considered from either a short-term or a long-term point of view. By short-term effects, we mean effects that take place within a small number of years before or after the bypass is constructed and opened. These effects typically depend on elapsed time before or after the construction. These effects can be described as disequilibrium effects, in the sense that they involve changes leading from one situation to another. By long-term effects, we mean effects that take some time to appear, and then remain steady. These effects can also be described as equilibrium effects, meaning that no change is expected over time unless other conditions change.

The second dimension is level of aggregation. We can look at effects on individual firms, effects on one industry or type of business within the town being bypassed, effects on total income or total employment within the town, effects on the whole county, or effects on the entire state. Each level of aggregation leads to a different perspective on the interests at stake.

The third dimension is data source or indicator of economic activity. Indicators used in this report include employment, payroll, sales-taxable sales, and business startups and failures. Many other indicators of economic activity can be used to study economic impacts. In particular, population, net migration, tax revenues, and total income are often used in impact studies. These additional indicators were not used in this report for two reasons: the data were not available at a sufficiently disaggregated level for a sufficient number of years, and some of these indicators were judged to be less sensitive to local economic disturbances than the chosen indicators.

The subsequent chapters look at different combinations of time, aggregation, and data source. The next chapter examines the long-term effects of a bypass on county retail sales. The following chapter examines the long-term effects of a bypass on aggregate employment and payroll for three types of business in the town that is bypassed. The third subsequent chapter examines short-term effects on aggregate employment and payroll for several types of business in towns that are bypassed, and also discusses the likely effects on tax revenues. The fourth subsequent chapter examines short-term effects on employment, payroll, and business startups and failures for individual firms in towns that are bypassed.

Many previous studies have examined effects of highways in general, and bypasses in particular, on local economies. We now turn to a brief summary of some of that literature.

General Impacts of Highways on Local Economies

Based on statistical and econometric studies, there is strong but not unequivocal evidence in the US that good local highways encourage local economic growth; or at a minimum, that good highways are one among several required factors that jointly warrant growth. For a somewhat critical review, see Rephann [1993]. For a general review of the causes and effects of highways and other infrastructure, see Gramlich [1994].

Several studies have emphasized the role of general public capital stocks in encouraging economic development [e.g. da Silva Costa *et al*, 1987; Eberts, 1986; Looney and Frederiksen, 1981]. These studies used public capital stocks as a measure of capital services. Since highways are a substantial part of public capital, these studies provide general support for a link between highways and economic growth.

Other studies have found a generally positive effect of interstate highways in particular on development, using cross section data (i.e. data from many locations for a single year) under a partial equilibrium or analysis of variance-type framework that does not control other factors very well [e.g., Briggs, 1983; Lichter and Fugitt, 1980]. On the other hand, Miller [1980] found no significant effect. Buffington and Burke [1991] used a more sophisticated, pooled times series-cross section approach to show significant positive effects on wages and employment from expenditures on bypass, radial, and loop highway improvements.

While these studies are useful, one would prefer a more general approach which estimated several or all of the major effects on growth simultaneously, within a well-defined economic model using large samples. Several recent studies have turned in this direction.

In one of these, a cross section study of rural Midwest counties, John, Batie and Norris [1988] found no significant effects of either interstate highways or higher education on county employment growth. However, they did find a significant positive effect from the percentage of commuters, which may act as a proxy for highways. Also, they apparently controlled for growth in earlier years, which would eliminate the effect of any highways or other variables which did not change over time. Therefore, we could interpret their results with respect to highways as being inconclusive.

Another cross section study [Hirschl and Summers, 1982] found positive effects on county employment growth from several variables. However, they found surprising negative effects from both interstate highway exits and also (in at least some variant regressions) from intergovernmental transfers. They used a prior assumption method to designate selected private industrial sectors as belonging entirely to the "export base."

Perhaps the most interesting study of this type is a cross section study of some 3000 US counties by Carlino and Mills [1987], which found positive growth effects from highways, but no important effects from some state policy-sensitive variables (unionization; industrial revenue bonds).

The Economic Model of Highway Impacts

It is very clear in this literature that the model of economic causality assumed by the researcher has critical effects on what variables are found to be significant, and even on the sign or direction of some measured effects. Therefore it is absolutely critical that the regressions should be grounded in a clearly defined economic model, and also that the economic model should be rooted in accepted economic theory. All of the reviewed models make at least some use of a particular class of theories known as export base theory.

Export-base and related models assume that the county or city economy and population are determined in a Keynesian sense by local exports to the rest of the world, or more generally by all sources of dollar flows into the locality. There may be some lag structure between occurrence of dollar flows across the regional boundary and the resulting local growth. Growth can also occur in anticipation of future dollar flows. In the case of bypasses, an important type of "export" is the sale of goods and services by locals to non-local persons passing through.

The generalized export concept consists of all sources of funds flowing into the county from the rest of the world. This includes public as well as private funds. Therefore non-local government employment in the county, as well as other non-local government purchases from county businesses, and also receipts of transfer payments, grants, and shared revenues, are directly a part of the county's export activity. Part of the earnings of these employees and businesses, as well as other receipts, recirculate in the county, leading to some additional county employment and income, with measurable positive multipliers. At the same time, state and federal taxes levied on county residents are leakages which reduce these multiplier effects.

Naive export base theory can be criticized because it seemingly ignores the effect that local conditions have on the amount of exports. However, the current level of exporting industry each year can usually be taken as predetermined by irreversible investment and exogenous demand conditions. Therefore an export-base model can be viewed as a simplification of Nijkamp's [1986] "regional development potential theory." In that theory, local activity is determined in the short term by exports; but exports are determined in the long term by local conditions and history.

Within this framework, state and national government activities such as highway provision have two conceptually different effects on the level of county exports. First, non-local government activity in the county is itself a direct export, bringing in dollars directly; for example, highway construction and maintenance brings construction dollars into the community. Second, non-local government services influence the growth of exporting industries in the private sector; for example, good highways encourage the growth of light industry or are at least a pre-condition for it.

However, in most counties the export sales of private firms constitute a much larger part of exports than do payments from non-local governments. Yet it is important to keep in mind the fact that highways and other government services affect the willingness of private firms to start up, expand, or relocate into the county. Highways also affect the propensity of existing firms to shrink, die off, or maintain their investment.

Bypasses in particular have at least five different effects on the export base. First, they may permanently affect impromptu spending by through traffic. Second, they may permanently encourage growth of basic industry. Third, bypass construction and maintenance expenditures may temporarily add to local sales and employment. Fourth, disruption and relocation of businesses at the time of construction may temporarily interrupt some business activity. Fifth, relocation of businesses nearer to the bypass may temporarily increase local demands for construction and real estate services.

Highways and other government services in the town or county consist of two very different parts. Services provided by non-local government can be viewed as exogenous (determined outside the model), or at least predetermined, while those provided by the county and other local governments should be assumed to be endogenous (i.e. they respond to other exports), or at least more rapidly varying in response to change in local conditions. Therefore local roads and other local government expenditures should appear as part of the local multipliers, not as part of the export base.

Thus the most important job of the economic model is to identify the various outside (exogenous or independent) influences which determine local growth, and clearly distinguish them from endogenous factors that are actually a part of growth. Regression studies of local activities on exogenous variables, including existence of the bypass, can then be used to show the causal influence of each exogenous variable.

A second task of the model is to explain which variables should come into the regression as levels, and which variables should come in as rates of change. In general, levels of local dollar flows are affected by levels of exports, i.e. levels of predetermined dollar flows into the community. (This is just one way to describe the multiplier effect). But rates of change (as well as levels) of the incoming dollar flows are affected by levels of government services, such as highways, in the community -- in other words, highways affect private investment, which leads to change in the level of exports.

Each of the next four chapters uses somewhat different specifications for the particular statistical studies, and these particular models are described in the individual chapters. But all of the models are rooted in this generalized export-base approach.

Methodology

From the impact study literature we can draw two other conclusions about the statistical methodology that is appropriate for studying economic effects on growth.

First, it is important to adopt a general approach which estimates several or all major effects on the local economy, simultaneously within a well-defined economic model. At a minimum, the approach should try to control for other economic effects that are not being estimated.

Second, it is important when possible to adopt a pooled time series - cross section approach (i.e. one which follows multiple cities or counties over time.) This approach has three major statistical advantages over other approaches:

1. it provides for a relatively large sample size at an affordable cost;

2. it allows for simultaneous controls on the year of data points, and also on the particular city or county; and

3. it allows the possibility of simultaneous control of spatial and temporal auto-correlation (i.e., persistent but temporary influences across time or across locations that were not directly measured.)

Two of the subsequent chapters use regression studies that generally follow these guidelines. The third and fourth chapters, which focus on effects on individual types of firms, rely on an event study approach because that is what the data can support. As we will see, previous bypass studies have generally not lived up to these standards.

Previous Studies of Bypass Impacts

While there is a substantial amount of literature on the economic impact of a highway bypass, little of it is in professionally reviewed journals. Moreover, the majority of the literature is in the form of case studies. Case studies can show a great deal of detail not available in a statistical data set, but they do have severe limitations. For an informal review *cum* compendium, see Iowa Department of Transportation [1992].

Apparently, no case studies of bypasses have been performed *ex post* (i.e. after the fact) in Kansas. However, Cook, Flatt and Strobel and Wilbur Smith Associates [1991] performed a *ex ante* (predictive) study on the effects of the planned bypass at Sedan,

Kansas. They found from surveys that only a small percentage of retail trade in Sedan was derived from unscheduled stops by through traffic.

Case studies in other states show a general picture in which local retail businesses may be set back somewhat during the construction of the bypass, but there is a recovery afterward, and the local economy as a whole is not noticeably hurt by the bypass. However, there can be significant distributional effects within the community, causing some of the service businesses to close down in the central city and other service firms to start up near the highway interchange. While a coherent picture does begin to emerge when one examines the mass of case studies informally, it would be helpful to have a more careful statistical review or meta-analysis of the case studies.

In the best earlier analysis of this type, Horwood, Zellner, and Ludwig [1965, Chapter 2] reviewed 24 studies of changes in retail sales caused by bypasses in 72 towns. Data for 36 of these towns were meaningful because information on a control town was included. Most of the studies compared two years prior to the bypass construction with the two years following the construction. The review found distinctly different impacts between towns under 5,000 and over 5,000 in population. Total retail sales in the larger towns actually tended to grow faster with a bypass than without it. The smaller towns tended to continue growing after the bypass was constructed, but growth was less rapid than in control towns. Restaurants, cafes, and bars were most negatively impacted by the bypasses. Generally, travel-related businesses were somewhat more negatively impacted, or less positively benefitted, than non-travel-related retail and service businesses.

A briefer review by the Federal Highway Administration [1972, p. 11] is somewhat more upbeat about travel-related industry, concluding that "Tabulations of bypass-study findings have so far failed to reveal any direct or consistent relationship between [retail] business activity and traffic change in bypassed areas." A very recent review by Liff *et al* [1996] reaches similar conclusions to the earlier review and contains an extensive bibliography and summary data from many individual bypass studies.

From a technical point of view, the best primary studies of bypasses have been those conducted recently on a sample of Texas towns and cities by researchers at the University of Austin [Andersen *et al.* 1993 and citations therein]. They employed several different techniques, but their most persuasive models are multiple regressions using pooled time-series - cross-section data. Even those models were limited, however, by the use of somewhat unsophisticated statistical regression methods.[9] A key variable they employ is the total traffic counts on and off the bypass. (In the studies in this report, we will attempt to improve on that variable by partitioning traffic counts into local traffic and through traffic.) Their general conclusion is that Texas bypasses have no detectable effects on larger cities; on cities under 25,000 they have a small but significantly negative effect on each type of retail sales.

Conclusion

The previous bypass studies we reviewed are helpful and generally consistent, but the total weight of evidence they provide is still rather thin in several respects. Among the studies that are technically most persuasive, only the Texas study is recent, and that study is confined to a single state. None of the studies have exploited Unemployment Compensation Insurance (UI) data. None of the studies have used the more sophisticated versions of pooled data set regressions. None of the regression studies have examined births and deaths of firms. Moreover, there are no Kansas-specific controlled studies at all. For these reasons we have undertaken the studies described in the subsequent chapters.

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[9] For example, they did not control in a systematic way for fixed effects related to cities or years, nor did they use any other of the special regression techniques that have been developed to exploit the rich information available in pooled data sets. In addition, they use dummy variables for certain cities selected because they greatly differ from other cities, in a manner which largely removes those cities from the bypass sample, and could bias the regression. Perhaps most importantly, their general approach uses various specification searches followed by final estimates (i.e. "pre-test estimators"). They fail to point out that this technique increases the probability of finding spurious correlations and leads to overstatement of the significance level of any relationship they do uncover [Leamer, 1978]. Since they do not describe their details of the specification search, the reader is unable to evaluate the true statistical significance of their results. Because of these sorts of problems, a sensitivity analysis is generally preferable to a specification search.

5 Economic Impacts II: Long-Term Effects on Taxable Consumption of Counties

Introduction

This chapter examines the long-term effect of a bypass on aggregate business conditions in the county as a whole. The data set employed is retail sales by industry category, derived from Kansas sales tax information. The method involves panel data regression of retail sales on population and income and dummy variables for presence or absence of the bypass.

In theoretic terms, a bypass might be expected to have positive effects on retail trade in the county as a whole even if it had negative effects on the particular town being bypassed. There are two channels of influence that lead to this conclusion. First, retail business is largely a "zero-sum game": the total volume of retail trade is fixed by macroeconomic conditions, so that retailing lost in one location is gained in another. If some retail business is lost in the town being bypassed, then there should be an offsetting increase in retail business in other locations. If the county has retail centers outside of the town being bypassed, then much of that offsetting increase may fall in the same county.

Second, the bypass improves the highways in the county, speeding up commercial transport and making the county more attractive to basic industries. As a result of improved transportation, new industries may locate in the county or old industries may be retained or may expand. As a result of an increased industrial base, there may be higher income and higher population, leading to more retail sales.

In most counties, however, the total effects of a bypass are expected to be small in comparison to overall retail sales in the county. Relocation of retail trade is expected to have small effects on the county, because previous research suggests that bypasses have small effects on retail trade in the town, and because offsetting effects are present at other locations in the county. Relocation of basic industry is expected to be small because previous research has shown that highways are only one among many factors that affect economic development and because the bypass is only a small part of the county highway system.

The Taxable Sales Data Set

Data on taxable sales are available as a by-product of the Kansas sales tax. The data are available by quarter by county, and can be additionally disaggregated by Standard Industrial Classification (SIC) code. However, the disaggregated data are subject to suppression for confidentiality reasons in cases where the number of firms is too small in a given cell of the tabulation. In this study, we used data for total retail sales by county by year. At this level of aggregation, there is no data suppression.

Because they are derived from administrative records for tax collection purposes, the taxable sales data have certain limitations. The most important limitation for our purpose is that "retail sales" would only be an approximate description of what sales are included in the data. A more exact description would be "sales that are subject to the Kansas Sales Tax and are reported in a timely manner." Moreover, the definition of the tax base changed in at least some respects during many different Kansas legislative sessions in the time period under study. However, the changes were generally small, and sales-taxable sales is a reasonably constant concept over the course of the time series used in this chapter.

Another problem is that some sales tax collections may be reported late and the times that other collections are reported may have a varying relationship to the quarters of the year. However, most collections are obtained within the same year as the liabilities are incurred, so that this problem is not severe at the annual level of aggregation. In addition, other sales tax liabilities may be evaded entirely, either through negligence or intent.

These idiosyncrasies of the data are potentially significant, however, only if they are relatively large and change materially over time or across counties. Even then they are actually significant only if they can't be controlled statistically. The regression method we used for this chapter involved dummy variables for each county and for each year. This method of control removes the effect of any error which is constant for a given county and/or a given year. Any remaining errors of this type are probably uncorrelated with the bypasses. Since the data set was rather large, uncorrelated errors in the dependent variable are not very consequential.

The term "retail sales" is something of a misnomer for another reason: the tax covers some non-retailing services, while omitting some classes of retail sales. This conceptual variation is unimportant for our purposes. The goal of this chapter is to examine the effect of bypasses on consumption activity in the county using whatever consumption index happens to be available. Although some categories of consumption are omitted from the data, all major categories of consumption are known to be positively correlated with each other. While an even broader index of consumption than sales taxable consumption would be somewhat better, what we have available is adequate.

The Regression Models

We used a variety of regression models based on the well-established idea that retail sales in a county are largely determined by income and population in that county. Of course, some sales are also related to traffic traveling to the county and traffic traveling through the county, independent of local income and population. Bypasses could affect sales of this type. Therefore our strategy was to test whether dummy variables representing the existence of a bypass have additional explanatory power in the regression equation.

Because the regressions included dummy variables for each county, the mere continued existence of a bypass in the county would have no detectable effect on the regression. That is, if the variable for the bypass always had the value of 1 for a given county, then the effect of that variable could not be distinguished from the county dummy (which also always equals 1 for the given county). Therefore, bypasses only affect the regression in counties where the variable changes over time; i.e. in counties where a bypass is built during the time period of the regression. There were 20 such counties in the sample. Therefore the regression analysis basically looks for the average change in sales that occurs after a bypass is built. The other 85 counties in the equation merely serve to standardize the effects of the consumption coefficients for income and population, plus the dummies that control for time.

We also considered an alternative hypothesis about the manner in which sales depend on existence of the bypass. It is possible that bypasses have a bigger effect on sales in larger counties than in smaller counties. This would be the case, for example, if local traffic on the bypass depended on the size of the county. To test this hypothesis we interacted the dummy variable for the bypass with county income. (In other words, the bypass variable was zero before the bypass was built, and equaled county income after the bypass was built.)

In regression studies of this kind, the apparent outcome is sometimes sensitive to details of how the regression model is set up. To test for that problem, we used a sensitivity analysis, examining a variety of models. Models could vary along several dimensions, as follows.

1. The bypass variable could be either a dummy variable, or a dummy interacted with population or income in the county.

2. Possible variant error structures included temporal autoregressive models, moving average models, and error components models.

3. The largest 5 counties could be omitted from the regression or included.

4. The main explanatory variable could be any combination of county population and/or income.

Results

Selected regression statistics are shown in Table 5.1. In all cases, bypasses were estimated to have an economically small effect on county sales. In terms of point estimates of the coefficient, the amount of sales affected by a typical bypass was less than 1% of total county sales. The error bands, as measured by the standard errors of the bypass coefficients, are sufficiently tight so that this conclusion can be viewed as reliable as well as robust.

In the dummy variable models, the measured effects of bypasses were generally negative but not statistically significant (p=0.05). In other words, if these models are appropriate then the data should be viewed as showing no discernable effect in either direction. While there could conceivably be some tiny effect that might show up with a larger sample, we have little information about whether bypasses help or hurt the county economy.

For the interactive models, the results are different. The effects of bypasses, while small, are sometimes measured to be positive and significant (p=0.05). That is, bypasses were estimated as increasing county sales by a small but definitely positive amount.

Conclusion

As expected, the effects of bypasses on county consumption sales are so small that they should not be viewed as important policy concerns. Counties should decide to support or oppose rural bypasses based on transportation needs and other considerations, and not based on their effect on county-wide retail sales and services.

If there is a small effect from bypasses, its sign remains ambiguous given the different results for the two different bypass variables. A possible interpretation consistent with the data is that bypasses have small positive effects in larger counties, and small negative effects in smaller counties. In the dummy variable regressions, the two types of effects cancel out. In the interactive variable regressions, the positive effects in larger counties dominate because they get a higher weight.

Because we used a reasonably long data series (21 years), this result applies mainly to the long term. Chapter 7 will consider some possible transitory effects of bypasses, though from the point of view of towns rather than counties. From the size of the effects measured in that Chapter as well as this, we would infer that any transitory effects at the level of the county are also likely to be quite small.

The small size of the effects on a given county does not imply anything one way or the other about the relative size of effects on the bypassed town. That is, negative effects in the town might be offset by positive effects in other parts of the county. Also, an effect that seems small from the point of view of the county might seem a bit larger from the point of view of a small town. Therefore the long-term effects of bypasses on individual towns could still be important. The next chapter will examine this question in more detail.

Table 5.1
Regressions of Retail Sales on County Population and Income

Independent Variable	*Statistical model variant* **1** **Fuller-Batese (random effects)**	**2** **Parks (fixed effects)**	**3** **Da Silva (moving average, 7 years lag)**
Intercept	14.612 (6.887)	4.611 (3.062)	14.426 (7.132)
Total personal income	0.3638 (0.0018)	0.4068 (0.0037)	0.3623 (0.0002)
Bypass dummy weighted by population	-3.162 (8.722)	-9.741 (14.369)	7.012 (1.073)
R-squared	0.802	0.384	0.792

Notes: Personal income and retail sales are measured in units of $1. Population is measured in units of 1 person. Numbers in parentheses are standard errors. Data set contains observations for 21 years by 105 counties. Coefficients for year and county (and lag structure, in model 3) are omitted.

Source: Calculated by IPPBR

6 Economic Impacts III: Long-Term Effects on Payrolls and Employment of Towns[10]

Introduction

This chapter describes the long-term or equilibrium effect of bypasses on aggregate economic activity in small towns. The method of study was regression analysis. We also performed a sensitivity analysis using a variety of variant models.

Economic activity in the towns was measured by total employment or total payroll taken from Unemployment Compensation Insurance (UI) records. These data can be broken out by type of business as measured by Standard Industrial Classification (SIC) codes.

The general approach in these studies related economic activity to different types of traffic: local traffic, through traffic not on a bypass, through traffic on a bypass, and through traffic on Interstate highways. We used several variant models in order to test the sensitivity of the outcomes to details of the model. In one series of models, retail activity was assumed to be strictly caused by local payroll plus activities proxied by the various types of traffic. In another series of models, traffic of each type was treated as not necessarily a strict cause of economic activity, but rather as a symptom of economic activity which can be used to help predict that activity.

In a third series of models, we omitted traffic from the model entirely. These models treated retail activity as being caused by population or income, and by non-retail payroll, with the presence of a bypass treated as a dummy variable.

All of the models were organized around panel data (i.e. observations of different towns, with data from multiple years for each town.) Observations were available for 7 years for some 300 towns, or some 2000 observations (although this number varies because of sample selection choices; for example, some estimates omitted towns with zero economic activity in some categories.) 23 towns had bypasses in at least some years, leading to around 140 observations that include effects of bypasses. This sample was large enough so that the results can be viewed as statistically reliable estimates of the underlying models. The results should be viewed as measuring "long-term" effects of bypasses because most of the bypasses had been in place for several years prior to the collection of data.

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[10] Some of the results described in this chapter are also discussed in Burress [1996].

The UI Data

The available unemployment compensation data consisted of all detailed firm-level records for 1988 through 1994. UI master files are generated by quarters; we selected out one quarter from each year (always the first quarter). The data include the employment of the firm for each month, and the total payroll of the firm for the quarter. These data are likely to be very accurate because they constitute the tax base for the UI tax.

The record also contains the SIC code of the firm and its name and address. The SIC code is reported with reasonable accuracy at the one-digit level, because the UI tax rate depends on the one-digit SIC category of the firm during the first few quarters after the firm is first enrolled on the tax rolls. However, after that, firms are experience-rated and SIC codes are not used except in statistical reports. Changes in SIC codes are not audited with any care, and there may be errors in this data field.

This data set has other limitations that should be noted. First, the following types of data are excluded from the administrative records by design:

1. employment of persons not subject to the UI tax. In economic terms, the most important exempt persons are partners and sole proprietors.

2. wages and salaries in excess of $8,000 per quarter for a given employee (which defines the UI tax base).

A second limitation is that the employment data merely count the total number of workers on the payroll. The data do not distinguish full time workers from part time workers. Therefore increases in work hours per worker do not show up in these data. A third limitation is that some records could not be classified by town or city of the business because the business was mobile or for other reasons took place in a county different from the reporting county. However, we believe that none of these problems are sufficiently severe as to affect the validity of the major conclusions of this report.

Categories of Business

For the statistical studies of this chapter, the data were aggregated by town or city and by three detailed business categories (determined by the SIC code) plus two larger categories. The categories of business are:

retail, travel-related
retail, non-travel-related
all-other
total retail
total

These categories are defined by SIC code group in Table 6.1

Types of Traffic

Using the KDOT traffic count data as a source, estimates were made of four different types of traffic for each town for each of the seven years. The types of traffic are:

1. local traffic
2. through traffic on bypasses
3. through traffic on Interstate Highways
4. other through traffic (e.g. downtown)

However, in the regression studies, the total of all three kinds of through traffic was used as a variable in place of the "other" category of through traffic. The purpose of this specification was to force the coefficients on bypass traffic and Interstate Highway traffic to represent *differences* between effects of traffic with and without a bypass or Interstate Highway. If effects of through traffic on bypasses are identical to effects of downtown through traffic, then the bypass coefficient will be zero; and similarly for Interstate Highways. Therefore tests of the significance of these coefficients are tests of whether diverting traffic onto bypasses or Interstate Highways affects the local economy, when total traffic is held constant.

The Size of Towns

In most of the regressions used in this chapter, observations were excluded for cities and towns that did not have positive employment and payroll in each year in each of the three major categories (travel-related retail, non-travel-related retail, and other). Observations were also excluded for the five largest counties. In other words, we are estimating effects on small towns and small cities, but not the effects on very tiny towns, larger cities, and urban areas that fall in Standard Metropolitan Statistical Areas (SMSAs).

A Family of Regression Models: Traffic and Economic Activity

The regression models used in this section look for a best predictor of economic activity for each town, using the four types of traffic as prediction variables. The models were estimated using a variance components model [described e.g. in Hsaio, 1986, pp. 32-39], using the Fuller-Batese algorithm provided in the SAS programming language. The generally maintained hypothesis is the following:

Any long-term effect of building a bypass should show up as a *difference* in the measured effect on business activity from through traffic traveling on bypasses, and the measured effect from through traffic that flows directly through town. The difference of these effects is viewed as an estimator of the effect of building a bypass.

So as to perform a sensitivity analysis, models were estimated with variations along a number of different dimensions.

First, the dependent variable ranged over the different measures of activity (payroll or employment; travel-related retail, non-travel-related retail, total retail, other, total.) Note that the effect of bypasses can be different for each class of industry. Retail business of both types (travel-related and non-travel-related) might be discouraged by a bypass because it reduces exposure of downtown business to through traffic. The same is true for some service businesses that are included in the "all-other" category. However, business in these nonbasic businesses might also be encouraged if the bypass encouraged development of basic industry: growth in basic industry leads to growth in population and income, which then leads to growth in retail and other non-basic expenditures in the town. In addition, travel-related businesses could be encouraged if locations by the bypass interchange proved to be especially attractive for gas stations and motels. Moreover, a given channel of influence might have different intensities for different types of business.

Second, the variables for "all-other" income and/or "all-other" employment could either be included or excluded as independent control variables for the model. With these control variables excluded, the four traffic variables should be interpreted as general symptoms of economic activity in the town rather than as purely causal variables. In other words, in models with nothing but traffic as independent variables, traffic can both cause economic activity and be caused by it.

This distinction is most obvious in the case of local traffic. Thus, local traffic is caused by all forms of economic activity, because workers and customers and suppliers have to reach the business. On the other hand, local traffic also causes some of the non-basic activity in the town. For example, a supplier on a trip to a business engaging in a basic industry might also stop at a retail business because it happened to be close by.

Through traffic variables, on the other hand, would generally be interpreted as causal variables rather than as caused variables. However, there is at least one factor that works in the opposite direction. A high volume of through traffic is a proxy for location on a major or important route. But location on a major route is a factor that encourages development of basic industry. Therefore, high through traffic may be correlated with development of basic industry without causing it.

When the control variables for "all-other" activity are included in the model, then the interpretation of the traffic variables changes somewhat. This is especially true in the case of the models for predicting nonbasic activity such as retailing. Retail activity is mainly caused by three variables: local income, which leads local consumers to spend locally; special trips of non-local residents who come to the town partly or solely for shopping; and exposure to through traffic. Local traffic on the highways is a joint indicator for local income and for special shopping trips. With "all-other" or total employment and payroll variables in the equation, the through traffic variables should no longer be serving as proxies for the location of basic industry. Instead, they should be serving purely as indicators of unscheduled stops that can lead to local retail sales. Moreover, local traffic should be serving purely as an indicator of special shopping trips.

Other variations in these models included the following:

1. In the main specifications, observations were excluded for cities in the 5 largest counties of Kansas and for very small towns, as described above. In variant runs, observations were included for the small and/or large towns.

2. Most models were estimated in both linear and log-linear forms.

3. In some variant models, the constant terms were omitted. In a linear model, this restriction assumes that the economic activity is fully explained by the traffic and control variables, and also assumes that economic activity is strictly proportional to traffic.

4. In some models, control variables included total payroll and/or total employment instead of "all-other" payroll or employment.

Regression Results: Traffic and Economic Activity

A selection of results is presented in Table 6.2. The following discussion refers to these results as well as additional results contained in Technical Appendix 6.1. We remind the reader that the coefficients for bypasses and Interstate Highways represent *additions to or subtractions from* the coefficient for all through traffic. In other words, to infer the net effect of bypass traffic on the town, we must add the coefficient for the bypass to the coefficient for all through traffic. If the bypass coefficient is zero, that means that diverting through traffic from the town onto the bypass has no effect on the town.

Effects of bypasses: point estimates

In most of the models, the estimated effects of diverting through traffic onto a bypass are not statistically significant. In many cases the sign of the effect is ambiguous, meaning that a small change in specification can change the sign. Ambiguity of the sign is not surprising, because the coefficients are often highly insignificant, i.e. small in comparison to their standard errors.

The estimated effects of bypasses also are not very significant in economic terms. Except in the log-linear models discussed below, the effect on the town from traffic traveling on the bypass is estimated to be within 15% of the effect of traffic traveling through the town. The two effects are even closer for some categories of business.

In about half of the models, bypasses appear to help rather than hurt the economy -- that is, the sign of the effect for bypasses is positive. In all cases showing negative effects on the local economy, the estimated effects due to bypasses are both economically small and statistically insignificant. In the most extremely negative models, diverting through traffic from the town to the bypass was estimated to cut the beneficial effects of through traffic on total economic activity by about 10% (though it was not statistically significant). In most models, the measured loss was either much less than 10%, or else there was a gain rather than a loss. The general implication of this finding is that bypasses do not significantly hurt employment and payroll in small towns.

The log-linear models give rather different results than the other models. In the log-linear models, the effect of a bypass is estimated to be strongly positive. The estimate is both statistically significant and economically substantial. However, this model is not very persuasive because the specification is suspect. Linear models make the common-sense assumption that units of traffic have independent effects on the town economy. The log-linear model assumes instead that different kinds of traffic interact with each other multiplicatively. In particular, through traffic would be assumed to have almost no effect in a town with little local traffic but would have very large effects in towns with large amounts of local traffic. Since this model isn't very realistic, its results should not be viewed as showing that bypasses have strong positive effects on the town economy. What this model does show, however, is that large changes in the underlying functional relationships are not likely to lead to large negative estimates for the effect of bypasses. In other words, it helps demonstrate robustness of the conclusion that there are probably no important negative effects on towns from bypasses.

Effects of bypasses: uncertainties

The previous statements refer to the "point estimates" of coefficients, as reported in Tables 6.2 and 6.3. These values are the most likely value for the true coefficients, given these data. However, the standard errors on these coefficients are uncomfortably large. That is, the data do not rule out a fairly wide range of values that are also reasonably likely values for the true coefficients. This leaves us with a problem of interpretation.

One way to look at what this means is to calculate a worst-case confidence interval. In other words, we can calculate a range of values that we are highly confident will contain the true value; then we look at the lower bound of the range as the worst case that is likely to be true. In this kind of calculation, it is conventional to use a "95% confidence interval," meaning a band of values that we believe the true value would fall within for about 95% of similar statistical experiments. In the cases of bypasses, the worst case analysis for any one regression implies that bypasses reduce the benefit from traffic by less than 50% or so (with 95% confidence).[11] This worst case is rather pessimistic about the effects of bypasses on towns; an event as large as a 50% drop in sales to through traffic would be economically significant for some towns.

However, looking at the entire collection of models leads us towards a more optimistic conclusion. That is, every one of the different regression models led to point estimates that imply that bypasses had only small economic effects. These models were based on three independent categories of business (travel-related, non-travel-related retail, all-other), plus subtotals and totals, with variant models and indicators for each category. While any one of these regression models has a wide error band, the fact that all models lead to about the same conclusion should make us reasonably confident of that conclusion.

Effects of Interstate Highways

In all of the models except the log-linear models, through traffic on Interstate Highways has around half as large an effect on the local economy as through traffic through town, or even less. In other words, according to this model diverting *existing* traffic from the downtown onto an Interstate Highway would have substantial negative effects on sales to through traffic.[12] This result is both economically significant and highly statistically significant. This result is also strikingly different than the result for bypasses.

If we viewed an Interstate Highway bypass as just another type of bypass, then the contrast in outcomes would be surprising. But traffic on Interstate Highways has different characteristics from ordinary bypass traffic on US and state highways. Traffic moves at higher speeds on Interstate Highways, which implies that greater distances are traveled between stops. Traffic on Interstate Highways travels longer total distances on average, which also may increase the distance between stops. Traffic is generally heavier on Interstate Highways, implying that the Interstate Highway as whole will attract more travel-related businesses, leading to stiffer competition at other towns along the route. It is known that retail businesses at Interstate Highway access points enjoy some economic gains from agglomeration, i.e. gains from grouping together at selected access points; some small towns may not be able to support a large enough agglomeration of businesses to be

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[11] That is, on each regression, we calculated the statistic (bypass coefficient - 1.65*(bypass standard error)/(through traffic coefficient). That coefficient is always around -.5, except for log-linear models. (In log-linear models it is around 0.) The factor of 1.65 in the statistic is based on 95% confidence level for a one-tailed t-test.

[12] This is very different from saying that Interstate Highways have negative effects on towns. While building an Interstate Highway near a town does divert some existing traffic away from the town, it also attracts a lot of new traffic near the town. The two effects work in opposite directions. As it happens, Norman Clifford and I have performed regressions not reported in this study which imply, on net, that building an Interstate Highway near a small town has a strong positive influence on the size of the local economy.

competitive. Therefore it is believable that, on a per-vehicle basis, Interstate Highway traffic is much less helpful to a small town than bypass traffic.

We should emphasize the point that a small town ordinarily should not fear the construction of an Interstate Highway near the town. While the Interstate Highway may divert some existing traffic away from downtown businesses, the Interstate Highway also greatly improves highway access to the town and greatly expands the volume of traffic traveling near the town. If the Interstate Highway merely doubled the amount of traffic passing near the town, then the effects from new traffic would completely cancel the effects from reduced sales per vehicle for the existing traffic. In reality, average Interstate Highway have traffic loads in Kansas that are much more than double the average loads on state and US highways.

Conclusion

These data provide evidence that bypasses have not had appreciably negative long-term effects on the aggregate employment and payroll of small towns in Kansas. The estimated effects are not statistically different from no effect at all. However, the error bounds are not sufficiently tight to absolutely reject the possibility of noticeable negative or positive effects from bypasses. But with high confidence we can reject the possibility that bypasses have had catastrophic effects on small towns in Kansas.

This finding may imply that businesses are able to adapt to any negative changes caused by a bypass. In particular, businesses that depend on through traffic may move to the bypass access points where they can be more visible. Or it may imply that bypasses create positive business opportunities which offset the negative impacts. In particular, improved transportation may make the town more attractive to basic industry, or improved location at access points may make travel-related businesses more attractive to passers-by.

This conclusion does not imply anything one way or the other about the effects of a new bypass on particular firms that already exist in the community. It also does not imply anything about short-term effects on the aggregate town economy at the time the bypass is constructed. If individual firms were either helped or hurt by construction of a bypass, or if labor market conditions were temporarily impacted, that would not show up in the type of regression studied in this chapter. What the regressions do show is that, if any firms do happen to lose business because of the bypass, then on average either the lost business will be only temporary, or else the lost business will eventually be replaced by business gained by new firms attracted by the bypass. And of course, this conclusion applies only on average across all towns; the situation could be different for individual towns. In the next two chapters we will look more closely at what has actually happened to a sample of towns and individual firms in Kansas at the point in time when a new bypass was opened.

Table 6.1
Categories and Sub-Categories of Businesses
Definitions for Chapter 6-8

- TOTAL
  - Total retail
    - Retail, travel-related - SIC 58, 70,72,75,76
    - Retail, non-travel-related - SIC 50-57, 7396, 8042
  - All-other

Table 6.2A
Effect of Traffic Flows on Retail Trade Employment

Independent Variable	Total Retail	Travel-Related Retail	Non-travel-Related Retail
		Dependent Variable	
Intercept	-365.0779	-59.1055	-238.0023
	(211.5983)	(23.7826)	(150.9437)
Local traffic in towns	0.2037	0.0163	0.1482
	(0.0754)	(0.0084)	(0.0542)
All through traffic	0.4960	0.0573	0.3588
	(0.0799)	(0.0089)	(0.0575)
Through traffic on bypasses	-0.0533	0.0023	-0.0466
	(0.1106)	(0.0124)	(0.0797)
Through traffic on I-roads	-0.2975	-0.0282	-0.2259
	(0.0819)	(0.0092)	(0.0590)
R-squared	0.257	0.264	0.254
Ratios:			
Bypass effect/effect of all through traffic	-0.107	0.0398	-0.123
I-road effect/ effect of all through traffic	-0.600	-0.493	-0.630

Note: Traffic is measured as average vehicles per day. Employment is measured as total number of employees in the specified retail trade category. Numbers in parentheses are standard errors.

Source: Calculated by IPPBR

Table 6.2B
Effect of Traffic Flows on Retail Trade Payroll

Independent Variable	*Dependent Variable* Total Retail	Travel-Related Retail	Non-travel-Related Retail
Intercept	-1,469,978 (1,044,156)	-145,003 (56,367)	-980,406 (863,873)
Local traffic in towns	743.8693 (395.5657)	33.2765 (20.0229)	560.6670 (333.3759)
All through traffic	2,212.1062 (417.8709)	122.0405 (21.1708)	1,756.4350 (352.1569)
Through traffic on bypasses	-304.2907 (576.3278)	5.0819 (29.2229)	-252.1023 (485.6728)
Through traffic on I-roads	-1,349.5109 (426.9721)	-55.6478 (21.6472)	-1,115.1566 (359.8130)
R-squared	0.084	0.210	0.060
Ratios:			
Bypass effect/ effect of all through traffic	-0.138	0.0416	-0.144
I-road effect/ effect of all through traffic	-0.610	-0.460	-0.635

Note: Traffic is measured as average vehicles per day. Payroll is measured as total dollar payments to employees for the first quarter of the year (3 months) in the specific retail trade categories. Numbers in parentheses are standard errors.

Source: Calculated by IPPBR

Table 6.2C
Effect of Traffic Flows on Total Employment
Results for Alternative Models

Independent Variable	Model 1 -	Model 2 -	*Model Variant* Model 3 (includes very small towns)	Model 4 (log-linear)
Intercept	-128.4509 (62.3633)	-	-122.7755 (38.6227)	-0.8988 (0.4458)
Local traffic in towns	0.0533 (0.0218)	0.0485 (0.0217)	0.0544 (0.0163)	0.7422 (0.0544)
All through traffic	0.1391 (0.0231)	0.1128 (0.0193)	0.1233 (0.0166)	0.0504 (0.0113)
Through traffic on bypasses	-0.0076 (0.0319)	0.0036 (0.0315)	0.0066 (0.0254)	0.0149 (0.0064)
Through traffic on I-roads	-0.0727 (0.0236)	-0.0527 (0.02157)	-0.0702 (0.0170)	0.0081 (0.0050)
Control variables:				
Employment of all other	-	-	-	0.0769 (0.0116)
R-squared	0.251	0.236	0.250	0.424
Ratios:				
Bypass effect/effect of all through traffic	-0.0547	0.0317	0.0533	0.296
I-road effect/ effect of all through traffic	-0.523	-0.467	-0.570	0.160

Note: Traffic is measured as average vehicles per day. Employment is measured as total number of employees in the specified industry. Numbers in parentheses are standard errors.

Source: Calculated by IPPBR

Table 6.2D
Effect of Traffic Flows on Total Payroll
Results for Alternative Models

Independent Variable	Model 1	*Model Variant* Model 2	Model 3 (log-linear)
	-	-	
Intercept	-471,069 (227,007)	-	6.3546 (0.5033)
Local traffic in towns	205.7843 (80.1995)	187.8856 (79.7996)	0.0870 (0.0133)
Through traffic in towns	433.2215 (84.9281)	334.8256 (70.5164)	0.0547 (0.0127)
Traffic on bypasses	-41.8070 (117.3980)	0.0427 (115.7474)	0.0118 (0.0072)
Traffic on I-roads	-218.3559 (86.9312)	-143.4595 (79.1493)	0.0075 (0.0056)
Control variables:			
Employment of all other	-	-	0.8036 (0.0611)
R-squared	0.208	0.194	0.420
Ratios:			
Bypass effect/ effect of all through traffic	-0.0965	0.000128	0.215
I-road effect/ effect of all through traffic	-0.504	-0.428	0.137

Note: Traffic is measured as average vehicles per day. Payroll is measured as total dollar payments to employees for the first quarter of the year (3 months) in the specific retail trade categories. Numbers in parentheses are standard errors.

Source: Calculated by IPPBR

Table 6.3A
Effect of Bypass on Travel-Related Payroll
Models with Bypass Dummy Variables

Independent Variable	Model 1	*Model Variant* Model 2	Model 3
Intercept	-7,501	-88,796	-413,533
	(377,008)	(378,966)	(681,494)
Total retail payroll	0.052	0.0526	-
	(0.000408)	(0.000410)	-
Total payroll except retail	-	0.0538	-
	-	(0.000404)	-
Bypass times total payroll	0.0283	-	0.0182
	(0.006)	-	(0.0104)
Bypass dummy	-	127,844	-
	-	(97,525.4)	-
Adj. R-square.	0.910	0.890	0.891

Note: Dummy variables for time and town are omitted. Numbers in parentheses are standard errors.

Source: Calculated by IPPBR

Table 6.3B
Effect of Bypass on Travel-Related Employment
Models with Bypass Dummy Variables

Independent Variable	Model 1	*Model Variant* Model 2	Model 3
Intercept	16.9	21.4	-121
	(43.4)	(21.7)	(240)
Total retail payroll/1000	0.0195	-	-
	(0.00014)	-	-
Total payroll except retail/1000	-	-	0.0204
	-	-	(0.00014)
Total retail employment	-	0.408	-
	-	(0.00143)	-
Bypass times total payroll/1000	0.0147	-	0.0126
	(0.00209)	-	(0.0037)
Bypass dummy	-	13.6	-
	-	(17.0)	-
Adj. R-square	0.902	0.976	0.904

Note: Dummy variables for time and town are omitted. Numbers in parentheses are standard errors.

Source: Calculated by IPPBR

7 Economic Impacts IV: Short-Term and Transitory Aggregate Effects on Towns

Introduction

This chapter focuses on the *short-term* events that occurred in Kansas towns when a bypass was first constructed, and focuses on aggregate employment and payroll in the town. In other words, we will treat each economic sector as if it were a single firm, with all gains and losses netted out. We looked at these events in Kansas using the unemployment compensation insurance (UI) data set. This data allowed us to examine changes in payroll and employment of individual firms by year, and identifies the type of firm; the data set is described more fully in Chapter 6 above. As in Chapter 6, these data were aggregated into town level totals by industry category. (In the next chapter, these data will be studied disaggregated to the level of individual firms.)

This chapter raises questions such as: Was there any stress on workers in particular industries? Was there any stress on workers in the town as a whole? That is, did the bypass lead to a reduction in total employment or payroll? Also, should we expect a bypass to cause a reduction in local tax revenues?

The studies in this chapter used an "event study" approach. That means that we studied the sequence of events that follow a triggering event, using data that are characterized by elapsed time before or after the triggering event. This approach allowed us to merge together data from different bypasses constructed in different years.

Because the sample of usable bypasses was small, the available data for this as well as the next chapter were much sparser than were the data used in the previous chapters. Consequently, we had to work harder to get any answers, and the answers we did find were more qualified, as compared with previous chapters.

The Triggering Event

This study assumed that the triggering event for economic changes in the town is the actual construction of the bypass. It might be objected that the "real" triggering event for economic changes in the town is the *announcement* that the bypass will be constructed. Once the bypass is announced, businesses could conceivably begin to change locations in anticipation of changes in traffic patterns; or they could conceivably begin to go out of business in a controlled manner so as to avoid an abrupt failure at the time of construction.

Indeed, certain kinds of economic changes probably do occur rapidly after a bypass is announced. Most significantly, land values are likely to rise rapidly at locations near the planned access points [Mellinger, 1995, gives an example]. However, previous studies have not demonstrated any anticipatory changes in the pattern of downtown business. The impression one receives from interviews and case studies is that businesses typically take a "wait and see" attitude toward the effects of a bypass [e.g. Finger, 1995]. Moreover, this attitude would appear to be rationally justified -- that is, previous research suggests that many or most small-town businesses do survive the construction of the bypass and do remain relatively intact, or even prosper.

Additionally, there is a highly variable lag between planning of the bypass and its actual construction. In Kansas, this lag has been as long as 13 years (US 36 at Highland was planned in 1978 and constructed in 1991) and as short as 1.5 years (for example, US 166 at Peru was planned during 1987-88 and constructed in 1988). There is also a variable period of public discussion before planning begins (discussion of the Peru bypass began in 1983). It seems reasonable to assume that businesses are aware of this uncertainty as to the time of construction, and that this uncertainty reduces any incentive to take immediate defensive action. But even if firms are *not* uncertain about the time of construction, if their defensive responses do not need to be rapid then the responses would probably be keyed more to the anticipated date of construction than to the time of announcement. In either case, is would be appropriate to assume that construction is the triggering event. However, in order to detect possible anticipatory effects, we looked at a period of up to 2 years prior to construction of the bypass, as well as at events that occur during and after the construction.

Categories of Business

This research used the same categories of businesses that were used in previous chapters, namely travel-related retail; non-travel-related retail; all-other; total retail; and total. In addition, in some cases we looked at travel-related retail data further disaggregated into three smaller categories:

1. service stations
2. restaurants and bars
3. motels/hotels

However, the sample sizes at this higher level of disaggregation were often statistically insufficient to draw usable conclusions, so we did not examine results at this level in much detail. For definitions of the business categories in terms of SIC codes, see Table 7.1.

Towns, Control Areas, and SIC Classes Used in This Study[13]

The available UI Master File (or "ES202") data were limited to 1988 through 1994. These data are most useful for examining towns that experienced bypass construction in this period. They are also of some use for examining towns that experienced bypass construction shortly before 1988. The Kansas towns that fit that description are listed in Table 7.2. These towns constitute the study group used in this chapter and the next.

The general goal in this chapter is to relate observed changes in a town's employment and payroll, and also changes in particular firms, to elapsed time before or after construction of a bypass. But since other factors could have caused any observed changes, attributing a causal influence specifically to the bypass could be problematical. In the event study frame-work, control of other causal influences is achieved by using a sample of events that occurred at different points of calendar time under different conditions. Because our available sample of towns was small, the effectiveness of that method was somewhat limited, and additional control methods were needed.

Moreover, because of the nature of the sample, different sets of bypass cities were available for different elapsed years. For example, the Peabody bypass was constructed in 1994, so no corresponding UI records are available after its construction. Several bypasses in the sample were constructed in or before 1988, so that no corresponding UI records are available before their construction. Four bypasses have corresponding UI records both before and after the bypass was constructed. Two different ways of controlling this problem were used for this chapter. One method involved the use of dummy variables for each city included in regression equations. The other method involved the use of comparison sets which hold the particular towns constant within selected pairwise comparisons of elapsed years. Both methods are described further below.

One additional way to control extraneous factors is by using a control group. Therefore, for each bypassed town, we defined a group of contiguous towns that serves as its control area. Each control area is geographically close to its corresponding bypassed town, limited to the same county when possible but otherwise including parts of an adjacent county. In all cases the control group area had a larger population than the bypassed town. Each control area needed to be larger than the corresponding bypassed town because the method of control involved matching businesses by detailed SIC code classes (as described in the next paragraph). If the control area were too small, then there would be too few firms in the control area, with the result that no matches would be available in many of the

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[13] This study had a complicated and layered control-group structure. To keep track of the language, we consistently refer to a (large) "business category," (small) "SIC class," and (geographic) "control area." The more complex ideas of "comparison set" and "analytic cell" are defined in the text. Each of these entities is a specific instance of the (generic) "control group."

detailed SIC code classes. The control area for each town is listed in Table 7.2.

When comparing growth of firms between a bypassed town and a control area, we need to take care to compare firms only with similar firms. In particular, economic conditions in a given year can be favorable to some types of firms and simultaneously unfavorable to other types. A blind comparison of firms across cities with different mixes of firm-types, would confound effects from the bypass with effects from the types of firms. To avoid this problem, firms in bypassed towns were matched with control firms within detailed SIC code classes. The classes are usually defined by 2 or 3 digit SIC codes; see Table 7.3 for definitions of the SIC control classes. These classes were constructed in such a way that, for every firm in every bypassed town, there was at least one comparison firm in the corresponding control area in the same SIC class.

Total Employment and Payroll in the Town

This section examines the transitory effects of bypass construction on aggregate employment and payroll in the town. The question we wanted to address is whether the total payroll and employment in various types of business were affected one way or the other by construction of the bypass.

The general method we used was a regression analysis. We report selected results for both employment and payroll in Table 7.4. In addition to controlling for elapsed time since construction of the bypass, we used all of the following methods to control for factors which may affect the growth rates for firms:

1. Payrolls were deflated into real 1985 dollars before calculating growth rates.

2. We looked at adjusted growth rates, defined as *differences* between growth rates of aggregate employment and payroll in the bypassed town, and what would have been predicted using growth rates of comparable control firms (i.e. firms in the same SIC classes in the control area for that town).[14] Since each sampled town is geographically

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[14] In the case of employment, the detailed formulae are as follows. For a given industry category in a given bypassed town, let

E_{it} be employment of firm i in year t;

g_i be the SIC control group for firm i; and

e(g,t) be total employment of all firms in the SIC control group g, in year t, in the control area for the given town.

Then aggregate employment in that town and industry in year t is

$E^*_t = \Sigma_i E_{it}$.

Actual aggregate growth in year t+1is

$E^*_{t+1} - E^*_t$.

Predicted aggregate growth if each firm had grown like its control group is

$\{\Sigma_i E_{it}[e(g_i,t+1)-e(g_i,t)]/e(g_t,t)\} - E^*_t$.

close to its control area, we were testing whether bypasses increase the rate at which jobs and payroll tend to migrate from the bypassed town into the nearby control area.

3. Some of the regressions included dummy variables to control for the average background growth rate in each town.

4. All regressions included a dummy variable for the recession years 1990-91.

The fourth type of control addressed the possibility that macro-economic disturbances in certain calendar years could cause statistically significant associations which had only a spurious relationship to the elapsed year since construction. The main such problem during the sample period is the recession of 1990-1991. If this recession affected firms in the bypassed towns differently than corresponding firms in the control areas, then significant differences could show up that reflect the recession rather than the bypass. The recession dummy controlled this problem.

In each of these regressions, the constant term represents the background adjusted growth rate before the bypass is constructed. The other three coefficients represent the average change in growth rates due to the recession, due to conditions during the year of construction, and due to conditions following the year of construction.

It turns out that these regressions are somewhat sensitive to inclusion of dummy variables for the different towns. Including dummy variables did not usually change the direction of the estimated effects, but it did reduce the size of most of the coefficients, and it also noticeably changed significance levels of coefficients (in both directions, depending on the coefficient). Because controlling for towns is theoretically better than not controlling for towns, when possible the regressions reported in Table 7.4 are those which do control for the level of growth in the different towns. However the coefficients for particular towns are omitted from the table. Also, some of the regressions did not include dummy variables for towns because the data would not support them;[15] these regressions are indicated as such in the table.

The 1991 recession

Hence the difference of the two growth amounts is

$$E^*_{t+1} - \{\Sigma_i E_{it}[e(g_i,t+1)-e(g_i,t)]/e(g_t,t)\}.$$

Therefore the adjusted aggregate growth rate is defined as

$$[E^*_{t+1} - \{\Sigma_i E_{it}[e(g_i,t+1)-e(g_i,t)]/e(g_t,t)\}]/ E^*_t.$$

The regressions were performed using this last quantity as the dependent variable. The formulas for real payroll are entirely similar.

[15] i.e. there was multicollinearity in the dummy variables. This could happen when the absence of any firms led to missing observations in certain business categories in certain years.

As it turns out, the recession was estimated to have negative effects in most regressions; in other words, the recession hurt the small towns worse than it hurt the control areas. In some cases the effect is statistically significant (with p=0.05), meaning that the sample is large enough to resolve at least the sign or direction of the effect with a reasonable degree of confidence. In some cases the effect is economically significant, meaning that the effect appears large enough to be important. According to these models, the recession had a strong and statistically significant negative effect on employment in travel related businesses, but not much effect on other businesses or on total employment. For purposes of this study, however, the main importance of this finding is that it confirms the need to control for the recession years when analyzing this data.

The year of construction

The regressions show no statistically or economically significant effects from the bypass during its year of construction, with the exception of total retailing. That sector showed a drop in payroll but not in employment. The measured drop in retail payroll was economically significant but had a low statistical significance (p=0.20), i.e. it was not significant using the conventional p = 0.05. In other words, the negative result may have been due to random sampling errors.

Continuing effects in travel-related industries

After the bypass was constructed, effects on aggregated employment and payroll in travel-related businesses were estimated to be negative, but neither of the effects is statistically significant (using the conventional p=0.05). Within the travel industry, restaurants were growing and service stations were declining, but again these effects are not statistically significant. However, the lack of statistical significance is *not* telling us that any of these effects are small in economic terms. Rather, it is telling us that the given data are not sufficient to resolve the sign of any one coefficient with high confidence. In fact, as it turns out, many of the estimated coefficients on their face appear to be large enough to be economically important. Also, some of the effects are approaching significance at the weak level of p=0.20. Moreover, the four most significant coefficients (which are those for service stations and aggregate travel-related business) all have the same sign. The cumulative evidence here definitely suggests that aggregate payroll in travel-related industry, and perhaps aggregate travel-related employment as well, was negatively impacted by the bypass.

Continuing effects in other industries

After the bypass was constructed, the measured effects on workers in non-travel-related industries were positive. All the coefficients were positive for non-travel-related retail and for "all-other" employment and payroll. Some of the coefficients are statistically significant. Most of the coefficients are economically significant. The point estimates imply that the bypass generated extra payroll growth in these business categories of 10% or more per year, and a lesser amount of employment growth, for several years after the bypass is constructed. (According to national research, payroll growth often exceeds employment growth when times are good, because firms may increase worker's total hours without increasing the total number of workers.)

This positive growth in major business sectors probably led to positive growth in total business activity. The point estimates of effects of the bypass on total retail and total overall payroll and employment are small but positive, but this result has a rather wide error band. However, the error bounds are tight enough to provide some confidence that, at worst, the true effects of the bypass were not strongly negative. In particular, in the case of payroll growth, we can be 95% confident that the true effect of a bypass in similar models for similar towns would have been no worse than a 2 or 3% temporary loss in annual growth. It is much more likely however that the true effect of the bypass would have been a small positive increase in the growth of total payrolls in the town.

This putatively positive effect on total payroll probably represents the net outcome of two offsetting processes: payrolls were rising in non-travel related businesses and falling in travel-related businesses. This story implies that there was some economic stress on workers in the travel-related businesses, but that stress did not translate into overall stress in the labor market. While some workers were being laid off from some travel-related firms, they were likely to have found new opportunities available in other firms. New labor market entrants probably did not experience any increased difficulty in finding a job; instead, they may have been made transitorily better off by the bypass.

Variation across towns

We should emphasize once again that we are only discussing *average* effects across towns. There was a great deal of variation between towns, and the experiences of individual towns differed quite a lot from the average. However, the most likely outcome for a given town appeared to be a positive aggregate growth in payroll and employment.

Effects on Local Tax Revenues

Citizens of a bypassed town are concerned not only with effects on businesses and on workers, but also with effects on the town government's revenue base. To directly examine short-term effects on tax collections in the town would be a study in itself, one that we can't perform with the data now in hand. However, the above results do suggest some inferences which, though less than definitive, are more than speculative.

Almost all towns in Kansas get some revenues from the property tax. Some towns have additional sources of revenue such as sales tax and intangibles tax, franchise fees and miscellaneous fees. In each town, each of these sources can be characterized by its relative responsiveness to local income, to local retail sales, and to other unrelated factors. The property tax rate, for example, responds to income (though slowly) because new property gets built partly out of local income, and also because the demand for old property, and hence its price, depends partly on local income.

The previous section suggests that total payrolls increased as a result of the bypass. We cannot directly examine the income of business owners from the UI data, because the owners' income is not covered by the payroll tax; but increases in employment and payroll are likely to correlate with increases in sales and revenues that lead to increases in the proprietor's income. Other forms of income, such as social security, rents and farm income are likely to be unresponsive to the bypass. Therefore, the bypass probably did not reduce total local income, and may have increased it.

Any effects of this income change on the property tax base were likely to be very small, however, because property values respond slowly to changes in income. A more important effect on the property tax base would be the new startups and relocations that followed from the bypass. These events were probably accompanied by new construction, which would augment the tax base. At the same time, as we shall see in the next chapter, the bypass led to additional business failures and relocations, especially in travel-related businesses. These events may have caused increased vacancies in real estate and eventually to reductions in the tax base through two channels: loss of market value, and abandonments of obsolete property. However these events would occur with a lag. That is, old property usually does not depreciate immediately and is not immediately taken off the tax rolls (though in some cases the property tax payments might become delinquent almost immediately). On net, the most likely effect of the bypass was to cause an increase in the property tax base (due to new construction), followed by a possible leveling off of the tax base (due to the depreciation of old property).

The previous section shows that total retail employment and payrolls were probably not hurt by the bypass, and may have been helped. Since these variables are moderately good predictors of retail sales, it is likely that sales tax receipts were not hurt in the average bypassed town, and may have been helped. However, experience here differed quite a bit between towns. As noted above, it seems likely that average employment fell in travel-related businesses even while it was rising in other retail businesses. Therefore a few towns which have a sales tax and rely disproportionately on travel-related services might have experienced some loss of sales tax revenues after the bypass was constructed. Since many small towns in Kansas are not growing, such an event could be serious for the affected town.

Conclusions

Over-all labor market conditions in the average bypassed town may have been helped by the bypass. Total activity, and each major category of activity except travel-related business, appeared to grow at a rate slightly faster than normal after the bypass was completed. This finding is implied by two different indicators: employment growth, and payroll growth. In the next chapter, we will see that this finding of improved growth is confirmed by independent evidence on startups and business failures. However, the data on this point are not absolutely conclusive. What we can say with some confidence is that a typical bypass did not cause substantial harm to the aggregate work force of the bypassed town.

In the case of non-retail firms, the previous chapter concluded that the bypass was probably not harmful, and possibly helpful in the long term. Data reviewed in this chapter show that the bypass could have been positively helpful in the short term as well. In other words, beneficial effects of the bypass on development or expansion of the town's non-retail economic base might show up rather quickly in the sampled towns.

A major finding of this chapter is that there is much variation in the experience of individual towns. Because of this variability, and because the sample of towns is small, it is hard to be certain about the average short-term experience of bypassed towns. A larger data set might allow us to measure the average effect across towns with greater precision than we accomplished here.

Table 7.1
Categories and Sub-Categories of Businesses
Definitions for Chapters 7 and 8

- TOTAL
  - Total retail
    - Retail, travel-related - SIC 58, 70,72,75,76
      - Service stations - SIC 75
      - Restaurants and bars - SIC 58
      - Motels/hotels - SIC 70
    - Retail, non-travel-related - SIC 50-57, 7396, 8042
  - All-other

Table 7.2
Towns and Years of Bypasses, and Corresponding Control Areas
Kansas bypasses in the years 1986-1994

City	Year	Control Area
Highland	1991	Doniphan county except Highland, Troy
Troy	1991	Doniphan county except Highland, Troy
Peabody	1994	Marion county except Peabody
Pleasanton	1990	Linn county except Pleasanton
Oakley	1986	Thomas county
Severy	1986	Elk and Greenwood counties except Severy
Peru	1988	Chautauqua county except Peru
Earlton[1]	1986	Allen and Neosho counties except Chanute, Earlton
Spring Hill	1987	Paola, Louisburg, Gardner, Edgerton
Chanute	1986	Allen and Neosho counties except Chanute, Earlton

[1]Earlton had no UI firms during 1988-94, so we dropped Earlton and its control from the analysis.

Source: IPPBR

Table 7.3
SIC Code Control Classes

017-019	305-308	521-525	723-724	864-869
027-029	323-324	541-542	729-738	873-874
072-074	327-344	543-545	753-754	912-964
076-078	359-369	551-553	782-783	
142-144	371-384	554-573	793-794	
148-154	394-399	594-599	802-805	
175-177	491-494	602-603	807-808	
201-204	501-508	611-614	821-829	
244-249	511-512	631-633	832-835	
251-283	517-519	653-671	839-841	

Note: The remaining codes are grouped by individual 3-digit codes.

Source: IPPBR

Table 7.4
Regressions of Aggregate Employment and Payroll Growth Rates on Dummy Variables

Industry: TOTAL

Independent Variables and Type of Statistic	*Employment Growth* Estimated Coefficient	Standard Error	Signif. Level	*Payroll Growth* Estimated Coefficient	Standard Error	Signif. Level
Constant	-0.0311	0.1528		-0.0277	0.1277	
Recession dummy	0.0178	0.0479	--	0.0028	0.0424	--
Construction year dummy	-0.0003	0.0958	--	0.0124	0.0846	--
Post-construction dummy	0.0245	0.0856	--	0.0991	0.0757	(*)
R-squared	0.0359			0.0613		
N	54			54		

Industry: Total Retail

Independent Variables and Type of Statistic	*Employment Growth* Estimated Coefficient	Standard Error	Signif. Level	*Payroll Growth* Estimated Coefficient	Standard Error	Signif. Level
Constant	-0.0272	0.2539		-0.1070	0.2858	
Recession dummy	0.0100	0.0592	--	-0.0093	0.0652	--
Construction year dummy	-0.1609	0.1135	(*)	0.1125	0.1251	--
Post-construction dummy	0.1370	0.1017	(*)	0.2471	0.1120	**
R-squared	0.1207			0.1084		
N	50			50		

Industry: Travel-Related Retail

Independent Variables and Type of Statistic	*Employment Growth* Estimated Coefficient	Standard Error	Signif. Level	*Payroll Growth* Estimated Coefficient	Standard Error	Signif. Level
Constant	0.1757	0.8610		0.4004	0.8098	
Recession dummy	-0.4527	0.2755	*	-0.5733	0.2591	**
Construction year dummy	-0.3788	0.5184	--	0.0877	0.4876	--
Post-construction dummy	-0.1688	0.3120	--	-0.3148	0.2934	--
R-squared	0.0803			0.1127		
N	48			48		

Industry: Service Stations

Independent Variables and Type of Statistic	*Employment Growth* Estimated Coefficient	Standard Error	Signif. Level	*Payroll Growth* Estimated Coefficient	Standard Error	Signif. Level
Constant	0.3463	0.5751		0.4820	0.6805	
Recession dummy	-0.5301	0.2107	***	-0.4778	0.2494	**
Construction year dummy	0.1470	0.3862	--	0.0517	0.4571	--
Post-construction dummy	-0.1370	0.2277	--	-0.3389	0.2694	--
R-squared	0.1617			0.1222		
N	37			37		

Table 7.4 continued

Industry: Restaurants and Bars

Independent Variables and Type of Statistic	Employment Growth Estimated Coefficient	Employment Growth Standard Error	Employment Growth Signif. Level	Payroll Growth Estimated Coefficient	Payroll Growth Standard Error	Payroll Growth Signif. Level
Constant	-0.0917	0.8379		-0.0979	0.6803	
Recession dummy	-0.2279	0.2793	--	-0.1572	0.2267	--
Construction year dummy	-0.1735	0.5565	--	-0.2020	0.4519	--
Post-construction dummy	0.1538	0.3073	--	0.1848	0.2495	--
R-squared	0.0384			0.0510		
N	44			44		

Industry: Non-Travel-Related Retail

Independent Variables and Type of Statistic	Employment Growth Estimated Coefficient	Employment Growth Standard Error	Employment Growth Signif. Level	Payroll Growth Estimated Coefficient	Payroll Growth Standard Error	Payroll Growth Signif. Level
Constant	-0.0994	0.3464		-0.1760	0.3121	
Recession dummy	0.1320	0.1062	--	0.0532	0.0796	--
Construction year dummy	-0.1318	0.2037	--	0.1199	0.1526	--
Post-construction dummy	0.2167	0.1824	--	0.2802	0.1367	**
R-squared	0.0861			0.1241		
N	50			50		

Industry: All-Other

Independent Variables and Type of Statistic	Employment Growth Estimated Coefficient	Employment Growth Standard Error	Employment Growth Signif. Level	Payroll Growth Estimated Coefficient	Payroll Growth Standard Error	Payroll Growth Signif. Level
Constant	-0.0467	0.1561		-0.0350	0.1640	
Recession dummy	0.0208	0.0471	--	0.0269	0.0543	--
Construction year dummy	0.0302	0.0940	--	-0.0055	0.1085	--
Post-construction dummy	0.0218	0.0841	--	0.1044	0.0970	--
R-squared	0.0549			0.0434		
N	54			54		

Notes: Significance levels for two tailed t-tests are denoted as follows:
*** p = 0.02 ** p = 0.05 * p = 0.1 (*) p = 0.2 -- = not significant at p = 0.2 level. Models for restaurants and bars, travel-related retail, and service stations were run without the city control dummy variables.

Source: IPPBR

8 Economic Impacts V: Short-Term and Transitory Effects on Firms

Introduction

This chapter focuses on the short-term events that occurred for individual firms when a bypass was first constructed. As in the previous chapter, we looked at these events using the unemployment compensation insurance (UI) data set. These data allowed us to examine changes in payroll and employment of individual firms by year and to identify the type of firm.

It is important to understand the differences in implications between *aggregate* growth of employment or payroll, and the *average* growth of individual firms. In particular, town-wide aggregate employment can be increasing even while the average firm faces declining employment. Two main pathways can lead to this seemingly paradoxical result:

1. If a few large firms grow while many smaller firms decline, then employment in the average firm can be declining even while total employment in the town is growing.

2. If older firms are declining (e.g. downtown firms) in a year in which new firms are being established (e.g. firms at bypass access points), then the average growth rate of firms is negative, because the new firms do not appear in the average (you cannot calculate a new firm's growth rate until its *second* year of life.) Yet aggregate growth in jobs could be positive because of new jobs in the new firms.

Therefore, when we want to examine effects on individual firms, we should look at the average firm growth rate of firms, as well as the spread or variance around that average. But when we want to examine effects on total employment in the town, we should look at the aggregate growth in employment (as in the previous chapter). Finally, to help us understand the relationship between the two, we should look at the rates of birth and deaths of firms.

Hence we performed two types of studies in this chapter. One type studied the employment and payroll gains and losses of individual firms. One type studied business startups and business failures.

We will use two different methodologies in this chapter. The first is regression analysis. The second employs a detailed comparison of firms between sequential years.

Methodology Shared with the Previous Chapter

The triggering event

This study assumed that the triggering event for economic changes in the town is the actual construction of the bypass.

Categories of business

This research used the same categories of businesses that were used in previous chapters, namely travel-related retail; non-travel-related retail; all-other; total retail; and total. In addition, in some cases we looked at travel-related retail data further disaggregated into service stations, restaurants and bars, and motels/hotels

Towns, control areas, and SIC classes

The town, control areas, and SIC control classes are the same as those shown in Tables 7.2 and 7.3.

Data Linking and the Definition of an "Establishment"

For examining the activities of individual business firms over time, we needed to locate all of the UI records that relate to a particular business. A threshold problem is defining what is meant by a "firm."

The unit of analysis adopted in this chapter is that of a single establishment located in a single town in a single line of business existing across time. (By definition, an establishment could have a sequence of different owners over time.) Unfortunately for our purposes, the UI master files are not organized in this fashion. Instead, a separate master file is constructed for each quarter, with only limited links between records across time. Therefore we undertook to assemble records for each establishment across time, using all years it was represented in a UI master file in the first quarter of the year during the years 1988-1994.

That simple problem led into a rather complicated computer analysis of matches on record IDs. The data fields in any administrative record can suffer from potential errors or differences in spelling, so it is hard to define exactly what matches should be considered as valid. In addition, there are complexities in the administrative procedures used to maintain the files. For example, the unique ID for a UI taxpayer is generally constant over time; however, it changes when the business is sold. Also, businesses may have multiple UI records if they have multiple business entities ("reporting units") at a single location. Finally, the sheer size of the dataset (around a half million records) made it a bit unwieldy to analyze.

More complete details on the methods of data linking are documented in Technical Appendix 8.1. We believe that the linking effort was largely successful. However, a later section discusses certain kinds of linking errors that may remain in our dataset, albeit at low rates of occurrence.

Firm-Level Employment and Payroll: Before, During, and After Construction

Our first empirical study was of the changes in growth rates of employment and payroll of individual firms that occur when a bypass is constructed. The question we wanted to address is whether individuals firms in various businesses were put under stress by construction of the bypass.

The general method is a regression analysis. We used methods similar to those described in Chapter 7 to control for factors other than the bypass which may affect the growth rates for firms:

1. Payrolls were deflated into real 1985 dollars before calculating growth rates.

2. We looked at adjusted growth rates. An adjusted growth rate is defined as the *difference* between the growth rate of a firm in the bypassed town, and the aggregated growth rate of firms in the same SIC class in the control area for that town. Since each sampled town is geographically close to its control area, we were testing whether bypasses increase the rate at which existing firms grow, as compared with similar firms in the nearby control area.

3. Some of the regressions included dummy variables to control for the average background growth rate in each town.

4. All regressions include a dummy variable for the recession years 1990-91.

In each of these regressions, the constant term represents the background adjusted growth rate before the bypass is constructed. The other three coefficients represent the average change in growth rates due to the recession, due to conditions during the year of construction, and due to conditions following the year of construction.

We examined the effects of bypasses on the average growth rate of employment using two kinds of regressions: either with dummy variables controlling for average growth in each town, or without. The coefficients related to the recessions and the bypass were almost the same when calculated either way, but the standard errors were smaller in regressions that did not control for the average growth rate in each town. Table 8.1 shows the latter results.

The 1991 recession

The recession was estimated to have negative effects on firms in most regressions; i.e., the recession hurts firms in bypassed towns worse than in control areas. In some cases the effect is statistically significant (with $p=0.05$), meaning that the sample is large enough to resolve at least the sign or direction of the effect with a reasonable degree of confidence. In some cases the effect is economically significant, meaning that the effect appears large enough to be important.

Variation across firms

A more important finding is that adjusted growth rates had a very high amount of variation across firms, even within a tightly defined analytic cell (i.e. for a particular category of business in a particular town in a particular year.) This was true well before any bypass was constructed, and it remained true during and after construction. (Average standard deviations of adjusted growth rates are shown for the different categories of business in Table 8.1; the average is taken across analytic cells for that business category.) Therefore we conclude that many firms were growing and others were declining in each year (relative to their control group), for reasons quite independent of the bypass. And therefore it is hard to separate out effects of the bypass from the ongoing background turmoil. Moreover, this finding implies that, even if negative effects did happen to be experienced by particular firms after a bypass wa constructed, some of the negative effects must be attributed to causes other than the bypass.

Variations in payroll growth rates turned out to be particularly high, and are much larger than variations in employment growth rates. Therefore payroll regressions were even less well defined than employment regressions. The pattern of coefficients in the payroll regressions was quite similar to that in the employment regressions, but the standard errors on the coefficients were up to twice as large. For this reason we present only the employment regressions in Table 8.1.

Non-travel-related business

Now let us consider more closely the regressions for businesses that are not specifically travel-related (meaning the following categories of employment: all other; non-travel retail; total retail; total). In terms of point estimates for average growth rates, these regressions tend to show either a negligible change or else an improvement in adjusted growth rates after the bypass is completed, although in some cases there may be very small measured decline during the year of construction. However, in terms of statistical significance, none of the results is statistically distinguishable from a finding of no effect due to the bypass. Also, all of the effects are relatively small in relation to the background deviations of the adjusted growth rates.

By itself this does *not* show that bypasses have positive or neutral effects on the growth rates of non-travel-related firms. Because the sample of bypasses is small, the regression has an uncomfortably wide error band. There is also a problem of interpretation -- we need to decide how large a given effect needs to be before it becomes really noticeable to the affected firms. To discuss these issues, we need another statistical indicator.

A lower bound indicator for economic importance

To that end, the table provides an additional statistic for each measured coefficient -- namely what we will call the "worst case deviation share." By comparing a lower error bound on the coefficient to the standard deviation of adjusted growth rates, this statistic places a down-side bound on the economic importance of each effect.[16] In particular, we can say with roughly 95% confidence that the actual effect, as a fraction of the measured deviation in growth rates for that category of business, is less negative than the statistic reported in the table. For example, if this statistic is negative but more positive than -1, then the measured effect is smaller than the standard deviation of the growth rate. An effect of this size would probably not be terribly noticeable to the firm, at least if the firm looked only at information on its total sales demand. (The firm might still become aware of an effect of this size, however, if for example it kept detailed records on what portion of demand came from through traffic.)

For every reported effect for every regression, the "worst case deviation share" is negative, meaning that negative effects on growth cannot be absolutely precluded with 95% confidence. However, in the case of non-travel-related businesses and business totals, all of these statistics are less negative than -1, meaning even at worst that the effects are not economically very important. Moreover, it is more likely that these effects are positive than that they are negative.

Travel-related business

We now turn to the regressions for types of business that are the most directly affected by travel (service stations; restaurants and bars; travel-related retail). These regressions have a different pattern from the regressions for non-travel-related businesses. The travel-related regressions all show definite negative effects on growth due to construction of the bypass. Half of the effects are statistically significant (with p=0.01 or 0.05). Some of the point-estimates of effects are economically significant - in particular, they are sometimes larger than the background standard deviation of growth rates. For restaurants and for the total of all travel-related retail, the main effects occur during the year of construction; but

[16] The "worst case deviation share" statistic is defined as: (regression coefficient - 1.65*standard error)/(standard deviation of adjusted growth rates). We used 1.65 based on a one-tailed test under normality assumptions.

for service stations, the main effect occurs after the construction is completed. In terms of the worst-case analysis, in no case can we absolutely preclude existence of a strongly negative coefficient that is economically significant.

Effects of a bypass on variation across firms

Using similar regressions, we also examined the effect of a bypass on the variation of adjusted growth rates across similar firms.[17] The variation is measured by a standard deviation. These results generally showed that the bypass and recession events had a small but positive effect on the total deviation in growth rates (see Table 8.2). That is, to the extent that this effect is small, these events shift the overall pattern of growth rates uniformly up or down, without singling out particular firms for special treatment. However, to a smaller extent, these stressful events also increase variation across firms.

Omitted information

The above regression studies did not utilize all the information available in the data. In particular, they did not take into the account the number of sample firms in each analytic cell. The number of firms is material because it affects how well the mean growth rate has been measured for a given analytic cell. Also, the periods before and after the bypass construction were not disaggregated by the particular elapsed years. We turn next to a method of analysis which uses more complete information.

Comparison Sets for Bypassed Towns and Elapsed Years

The next section will compare growth rates for a given type of firm across pairs of "elapsed years" before or after construction of the bypass. The firms included in each comparison are collected from all available towns and lumped together. To the extent that the lump of firms for one elapsed year contains firms from several different calendar years, this procedure controls for unrelated causes of variation in growth rates.

However, it happens that different sets of elapsed years are available for the different towns in our data set. For example, only positive elapsed years are available for towns with bypasses constructed before 1988; only non-positive elapsed years are available for a town with a bypass constructed in 1994. This could lead to bias if different cities were included in two different lumps being compared. That is, unless we take care to hold the included towns constant when we compare lumps of cities, we cannot tell if a given difference is truly caused by elapsed time, or merely reflects a difference in the choice of towns.

Therefore, in order to avoid comparing apples to oranges, we confined our attention to

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[17] i.e., standard deviations are calculated within an analytic cell defined by business category, town, and year.

comparisons that hold the particular included towns constant. The nature of the available data then leads to particular comparison sets, i.e. possible combinations of cities and elapsed years for which comparisons of this type are possible. The comparison sets used in the next section are summarized in Table 8.3

Firm-Level Employment and Payroll: Significant Years

This section raises the following question: in relation to the year of construction, were there particular years of elapsed time in which particular categories of business did especially well or did especially poorly?

In particular, this section looks for "significant events" for a given analytic cell.[18] A "significant event" is defined by two characteristics of a particular year and business type, in a particular comparison set:

1. employment and/or payroll growth is statistically significantly higher or lower in that year than in the previous or following year, using that comparison set; and

2. employment and/or payroll growth is especially high or low as compared with all other elapsed years available in the comparison set.

We used several methods to control for factors other than elapsed time. First, as in the previous analyses:

1. Payrolls were deflated into real 1985 dollars before calculating growth rates.

2. We looked at adjusted growth rates, i.e. at *differences* between growth rates of individual firms in the bypassed town, and aggregated growth rates of firms in the same SIC class in the control area for that town.

In addition, we used two new methods of control:

3. We used significance tests which examine *difference of differences*. That is, we searched for significant *differences* between adjusted growth rates (i.e. growth rate *differences*) in two consecutive years.

4. In each case we formed a comparison set that includes towns which were bypassed in different calendar years. (To some extent, this controls for effects related to the calendar year. However, because of the small number of bypass years in each comparison set, this kind of control is incomplete. In particular, there may be some

[18] An "analytic cell" in this case refers to a particular category of firm for a particular elapsed year, as observed in a particular comparison set.

contamination from the recession years. We address this problem further below.)

The detailed statistical method is complicated and we will relegate it to a footnote.[19] Some of the key data items are summarized in Table 8.4.

Significant patterns that emerge from this analysis are discussed below. Note that the bypass towns were generally growing more slowly than the comparison areas, so that adjusted growth rates tended to be negative. *Per se*, this negative relative growth rate should not be viewed as an effect of the bypass, since the slow growth pattern began well before the construction of the bypass. Instead, it probably reflects the fact the bypass towns

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[19] First, aggregate rates of change were calculated for each year in the control area -- that is, all firms in an SIC control class in a control area were joined together as if they were a single firm and their growth rate was calculated. Second, because rates of change have a skewed distribution (they cannot be less than -1 but they are unlimited in the positive direction) the calculated growth rates controls were truncated at +1. Third, a raw individual rate of change was calculated for each year for each firm in the bypassed towns and truncated at +1. Next the corresponding control growth rate was subtracted from the individual growth rate to yield an adjusted growth rate. Next an arithmetic mean of the adjusted growth rates was calculated for each industry category for each elapsed year within each comparison set. A standard deviation was also calculated for the same analytic cells. A standard error of estimate was also calculated using the standard deviation and the count of firms in the analytic cell.

An initial individual significance test was conducted as follows: for a given industry category and comparison set, use the mean growth rates and standard errors for two successive years to calculate a z-score (i.e. difference of the means divided by the root-mean-square of the standard errors of estimate.) In the absence of any genuine difference between years, the z-score has an asymptotic t distribution. Using a conventional 5% significance level, two years are significantly different if the z-score exceeds 1.96 in absolute value. (The z-score is not exactly t-distributed because the adjusted growth rates are bounded by +2 and -2 and hence are not normal. The z-score is asymptotical t-distributed because the mean of the adjusted growth rates in a group is asymptotically normal.)

This test is misleading, however, because we applied it to a total of 97 different comparisons for payroll, and another 97 for employment. Out of that many comparisons we should expect about 10 spurious p=0.05-"significant" events even if all of the measured differences were truly random. To test for this possibility, we calculated an asymptotic χ^2 statistic by summing the squared z-scores. Those statistics turned out to be 227 for adjusted payroll growth and 337 for adjusted employment growth. From a χ^2 table, it is apparent that this outcome is non-random with an extremely high probability (p=0.0000+), so apparently at least some of the comparisons were truly significant. Our problem now was to infer which individual comparisons are truly significant, and which are merely spurious.

The adopted procedure is as follows: we removed individual comparisons from the χ^2 calculation successively, starting with the most significant. When the probability of the χ^2 rose above the p=0.05 level, then we judged that the comparisons still remaining in the χ^2 test were not significant. In effect, we raised the threshold t-value on the t-test. The new t-test thresholds turned out to be t=2.40 for payroll and t=2.01 for employment.

are smaller than some towns in the control areas, and small towns have been growing slowly in Kansas. In the following, "low growth" means that the adjusted growth rate was more negative than usual; "high growth" means that the adjusted growth rate was less negative or more positive than usual.

Because of limitations in the control scheme, we cannot necessarily conclude that bypasses caused an event, even when it is statistically significant. In most cases, the main alternative explanation is that the 1991 recession may have caused the event. In the following, we will discuss whether that possibility applies in each significant event.

Findings

<u>For elapsed year -2 (i.e., 2 years prior to construction):</u>

No significant events were observed. (Note that, since no bypasses were constructed during 1992 or 1993, there was no opportunity for any data at -2 elapsed years to be influenced by the 1990-91 recession.)

<u>For elapsed year -1:</u>

Low growth was observed in travel and in total retail, in comparison set 9194. However, no such pattern showed up in the 9091 and 9094 comparison sets.

Low growth was observed in service stations but high growth was observed in restaurants, in comparison set 9091. These effects did generally show up in comparison sets 9094 and 9194 as well, but were not calculated to be significant. This pattern did *not* show up in Peabody, the only relevant case not confounded with the recession (but the Peabody sample is small). The effects were not very strong in Highland and Troy.

<u>For elapsed year 0 (the year of construction):</u>

Low growth was observed in restaurants and in travel, in comparison set 9091. The same effects generally occurred in sets 9194 and 9094 but were not calculated to be significant.

High growth was observed in service stations, in comparison set 9194. The same pattern was observed but not significant in set 9094, and was not observed in set 9091.

All of these effects could potentially be explained by 1991 recession effects in Highland and Troy.

For elapsed year 1:

High growth was observed in service stations, restaurants, and travel, in comparison set 9091. No other comparison set is available for this elapsed year. This outcome persists in Highland and Troy, where it cannot be associated with recession years.

For elapsed year 2:

No significant event was observed.

For elapsed year 3:

Low growth was observed in service stations and travel, in comparison set 9091. This outcome is not explainable by any recession effects. This pattern was not observed in comparison set 8688 or 8688x.

High growth was observed in motels and in travel, in comparison set 8688. High growth was observed in motels but was not significant, in comparison set 8688x. High travel was not observed in comparison set 8688x. This outcome could be explained by recession effects in Spring Hill and Peru.

For elapsed year 4:

Low growth was observed in service stations, restaurants, and travel, in both comparison sets 8688 and 8688x.

High growth was observed in "all-other" businesses in comparison set 8688. This pattern was not observed in set 8688x.

These outcomes could be explained by recession effects in most towns in the comparison sets.

For elapsed year 5:

Low growth was observed in motels and in total retail, in both comparison sets 8688 and 8688x. This outcome could be explained by recession effects in most of the sample towns.

For elapsed year 6:

High growth was observed in non-travel, service stations, motel, and travel, in both comparison sets 8688 and 8688x. This outcome is not directly explainable by recession year effects. However, this could be a rebound from low growth in the previous year.

Discussion

The majority of the significant effects occurred in pairs, with slow growth in one year followed by high growth in the next year, or vice versa. In such a case, there would not be much permanent effect from the event. Moreover, every single significant event is potentially explainable either:

1. as a recession effect, or
2. as a rebound the year after the recession, or
3. as an anomaly that is disconfirmed, i.e. does not appear in other comparison sets.

Moreover, significant events appeared about as likely to be observed in years 3-6, long after transitory bypass effects should have dissipated, as in the critical years 0 and 1. Consequently, these comparison set data by themselves do not *conclusively* show that a bypass has any transitory effects at all on the growth rate of any type of businesses.

At the same time, some of the comparisons appear to be confirmed by an examination of more disaggregated town-level data. In particular, a slow growth of travel-related industries (except service stations) was observed during the year of construction, followed by a high rate in the next year. This result was generally persistent across the four towns for which it could be examined,[20] and these towns had three different calendar years of construction. (See Table 8.5) This result is also consistent with the pattern predicted by previous research, in which some existing travel-related businesses are initially impacted by the bypass, and then make adjustments, in some cases by moving their locations out toward the bypass. Most elements of this expected pattern are not disconfirmed in any comparison sets. In other words, these data do tend to support the predicted pattern (transitory adjustments in travel-related business; no transitory effects in other businesses).

Of the remaining years, -2, -1, 2, and 3 either had no significant event, or had significant events which were disconfirmed in some comparison sets. Most of the significant events of years 4, 5, and 6 are confirmed in both comparison sets, but the two sets are not independent (they contain all the same towns except Chanute) and the events are disconfirmed in some of the individual towns. In other words, these data do not give much evidence of any persistent effects.

Defining Startups and Business Failures

The next section discusses the pattern of startups and failures in relation to the construction of a bypass. This section explains in technical terms how those data were created, and what errors may exist in the data.

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[20] except that no observation was available for Peabody for elapsed year = +1. Peru however constitutes a fourth town for elapsed year +1, though the travel-related sample size was very small.

A "startup" was assumed to be observed in the year when an ES202 record appeared for the first time for that establishment. (Recall that an "establishment" is a particular firm at a particular mailing address using a particular reporting unit code or its successor and a particular SIC code). A "failure" was observed when such an establishment record appeared for the last time. Startups could not be observed in 1988, since we had no earlier records to compare to. A failure could not be observed in 1994, since we had no later records to compare to. Therefore only 6 years of data were available on startups and failures.

A possible limitation of this definition is that some records for a given establishment might be missing for reasons that are spurious. For example, although we attempted to link similar mailing addresses, an error in a mailing address could lead to a spurious change of establishments. Also, there may be erroneous changes in the recorded SIC code. Also, in a few cases of chains of stores, the mailing address of the tax filing might shift back and forth between an establishment and its headquarters (even though UI rules require that all tax filing be from the local establishment address.)

As a partial test for this kind of error, we looked for a certain type of event in the data set that can be referred to as a "data hole." By definition, a data hole occurs among the records for a given establishment if no record occurs in a certain year, yet records occur both in a previous year and in a subsequent year. Holes could occur either because errors or other temporary changes occurred in the data in a given year ("apparent holes"), or else because an establishment actually did not file a record in the first quarter of that year ("genuine holes"). A genuine hole is a rather rare event which usually implies that the establishment actually had no payroll or employment in the given quarter.[21]

In the case of merely apparent holes, it was generally possible to identify the data change that caused the hole. That is, by careful searching, a second "establishment" could be located that had records that are exactly complementary to those of the first establishment -- that is, its records exactly fill in the holes of the first establishment -- and which matches on most ID fields. The ID fields that didn't match then revealed what data had changed. In some cases, the changed data were merely an obvious changed spelling, as of a street name. In those cases, we could sometimes eliminate the holes by relinking the records. In other cases, such as a change in SIC code, there was no simple way to be sure whether the change was genuine, or instead represented a data error. As a worst case assumption, we assumed that all such cases represented data errors.

At the end of the linking procedure, we found that about 1% of establishments had remaining holes that could be observed within the 7 years of available data. This fact can

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[21] In a few cases the firm merely failed to file on time. In such cases, normal UI enforcement procedures would usually lead the firm to file at a later date, leading to a valid late-filed record in a subsequent master file.

be used to generate an upper bound estimate on the rate of spurious startups and business failures in our data. In particular, a spurious startup would be observed whenever a hole overlapped the beginning of the six years. Presumably, undetected holes leading to spurious startups would occur with a pattern similar to the pattern of holes that we did observe. If that is the case, then at worst roughly 1% of establishments could have spurious startups during our 6 year observation period, which amounts to about 1/6% of spurious startups per year.[22] This rate is sufficiently small in comparison to the observed rate of startups that it can generally be ignored. Similar remarks apply to the observed rate of business failures.

Patterns of Startups and Failures

This section examines the effects of bypass construction on business startups and failures. The regression models were similar to those used for average growth rates, but there is one difference in approach. In particular, we made no attempt to control for startups and failures in the control areas; thus the dependent variables are absolute startup and failure rates in the town rather than rates adjusted to reflect differences. Some results of the regressions are shown in Table 8.6. Once again, dummy variables for individual towns had no appreciable effects on the coefficients we are interested in (though the R^2's are much higher with the town controls included). We have presented the regressions that do not contain the controls for town.

The coefficients for the 1990-91 recession show an increase in business startups for most of the business categories. Most of the increases were statistically and economically significant, and the startup rate was twice as high or more during the recession than before. (This is consistent with national research that shows a pattern of increased business startups during recessions.) Recessionary business failures, however, showed no strong patterns. (National research shows that failures tend to peak after the recession is over.)

Except in travel-related businesses, the effects of building a bypass on startups and failures were estimated to be economically small and statistically insignificant. However, in travel-related services, startups increased dramatically both during and after construction, at least in terms of point-estimates. Startups also increased more specifically in service stations, at least after the bypass is completed, but not in restaurants and bars. Business failures also increased dramatically in these industries, in the exactly

[22] This argument is greatly oversimplified in several respects. Thus, holes can represent a gap of a varying number of years. Our method could detect holes of up to five years in length, but holes of shorter lengths are much more likely to be detected. Holes of longer length could occur but could not be detected. Also, holes are more likely to cause spurious startups at the beginning of the seven years than at the end. However, the observed rates of occurrence for holes are sufficiently small in comparison to the observed rates of startups that it is not important to give a more careful analysis that accounts for these factors.

corresponding time periods. Business failures increased dramatically in restaurants and bars as well, both during and after the construction of the bypass.

However, none of these estimated effects quite reaches the 0.05 level of statistical significance. Yet the results do show a pattern which is both internally consistent and consistent with previous research on bypasses and on recessions. In particular, increased startups typically go together with increased failures as responses to business stress. In fact, the new startups lead to increased competition, which is probably one cause of the new failures; conversely, new failures tend to free up personnel, locations, and opportunities that may encourage new startups. In addition, economic stress often changes the set of opportunities and market niches, which encourages both startups to serve the new market niches, as well as failures of businesses serving old market niches.

The standard errors of the coefficients are uncomfortably large in these regressions. This implies that different towns had very different experiences from each other during construction of the bypass. It also implies that the average experience of similar towns could be noticeably more positive or more negative than what we observed in this sample.

The Combined Evidence on Economic Stress

At the same time, the cumulative evidence confirms the existence of transitory stress on travel-related businesses in our sample towns at the times when bypasses were built. The same pattern appeared in three independent data sources:

- reduced employment and payroll growth was observed for existing firms that did not fail, and there was an increase in the variance of growth;
- increased failure rates were observed for existing firms; and
- increased startup rates were observed for new firms.

The pattern showed up only in travel-related firms, and it showed up in several different categories of travel-related firms. The pattern is consistent with previous research. Some but not all of the individual estimated coefficients (especially those for employment growth) were statistically significant as well as economically significant.

The combined evidence strongly suggests a *lack* of stress in other business categories. Most of the point estimates show positive growth effects of the bypass on non-travel-related business. While these point estimates are generally not statistically significant, the persistence of the pattern across different types of evidence suggests that bypasses may actually be helpful to these businesses in the short term.

Conclusions

There is some evidence of stress on existing firms, particularly on travel-related firms, during and immediately after the year of construction of the bypass.

The evidence on business failures implies that this stress was sufficient to drive some travel-related establishments out of business. The data are consistent with the hypothesis that the stress was as much from competition from the new establishments on the bypass, as from loss of demand downtown.

In the case of non-retail firms, Chapter 6 concluded that the bypass is probably not harmful, and possibly helpful in the long term. Data reviewed in this chapter and in Chapter 7 show that the bypass could have been positively helpful in the short term as well. In other words, beneficial effects of the bypass on development or expansion of the town's non-retail economic base might show up rather quickly in the sampled towns.

A major finding of this chapter is that there is much variation in the experience of individual businesses. A larger dataset on towns with bypasses would not change the finding that there is large variation across individual businesses in a given town. Hence, even with better knowledge, some irreducible uncertainty will continue to face individual business owners in any town that is about to be bypassed. We will discuss some policy implications of that uncertainty in the concluding chapter of this report.

Table 8.1
Regressions of Adjusted Employment Growth Rates on Dummy Variables

Industry: TOTAL

Independent Variables and Type of Statistic		**Estimated Coefficient**	**Standard Error**	**Worst Case Deviation Share**
Constant		-0.0627	0.1919	
Recession dummy		-0.1053	0.0602	-0.2949
Construction year dummy		-0.0602	0.1235	-0.3803
Post-construction dummy		0.0314	0.2364	-0.1416
R-squared	0.0980			
N	51			
Av. st. deviation of growth rate	0.6941			

Industry: Total Retail

Independent Variables and Type of Statistic		**Estimated Coefficient**	**Standard Error**	**Worst Case Deviation Share**
Constant		-0.0933	0.3996	
Recession dummy		-0.1942	0.1337	-0.6602
Construction year dummy		-0.1066	-0.8472	-0.8472
Post-construction dummy		-0.0159	-0.4589	-0.4589
R-squared	0.0639			
N	47			
Av. st. deviation of growth rate	0.6284			

Industry: Travel-Related Retail

Independent Variables and Type of Statistic		**Estimated Coefficient**	**Standard Error**	**Worst Case Deviation Share**
Constant		0.1719	0.3854	
Recession dummy		-0.1195	0.1435	-0.7455
Construction year dummy		0.0104	0.2502	-1.3989
Post-construction dummy		0.1214	0.1615	-0.8698
R-squared	0.6445			
N	42			
Av. st. deviation of growth rate	0.6445			

Industry: Service Stations

Independent Variables and Type of Statistic		**Estimated Coefficient**	**Standard Error**	**Worst Case Deviation Share**
Constant		0.5112	0.5184	
Recession dummy		-0.5242	-0.5242	-1.4505
Construction year dummy		-0.0300	0.3727	-1.0902
Post-construction dummy		-0.5651	0.2364	-1.6144
R-squared	0.2851			
N	34			
Av. st. deviation of growth rate	0.5917			

Table 8.1 continued

Industry: Restaurants and Bars

Independent Variables and Type of Statistic		Estimated Coefficient	Standard Error	Worst Case Deviation Share
Constant		-0.0944	0.5619	
Recession dummy		0.0160	0.1981	-0.4911
Construction year dummy		-0.6625	0.3640	-1.9956
Post-construction dummy		-0.3080	0.2359	-1.1016
R-squared	0.0885			
N	41			
Av. st. deviation of growth rate	0.6329			

Industry: Non-Travel-Related Retail

Independent Variables and Type of Statistic		Estimated Coefficient	Standard Error	Worst Case Deviation Share
Constant		-0.1980	0.1719	
Recession dummy		-0.1195	0.1435	-1.4582
Construction year dummy		0.0104	0.2769	-1.8273
Post-construction dummy		0.1214	0.1772	-0.6997
R-squared	0.0369			
N	47			
Av. st. deviation of growth rate	0.2443			

Industry: All-Other

Independent Variables and Type of Statistic		Estimated Coefficient	Standard Error	Worst Case Deviation Share
Constant		-0.0465	0.2068	
Recession dummy		0.2068	0.0649	-0.2113
Construction year dummy		-0.0049	0.1331	-0.3270
Post-construction dummy		0.0273	0.0847	-0.1638
R-squared	0.0133			
N	51			
Av. st. deviation of growth rate	0.6864			

Source: IPPBR

Table 8.2
Regression of Standard Deviation of Growth Rates on Dummy Variables

Industry: TOTAL

Independent Variables and Type of Statistic		Estimated Coefficient	Standard Error
Constant		0.5917	0.3146
Recession dummy		-0.0641	0.0987
Construction year dummy		0.0883	0.2025
Post-construction year dummy		0.1473	0.1289
R-squared	0.0390		
N	51		

Industry: Travel-Related Retail

Independent Variables and Type of Statistic		Estimated Coefficient	Standard Error
Constant		0.6757	0.3632
Recession dummy		-0.1195	0.1398
Construction year dummy		0.0104	0.2502
Post-construction year dummy		0.1214	0.1615
R-squared	0.2025		
N	42		

Industry: Non-Travel-Related Retail

Independent Variables and Type of Statistic		Estimated Coefficient	Standard Error
Constant		0.3791	0.2844
Recession dummy		0.1361	0.0959
Construction year dummy		0.0769	0.1838
Post-construction year dummy		0.1608	0.1181
R-squared	0.0850		
N	45		

Source: IPPBR

Table 8.3
Comparison Sets of Towns and Elapsed Years

Comparison Set	Included Towns	Elapsed Years
Set 9194	Peabody, Highland, Troy	-2,-1,0
Set 9094	Peabody, Highland, Troy, Pleasanton	-1,0
Set 9091	Highland, Troy, Pleasanton	-1,0,1,2,3
Set 8688	Peru, Spring Hill, Oakley, Severy, Earlton, Chanute	3,4,5,6
Set 8688x	Peru, Spring Hill, Oakley, Severy, Earlton	3,4,5,6

Note: Theses comparison sets are named by the years in which the corresponding bypasses were constructed. The set 8688x omits Chanute because Chanute is much larger than other towns in the set 8688.

Table 8.4
Significant Growth Events by Comparison Group

Business Category	Elapsed Years	Number of Firms	Adj. Payroll Growth Rates Mean	Adj. Payroll Growth Rates Standard Deviation	Adj. Payroll Growth Rates z score	Adj. Employment Growth Rates Mean	Adj. Employment Growth Rates Standard Deviation	Adj. Employment Growth Rates z score
Comparison group: 8688								
All Other	4	250	0.042	1.597	1.023	-0.005	0.933	2.460 *
	5	269	-0.111	1.814		-0.186	0.723	
Motels/Hotels	3	7	0.360	0.987	1.964 *	0.599	1.139	2.428 *
	4	7	-0.436	0.420		-0.568	0.565	
Retail Non-Travel	5	100	-0.305	0.698	-3.028 **	-0.336	0.704	-3.014 **
	6	137	0.037	1.039		-0.041	0.797	
Retail Travel	3	66	-0.062	0.520	2.651 **	-0.020	0.666	2.886 **
	4	67	-0.314	0.576		-0.327	0.555	
Service Stations	5	13	-0.300	0.443	-1.431	-0.261	0.442	-2.406 *
	6	16	1.053	3.749		0.155	0.488	
Total	4	414	-0.052	1.491	1.528	-0.095	0.833	2.469 *
	5	440	-0.180	0.856	-2.865 **	-0.226	0.705	-3.601 **
	6	547	0.003	1.151		-0.064	0.700	
Total Retail	3	161	-0.006	0.729	2.285 *	-0.046	0.612	2.718 **
	4	164	-0.194	0.757	1.184	-0.233	0.629	0.784
	5	171	-0.287	0.680	-2.935 **	-0.289	0.674	-3.316 **
	6	216	0.025	1.367		-0.054	0.715	
Comparison group: 8688x								
Motels/Hotels	3	5	0.431	0.492	1.760	0.231	0.407	2.104 *
	4	5	-0.137	0.527	2.462 *	-0.358	0.475	1.033
	5	5	-0.949	0.516		-0.760	0.730	
Retail Travel	3	22	0.001	0.570	2.822 **	-0.018	0.597	3.009 **
	4	25	-0.490	0.622		-0.569	0.658	
Service Stations	3	4	-0.076	0.164	2.519 *	0.014	0.218	4.626 **
	4	5	-0.693	0.516	-1.421	-0.978	0.413	-1.789
	5	4	-0.129	0.645	-1.114	-0.337	0.615	-2.122 *
	6	5	3.197	6.635		0.484	0.525	
Total Retail	3	55	0.070	0.919	1.252	0.022	0.586	2.206 *
	4	58	-0.159	1.026		-0.274	0.825	

Table 8.4 Continued

Business Category	Elapsed Years	Number of Firms	Adj. Payroll Growth Rates Mean	Standard Deviation	z score		Adj. Employment Growth Rates Mean	Standard Deviation	z score	
			Comparison group: 9091							
Restaurants and Bars	-1	6	0.009	0.745	2.591	**	0.129	0.743	2.280	*
	0	7	-0.877	0.412	-3.108	**	-0.700	0.529	-2.005	*
	1	6	0.014	0.589			-0.060	0.609		
Retail Travel	0	11	-0.648	0.537	-3.259	**	-0.553	0.546	-3.007	**
	1	12	0.118	0.591	0.411		0.136	0.551	0.076	
	2	13	0.007	0.762	1.807		0.114	0.827	3.028	**
	3	13	-0.574	0.872			-0.741	0.594		
Service Stations	0	3	0.002	0.235	-4.451		-0.026	0.096	-7.513	**
	1	3	0.719	0.150	1.479		0.524	0.082	0.500	
	2	4	0.447	0.323	1.509		0.500	0.000	4.917	**
	3	5	-0.348	1.121			-0.671	0.533		
			Comparison group: 9094							
Restaurants and Bars	-1	8	-0.097	0.732	2.152	*	-0.031	0.744	2.011	*
	0	11	-0.758	0.547			-0.654	0.543		
Retail Travel	-2	11	0.052	0.733	1.295		0.290	0.852	2.101	*
	-1	11	-0.282	0.443			-0.333	0.492		
			Comparison group: 9194							
Service Stations	-2	4	0.756	0.427	3.216	**	0.966	0.674	3.121	**
	-1	4	-0.085	0.303			-0.289	0.438		

Notes: z-scores compare current-year statistics with the next year.
Significance levels for 2 tailed t-tests: * indicates p=0.05 ** indicates p=0.01

Source: IPPBR

Table 8.5
Adjusted Growth Rates in Travel Related Services

Industry	Elapsed Year	Calendar Year	Payroll Growth	Number of Firms	Employment Growth
City: Peabody					
Restaurants and bars	-2	1992	-0.4417	3	-0.0246
	-1	1993	*	*	*
	0	1994	-0.5497	4	-0.5726
	1				
	2				
	3				
	4				
Mean: Restaurants and bars			*		*
Travel-related retail	-2	1992	-0.2151	5	0.1607
	-1	1993	-0.2576	4	-0.3429
	0	1994	-0.2209	6	-0.2610
	1				
	2				
	3				
	4				
Mean: Travel-related retail			-0.2312		-0.1477
Service stations	-2	1992	*	*	*
	-1	1993	*	*	*
	0	1994	*	*	*
	1				
	2				
	3				
	4				
Mean: Service stations			0.3833		0.2910
City: Pleasanton					
Restaurants and bars	-2				
	-1	1989	*	*	*
	0	1990	-0.9414	3	-0.7521
	1	1991	0.1746	4	0.1269
	2	1992	*	*	*
	3	1993	*	*	*
	4	1994	-0.5381	3	0.1153
Mean: Restaurants and bars			-0.2853		-0.2435
Travel-related retail	-2				
	-1	1989	0.4569	3	0.5130
	0	1990	-0.9563	4	-0.8391
	1	1991	0.1746	4	0.1269
	2	1992	-0.6308	3	-0.5083
	3	1993	-0.4193	4	-0.7321
	4	1994	-0.4280	4	0.1907
Mean: Travel-related retail			-0.3005		-0.2082

Table 8.5 continued

Industry	Elapsed Year	Calendar Year	Payroll Growth	Number of Firms	Employment Growth
City: Pleasanton (continued)					
Service stations	-2				
	-1	1989		0	
	0	1990		0	
	1	1991		0	
	2	1992		0	
	3	1993	*	*	*
	4	1994		0	
Mean: Service stations			*		*
City: Highland, Troy (data merged to preserve confidentiality)					
Restaurants and bars	-2	1989	-0.4153	3	-0.4266
	-1	1990	-0.4036	4	-0.2335
	0	1991	-0.8279	4	-0.6608
	1	1992	*	*	*
	2	1993	-0.4603	3	-0.2989
	3	1994	*	*	*
	4				
Mean: Restaurants and bars			-0.6921		-0.6110
Travel-related retail	-2	1989	0.2746	6	0.3978
	-1	1990	-0.2967	7	-0.3275
	0	1991	-0.4721	7	-0.3887
	1	1992	0.0901	8	0.1401
	2	1993	0.1980	10	0.3012
	3	1994	-0.6424	9	-0.7442
	4				
Mean: Travel-related retail			-0.1414		-0.1036
Service stations	-2	1989	0.9646	3	1.2222
	-1	1990	-0.1541	3	-0.4527
	0	1991	0.0023	3	-0.0259
	1	1992	0.7186	3	0.5238
	2	1993	0.4473	4	0.5000
	3	1994	-0.1925	4	-0.5893
	4				
Mean: Service stations			0.2977		0.1964

* denotes data suppressed to preserve confidentiality.

Source: IPPBR

Table 8.6
Regressions of Business Startups and Business Failures on Dummy Variables

Industry: TOTAL

Independent Variables and Type of Statistic	*Business Startups* Estimated Coefficient	Standard Error	*Business Failures* Estimated Coefficient	Standard Error
Constant	0.1184	0.1095	0.1343	0.0945
Recession dummy	0.1201	0.0328	-0.0035	0.0283
Construction year dummy	0.0472	0.0658	-0.0060	0.0568
Post-construction dummy	0.0271	0.0389	0.0018	0.0336
R-Squared	0.2360		0.0011	
N	54		54	

Industry: Total Retail

Independent Variables and Type of Statistic	*Business Startups* Estimated Coefficient	Standard Error	*Business Failures* Estimated Coefficient	Standard Error
Constant	0.1034	0.1783	0.1270	0.1831
Recession dummy	0.1790	0.0552	0.0117	0.0566
Construction year dummy	-0.0377	0.1072	0.0753	0.1101
Post-construction dummy	0.0651	0.0639	0.0653	0.0657
R-Squared	0.1893		0.0227	
N	51		51	

Industry: Travel-Related Retail

Independent Variables and Type of Statistic	*Business Startups* Estimated Coefficient	Standard Error	*Business Failures* Estimated Coefficient	Standard Error
Constant	0.1486	0.2505	0.1474	0.3338
Recession dummy	0.0667	0.0802	0.0905	0.1068
Construction year dummy	0.1764	0.1509	0.0972	0.2010
Post-construction dummy	0.1606	0.0908	0.1802	0.1209
R-Squared	0.0818		0.0573	
N	48		48	

Industry: Service Stations

Independent Variables and Type of Statistic	*Business Startups* Estimated Coefficient	Standard Error	*Business Failures* Estimated Coefficient	Standard Error
Constant	-0.0064	0.3067	-0.0442	0.2358
Recession dummy	0.0144	0.1116	0.0994	0.0858
Construction year dummy	-0.0032	0.2060	-0.0221	0.1584
Post-construction dummy	0.2237	0.1206	0.1469	0.0927
R-Squared	0.1097		0.0949	
N	39		39	

Table 8.6 continued

Industry: Restaurants and Bars

Independent Variables and Type of Statistic	*Business Startups* Estimated Coefficient	*Business Startups* Standard Error	*Business Failures* Estimated Coefficient	*Business Failures* Standard Error
Constant	0.3326	0.3849	0.2678	0.3852
Recession dummy	-0.1024	0.1236	-0.0862	0.1237
Construction year dummy	-0.0683	0.2318	0.3177	0.2320
Post-construction dummy	0.0933	0.1398	0.1681	0.1399
R-Squared	0.0471		0.0591	
N	47		47	

Industry: Non-Travel-Related Retail

Independent Variables and Type of Statistic	*Business Startups* Estimated Coefficient	*Business Startups* Standard Error	*Business Failures* Estimated Coefficient	*Business Failures* Standard Error
Constant	0.0759	0.1843	0.1031	0.1752
Recession dummy	0.2067	0.0570	-0.0279	0.0542
Construction year dummy	-0.0997	0.1108	0.0845	0.1054
Post-construction dummy	0.0331	0.0661	0.0292	0.0628
R-Squared	0.2201		0.0173	
N	51		51	

Industry: All-Other

Independent Variables and Type of Statistic	*Business Startups* Estimated Coefficient	*Business Startups* Standard Error	*Business Failures* Estimated Coefficient	*Business Failures* Standard Error
Constant	0.1322	0.1200	0.1516	0.0965
Recession dummy	0.1030	0.0360	-0.0261	0.0289
Construction year dummy	0.0676	0.0721	-0.0638	0.0580
Post-construction dummy	0.0135	0.0427	-0.0291	0.0343
R-Squared	0.1793		0.0471	
N	54		54	

Source: IPPBR

9 Conclusions, Policy Implications, and Future Research

Summary of Research and Findings

The information in this report is based on three main types of models:

1. An origin-destination model of Kansas was developed showing the number of trips that take place between each town and city in Kansas. This model was used to estimate the amount of local traffic and through traffic in every town and city in Kansas.

2. A model was developed and data were gathered to estimate the value of the time-saving generated for through traffic by bypasses in Kansas.

3. A variety of economic impact models were developed and estimated using regression analysis of economic data from several sources. These models were used to quantify the effects of bypasses on business activity in bypassed towns.

The basic findings from these models are straightforward and consistent with previous research in other states.

First, in the long term, typical bypasses in Kansas probably do not have significant negative effects on the local economy. Most counties and many towns may have benefited in the long term from the construction of bypasses. The major part of this benefit probably consisted in an encouragement of basic industries, presumably due to the improved transportation system. Growth in basic industry would then have second-round effects on local retailing and services.

Second, in the short term, effects on individual firms are different from effects on the aggregate work force. In Kansas towns, bypasses typically did *not* have negative short-term effects on the town as a whole. Bypasses probably *did* have transitory negative impacts on selected firms. The negatively-impacted firms are concentrated in travel-related businesses, including restaurants, bars, motels, and services stations. However, not all travel-related firms in a bypassed town were negatively impacted.

Third, there is a large amount of background variation in the experience of individual towns and individual firms. The average effects of bypasses are generally small in comparison to this background. Moreover, individual towns and firms could be affected by bypasses in ways that differ quite a lot from the average effects. In particular, it is possible that some towns suffered permanent gains or losses due to bypasses. Also, some individual firms may have chosen to go out of business rather than adjust to changed circumstances caused by the bypass. Those firms typically were replaced by other firms.

Fourth, the size of this unrelated background variation implies that many factors other than bypasses affect the economy of small towns and of individual firms, and these various factors together are substantially more important than bypasses. In particular towns, these factors could either offset or exaggerate the apparent effects of bypasses. Two important factors that were touched on directly in this report are the short-term effects of recessions (the 1990-91 recession in particular), and the long-term health of small towns in Kansas. The recession was found to have an effect on the growth of travel-related firms that was substantially more negative in small bypassed towns than in the rest of the county. The growth rates of businesses in small towns were found to be less than the corresponding growth rates in the rest of the county, both before and after the bypass was built.

Finally, bypasses around small towns are highly beneficial to through traffic. Bypasses of 21 small towns in Kansas generated average time-savings for through traffic that are conservatively valued at upwards of $1 million per year (in 1994 dollars). Even if we ignore all other benefits of the bypass, then such a bypass would be justified on a benefit-cost test if the present value of all costs was less than ten million dollars.[23] Assuming that the initial costs of land acquisition and construction constitute at least half of the social cost, then such a bypass would be justified in a benefit-cost test if those initial costs were less than five million dollars. (Of course, individual bypasses could be either less or more valuable, depending on the traffic volume and the time saved per vehicle.) Larger costs could almost certainly be justified, but doing so would require a more detailed analysis.

The Benefit-Cost Analysis of Bypasses

This report is not a benefit-cost analysis of Kansas bypasses. A complete analysis, as described for example in Nash [1993], would need to address many types of costs and benefits not considered here. The benefits of highway investments are generally classified by who receives them, leading to two types: highway user benefits; and non-user benefits. The costs of highway investments can be classified by who bears them, leading to three types: taxpayer costs; user fees; and external costs. The various types of costs and benefits are treated differently in most benefit-cost analyses of highways, for reasons that are based more on practicality than on defensible conceptual arguments.

Highway user benefits

This category includes benefits to existing traffic plus benefits to new traffic generated by the development. Benefit-cost analyses generally try to list these benefits exhaustively and place a dollar value on each. These benefits consist mainly in two parts: costs savings to highway users, composed both of reduced operating costs and also reduced travel time;

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[23] This statement assumes a discount rate of 10%, which again may be conservative. With a lower discount rate, higher costs could be justified.

and the value of reduced accidents.

This report has examined only one narrow benefit item, namely the cost savings that bypasses provide to through traffic. A bypass also provides cost savings to local traffic in the town, because removing through traffic reduces congestion on the old highway route inside the town. Some bypasses also provide benefits to some local traffic that uses the bypass to get from one side of town to the other. In addition, bypasses reduce the rate of accidents for through traffic by allowing it to avoid the friction that takes place on city streets. Bypasses also reduce the rate of accidents for local traffic on the old highway route, because it faces less interference from through traffic. A benefit-cost analysis of bypasses would need to evaluate all of these factors.

Non-user benefits

This category includes benefits to all parties in all roles other than as highway users. Examples include reduced air pollution (because vehicles operate more efficiently at bypass speeds), reduced congestion on off-highway city streets, and improved safety for pedestrians. For an exhaustive catalog, see Gamble and Davinroy [1978]. In many cases, however, items that might appear to be genuine non-user benefits are actually spurious, because they amount to a double-counting of user benefits. For example, the increased value of a building site near a freeway is certainly a benefit of the freeway, but it is a benefit that mainly represents the reduced cost of transportation to that site. However, with careful analysis, the double-counting problem can be managed.

Non-user benefits are typically omitted from a benefit-cost analysis of highway projects, not so much because of the double-counting problem as because they are difficult to evaluate. If the project can be justified by a benefit-cost analysis restricted in this fashion, then the omission is of limited significance. That is, if the project can be justified by its user benefits alone, then it can certainly be justified by its user benefits plus its non-user benefits. (On the other hand, the assignment of priorities among the projects that pass a benefit-cost test might be sensitive to the inclusion of non-user benefits.)

One major non-user benefit of highways is the positive influence highways may have on the growth and development of municipalities and regions. This benefit is very hard to evaluate, however, for several reasons. First, it is hard to measure how much growth is due to highway construction, and how much is due to other factors. Second, some portion of the growth in one location represents a reduction of growth in competitive locations; and (at least in a global or nationally-oriented benefit-cost analysis) this reduction must also be accounted for. Third, this growth depends on other public and private investments in addition to highways; i.e. it has associated costs other than highway costs, and it is hard to disentangle how these other costs should be accounted for.

Some of these problems are reduced when the benefit-cost analysis adopts a local rather than a global point of view. For example, consider a benefit-cost analysis commissioned by a town government and intended only to guide town decisions, rather than state or national decisions.[24] Such an analysis would ignore all costs and benefits that accrue outside the town, and growth effects in the town would then be very important. In that case, models like those used in Chapter 6 might be used to estimate the growth effects of a bypass construction on the affected town.

Taxpayers' costs and user fees

The highway dollars paid by the taxpayers, together with the dollars paid for road tolls, are a valid measure of (one component of) the social cost of the highway investment. That is, these funds are generally expended on land acquisition, construction costs, publicly-subsidized relocation costs, and operation and maintenance of the highway, and each of these expenditures represents a withdrawal of society's resources from other possible uses. These costs are easily calculated because we have public budget information accounting for them.

External costs

External costs are the cost-side analog of non-user benefits, and they raise similar conceptual, measurement, and double-counting difficulties. External costs include all losses of human utility that happen because of the bypass, unless they are offset by government payments (as defined by administrative law). (However, we should exclude monetary losses to one private party that are offset by monetary gains to another private party; these are referred to as "pecuniary externalities.") Examples of external costs include the extra pollution that could result if the bypass leads to an increase in total traffic, and certain private business relocation and adjustment costs caused by the bypass.

Because of the rule that excludes monetary losses offset by monetary gains, some very significant external negative effects are usually not counted as costs. In particular, any negative effects that downtown retail businesses suffer from the bypass are treated as "distributional effects" rather than as costs, because losses to those merchants are likely to be offset by equal gains to other merchants. In this case the bypass merely redistributes income without creating or destroying it.

However, some of these effects might legitimately be included as costs in a benefit-cost analysis with a local or "open economy" orientation. The purpose of a study of this type is to advise a particular group of persons about their own best interests. For example, in a benefit-cost study oriented to existing residents of a particular town, firms located in other towns that benefited from the bypass could legitimately be excluded from the

[24] An analysis of this type has been referred to as an "open economy" analysis by Mohring [1993].

analysis, and benefits to firms started up by newcomers could also be excluded. Therefore the usual double-counting rules would not apply, and any negative effects on downtown merchants would count as costs.

In theory, distributional effects could also be legitimately included in a state or nationally oriented benefit-cost analysis, but with a different rationale. In particular, if the relevant political authority responsible for the highway project approved a coherent set of standards for placing a dollar value on certain kinds of distributional effects, then, of course, those values should be included in the benefit-cost analysis.

Distributional Impacts of a Bypass

The results in this report show that bypasses have probably not been noticeably harmful to small towns in Kansas in the aggregate or in the long term. Yet in the short term, real costs may be borne by certain individual firms. This report examined three kinds of individual stakeholders in small towns: business owners, workers, and (to a lesser extent) taxpayers. The costs borne by individual workers and by taxpayers do not appear to be especially onerous. The same can be said for many of the business owners. However, in some cases particular business owners, probably concentrated in travel-related business, may experience real economic stress.

Moreover, in advance of the construction, some legitimate apprehension might be felt by business owners even if they later turn out not to bear any stress. This apprehension is itself a cost to the business owners.

Public agencies might respond to the uncertainty and stress faced by individual business owners in several ways:

1. a promise of compensation. For example, public agencies do sometimes subsidize the costs of relocating the firm in cases where existing business access would be land-locked by the bypass. Knowledge that costs to the firm will be compensated certainly will reduce apprehension.

2. assisting businesses with technical support for responding to change, helping them take effective steps to reduce adverse consequences to the business.

3. actually reducing uncertainty by increasing the firm's knowledge about what will happen to its particular business. In other words, we need predictive models at the level of the individual firm. To create them, we would need a new kind of modeling, an idea which will be discussed further below.

4. taking redistributional effects into account when deciding whether to build a bypass, as discussed below.

The Decision to Build a Bypass

If a proposed bypass has substantial social benefits and passes a traditional benefit-cost test, then it does not seem reasonable to reject it merely because a small number of businesses will be stressed or displaced. This seems particularly true in a country as dedicated to free enterprise as the US, where a substantial amount of unpredictable business stress is a normal part of the competitive game. A new bypass is not intrinsically different from other economic investments that put stress on second parties, much like any new business startup that puts stress on its competitors (though of course a bypass is built by the government and not by a private firm).

Yet at the same time, predictable and planned public investments that cause stress do not seem entirely comparable to private competitive investments that cause stress. Democratic government, after all, is in the business of providing services that everyone can benefit from, and it tries to promote an element of fairness among individuals. In particular, there may be a perception of unfairness when particular individuals seem to bear disproportionate costs of a public project, especially in cases where those individuals do not seem to enjoy disproportionate benefits.

Including economic impacts in the policy decision

Thus it is entirely appropriate for decision-makers to take these distributional costs into account when they decide whether to build a particular bypass. The question this raises is whether an objective method is available for making such a determination. The research reported here suggests two conclusions:

1. While long-term effects may differ between bypasses in different towns, it is not feasible at present to predict these differences in an objective manner. Since long-term effects are more likely to be positive than negative, they can reasonably be ignored.

2. For similar reasons, it is reasonable to ignore any short-term effects outside the retail and travel-related sectors.

However, in the particular case of short-term effects in retailing and travel-related services, matters are more complicated, because there is a known potential for some individual businesses to experience losses while other businesses experience gains. If policy-makers choose to explore these speculative short-term effects, then additional research will be needed which is beyond the scope of this report.

Adjustment costs in a conventional benefit-cost analysis

Adjustment costs expected to result from the bypass have generally not been included in traditional benefit-cost analyses of highways. There are several reasons for this. First, in actual cases some travel-related businesses will choose to go out of business rather than pay the adjustment cost. In those cases, the measured adjustment cost will overstate the true economic cost. In particular, the true lost value to the business owner from closing the shop must be less than the adjustment cost, because otherwise the owner would choose to make the adjustment and stay in business. The lost value, and not the adjustment cost, is the true economic cost entailed by the bypass.

Second and more importantly, as noted previously there is a subtle double counting problem. In particular, suppose that every business chooses to make no adjustments. Then the lost patronage at any one establishment will be replaced by new business at some other establishment, either in the same town or elsewhere. (This is true because retailing and services are basically zero-sum games, in which total sales remain constant.) Since essentially the same level of services can be provided without making any adjustments, the adjustment costs generally are not viewed as part of the social cost of the highway project. Instead, they are generally viewed as an investment in an improved level of service. Since the benefits from the improved service are not included on the benefit side of the analysis, adjustment costs should not be included on the cost side.

On the other hand, depending on the details of the benefit-cost model, some part of these adjustment costs arguably could be a legitimate part of the social cost of the new highway construction project and therefore logically could be included in a benefit-cost analysis -- but a very complex accounting scheme would be required to establish this. Traditional benefit-cost analysis simply makes the assumption that effects in what are known as "secondary markets" cancel out.[25]

Other Possible Research

The work described in this report could be extended in several other directions that are relevant for Kansas transportation policy.

Predicting and managing effects of highway construction on individual firms

Like previous research, this report has examined the effects of highway investment on firms only in an average sense. There has been no effort we are aware of to study effects of highway investments at a completely disaggregated level - that is, to try to predict with specificity what will happen to a particular firm when a highway is constructed.

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[25] This question has to do with the difference between a partial equilibrium analysis and a general equilibrium analysis. In a general equilibrium model, it is possible to take all major effects in secondary markets into account. But traditional benefit-cost methods use a partial-equilibrium approach that ignores secondary markets. There is in fact no firm theoretical justification for ignoring secondary markets; it is simply a practical assumption.

The absence of such research is unfortunate. Models that could help firm owners and others predict impacts on particular firms from highway construction projects would have important benefits both to the firm and to society at large. First, many firm owners would find it valuable merely to know more about what is likely to happen in the future. Second, knowledge of the future can be used to change the future and adjust to it; thus, predictive models might assist some firm owners to make timely plans to relocate their businesses. Third, existence of more individualized knowledge may reduce some private resistance to public investments; for example, firms that were assured of the absence of adverse consequences might be persuaded to drop their opposition to a given project.

Firm-level modeling is not a pipe dream. It is not impossible in principle to make useful predictions about individual investment activities. In most commercial banks, for example, each commercial loan requires an explicit decision based on an explicit data-based model which at least implicitly embodies quantitative predictions about the outcome of the investment being financed. Implicit or explicit predictive models are used for a variety of other investment decisions. Thus venture capitalists and research corporations often have formal decision models which embody predictions about investment outcomes.

In general, these models are not merely predictive; they are also normative. That is, they are based on specific assumptions about what actions the investor should take in order to increase the chances of success. As a result of the existence of these assumptions, investors often do in fact take those beneficial actions. For example, bankers do not merely require existence of a business plan before making a business loan; they also review the plans carefully for quality and internal consistency. Therefore loan applicants do in fact become more careful in preparing business plans, and presumably these improved plans lead to improved prospects for the various ventures. Similarly, a predictive model of the effects of highway construction on a firm, would contain assumptions about that firm's preferred response to the construction; those assumptions could then help guide the actions of the firm.

The existing investment models for firms try to predict the effects of first party investment on the first parties, i.e. on the investors, whereas our concern with respect to highway investments is to predict the effects on secondary investments. Yet the problem is not drastically different. It boils down to one of searching for factors which affect the outcome for the party and then quantifying the importance of those factors.

The largest problem is one of data. Constructing and testing models of this type would depend on creating a sizable and very detailed database on individual firms and their environment before and after they were affected by highway construction projects. Such a database would probably have to use nation-wide construction projects, and would probably have to be supported by national funding sources.

Policy guidelines for including adjustment costs of travel-related firms in the bypass decision

A particular aspect of this problem that may be more suited to state-level research is that of predicting the short-term costs that travel-related businesses may incur when adjusting to new conditions after a bypass is built. This problem is simplified in that it narrows the problem in three ways: it looks only at travel-related business; it looks only at the short-term; and it looks only at the adjustment efforts that are expected to be made, without predicting the extent to which those efforts will pay off.

For reasons suggested previously, policy-makers may want to take these adjustment costs into account in the decision to build a bypass. If so, then additional research is needed to support the formation of an appropriate policy. The main problems entail conceptualizing what specific types of items should count as valid adjustment costs for purposes of this policy, proposing rules for measuring those costs, and then testing whether measurements based on those rules are sufficiently accurate to be used in routine policy decisions. The research should also propose alternative procedures for including these adjustment cost measurements in the bypass decision model.

The value of time for automobile travel in Kansas

In evaluating the time-saving from Kansas bypasses, we found that the largest source of uncertainty lay neither in determining the amount of time saved per trip, nor in determining the number of trips for different types of vehicles. Instead, it lay in determining the dollar value to be placed on a unit of time saved for auto travelers.

Because these uncertainties are so large, and also because the travel time savings turn out to be so very important in justifying highway projects, accurate knowledge of the value of time could conceivably change the outcomes of benefit-cost analyses of highway construction projects in Kansas. That is, there may be cases where we could not determine whether a given road project on net is socially helpful, or socially harmful, simply because we do not have sufficiently accurate information on the value of travel time by automobile in Kansas.

That value is uncertain in part because it is innately difficult to measure. By its nature, no market data are available on the value of travel time savings. Also, direct surveys of driver's evaluations of time-saving can be unreliable, because of the difficulty of formulating and answering questions about purely hypothetical evaluations. Thus, drivers have generally not formed a reasoned judgement on what they would be willing to pay to save travel time, because they have generally not been faced with much opportunity to purchase a reduction in auto travel time. (These direct survey methods are referred to as "contingent evaluation" methods.) Indirect ways to estimate the value of time-saving do exist, and are considered to be more reliable, but they are expensive to apply. These indirect methods depend on collecting and analyzing survey data on choices among competing modes of travel for a given trip.

A simple extension of existing research would be to perform a simultaneous study using both indirect methods and also contingent evaluation methods on the same data set. One result of this study would be to measure the inherent bias in contingent evaluation approaches. Using these bias measurements as adjustment factors, it would then be possible to use the less expensive contingent evaluation approaches in place of the more expensive indirect approaches for measuring travel time values.

An additional problem is that no studies of travel time values have been performed in Kansas. Moreover, the studies that have been performed nationally tend to have an urban bias and are difficult to extrapolate to rural Kansas. It would be very helpful to perform Kansas-specific and rural-specific studies of the value of time-saving for automobile travel.

Predictive Origin-Destination models of Kansas

We have discussed in some detail the gravity model of origins and destinations of trips in Kansas that was developed for this project. We believe that the model was reasonably successful, and sufficiently accurate for the purposes of this report. However, we have also designed a number of improvements in the model that we did not have time to explore. Those improvements would be likely to increase the accuracy of the model.

Increased accuracy could be important in some contexts, especially in cases where it was important to predict the number of trips on a particular proposed highway segment. In such cases, the decision to build the highway segment could depend critically on the anticipated short-term traffic volume on the new segment. Augmenting the gravity model with additional sources of information could lead to a model that was useful in this kind of short-term planning. However, because this particular gravity model is adapted to long distances and rural areas, the model would not be very useful in urban areas.

Another kind of planning has to do with long-term economic trends, which can affect the design of major new highway systems. An augmented gravity model could be useful for predicting future changes in highway usage over a decade or more. The augmented model would begin with a time-series of historical gravity models of Kansas. Then, econometric studies of the parameters of the model could be used to predict how the model would change in response to future economic conditions. (Future conditions could be predicted by other, existing economic models.)

Effects of highways on growth and development in Kansas

It is widely believed that a major benefit of highway construction consists in its effects on the growth and economic development of towns, cities and regions. Yet these and other "non-user" benefits are normally omitted from benefit-cost analyses of highway construction, for reasons discussed above.

At the same time, recent research (much of it cited in Chapter 4) has made progress in measuring these effects. This report also contributes to that literature. The methods developed in this report can be used to make additional progress on this issue. In particular, data on through traffic and on business activity can be combined with richer panel data on particular communities, and used to estimate various types of effects of highways on growth.

We believe that economic impact research is now at the point where it can provide reasonably reliable and replicable estimates of the average effects of highway construction on the growth of communities. The accuracy of these estimates may soon improve to the point where they could be included in benefit-cost analyses using the "open economy" or local-benefits approach -- i.e. it could be used in analyses that ignore the diffuse effects that impact on individuals and businesses outside of the limited area of the project. In particular, we might be able to estimate the costs and benefits of highway-induced growth from the point of view of a county, or of Kansas as whole. This kind of information would be useful to Kansas and local decision-makers.

As noted, this kind of analysis omits the negative effects that growth in one place may have on competing towns and cities in other states. These effects will be small in any particular out-of-state town or city, but because there are so many out-of-state towns and cities, the total effect across the country may not be small. For this reason, economists are probably a long way from being able to incorporate nation-wide growth effects into a nationally-oriented benefit-cost model for highways.

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