Infrastructure and Economic Growth

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The President:

Prof. Dr. Thomas Bieger

To Regina,
who learned more about infrastructure
over the course of my PhD than she ever wanted,
for her love and endless support,

and

to my parents,
who laid the foundation upon which
this dissertation is built

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Abstract

Infrastructure is considered to be a core ingredient to economic growth. However, empirical evidence on the effects of new infrastructure on economic outcome variables as well as on the determinants of infrastructure provision is still relatively scarce. This dissertation contributes to a better understanding of the role infrastructure plays in the economy with a collection of empirical studies from mature economies.

The first two studies examine the local and regional effects of the Swiss highway network. The analysis exploits the fact that the Swiss highway network was to a large extent defined by the Swiss federal parliament in 1960, but only gradually constructed over the decades that followed. The results suggest that highways led to an increase in the number of taxpayers and income per capita in municipalities within 10 km reach of a newly opened highway access point, and to a decrease in local tax rates. Two quantitative case studies show that observed local effects mainly constitute redistribution across regions, rather than net economic gains for the larger area.

Studies 3 and 4 investigate the impact that the roll-out of fixed broadband starting in the late 1990s had on growth in the OECD. Most existing firm- and industry-level studies find no short-term effects of broadband on productivity, whereas some country-level analyses report large output gains already shortly after broadband deployment. An analysis at the country level resolves this puzzle by showing that also at the aggregate level, there is no evidence for a positive impact of broadband during the initial years following its introduction. This finding is confirmed by a study at the industry level which tests for alternative growth channels and impact shapes.

The last study focuses on the influence of political and institutional factors on infrastructure expenditures in the United States. The results indicate that parties and institutional features such as term limits for governors have little effect on infrastructure expenditures. In contrast, political competition in the form of divided government is positively associated with higher highway expenditures, particularly capital outlays.

In sum, the findings suggest that in mature economies a new infrastructure typically has little effect on aggregate output in the short and medium run, but may lead to a spatial reallocation of economic actors and generate substantial non-monetary benefits.

Zusammenfassung

Infrastruktur wird als zentrale Voraussetzung für Wirtschaftswachstum angesehen. Die Faktenlage zu den wirtschaftlichen Auswirkungen neuer Infrastrukturen sowie zu den Determinanten der Infrastruktur-Bereitstellung ist jedoch immer noch relativ spärlich. Die vorliegende Dissertation trägt mit einer Reihe von empirischen Studien aus Industrienationen zu einem besseren Verständnis bei.

Die ersten beiden Studien untersuchen die lokalen und regionalen Effekte des Schweizer Autobahnnetzes. Die Analyse nutzt den Umstand, dass das Schweizer Autobahnnetz im Jahre 1960 durch das Schweizer Parlament weitgehend definiert, aber erst im Laufe der folgenden Jahrzehnte schrittweise erstellt wurde. Die Resultate zeigen eine Zunahme der Anzahl Steuerzahler und des Pro-Kopf-Einkommens sowie eine Senkung der lokalen Steuersätze in Gemeinden, die in einer Entfernung von bis zu 10 km zu einer neu eröffneten Autobahnauffahrt liegen. Zwei quantitative Fallstudien legen nahe, dass diese lokalen Effekte primär eine Umverteilung zwischen den Regionen denn einen Netto-Einkommenszuwachs für das gesamte Gebiet darstellen.

Studien 3 und 4 analysieren den Einfluss von fixem Breitband, dessen Verbreitung in den späten 1990er-Jahren begann, auf das Wachstum in der OECD. Die meisten bestehenden Studien auf Firmen- oder Industrie-Ebene finden keine kurzfristigen Effekte von Breitband auf die Produktivität, währenddessen gewisse Länder-Studien einen deutlichen Anstieg der Wirtschaftsleistung vermelden. Eine eigene Analyse auf Länder-Ebene zeigt, dass auch auf der aggregierten Ebene keine Evidenz besteht für einen positiven Einfluss von Breitband in den ersten Jahren nach der Einführung, und löst damit diesen vermeintlichen Widerspruch auf. Eine Studie auf Industrie-Ebene, die verschiedene Wachstumskanäle und Formen des Effekts in Betracht zieht, bestätigt dieses Ergebnis.

Die letzte Studie untersucht den Einfluss von politischen und institutionellen Faktoren auf Infrastruktur-Ausgaben in den Vereinigten Staaten. Die Resultate zeigen, dass Parteien und institutionelle Vorkehrungen wie Amtsperiodenbeschränkungen für Gouverneure wenig Einfluss auf Infrastrukturausgaben ausüben. Im Gegensatz dazu besteht ein positiver Zusammenhang zwischen politischem Wettbewerb in der Form von geteilten Mehrheiten in der Regierung und höheren Ausgaben für Strassen, insbesondere Kapitalausgaben.

Zusammenfassend legen die Resultate nahe, dass neue Infrastrukturen in Industrienationen wenig kurz- und mittelfristigen Einfluss auf die aggregierte Wirtschaftsleistung nach sich ziehen, aber zu einer räumlichen Umverteilung der Wirtschaftsakteure führen und substanziellen nicht-monetären Nutzen stiften können.

Chapter 1

Introduction

1.1 Motivation

"The third and last duty of the sovereign or commonwealth is that of erecting and maintaining those public institutions and those public works, which, though they may be in the highest degree advantageous to a great society, are, however, of such a nature, that the profit could never repay the expence to any individual or small number of individuals, and which it, therefore, cannot be expected that any individual or small number of individuals should erect or maintain."

Adam Smith (1778, pp. 331-332)

Public works and institutions have long been recognized as important drivers of prosperity. Over the last two decades, institutions – in their various forms – have received considerable attention in the economic research community. Since Adam Smith, who mainly referred to the institutions of education, the understanding of the term institutions in the economic literature has evolved. Following the seminal contributions by North and Weingast (1989) and Acemoglu, Johnson and Robinson (2001), institutions today denote more broadly the "rules, regulations, laws, and policies that affect economic incentives and thus the incentives to invest in technology, physical capital, and human capital" (Acemoglu, 2009, p. 111).

In contrast to institutions, public works such as roads, bridges, electricity grids, and communication networks were a much less prominent topic of recent academic discourse on economic growth. In my view, this negligence is not justified. On the one hand, a well functioning system of infrastructure assets and services is a prerequisite for long-term economic growth. It lowers transaction costs, facilitates the exchange of goods, services and ideas, fosters competition, and makes regional specialization and the realization of economies of scale possible. On the other hand, sound institutions and an adequate provision of infrastructure are closely interlinked.

Originally, infrastructure meant "...the subordinate parts of an undertaking; substructure, foundation; spec. the permanent installations forming a basis for military operations, as airfields, naval bases, training establishments, etc." (Oxford English Dictionary, 1989, VII: 950, cited in Baldwin & Dixon, 2008, p. 14). In the economic literature, the term infrastructure is used in various ways. In a narrow sense, infrastructure refers to economic (also called core or physical) infrastructure, which encompasses works in the areas of transportation (e.g., roads, highways, bridges, railway networks, ports, airports), telecommunication (e.g., telephone lines, broadband networks), energy (e.g., electricity, gas), as well as water supply and sewerage systems. In a broader sense, infrastructure also includes social infrastructure, which consists primarily of health infrastructure (e.g., hospitals) and education infrastructure (e.g., schools, universities)[1].

Most infrastructure assets according to the narrow definition share the following characteristics[2]:

- They are fixed (i.e., immobile) structures with long lives.
- They require large, lumpy upfront investments.
- They provide services that are complementary to other goods, services, or factors of production and have few substitutes in the short run.
- They are associated with market failures such as public goods characteristics, (network) externalities, or decreasing marginal costs (leading to natural monopolies).

These characteristics imply that infrastructure assets are often subject to some form of public intervention – be it regulation or even outright provision. Moreover, they are generally difficult to finance through private investment given that returns only materialize over long periods and often cannot be easily captured (see McKinsey Global Institute, 2013).

Surprisingly, there is still fairly limited systematic theoretical and empirical understanding of the relationship between infrastructure and economic growth. Most existing empirical studies following Aschauer's (1989) seminal paper on the productivity of public capital employed basic regression specifications, in particular aggregate production functions with infrastructure as additional input variable. In a recent meta analysis, Bom and Ligthart (2011) identified more than 60 studies of this type for the period 1983-2008. In general, these studies do not appropriately take into account characteristics that are important in understanding the effects of infrastructure investments, such as non-linearities, complementarities with other production factors, or network effects. These shortcomings have been

[1]In addition, social infrastructure also includes institutions such as courts, museums, parks, or playgrounds which "entail capital goods that have some public use" (Fourie, 2006, p. 531).

[2]This list is compiled based on definitions by Baldwin and Dixon (2008) and Prud'homme (2005).

pointed out by a number of surveys which highlight the need for a better understanding of how infrastructure fosters economic growth[3].

During recent years, researchers such as Donaldson (2010), Banerjee, Duflo and Qian (2012), Duranton and Turner (2012), Ghani, Goswami and Kerr (2012), or Andersen and Dalgaard (2013) have started to embark on that journey. Rather than relying on aggregate production functions, they use more sophisticated empirical strategies to identify the effects of specific types of infrastructure on economic output variables. With Chapters 2-5 of my thesis, I aim to contribute to this growing body of knowledge.

Infrastructure is not given, but man-made. Due to its public good and network properties, the provision of key infrastructure such as electricity and transport is deeply rooted in the public sphere. As a result, infrastructure has been and remains high on the political agenda across the world, from the United States to China. Chapter 6 therefore focuses on the political and institutional determinants of infrastructure provision.

The following section 1.2 summarizes the main findings of the work in light of the theory on economic growth and political economy.

1.2 Synthesis

As many forms infrastructure can take, as diverse are its potential effects on economic growth. Even for the same type of infrastructure – e.g., a road –, the economic effect may be very different depending on the setting: A new road may be very beneficial if it is the first road between two cities, or if it removes a bottleneck, or it may be of little economic value in case it constitutes an unnecessary capacity expansion or a road to nowhere.

As a result, more infrastructure may not necessarily help, while a lack of adequate infrastructure may constitute a barrier to economic growth. Understanding under which conditions more or a new infrastructure does indeed foster economic growth is therefore an important issue.

From a theoretical point of view, one of the main questions concerning the relationship between infrastructure and economic growth is whether infrastructure raises growth in a transitory way, as the inclusion of infrastructure in a neoclassical growth model would predict[4], or whether it spurs growth on a permanent basis in the spirit of the endogeneous growth theory[5]. Again, this may depend on the specific circumstances and differ across infrastructure types – transportation may primarily bring transitory benefits, whereas telecom-

[3]See, e.g., Gramlich (1994), Sturm, Kuper and de Haan (1998), Fourie (2006), Romp and de Haan (2007), or Straub (2008/2011).

[4]Infrastructure may increase aggregate output through the services it provides as input factor or by raising total factor productivity, see, e.g., Romp and de Haan (2007) or Straub (2011).

[5]Barro (1990) incorporates government spending in a model of endogeneous growth and shows that based on the assumption that public capital is complementary to private capital, higher government investments in productive infrastructure may initially raise growth and saving rates.

munications infrastructure may also increase the rate of innovation in the economy and thus growth in the longer run.

From an empirical point of view, the analysis of the effect of infrastructure on economic growth is characterized by a number of difficulties. Above all, establishing causality between the infrastructure in place and economic growth emerges as the major challenge. Investments in infrastructure may foster economic growth. At the same time, however, growth can also create demand for additional or new types of infrastructure and render existing installations obsolete. Due to this two-way interdependency between infrastructure provision and economic growth, econometric analyses of infrastructure investments may be subject to endogeneity biases. My first four contributions tackle this challenge in different ways. Chapters 2 and 3 focus on the local and regional impact of the Swiss highway network, Chapters 4 and 5 on the growth effect of broadband infrastructure.

Given that infrastructure is man-made, the last study focuses on the determinants of infrastructure from a political economy angle. In Chapter 6, I analyze how party control, political competition, and other institutional factors affect infrastructure expenditures across U.S. states.

Transportation: Swiss highway network

Transportation is maybe the most archetypical form of infrastructure. Transportation networks require large upfront investments, but then provide services over many years by reducing transportation costs for goods and people. For a long time, roads, canals, rail, and later airports have been considered important ingredients to economic growth, and they continue to be high on policymakers' agendas until today. In many countries, projects for high-speed rail are either under consideration or already in the stage of construction or completion. Often, policymakers argue that these new connections help spur economic growth. However, not all observable effects necessarily constitute a net increase in economic activity. They may also reflect a spatial relocation of people and businesses across regions. This point has been left largely unaddressed in the empirical literature to date. Chapters 2 and 3 contribute to this discussion.

In **Chapter 2**, Raphaël Parchet and I examine the impact of the Swiss highway network on people's residential location choices. Commuting costs to work – both out-of-pocket expenditures as well as time spent commuting – are a key determinant of people's decisions about their place of residency. The opening of a new highway changes the accessibility of municipalities, in particular those that are located outside of agglomerations, but in close distance to a metropolitan center. People may decide to change their place of residency while continuing to work for the same employer in the metropolitan center. As a result, the population is expected to rise over-proportionally in municipalities with good highway access, with a corresponding increase in the tax base. As a second-order effect, municipalities may react to these changes of their tax bases with an adjustment of their local tax rates.

We investigate these hypotheses based on federal income tax data for Swiss municipalities over the period 1971-2010. The decision to construct a national road network in Switzerland had been taken in 1958, and the network was largely defined in 1960 by the federal parliament. The construction, however, took several decades, and still today not all parts of the network are complete. We exploit this variation over time to identify the effect of the opening of a new highway access point on the total number of taxpayers, income per capita, the share of taxpayers in different income categories, and local tax rates in municipalities located at different distances from the new highway access points.

Purpose of the Swiss national road network was to connect Switzerland's large cities; yet it also increased the accessibility of municipalities located along the highways. From the viewpoint of these non-agglomeration municipalities, the opening year of a particular highway section in their region can be regarded as largely random and exogenous to local economic development. The fact that the planning process involved many actors from different political levels and that there was fierce opposition against the construction of certain parts of the network created additional randomness in the timing. Moreover, also the geographical location of highways was subject to a random component, at least in certain parts of Switzerland. For the highway between Lausanne and Berne, for instance, roughly a dozen different routes had initially been considered for the location of the highway. The decision for the final location of the highway was taken based on network, but also construction and cost considerations, among others.

For the analysis, we collected information on the opening years of all highway access points in Switzerland. For each year and non-agglomeration municipality, we calculated the distance to the closest highway access point using the non-highway road network. To further reduce potential endogeneity concerns, we excluded all municipalities with a highway access point located directly on their territory.

Based on fixed-effects panel regressions that control for municipality-specific linear time trends, we find that municipalities within 10 km reach of the next highway access point indeed experienced a longer-term increase in the number of taxpayers and in average income per capita of about 4 percent. In contrast, municipalities between 10 and 15 km away did not see a similar increase of their tax base. These results are robust to exploiting variation over time only and more pronounced for municipalities in close reach of a nearby metropolitan area. The effect became stronger over time as the highway network was more and more complete and space in cities became scarce.

The results for the second-order impact on tax rates indicate that municipalities receiving a nearby highway access point reduced their tax rates by about 2 percent, with a considerable lag of approximately 10 years. This finding suggests that municipalities subject to a permanent positive shock to their tax base do not use all additional tax receipts to provide additional public goods, but instead lower their municipality tax rate.

In **Chapter 3**, Christoph Gorgas and I approach the question to what extent observed increases in income per capita reflect net economic gains explicitly based on two case studies of major highway expansions in Switzerland. To avoid that the results simply reflect the relocation of people from one region to the another, we focus on two network expansions where relocation into or out of the region as a result of the new highway was of no major importance: The highway between Geneva and Lausanne, completed in 1964, and the Gotthard Road Tunnel, which was opened to traffic in late 1980 and established an all-year road connection from central Switzerland to Ticino.

Without doubt, also these highway expansions affected the residential location choices of people. However, in the Geneva–Lausanne case, the highway primarily increased accessibility of the municipalities along the Lake of Geneva, within the joint region. Ticino remained outside of regular commuting distance from the next German speaking center Lucerne even after the opening of the Gotthard Tunnel.

Similar to the study in Chapter 2, our analysis exploits the fact that the particular opening year of the these highway expansions can be regarded as largely random and exogenous to regional economic development. The highway A1 section between Geneva and Lausanne was prioritized to be ready for the Swiss national exhibition in Lausanne in 1964. The decision to build the Gotthard Road Tunnel was taken in 1965 in order to facilitate the North-South transit through Switzerland, and not due to the development of the regional economy in Ticino in particular. Moreover, construction took ten years, creating additional randomness in the timing.

We analyze how income per capita evolved in the first 10 years following the expansion of the network using the synthetic control method developed by Abadie, Diamond and Hainmueller (2010). The synthetic control represents the counterfactual of how the Geneva-Vaud region respectively the canton of Ticino would have developed in the absence of the new highway or tunnel and is constructed as a weighted combination of cantons not undergoing a similar highway expansion at the same time. To ensure that the counterfactual is relevant for our comparison, we require that the synthetic control resembles the original canton in terms of major characteristics such as industry sector shares, unemployment rate, and the number of newly built apartments. Income per capita is proxied based on federal income tax data at the cantonal level.

In both cases, we do not find a positive impact of the highway on cantonal income per capita during the first 10 years following its opening. In the case of Ticino, it seems that the canton even underperformed its synthetic control – a trend that started shortly before the opening of the tunnel, however. Robustness checks suggest that these results are not driven by specific cantons in the synthetic control, the selection of canton characteristics, the time period of the optimization procedure, or the construction period. Our findings therefore imply that advanced network expansions – building a faster connection where a slower connection already exists – do not necessarily boost the output of a region, at least not in the short to medium run. However, such expansions may still have effects on people's welfare, e.g., in the form of reduced commuting times or the possibility to live in the countryside while working in the metropolitan center, as Chapter 2 shows.

Telecommunications: Broadband infrastructure

Chapters 4 and 5 analyze the economic impact of a type of infrastructure that is said to have general purpose technology (GPT) properties: fixed broadband. Fixed broadband is defined as an access line with a speed of at least 256 kilobits per second. Today, measured average connection speed reaches up to more than 40 megabits per second in leading countries of the Organisation for Economic Co-operation and Development (OECD).

Analyzing the impact of a general purpose technology is difficult, for several reasons. First, the effect may not materialize at the time of deployment of the infrastructure, but with a considerable time lag. In the case of the steam engine or electricity, the adoption of the new technology took several decades, as companies had to reorganize their business processes[6]. Second, the effects of a general purpose technology may only materialize in combination with other complementary factors, such as a large share of a certain production factor (e.g., human capital), or in a specific institutional setting. Identifying these conditions is challenging. Third, it may be difficult to disentangle the effect of a GPT from related innovations that make use of the GPT. Was it electricity that provided the actual benefit, or rather the machinery and equipment that used electricity as an input? Would such machines have been developed also without electricity?

In the case of broadband infrastructure, there are a number of additional factors that complicate the empirical analysis even further. First, the internet existed before broadband was introduced, but both broadband and internet penetration went hand in hand. Second, all developed countries introduced broadband, and did so at roughly the same time. Exploiting variation both in availability as well as timing is therefore difficult. Third, other innovations such as increased processing speed of computers and the advent of mobile and smart phones took place at almost the same time.

Against this backdrop, it does not come as a surprise that existing studies on the economic effect of broadband come to very different findings, ranging from a zero effect to substantial impacts on the level or growth rate of economic output or productivity measures (see, e.g., International Telecommunication Union ITU, 2012, for an overview). Interestingly,

[6] See OECD (2008) for details.

most firm- and industry-level studies find no short-term effects of broadband on productivity, whereas some country-level studies report a large impact on GDP per capita.

In **Chapter 4**, Uwe Sunde and I outline this puzzle in detail based on an extensive literature review. Next, we propose a resolution of the puzzle in two steps. First, we analyze the relationship between total factor productivity and broadband penetration across the OECD based on country-level data from the EU KLEMS database. Total factor productivity is the residual of a growth accounting exercise and constitutes the part of growth that cannot be explained by changes in input factors, including information and communications technology (ICT) goods. In case broadband infrastructure had a positive impact on growth, we would expect a positive correlation between productivity changes and broadband penetration over the period of broadband roll-out. However, our results show no such relationship between broadband and productivity, even when allowing for a non-linear pattern.

Second, we revisit the findings of the most prominent study suggesting a large positive effect of broadband on growth, conducted by Czernich, Falck, Kretschmer and Woessmann (2011). We review their analysis and provide an alternative interpretation of their results that is consistent with the absence of a positive impact of broadband at the country level. Additional robustness checks confirm our finding. Thus, neither micro- nor macro-level studies point towards a systematic positive effect of broadband on growth across the OECD during the initial years following its introduction.

It is important to note that this finding does not preclude a positive effect of broadband on growth altogether. Broadband could still have been beneficial in the early years under specific conditions, or for certain firms or industries. In **Chapter 5**, I test for such a heterogeneous effect based on productivity data at the industry level. The analysis rests on the idea that the effect of broadband likely was not homogeneous across industries. Rather, certain industries were better positioned to realize productivity gains from broadband than others.

I adopt an approach inspired by the finance-growth literature pioneered by Rajan and Zingales (1998) and analyze whether industries with strong broadband dependence experienced over-proportional productivity growth as broadband diffusion progressed. I proxy broadband dependence in three ways – with the share of IT employees, R&D intensity, and the share of high-skilled workers in an industry prior to broadband roll-out. Each measure addresses a different potential growth channel of broadband. Given the uncertainty around broadband's effect on growth, I allow for two different impact patterns: an impact shortly after broadband became available in a country, and a more gradual effect that rose as penetration among firms and households progressed.

Typically, penetration levels were well below 1 subscription per 100 inhabitants in the first years following broadband introduction. A positive impact from broadband during the first years could still have materialized in case companies were able to benefit from broadband, e.g., by reorganizing their business processes such as switching to e-procurement or improving their logistics. Given that internal reorganizations normally are costly and time-consuming, finding a large impact within the first years would be rather surprising, however. It seems more likely that a positive impact of broadband would have materialized gradually as penetration increased and both businesses as well as consumers used broadband for ever more purposes.

The underlying nature of this impact may have taken many forms – broadband may have induced new consumer and business applications, which enabled companies to realize productivity gains. It may also have fostered competition (e.g., thanks to price comparison websites), which enabled more productive companies to grow at the detriment of others. It may also have created entire new markets (e.g., online gaming), or fundamentally changed existing industries (e.g., travel). In general, these effects should have resulted in a positive correlation between broadband penetration and productivity growth. Potentially, the effect was characterized by either a critical mass threshold, or a saturation point. The critical mass threshold represents a minimum penetration rate that needs to be reached in order for broadband to actually yield an effect on output. The saturation point on the other hand denotes a diffusion level above which additional increases in broadband penetration do not lead to higher output.

However, the results of the analysis suggest that broadband-dependent industries did not experience over-proportional productivity growth during the first years when broadband was rolled out across the OECD. This finding is consistent across specifications and applies to broadband introduction as well as broadband diffusion. In contrast, I find some evidence that broadband may have fostered productivity growth in industries that traditionally had a low level of R&D intensity, in particular within manufacturing.

Overall, broadband does not seem to have had a positive impact on productivity across the OECD, at least not in a systematic way across countries during the first years following its adoption. Therefore, studies that claim that the introduction of broadband led to strong increases in output or growth should be interpreted with care. Broadband may still have generated important consumer benefits that do not enter official output statistics, or generate benefits that materialize in the longer run. Further research will be required to identify the specific conditions under which high-speed internet can contribute to economic growth. Recent findings by Forman, Goldfarb and Greenstein (2012) suggest that internet's complementarity with other production factors, particularly human capital, may play a crucial role.

Determinants of infrastructure expenditures: Evidence from U.S. states

Chapter 6 focuses on the determinants of infrastructure provision. Investments in infrastructure depend on a number of factors, including geography and a country's level of development. The strongest impact, however, seems to stem from institutions. Already Adam Smith (1778) recognized how differences in the institutional set-up across European countries had a direct influence on road maintenance in the 18th century.

More recently, Persson and Tabellini (1999, 2004) provide empirical evidence on the provision of public goods under different democratic regimes, and Acemoglu (2005) proposes a theoretical model that explains why governments in weak states tend to invest less in public goods than stronger states. Also the quality of the administration seems to matter: Rauch (1995) argues that the professionalization of the bureaucratic apparatus in U.S. cities at the beginning of the 20th century had a positive impact on growth thanks to higher investments in infrastructure.

I contribute to the literature with an analysis of how parties, political competition, and other institutional factors affect infrastructure expenditures in U.S. states. An analysis at the state level has the benefit that a number of factors – for instance property rights or the national institutional set-up – are the same for all states. Moreover, there have been numerous reports in recent years on the worrying condition of America's infrastructure (see, e.g., Economist, 2011, Apr 28). We may therefore expect that over the past decades infrastructure expenditures have not been at their optimal level, with variation across states and time.

Existing studies suggest that Republican state governments tend to spend more on physical infrastructure than Democrats, despite their preference for a lower government share overall. Moreover, Besley, Persson and Sturm (2010) show that infrastructure expenditures seem to increase with higher levels of political competition. However, these existing studies rely on aggregate measures of capital outlays and do not account for potential heterogeneous effects across different infrastructure classes.

I employ a broad definition of infrastructure expenditures and examine four categories of transportation and social infrastructure based on U.S. state-level data over the period 1970-2000. Infrastructure expenditures are often considered to be a bi-partisan priority. Therefore, I test the hypothesis whether divided governments raise infrastructure expenditures compared to unified Democratic or Republican control. In the analysis, I exploit within-state variation in the type of government over time and control for a number of state- and time-specific variables, including contributions from the federal government.

The results suggest that divided governments do raise expenditures for highways, but not for other types of infrastructure such as hospitals and parks. The impact is more pronounced for capital outlays than for total expenditures. Unified Republican governments also tend to raise expenditures for highways compared to unified Democratic control, but the increase is not statistically significant and smaller in magnitude. An analysis of total state expenditures confirms that the increased level of highway expenditures is not simply the result of an overall increase of the state budget.

As an extension, I consider an alternative definition of political competition based on the vote shares of parties in state-wide elections. The findings suggest that the positive impact of political competition on highway expenditures is the outcome of the division of government rather than the result of intensified electoral competition.

I also study the impact of two additional institutional features that may affect infrastructure expenditures, the role of gubernatorial term limits and the presence of taxation or expenditure limits. Term limits may change incentives for governors during terms when they cannot be re-elected. Taxation or expenditure limits may discourage infrastructure expenditures, especially in times of economic stress. Yet the results imply that also these features do not affect expenditures in a systematic way.

Finally, I test for a potential election cycle effect and find that governments tend to increase highway expenditures in the year prior to elections, pointing towards an opportunistic timing of these expenditures.

Taken together, the findings suggest that infrastructure expenditures are not subject to major partisanship influences, but that political competition in the form of divided government may affect investments in core economic infrastructure. As more data on infrastructure quality becomes available, it will be interesting to see whether political competition also influences the provision of infrastructure via an effect on the efficiency of infrastructure expenditures. In particular, higher political competition might reduce politically motivated expenditures – so-called 'pork barrel' spending[7] – and lead to an increase in productive infrastructure investments.

[7]See Kemmerling and Stephan (2002), Castells and Sollé-Ollé (2005), Cadot, Röller and Stephan (2006), and Sollé-Ollé (2009) for analyses of the pork-barrel aspect of infrastructure investments in European countries.

Chapter 2

Impact of highways on local tax bases and tax rates: Evidence from Switzerland

2.1 Introduction[1]

Commuting time to work and the related costs are decisive factors in people's residential location choices. If these parameters change, e.g., due to the advent of a new highway, people may revisit their original location decision and move to another place. In countries with strong local tax autonomy, the resulting changes in the tax base may trigger second-order effects on municipality tax rates.

In this chapter, we examine the effect of improved accessibility on local tax bases and tax rates, exploiting variation in municipalities' accessibility over time resulting from the construction of the Swiss highway network. Switzerland provides an ideal setting for two reasons. First, Swiss municipalities enjoy a high degree of fiscal freedom, and changes in local tax rates are transparent to individuals given that local tax rates are expressed as a multiplier on the cantonal tax rate. Second, the Swiss highway network was to a large extent defined in 1960 by the federal parliament to connect Switzerland's largest cities, but only gradually constructed over the decades that followed. From the perspective of a non-agglomeration municipality, the opening date of a new highway section in its region can be regarded as close to random and exogenous to local development. We exploit this variation over time to identify the effect of the opening of a new highway access point on the total number of taxpayers, income per capita, the share of taxpayers in different income categories, and local tax rates in municipalities located at different distances from the new highway access points.

Based on our analysis for the period 1971-2010, we find that both population and income per capita rose in non-agglomeration municipalities within 10 km reach of newly

[1]This chapter is joint work with Raphaël Parchet.

opened highway access points. The impact was most pronounced in municipalities close to metropolitan centers, and later in time when the highway network was nearly complete and space in cities became scarce. In contrast, we find no such positive effects for municipalities located between 10 and 15 km away, nor for agglomerations.

In a second step, we analyze the resulting impact on local tax rates. A rise in the population and income per capita can be viewed as a revenue increase for municipalities at pre-existing tax rates[2]. We examine whether municipalities close to new highways adjusted their local tax rate as a result of the increase in the tax base. We show that municipalities close to access points reduced their tax rates after the opening of the highways, with a time lag of approximately 10 years.

Our analysis contributes to four main strands of the literature. First, it adds to the growing body of literature that exploits spatial variation to analyze the impact of transportation infrastructure on various economic outcomes such as regional output (Ahlfeldt & Feddersen, 2010; Banerjee, Duflo & Qiang, 2012; Storeygard, 2013), regional earnings by industry (Chandra & Thompson, 2000), urban development (Atack et al., 2009; Duranton & Turner, 2012), trade (Donaldson, 2010; Donaldson & Hornbeck, 2013; Duranton, Morrow & Turner, 2014; Faber, 2012; Volpe Martincus & Blyde, 2013), firms (Datta, 2012; Ghani, Goswami & Kerr 2012; Gibbons et al., 2012), or labor market outcomes (Michaels, 2008; Sanchis-Guarner, 2012)[3].

One of the challenges of these studies is that many of them rely on spatial variation only for identifying the effect of transport infrastructure improvements. To account for potential endogeneity of the location of today's transport infrastructure, the authors employ different instrumental variables strategies (e.g., based on historical maps, as in Duranton and Turner, 2012), or they argue that the placement of the transport network was exogenous to the regional economic development (see, e.g., Chandra and Thompson, 2000, who introduced this line of reasoning). Still, the common trend assumption underlying these analyses is fundamentally untestable. Would regions close to a highway indeed have had the same growth path as regions further away in the absence of the highway?

There are several reasons that may cast doubt on such an assumption. First, even without the highway, municipalities along the route of the new highway may have a higher degree of accessibility than regions further away in the periphery and therefore follow a different growth path. For instance, municipalities along the route between two major cities may benefit from an increased trade level, or from more revenues in the food service and accommodation industry. Second, municipalities further away from highways may be characterized

[2]Income tax revenue is the main source of financing for municipalities, accounting for 70% of all tax revenue (35% of overall revenue). In contrast, corporate taxes make up for only 13% (7%) (averages over the period 1990-2011 calculated based on data from the Swiss Federal Department of Finance).

[3]Other studies focus on highlighting the positive association between a higher degree of accessibility and economic outcomes, rather than on identifying causals effects (see, e.g., D'Costa et al., 2013, for a recent example from the United Kingdom).

by different geographical conditions (e.g., mountainous regions), which – independent of any accessibility criterium – may lead to a differential growth path.

Our study addresses this issue by exploiting differences in the timing of highway access. In that respect, our identification is closest to the analysis by Chandra and Thompson (2000), who also exploit variation across space and time. However, in their case variation across time is limited, and they do not control for different growth trends across the counties in their sample. At the beginning of their observation period, average earnings per capita were different in the highway counties, adjacent counties, and the control counties that were not affected by a highway at all. These differences may have led to diverging growth trends independent of the availability of highways and thus undermine the common trend assumption.

Second, our study provides empirical evidence on the residential location choices of individuals. In that respect, it is most closely related to studies examining the impact of infrastructure on the spatial distribution of economic actors. For the U.S., Baum-Snow (2007) shows that limited-access highways passing through city centers led to a substantial decrease in the population of cities in the second half of the 20th century. Ahlfeldt and Wendland (2011) report a similar effect for the rapid transit network in Berlin, and Baum-Snow et al. (2014) for urban railroads and highways in Chinese cities after 1990. Moreover, in a recent working paper, Bird and Straub (2014) investigate the impact of the expansion of the Brazilian road network on the spatial allocation of population and economic activity, using the creation of the capital city Brasília in 1960 as natural experiment.

By focusing on the impact of infrastructure on people's places of residency, we account for the fact that a large portion of the observed changes induced by highways may be of distributional nature, rather than that they constitute net economic gains[4] (see Chandra & Thompson, 2000, and Chapter 3 of this thesis). Grotrian (2007, p. 49) notes that the rise of the car in Switzerland after World War II had a crucial impact on the spatial distribution of economic activities, with an increasing separation of places of work, residency, and leisure. We provide empirical evidence for this development.

Third, and as novelty in the literature on the impact of infrastructure investments, we analyze the effects of a new transportation infrastructure on tax rates. In our analysis, we show that infrastructure investments at the federal level may constitute an exogenous increase in tax bases at the local level, which can trigger subsequent changes in local tax rates. Under the assumption of economies of scale in the provision of public goods, higher tax receipts allow municipalities to conduct additional investments or reduce the local tax rate (see Frey, 1979, p. 64). Our results suggest that municipalities indeed decrease their tax rates. Further research will be required to understand to what extent municipalities also

[4] Or as Chandra and Thompson (2000) put it: "a rearrangement, but not increase, in economic activity" (p. 488).

provide additional public goods rather than keeping their expenditure level fixed[5].

Finally, the analysis is of particular interest for policymakers in Switzerland. So far, several studies have analyzed the impacts of certain parts of the highway network, mostly in the form of case studies[6]. To the best of our knowledge, this study is the first to analyze the effects in a more systematic manner.

The remainder of the chapter is structured as follows. Section 2.2 provides background on the Swiss highway network. Section 2.3 describes the data underlying our analysis, including the construction of the database on highway access points. Section 2.4 outlines our empirical strategy. Section 2.5 presents the results of the analysis of the impact on the tax base. Section 2.6 contains the findings for the impact on tax rates. Section 2.7 provides some robustness checks, and Section 2.8 concludes.

2.2 Background on the Swiss highway network

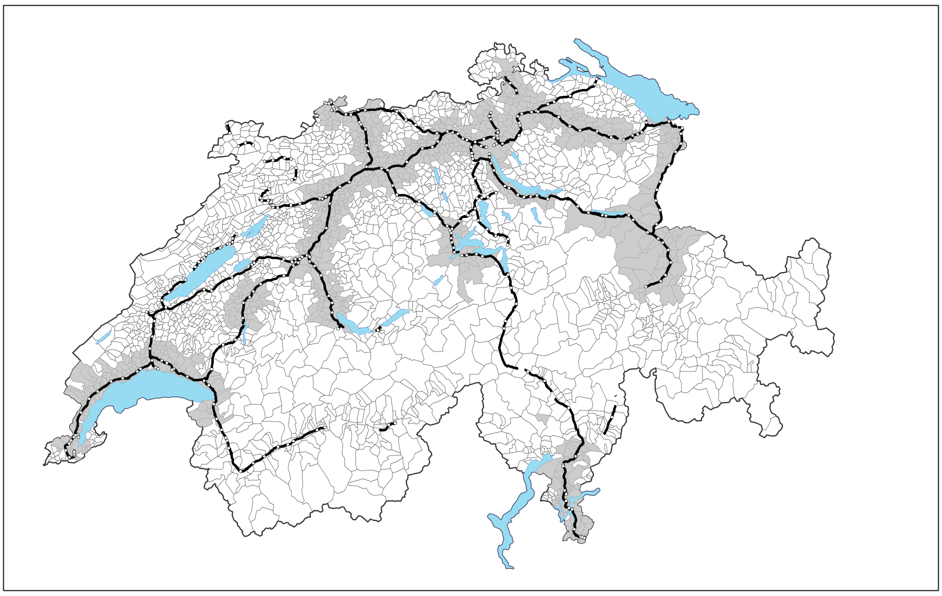
Compared to some of its neighboring countries such as Germany and Italy, Switzerland began relatively late to construct its own national highway network. After World War II, the number of cars in Switzerland experienced a strong increase: from 18,000 personal cars in 1945 to 150,000 in 1950 and 500,000 in 1960 (Grotrian, 2007, p. 44). As a result, motorized traffic rose strongly and in the late 1950s, the federal government created a planning commission for a national road network. Before 1950, several ideas for highway projects in specific regions (e.g., between Berne and Thun) had been put forward, but none had been realized (see Blum, 1951, pp. 137-144).

The commission analyzed different options for the scope of the future road network, based on the guiding principle that the national road network should only serve the most important transport needs, i.e., primarily long-distance travel (Planungskommission, 1959, Band 2, p. 1). The proposal for the national road network not only consisted of highways, but of three different types of roads, including some class III roads that were opened to non-motorized traffic as well. In contrast, class I and class II roads were restricted to motorized travel, with class I roads always requiring a complete separation of the directions of travel and at least four lanes, two in each direction (Planungskommission, 1959, Band 1, pp. 65-66). In 1960, the Swiss parliament passed the national roads law and thus also defined the

[5]Wildasin (1988) points to the possibility that local jurisdictions may optimize over their expenditure level and let their tax rate adjust residually through their budget constraint.

[6]Barbier et al. (1978) study the local effects of the highway between Geneva and Lausanne 12 years after its opening. Maggini (1979) analyzes the impact of the highway construction within the canton of Ticino on the localisation of firms. Peyer (1982) studies the impact of the Gotthard Road Tunnel on the municipality of Airolo during the first year after its opening. Service d'étude des transports (1983) review the impact of the Gotthard Road Tunnel on the transportation of goods during the first years following its opening. Bruns et al. (2011) look at the more recent expansion of the highway A3 between Frick and Brugg. Finally, Chapter 3 of this thesis examines the net economic gains of the highway between Geneva and Lausanne and the Gotthard Road Tunnel.

Figure 2.1: Municipalities with a highway access within 10 km in 1971/1972



future location of highways in Switzerland to a large extent.

The subsequent construction of the network spanned over several decades, with cantons being responsible for detailed planning and actual construction work. As one of the first sections, the highway between Geneva and Lausanne was opened to public in 1963/1964, on time for the 1964 national exhibition in Lausanne. A substantial portion of the highway A1 connecting Geneva in the west with St. Gallen in the east was completed in the late 1960s. Other highways constructed during this early phase included a large portion of the A3 linking Zurich with the canton of Grisons, and first highway sections in the canton of Ticino. Other parts of the network, including the construction of the A2 crossing Switzerland from north (Basel) to south (Chiasso), followed a few years later.

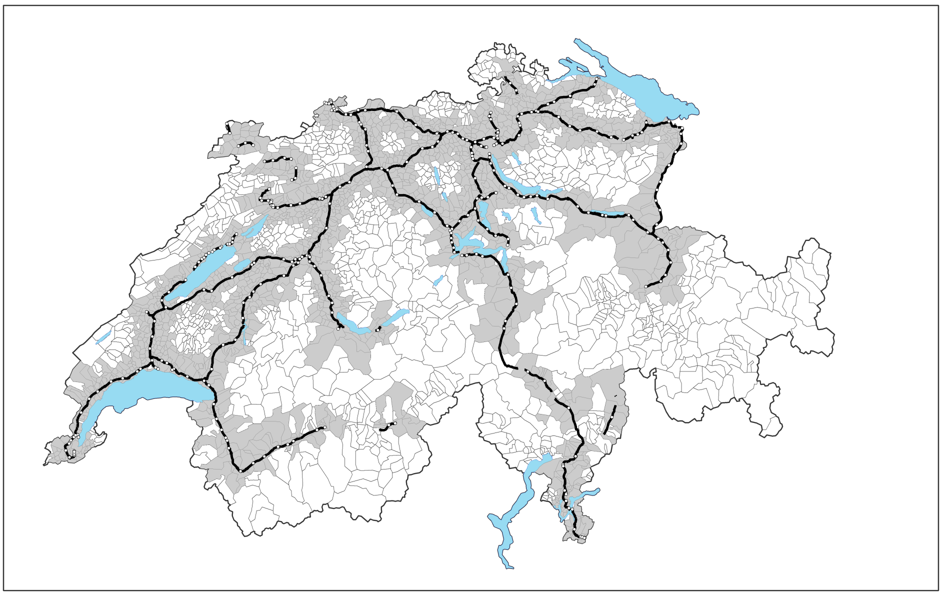
Figures 2.1 and 2.2 contain a map of Switzerland's municipalities and its national motorway network as of 2012[7]. All municipalities with an access point located within 10 km reach are shaded in grey. Figure 2.1 depicts the status for 1971/1972, the beginning of our observation period, and Figure 2.2 for 2009/2010, the end of our observation period.

Over time, several sections of the network were subject to capacity enhancements (e.g., additional lanes, city by-passes). The spatial reach of the network, however, has remained largely unchanged until today, with a few notable exceptions, including the Gotthard Road Tunnel, whose construction was passed by the federal parliament in 1965.

Besides the cantons, also municipalities and the general public (e.g., land owners) were granted a say in the planning process of the highway network. As a result, the construction of the Swiss highway network was characterized by a 'certain inertia' not present in other countries (Ruckli, 1966, p. 7). Moreover, starting in the 1970s, there was growing opposition

[7]See Section 2.3 for a definition of motorways and other types of highways.

Figure 2.2: Municipalities with a highway access within 10 km in 2009/2010



against new highway construction by environmental groups, which further slowed down the process (Schärer, 1999). In 1978, the Federal Council established a commission to reevaluate the expected benefits of six highway sections that were planned, but not yet constructed. The recommendation by the commission was to keep all investigated sections in the network, with the exception of one (see Kommission zur Überprüfung von Nationalstrassenstrecken, 1981). In 1986, the national parliament decided differently, removing two other sections from the network (Fischer & Volk, 1999).

Today, the Swiss national road network has a defined length of 1,892.5 km, only slightly more than the originally planned network of 1,840 km. Of these national roads, roughly 150 km still have to be built (Bundesamt für Strassen, 2013). In addition, some cantons have also established cantonal highways that are not part of the national road network. These highways represent only a small fraction of all highways, however.

Until 2007, construction and maintenance of the national roads was the responsibility of the cantons, under supervision of the federal government. However, the cantons received substantial financial contributions from the federal government (Ruckli, 1966). These federal funds came from taxes on gasoline, payments from the general budget, and since 1985 from a yearly lump-sum user fee to use the national highways (the so-called 'vignette'). In the initial years between 1959 and 1965, the average funding share of the federal government equaled 86 percent (Ruckli, 1966, p. 9). Cost of highway construction rose substantially over time, soon reaching a multiple of the originally projected cost. By 1996, the estimated cost of the network had increased tenfold (Heller & Volk, 1999). As of January 1, 2008, the national roads and all responsibilities were transfered to the federal government (Galliker, 2009).

The national road network plays a crucial role for traffic both within Switzerland and transit across Europe. The national roads account for only 2.5 percent of all roads in Switzerland, but carry more than 40 percent of all motorized traffic (Bundesamt für Strassen, 2014).

2.3 Data

2.3.1 Highway access points

Our highway access database is based on all access points contained in the VECTOR200 database from the Swiss Federal Office of Topography swisstopo (version as of 2013). For each access point, we identified the opening date based on a list with highway section opening dates provided by the Swiss Federal Roads Office (ASTRA). For access points for which the list from ASTRA did not contain an opening date, we relied on information presented in Fischer and Volk (1999), historical maps from swisstopo accessed via their website, and on other public information sources (press releases, newspaper articles, etc.). For access points that were subject to capacity enhancements, we used the year when a new section was originally opened to public, rather than when it was upgraded later. One limitation of this definition is that some important upgrades of the network (such as the Kerenzerberg Tunnel along Walensee) are not captured in our database.

We limit our analysis to highways that have the status of national motorways ('Autobahnen') at the end of 2012, i.e., we do not take into account national dual-carriageways ('Autostrassen') nor cantonal highways. According to the swisstopo classification, these national motorways encompass all fast-traffic controlled-access national roads with at least two lanes in each direction and a dividing strip[8]. National dual-carriageways are relatively rare and often serve regional transportation needs. Similarly, cantonal highways (motorways and dual-carriageways) were not necessarily built in order to connect Switzerland's large cities, but to improve accessibility of specific cities or regions[9]. Therefore, their construction is prone to be endogenous to local economic development.

For each town and year, we use the ArcGIS software to calculate the minimum road distance to the next highway access point based on the non-highway road network listed in the VECTOR200 database. These distances serve as proxy for the actual road distance between the place of residency in a municipality and the next highway access point. The actual road distance may slightly deviate from this measure for two main reasons: First, a municipality may contain more than one town and not all residents live at the town center.

[8]National dual-carriageways are defined by swisstopo as national motorways without a dividing strip. The speed limit is lower than for motorways.

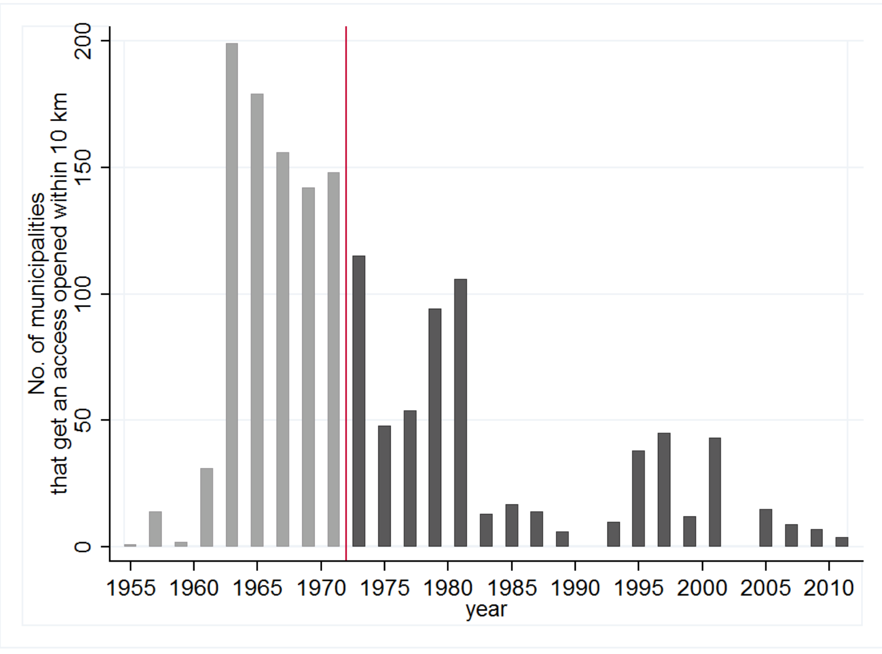
[9]We distinguish between national and cantonal motorways by the requirement of national motorways to display the national motorway tax vignette on the car.

Second, certain local roads which are part of our road network database (consisting of all roads opened at the end of 2012) may have only been constructed during the course of the observation period. However, the average area of a municipality only equals approximately 16 km^2. Therefore, these deviations should be fairly small and not systematically bias the results, as distances would be overestimated for some residents and underestimated for others. For municipalities consisting of more than one town, we use the minimum distance. The same procedure was used for municipalities that merged during the observation period.

Based on the estimated travel distance to the next highway access point, we clustered municipalities into different distance bands: 1-5 km, 5-10 km, and 10-15 km. Distance band 1-5 km includes all municipalities with a calculated road distance of up to 5.0 km, band 5-10 km all municipalities with a distance larger than 5.0 and up to 10.0 km, and band 10-15 km all municipalities with a distance larger than 10.0 km and up to 15.0 km. We exclude all municipalities with a highway access point on their territory, as well as all 908 municipalities that are part of an agglomeration based on the definition by the Swiss Federal Statistical Office in 2000, to further reduce potential endogeneity concerns (see Section 2.4.1 for details). We also do not look at municipalities located 15 km and further away from highway access points as these municipalities are to a large extent concentrated in mountainous regions in the cantons of Valais and Berne.

Figure 2.3 shows the distribution of municipalities receiving a highway access within 10 km reach, by opening year. Our sample includes all municipalities that received their highway access after 1971/1972 (in dark grey) and thus were not treated at the beginning of the observation period for which data on local tax bases and rates are available.

Figure 2.3: Number of municipalities receiving a highway access within 10 km, by opening year



2.3.2 Tax base and tax rates

Data on the tax base are constructed using individual-level data from the Swiss federal income tax statistics. These statistics encompass all taxpaying units (individuals and households, depending on the marital status) subject to the federal income tax, thus excluding taxpayers with annual income below a certain threshold (CHF 16,000 for singles and CHF 27,000 for couples in 2008)[10]. Data cover the years 1971 through 2010. They are available on a two-year basis from 1971 to 2000 and then on a yearly basis. We therefore aggregate all data into two-year averages[11].

Our two main measures of the tax base are the number of taxpayers and per capita taxable income. For the latter, we use population rather than the number of taxpayers as denominator to avoid unnecessary variations due to changes in the federal tax law (e.g., on minimum annual income below which households are not subject to the federal income tax). We also investigate distributional effects by focusing on the number of taxpayers with income above some income percentiles (median, 75th and 90th percentile). Percentiles are calculated on the basis of the nation-wide population of taxpayers for each tax period[12]. They correspond to pre-tax income of CHF 44,000, CHF 70,200, CHF 106,900 in 2009 for the 50th, 75th and 90th percentile, respectively[13].

Tax rates refer to cantonal plus municipal plus church taxes on personal income and are calculated for different income percentiles[14]. Details on the calculation of these tax rates can be found in Parchet (2014).

2.3.3 Other explanatory variables

Besides our variable of interest, highway access, we control for several municipality characteristics including the percentage of foreigners, the percentage of young (below 15) and old (above 80) people, the percentage of unemployed, the percentage of workers employed in the secondary and in the tertiary sector, and the number of movie theaters within 10 kilome-

[10]This threshold is higher than the one used for cantonal and municipal income taxes. Our measures thus miss low-income taxpayers that are subject to personal income taxes at the local level and therefore are only an approximation of the relevant tax base.

[11]The tax collection changed from a bi-annual praenumerando system to an annual postnumerando system during the early 2000s (the exact timing varies by canton). In a praenumerando system, the taxable income is computed on the basis of income earned during the two preceding years. We thus assign tax base statistics of a given fiscal period to the two preceding years. Our first tax period, 1971/1972, is therefore listed in statistics available for the fiscal period 1973/1974. Missing data due to the change from a praenumerando to a postnumerando tax system are replaced by linear interpolation.

[12]We correct for taxpayers not paying the federal income tax in the following manner. For each fiscal period we predict the total number of taxpayers as the population of unmarried adults aged over 18 plus the number of married adults divided by two. This prediction is adjusted by a fixed correction factor based on the average difference between predicted and actual total number of taxpayers for years 2003-2009. Taxpayers that do not pay a federal income tax according to this prediction are given an income of zero.

[13]The average exchange rate over our sample period is 1.60 Swiss francs (CHF) to the US dollar.

[14]Church tax rate is computed for the confession of the majority in each municipality based on the population census of 2000.

ters[15]. Data come from the Swiss Federal Statistical Office and are available for decennial census years, with the exception of the number of movie theaters which are yearly data.

2.3.4 Summary statistics

Table 2.1 shows the mean values for different sub-samples of the data. Column (1) refers to all municipalities, and Column (2) to all municipalities that did not have a highway access within 10 km distance in 1971/1972 (= beginning of observation period). Column (3) further excludes all agglomeration municipalities, and Column (4) only includes municipalities that got a highway access within 10 km reach during the observation period.

Overall, the differences between the sub-samples are relatively small. However, restricting the sample to only treated municipalities that got a highway access during the observation period (Column 4) indicates that these municipalities are on average located closer to the next agglomeration center. Therefore, they might follow a different development path independent of highway access. We address this issue in our empirical specification.

2.4 Empirical strategy

Our analysis examines the impact of highways on local tax bases of individuals. Therefore, the analysis is more closely related to models of residential location choice than models in the new economic geography tradition following Krugman (1991) that focus on economic activity. Models of residential location choice take various forms and – given the high interdependencies of the various elements influencing the decisions – are complex in nature. Pagliara and Wilson (2010) provide an overview of the current state-of-the-art in residential location modelling.

For the derivation of the hypotheses to be tested as part of our empirical analysis, we draw on a key insight from an early contribution to the field by Lowry (1964). Lowry points out that "with trivial exceptions, such as the bus fare paid to domestic servants, the individual employee pays his own transportation costs out of a standard wage; and he is able to modify the amount of this payment by his choice of residential location much easier than by his choice of job" (p. 31). As a result, commuting costs – both actually incurred costs as well as commuting time – are a key factor in people's residential location decisions.

Clearly, commuting cost is not the only factor influencing residential location choices. Equally important are the availability and price of housing. Lowry (1964) notes that an individual household's optimization problem therefore is to find a combination of housing cost and commuting cost that best fits its preferences, respecting the budget constraint.

[15]The number of movie theaters are computed using the road network as of 2012. Note that we do not control for population, for which our dependent variable serves as proxy.

Table 2.1: Mean values over observation period

Mean values	(1) All munici-palities	(2) Mun. without highway access in 1971/1972	(3) Mun. without highway access in 1971/1972, without agglo. mun. & without access point	(4) Mun. without highway access in 1971/1972, without agglo mun. & without access point, with access in 1-10 km
Number of taxpayers	1,245.21	736.86	445.79	459.85
	(5,170.77)	(1,396.25)	(532.67)	(522.16)
Income per capita (in 1,000 CHF)	20.22	18.72	17.40	18.19
	(10.72)	(9.72)	(8.75)	(10.15)
Share of taxpayers below 50%	0.42	0.46	0.48	0.44
	(0.12)	(0.12)	(0.11)	(0.10)
Share of taxpayers - top 50%	0.58	0.54	0.52	0.56
	(0.12)	(0.12)	(0.11)	(0.10)
Share of taxpayers - top 25%	0.26	0.23	0.20	0.23
	(0.11)	(0.10)	(0.08)	(0.08)
Share of taxpayers - top 10%	0.10	0.08	0.06	0.08
	(0.07)	(0.06)	(0.04)	(0.04)
Tax rate on top 10% income	15.26	15.59	15.86	15.97
	(2.63)	(2.68)	(2.58)	(2.53)
Highway access within 10 km	0.54	0.28	0.24	0.68
	(0.50)	(0.45)	(0.43)	(0.47)
% Foreign nationals	10.10	8.34	6.90	7.76
	(8.61)	(7.56)	(6.84)	(7.05)
% Young (< 15)	21.46	21.76	21.97	22.04
	(4.80)	(4.97)	(5.10)	(4.56)
% Old (>= 80)	2.94	3.12	3.26	3.02
	(1.77)	(1.90)	(1.95)	(1.72)
% Workers in secondary sector	34.60	34.56	34.07	35.52
	(13.69)	(13.55)	(13.50)	(13.29)
% Workers in tertiary sector	50.10	47.43	44.73	45.43
	(17.26)	(16.83)	(16.15)	(15.54)
Unemployment rate (%)	1.62	1.56	1.48	1.49
	(1.83)	(1.83)	(1.84)	(1.78)
No. of movie theaters within 10 km	2.93	1.68	1.10	1.64
	(5.65)	(3.35)	(1.80)	(2.00)
Part of agglomeration	0.37	0.22	0.00	0.00
	(0.48)	(0.42)	(0.00)	(0.00)
Distance to agglom. center (km)	15.27	17.42	19.10	13.94
	(13.03)	(14.54)	(14.54)	(8.41)
No. of observations	52059	33537	25893	9282
No. of municipalities	2479	1597	1233	442

Standard deviation in parentheses

The opening of a new highway reduces travel times in the respective area and thus increases the number of potential places of residency within the same reach of a given work place. As a consequence, municipalities that experience an improvement in accessibility become more attractive as potential residential places. Such an effect may in particular be relevant for regions close to a metropolitan area where housing is still comparably cheap. The impact likely is stronger for new highway access points opened later in time when available space in cities is scarce and the highway network close to complete.

Besides commuting and housing costs, there are various additional factors that may influence an individual's residency decision. These factors include the availability of certain services (e.g., a nearby kindergarten or school), the presence of amenities (e.g., theatres or unspoilt nature), or the proximity to friends and relatives, among many others. Brueckner, Thisse and Zenou (1999), for instance, highlight the importance of amenities for the location decisions of the rich. Barbier et al. (1978) show based on evidence from the Lake Geneva region that people preferrably do not move to municipalities directly around highway access points, but to nearby municipalities that offer nice housing. In Switzerland, local tax rates are another important driver of location choices, particularly for high-income earners.

Income per capita is expected to increase as people attracted by a new highway access often work in nearby cities with a higher wage level than outside of metropolitan areas. The shift towards higher incomes will also be reflected in the income distribution, with a higher share of above-median income earners.

The described effects are expected to happen at the pre-existing local tax rates. However, as a second-order effect, the changes in the spatial distribution of the tax base may in turn have an impact on local tax rates. Higher tax receipts from the increase in the number of taxpayers and income per capita imply higher public good provision at pre-existing local tax rates through the municipality budget constraint. Under the assumption of economies of scale in the provision of public goods, municipalities can either provide additional goods (e.g., new recreation facilities such as a swimming pool) or reduce the tax rate. If a potential effect on tax rates is indeed the result of the increase in the tax base following the newly opened highway access, the impact should only materialize with a time lag compared to the increase in the tax base.

2.4.1 Identification

Many of the existing papers analyzing the effects of infrastructure on economic development face the challenge that they can exploit variation across space only, which is problematic as the common trend assumption is untestable (see, e.g., Datta, 2012, p. 55). Even before the opening of the highway, peripheral regions that are located along the direct route between two larger cities may follow a different growth and development path than those located further away. In the Swiss case, for instance, the Leventina valley in Switzerland's Italian speaking canton of Ticino in the South benefited from the traffic that had cross the Gotthard pass in terms of car repair and tourist nights even before the road tunnel was opened in 1980. Moreover, there are topological reasons why the highway passes through this valley (which also hosts industry and military) and not through other valleys. As a result, even before the advent of the highway, this valley had a higher degree of accessibility than other regions in Ticino. It was also better connected to the train network.

We address the issue resulting from exploiting spatial variation only in three ways. First, we rely on a panel data set in which the timing of the treatment (i.e., opening of the highway access point) differs across the various sections of the highway network. We can thus make use of both variation across space (distance to the next highway access point) and time (year of opening) to identify the effect of the highway on the local economic variables. Second, we include a municipality time trend that captures unobservable differences in the growth rate that are independent of the highway opening. Third, in some specifications we restrict the sample to municipalities that are being treated during the observation period and exploit only variation across time. Restricting the sample to municipalities that all are located in the same distance band to the next highway further increases the comparability of municipalities in the sample (e.g., exclusion of mountain villages) and thus further mitigates a potential bias from differing growth trends across municipalities.

Building on an idea pioneered by Chandra and Thompson (2000), we focus on non-agglomeration municipalities to reduce potential endogeneity-induced biases[16]. Moreover, the advent of highways may have had a different effect for municipalities in agglomerations than for those outside of agglomerations. We also exclude all municipalities with a highway access point on their territory given that the exact positioning of a highway access point along the route may have been driven by regional economic considerations[17].

We do not instrument the highway access variable given that from a non-agglomeration municipality point of view, the specific year when the municipality got access to a newly opened nearby highway can be regarded as close to random[18] and exogenous to local economic development. As pointed out in Section 2.2, the Swiss highway network was defined to a large extent in 1960, but only gradually constructed over time. The prioritization in construction was mainly conducted based on inter-city transportation considerations, not to connect certain municipalities outside of agglomerations to the highway network. Besides, the opening of certain highway sections was subject to substantial delays, e.g., due to opposition by environmental groups, creating additional randomness in the timing. Finally, also the placement of the highways was subject to a random component, at least in those parts of Switzerland where several alternative routes had initially been considered for the location of the highway (e.g., between Berne and Lausanne).

Table 2.2 shows that in period 1971/1972, municipalities that got a highway access within 10 km during the first vs. second part of the observation period were not systematically different in terms of numbers of taxpayers nor income per capita. For both variables, the difference of the means is statistically insignificant. This finding provides comfort that the opening year is not driven by the key dependent variables. There is also no evidence that municipalities receiving a highway access in the first part of the observation period are located closer to nearby agglomerations. In contrast, the differences for some of the other variables, including the population shares according to income brackets and the tax rate for the top ten percent income, are statistically significant. However, the differences are

[16]Chandra and Thompson (2000) note that "interstate highways typically connect two metropolitan areas, therefore, the non-metropolitan counties that they pass through may be thought of having received a new highway as an exogenous event" (p. 482).

[17]The exclusion of municipalities with an access point on their territory does not affect the findings. Results including these municipalities are available from the authors upon request.

[18]This line of argument goes back to Strittmatter and Sunde (2013), who analyzed the impact of the introduction of public health care on economic development in European countries.

Table 2.2: Comparison of municipalities receiving a highway access within 10 km early vs. late during the observation period

	Mean values for period 1971/1972 for restricted sample of municipalities			
	(1) All opening years	(2) Access opened 1973-1990	(3) Access opened 1991-2010	(4) Difference
Number of taxpayers	255.43	267.09	228.41	-38.68
	(292.71)	(309.23)	(249.29)	
Income per capita (in 1,000 CHF)	5.43	5.44	5.41	-0.02
	(2.72)	(2.91)	(2.23)	
Share of taxpayers below 50%	0.33	0.32	0.35	0.02**
	(0.09)	(0.08)	(0.10)	
Share of taxpayers - top 50%	0.67	0.68	0.65	-0.02**
	(0.09)	(0.08)	(0.10)	
Share of taxpayers - top 25%	0.24	0.25	0.23	-0.01
	(0.09)	(0.09)	(0.08)	
Share of taxpayers - top 10%	0.08	0.08	0.07	-0.01**
	(0.05)	(0.05)	(0.04)	
Tax rate on top 10% income	15.44	15.10	16.22	1.12***
	(4.05)	(4.41)	(2.94)	
% Foreign nationals	7.06	7.56	5.91	-1.65**
	(7.99)	(8.82)	(5.46)	
% Young (< 15)	26.38	26.40	26.34	-0.05
	(5.31)	(5.77)	(4.05)	
% Old (>= 80)	1.99	1.99	1.99	-0.00
	(1.23)	(1.30)	(1.03)	
% Workers in secondary sector	44.23	41.91	49.62	7.72***
	(15.91)	(15.31)	(16.01)	
% Workers in tertiary sector	26.39	27.65	23.46	-4.18***
	(10.19)	(10.72)	(8.16)	
Unemployment rate (%)	0.15	0.13	0.18	0.05
	(0.38)	(0.33)	(0.47)	
No. of movie theaters within 10 km	2.16	2.30	1.83	-0.48***
	(2.00)	(2.18)	(1.46)	
Distance to agglom. center (km)	13.94	14.35	13.00	-1.35
	(8.42)	(8.27)	(8.69)	
No. of municipalities	442	309	133	442

Notes. Standard deviation in parentheses. For the differences between the two sub-samples, levels of statistical significance are denoted by *** p<0.01, ** p<0.05, * p<0.10. The sample includes all municipalities that did not have a highway access within 10 km in 1971/1972, but received one during the observation period, excluding agglomeration municipalities and municipalities with an access point on their territory.

relatively small with respect to the mean value such that it is unlikely that these variables would have affected the highway opening year in a substantial way.

We focus our analysis on highways, disregarding potential expansions of the train network. This does not mean that trains are not important for people's residential decisions. However, such expansions would only be an issue for our identification if improvements in train connections were systematically correlated in time and space with the opening of highways. For our observation period, there is little evidence that this is the case. Particularly, the S-Bahn commuter train system of Zurich was only launched in 1990, years after the highways around Zurich had been constructed. Another major improvement of the public transportation offering, the introduction of the hourly timetable ensuring that the same train connections are offered at the same time every hour, had been introduced in May 1982 by the Swiss Federal Railways. However, this new schedule affected most municipalities simultaneously and thus should be captured by the year fixed effects and not interfere with our identification based on variation across time.

2.4.2 Specification

Our main specification exploits variation across time and space, i.e., analyzes which effect the opening of a new highway access point has on the development of the economic indicators in municipalities located in different reach of the new highway. We exclude all municipalities that already had access to a highway at the corresponding distance at the beginning of the observation period in 1971/1972. We also exclude all municipalities that are part of an agglomeration or have a highway access point located on their territory.

Given that we expect the effect of highways to materialize over time, we use a distributed lag model and focus on the long-term effect. In the baseline specification, we include 8 lags of the highway access variable (i.e., 16 years)[19], yielding the following regression equation:

$$y_{it} = \sum_{j=0}^{8} \beta_j \, Access_{it-j} + \gamma \, \mathbf{X}_{it} + \alpha_i + \rho_i \, t + \lambda_t + \varepsilon_{it} \tag{2.1}$$

where

- i is a municipality and t a two-year period (1971-2010)
- y_{it} is a measure of the tax base or of the tax rate (in logs)
- $Access_{it}$ is a dummy variable that takes the value 1 for municipalities with access to a highway within a road distance of a certain number of kilometers, and zero otherwise
- $\mathbf{X}_{it}$ is a vector of municipality-specific, time-varying controls: % foreigners, % of young and old people, % of employees in the secondary and tertiary sectors, % unemployment, no. of movie theaters within 10 km
- α_i is a municipality fixed effect and $\rho_i \, t$ a linear municipality time trend
- λ_t is a year fixed effect
- ε_{it} is a municipality and year specific error term, clustered at the municipality level

We are interested in the long-term impact of highway access, $\gamma = \sum_{j=0}^{8} \beta_j$. Following Davidson and MacKinnon (2004, p. 575), we therefore reparametrize equation (1) by adding and subtracting $\sum_{j=1}^{8} \beta_j \, Access_{it}$, so that we can estimate γ directly:

[19] We use 8 lags to make best use of the available data on the explanatory variables during the pre-observation period and to allow for sufficient time for the effect to materialize, in particular for the impact on tax rates.

$$y_{it} = \gamma \, Access_{it} + \sum_{j=1}^{8} \beta_j \, (Access_{it-j} - Access_{it}) + \gamma \, \mathbf{X}_{it} + \alpha_i + \rho_i \, t + \lambda_t + \varepsilon_{it} \qquad (2.2)$$

In the simplest specification, we only include one specific $Access_{i,t}$ variable, relating to all municipalities that got an access within a reach of 1-10 km to the highway. The dummy variable takes the value 1 if the distance is 1-10 km, and zero otherwise. In other specifications, we include several $Access_{i,t}$ variables (e.g., for 1-5, 5-10 and 10-15 km) to identify the long-term effect for different distance bands at the same time. We also test for heterogeneous effects with regard to certain municipality characteristics (e.g., distance to the next metroplitan center).

To account for the fact that growth paths of municipalities may be systematically different with respect to their distance from the next highway irrespective of the opening of the highway itself (e.g., due to geographical reasons), we include a linear municipality time trend in most regressions of the tax base. The most restrictive specifications exploit time variation only, i.e., restrict the sample to municipalities that got a highway access in the same distance band (e.g., 1-10 km) during the observation period.

10 km appears as a reasonable threshold for our baseline specification for several reasons. First, the typical distance between two highway stops in Switzerland equals 5-10 km. A distance band of 10 km around each access point ensures that a corridor of roughly 10 km on both sides of the highway is considered in the sample of treated municipalities. Second, a distance of 10 km to the next highway access point implies a travel time of approximately 10 minutes, the exact time depending on the ratio between within-municipality (50 km/h) versus out-of-municipality (80 km/h) travel. For daily commute to work, 10 minutes only to reach the highway seem to constitute a reasonable upper bound after which the likelihood that somebody uses the highway to work decreases. Third, as explained in Section 2.3, actual travel distances of individuals likely deviate from our proxy by up to a few kilometers, due to the geographical spread of people within municipalities. Therefore, a more narrow definition (e.g., 5 km) might exclude municipalities with individuals that are actually in close reach of the highway. Finally, to avoid the adverse effects of highways, people tend to prefer to live in municipalities close, but not directly located along a highway. Allowing for a distance of up to 10 km takes such preferences into account.

For the analysis of tax rates, we modify the specification slightly: Regressions do not include a municipality time trend; instead year fixed effects are replaced by canton-specific year fixed effects to account for changes in cantonal tax rates.

Table 2.3: Impact of highway access points on tax base

	all		1-10 km		all		1-10 km	
	(1) taxp	(2) inc/pop	(3) taxp	(4) inc/pop	(5) taxp	(6) inc/pop	(7) taxp	(8) inc/pop
Long-term effect of access 1-10 km	0.106***	0.031***	0.071***	0.047**	0.043***	0.043***	0.045***	0.044***
	(0.013)	(0.011)	(0.019)	(0.020)	(0.013)	(0.014)	(0.012)	(0.014)
% Foreign nationals	0.005***	-0.005***	0.008***	-0.003***	0.000	-0.001	0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
% Young (< 15)	-0.003***	-0.009***	-0.005***	-0.008***	-0.003***	-0.008***	-0.003***	-0.007***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
% Old (>= 80)	-0.012***	-0.002	-0.010***	-0.004	-0.003	-0.002	0.001	0.003
	(0.002)	(0.004)	(0.004)	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)
% Workers in secondary sector	0.005***	0.006***	0.008***	0.006***	0.003***	0.001	0.004***	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
% Workers in tertiary sector	0.008***	0.005***	0.011***	0.005***	0.004***	0.002**	0.005***	0.003***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Unemployment rate	0.002	0.003	-0.004	-0.003	0.003**	0.001	-0.002	0.001
	(0.002)	(0.002)	(0.003)	(0.002)	(0.001)	(0.002)	(0.002)	(0.003)
No. of movie theaters within 10 km	0.010***	0.011***	0.008**	0.008***	0.004**	0.002	-0.000	0.002
	(0.003)	(0.003)	(0.004)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)
8 lags of highway access included	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality time trend	No	No	No	No	Yes	Yes	Yes	Yes
No. of observations	24639	24639	8829	8829	24639	24639	8829	8829
No. of municipalities	1233	1233	442	442	1233	1233	442	442
R2-adjusted	0.990	0.952	0.990	0.966	0.995	0.973	0.996	0.979

Notes. *** p<0.01, ** p<0.05, * p<0.10. Standard errors (in parentheses) are clustered by municipality. Two-yearly panel for the period 1971-2010, with the log number of taxpayers respectively log income per capita as dependent variable.

2.5 Results for tax base

2.5.1 Baseline results

Table 2.3 contains the baseline results for the analysis of the long-run impact of highways on local tax bases. The regression includes 8 lags of the highway access dummy variables, the set of control variables, and municipality and year fixed effects. Columns (5)-(8) also control for a linear municipality time trend.

Columns (1)-(2) and (5)-(6) exploit variation both across time and space. Of all the municipalities that are not part of an agglomeration or have an access point on their territory, the sample covers all those municipalities that did not already have a highway access point within 10 km at the beginning of the observation period in 1971/1972. The long-term effect of getting a highway access within 10 km is positive and highly statistically significant for both the number of taxpayers and income per capita. Controlling for a municipality time trend, Columns (5) and (6) suggest that on average, both variables are approximately 4% higher 8 periods or 16 years after the opening of an access point within 10 km compared to the counterfactual case without a highway access.

In Columns (3)-(4) and (7)-(8), we restrict the sample to only municipalities that did not have a highway access within 10 km in 1971/1972, but got access in the distance 1-10 km during the observation period. We thus exploit variation in the timing of the opening of the highway access only, i.e., the treatment and control groups are the same. Variation comes from the fact that not all municipalities received their access at the same time. Benefit of this approach is that the results may not be driven by differences between the treatment group (i.e., those regions that got a highway access within a certain distance) and the control group (i.e., those regions that did not get a corresponding access). For the specification without time trends, the coefficient estimates change, suggesting that different municipality trends may indeed affect the results. In contrast, estimates are almost identical for Columns (5) and (7) and Columns (6) and (8), respectively. The linear municipality time trends therefore seem to effectively control for differential growth paths between municipalities close to and further away from highways.

2.5.2 Distance bands

Results in Table 2.3 are calculated for a single distance band of 1-10 km around the highway access point. In Table 2.4, we differentiate the impact for distance bandwidths of 5 km length and explore alternative specifications with an impact threshold of 5 respectively 15 km. The sample only includes municipalities within the corresponding threshold of $D = 5$, $D = 10$, or $D = 15$ km for the distance to the next highway access. The specification thus exploits variation across time only and includes municipality time trends. Municipalities with an access within D km in 1971/1972, agglomeration municipalities and municipalities with a highway access on their territory are excluded.

Table 2.4: Impact of highway access points on tax base – Distance bands

	D = 5 km		D = 10 km		D = 15 km	
	(1)	(2)	(3)	(4)	(5)	(6)
	taxp	inc/pop	taxp	inc/pop	taxp	inc/pop
Long-term effect of access 1-5 km	0.031	0.001	0.045**	0.037*	0.048**	0.035*
	(0.020)	(0.019)	(0.021)	(0.019)	(0.023)	(0.021)
Long-term effect of access 5-10 km			0.046***	0.049***	0.042**	0.033*
			(0.014)	(0.016)	(0.017)	(0.019)
Long-term effect of access 10-15 km					-0.011	0.006
					(0.016)	(0.020)
No. of observations	3589	3589	8829	8829	10730	10730
No. of municipalities	180	180	442	442	537	537
R2-adjusted	0.997	0.983	0.996	0.979	0.996	0.977

Notes. *** p<0.01, ** p<0.05, * p<0.10. Standard errors (in parentheses) are clustered by municipality. Two-yearly panel for the period 1971-2010, with the log number of taxpayers respectively log income per capita as dependent variable. All regressions include the set of control variables, 8 lags of highway access, municipality and year fixed effects, and a municipality-specific linear time trend.

Columns (1) and (2) show that when only looking at the municipalities between 1-5 km, the impact on population and income per capita is not statistically significantly different from zero. In Columns (3) and (4), the impact for the distance band 1-10 km is broken down into the two sub-bands 1-5 km and 5-10 km. The results suggest that the effect is fairly similar across the two different sub-bands. Columns (5) and (6) confirm this finding. Moreover, municipalities with a distance above 10 km to the next highway do not experience an increase in their tax base.

Overall, the results confirm the hypothesis that following the opening of a nearby highway access point, both the number of residents and income per capita increase in municipalities close to highway access points, even when controlling for a municipality time trend. In contrast, municipalities 10 km and further away do not benefit from the improved accessibility of the region.

2.5.3 Heterogeneous effects

Table 2.5 examines impact heterogeneity with respect to agglomerations, distance to the next metropolitan center, and evolution over time.

Columns (1) and (2) adds agglomeration municipalities and municipalities with a highway access point on their territory to the sample. The interaction term between access and being part of the agglomeration is negative and highly significant, suggesting that the positive impact of highways on population and income per capita is confined to non-agglomeration municipalities. For agglomeration municipalities, the point estimates are negative, but do not reach the 10 percentage point significance level (p-value of 0.179 for the number of taxpayers and 0.218 for income per capita).

Columns (3) and (4) test whether the distance to the next agglomeration center matters for the impact (excluding agglomeration municipalities and municipalities with a highway access point on their territory). In line with expectations, the interaction terms between access and distance to the next center have a negative coefficient. This finding implies that the positive impact of a highway access is larger, the closer a municipality is located to the next metropolitan center. This effect seems to be more pronounced for income per capita than population.

Finally, Columns (5) and (6) investigate the magnitude of the impact over time. Results in Column (5) indicate that the positive impact of highways on population comes from the highway openings in the 1990s and 2000s. This finding confirms the hypothesis that new highways had a particularly strong effect on people's residential location choices in the later phase of the highway development when space in the cities plausibly became scarce.

In contrast, Column (6) suggests that the positive impact on income per capita was relatively homogeneous across time. A potential interpretation of this finding is that in the

Table 2.5: Impact of highway access points on tax base – Heterogeneous effects

	1-10 km					
	(1) taxp	(2) inc/pop	(3) taxp	(4) inc/pop	(5) taxp	(6) inc/pop
Long-term effect of access 1-10 km	0.044***	0.046***	0.071***	0.091***	0.008	0.058*
	(0.012)	(0.014)	(0.023)	(0.029)	(0.028)	(0.031)
Long-term effect of access * agglo.	-0.070***	-0.067***				
	(0.022)	(0.020)				
Long-term effect of access * dist. to agglo. cent.			-0.002	-0.003*		
			(0.001)	(0.002)		
Long-term effect of access * access opened 1981-1990					-0.045	-0.029
					(0.032)	(0.051)
Long-term effect of access * access opened 1991-2000					0.129**	0.027
					(0.056)	(0.078)
Long-term effect of access * access opened after 2000					0.071**	0.001
					(0.036)	(0.043)
No. of observations	12978	12978	8829	8829	8829	8829
No. of municipalities	650	650	442	442	442	442
R2-adjusted	0.997	0.981	0.996	0.979	0.996	0.979
Long-term effect for agglomeration mun.	-0.026	-0.021				
Test effect = 0 (p-value)	0.179	0.218				
Long-term effect for access opened between 81 & 90					-0.037	0.030
Test effect = 0 (p-value)					0.256	0.472
Long-term effect for access opened between 91 & 00					0.137	0.085
Test effect = 0 (p-value)					0.000	0.166
Long-term effect for access opened after 2000					0.079	0.059
Test effect = 0 (p-value)					0.000	0.044

Notes. *** p<0.01, ** p<0.05, * p<0.10. Standard errors (in parentheses) are clustered by municipality. Two-yearly panel for the period 1971-2010, with the log number of taxpayers respectively log income per capita as dependent variable. All regressions include the set of control variables, 8 lags of highway access, municipality and year fixed effects, and a municipality-specific linear time trend.

early phase, a nearby highway access led to an increase in income per capita mainly by allowing residents to take on better-paid jobs in nearby cities rather than by in-migration, which became more important later.

2.5.4 Distributional effects

Table 2.6 investigates the effect of a highway access on the income distribution within municipalities for different distance bands. The dependent variable is the log of the share of taxpayers or total income reported in a municipality by different groups of taxpayers classified according to the percentiles of the nation-wide income distribution.

Column (1) shows that the opening of a nearby highway access within 1-10 km led to a decrease of the share of taxpayers with a below-median income by roughly 4 percent. The decrease seems to have been concentrated in the distance band 5-10 km (Column 5). At the same time, the shares of the population with above-median incomes increased (Column 2). The relative increase was highest for the top earners (Columns 3 and 4), and slightly more pronounced for the distance band 5-10 km than 1-5 km (Columns 6-8).

Table 2.7 examines the heterogeneity of the impact with respect to the distribution of

Table 2.6: Impact of highway access points on tax base – Distributional effects I

	Share of taxpayers (in log)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	below 50%	top 50%	top 25%	top 10%	below 50%	top 50%	top 25%	top 10%
Long-term effect of access 1-10 km	-0.043***	0.074***	0.130***	0.199***				
	(0.014)	(0.012)	(0.025)	(0.048)				
Long-term effect of access 1-5 km					-0.022	0.051***	0.117***	0.177***
					(0.023)	(0.017)	(0.033)	(0.061)
Long-term effect of access 5-10 km					-0.052***	0.082***	0.138***	0.209***
					(0.015)	(0.014)	(0.029)	(0.057)
R2-adjusted	0.855	0.836	0.835	0.802	0.855	0.836	0.836	0.802

Notes. *** p<0.01, ** p<0.05, * p<0.10. Standard errors (in parentheses) are clustered by municipality. Two-yearly panel for the period 1971-2010, with the log of different shares of taxpayers as dependent variable. All regressions include the set of control variables, 8 lags of highway access, municipality and year fixed effects, and a municipality-specific linear time trend.

Table 2.7: Impact of highway access points on tax base – Distributional effects II

	Share of taxpayers (in log)			
	(1)	(2)	(3)	(4)
	below 50%	top 50%	top 25%	top 10%
Long-term effect of access 1-10 km	0.029	0.104***	0.167***	0.238***
	(0.022)	(0.016)	(0.030)	(0.057)
Long-term effect of access * share of taxp. below med. at opening	-0.116***			
	(0.027)			
Long-term effect of access * share of taxp. above med. at opening		-0.081***		
		(0.023)		
Long-term effect of access * share of top 25% taxp. at opening			-0.179***	
			(0.045)	
Long-term effect of access * share of top 10% taxp. at opening				-0.228***
				(0.087)
No. of observations	7494	7494	7485	7337
No. of municipalities	422	422	422	422
R2-adjusted	0.857	0.839	0.837	0.805
Long-term effect for (interaction term)	-0.087	0.022	-0.012	0.010
Test effect = 0 (p-value)	0.000	0.222	0.751	0.886

Notes. *** p<0.01, ** p<0.05, * p<0.10. Standard errors (in parentheses) are clustered by municipality. Two-yearly panel for the period 1971-2010, with the log of different shares of taxpayers as dependent variable. All regressions include the set of control variables, 8 lags of highway access, municipality and year fixed effects, and a municipality-specific linear time trend.

taxpayers at the time of the opening of the highway access. Each regression includes an interaction of the highway access with a dummy that takes the value 1 if the respective share of taxpayers in the municipality at the time of access opening was higher than the national average.

The findings suggest that the results are driven by poor municipalities. In Column (1), the negative interaction term implies that the share of below-median taxpayers only decreases in municipalities with a large portion of poor households. In Columns (2)-(4), the negative interaction terms nets out the positive main effect for those municipalities that already have a high portion of high-income households. Only in municipalities with a low share of high-income earners, highways lead to an increase in the share of these households.

Table 2.8: Impact of highway access points on tax rates

	Tax rate on top 10% income (in log)		
	(1)	(2)	(3)
Long-term effect of access 1-10 km	-0.017**		-0.025**
	(0.008)		(0.012)
Long-term effect of access 1-5 km		-0.015	
		(0.011)	
Long-term effect of access 5-10 km		-0.018**	
		(0.008)	
Long-term effect of access * access opened between 1981 & 1990			0.014
			(0.013)
Long-term effect of access * access opened between 1991 & 2000			0.002
			(0.013)
Long-term effect of access * access opened after 2000			-0.025*
			(0.014)
No. of observations	8879	8879	8879
No. of municipalities	441	441	441
R2-adjusted	0.946	0.946	0.946
Long-term effect for access opened between 81 & 90			-0.011
Test effect = 0 (p-value)			0.307
Long-term effect for access opened between 91 & 00			-0.023
Test effect = 0 (p-value)			0.016
Long-term effect for access opened after 2000			-0.050
Test effect = 0 (p-value)			0.000

Notes. *** p<0.01, ** p<0.05, * p<0.10. Standard errors (in parentheses) are clustered by municipality. Two-yearly panel for the period 1971-2010, with the log local tax rate on the top 10% income as dependent variable. All regressions include the set of control variables, 8 lags of highway access, municipality fixed effects, and canton-specific year fixed effects.

2.6 Results for tax rates

Results in Section 2.5 show that municipalities in the neighborhood of highways experience an increase in the tax base, both with respect to the number of taxpayers as well as income per capita. Assuming economies of scale in the provision of public goods, those municipalities can use the additional tax receipts to either provide more or better public goods (e.g., building new public sports or recreation facilities), reduce debt levels, or they can reduce their local tax rates.

Table 2.8 shows the results of an analysis of the impact of highway access on the tax rate for an income corresponding to the top 10% of the income distribution (computed for each period). Given that tax rates are subject to common canton-specific shocks when the cantonal tax rate changes, the specification includes canton-specific year fixed effects, but no linear time trend.

The results suggest that nearby highway access induces municipalities to decrease their local tax rates on the top 10% income by approximately 2 percent in the long term (Column 1). In line with the finding that benefits of highways are concentrated in the 5-10 km distance band, the effect seems to be more pronounced for municipalities located between 5 and 10 km away from the next highway access point (Column 2). The effect appears to have been fairly constant over time, with lower decreases for highways opened in the 1980s, but stronger reductions for the most recent highways opened after 2000. The effect is materially the same for tax rates on other income thresholds (e.g., the median income)[20].

2.7 Robustness tests

2.7.1 Impact over time

The specifications in Sections 2.5 and 2.6 examine the long-term effect of a highway access after 8 periods or 16 years. However, the opening of a nearby highway access likely already leads to effects on the tax base that materialize with a shorter time lag. As a robustness check, we therefore investigate the shape of the impact over time. To do so, we employ a strategy similar to Chandra and Thompson (2000) and include dummy variables for each specific age of the highway access, measured as number of years since the opening of the highway access point. These dummy variables take a value of 1 only once for each municipality (e.g., the variable for an access within 10 km and age 4 years only takes the value of 1 exactly two periods after the municipality got access to a highway within 10 km reach), and zero otherwise. We also test for potential run-up effects by including respective dummy variables for the 4 periods (8 years) before the highway access point was opened.

The results presented in the figures are derived based on regressions using the restricted sample of municipalities that got a highway access within 10 km during the observation period, thus exploiting only variation over time. We control for the set of covariates and municipality and time fixed effects[21].

Figure 2.4 displays the results for the number of taxpayers as dependent variable. Years 16 and more are grouped together (i.e., same dummy variable) given that the number of observations decreases, the longer the lag since the time of opening.

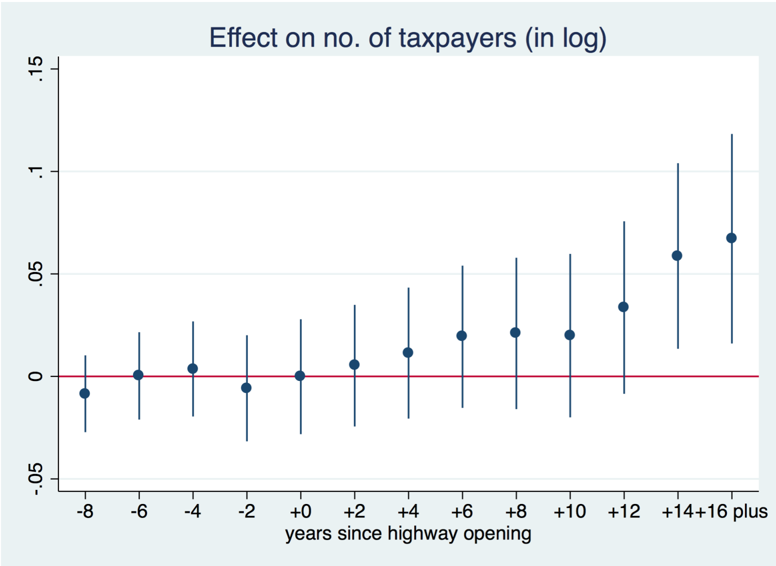
In line with expectations that the impact only materializes gradually as new housing becomes available, the impact estimates rise over time. The imprecision of the estimates is likely driven by the fact that the exact impact pattern varies between different highway access points, particularly those opened earlier and later in time. We find no evidence for pre-opening effects during the 8 years before the opening, corroborating our interpretation that the observed effect is indeed the result of the opening of the nearby highway access.

Figure 2.5 plots the same results for income per capita. The results are similar to those for the number of taxpayers, with the difference that already for the pre-opening period, there is some evidence for a positive effect on income per capita. In principle, a positive

[20] Results are available from the authors upon request.

[21] In contrast to the baseline specification, we do not include the municipality-specific linear time trends. Given that for many municipalities there are only few pre-treatment observations available, the inclusion of the time trend renders the period-by-period estimates unreliable, particularly before and around the time of opening.

Figure 2.4: Impact on number of taxpayers over time



Notes. The figure shows the point estimates of dummy variables for 8 years before and up to 16 years after the opening of the highway access. The last dummy variables takes the value 1 after 16 years and during all years thereafter. Lines denote the 95% confidence interval of the estimates. The sample includes all municipalities that did not have a highway access within 10 km in 1971/1972, but received one during the observation period, excluding agglomeration municipalities and municipalities with an access point on their territory. The regression includes the set of control variables and municipality and time fixed effects and covers the period 1971-2010.

effect prior to the opening of the highway could reflect the impact of the construction work in the region. However, it seems unlikely that construction would have generated a positive effect already 8 years before the opening. More likely, these positive point estimates are the result of non-highway related pre-treatment income dynamics among municipalities that got an access relatively late during the observation period.

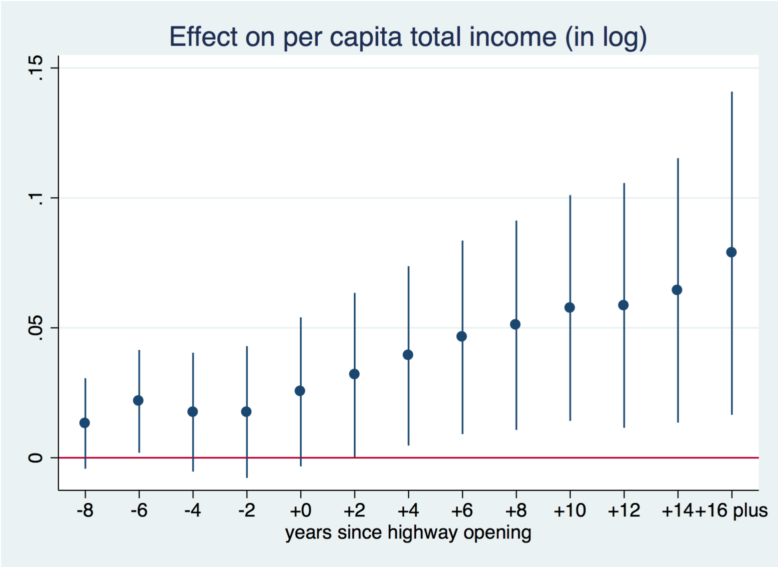
Reassuringly enough, the pre-opening impact is fairly stable and the effect starts to rise at the time the highway access was opened, consistent with a nearby highway access leading to an increase in income per capita.

For tax rates, we expect the adjustment to take place only with a considerable time lag if observed changes in the tax rates are indeed the result of highway access induced changes in the tax base. Figure 2.6 provides evidence for such a delayed impact. The decrease in tax rates only starts to materialize after approximately 10 years.

2.7.2 No control variables

The baseline specification controls for a number of municipality- and time-specific variables that may affect the outcome variables. A higher share of children, for instance, may negatively affect both the number of taxpayers and income per capita. However, these control variables are not necessarily exogenous, but may also be influenced by highway access. For

Figure 2.5: Impact on income per capita over time

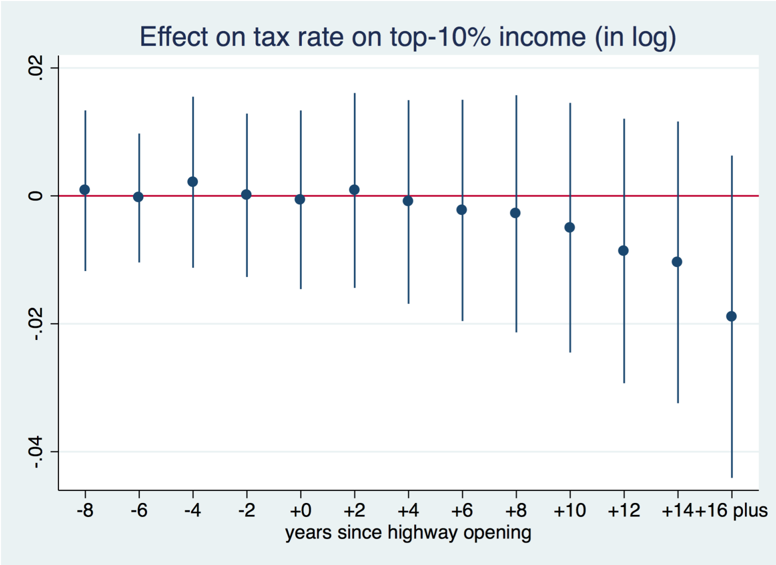


Notes. The figure shows the point estimates of dummy variables for 8 years before and up to 16 years after the opening of the highway access. The last dummy variables takes the value 1 after 16 years and during all years thereafter. Lines denote the 95% confidence interval of the estimates. The sample includes all municipalities that did not have a highway access within 10 km in 1971/1972, but received one during the observation period, excluding agglomeration municipalities and municipalities with an access point on their territory. The regression includes the set of control variables and municipality and time fixed effects and covers the period 1971-2010.

example, a nearby highway access may primarily attract taxpayers without children, leading to a decrease in the share of young in the population. The negative coefficient between the number of taxpayers and the share of children may therefore potentially be subject to reverse causation. A similar argument can be made for the other control variables.

To test for the impact of the covariates on the results, Table 2.9 reports the long-term effect of highway access for regressions that only include the highway access variables, municipality and year fixed effects, and in Columns (5)-(8) the municipality-specific linear time trends. Compared to the results in the baseline specification in Table 2.3, the findings remain materially unchanged. The point estimates in Columns (5)-(8) with municipality time trends are very similar to those controlling for the set of control variables and suggest that a nearby highway access within 10 km reach leads to an increase in the number of taxpayers and income per capita of approximately 4 percent.

Figure 2.6: Impact on tax rate on top 10% income over time



Notes. The figure shows the point estimates of dummy variables for 8 years before and up to 16 years after the opening of the highway access. The last dummy variables takes the value 1 after 16 years and during all years thereafter. Lines denote the 95% confidence interval of the estimates. The sample includes all municipalities that did not have a highway access within 10 km in 1971/1972, but received one during the observation period, excluding agglomeration municipalities and municipalities with an access point on their territory. The regression includes the set of control variables, municipality fixed effects, and canton-specific year fixed effects and covers the period 1971-2010.

Table 2.9: No controls – Impact of highway access on tax base

	all		1-10 km		all		1-10 km	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	taxp	inc/pop	taxp	inc/pop	taxp	inc/pop	taxp	inc/pop
Long-term effect of access 1-10 km	0.125***	0.023*	0.106***	0.056**	0.042***	0.042***	0.039***	0.042***
	(0.015)	(0.013)	(0.024)	(0.022)	(0.013)	(0.014)	(0.012)	(0.014)
8 lags of highway access included	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality time trend	No	No	No	No	Yes	Yes	Yes	Yes
No. of observations	24639	24639	8829	8829	24639	24639	8829	8829
No. of municipalities	1233	1233	442	442	1233	1233	442	442
R2-adjusted	0.786	0.941	0.821	0.958	0.910	0.969	0.947	0.976

Notes. *** p<0.01, ** p<0.05, * p<0.10. Standard errors (in parentheses) are clustered by municipality. Two-yearly panel for the period 1971-2010, with the log number of taxpayers respectively log income per capita as dependent variable.

2.7.3 Population

The analysis presented in Section 2.5 focuses on the tax base and therefore uses the number of taxpayers as one of the dependent variables. In Table 2.10 we show that the results are robust to replacing the number of taxpayers by the population.

Columns (1) and (2) are calculated based on the sample of all non-agglomeration municipalities that did not have a highway access in 1971/1972 and never got a highway access

Table 2.10: Impact of highway access on population

	all		1-10 km	
	(1)	(2)	(3)	(4)
	pop	pop	pop	pop
Long-term effect of access 1-10 km	0.130***	0.057***	0.024**	0.036***
	(0.013)	(0.017)	(0.011)	(0.012)
% Foreign nationals	0.010***	0.012***	0.002***	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
% Young (< 15)	0.008***	0.006***	0.007***	0.008***
	(0.001)	(0.001)	(0.001)	(0.001)
% Old (>= 80)	-0.017***	-0.013***	-0.002	-0.001
	(0.003)	(0.005)	(0.002)	(0.002)
% Workers in secondary sector	0.001***	0.005***	0.003***	0.003***
	(0.001)	(0.001)	(0.000)	(0.001)
% Workers in tertiary sector	0.006***	0.010***	0.002***	0.003***
	(0.001)	(0.001)	(0.000)	(0.001)
Unemployment rate	-0.002	-0.004	0.000	-0.001
	(0.002)	(0.003)	(0.001)	(0.002)
No. of movie theaters within 10 km	-0.002	0.004	-0.000	-0.000
	(0.002)	(0.003)	(0.001)	(0.001)
8 lags of highway access included	Yes	Yes	Yes	Yes
Municipality fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Municipality time trend	No	No	Yes	Yes
No. of observations	25893	9282	25893	9282
No. of municipalities	1233	442	1233	442
R2-adjusted	0.991	0.991	0.997	0.997

Notes. *** p<0.01, ** p<0.05, * p<0.10. Standard errors (in parentheses) are clustered by municipality. Two-yearly panel for the period 1971-2010, with log population as dependent variable.

on their territory; Columns (3) and (4) exploit variation over time only by restricting the sample to non-agglomeration municipalities that got a highway access within 10 km during the observation period, but not directly on their territory. Columns (2) and (4) also include a municipality-specific linear time trend.

The results are similar to those presented in Table 2.3 for the number of taxpayers. The estimates for the impact of highway access on population that control for municipality time trends (Columns 3 and 4) are slightly lower than the corresponding results for the number of taxpayers. This difference may reflect the fact that highways likely attract a disproportional share of non-married taxpayers, and taxpayers without children.

2.8 Conclusion

In this chapter, we analyze the impact of the Swiss highway network on local tax bases and tax rates based on federal income tax data from 1971 through 2010 at the municipality level. Getting access to a nearby highway increases the accessibility of a municipality and makes it a more attractive place of residence. An increase in the tax base may trigger a change in the local tax rates.

As a novelty in the growing literature on the spatial effects of transport infrastructure, we primarily rely on time variation for identification, exploiting the fact that not all sections of the highway network, which was to a large extend planned in 1960, were opened at the same time. This restriction allows us to mitigate potential issues resulting from differences in growth trends across municipalities that are unrelated to the transport infrastructure of interest, but might be systematically correlated with accessibility measures and thus undermine the common trend assumption.

Our results suggest that on average, a new highway led to an increase in both the number of residents and the average income per capita in non-agglomeration municipalities within 10 km distance to the next highway access point by approximately 4%. As expected, the positive impact was stronger for municipalities closer to a nearby metropolitan center and later in time when available space in cities became scarce and the highway network was close to complete. In contrast, municipalities 10-15 km away from the next highway did not benefit from the opening. These findings are robust to a variety of robustness checks, including tests that exploit variation both across time and across space.

In the second part of the analysis, we find evidence that municipalities within 10 km reach of highways decreased their tax rates by approximately 2%. The adjustment was concentrated in the 5-10 km distance band, which also experienced the strongest rise in the tax base.

Overall, the results underline the importance of highways for local development. Highways may attract additional taxpayers to the regions where they pass through, and in turn reduce the tax burden for their residents.

Chapter 3

The regional net benefit of transport infrastructure expansions: Evidence from the Swiss highway network

3.1 Introduction[1]

In recent years, there has been renewed interest in analyzing the economic effects of transportation networks. Exploiting spatial variation in the availability of transportation infrastructure, studies have investigated the impact of highways on regional earnings by industry (Chandra & Thompson, 2000), urban development (Baum-Snow, 2007; Duranton & Turner, 2012), firms (Datta, 2012; Ghani, Goswami & Kerr 2012; Gibbons et al., 2012), or labor market outcomes (Michaels, 2008; Sanchis-Guarner, 2012). Similarly, authors have also analyzed the impact of railroads on economic activity[2].

Typically, these studies compare the development of areas close to a transportation network against areas with similar characteristics, but no direct access to the transportation infrastructure. The results are somewhat mixed, with certain studies finding strong impacts on economic outcomes (in particular those looking at the Golden Quadrilateral Highway Project in India), and others such as Banerjee, Duflo and Qiang (2012) for China only reporting small effects. From a theoretical point of view, the absence of a uniform systematic effect is no surprise. Krugman (1991) has shown in his seminal paper on "Increasing Returns and Economic Geography" that the impact of better accessibility on regions depends on the context and may be positive or negative. The impact also varies between basic infrastructure investments in developing countries and advanced network upgrades in mature economies (see, e.g., Canning & Bennathan, 2007).

[1]This chapter is joint work with Christoph Gorgas.

[2]See Ahlfeldt and Feddersen (2010), Ahlfeldt and Wendland (2011), Atack et al. (2009), Banerjee, Duflo and Qiang, (2012), and Donaldson (2010).

Common to most of these studies is that they leave unanswered the question of whether the reported benefits represent net economic gains, or rather redistribution within the larger region[3]. Disentangling efficiency from redistributive gains empirically is difficult, yet important from a policy perspective. Today, many development assistance programs rely heavily on investments in transportation networks to promote economic development and foster convergence. The World Bank, for instance, has allocated roughly 20% of its lending in recent years to transport infrastructure – a share that is larger than that devoted to health, education, and social services combined (Crescenzi & Rodríguez-Pose, 2012). In the European Union, investments in transport infrastructure are a key element of regional development policies (see, e.g., European Commission, 2006). Understanding whether a larger area in fact benefits from transport network expansions is therefore an important issue.

In this chapter, we address this question explicitly based on a quantitative assessment of two case studies from the Swiss highway network: the highway between Geneva and Lausanne, opened in 1963/1964, and the Gotthard Road Tunnel, opened in 1980. We use the synthetic control method developed in Abadie, Diamond and Hainmueller (2010) and exploit the fact that the particular timing of these network expansions, which had been defined at the federal level, can be regarded as largely random and exogenous to regional economic development. Moreover, in both cases redistributional concerns play no major role. Given the specific geographic setting, potential relocation of people and firms as a result of the new highway would likely have taken place within the two regions themselves (Geneva-Vaud and Ticino respectively).

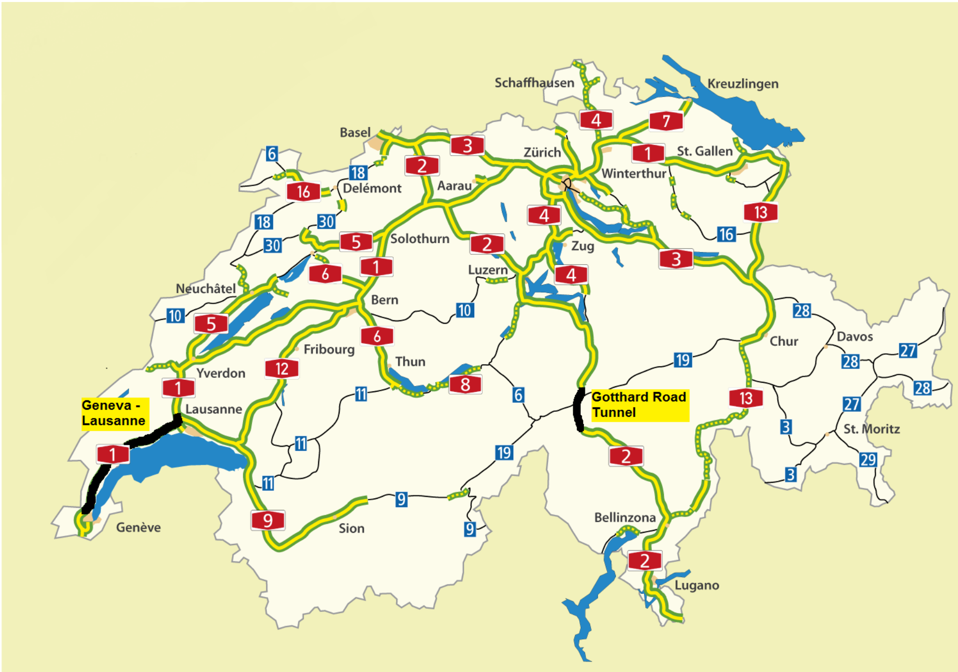
In both cases, our results provide little evidence for positive short- or medium-term effects of the network expansion on regional income per capita. The findings therefore suggest that in contrast to claims often made by policymakers, regional net gains of advanced transport network expansions in terms of output or productivity may be small.

3.2 Case selection

Switzerland's highway network was to a large extent defined in 1960 when the Swiss parliament passed the national roads law based on a comprehensive report by the corresponding planning commission (see Planungskommission, 1959). According to the commission, goal of the new road network was to cater for only the most important transport needs, i.e., primarily long-distance travel (Band 2, p. 1). Based on this criterium, the network was designed to connect Switzerland's main cities, as well as Vevey/Montreux, Chur and Lugano in the

[3] A notable exception is the analysis of the regional effects of the rural interstate highway system in the U.S. by Chandra and Thompson (2000). The authors report that earnings in rural counties with access to the new highways increased. At the same time, neighboring counties without a highway experienced an earnings decline. The net effect of the new highways on growth of the overall region was "essentially zero" (p. 460).

Figure 3.1: Swiss highway network including case studies



mountainous and southern parts of Switzerland (Band 2, p. 2). It consisted of three different classes of roads, including certain roads that were not restricted to motorized vehicles only.

In the following decades, the Swiss national road network – and with it the highway network – was gradually constructed. Over time, several routes have been enhanced, e.g., in terms of the number of lanes. However, the spatial reach of the network has remained largely unchanged until today, with a few notable exceptions (e.g., the Gotthard Road Tunnel, whose construction was passed by the federal parliament in 1965). Today, the Swiss national road network has a defined length of 1,892.5 km, only slightly more than the originally planned network of 1,840 km. Of these national roads, roughly 150 km still have to be built (Bundesamt für Strassen, 2013).

In our analysis, we focus on regional economic effects beyond simple relocation of people and firms across regions. Therefore, we look at cases where relocation into or out of the region plays no important role: the highway between Geneva and Lausanne, opened in 1963/1964, and the Gotthard Road Tunnel, opened in 1980.

A1: Geneva - Lausanne (Vaud)

As one of the first sections, the highway between Geneva and Lausanne, the capital of the canton of Vaud (VD), was opened to public in 1963/1964, on time for the 1964 national exhibition in Lausanne. For the analysis whether the new highway had a positive impact on overall income per capita of the larger area, we look at the joint region GE_VD. This region had an income per capita of 103% of the Swiss average at the time the new highway was completed.

The section of the highway A1 between Geneva and Lausanne reduced travel time between Geneva and Lausanne by roughly 20 minutes (Barbier et al., 1978), resulting in a travel time of approximately one hour after the opening of the new highway. It also provided easy access to the cities from the districts along the lake. Meanwhile, accessibility of the two cantons from the rest of Switzerland remained largely unchanged. Vaud is the only neighboring canton of Geneva, and at the time Lausanne was relatively far away from other larger Swiss cities such as Berne[4].

As a result of the new highway, people may have moved across cantonal borders along Lake Geneva, with Geneva now being more easily reachable also from the south-western areas of Vaud (e.g., Nyon). The effects may have been most pronounced for the districts located in the middle area between the two agglomerations, where commuting times to the cities experienced the strongest declines (Barbier et al., 1978, p. 133).

For the combined region GE_VD, however, we would only expect an impact on income per capita in case the new fast connection between the two cities led to efficiency gains in either or both of the two cantons. Such gains may have taken various direct and indirect forms, see, e.g., Gibbons and Overman (2009). Given that the new highway between Geneva and Lausanne provided travel time reductions of an order of magnitude that were more important for the transportation of people than goods, it seems likely that potential benefits would have resulted primarily from improved labor market outcomes, for instance thanks to better matching of workers and jobs, or increased labor supply[5].

A2: Gotthard Road Tunnel (Ticino)

In September 1980, the Gotthard Road Tunnel was opened to public, establishing an all-year road connection between the canton of Ticino and central Switzerland. Ticino is Switzerland's Italian speaking canton located in the southern part of the country, with a focus on tourism[6]. In the years 1979/1980, income per capita stood at 93% of Swiss average. The canton is separated by the Alps from the central part of Switzerland. Since 1882, a rail tunnel connects Airolo in Ticino with Göschenen in Uri. By car, for a long time the canton of Ticino was only reachable from central Switzerland over the Gotthard pass[7]. However, the pass is only open in summer, typically from May to October. In winter, cars had to use

[4] The construction of the connection between Lausanne and Berne only began in the 1970s, and was completed in 1981.

[5] See Sanchis-Guarner (2012) for a detailed discussion of the potential effects of reduced commuting costs on labor market outcomes.

[6] In 1980, Ticino had a ratio of 28 tourist nights per inhabitant, compared to the Swiss average of 12 (Peyer, 1982, p. 25).

[7] There exist alternative routes to Ticino from the cantons of Valais and Grisons. In 1967, the San Bernardino Road Tunnel was completed. However, from the main cities in central Switzerland such as Zurich, Basel and Berne, these alternative routes take considerably longer than the Gotthard route.

the rail to cross the tunnel ("Autoverlad").

The Gotthard Road Tunnel, with a length of 16.9 km the longest road tunnel at the time, removed this bottleneck. Construction of the tunnel took 10 years or 9,200,000 work hours and cost 686 million Swiss Francs at the time (Peyer, 1982). Since its opening, the tunnel has become one of the major routes for people and goods to cross the Alps. In 2010, 927,000 heavy trucks passed through the tunnel (Bundesamt für Statistik, 2013).

The opening of the Gotthard Road Tunnel represented a major expansion of the Swiss highway network. It was accompanied by the completion of other sections of the north-south highway A2, such as the Seelisberg Tunnel, and had a fundamental impact on transportation in Europe. Therefore, at the time of the opening, people expected that the tunnel would lead to an increase in Switzerland's overall aggregate economic productivity (Jakob, 1980). In contrast, there was more uncertainty about the regional economic effects of the tunnel. Ticino's economic activity is concentrated in its southern part (Bellinzona, Locarno, Lugano), 1.5 car hours and more away from Lucerne, the closest city in central Switzerland. Market integration may have been further constrained by the language barrier and imports from Italy[8]. Moreover, even before the opening of the Gotthard Road Tunnel, Ticino was not disconnected from central Switzerland, thanks to the rail tunnel and the Gotthard pass in summer.

As a result, the study group 'Gotthard Tunnel' (see Peyer, 1982, p. 32), some commentators (e.g., Jakob, 1980), and a majority of firms[9] expected a positive impact for Ticino's economy, while others were less optimistic. Rossi (1980), for instance, feared that the improved connection to central Switzerland might lead to a destablisation of Ticino's economy. According to him, the only industry that was likely to benefit from the tunnel was tourism. He expected a rise in the number of guests from the German-speaking part of Switzerland – but not uniformely across the canton. For the municipalities along the transit route in the Leventina and Riviera, Rossi even predicted a strong decline.

3.3 Methodology and Data

3.3.1 Synthetic Control Method

Goal of this study is to quantitatively assess the impact of improved regional accessibility on the basis of case studies. In each case, only one or a few cantons are affected by a transport infrastructure expansion. The synthetic control method developed in Abadie,

[8]According to a business survey conducted in 1973, 33% of the manufacturing companies in Ticino had Italian suppliers. A majority of 63% of the companies sourced their products from the rest of Switzerland (Maggini, 1979, p. 276).

[9]In a survey conducted in 1973 among 265 manufacturing companies in Ticino, 55% estimated that the tunnel would have a favorable effect, compared to 34% that were indifferent and a small minority that saw the tunnel as unfavorable (Maggini, 1979, p. 278).

Diamond and Hainmueller (2010) is well suited for this purpose. Based on a donor pool of unaffected cantons, the synthetic control method explicitly models a counterfactual that mimics the behavior of the outcome variable as it would have evolved in the absence of the treatment. The difference between the actual development in the post-treatment period and the evolution of the synthetic control serves as a measure for the effect of the treatment.

More precisely, we construct a synthetic control that consists of non-negative weights of other cantons that sum to 1. The weights are chosen so that the deviation between the actual canton and its synthetic control during the pre-treatment period is minimized. The dependent variable is taxable income per taxpaying unit, as proxy for income per capita. To ensure that the counterfactual is indeed relevant, we require that the synthetic control resembles the original canton with respect to a number of economic, demographic, and political characteristics that may potentially affect cantonal income per capita. In the optimization process, the synthetic control method puts most emphasis on a high congruence of those characteristics that are most powerful in explaining variation in the dependent variable[10].

We consider a broad set of predictor variables: First, the population share aged 20-64 years accounts for potential differences in the proportion of people in the working age. A higher working-age population share should translate into higher income per capita. Second, population density proxies for the degree of urbanization. Typically, income per capita is lower in more rural cantons. Third, the number of newly built apartments per inhabitant captures cantonal population dynamics. Different population trends starting already in the years before the opening of a new highway could bias our results. Fourth, a high share of foreigners may have acted as a drag on income per capita, given that historically foreigners in Switzerland primarily worked in lower-skilled jobs. Fifth, the primary, secondary and tertiary sector industry shares account for differences in the economic structure of the cantons. Likely, a large primary sector leads to lower income per capita. Sixth, we include the unemployment rate as proxy for structural differences in labor market outcomes across cantons. Finally, we also consider two political variables: The vote share of social democrats in federal elections, and the average cantonal and municipality tax rate for the top 1 percent income earners. A higher vote share of social democrats might lead to a lower cantonal income per capita, under the assumption that social democrats prefer a larger government, and that these public expenditures are paid with distortionary taxes and do not yield disproportionally large returns. A low tax rate for the top 1 percent may attract people with very high incomes and thus lead to a comparably high income per capita.

Note that our analysis focuses only on tangible benefits of highway expansions in terms of income per capita. Without doubt, transport infrastructure networks may also create certain

[10]See Abadie, Diamond and Hainmueller (2010) for a more formal explanation of the synthetic control method.

benefits that do not show up in official output or income statistics, but still contribute to aggregate welfare. Such benefits include shorter commuting times, the possibility to separate place of work and place of residence, or an increased set of leisure activities within reach.

3.3.2 Identification

Investments in transport networks may be affected by regional economic considerations, even if the political decision to build or expand the network – as in the studied cases – is taken at the federal level. Particularly, the federal government may prioritize highways in regions experiencing strong increases in traffic. This situation gives rise to potential endogeneity concerns. Is the new infrastructure indeed the cause of observed growth in later periods? Or does the road network expansion simply reflect expectations of future growth? Identification of a causal impact of expansions on economic outcomes is therefore a difficult endeavor (see Datta, 2012).

Our identification strategy based on the synthetic control method is able to address this concern. Similarly to Sunde and Strittmatter (2013), we argue that while a transport network expansion might be related to a region's economic outlook, "the particular year in which the implementation [i.e., in our case the opening of the new highway section] takes place is largely random" (p. 3), given that building a new federal highway or road tunnel involves both a political decision process and a construction period of several years.

In case of the Gotthard Road Tunnel, for instance, the first public requests for a road tunnel through the Gotthard date back to 1939, as Peyer (1982, pp. 30-32) notes in his summary of the planning and construction phase of the tunnel. Yet only in 1960 the planning process at the federal level started, resulting in a 300-page report that proposed a tunnel from Göschenen to Airolo in order to ensure an all-year road connection through the Gotthard. In 1965, the federal parliament passed the proposal by the study group, and in 1968 the Swiss Federal Council approved the construction project. In 1970, construction started; on September 1980, the tunnel was finally opened to public.

For the highway between Geneva and Lausanne, the decisive factor for the timing of its opening was a political event – the national exhibition in Lausanne in 1964. As Fischer and Volk (1999) note, this highway section was the only case where the federal government used a provisional funding scheme in order to ensure its timely completion.

From a methodological point of view, it is important that the cantons that make part of the donor pool do not experience similar network expansions at the same time as the area of interest. Otherwise, our counterfactual could be subject to treatment as well and thus – in case of a positive impact – bias our results downwards. Ideally, we would like to investigate infrastructure expansions that happened in isolation.

Unfortunately, a large portion of the Swiss highway network was constructed during the 1960s and 1970s. Excluding all these cantons from the donor pool would eliminate a potential bias from highway construction in those cantons. At the same time, however, it would also weaken the congruence of the area of interest with its synthetic control in terms of economic, demographic, and political characteristics.

We address this trade-off in the following way. First, in the baseline specification, the donor pool consists of all other cantons not directly affected by the expansion under investigation. These cantons may include cantons that also experienced highway openings around the same time. Goal is to ensure the best possible match in terms of pre-treatment characteristics between the actual canton and its synthetic control. Second, we test the robustness of the findings by excluding all cantons that were subject to expansions of major highway sections during a period of plus/minus 5 years around the treatment year. Third, we exclude all cantons that account for a large fraction of the baseline synthetic control individually, one after another, from the donor pool. This last set of robustness checks also ensures that our results are not driven by other canton-specific shocks (e.g., changes in the tax rates or improved accessibility by public transport) that are not related to the highway network. Fourth, we focus on the short- and medium-term effects of the transport infrastructure expansions over a 10-year period after the opening of the highway. For longer time periods, the synthetic control may gradually lose its relevance due to canton-specific shocks and developments. Finally, when selecting the cases, we refrain from looking at expansions during the late 1960s and early 1970s when a substantial share of cantons located in the Swiss Plateau were affected by the construction of new highways, in particular the highway A1.

3.3.3 Data

All main data series used in the analysis are at the cantonal level. We approximate nominal income per capita according to the GNI concept with the average taxable income per taxpaying unit based on an updated version of the data presented in Schaltegger and Gorgas (2011). The numbers refer to taxable income according to the federal income tax system, i.e., they represent total labor and capital income minus deductions, and are generally available on a 2-year basis. For our analysis, we use the data starting after World War II in 1945/1946 up to the period 1995/1996 and divide total taxable income of a canton in a given year by the number of taxpaying units (households and individuals) to obtain average taxable income per taxpaying unit. This variable serves as proxy for the average nominal income per capita of the residents in the canton.

Opening dates of the different sections of the Swiss national road network were provided by the Swiss Federal Roads Office. The unemployment rate was published in various volumes of the Swiss periodical *Die Volkswirtschaft*. Industry sector shares, population variables, vote share of the Social Democratic Party in federal elections, as well as the percentage of foreigners come from the Historical Statistics of Switzerland and the Swiss Federal Statistical Office. Where required, we interpolated missing data between existing data points in a linear manner. The number of newly built apartments refers to the number of new apartments completed in a given year and has been obtained from *Die Volkswirtschaft* (various volumes). The data on tourist nights and the hectares of alpine pastures have been retrieved from the Swiss Federal Statistical Office. Finally, the average cantonal and municipality tax rate for the top 1 percent income earners in each cantonal capital was calculated based on federal and cantonal tax statistics (see Gorgas & Schaltegger, 2012, for details).

3.4 Results

3.4.1 A1: Geneva - Lausanne (Vaud)

Main results

Figure 3.2 displays the evolution of average taxable income per taxpaying unit for the combined area GE_VD and the baseline synthetic control. We have calculated the synthetic control using the list of demographic, economic, and political predictor variables introduced in Section 3.3: the share of the population aged 20-64, population density, the number of newly built apartments per inhabitant, the share of foreigners, the shares of economic activity in the primary, secondary, and tertiary sectors, the unemployment rate, the vote share of social democrats in federal elections, and the average cantonal and municipality tax rate for the top 1 percent income earners in the cantonal capital. Following Abadie, Diamond and Hainmueller (2010), we also include the lagged values of taxable income per taxpaying unit in the years 1946, 1954 and 1962[11].

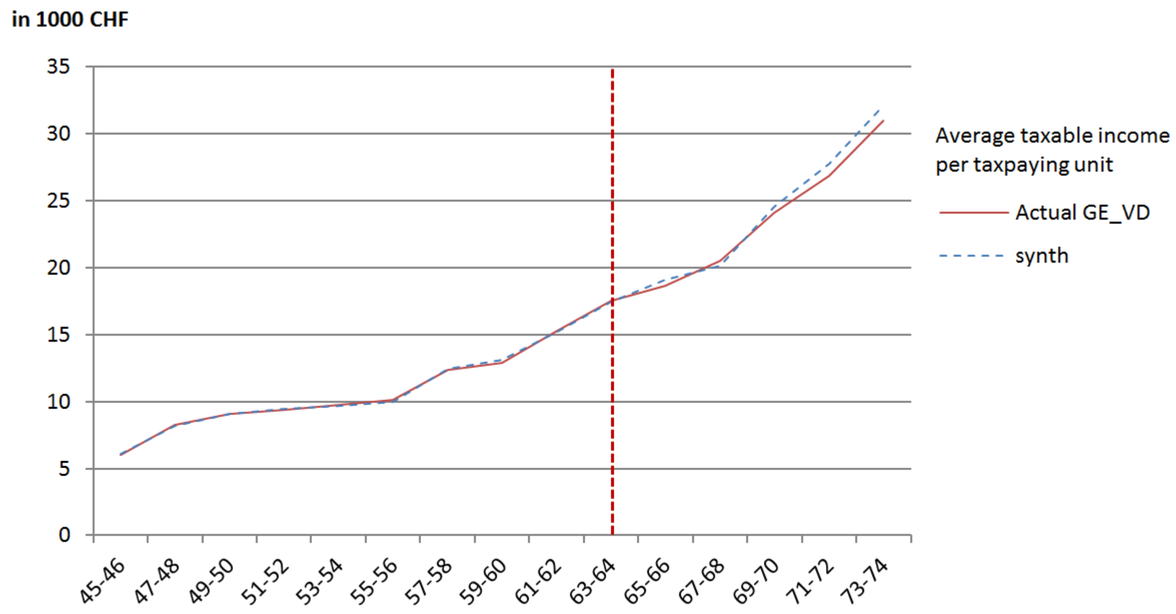
The optimization procedure covers the full pre-treatment period (1946-1962) and uses averages of the predictor variables over the same period. The donor pool consists of all other Swiss cantons except Geneva, Vaud as well as Jura, which was only founded in 1979.

Figure 3.2 shows that in the years after 1964, income per capita in GE_VD did not increase by more than did its synthetic control. If anything, the baseline synthetic control specification suggests that actual GE_VD may have slightly underperformed during the first half of the 1970s.

Table 3.1 provides details on the composition of the baseline synthetic control (left-hand side). It also shows the degree of congruence in the pre-treatment characteristics (right-hand side). The synthetic control method ensures that the variables with the highest predictive power for the dependent variable show the best match (see Abadie, Diamond & Hainmueller,

[11] In the text, we always refer to the later year of the 2-year tax periods. For instance, 1962 denotes the period 1961/1962.

Figure 3.2: Average taxable income per taxpaying unit: Evolution of combined GE_VD and its synthetic control



2010, p. 500). For most variables, conformance between the actual region GE_VD and the synthetic control is relatively high. A notable exception is population density, which is considerably larger in the synthetic control than in the combined GE_VD region.

Table 3.1: Details on synthetic control for combined GE_VD

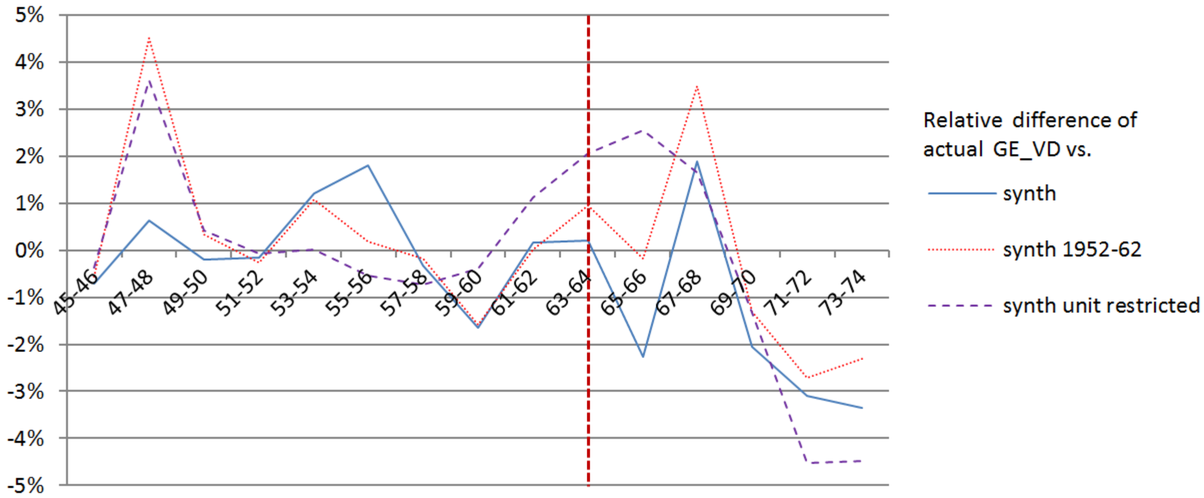
A. Composition		**B. Pre-treatment characteristics**		
Canton	Weight	Variable	Actual GE_VD	Synthetic GE_VD
BS	0.035	Population share aged 20-64 (%)	62.812	62.109
GR	0.090	Population density (Inhabitants per km^2)	202.1	487.5
TI	0.343	Newly built apartments per inhabitant (%)	0.674	0.640
ZH	0.531	Share of foreigners (%)	13.181	12.202
		Primary sector share (%)	9.932	11.326
		Secondary sector share (%)	40.046	45.334
		Tertiary sector share (%)	49.868	43.015
		Unemployment rate (%)	0.189	0.218
		Vote share of social democrats (%)	20.613	23.986
		Top 1 percent tax rate (%)	16.246	18.442
		Dep. variable (1946)	6,052	6,096
		Dep. variable (1954)	9,769	9,653
		Dep. variable (1962)	15,272	15,246

Robustness checks

Figure 3.2 above suggests that the new highway between Geneva and Lausanne did not have a major impact on overall income per capita in the combined area of the two cantons. In the following, we examine the robustness of this finding.

Figure 3.3 displays the results from the first part of robustness tests. The graphs represent the percentage difference between the actual evolution of income per capita and the counterfactual for different synthetic control specifications. The blue graph (synth) depicts our baseline case presented above, providing no evidence for a positive impact in the years following the opening of the new highway. Rather, as mentioned before, the Geneva-Vaud area seems to have underperformed its synthetic control, in particular during the early 1970s.

Figure 3.3: Robustness checks, part 1: Relative evolution of average taxable income per taxpaying unit in GE_VD vs. its synthetic counterfactuals



As cantons undergo changes over time, the length of the optimization period to construct the synthetic control may affect the results. Therefore, we test a different specification where the synthetic control is calculated over the shorter optimization period 1952-1962 (red graph, synth 1952-62). Two observations stand out: First, actual income per capita shows a relatively large deviation from the synthetic control during the period 1947/1948, when a special inflation deducation affected reported taxable incomes. Second, there is still no consistent increase in income per capita observable after 1964. For the period 1967/1968, the results based on the shorter optimization period indicate a substantial outperformance of 3.5 percent. However, the figure plots deviations in levels and not growth rates, which points to an increase in income per capita that lasted only for one period. It is unlikely that this patterns reflects the effect of the highway, which was of permanent nature. Rather, the spike seems to be caused by a strong increase in the number of taxpayers in these years (see the Ticino results below for a detailed discussion).

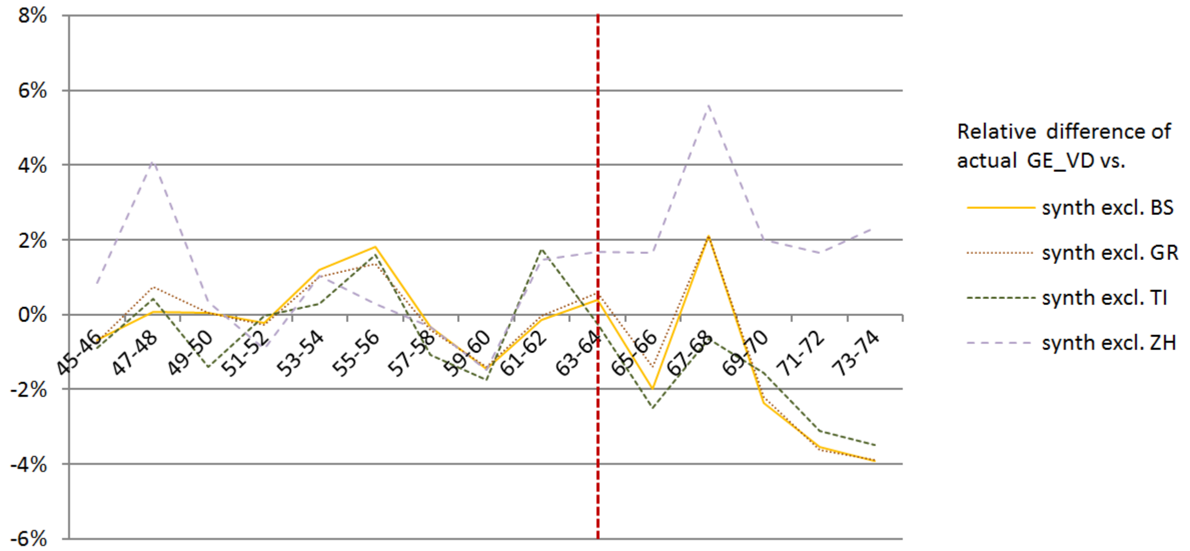
So far, we have only excluded the canton of Jura from the donor pool. There were a number of other cantons, however, which – as Geneva and Vaud – started to construct first highway sections during the 1960s. The inclusion of these cantons may confound the results. In particular, a positive effect of the highway in Geneva and Vaud may be underestimated if other cantons that experienced similar gains make part of the synthetic control. Of the four cantons of our baseline specification, one was subject to major highway openings: In Zurich, the A3 along the lake as well as the cantonal highway A51 to Bülach were opened. Do these developments explain the absence of a positive impact in the Geneva-Vaud region?

The additional robustness checks do not suggest that this seems to be the case. First, in

Figure 3.3, the purple graph (synth unit restricted) depicts the results based on a synthetic control optimization that excludes from the donor pool all cantons that were subject to major highway openings in the period 5 years before and after the treatment year[12]. For the time after 1964, the result is mixed, with an outperformance of actual GE_VD for the first two periods of roughly 2%, followed by an even larger underperformance. Note, however, that already in the years just before the opening, the values for actual GE_VD are above those of the synthetic control. These deviations could therefore also imply that due to the smaller size of the donor pool, the fit of the synthetic control is worse than in the baseline specification.

In the second part of the robustness checks, we test whether the findings are robust to the exclusion of individual cantons from the synthetic control. More precisely, the orange, brown, olive and violet graphs in Figure 3.4 represent tests whether our results are driven by canton-specific developments in Basel City (BS), Grisons (GR), Ticino (TI), or Zurich (ZH). In particular Ticino and Zurich receive a relatively large weight in the baseline synthetic control and thus could potentially bias the results. We exclude one canton after the other from the donor pool. The graph for the exclusion of Zurich points to a potential outperformance of GE_VD compared to the synthetic control. However, as before, the deviation is already visible in the period 1961/1962, suggesting that the difference does not reflect the impact of the opening of the new highway.

Figure 3.4: Robustness checks, part 2: Relative evolution of average taxable income per taxpaying unit in GE_VD vs. its synthetic counterfactuals



[12]These cantons are Aargau, Berne, Basel Country, St. Gallen, Schaffhausen, Solothurn, Schwyz, and Zurich.

Supporting evidence

In 1978, Barbier et al. (1978) completed a report that analyzes the effects of the highway between Geneva and Lausanne 12 years after its opening. The study undertakes no attempt in quantifying the net benefit of the new highway for the combined region, but focuses on its local effects[13].

According to this analysis, the benefit of the new highway seems to have mainly accrued to the two cities Geneva and Lausanne in the form of lower congestion and increased accessibility of the suburbs close to the highway. As a result, the cities regained their attractivity and were able to absorb a large fraction of regional population and employment growth. In particular the suburban areas close to the highway attracted businesses from less accessible parts of the agglomeration and became the prime location to establish retail outlets such as supermarkets or furniture stores.

At the same time, the highway also led to a decentralization of the places of residence, and to an increase in commuting. Therefore, in percentage terms, the increase in population between 1955 and 1975 was largest in the districts of the two smaller satellite centers Nyon and Morges. But also smaller villages in the region that provided attractive housing (in particular single-family homes) increased their number of inhabitants. In contrast, the rural regions along the lake further away from the two agglomerations – Rolle and in particular Aubonne – did not seem to substantially benefit from the highway during the first decade after its opening.

The highway also provided other benefits not measured in residents or employment statistics. It reduced travel times by 30 to 45%, cut the number of accidents by half, and thanks to the bypass allowed for a stablization of the car traffic in Lausanne's city center.

From a transportation system point of view, the new highway was a success. Already in 1970, the highway, which only made up for 18% of the regional road network, accounted for more than 60% of regional traffic. As a result, traffic on the cantonal road along the lake stabilized on a level substantially lower than during the years before the opening of the highway. Moreover, the full increase in car travel on the route between Geneva and Lausanne during the first 12 years after the opening of the A1 was absorbed by the highway.

Taken together, the findings by Barbier et al. (1978) seem to be consistent with a story where the highway A1 between Geneva and Lausanne did not have substantial effects on income per capita in the region, but generated other benefits for its inhabitants, in particular easier commute and the possibility to live outside the large agglomerations while working in the cities. According to Obermeyer, Evangelinos and Besherz (2013), reductions in commuting times, quantified in monetary terms, may account for 70 to 90 percent of the total value of a transportation project (p. 118).

[13]Unless otherwise stated, all evidence reported in this subsection stems from this study by Barbier et al. (1978).

3.4.2 A2: Gotthard Road Tunnel (Ticino)

Main results

Figure 3.5 shows the development of average taxable income per taxpaying unit in Ticino relative to its synthetic control. To calculate the baseline synthetic control, taxable income per taxpaying unit in the years 1958, 1968 and 1978 was used in the set of predictor variables. The other predictor variables were averaged over a 20-year pre-treatment period[14]. The same 20-year period was also used in the optimization process to calculate the weights of the synthetic control. Figure 3.5 indicates that for the time after the opening of the tunnel in 1980, actual income per capita in Ticino was not higher than in the synthetic control – if anything, it was lower.

Figure 3.5: Average taxable income per taxpaying unit: Evolution of Ticino and its synthetic control

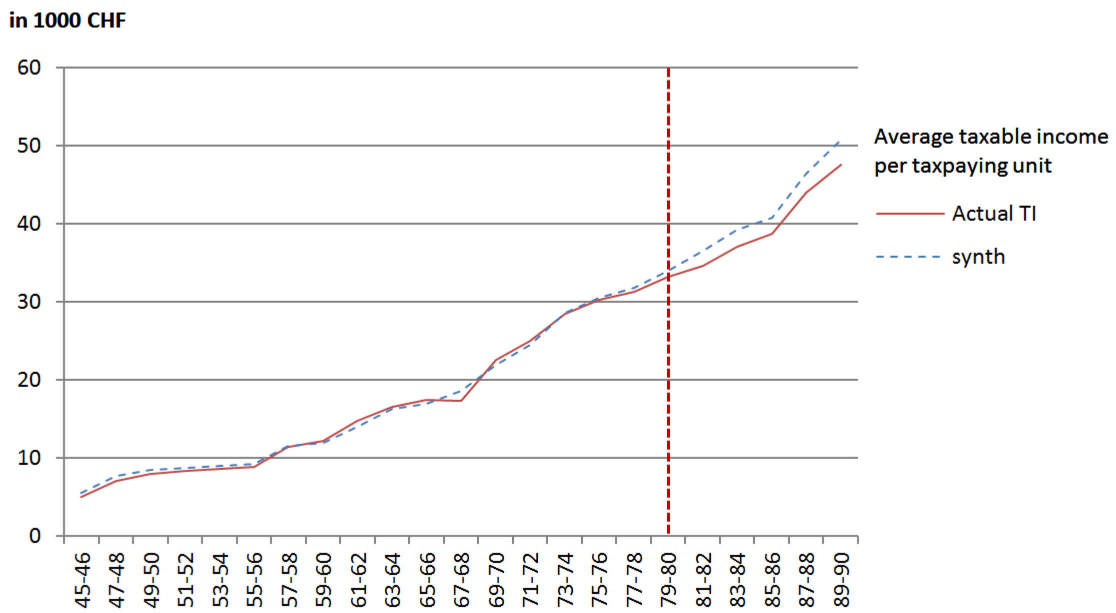


Table 3.2 provides details on the weights assigned to each canton in the synthetic control (left-hand side), as well as on the predictor variables in the pre-treatment period (right-hand side). The composition shows that the synthetic control consists to almost 3/4 of the canton of Vaud. It will therefore be important to test the sensivity of the results to the inclusion or exclusion of Vaud in the donor pool.

Compared to the case of GE_VD, the congruence between actual TI and synthetic control is weaker. Again, the difference in population density is relatively large.

[14]For the number of newly built apartments per inhabitant, the time period for calculating the average pre-treatment value is shorter due to a break in the data series in 1967.

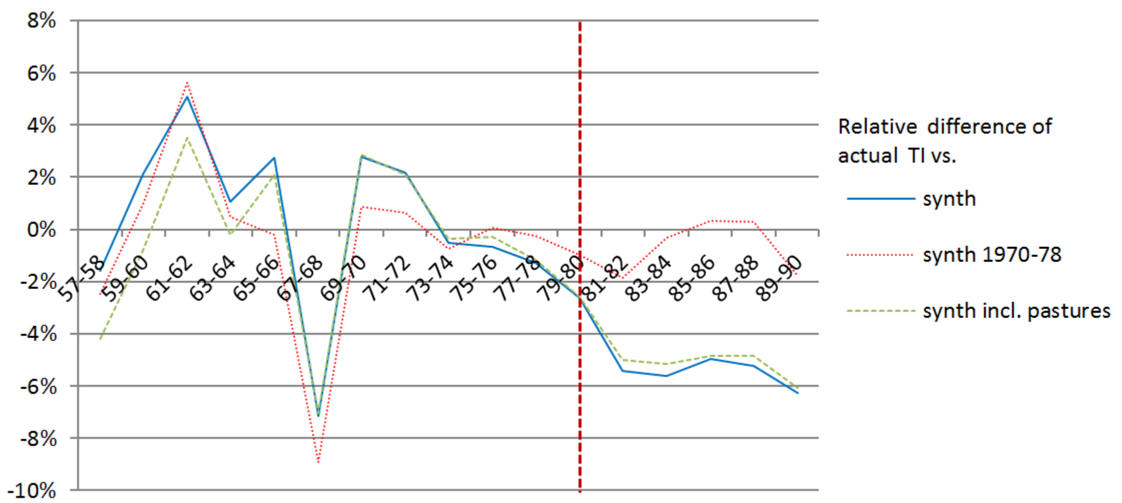
Table 3.2: Details on synthetic control for Ticino

A. Composition		B. Pre-treatment characteristics		
Canton	Weight	Variable	Actual TI	Synthetic TI
GL	0.236	Population share aged 20-64 (%)	59.997	59.020
VD	0.764	Population density (Inhabitants per km^2)	83.2	143.3
		Newly built apartments per inhabitant (%)	1.330	0.853
		Share of foreigners (%)	23.484	17.607
		Primary sector share (%)	6.761	9.387
		Secondary sector share (%)	42.197	45.472
		Tertiary sector share (%)	50.524	45.024
		Unemployment rate (%)	0.131	0.080
		Vote share of social democrats (%)	16.473	21.003
		Top 1 percent tax rate (%)	19.745	20.632
		Dep. variable (1958)	11,448	11,632
		Dep. variable (1968)	17,318	18,654
		Dep. variable (1978)	31,391	31,800

Robustness checks

We test the robustness of the baseline specification in a number of ways. First, we re-run the analysis for alternative specifications of the synthetic control optimization procedure. The results are displayed in Figure 3.6.

Figure 3.6: Robustness checks, part 1: Relative evolution of average taxable income per taxpaying unit in Ticino vs. its synthetic counterfactuals



The blue graph (synth) represents our baseline specification. From the figure, one can see that also in terms of income per capita, the match of the synthetic control with the actual development in the pre-treatment period is not as strong as in the Geneva-Vaud case before. In particular for the period 1967/1968 the actual value shows a strong deviation from the synthetic control of more than 7%. Reason for this divergence is a substantial increase in the number of people subject to federal tax in the periods 1965/1966 and 1967/1968, fueled by relatively high inflation rates of 4-5% during the years 1966 and 1967[15]. The increase was larger in poorer cantons with more people below the tax-free income threshold, and

[15]On average, the number of taxable units rose by 19.5% in period 1965/1966 and by 14.9% in period 1967/1968. In Ticino, the corresponding growth rates were even larger, equaling 30.0% and 18.6% respectively.

it led to a change in the composition of taxpayers that was heterogeneous across cantons. In 1969/1970, the tax-free income threshold was raised, inducing another change in the tax base. We therefore calculate an alternative synthetic control specification based on the shorter 1970-1978 pre-treatment optimization period (red graph, synth 1970-78). As a result, the negative deviation of actual Ticino from the synthetic control for the time period after the opening of the tunnel becomes considerably smaller, from close to -6% to between 0 and -2%. However, there is still no evidence for a positive effect of the opening of the tunnel on income per capita.

As outlined above, Ticino has a strong focus on tourism. If the synthetic control should not accurately reflect this characteristic, then the evolution of the synthetic control might not be a relevant counterfactual. To test whether this issue might be a driver of our results, we include an additional predictor variable that is able to explain a large part of the cross-sectional variation in the number of tourist nights per canton: the number of hectares of alpine pastures in 1980[16]. The olive graph (synth incl. pastures) shows the difference of actual Ticino from the corresponding counterfactual. Again, the results are very similar to the baseline specification.

Overall, a consistent picture emerges from Figure 3.6: Ticino's income per capita did not increase relative to its synthetic counterfactual in the years following the opening of the Gotthard Road Tunnel. Rather, the results suggest that Ticino's income per capita might even have experienced a below-average development after 1980. This trend seems to have started already in the mid-1970s, however, and therefore may not have been the result of the tunnel opening.

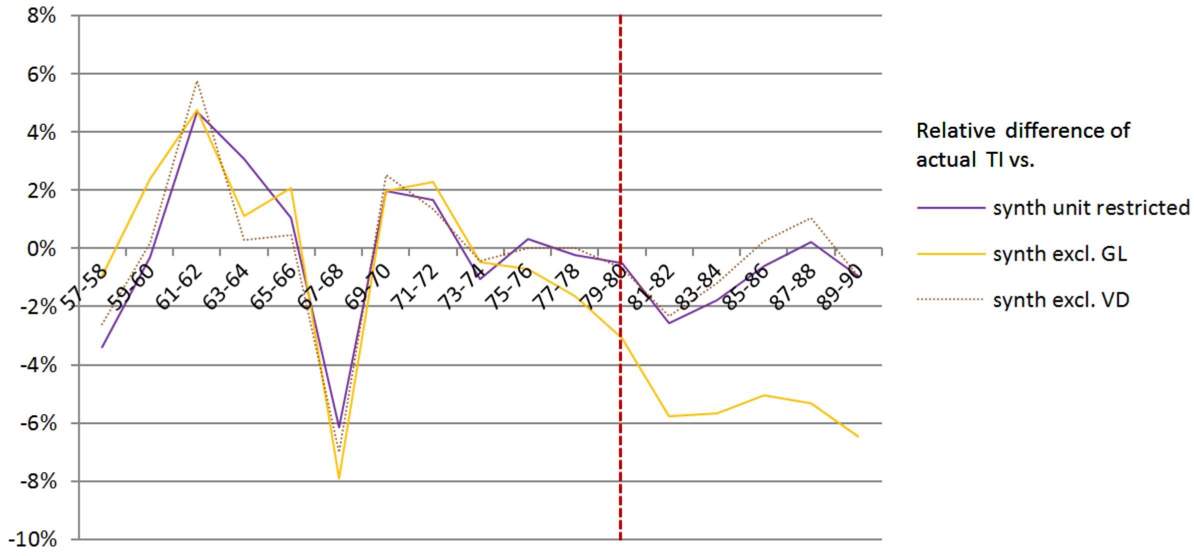
As in the case of GE_VD above, we next test whether our findings are driven by canton-specific developments. First, we exclude all cantons from the donor pool that also experienced major highway openings during the five years before and after the opening of the Gotthard tunnel. In particular Vaud might bias our results, given its large weight in the synthetic control and the completion of the A12 between Bulle and Vevey in 1981[17]. The purple graph (synth unit restricted) in Figure 3.7 depicts the corresponding result, which again points to an underperformance of Ticino of between 0 and 2% during the 1980s.

Figure 3.7 also contains the results for the specifications that exclude the two cantons that make part of the baseline synthetic control one after the other from the donor pool. Vaud indeed seems to matter: If Vaud is excluded from the synthetic control (brown graph), the development of Ticino after the opening of the tunnel appears to have been broadly in line with its synthetic control. In contrast, the exclusion of Glarus does not materially change the baseline result. Taken together, these robustness checks suggest that the substantial

[16]We use this proxy since data on tourist nights are not available on a cantonal basis before 2003 (only by touristic region).

[17]The other excluded cantons are Grisons, Fribourg, Lucerne, Nidwalden, Thurgau, Uri, and Valais.

Figure 3.7: Robustness checks, part 2: Relative evolution of average taxable income per taxpaying unit in Ticino vs. its synthetic counterfactuals



deviation visible in the baseline specification for the late 1970s and the 1980s may in fact reflect an outperformance of Vaud rather than an underperformance of Ticino. Still, there is no sign that Ticino outperformed its synthetic control after the opening of the tunnel.

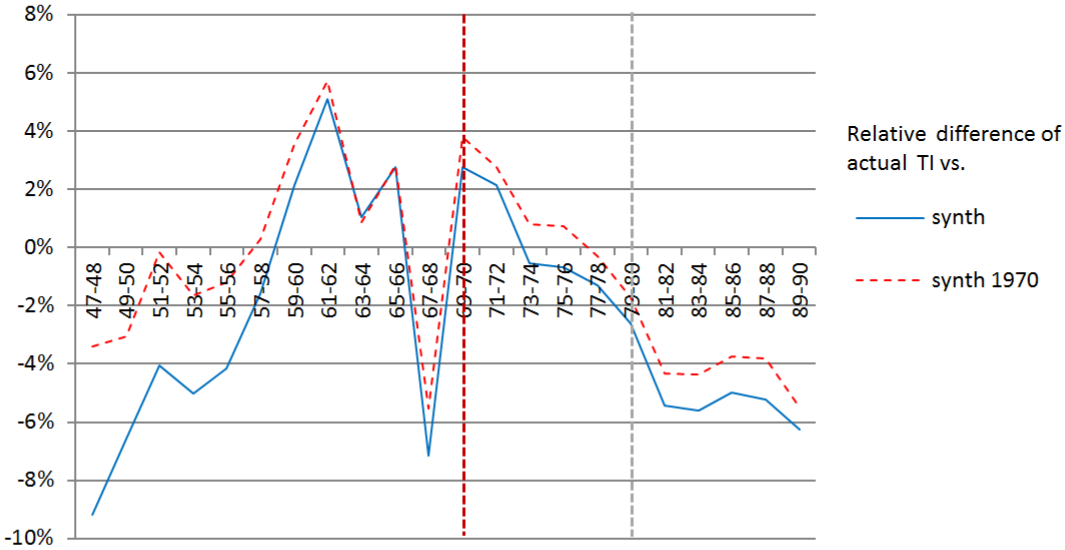
One reason for not finding a positive effect after 1980 could be that tunnel construction might have impacted income per capita in the 1970s. More precisely, the construction of the tunnel between 1970 and 1980 could have generated one-off economic benefits for Ticino, in particular in the construction industry. In that case, matching Ticino's income per capita in the years prior to the opening of the tunnel might bias our results. It could lead to a counterfactual that is too high compared to the case without a tunnel construction. Indeed, Rutishauser (2000) shows that the value added in the construction industry declined along the Gotthard route from Lucerne to Bellinzona between 1975 and 1985 by more than 20%, while it remained fairly stable along the San Bernardino route (-3%).

To address this concern, we conduct an analysis similar to our baseline analysis, but with 1970 – the starting year of construction – as treatment. Again, optimization is performed over a 20-year pre-treatment period (1948-1968), and taxable income per taxpaying unit in 1948, 1958 and 1968 is used together with the other predictor variables.

The result is depicted as red graph (synth 1970) in Figure 3.8 and provides little support for this hypothesis. Actual Ticino does not seem to have consistently outperformed its counterfactual during the 1970s in terms of income per capita. The red graph with treatment year 1970 shows a development similar to the blue baseline synthetic control with treatment year 1980. This finding implies that the construction period does not seem to have had a major impact on average income in the canton of Ticino[18].

[18] If we exclude the period 1967/1968 from the optimization procedure, there is even no outperformance at all visible after 1970.

Figure 3.8: Robustness checks, part 3: Relative evolution of average taxable income per tax-paying unit in Ticino vs. its synthetic counterfactual using the start of tunnel construction in 1970 as treatment year



In consequence, the robustness check with treatment year 1970 does not only seem to confirm, but even to broaden the scope of our finding: First, the result confirms that the *opening* of the tunnel in 1980 does not seem to have led to an increase in income per capita. Second, it suggests that also the *construction* of the tunnel itself did not result in any substantial gains in income per capita. As a caveat, we acknowledge that the match in the pre-treatment period is not ideal and hence the findings have to be interpreted with care.

Supporting evidence

The finding that the tunnel did not substantially boost Ticino's economy is supported by data on tourists from the Swiss Federal Statistical Office. The number of overnight stays in Ticino increased slightly over-proportionally in the initial years following the opening of the tunnel. Between 1979 and 1981, the number of nights rose by 22.1%, compared to 17.8% in total Switzerland[19]. Berner Oberland was the only touristic region in Switzerland that experienced a larger increase during that period (27.3%). However, this rise did not last. In the five years after 1981, Ticino lost 9.9% in tourist nights, which was almost double the Swiss average decline of 5.0%. This below-average trend continued in the 1990s.

The negative effects on tourism were particularly pronounced for the Leventina region, which is located directly on the southern side of the Gotthard Road Tunnel between Airolo and Biasca. In the smaller villages that could now be by-passed on the highway, tourist numbers decreased substantially in the years following the opening of the tunnel and the

[19] This increase was also visible in the number of passengers of the mountain railways in the Leventina. Both the aerial cableway Airolo - Sasso della Boggia as well as the funicular railway Piotta - Ritom experienced increases in the year after the opening of the tunnel, as Peyer (1982) reports.

completion of the highway through the Leventina in the early 1980s. In Faido, for instance, the average number of tourists per year declined by 30% from 8,180 in 1970-1979 to 5,740 in 1981-1990, according to data from the Swiss Federal Statistical Office. In Ambri-Piotta, the drop was even stronger (-45%). In contrast, tourist numbers remained stable in Airolo (+2%), and even increased in Biasca (+15%). The Gotthard Road Tunnel had also adverse effects on the garages in the Leventina. After the opening of the tunnel, they experienced strong decreases in the amount of repair work (Peyer, 1982).

Overall, the Gotthard Road Tunnel does not seem to have led to an increase in Ticino's income per capita in the decade after its opening, despite its fundamental role for traffic. During the first 12 months after the opening of the tunnel, the number of cars crossing the Gotthard increased by 121% compared to the average during the years 1970-1979 (Peyer, 1982, p. 42-43). The number of trucks that used the Gotthard route rose by even more: In 1979/1980, approximately 21,000 trucks passed over the Gotthard during the summer months. In 1981, after the opening of the tunnel, roughly 170,000 trucks used the Gotthard route. At the same time, the number of trucks on the San Bernardino route declined to less than half (Service d'étude des transports, 1983).

3.5 Conclusion

In recent years, several studies have shown that transport infrastructure expansions may have important effects on regional economic activity. Not all of these effects represent net economic gains, however. A new transport link may also benefit certain regions at the detriment of others.

In this chapter, we examine the net effect that expansions of the Swiss highway network had on income per capita. Using the synthetic control method developed in Abadie, Diamond and Hainmueller (2010), we analyze two cases – the highway between Geneva and Lausanne and the Gotthard Road Tunnel – where a potential effect would not have been the result of simple relocation of people and businesses across regions. In both cases, we find little evidence for a positive short- or medium-term impact of the network expansion on regional income per capita.

More generally, our findings suggest that advanced network expansions – i.e., improvements of a transport link that already existed before in terms of capacity or speed – may not lead to substantial increases in income per capita of the affected regions. This result confirms findings from earlier studies (e.g., Frey, 1979) that a sound infrastructure is only a necessary, but not a sufficient condition for regional growth, and has two important policy implications. First, with respect to development programs, the Ticino example provides evidence that transport infrastructure investments are no silver bullet to promote economic convergence of peripheral regions, even though they may yield substantial benefits at a more aggregate macro level. Second, the Geneva-Lausanne example shows that the benefits of improving the connectivity between two already well developed economic centers seem limited. This result is relevant for today's discussions on high-speed rail between cities if one assumes that these fast railway connections constitute a similar upgrade to normal rail as do highways to general roads.

Despite the absence of an overall net benefit for the affected regions in terms of income per capita, transport infrastructure expansions do not remain without effect altogether. First, new highways may have a strong impact at the local level, by making certain places more attractive than others, as the analysis in Chapter 2 shows.

Second, this study only examines the short- and medium-term effects of the transportation network. It could be that the construction of the highway system had effects that only materialized in the longer term, or only when the full network was completed.

Third, certain benefits may not show up in official output or income statistics, but still contribute to aggregate social welfare. Such benefits include shorter commuting times, less accidents, the possibility to separate place of work and place of residence, or an increased set of options of leisure activities within reach.

Understanding these longer-term and less tangible benefits of transport networks, as well as further disentangling the redistributive from the productivity-enhancing effects of infrastructure investments are priorities for future research. Moreover, one can conjecture that in the knowledge economies of the 21st century, where gains from transportation network expansions "come ultimately from the benefits of easy access to other people, not from saving transport costs for goods" (Glaeser & Kohlhase, 2003, p. 200), airports may play a role similar to that of highways in the last century. Quantifying these benefits would be an important extension of the existing literature.

Chapter 4

Fixed broadband and economic growth: Resolving the aggregate impact puzzle

4.1 Introduction[1]

At the end of the 1990s and at the beginning of the new millennium, fixed broadband infrastructure was deployed across OECD countries. Since then, a number of studies have tried to quantify the impact of broadband on economic outcomes such as output or productivity. Most studies conducted at the firm, industry and regional level find little evidence for a positive effect of broadband during the first years of its roll-out. In contrast, some cross-country analyses – in particular the study by Czernich, Falck, Kretschmer and Woessmann (2011) published in the Economic Journal – report a strong growth impact.

The identification of a strong impact of broadband on GDP per capita at the country level is puzzling for two reasons. First, one would expect that if broadband triggered substantial growth at the aggregate level, this effect should also have been visible at a more granular level. However, as the literature review in this chapter suggests, such micro evidence is scarce.

Second, in contrast to the U.S., productivity growth in many European countries and Japan was relatively slow during the period of broadband roll-out. Between 1995 and 2005, improvements in total factor productivity on average only contributed 0.4 percentage points to annual value added growth in Europe and 0.5 in Japan, compared to 1.3 percentage points in the U.S (O'Mahony and Timmer, 2009, p. F385). Commentators agree that this productivity gap primarily stems from the failure of European and Japanese firms to exploit the potential of IT in the same way as their U.S. competitors (see Bloom, Sadun and Van Reenen, 2012). At the same time, Europe and Japan also invested less in IT capital than the U.S. Against this backdrop, an average GDP per capita increase of 2.7–3.9 percent across

[1] This chapter is joint work with Uwe Sunde.

the OECD as a result of broadband introduction, as reported by Czernich et al. (2011), appears large.

How can this aggregate impact puzzle be explained?

In Section 4.2, we review the literature on the role of broadband infrastructure for economic growth at different levels of aggregation and provide evidence that reports of large broadband impacts on growth seem confined to country-level studies. In Section 4.3, we resolve the aggregate impact puzzle in two steps. First, we show that there is no positive relationship between broadband penetration and total factor productivity, even when allowing for a non-linear pattern. Second, we offer an alternative interpretation of the results by Czernich et al. (2011) that is consistent with the absence of a positive impact of broadband on growth. Section 4.4 concludes.

4.2 Summary of existing evidence

The question whether the roll-out of fixed broadband infrastructure had a positive impact on economic outcomes has attracted considerable attention in the economic literature over recent years. Broadly, the studies can be classified into four categories: Macro-, industry- and firm-level studies, as well as quantifications of the consumer surplus resulting from broadband.

4.2.1 Macro level

Several studies examine the impact of broadband and the macro level, exploiting variation in broadband adoption across countries, regions or municipalities.

Generally, studies conducted at smaller levels of aggregation (e.g., ZIP-code level or counties) find no or only small productivity gains from broadband. Forman, Goldfarb and Greenstein (2012), for instance, show that over the period 1995 through 2000, only a small fraction of counties in the U.S. benefited from advanced internet applications in terms of wage growth. The counties that gained from the internet were better off in terms of income, population, education, and ICT use already back in 1995. For these counties, advance internet applications explains more than half of the additional wage growth over the observation period, suggesting that the advent of the internet further exacerbated regional wage inequality. Across all counties, however, the rise of the internet only explains a small fraction of the total wage growth between 1995 and 2000.

Similarly, Gillett, Lehr, Osorio and Sirbu (2006), exploiting differences in the availability of mass-market broadband across communities in the U.S., do not find a statistically significant impact of broadband on average wage levels over the period 1998 and 2002.

In contrast, country-level studies typically report positive economic effects of broadband on GDP per capita. However, the results vary widely across studies. The estimates for the impact of every 10 percentage-point increase in broadband penetration on GDP per capita growth range from 0.25 to 1.38 percent (ITU, 2012, p. 5). These differences can only be partly explained by differences in aggregation levels or country samples. The two studies by Koutroumpis (2009) and Czernich, Falck, Kretschmer and Woessmann (2011), for instance, both focus on OECD countries, but their estimates of the impact differ substantially.

Using a simultaneous equations model, Koutroumpis (2009) shows that the growth impact of broadband is higher in countries with a higher adoption rate. According to the three-stage least squares estimates, which he deems more precise, a 10 percentage-point increase in the broadband penetration rate is associated with a GDP increase by 0.08 percent in countries with low broadband penetration (<20% in 2006). In countries with medium (<30% and >20%) and high (>30%) penetration levels, the corresponding increases amount to 0.14 and 0.23 percent, respectively.

The growth impact reported by Czernich et al. (2011) is much larger. Based on a panel of 25 OECD countries over the period 1996-2007, Czernich et al. consider two different regression specifications. In the first specification, they analyze the impact of the *introduction* of broadband on the *level* of GDP per capita. The OLS results suggest that GDP per capita was on average about 1.9 to 2.5 percent higher after than before broadband introduction. The estimate based on an instrumental variable approach that uses the prevalence of traditional voice-telephony and cable-TV networks to instrument for broadband penetration is even higher, at 2.7–3.9 percent. The empirical specification makes strong steady state assumptions, however, which are violated in practice[2]. In the second specification, the authors investigate the impact of broadband *diffusion* on the *growth rate* of GDP per capita. The OLS estimates imply that a ten percentage-point increase in the broadband penetration rate leads to a rise in the annual GDP per capita growth rate of 0.65 to 0.91 percentage points. Again the IV results are larger, implying a rise in the growth rate by 0.9 to 1.5 percentage points for every 10 percentage-point rise in broadband diffusion over the sample period.

There are a few other country-level analyses that report large benefits of broadband on growth, e.g., the studies by Qiang, Rossotto and Kimura (2009) and by Ng, Lye and Lim (2013). These studies do not specifically focus on OECD countries, however. Qiang, Rossotto and Kimura (2009), for instance, estimate the impact of broadband penetration on average GDP per capita growth between 1980 and 2006 based on a cross-section of roughly 120 countries; Ng, Lye and Lim (2013) examine 10 countries of the Association of Southeast Asian Nations (ASEAN) over the period 1998 through 2011. Moreover, in the study by Qiang, Rossotto and Kimura (2009), the positive coefficient for broadband likely

[2]The coefficient on the variable "GDP per capita in 1996" in the second specification has a highly significant negative coefficient (see Column 1 in Table 4.2 below), pointing to convergence within the sample of OECD countries.

also proxies for other differences across countries that affect growth.

Looking at the more recent period 2005 through 2009, Thompson and Garbacz (2011) analyze the impact of fixed and mobile broadband on GDP per household in both high- and low-income countries. They find that in contrast to mobile broadband, fixed broadband penetration was not positively associated with output in high-income countries over the observation period.

4.2.2 Industry level

Evidence on the effects of broadband at the industry level is scarce. In an early cross-state study for the U.S., Crandall, Lehr and Litan (2007) find that output of goods and services seemed to be positively associated with broadband use over the period 2003-2005. However, their estimates are not statistically significant, in contrast to the findings for employment. Kolko (2010, 2012) conducts a similar analysis for the period 1999-2006 at the local level. He also finds a positive effect of broadband on employment growth, but no effect on the average wage level nor the employment rate.

Bartelsman (2008) investigates the impact of broadband use on industry output for a sample of eight European countries over the period 2001-2005. In a cross-country regression, he identifies a positive correlation between the share of workers with broadband access and real value added of the industry. However, the coefficient is also positive and highly statistically significant when broadband use is alternatively replaced by the share of workers with a computer, the share of workers with internet access, or with the percentage of sales conducted over the internet. Moreover, when looking at individual countries, the relationship between broadband use and industry value added becomes insignificant for Austria, Germany and Italy.

Heger, Rinawi and Veith (2011) examine the impact of broadband availability on entrepreneurial activity in Germany. They report that broadband has a positive influence on company foundations in high-tech industries, but not in other sectors.

Finally, in Chapter 5 of this thesis, I find no evidence that broadband-dependent industries showed over-proportional productivity or output growth in countries with a high broadband penetration rate over the period 2000-2006.

4.2.3 Firm level

The support for a positive impact of broadband from studies at the firm level is relatively weak. Bertschek, Cerquera and Klein (2011) report that during the early phase of broadband deployment in Germany from 2001 to 2003, broadband had no impact on labor productivity of firms. Broadband did, however, positively affect innovation activity and thus could have had positive effects on productivity in the long run. Looking at a sample of 2200 manufacturing firms in Ireland over the period 2002-2009, Haller and Lyons (2012) cannot confirm such longer-term effects. They do not find a statistically significant impact of broadband adoption on the productivity of firms.

Similarly, Colombo, Croce & Grilli (2013) report that the effects of broadband access and broadband applications on the productivity of small and medium-sized enterprises in Italy over the period 1998-2004 generally have been very small. The authors only find positive effects for certain advanced broadband applications in combination with specific organizational changes. Meanwhile, the adoption of basic broadband applications such as e-mail did not have a positive effect on SMEs' productivity.

In 2008, the UK Office for National Statistics published a report funded by Eurostat that linked data on ICT use by businesses from different surveys across 13 European countries. The results on the impact of broadband use on firm productivity are rather inconclusive. Franklin (2008) shows that within the manufacturing sector of most European countries, the share of a firm's workers with access to high-speed internet in 2004 is positively correlated with the level of total factor productivity. However, this correlation does not imply any causal relationship.

In the same report, van Leeuwen and Farooqui (2008) analyze the impact of different ICT variables on firm productivity for the Netherlands and the U.K. In a simultaneous equation model, the different ICT variables are allowed to affect both the stock of ICT capital as well as total factor productivity. The results suggest that broadband use is positively associated with ICT capital, but not with total factor productivity.

The only firm-level study that shows a strong positive impact of broadband is the one by Grimes, Ren and Stevens (2012). These authors compare firms with and without broadband access in New Zealand in 2006 using propensity score matching and report that productivity was 7-10% higher in firms using broadband.

4.2.4 Consumer surplus

A fourth strand of the literature focuses on the consumer surplus generated by broadband. These benefits for consumers are undisputed, but not captured by official GDP statistics and therefore hard to quantify (see Economist, 2013, March 9). Several authors have tried to derive an estimate. Most prominently, Greenstein and McDevitt (2009, 2012) quantify the economic value related to the diffusion of broadband in the U.S. and in a sample of 30 OECD countries. In their analysis, they explicitly account for the fact that broadband cannibalized revenues from dial-up internet access. They calculate a so-called "broadband bonus" defined as broadband revenues minus forgone dial-up revenues plus consumer surplus. For 2010, the quality-adjusted broadband bonus for the sample of 30 OECD countries amounted to roughly USD 550 billion, compared to USD 140 billion in 2006. Similarly, Dutz, Orszag and

Willig (2009) quantify the net consumer benefits from home broadband in the U.S. based on an analysis of people's willingness to pay. For 2006, they report an annual consumer surplus of USD 25.4 billion compared to no internet access, and of 18.7 billion relative to dial-up. This estimate does not include broadband benefits accruing to companies, however.

4.3 Resolving the puzzle

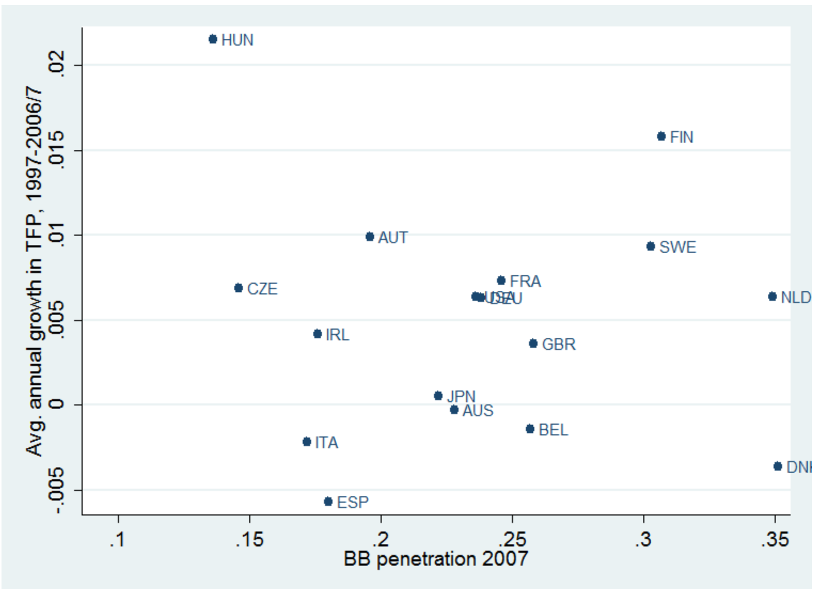
Studies at the firm, industry and regional level typically find no evidence for a positive effect of broadband on productivity. In contrast, certain country-level studies report large GDP gains from broadband. How can we explain this puzzle?

We unravel the puzzle in two steps. First, we provide evidence that total factor productivity did not increase over-proportionally in countries with a high broadband penetration rate. Second, we provide an alternative interpretation of the results by Czernich et al. (2011) that corroborates our findings.

4.3.1 Broadband penetration and total factor productivity

In case the adoption of broadband internet across the OECD led to wide-spread productivity increases in the economy, this should have been reflected in a corresponding rise of total factor productivity. Total factor productivity (TFP) measures 'disembodied technological change', i.e., technological change that cannot be explained by investments physical, human or ICT capital (see O'Mahony & Timmer, 2009, p. F394-F395).

Figure 4.1: Broadband penetration 2007 and average annual growth in TFP of total industries during 1997-2006/7



We analyze the relationship between broadband penetration and total factor productivity for 17 countries[3] for which TFP numbers are available from the EU KLEMS database.

Figure 4.1 plots the correlation between the average annual TFP growth of the economy during 1997-2006/7 and the broadband penetration rate in 2007. The message is clear: Overall, there seems to be no strong positive correlation between broadband penetration and TFP growth.

We also test for a potential non-linear relationship between broadband penetration and TFP growth. Potentially, broadband roll-out only contributed to TFP growth once a certain minimum penetration in the economy had been reached, and/or its effect flattened out once adoption moved from the most productive users to a more wide-spread broadband coverage. To account for such a potential critical mass threshold or saturation point, we include both a broadband squared and broadband^3 term in our regression analysis.

Table 4.1 shows the results for different regression specifications, with TFP level (Columns 1 and 2) respectively TFP growth (Columns 3 and 4) as dependent variable. The analysis covers the period from the year before the first country adopted broadband (1996) until the beginning of the financial crisis (2007). Columns 2 and 4 include the lagged TFP level as additional explanatory variable[4].

Table 4.1: Impact of broadband diffusion on total factor productivity in total industries

Dependent variable: Log level or growth of TFP, total industries, panel 1996-2007

Dep. variable	(1) Level	(2) Level	(3) Growth	(4) Growth
Broadband penetration	-0.668	-0.115	-0.0229	-0.115
	(0.432)	(0.0801)	(0.135)	(0.0801)
Broadband penetration^2	2.633	1.050*	0.785*	1.050*
	(2.866)	(0.575)	(0.376)	(0.575)
Broadband penetration^3	-3.536	-2.113	-1.875*	-2.113
	(5.419)	(1.391)	(0.919)	(1.391)
Lagged log TFP		0.857***		-0.143***
		(0.0283)		(0.0283)
Year dummies	Yes	Yes	Yes	Yes
Country dummies	Yes	Yes	Yes	Yes
Constant	4.612***	0.667***	0.00653*	0.667***
	(0.0112)	(0.131)	(0.00340)	(0.131)
Observations	202	202	202	202
Number of countries	17	17	17	17
R-squared	0.326	0.879	0.119	0.219
Adjusted R-squared	0.275	0.869	0.0529	0.156

Robust standard errors, clustered by country, in parentheses
*** p<0.01, ** p<0.05, * p<0.1
In none of the regressions, the three broadband coefficients are jointly significant.

[3]Australia, Austria, Belgium, Czech Republic, Denmark, Finland, France, Germany, Hungary, Ireland, Italy, Japan, Netherlands, Spain, Sweden, the United Kingdom, and the United States.

[4]Due to the inclusion of the lagged dependent variable, these results are subject to the Nickell bias.

The results provide a consistent picture: In none of the four regressions, the three broadband coefficients are jointly significant at the 10% significance level. Thus, broadband cannot explain the evolution of total factor productivity over the period when broadband was rolled out across the OECD. In the next section, we show that this result is also consistent with the findings of existing macro-level studies that report a positive effect of broadband on growth.

4.3.2 Revisiting the evidence

The most prominent study reporting a positive growth impact of fixed broadband infrastructure is the analysis by Czernich et al. (2011). In Column 1 of Table 4.2, we replicate the original result from Czernich et al. (2011, p. 515) for their smaller sample of 20 countries for which all controls are available. The panel specification uses annual growth in GDP per capita over the period 1996-2007 as dependent variable. Besides the broadband-related variables, the set of explanatory variables includes the growth of capital formation as share of GDP, the growth of average years of schooling of the population aged 15-64, the change in the growth of the working-age population, and the log level of GDP per capita in 1996. We focus on the OLS results to separate the discussion of the main findings from the discussion of the validity of the instrument[5].

As reported by Czernich et al., the broadband penetration variable has a highly significant, positive coefficient. The coefficient of 0.065 implies that for each 1 percentage-point increase in the broadband penetration rate, the growth rate of GDP per capita increases by 0.065 percentage points. However, this coefficient needs to be interpreted carefully. In addition to the broadband penetration rate, the regression equation also includes a variable that measures the number of years since broadband introduction. This coefficient is also highly significant – but has a negative sign. As broadband diffusion progresses, the effects of these two variables are working in opposite directions: A higher broadband penetration rate positively contributes to growth, but at the same time years since broadband deployment increase, acting as drag on economic growth. Which of these two effects dominates, and at what point in time?

Figure 4.2 provides the answer. For the two countries Denmark and Greece, we plot the separate as well as the joint effect of the broadband penetration rate and years since broadband introduction on growth. Given that years since broadband introduction are directly linked to broadband introduction, one should use this joint impact to gauge the

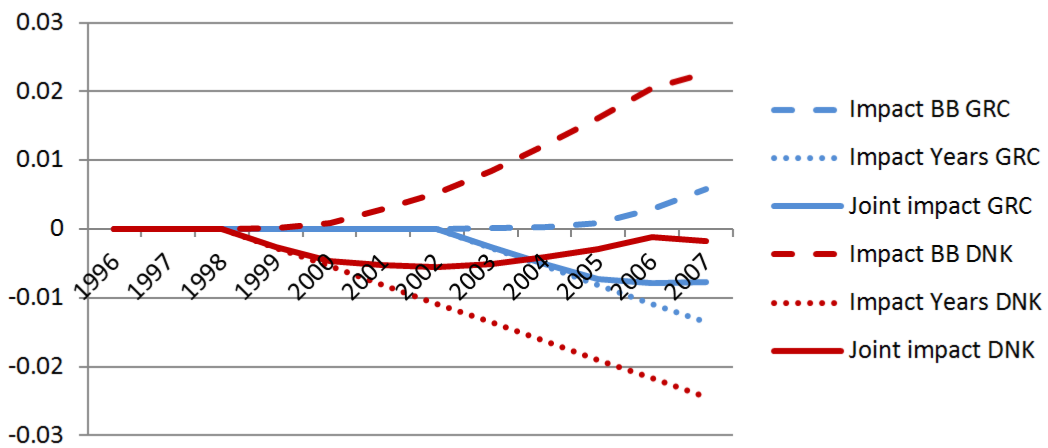
[5]Main underlying rationale for applying an instrumental variable strategy is the concern that the roll-out of broadband infrastructure may have been endogenous to economic growth. In particular, higher economic growth may have led to faster adoption of broadband among companies and households. If this were the case, the OLS coefficient of broadband would overestimate the effect of broadband on growth, and IV results should be lower. However, the results by Czernich et al. (2011) show the opposite relationship, with IV results being higher than OLS results, raising doubts about the validity of the instrument.

Table 4.2: Impact of broadband diffusion on growth

Dependent variable: Annual growth of GDP per capita, panel 1996-2007				
	(1)	(2)	(3)	(4)
Broadband penetration	0.0645***	-0.299***	-0.293***	-0.225
	(0.0210)	(0.0689)	(0.0836)	(0.134)
Years since broadband introduction	-0.00272***			
	(0.000920)			
Broadband penetration^2		2.059***	1.666**	1.430**
		(0.545)	(0.612)	(0.621)
Broadband penetration^3		-3.588***	-2.561*	-2.368*
		(1.130)	(1.261)	(1.155)
Growth of capital formation / GDP	0.0693***	0.0629***	0.0622***	0.0579***
	(0.0103)	(0.0101)	(0.0113)	(0.0106)
Growth of years of education	-0.00660	0.000882	-0.00134	0.00683
	(0.0217)	(0.0202)	(0.0126)	(0.0149)
Δ Growth of working-age population	-0.231	-0.226	-0.247	-0.309**
	(0.218)	(0.207)	(0.152)	(0.127)
GDP per capita in 1996	-0.000625***	-0.000609***	-0.000545***	
	(0.000160)	(0.000148)	(0.000168)	
Year dummies	No	No	Yes	Yes
Country dummies	No	No	No	Yes
Constant	0.0495***	0.0493***	0.0392***	0.0198***
	(0.00709)	(0.00666)	(0.00724)	(0.00254)
Observations	240	240	240	240
Number of countries	20	20	20	20
R-squared	0.299	0.329	0.450	0.460
Adjusted R-squared	0.281	0.309	0.405	0.418

Robust standard errors, clustered by country, in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Figure 4.2: Impact of broadband diffusion on growth according to Czernich et al. (2011) for Denmark and Greece

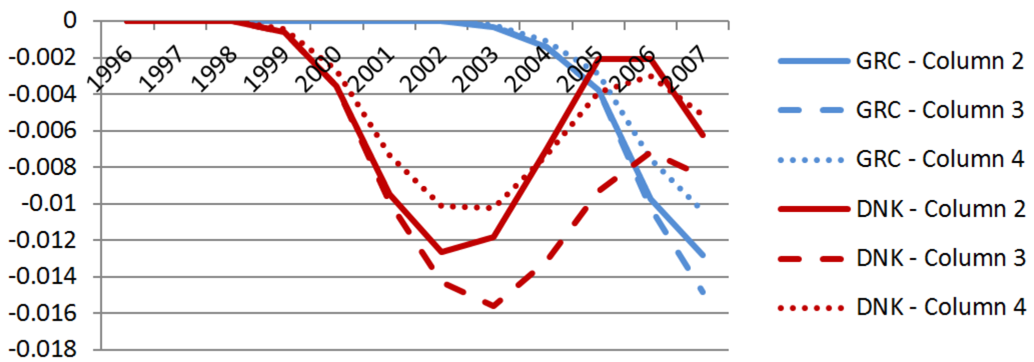


impact broadband had on growth. In 2007, Denmark was the country with the highest broadband penetration rate, Greece that with the lowest.

The figure shows that for both countries, the joint impact of the two broadband-related variables was negative across the whole observation period. Therefore, judging based on their own specification, the results by Czernich et al. (2011) cannot be taken as evidence for a positive impact of broadband diffusion on growth. In contrast, the effect seems to have been negative, likely reflecting the economic downturn following the dot-com crisis.

Using the year of broadband introduction in the analysis is problematic due to some uncertainty around the actual introduction year[6]. Therefore, in Column 2, we exclude the years since broadband introduction variable and instead include the broadband penetration squared and broadband^3 term into the regression equation. Jointly, the three broadband terms could capture both a potential critical mass threshold as well as a saturation point above which the impact of a higher broadband diffusion on growth flattens out. The results in Column 2 show that all three broadband terms are statistically highly significant. To interpret their effect on growth, we again calculate the joint impact for Denmark and Greece over time and present it in Figure 4.3. The main finding remains unchanged: Also for the specification including broadband squared and broadband^3, the net impact on growth is negative over the whole period for both countries. The negative impact seems to be most pronounced at intermediate penetration levels.

Figure 4.3: Impact of broadband diffusion on growth for various specifications for Denmark and Greece



Column 2 in Table 4.2 still does not control for yearly changes in the growth rate that are common to all countries. Therefore, in Column 3, we include year dummies. All three broadband coefficients remain significant at least at the 10 percent level, and the shape of the impact remains very similar to the specification discussed before.

[6]The uncertainty stems from two sources: On the one hand, the year of introduction depends on the threshold at which a country is considered to have adopted broadband. Czernich et al. (2011) use a threshold of 0.1 subscriptions per 100 inhabitants in their OLS analysis, and a threshold of 1 subscription per 100 inhabitants when using predicted broadband introduction based on their instrumented diffusion model. Alternatively, one could also use the first year for which a broadband penetration rate larger than zero has been reported. The second source of uncertainty stems from the fact that different sources – OECD vs. World Bank – contain different penetration values for the first years of adoption. For instance, the World Bank features the first data for Belgium, Japan, South Korea and the Netherlands already in year 1998, whereas the first available data from the OECD for these countries stems from 1999.

Finally, in Column 4 we replace the GDP per capita term from 1996 by country fixed effects to restrict identification to within-country variation only. In addition to convergence, the country dummy also controls for other factors that may lead to growth differences across countries (e.g., the degree of innovation). The broadband penetration variable fails to reach the 10 percent significance level by a small margin (p-value of 0.109), but the coefficients of the three broadband variables are still jointly significant at the 5 percent level and remain largely the unchanged, as does the shape of the impact.

Taken together, the regressions corroborate our finding that broadband penetration did not lead to an increase in output per capita at a large scale. Rather, it seems that the correlation between broadband penetration and growth reported by Czernich et al. reflects a heterogeneous development across countries with respect to the dot-com crash and the subsequent rebound. Based on the original specification used by Czernich et al., we are able to show that a higher broadband penetration rate does not imply a higher growth rate for any of the observed penetration levels until 2007. This finding is confirmed by analyses including the broadband penetration, broadband penetration squared and broadband^3 term to account for a potential critical mass threshold and/or saturation point.

As mentioned in Section 4.2, there are a number of other studies at the country level that report smaller, but still positive growth impacts for broadband. Koutroumpis (2009), for instance, states that on average, approximately one-tenth of annual growth in the OECD over the period 2002-2007 can be attributed to broadband infrastructure. While we cannot rule out that these results indeed reflect true gains from broadband, it seems likely that they also capture at least partly the impact of other growth drivers.

Country-level analyses may overestimate the impact of broadband on growth if broadband proxies for other variables that are correlated with broadband, but not included in the regression. Typically, studies include broadband as only technology-related variable. Therefore, the positive coefficient may reflect other trends happening at the same time that had a heterogeneous effect across countries. Such trends may include the rise of other ICT-related variables (e.g., computers or mobile phones), or other changes such as the ongoing disaggregation of global value chains[7], or the liberalization of labor markets in certain European countries. Vu (2011) shows in an analysis of ICT as source of economic growth that when included individually, the diffusion of personal computers, mobile phones and internet are all positively associated with economic growth over the period 1996-2005.

[7]See Autor, Hanson and Dorn (2013) for an analysis of the impact of rising Chinese import competition on local labor markets in the U.S.

4.4 Conclusion

Existing empirical evidence on the impact of fixed broadband on economic growth poses a puzzle: While most studies conducted at the firm, industry, and regional level do not find a positive impact on productivity, certain studies at the country level suggest that the deployment of fixed broadband infrastructure led to a substantial increase in GDP per capita across OECD countries. Such a large impact would have particularly been remarkable given that during the broadband roll-out period in the late 1990s and early 2000s, many European countries and Japan only experienced relatively small increases in aggregate productivity levels.

In this note, we summarize the existing literature and resolve the puzzle in two steps. First, we show that at the country level, data suggest no positive relationship between broadband penetration and total factor productivity growth across the OECD, even when allowing for a non-linear pattern. Second, we revisit the study by Czernich, Falck, Kretschmer and Woessmann (2011) and offer an alternative interpretation of their results that is consistent with the absence of a positive impact of broadband on growth.

Taken together, the results confirm the findings from studies at more granular levels that broadband does not seem to have led to productivity increases at a large scale across the OECD during the first decade after its deployment. This finding does not preclude that certain countries, regions, industries, or firms may in fact have benefited from broadband. Bartelsman, Gautier and de Wind (2010), for instance, argue that a higher degree of employment protection may have been the reason why Europe was less successful in exploiting the benefits from ICT than the U.S. With respect to industries, it seems likely that certain services were better positioned to benefit from broadband than other industries in the primary and secondary sector. Even within industries, certain companies may have been more successul than others in realizing gains from the new technologies available. Bloom, Sadun and Van Reenen (2012) provide evidence that American-style people management practices may have played an important role. Understanding such heterogeneities as well as broadband's longer term impact on the economy will be a research priority going forward.

Chapter 5

Fixed broadband and economic growth: Industry-level evidence from the OECD

5.1 Introduction

With the rise of the internet, information exchange has undergone a fundamental transformation over the last two decades. A key enabler of this process was the deployment of fixed broadband infrastructure at the end of the 1990s and beginning of the new millennium. Broadband infrastructure is commonly defined as a fixed line with a download bandwidth of 256 kbit/s or more. Today, the average advertised download speed in the OECD has reached 40 Mbit/s, and penetration ranges from 10 subscriptions per 100 inhabitants in Turkey, to 40 in Switzerland.

While the impact on individuals' lives is beyond debate, the identification of broadband's impact on output and productivity has proven challenging. A number of empirical studies have tried to estimate the magnitude of broadband's impact on economic output variables over the last years. Overall, the results are "not quite conclusive", as the International Telecommunication Union ITU (2012, p. 2) states in a recent literature survey. Studies conducted at the macro level generally report a positive impact on GDP per capita, but estimates differ strongly across studies[1]. Evidence from industry- and firm-level analyses is even weaker, with the majority of studies finding no positive effect of broadband on productivity[2]. Given the multitude of methodologies applied and research challenges in-

[1]See Koutroumpis (2009), Qiang, Rossotto and Kimura (2009), Czernich, Falck, Kretschmer and Woessmann (2011), Thompson and Garbacz (2011), or Ng, Lye and Lim (2013) for studies conducted at the country level, and Gillett, Lehr, Osorio and Sirbu (2006), Del Bo and Florio (2012), or Forman, Goldfarb and Greenstein (2012) for analyses at the community or regional level. For a more detailed review of the literature and the main research challenges, see ITU (2012). Holt and Jamison (2009) provide a survey of the studies focusing on the U.S. experience.

[2]The main findings from firm-level studies are summarized in Section 5.2. For studies focusing on industries, see, e.g., Crandall, Lehr and Litan (2007), Bartelsman (2008), Kolko (2010, 2012), or Heger, Rinawi and Veith (2011).

volved, these discrepancies do not come as a surprise, but demonstrate the need for a better understanding of the role of broadband in economic growth.

Meanwhile, policymakers almost unanimously continue to advocate further government efforts to foster the roll-out of broadband technologies. The European Commission (2012), for instance, states that innovation triggered by broadband may create 2 million additional jobs by 2020 (pp. 13-14). In the U.S., President Barack Obama promotes the construction of a nationwide broadband network as a way to "strengthen our economy and put more Americans back to work" (The White House, 2012).

Against this backdrop, the question whether broadband stimulates growth – and if, how and under what conditions – remains an important issue. The literature offers a broad list of ways how broadband may have led to a rise in output or productivity (see Section 5.2). Interestingly, to date, these channels have largely remained untested.

In this chapter, I investigate three potential growth channels of broadband infrastructure based on productivity data at the industry level. My approach rests on the assumption that not all industries benefited from broadband in the same way. For each potential growth channel, I expect a different set of industries to have been more likely to realize productivity gains than others.

For my analysis, I rely on an empirical strategy that is inspired by the seminal paper by Rajan and Zingales (1998). In their "Financial Dependence and Growth" analysis, they highlight the importance of a well-functioning financial system for economic growth. They show that industries that are strongly dependent on external finance grow disproportionally faster in countries with a more developed financial system. Analogously, I examine broadband dependence and test whether industries with strong broadband dependence increased their productivity over-proportionally, either directly after broadband introduction or as broadband diffusion progressed.

I proxy broadband dependence in three ways, each of which addresses a different potential growth channel. First, broadband dependence is measured as share of IT employees in an industry before broadband roll-out, accounting for the fact that industries with a high IT intensity were best positioned to realize substantial productivity enhancements from broadband. Second, broadband dependence is proxied by the research and development (R&D) intensity of an industry prior to broadband roll-out. This definition could be particularly relevant if innovation-enhancing effect of broadband (e.g., in the form of knowledge spillovers within and across firms) was the main growth driver. Third, broadband dependence is proxied by the share of high-skilled workers in an industry. According to the literature on skill-biased technical change, high-skilled workers may have benefited most from broadband-related technologies[3].

[3]For a general introduction to skill-biased technological change, see Acemoglu (2009, Chapter 15); for evidence on broadband's complementarity with human capital, see, e.g., Forman, Goldfarb and Greenstein

The sample analyzed in this chapter consists of 21 OECD countries for which industry-level data on real value added and the number of persons engaged are available from the EU KLEMS database. The main observation period ranges from 1996, the year before the first OECD countries in the sample began to deploy broadband, until before the beginning of the financial crisis (2006).

Consistently across specifications, I find no evidence that broadband led to over-proportional productivity growth in broadband-dependent industries. Rather, in contrast, broadband may have fostered productivity growth in industries that traditionally had a low level of R&D intensity.

More generally, the findings suggest that the deployment of broadband across the OECD did not lead to substantial productivity improvements at the industry level. Further research will be required to identify the circumstances under which broadband still contributes to economic growth.

The remainder of this chapter is structured as follows. Section 5.2 discusses the nature of broadband's impact on productivity in light of existing firm-level evidence. Section 5.3 presents the empirical strategy inspired by Rajan and Zingales (1998). Section 5.4 introduces the data and provides background on the broadband dependence measures. Section 5.5 reports the results of the broadband dependence analysis. Section 5.6 concludes.

5.2 Nature of broadband impact

The nature of broadband's impact on productivity and output is still little understood. Did the impact primarily materialize in the form of productivity increases among early adopting companies? Or did the effect only kick in once a large enough portion of the population had adopted broadband? Was it a one-time increase in output – or does broadband also fuel growth in the long run thanks to a positive effect on innovation?

Deriving answers to these and related questions is challenging, given that there are a multitude of channels via which broadband may have affected productivity growth[4]: directly through cheaper and faster information exchange, process automatization (e.g., e-procurement and e-sales), or offshoring, and, indirectly, by strengthening allocative efficiency thanks to increased market transparency and competition[5], or by fostering innovation. The last channel points to the potential role of broadband as a general purpose technology. Broadband-based applications may increase the overall stock of knowledge in the economy

(2012), or Atasoy (2013).

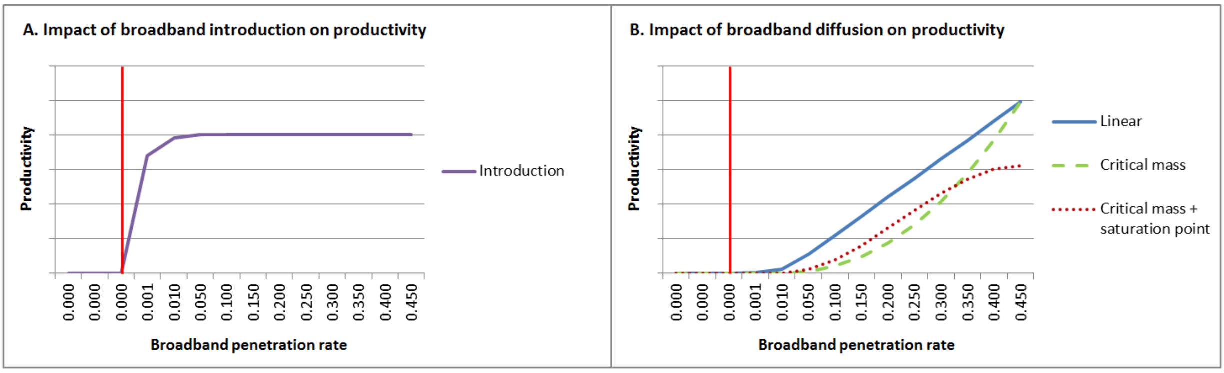
[4]See also OECD (2008) and ITU (2012) for a discussion of different channels through which broadband's effect may have materialized.

[5]Jensen (2007) provides a stark example of how communication technologies may improve market efficiency by analyzing the impact of mobile phones on the fishing industry in the Indian state Kerala. Ater and Orlov (2012) show how the internet transformed the mode of competition in the airline industry.

and at the same time lower the transaction costs for individual firms to make use of this knowledge (Prieger, 2012).

For our analysis, particularly the question whether the impact materialized at the time of broadband introduction or only gradually as diffusion progressed, is crucial. Figure 5.1 depicts the two stylized patterns.

Figure 5.1: Stylized shapes of potential broadband impact



5.2.1 Impact of broadband introduction

The first pattern, displayed in Panel A, shows an impact of broadband introduction on productivity that materializes shortly after broadband becomes available in a country. Such an impact shape would be relevant if broadband led to productivity increases among early adopting companies, and later penetration among the broader business world and population had no substantial impact on productivity.

Companies tend to adopt new technologies faster than households. According to Eurostat, by 2006, broadband adoption in the Euro area had reached 77 percent among companies, but only 31 percent among households. Still, there are several factors that may preclude a substantial effect on productivity during the initial years after broadband becomes available: Companies may have ongoing contracts with existing suppliers, face barriers in adjusting their workforce due to labor market regulations, or require time to adjust their internal business processes. Offshoring certain functions (e.g., call centers) to other countries may take even longer. Moreover, these internal changes typically come at a cost, which may depress productivity in the short run (Basu and Fernald, 2006). Finally, companies may also adopt a 'wait-and-see' attitude until the benefits of the new technologies become apparent (Sadowski, Maitland and Dongen, 2002). For potential gains resulting from broadband's role as a general purpose technology, which may lead to an increase in the innovation rate of the economy in the long run, the time lag might be even greater. The OECD (2008) notes that for the general purpose technologies electricity and steam, it took several decades before the growth effects unfolded.

The empirical evidence for a positive impact of broadband on productivity shortly after the introduction is weak. Bertschek, Cerquera and Klein (2011) report that during the early phase of broadband deployment in Germany, from 2001 to 2003, broadband had no impact on the labor productivity of firms. Broadband did, however, positively affect innovation activity and thus could have had positive effects on productivity in the long run.

Similarly, Colombo, Croce and Grilli (2013) report that the effects of broadband access and broadband applications on the productivity of small and medium-sized enterprises (SMEs) in Italy over the period 1998-2004 generally were very small. The authors only find positive effects for certain advanced broadband applications in combination with specific organizational changes. Meanwhile, the adoption of basic broadband applications such as e-mail did not have a positive effect on productivity.

Therefore, overall, it seems more likely that broadband-related productivity increases among firms materialized only gradually and with a time lag of several years[6].

5.2.2 Impact of broadband diffusion

Panel B in Figure 5.1 depicts a pattern where the impact of broadband is related to broadband diffusion. In this case, the deployment of broadband infrastructure does not have an immediate impact on productivity; it only gradually leads to higher productivity once penetration increases. The relationship between the impact and the penetration rate can take various forms. In the simplest case, the impact increases in a linear way as diffusion progresses. However, as has been argued in the literature (see ITU, 2012), the impact may also be characterized by a critical mass threshold and/or saturation point. The critical mass threshold represents a minimum penetration rate that needs to be reached in order for broadband to actually yield a sizable effect. In the figure, that threshold would be at around 10 subscriptions per 100 inhabitants. Meanwhile, the saturation point denotes a diffusion level after which additional increases in broadband penetration do not lead to higher output. This may be due to the fact that late adopters of broadband are likely to realize a smaller benefit from broadband than early adopters (Gillett, Lehr, Osorio and Sirbu, 2006). In the figure, the saturation point would be reached at approximately 40 subscriptions per 100 inhabitants.

A recent literature review by the International Telecommunications Union ITU (2012) suggests that broadband diffusion might indeed influence the magnitude of broadband's impact on growth. In particular, broadband seems to contribute more to economic growth in countries with higher broadband diffusion (see Koutroumpis, 2009).

The published broadband penetration rate relates to the total number of broadband subscriptions by businesses and households in the economy. In the year when broadband

[6]Brynjolfsson and Hitt (2003) provide evidence for such time lags when analyzing the productivity impact of computerization.

was introduced in a particular OECD country, broadband adoption was typically well below 1 subscriber per 100 inhabitants. It seems unlikely that at this low penetration level, broadband would have made a fundamental difference. Moreover, households typically adjust their behavior only incrementally, and not in a disruptive manner. E-commerce, for instance, did not replace traditional retail sales from one day to the next. Rather, it led to a gradual shift across the different sales channels.

Yet also for the period following the initial roll-out years, empirical evidence for a positive impact of broadband on firm productivity is relatively weak. Haller and Lyons (2012), for instance, look at a sample of 2,200 manufacturing firms in Ireland over the period 2002-2009 and do not find a statistically significant impact on the productivity of firms from broadband adoption.

In 2008, the UK Office for National Statistics published a report, funded by Eurostat, that linked data from different surveys across 13 European countries on information and communication technologies (ICT) use by businesses. The results for the impact of broadband use on firm productivity are rather inconclusive. Franklin (2008), for instance, shows that within the manufacturing sector of most European countries, the share of a firm's workers with access to high-speed internet in 2004 is positively correlated with the level of total factor productivity. However, this correlation does not necessarily imply a causal relationship.

In the same report, van Leeuwen and Farooqui (2008) analyze the impact of different ICT variables on firm productivity for the Netherlands and the U.K. In a simultaneous equation model, the different ICT variables are allowed to affect both the stock of ICT capital as well as total factor productivity. The results suggest that broadband use is positively associated with ICT capital, but not with total factor productivity.

The only firm-level study that shows a strong positive impact of broadband is the one by Grimes, Ren and Stevens (2012). These authors compare firms with and without broadband access in New Zealand in 2006 using propensity score matching and report that productivity was 7-10% higher in firms using broadband.

In the analysis, I test for both potential impact shapes, an effect shortly after introduction, as well as a gradual effect that rose as diffusion progressed. The empirical strategy is explained in the next section.

5.3 Empirical strategy

In a seminal contribution, Rajan and Zingales (1998) show that industries that are more dependent on external finance grow disproportionally faster in countries with a well-developed financial system. In their research set-up they exploit the fact that financial dependence varies across industries, while the quality of a financial system differs between countries.

I apply an approach similar to theirs to investigate the effects of broadband infrastructure on productivity growth. Specifically, I construct an interaction term between broadband penetration at the country level and broadband dependence at the industry level. Broadband dependence is defined in three ways: First, as share of IT employees as percent of total employees in the industry (IT dependence). Second, as share of R&D expenditures as percent of total value added (R&D dependence). Third, as share of high-skilled workers (High-skill dependence). For each industry, broadband dependence is held constant across countries. All measures are derived from values in the U.S. in 1997 (= start of broadband deployment), under the assumption that before the introduction of broadband the U.S. economy was a leading economy in terms of IT adoption, R&D activity and the prevalence of high-skilled workers. Using values prior to the roll-out ensures that the analysis is not confounded by potential effects broadband had on the industry structure (e.g., offshoring of certain activities). The dependent variable is growth in productivity, defined as real value added (VA) per worker.

Each of the three broadband dependence measures investigates a different way broadband could have spurred growth. A statistically significant positive coefficient for the IT dependence interaction term, for instance, would suggest that broadband led to substantial productivity improvements among firms with a strong reliance on IT. A positive coefficient for the R&D dependence interaction term would imply that industries with a focus on innovation benefited most from broadband. According to Koutroumpis (2009), broadband "guarantees the provision of information and decreases search and transaction times" (pp. 471-472), thus enhancing the capabilities of workers and fostering the communication between companies. Such a reduction in the cost of information exchange could have had a positive impact on innovation and knowledge spillovers across firms. Finally, a positive coefficient for the high-skill dependence interaction term would indicate that broadband's impact mainly accrued in industries with a large share of high-skilled workers, confirming findings by, e.g., Forman, Goldfarb and Greenstein (2012) and Atasoy (2013) that the internet had a skill-biased impact similar to earlier generations of information technologies.

The approach of exploiting within-country differences across industries not only allows us to identify whether broadband seems to have had a positive impact on productivity. It also provides insights into which channel produced such a potential growth effect. Moreover, thanks to the inclusion of fixed effects, the approach should be less prone to omitted variable bias or model misspecification than other estimation strategies (see Rajan & Zingales, 1998, p. 563).

The approach rests on the assumption that the impact of broadband differs across industries according to a certain characteristic (i.e., broadband dependence, defined in any of the proposed ways). As a consequence, I would not be able to detect a potential growth effect if the impact were the same across all industries (which seems unlikely given existing evidence[7]), or if it were not related to any of the characteristics used as proxy for broadband dependence.

To capture the different impact shapes presented in Section 5.2, I apply two different empirical specifications: the first focusing on the potential short-term impact of broadband introduction, and the second testing for a gradual impact related to the broadband penetration rate.

5.3.1 Impact of broadband introduction

The analysis of a short-term impact of broadband introduction on productivity is challenging given that all countries introduced broadband within a short period of time (i.e., between 1997 and 2000). Moreover, there is some uncertainty around the true introduction year for certain countries (due to limited data availability and some pre-launches in certain regions within countries), and in 2001 the burst of the dot-com bubble led to a decrease in productivity that was not entirely driven by fundamentals, but also included a cyclical component. Nonetheless, one can make the following conjecture: If broadband's main impact materialized shortly after introduction, one should see a difference in productivity growth of broadband-dependent industries between early and late adopters in particular during the period 1996-2000 when the roll-out of broadband started.

The empirical specification closely follows Rajan and Zingales (1998), but uses growth of productivity rather than output as dependent variable. In contrast to Rajan and Zingales, I do not include the lagged industry share as additional explanatory variable besides fixed effects and the interaction term, given that productivity growth is not necessarily linked to the industry share. The cross-sectional growth regression equation reads as follows:

$$\Delta\, ln\ Prod_{j,i} = constant + \beta\,(YBBI_i * BBDep_j) + \delta_i + \rho_j + \epsilon_{j,i} \tag{5.1}$$

$\Delta ln Prod_{j,i}$ is the growth of value added per worker in industry j in country i over the period when broadband roll-out started across the OECD (i.e., 1996-2000). $(YBBI_i{*}BBDep_j)$ is our variable of interest, the interaction between the year of broadband introduction at the country level and broadband dependence at the industry level. In the event of a sizable short-term effect of broadband on productivity in broadband-dependent industries, we would expect this interaction term to have a statistically significant, negative coefficient. In contrast to the second specification, the identification exploits differences in the timing of broadband roll-out rather than changes in the broadband penetration rate. δ_i and ρ_j are a set of country and industry dummies, and $\epsilon_{j,i}$ denotes the country-industry-specific error term.

[7]In a review of the literature on broadband's impact on employment, the ITU (2012) notes that, "research is starting to pinpoint different employment effects by industry sector" (p. 14).

5.3.2 Impact of broadband diffusion

The empirical specification for a potential gradual impact is also inspired by Rajan and Zingales (1998), but uses a panel rather than a cross-sectional set-up. The panel specification accounts for the fact that broadband penetration increased strongly over the observation period, and potential productivity increases may have been related to this rise in broadband diffusion rather than to differences in the level of broadband penetration across countries. I include country-year-specific fixed effects to control for any economy-wide shocks that affected all industries in a similar way in a given year (e.g., cyclical fluctuations). The country-year FEs also capture the part of broadband's effect on productivity that was uniform across the economy. Note that this specification is very flexible in that it does not require broadband's impact on productivity to have been linear, nor the same across countries. It thus would also capture a potential critical mass threshold or saturation point. Similar to the original Rajan and Zingales set-up, the broadband penetration rate only enters the regression equation as part of the interaction term, testing whether productivity grew more in industries with strong broadband dependence as broadband diffusion progressed.

The regression equation reads as follows:

$$\Delta\, ln\ Prod_{j,i,t} = constant + \beta\, (BB_{i,t} * BBDep_j) + \delta_{i,t} + \rho_{j,i} + \epsilon_{j,i,t} \tag{5.2}$$

$\Delta\, ln\ Prod_{j,i}$ is the growth rate of value added per worker in industry j in country i in year t. $(BB_{i,t} * BBDep_j)$ is our variable of interest, the interaction between broadband penetration at the country level and broadband dependence at the industry level. $\delta_{i,t}$ is a set of country-specific year fixed effects that capture shocks that are common to all industries in a country, in particular the business cycle. $\rho_{j,i}$ is a full set of country-industry dummies, which account for the fact that the productivity growth trend may differ across industries and countries. Differences may, for instance, arise between manufacturing and services, more and less developed OECD countries, and due to country-specific factors affecting certain industries (e.g., regulation). $\epsilon_{j,i,t}$ denotes the error term.

5.4 Data

5.4.1 Broadband penetration data

I use data on broadband penetration from the World Bank World Development Indicators. The broadband penetration rate is defined as fixed broadband Internet subscriptions per 100 people, i.e., "the number of broadband subscribers with a digital subscriber line, cable modem, or other high-speed technology" per 100 inhabitants (see http://data.worldbank.org/indicator/IT.NET.BBND.P2). For the first years, I complemented the World Bank data with data from the OECD fixed (wired) broadband penetration historical time series (June 2010). I always use the earliest penetration data that is available in either of the two databases.

Figure 5.2: Broadband subscriptions per 100 inhabitants in the OECD country sample, 1997-2006

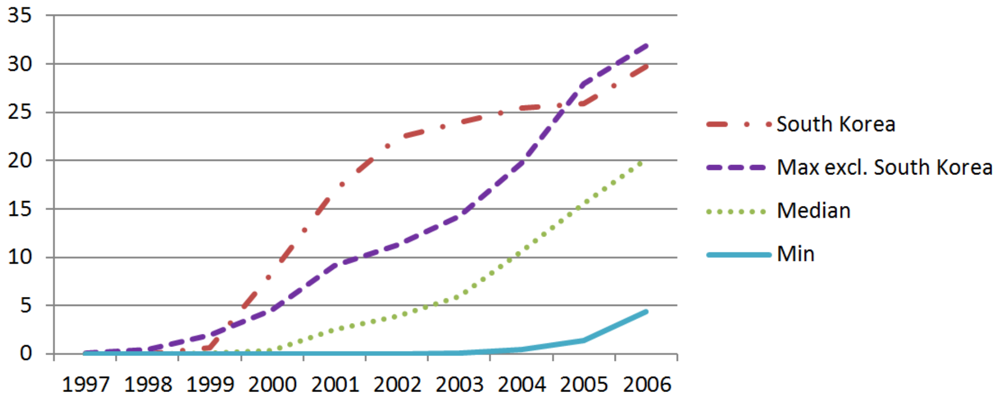


Figure 5.2 shows that during our observation period 1997-2006, broadband penetration differed strongly across the 21 OECD countries in our sample[8]. By the year 2000, all countries in the sample had started to deploy broadband, but only in 2005 did the last country (Greece) reach a penetration level of at least 1 subscription per 100 inhabitants. South Korea was the clear leader in terms of broadband adoption between 2000 and 2004.

5.4.2 Industry-level economic data

Industry-level data on value added and employment come from the "EU KLEMS Growth and Productivity Accounts: November 2009 Release" and cover 21 OECD countries. Luxembourg is excluded from the analysis due to its small size and strong focus on financial services. Data are available through 2006 or 2007. The only exception is Canada where I have to rely on data from the March 2008 release that runs until 2004.

To calculate value added in real 2005 USD, I use the following variables: Gross value added at current basic prices in local currency (VA), Gross value added price indices (VA_P), and average exchange rates for year 2005 from Global Insight and Oanda. For Canada, value added is expressed in real 2004 USD terms.

The number of workers in an industry is measured as number of persons engaged (EMP).

[8]Australia, Austria, Belgium, Canada, Czech Republic, Denmark, Finland, France, Germany, Greece, Hungary, Ireland, Italy, Japan, South Korea, Netherlands, Portugal, Spain, Sweden, the United Kingdom, and the United States.

5.4.3 Broadband dependence data

As mentioned above, I define the degree of broadband dependence of an industry in three different ways. First, I approximate broadband dependence by an industry's dependence on IT, measured as share of IT workers as percent of total workers in the industry in the United States in 1997 (= start of broadband deployment in the U.S.). Atkinson and Stewart (2012) write that, "the number of IT workers in non-IT industries is a good proxy to measure the extent to which traditional industries are making use of IT" (p. 19). Using the values before the roll-out of broadband, this definition of broadband dependence characterizes industries that were likely to benefit from broadband early on in the adoption process in the form of productivity enhancements. The data come from the U.S. Bureau of Labor Statistics.

Second, I use an industry's R&D intensity as a proxy for its broadband dependence. The R&D intensity of an industry is measured as share of R&D expenditures as percent of industry value added in the United States in 1997. This definition accounts for the fact that broadband may have facilitated the exchange of information and ideas and thus may have particularly benefited innovation-driven industries. The data come from the OECD STAN database.

Third, I use the share of high-skilled workers in the United States in 1997, taken from the EU KLEMS March 2008 release. Given its potential complementarity to human capital, broadband may have been particularly beneficial for industries with a large share of high-skilled employees.

Table 5.1 contains the correlations between the different broadband dependence measures. Strong correlations would undermine the analysis in the sense that the different proxies for broadband dependence would measure more or less the same. The correlation matrix shows that this does not seem to be the case. The correlations between IT, R&D and high-skill dependence measures are all below 0.5.

Table 5.1: Correlations between broadband dependence measures

	IT dep.	R&D dep.	High-skill dep.
IT dependence	1.00		
R&D dependence	0.4336 (0.0438)	1.00	
High-skill dependence	0.4688 (0.0078)	0.4259 (0.0690)	1.00

p-values in parentheses

Note: Not all broadband dependence measures cover the same set of industries. IT dependence and high-skill dependence cover 31, R&D dependence 21 industries.

5.5 Results

5.5.1 Impact of broadband introduction

Table 5.2 contains the results testing for the potential short-term impact of broadband introduction. The dependent variable is the 4-year productivity growth between 1996 and 2000, calculated as the difference in the log real value added per worker. The interaction term is constructed as a product of broadband dependence at the industry level and the year of broadband introduction at the country level. In the case of a positive short-term impact in broadband-dependent industries shortly after broadband introduction, we would expect the interaction term to be negative – indicating that broadband-dependent industries grew disproportionally slower in countries that introduced broadband only with a delay.

The results provide no evidence for such a short-term impact. All three interactions are not statistically different from zero, and their point estimates are even positive. This finding is in line with the firm-level studies mentioned in Section 5.2, which found no effect on firm productivity in the initial years after broadband introduction.

Table 5.2: Impact of broadband introduction

Dependent variable: Change in log real VA per worker, 1996-2000

	(1) IT	(2) R&D	(3) High-skill
Year of BB intro * IT dep.	0.00737		
	(0.0108)		
Year of BB intro * R&D dep.		0.00102	
		(0.00461)	
Year of BB intro * High-skill dep.			0.00176
			(0.00156)
Country fixed effects	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes
Constant	-0.604	-2.836	-47.53
	(0.946)	(13.05)	(42.29)
Observations	630	438	648
R-squared	0.063	0.085	0.063

Robust standard errors, clustered at the country level, in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 5.3 shows that the finding does not substantially change if the analysis is conducted for manufacturing and services (excluding utilities) separately. Only the interaction term for the share of high-skilled workers in the services regression is statistically significant – yet again has a positive sign. The coefficient suggests that in countries that introduced broadband comparably late, industries with a large share of high-skilled workers experienced disproportionate productivity growth.

Table 5.3: Impact of broadband introduction for manufacturing and services

Dependent variable: Change in log real VA per worker, 1996-2000

	(1)	(2)	(3)	(4)	(5)	(6)
	IT		R&D		High-skill	
	Manu.	Serv.	Manu.	Serv.	Manu.	Serv.
Year of BB intro * IT dep.	-0.00241	0.00676				
	(0.0402)	(0.00518)				
Year of BB intro * R&D dep.			0.00282	-0.0705		
			(0.00460)	(0.108)		
Year of BB intro * High-skill dep.					-0.000203	0.00188**
					(0.00476)	(0.000873)
Country fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Constant	0.333	-0.992	-7.839	10.55	5.632	-39.30**
	(3.485)	(0.792)	(13.00)	(16.13)	(128.8)	(18.25)
Observations	315	294	273	144	315	312
R-squared	0.120	0.146	0.146	0.203	0.120	0.135

Robust standard errors, clustered at the country level, in parentheses
*** p<0.01, ** p<0.05, * p<0.1

How to interpret this finding? It seems unlikely that the positive interaction term in fact reflects a positive impact of broadband on the productivity growth of high-skilled service workers among broadband late-comers. In countries that introduced broadband in later years, in particular 1999 and 2000, the effect had even less time to materialize. However, services with a high share of skilled workers, such as financial intermediation, may have over-proportionally increased their productivity in these countries independent of broadband deployment.

An alternative interpretation: that the outperformance of services with a large share of high-skilled workers among broadband late-comers actually reflects the outperformance of industries with many low-skilled workers in early adopting countries. The service industries with the lowest share of high-skilled workers are privately employed persons, the sale and repair of motor vehicles, hotels and restaurants, transport and storage, and retail trade. Potentially, these industries experienced disproportional productivity gains not only thanks to broadband, but also in the form of cyclical effects due to the dot-com economy boom in the late 1999s. For the analysis, the year 2000 seems to play a crucial role: For a similar analysis with ending year 1999 or 2001 over 3 respectively 5 years, the coefficient turns negative and loses its statistical significance.

More research will be required to understand the impact of broadband on specific service industries in more detail. However, overall the short-term broadband dependence analysis in the spirit of Rajan and Zingales provides little evidence for a sizable impact of broadband on productivity growth during the initial years following its introduction. Broadband-dependent industries – according to any of the three definitions – did not experience over-proportional growth in countries that introduced broadband relatively early.

5.5.2 Impact of broadband diffusion

Table 5.4 contains the main results for the broadband diffusion panel analysis. The dependent variable is the annual growth of value added per worker. Columns (1), (3) and (5) include the interaction term of the current period; Columns (2), (4) and (6) allow for a lag of one year.

None of the interaction terms is statistically significant, thus again providing no evidence for an outperformance of IT, R&D, or high-skill intensive industries as broadband penetration progressed over the period 1997-2006.

Table 5.4: Impact of broadband diffusion

Dependent variable: Annual growth of real VA per worker, panel 1997-2006

	(1)	(2)	(3)	(4)	(5)	(6)
	IT		R&D		High-skill	
BB penetration * IT dep.	-2.94e-05 (6.41e-05)					
Lag BB penetration * IT dep.		-1.68e-05 (8.33e-05)				
BB penetration * R&D dep.			-1.29e-05 (1.84e-05)			
Lag BB penetration * R&D dep.				-5.62e-06 (2.36e-05)		
BB penetration * High-skill dep.					-9.98e-06 (6.89e-06)	
Lag BB penetration * High-skill dep.						-1.15e-05 (9.06e-06)
Country-industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Constant	0.0156*** (0.00546)	0.0213*** (0.00267)	0.0176** (0.00754)	0.0256*** (0.00648)	0.0147*** (0.00558)	0.0246*** (0.00313)
Observations	6,240	6,240	4,344	4,344	6,418	6,418
R-squared	0.049	0.049	0.065	0.065	0.049	0.049
Number of country-industries	630	630	438	438	648	648

Robust standard errors, clustered at the country-industry level, in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Potentially, the impact of broadband was different for manufacturing and services. For instance, R&D dependence might have been more relevant for manufacturing than services. Therefore, Table 5.5 presents results for manufacturing and services (excluding utilities) separately.

The results provide little evidence for potential impact heterogeneity. Only the interaction between broadband penetration and R&D dependence for manufacturing shows a significant, yet negative coefficient. The negative interaction suggests that as broadband diffusion progressed, manufacturing industries with larger R&D intensity experienced slower productivity growth compared to other industries with a smaller dependence on R&D. Services seem to have followed a similar trend, with a negative point estimate that is even larger in magnitude and that almost reaches the 10-percent significance level (p-value of 0.145). A potential interpretation for this finding could be that broadband helped increase

Table 5.5: Impact of broadband diffusion for manufacturing and services

Dependent variable: Annual growth of real VA per worker, panel 1997-2006

	(1)	(2)	(3)	(4)	(5)	(6)
	IT		R&D		High-skill	
	Manu.	Serv.	Manu.	Serv.	Manu.	Serv.
BB penetration * IT dep.	0.000182	-4.94e-05				
	(0.000181)	(6.43e-05)				
BB penetration * R&D dep.			-4.57e-05**	-0.000419		
			(2.07e-05)	(0.000286)		
BB penetration * High-skill dep.					7.56e-06	-6.74e-06
					(2.59e-05)	(5.70e-06)
Country-industry fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Country-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Constant	0.0164	0.0169***	0.0233**	0.0181***	0.0169	0.0147***
	(0.0104)	(0.00309)	(0.0117)	(0.00364)	(0.0111)	(0.00346)
Observations	3,120	2,912	2,704	1,432	3,120	3,090
R-squared	0.095	0.075	0.109	0.133	0.095	0.078
Number of country-industries	315	294	273	144	315	312

Robust standard errors, clustered at the country-industry level, in parentheses
*** p<0.01, ** p<0.05, * p<0.1

productivity in industries that traditionally were *not* strong in innovation, e.g., through the reorganization of business processes.

5.6 Conclusion

In recent years, several authors have tried to quantify the impact of fixed broadband on economic growth. The findings of these studies at the macro, industry and firm level remain inconclusive, however. Estimates vary from zero effect to sizeable impact on the level or growth rate of output per capita. Moreover, the analyses do not provide insights into the growth channels at work.

In this chaper, I investigate three potential channels for how broadband could have led to growth. The analysis is based on industry-level productivity data for a sample of 21 OECD countries during the period 1997-2006. I rely on an empirical strategy inspired by Rajan and Zingales (1998), where identification of the growth effect of broadband rests on the assumption that the magnitude of the impact differed across industries based on their degree of broadband dependence. I use IT intensity, R&D intensity and the share of high-skilled workers to proxy for an industry's broadband dependence and test for two different impact patterns: a potential impact shortly after broadband introduction that would have been triggered by early adopting companies, and a more gradual effect that rose as broadband was adopted among firms and households across the broader economy.

Consistently across specifications, the results suggest that broadband-dependent industries did *not* experience over-proportional productivity growth during the early years when broadband was rolled out across the OECD. In contrast, I find some evidence that broadband may have fostered productivity growth in industries that traditionally had a low level of R&D intensity, in particular within manufacturing.

The findings cast doubt on some of the growth-enhancing effects of broadband reported in studies at the macro level. They also confirm the results of existing firm-level studies, which generally find no substantial productivity benefits from the use of broadband. In consequence, it seems likely that a sizable portion of broadband's welfare impact has accrued to individuals in the form of higher consumer surplus (thanks to online search, price comparison websites, etc.), which is not captured by traditional output or productivity statistics.

Further research will be required to identify the conditions under which broadband indeed leads to measurable productivity gains, both in manufacturing and in services. Recently, Forman, Goldfarb and Greenstein (2012) have shown that the introduction of advanced internet applications among businesses in the U.S. during 1995-2000 had a heterogeneous effect on regional wages: Only 6 percent of counties experienced an increase in wages, whereas the other 94 percent do not seem to have benefited. The counties with wage growth were typically characterized by a combination of high income, education, population, and IT intensity levels. Potentially, similar complementarities may be at work between broadband and the productivity growth in industries.

Until these channels and interactions are better understood, claims regularly made by policymakers that higher broadband penetration directly translates into higher productivity or output should be treated with considerable caution.

Chapter 6

Parties, political competition, and infrastructure expenditures: Evidence from U.S. states

6.1 Introduction

In recent years, America's allegedly 'crumbling infrastructure' has been a prominent topic of public debate. As part of the American Recovery and Reinvestment Act following the financial crisis, large amounts have been spent on infrastructure maintenance and upgrades. Still, the American Society of Civil Engineers graded America's infrastructure only as D+ in their latest national report card issued in 2013 (Economist, 2013, Apr 6).

Government expenditures are a key determinant of infrastructure provision. In many infrastructure areas, the public or common good characteristics create a role for the government to ensure the provision of a sound infrastructure. As a result, political and institutional factors may potentially have an important influence on infrastructure provision.

Investments in key infrastructure such as transportation are often considered to be a bi-partisan priority, in contrast to other government policies such as public welfare which are subject to partisanship. Therefore, government expenditures for infrastructure may be particularly high in situations of divided government, when at least one chamber of the legislature is not controlled by the party that holds the governorship. In this situation, parties are not able to implement their preferred policies, but need to find a consensus with the opponent party (see, e.g., Alesina & Rosenthal, 1995/1996). Higher investments in infrastructure may be one potential outcome from this balancing of policies between the opposing parties. Likely, the effect is more pronounced for transportation infrastructure than for social infrastructure such as hospitals or parks and recreation facilities, which have a more consumptive character.

I test this hypothesis based on U.S. state-level data on infrastructure expenditures over the period 1970-2000 from the U.S. Census Bureau. I focus on four types of economic and social infrastructure with relevant expenditures at the state level: highways, air transportation, hospitals, and parks and recreation facilities. Applying a panel regression set-up with state and year fixed effects, I exploit changes in true party control of government within the different states over time. I also control for a state-specific linear time trend and a number of state- and time-specific variables, including contributions from the federal government. Particularly for highways, intergovernmental transfers from the federal government are an important funding source for expenditures at the state level.

The results suggest that divided governments increase expenditures for highways compared to true Democratic control. With a rise of close to 6 percent, the impact is particularly pronounced for capital outlays. Unified Republican governments also tend to raise highway expenditures, but the increase is not statistically significant and smaller in magnitude. For airports, which only constitute a small fraction of state expenditures and are lumpy in nature, results provide little evidence for a robust positive effect of divided government. As expected, divided government has little impact on expenditures for social infrastructure such hospitals and parks. An analysis of total state expenditures confirms that the increased level of highway expenditures is not simply the result of divided governments' inability to manage the budget.

As an extension, I also test for an alternative, continuous measure of political competition based on the vote shares of parties in state-level elections. The results suggest that the positive impact of political competition on highway investments is the consequence of the division of the government rather than the outcome of a higher degree of electoral competition. The effect is slightly more pronounced for split legislatures than for split-branch governments.

I also examine the impact of two additional institutional features that may influence infrastructure expenditures: the impact of gubernatorial term limits and lame ducks, and the presence of taxation or expenditure limits. These features do not seem to affect infrastructure expenditures in a significant way. I thus find no evidence that taxation or expenditure limits discourage infrastructure investments, as it is sometimes feared in the literature (see, e.g., Jimenez & Pagano, 2012).

Finally, I test for the impact of the election cycle by including dummy variables for each year of the gubernatorial election cycle. The results suggest that governments increase highway expenditures in the year prior to elections, pointing towards an opportunistic timing of these expenditures.

My study contributes to literature on the political economy of government expenditures with a systematic account of party influences on infrastructure expenditures in the U.S. Studies so far have mainly focused on the impact on total government spending, with mixed findings (see, e.g., Besley & Case, 2003, for an overview). Some studies use total capital outlays as a crude measure of infrastructure investments; none has examined the impact of political characteristics on specific types of infrastructure, however.

In addition, my findings also inform the current debate about the benefits and drawbacks of divided government. In recent years, and at the latest since the government shutdown in the U.S. in fall 2013, the challenges related to divided government have regained attention in the public discourse. Commentators often argue that divided governments lead to political gridlock as parties block each other in the political process. Several theoretical and empricial studies have investigated this issue, again with mixed findings (see Burden & Kimball, 2004, pp. 6-11). My analyses suggest that higher investments in transportation infrastructure are one specific channel how divided governments may lead to policy outcomes that are beneficial for the economy[1].

The remainder of this chapter is organized as follows. Section 6.2 provides an overview of the relevant literature. Section 6.3 outlines the empirical strategy. Section 6.4 introduces the data. Section 6.5 presents the main results for the different infrastructure expenditure categories. Section 6.6 investigates the robustness of the findings, and Section 6.7 contains several extensions. Section 6.8 concludes.

6.2 Literature review

6.2.1 Impact of political parties on government policy

Whether differences in government policies are directly influenced by political parties is an old and heavily disputed question in the political economy literature. According to the median voter theorem formalized by Downs (1957), in a two-party system both parties should implement the policy that is favored by the median voter. In such a theoretical framework, a change of the political party at power should not have an impact on government policies. Consistent with this hypothesis, several authors report that the party label has little explanatory power when investigating policy differences across U.S. states. Garand (1988), for instance, concludes that differences in the growth of government size cannot be explained with political parties. Similarly, Gilligan and Matsusaka (1995) find only weak evidence that political parties influence the overall level of government spending.

However, over the last decades several authors have shown that parties may have more explanatory power than the median voter model predicts. The empirical results come from analyses at different political levels and focus on the following two questions:

1. Do parties have an impact on the *size* of government expenditures?

2. Do parties have an impact on the *composition* of government expenditures?

[1] Another channel may be that divided governments are more likely to adopt welfare reforms than unified governments, see Bernecker (2014).

For the U.S. state level, the literature tends to answer both questions in the affirmative. Overall, Republican governments seem to spend less than Democrats, and on other purposes. I review the literature on both questions in turn. For my analysis, the resulting net effect on infrastructure provision is of primary importance.

A number of authors provide evidence that Democratic governments tend to have a higher level of expenditures and taxes than Republican governments. In an early study, Alt and Lowry (1994) find that expenditures and taxes tend to be higher in states controlled by Democrats. Besley and Case (2003) confirm this finding. Based on their fixed effects panel regression, they report that in particular the fraction of Democrats in the state lower house is positively correlated with a higher level of taxation and spending per capita. In contrast, the effect of a Democratic governor seems to be less clear. Reed (2006) comes to a very similar conclusion. He also finds that taxes are higher when Democrats are in control of the state legislature, and that the party of the governor seems to be of minor importance[2]. Additional studies by Denk (2009), Warren (2009), Besley, Persson and Sturm (2010), and Johnson, Mitchell and Yamarik (2012) confirm that state governments tend to be bigger when controlled by Democrats[3].

Political parties do not only affect the size of government expenditures, but also their composition. Various studies document these differences between Democratic and Republican governments, some of which are directly linked to infrastructure expenditures. Gilligan and Matsusaka (1995), for example, report that "Democratic control of both the executive and the legislative branches leads to significantly higher welfare expenditure than Republican control, and significant lower highway expenditure" (p. 385). Similarly, results by Besley, Persson and Sturm (2010) suggest that Democratic governments controlling both the state house and the senate spend less on infrastructure, measured as capital outlays in percent of total state government expenditure.

Johnson, Mitchell and Yamarik (2012) investigate the same question in even more detail. In their recent working paper, they analyze the impact of political control on five state revenue and five expenditure policies over the period 1970-2010. They define a state to be controlled by Democrats or Republicans if one party controls both the legislature and the governorship. They find strong evidence for policy differences between the two parties. When Republicans are in control, they tend to reduce general and welfare spending and increase capital outlays. In contrast, Democrats increase spending on welfare and reduce capital outlays.

Watkins (2012) conducts a similar analysis of state government spending for the period

[2]Similarly, Ferreira and Gyourko (2009) find in an analysis based on city-level data from the U.S. that the party affiliation of the mayor has no effect on the size of the government, the composition of public spending, or crime rates.

[3]Using a regression-discontinuity design, Pettersson-Lidbom (2008) provides evidence for similar expenditure and tax patterns across local governments in Sweden.

1971-2001. He analyzes how party control at the state level affects growth of total government expenses as well as the share of expenditures for specific types of expenditures, such as education, healthcare, or transportation. He controls for a large set of factors including policy decisions of the nearest neighbor states. His results suggest that Democratic governments spend more on education and welfare than their Republican counterparts, but less on public safety. In contrast, Watkins finds only very weak evidence that Republicans spend more on transportation than Democrats.

With regard to infrastructure expenditures, the review of the literature suggests that Republicans tend to spend more on capital outlays than Democrats, both in percent of government budget as well as in per-capita terms, despite their preference for a lower government share overall.

6.2.2 Impact of political competition on government policy

One of the topics that has attracted considerable attention in recent years is the effect of political competition on policy outcomes. Besley and Case (2003), for instance, find that party competition in the legislature has a statistically significant effect on the level of total taxes per capita and workers compensation across U.S. states. Both taxation and workers compensation are lower when competition between parties is higher. In contrast, political competition does not seem to affect total spending per capita.

More recently, Besley, Persson and Sturm (2010) exploit the substantial variation in political competition across and within U.S. states in the 20th century. They find evidence that higher political competition between Republicans and Democrats made state governments adopt more growth-enhancing policies, e.g., in the form of lower taxes and higher spending on capital outlays (expressed as share of government budget).

Relatedly, there is also a growing literature on the causes and consequences of divided government. In their classical contributions on the causes of divided government, Alesina and Rosenthal (1995, 1996) argue that divided government "is not an accident, but the result of the voters' desire for policy moderation" (1995, p. 2). As such, divided government may lead to an intended balancing of policies between the different parties. Other authors, such as Burden and Kimball (2004), challenge this claim and maintain that divided government is the by-product of other factors, such as the competitivenes of congressional elections.

With regard to the consequences of divided government, the literature has pointed out several drawbacks, such as legislative gridlock, increased interbranch conflict, slower reactions to shocks, and reduced government accountability (see Burden and Kimball, 2004, pp. 6-11, for an overview). In contrast, empirical evidence on beneficial effects of divided government is still scarce. A recent example is Bernecker (2014), who shows that in contrast to the common gridlock argument, divided U.S. state governments are approximately 25 percent more likely to adopt a welfare reform than unified governments. No evidence is provided so far on the role of political competition and divided government for infrastructure expenditures.

6.3 Empirical strategy

For the analysis of the impact of parties and political competition on different types of infrastructure expenditures, I use log infrastructure expenditures at the state level in real terms as dependent variable. Rationale for using log state expenditures rather than (log) expenditures *per capita* is the fact that the the political budgeting process relies on total expenditures as well. I build on a panel regression set-up with state and time fixed effects that exploits within-state variation over time.

Building on Besley and Case (2003), but with the respective adjustments for the log specification, the baseline regression includes a constant, state and time fixed effects, and controls for log state population, the share of the state population aged 65 and above, the share of population aged 5 to 17, a log real state income per capita in year 2000 USD terms. I also add a state-specific linear time trend and a number of additional controls that may influence infrastructure expenditures: Population growth, urban population share, unemployment rate, and thUnion membership rate. As Gilligan and Matsusaka (1995) point out, population growth may lead to an increased demand for infrastructure expenditures, and infrastructure needs may differ between urban and rural areas (see Randolph, Bogetić & Heffley, 1996). ThUnemployment raterate controls for potential cyclical changes in government expenditures over the business cycle. Unions may affect government expenditures through higher wage levels.

In some regressions, I also control for intergovernmental revenues for the respective infrastructure classes from the federal government to states. Particularly for highways, contributions from the federal governments are an important source of funding. Leduc and Wilson (2012), for instance, note that for so-called federal-aid highways, states are often reimbursed for 80 percent of the cost of construction or improvement, up to a certain overall limit. This limit is set through grant apportionment formulas which are based on indicators such as a state's share of the national interstate highway network, or vehicle-miles traveled on interstate highways (p. 8).

The inclusion of these federal contributions as explanatory variables into the regression rests on the assumption that they are not driven by state-level expenditures. This is a strong assumption given that federal contributions are only paid on a project by project basis. As such, an increase in state-level expenditures may lead to an increase in intergovernmental revenue. However, each state's grant apportionments – respectively the share of these apportionments that are actually available to be obligated – constitute an upper limit. Historically, all funds available for states to be obligated have been utilized[4]. Therefore, changes in apportionments can be considered as exogenous shocks to state-level expenditures.

Some authors report that apportionments to states may be affected by political considerations at the federal level. Albouy (2009), for instance, studies the allocation of federal grants by the U.S. Congress. The results of his fixed effects regression suggest that states with Congress representatives of the majority party receive more federal grants than states which are representated by members of the opposition party. This finding in particular applies to transportation and defense spending grants. Similarly, Zhu and Brown (2013) show that states that are better represented on the four Congressional committees responsible for the authorization and appropriation of highway funds receive a disproportionally large share of federal contributions. Including contributions from the federal government into the regression also helps control for such influences.

The definition of the political variables of interest is not completely straightforward. As Besley and Case (2003) note, both the governor as well as the state legislative have an influence on the budget. The incorporation of party control into empirical models has therefore taken various forms. Indicators used include the fraction of Democrats in the lower and upper house, a dummy variable whether the governor is a Democrat, Democratic or Republican control of the lower and upper house (separately or jointly), and the Democratic vote share. Using the fraction of Democrats in the legislative seems problematic as the effect of a percentage point increase likely is not linear, but may depend, e.g., on whether the party has a majority in one or both houses. Moreover, the fraction values might pick up effects that are related to political competition.

Therefore, I use a simple specification that is based on indicators whether Democrats or Republicans are in true control of the government. According to Klarner's (2011) definition, there are two possibilities how a party can be in true control of the government: Either the party has veto-proof majorities in both houses (in which case the party of the governor does not matter), or the party controls both houses and the governor.

To ensure that expenditures relate to the government that enacted them, I include a 1-year lag between the political variables and the corresponding expenditure values. Expenditures in the fiscal year 2000, for instance, relate to the political situation in 1999. As Gilligan and Matsusaka (1995) point out, this time structure is appropriate given that for most states, the fiscal year 2000 covers the period July 1999 through June 2000. I also apply the same 1-year lag to the relationship between the other explanatory variables and government expenditures.

[4]This information was kindly provided by Kimberly Monaco of the Formulation & Apportionments Team of the Federal Highway Administration (FHWA) upon request. On average, 92% of the apportionments (the portion of the federal-aid program provided directly to the States) are available to be obligated.

The regression equation reads as follows:

$$log\, InfraExp_{s,t} = \alpha + \beta\, ControlVar_{s,t} + \gamma\, PoliticalVar_{s,t} + \mu_s + \rho_t + \theta_s\, t + \epsilon_{s,t} \quad (6.1)$$

μ_s denotes the state fixed effect, ρ_t the time fixed effect, and $\theta_s\, t$ the state-specific linear time trend. $\epsilon_{s,t}$ is a state and time specific error term. Standard errors are clustered at the state level to account for autocorrelation over time.

6.4 Data

6.4.1 Infrastructure expenditures

Data on state infrastructure expenditures and intergovernmental revenues from the federal government come from the State Government Finances database compiled by the U.S. Census Bureau. I transformed the nominal expenditures into real year 2000 USD using the consumer price index (CPI) for all urban consumers from the U.S. Bureau of Labor Statistics.

The analysis covers 47 states; Alaska and Hawaii are excluded from the analysis due to their remote location and a lack of data; Nebraska is excluded because its politicians do not affiliate with parties (see Watkins, 2012, pp. 32-33).

Figure 6.1: Median infrastructure expenditures per capita in year 2000 USD

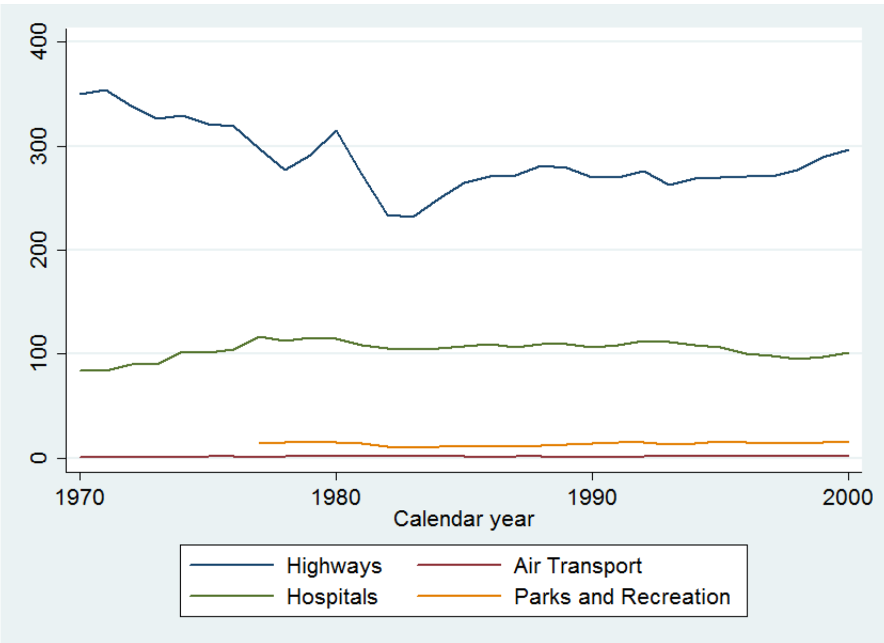


Figure 6.1 displays the median real expenditures per capita for the four different categories of transportation and social infrastructure. The overview shows that highways are by far the largest infrastructure expenditure category at the state level, followed by spending

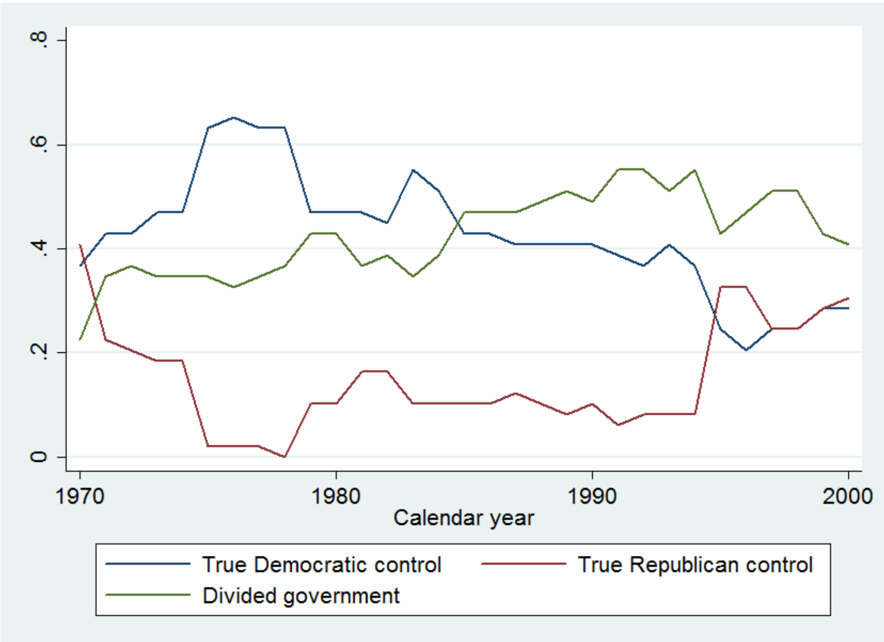
for hospitals. Also the median expenditures for the other two classes are larger than zero, however, for the time periods for which data are available[5].

6.4.2 Political variables

All party and election related variables, in particular the indicators whether Democrats or Republicans are in true control of state government, come from the State Partisan Balance Data prepared by Klarner (2011). The only exception is the measure of political competition, which is directly obtained from Besley, Persson and Sturm (2010).

Figure 6.2 displays the distribution of the different government types between 1970 and 2000. The figure shows that there was substantial variation in the relative distribution of true Democratic, true Republican and divided governments over time, particularly in the 1970s and 1990s. The frequency of divided government increased over time, suggesting that political competition intensified.

Figure 6.2: Relative distribution of government types



6.4.3 Other controls

Of the main economic variables, state income per capita and population size stem from the data collection by Besley, Persson and Sturm (2010). I deflated current income using the consumer price index to obtain real income per capita in year 2000 USD terms. The state population numbers originally come from the U.S. Census Bureau. ThUnemployment raterate has been provided by Watkins (2012), who compiled the time series based on data

[5]Expenditures for parks and recreation have only been separately reported from natural resources since 1977.

from the U.S. Census Bureau, Labor Department, and the Bureau of Labor Statistics. Watkins (2012) is also the source for the states' union membership rates over time.

Data on population demographics – the share of people aged 65 or above and the share of people between 5 and 17 – come from List and Sturm (2006). The share of urban population stems from the U.S. Census Bureau. I have interpolated missing years in a linear manner.

Of the institutional characteristics, the dummy variables for whether a state has a gubernatorial term limit legislation and whether the governor is a lame duck come from List and Sturm (2006). The indicator for the presence of a taxation or expenditure limit in a state has been constructed by Watkins (2012).

6.5 Results

6.5.1 Transportation infrastructure expenditures

Table 6.1 contains the results for the two transportation infrastructure expenditure categories. Columns (1) and (4) provide the regression results that do not control for contributions from the federal government. Columns (2) and (5) include these contributions, and Columns (3) and (6) also a state-specific linear time trend.

The results for highway expenditures in Columns (1)-(3) provide some evidence that these expenditures rise in situations of divided government. When controlling for contributions from the federal government in Column (2), the point estimate suggests an increase of close to 3% compared to unified Democratic control, which serves as the baseline. As expected, the evolution of highway expenditures seems to be strongly affected by intergovernmental contributions from the federal government. The estimate loses its statistical significance at the 10 percent signficance level when state-specific linear time trends are included in Column (3), but only by a relatively small margin (p-value of 0.184). The magnitude of the impact remains largely unchanged.

In contrast, the results in Columns (4)-(6) for airports (air transport) do not point to a relevant influence of parties and divided government on expenditures. The point estimates are by far not statistically significantly different from zero. Again, the contributions from the federal government have the largest explanatory power for the evolution of expenditures at the state level.

Taken together, the baseline results provide some evidence for the hypothesis that divided control affects transportation expenditures in a positive way. At the state level, this effect seems to be limited to highways, which constitute a large fraction of spending.

Table 6.1: Transportation infrastructure

Total expenditures, yearly panel, 1970-2000

Dep. variable	(1) Highways	(2) Highways	(3) Highways	(4) Air Transport	(5) Air Transport	(6) Air Transport
True Rep. control	0.0318	0.0190	-0.00204	0.0359	-0.0756	0.0942
	(0.0303)	(0.0234)	(0.0264)	(0.152)	(0.148)	(0.128)
Divided government	0.0451**	0.0281*	0.0223	0.0276	-0.0491	0.0894
	(0.0202)	(0.0149)	(0.0166)	(0.119)	(0.128)	(0.0963)
Share aged 65+	0.736	-0.584	-1.465	-2.625	-3.383	10.14
	(2.662)	(2.434)	(2.941)	(10.24)	(11.01)	(15.18)
Share aged 5-17	-1.115	-1.057	-1.379	2.271	2.271	4.752
	(0.883)	(0.702)	(0.924)	(5.286)	(5.878)	(5.324)
Log Income per capita	0.448	0.169	0.635*	-0.294	-0.185	0.313
	(0.312)	(0.233)	(0.332)	(1.541)	(1.450)	(1.116)
Log Population	0.272	0.552**	0.425	2.976*	3.297**	4.845**
	(0.359)	(0.256)	(0.595)	(1.610)	(1.475)	(2.383)
Population growth	0.954	0.732	-0.712	4.938	10.22	3.348
	(1.504)	(1.259)	(1.220)	(6.139)	(6.599)	(4.968)
Urban pop. share	0.186	0.0638	0.652	1.787	0.846	5.481
	(0.655)	(0.515)	(0.959)	(3.069)	(2.250)	(4.295)
Unemployment rate	-1.813**	-1.922***	-1.611**	-2.019	-1.076	0.0638
	(0.863)	(0.652)	(0.675)	(5.158)	(4.297)	(3.009)
Union membership rate	0.439	0.0714	-0.0595	4.263*	-3.269*	-0.362
	(0.437)	(0.367)	(0.294)	(2.200)	(1.875)	(2.079)
Log Fed. highways contr.		0.285***	0.265***			
		(0.0479)	(0.0593)			
Log Fed. air transport contr.					0.194***	0.181***
					(0.0472)	(0.0371)
Constant	1.923	-0.619	37.12**	-33.44***	-38.58***	-2.782
	(2.213)	(1.970)	(16.09)	(11.85)	(12.47)	(58.11)
State dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
State-specific linear time trend	No	No	Yes	No	No	Yes
Observations	1,457	1,455	1,455	1,247	787	787
R-squared	0.968	0.977	0.983	0.795	0.875	0.916

Robust standard errors, clustered by state, in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The dependent variables are expressed as log state expenditures in year 2000 USD terms. Alaska, Hawaii and Nebraska are excluded from the analysis.

6.5.2 Social infrastructure expenditures

Table 6.2 presents the results for the two types of social infrastructure expenditures. The findings in Columns (1) and (2) for hospitals provide some evidence for the hypothesis that divided governments may lead to an increase in infrastructure expenditures. However, the coefficient becomes substantially smaller and loses its significance at conventional confidence levels when state-specific linear time trends are included in Column (3). Thus, overall the results do not suggest that divided governments raise expenditures for hospitals compared to unified Democratic control.

For parks (Columns 4-6), I find a similar pattern with the inclusion of state-specific linear time trends rendering the coefficients of the political variables insignificant. In contrast to hospital expenditures, the point estimate for the impact of divided government on park expenditures in Column (6) misses the conventional level of statistical signficance only by a relatively small margin (p-value of 0.168). This finding suggests that divided governments may indeed spend more for parks than unified Democratic governments. However,

Table 6.2: Social infrastructure expenditures

Total expenditures, yearly panel, 1970-2000

Dep. variable	(1) Hospitals	(2) Hospitals	(3) Hospitals	(4) Parks	(5) Parks	(6) Parks
True Rep. control	0.157	0.155	-0.0407	0.145	0.213**	0.0735
	(0.116)	(0.115)	(0.0328)	(0.105)	(0.0922)	(0.0707)
Divided government	0.149*	0.152**	0.00439	0.137**	0.134**	0.0530
	(0.0744)	(0.0738)	(0.0227)	(0.0514)	(0.0517)	(0.0379)
Share aged 65+	-0.715	-0.726	-4.182	9.051*	5.841	-8.984
	(6.241)	(6.166)	(3.297)	(5.311)	(4.663)	(10.64)
Share aged 5-17	0.922	0.743	-1.902	1.173	1.393	2.023
	(2.404)	(2.362)	(1.302)	(1.763)	(2.298)	(2.321)
Log Income per capita	0.289	0.351	0.188	1.778**	1.606*	2.294***
	(0.684)	(0.652)	(0.356)	(0.868)	(0.857)	(0.717)
Log Population	0.341	0.239	1.011	-1.821*	-1.796*	-2.469
	(0.865)	(0.840)	(0.774)	(0.993)	(0.976)	(1.967)
Population growth	2.293	1.993	-1.294	5.746	7.452**	3.167
	(3.549)	(3.367)	(1.362)	(3.906)	(3.603)	(3.087)
Urban pop. share	0.557	0.404	0.529	5.124**	5.358***	-3.458
	(1.825)	(1.821)	(1.870)	(1.936)	(1.868)	(2.793)
Unemployment rate	-0.785	-0.669	-2.013**	2.576	2.992	1.376
	(1.610)	(1.588)	(0.893)	(3.219)	(3.247)	(3.062)
Union membership rate	-0.400	-0.232	0.499	0.540	0.930	-0.214
	(1.024)	(1.008)	(0.375)	(1.012)	(0.932)	(0.848)
Log Fed. health & hospitals contr.		0.102	0.00537			
		(0.0756)	(0.0384)			
Log Fed. other nat. resources contr.					0.0444*	0.0410*
					(0.0249)	(0.0218)
Constant	1.708	1.160	6.833	0.935	2.841	-2.236
	(6.782)	(6.779)	(15.63)	(6.302)	(5.673)	(31.92)
State dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
State-specific linear time trend	No	No	Yes	No	No	Yes
Observations	1,456	1,456	1,456	1,174	1,156	1,156
R-squared	0.948	0.948	0.984	0.916	0.917	0.942

Robust standard errors, clustered by state, in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The dependent variables are expressed as log state expenditures in year 2000 USD terms. Alaska, Hawaii and Nebraska are excluded from the analysis.

this increase is likely driven by partisanship differences between the two parties. Also the coefficient for unified Republican control is positive in all three columns.

The contributions from the federal government have relatively little explanatory power. Potential explanation is that these contributions do not one-to-one relate to the expenditure category under examination, but cover a broader area (e.g., health and hospitals rather than hospitals only). In contrast, expenditures for parks seem to rise as income per capita increases.

In sum, we find only weak week evidence that divided government affects expenditures for social infrastructure in a systematic way. We also do not see major differences between the two parties. One might have expected Republican governments to decrease expenditures for social infrastructure compared to unified Democratic control, for instance.

6.6 Robustness checks

In this section, I examine the robustness of the baseline results in three ways: First, I use 4-year averages over the gubernatorial election cycles instead of the yearly panel to account for the long gestation period of infrastructure projects. Second, I replace total state expenditures on a certain infrastructure category with the corresponding capital outlays only. Third, I check that the observed increases for highways do not simply reflect the inability of divided governments to manage their budget.

6.6.1 4-year averages over gubernatorial election cycles

The baseline specification relies on a yearly panel structure. This specification is best suited to examine effects resulting from changes in the type of government that materialize shortly after the change takes place. For expenditures such as maintenance of roads or improved service levels in hospitals, this specification may be a good fit.

Some infrastructure projects have long gestation periods, however. For such projects, it may take several years until these investments show up in state finances. To account for such a lagged effect, I use an alternative specification where I average expenditures and control variables over 4-year periods for each starting year of a new gubernatorial election cycle[6]. This specification also accounts for the fact that as Johnson, Mitchell and Yamarik (2012) point out, a considerable number of states do not use an annual, but a biennial budget cycle anyways.

The results of the regressions including all controls and state-specific linear time trends are presented in Table 6.3. None of the coefficients for the political variables is statistically significant. Given the large reduction in the number of observations, this is not surprising. However, the point estimate for the impact of divided government on highways implies an increase of 1.4%, which is in the same order of magnitude than the estimate from the yearly panel before.

For the result displayed in Table 6.3, averages are calculated for every starting year of a new gubernatorial election cycle, with a forward lag of one year. For most states, the number of 4-year periods used in the analysis equals the number of gubernatorial election cycles during the observation period. There are a few exceptions, however. Most notably, there are states that hold elections every second year (New Hampshire, Vermont). For these states, the baseline specification in Section 6.5 includes 4-year averages every second year. A number of other states (Arkansas, Iowa, Kansas, Rhode Island, South Dakota and Texas) have switched from a 2-year to a 4-year election cycle during the observation period. Finally, Illinois and Louisiana generally followed a 4-year election cycle, but experienced one period

[6]Note that not all states hold their gubernatorial elections in the same year, and some states follow a 2-year election cycle.

Table 6.3: Robustness 1: 4-year averages over gubernatorial election cycles

Total expenditures, panel based on 4-year gubernatorial election cycle averages, 1970-2000

Dep. variable	(1) Highways	(2) Air Transport	(3) Hospitals	(4) Parks
True Rep. control	-0.0398	0.126	-0.0593	0.0166
	(0.0356)	(0.259)	(0.0795)	(0.105)
Divided government	0.0142	0.0561	-0.0110	0.00264
	(0.0235)	(0.175)	(0.0411)	(0.0766)
Share aged 65+	-3.376	8.457	-7.079	-15.89
	(7.297)	(41.04)	(9.774)	(18.54)
Share aged 5-17	-1.666	11.49	-2.043	4.374
	(1.736)	(17.11)	(2.586)	(4.171)
Log Income per capita	0.524	1.376	0.132	2.445**
	(0.434)	(2.766)	(0.918)	(0.972)
Log Population	0.265	4.293	0.815	-3.656
	(1.006)	(5.202)	(1.790)	(3.005)
Population growth	-0.0863	0.823	-0.443	1.972
	(0.591)	(3.810)	(0.865)	(1.595)
Urban pop. share	1.397	3.798	1.659	-4.203
	(1.102)	(6.400)	(2.636)	(4.261)
Unemployment rate	-1.541**	2.627	-2.791	4.072
	(0.716)	(6.377)	(1.757)	(4.271)
Union membership rate	-0.0186	2.862	0.972	-1.567
	(0.590)	(3.846)	(0.980)	(2.291)
Log Fed. highways contr.	0.355***			
	(0.0672)			
Log Fed. air transport contr.		0.177**		
		(0.0760)		
Log Fed. health & hospitals contr.			-0.0202	
			(0.0646)	
Log Fed. other nat. resources contr.				0.0283
				(0.0326)
Constant	25.98	-7.179	10.60	-51.46
	(31.25)	(143.8)	(35.87)	(55.52)
State dummies	Yes	Yes	Yes	Yes
Time dummies	Yes	Yes	Yes	Yes
State-specific linear time trend	Yes	Yes	Yes	Yes
Observations	357	239	357	318
R-squared	0.994	0.961	0.992	0.973

Robust standard errors, clustered by state, in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The dependent variables are expressed as state expenditures in year 2000 USD terms. All variables are averaged over 4 years for each starting year of a new gubernatorial election period. Alaska, Hawaii and Nebraska are excluded from the analysis.

of 2 respectively 3 years in-between.

To ensure that the results are not driven by these observation periods smaller than 4 years, I also estimated the results based on a restricted sample containing of all gubernatorial election cycles that last exactly 4 years. The findings remain materially unchanged to the ones using all 4-year averages.

6.6.2 Capital outlays rather than total expenditures

The regressions presented in Section 6.5 are based on total expenditures for each infrastructure category. Given that current operational expenditures on infrastructure are largely driven by past investments, one might argue that one should rather look at infrastructure capital outlays rather than at total expenditures. Drawback is that for the two social in-

Table 6.4: Robustness 2: Capital outlays only

Capital outlays, yearly panel, 1970-2000				
	(1)	(2)	(3)	(4)
Dep. variable	Highways	Air Transport	Hospitals	Parks
True Rep. control	0.0272	1.050	-0.0283	0.222
	(0.0369)	(0.652)	(0.146)	(0.159)
Divided government	0.0573**	0.525*	0.0345	0.153
	(0.0235)	(0.307)	(0.0998)	(0.131)
Share aged 65+	0.881	45.77	-6.903	-43.81**
	(4.414)	(36.60)	(8.428)	(19.96)
Share aged 5-17	-2.777*	38.06**	-4.254	-3.291
	(1.401)	(18.42)	(6.397)	(5.839)
Log Income per capita	0.714*	7.378	1.648	2.229
	(0.423)	(4.875)	(1.412)	(1.946)
Log Population	1.272	5.169	-1.172	-2.435
	(0.759)	(11.68)	(2.784)	(4.164)
Population growth	-1.297	-29.13	5.575	4.687
	(1.462)	(21.82)	(8.806)	(8.102)
Urban pop. share	0.593	17.33	3.567	8.212
	(1.736)	(24.13)	(5.255)	(6.737)
Unemployment rate	-2.522***	-10.24	-2.452	-3.541
	(0.890)	(10.71)	(4.709)	(6.007)
Union membership rate	-0.394	-11.71*	1.160	0.582
	(0.394)	(6.046)	(1.425)	(1.843)
Log Fed. highways contr.	0.400***			
	(0.0789)			
Log Fed. air transport contr.		0.358***		
		(0.0585)		
Log Fed. health & hospitals contr.			-0.0290	
			(0.150)	
Log Fed. other nat. resources contr.				0.0640
				(0.0587)
Constant	72.32***	297.6	109.1*	60.08
	(22.28)	(181.0)	(61.09)	(70.63)
State dummies	Yes	Yes	Yes	Yes
Time dummies	Yes	Yes	Yes	Yes
State-specific linear time trend	Yes	Yes	Yes	Yes
Observations	1,455	540	1,453	1,156
R-squared	0.957	0.823	0.829	0.747

Robust standard errors, clustered by state, in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The dependent variables are expressed as log state expenditures in year 2000 USD terms. Alaska, Hawaii and Nebraska are excluded from the analysis.

frastructure categories, the share of capital outlays in terms of total expenditures is small.

Table 6.4 contains the results with capital outlays rather than total expenditures as dependent variable. The findings in Column (1) for highways provide additional support for the hypothesis that divided governments invest more in economic infrastructure. The coefficient is now larger in magnitude (5.7%) and significant at the 5 percent signficance level. In contrast, the estimate for unified Republican governments is by far not statistically significant.

For air transport, results in Column (2) suggest that if anything, both divided and Republican governments raise capital outlays for airports compared to unified Democratic control. The estimates are very large, however, likely driven by the fact that investments in airports only account for a very small share of state budgets and are lumpy in nature.

For hospitals, the estimates in Column (3) do not point to significant differences between the different types of government. Yet as it is the case for total expenditures, the point estimate for divided government is positive.

Finally, Column (4) provides some evidence that unified Democratic governments (baseline) spend less on capital outlays for parks than true Republican and divided governments. The estimates are not statistically significant, however. As mentioned before, these insignificant findings are no surprise as capital outlays only constitute a small fraction of the the total expenditures on social infrastructure.

In sum, the results based on capital outlays confirm the finding that the division of government seems to have a positive influence on highway expenditures, but not on the other three infrastructure categories.

6.6.3 Aggregate expenditures

The results for the disaggregated infrastructure categories suggest the impact of divided government on state budgets is concentrated in the area of highway expenditures. The type of government may also influence expenditures in non-infrastructure areas, however. Therefore, I check that the observed increases for highways do not simply reflect the inability of divided governments to keep expenditures under control.

Table 6.5 presents the results for the impact of the political and institutional variables on aggregate state expenditures. As for the analysis of infrastructure expenditures, the regression includes total contributions from the federal government as explanatory variable. I also control for state-specific linear time trends.

The findings suggest that increases resulting from divided government are confined to capital outlays (Column 3). Expenditures for current operations (Column 2) as well as total expenditures (Column 1) do not rise in a significant way compared to true Democratic control. The point estimate for current expenditures is even negative. Therefore, the results do not point to a systematic increase of government expenditures under divided control.

In line with the findings from previous studies (see, e.g., Johnson, Mitchell and Yamarik, 2012), Republicans seem to raise capital outlays compared to unified Democratic control. The impact misses conventional levels of significance only by a small margin (p-value of 0.153). However, the point estimate is smaller than for divided government.

Table 6.5: Aggregate state expenditures

Aggregate expenditures, yearly panel, 1970-2000			
	(1)	(2)	(3)
Dep. variable	Total exp.	Current oper.	Capital outlays
True Rep. control	-0.0139	-0.0146	0.0479
	(0.0127)	(0.0115)	(0.0329)
Divided government	0.00184	-0.00904	0.0617***
	(0.00881)	(0.00879)	(0.0182)
Taxation or exp. limits	0.00212	-0.00563	0.0237
	(0.0122)	(0.0126)	(0.0346)
Gub. term limit legislation	-0.0184	0.00369	0.0216
	(0.0186)	(0.0216)	(0.0721)
Gov. = lame duck	4.35e-05	0.00407	0.00637
	(0.00388)	(0.00501)	(0.0183)
Share aged 65+	-2.292*	-0.246	2.365
	(1.255)	(1.204)	(3.731)
Share aged 5-17	-0.969**	-0.596	-2.610**
	(0.447)	(0.419)	(1.189)
Log Income per capita	0.348*	0.596***	0.908*
	(0.183)	(0.211)	(0.456)
Log Population	0.767**	-0.115	1.179
	(0.337)	(0.416)	(0.712)
Population growth	-0.106	-0.0217	1.228
	(0.679)	(0.765)	(1.517)
Urban pop. share	0.889	1.307**	3.069
	(0.655)	(0.629)	(1.852)
Unemployment rate	0.170	-0.0293	-1.156
	(0.345)	(0.382)	(1.047)
Union membership rate	-0.00544	0.0559	-0.338
	(0.156)	(0.139)	(0.439)
Log Fed. contributions	0.267***	0.302***	0.415***
	(0.0296)	(0.0312)	(0.116)
Constant	-12.93**	-23.51***	98.71***
	(5.933)	(6.397)	(21.87)
State dummies	Yes	Yes	Yes
Time dummies	Yes	Yes	Yes
State-specific linear time trend	Yes	Yes	Yes
Observations	1,457	1,457	1,457
R-squared	0.998	0.997	0.968

Robust standard errors, clustered by state, in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The dependent variables are expressed as log state expenditures in year 2000 USD terms. Alaska, Hawaii and Nebraska are excluded from the analysis.

6.7 Extensions

6.7.1 Alternative measures of political competition

So far, I have measured political competition as the difference between divided vs. unified control of the government. Divided government can be further subdivided into two classes: Split-branch governments, where one party holds the governor and the two legislative chambers are controlled by the other party, and split legislature, where each party controls one of the legislature chambers. According to this discrete measure of political competition, competition increases from unified control over split-branch governments to split legislatures.

Given that the political dynamics are different under the two different forms of divided government, the impact on infrastructure expenditures may vary[7]. Potentially, the intense political bargaining resulting from a split legislature may lead to less infrastructure investments than a split-branch government.

Table 6.6 summarizes the results using this additional distinction within divided governments. None of the estimates is statistically significant. However, Column (1) suggests that the positive impact of divided governments on highways is primarily driven be split legislatures (p-value of 0.110). Similarly, also for air transport and hospitals the point estimates are larger for split legislatures than split-branch governments. Only for expenditures on parks and recreation, split-branch governments seem to raise expenditures more than split legislatures.

Table 6.6: Extension 1a: Split-branch vs. split-legislature government

Total expenditures, yearly panel, 1970-2000

Dep. variable	(1) Highways	(2) Air Transport	(3) Hospitals	(4) Parks
True Rep. control	-0.000514	0.104	-0.0361	0.0679
	(0.0268)	(0.133)	(0.0325)	(0.0734)
Split-branch government	0.0163	0.0705	-0.0140	0.0613
	(0.0176)	(0.0958)	(0.0240)	(0.0448)
Split legislature	0.0310	0.118	0.0312	0.0389
	(0.0190)	(0.115)	(0.0313)	(0.0422)
Share aged 65+	-1.583	9.254	-4.542	-8.800
	(2.953)	(15.61)	(3.317)	(10.71)
Share aged 5-17	-1.386	4.906	-1.925	2.001
	(0.926)	(5.357)	(1.277)	(2.296)
Log Income per capita	0.647*	0.390	0.224	2.281***
	(0.332)	(1.154)	(0.358)	(0.716)
Log Population	0.409	4.716*	0.962	-2.467
	(0.595)	(2.420)	(0.763)	(1.967)
Population growth	-0.736	3.190	-1.367	3.222
	(1.216)	(4.981)	(1.340)	(3.076)
Urban pop. share	0.611	5.322	0.400	-3.402
	(0.953)	(4.320)	(1.826)	(2.746)
Unemployment rate	-1.590**	0.150	-1.950**	1.392
	(0.678)	(2.992)	(0.872)	(3.064)
Union membership rate	-0.0740	-0.325	0.454	-0.219
	(0.287)	(2.066)	(0.377)	(0.847)
Log Fed. highways contr.	0.265***			
	(0.0594)			
Log Fed. air transport contr.		0.181***		
		(0.0371)		
Log Fed. health & hospitals contr.			0.00570	
			(0.0385)	
Log Fed. other nat. resources contr.				0.0414*
				(0.0219)
Constant	37.27**	-3.467	7.337	-2.275
	(16.15)	(58.28)	(15.89)	(31.86)
State dummies	Yes	Yes	Yes	Yes
Time dummies	Yes	Yes	Yes	Yes
State-specific linear time trend	Yes	Yes	Yes	Yes
Observations	1,455	787	1,456	1,156
R-squared	0.983	0.916	0.984	0.942

Robust standard errors, clustered by state, in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Note: The dependent variables are expressed as log state expenditures in year 2000 USD terms. Alaska, Hawaii and Nebraska are excluded from the analysis.

[7] Alt and Lowry (1994) explain the difference between these two forms of divided government in terms of budgeting dynamics and report differential responses to state revenue shocks.

Definitions of political competition based on party control of the different government branches assume that the composition of the government is decisive for the implementation of policies. Alternatively, political competition can be modeled as the degree of electoral competition. Electoral competition may induce politicians to promote certain policies as part of their campaigns in order to win the support of voters (pre-election politics), or it may discipline incumbent politicians as part of retrospective voting (see Persson and Tabellini, 2000, for an introduction to pre- and post-election politics).

To test for such an alternative definition based on electoral competition, I rely on a political competition variable by Besley, Persson and Sturm (2010). They define political competition based on the average vote share of Democrats in all state-wide elections for executive offices, d_{st}, which include U.S. representatives and the governor, but also down-ballot officers such as lieutenant governor or secretary of state. Specifically, the party-neutral measure of political competition is calculated as $\kappa_{st} = -|d_{st} - 0.5|$. The minus sign ensures that higher values of the variable (i.e., less negative numbers) indicate higher inter-party competition.

Table 6.7 reports the respective regression results. The coefficients suggest that the continuous measure of political competition has little explanatory power. In Columns (1)-(3), the standard errors are large compared to the point estimates. Only for parks, the p-value (0.215) comes somewhat closer to conventional levels of statistical significance, but the point estimate is negative, suggesting that more electoral political competition may lead to lower expenditures for parks. The results remain materially the same when the continuous measure of political competition is replaced by a dummy variable that takes the value of 1 in relatively close elections, with closeness defined as $\kappa_{st} > -0.05$ or $\kappa_{st} > -0.01$.

Overall, the inclusion of the continuous measure of political competition confirms that the positive effect of political competition on highways seems to stem from the division of government, rather than from an intensified level of electoral competition.

6.7.2 Additional institutional features

In addition to parties and political competition, state governments are subject to additional institutional features that may affect infrastructure expenditures, in particular: taxation or expenditure limits (TELs), and gubernatorial term limit legislations, leading to governors being so-called 'lame ducks' during their last term.

Taxation or expenditure limits were introduced starting in the 1970s to curb growth of government revenues or expenditures. As Watkins (2012) notes, by 2011 a majority of states had adopted some sort of taxation or expenditure limits. Their introduction may have negatively affected infrastructure expenditures, particularly in times of economic stress.

For gubernatorial term limits, List and Sturm (2006) show based on the example of

Table 6.7: Extension 1b: Continuous measure of political competition

Total expenditures, yearly panel, 1970-2000

	(1)	(2)	(3)	(4)
Dep. variable	Highways	Air Transport	Hospitals	Parks
True Rep. control	-0.00277	0.100	-0.0404	0.0634
	(0.0262)	(0.129)	(0.0331)	(0.0699)
Divided government	0.0220	0.0882	0.00452	0.0505
	(0.0166)	(0.0974)	(0.0226)	(0.0377)
Political competition	-0.0360	0.302	0.0161	-0.379
	(0.0917)	(0.521)	(0.131)	(0.301)
Share aged 65+	-1.495	10.61	-4.169	-9.684
	(2.944)	(14.80)	(3.310)	(10.70)
Share aged 5-17	-1.386	4.824	-1.898	1.861
	(0.923)	(5.306)	(1.303)	(2.364)
Log Income per capita	0.633*	0.369	0.189	2.211***
	(0.333)	(1.120)	(0.358)	(0.698)
Log Population	0.422	4.873**	1.012	-2.537
	(0.591)	(2.354)	(0.774)	(1.959)
Population growth	-0.698	3.177	-1.300	3.311
	(1.222)	(4.907)	(1.375)	(3.032)
Urban pop. share	0.652	5.546	0.529	-3.308
	(0.961)	(4.270)	(1.870)	(2.759)
Unemployment rate	-1.604**	0.0644	-2.016**	1.428
	(0.678)	(3.004)	(0.894)	(3.035)
Union membership rate	-0.0603	-0.385	0.499	-0.194
	(0.294)	(2.099)	(0.375)	(0.852)
Log Fed. highways contr.	0.266***			
	(0.0595)			
Log Fed. air transport contr.		0.182***		
		(0.0361)		
Log Fed. health & hospitals contr.			0.00525	
			(0.0386)	
Log Fed. other nat. resources contr.				0.0388*
				(0.0215)
Constant	37.00**	0.0143	6.867	-6.760
	(16.17)	(56.63)	(15.70)	(31.28)
State dummies	Yes	Yes	Yes	Yes
Time dummies	Yes	Yes	Yes	Yes
State-specific linear time trend	Yes	Yes	Yes	Yes
Observations	1,455	787	1,456	1,156
R-squared	0.983	0.916	0.984	0.942

Robust standard errors, clustered by state, in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The dependent variables are expressed as log state expenditures in year 2000 USD terms. Alaska, Hawaii and Nebraska are excluded from the analysis.

environmental expenditures that they may have a direct impact on the policies implemented by governors. With regard to infrastructure, different hypotheses are possible: Governors that cannot be re-elected anymore may increase infrastructure expenditures either to build a legacy (out of self-interest) or to pursue long-term projects that only generate benefits after the next election (fostering the common good). Alternatively, infrastructure expenditures could also decrease in the presence of lame ducks as the need for pork-barrel projects for constituencies decreases. Moreover, the introduction of the term limit legislation per se may be a sign for a professionalization of the government and thus be correlated with a change in infrastructure expenditures. Recent research by Jimenez and Pagano (2012), for instance, suggests that legislative term limits may lead to an improvement of state infrastructure management.

Table 6.8 presents the results for the impact of these institutional features. The regression includes dummy variables for taxation or expenditure limits, the presence of a gubernatorial term limit legislation, and whether the governor is a lame duck.

The estimates suggest that these institutional features have little impact on infrastructure expenditures. Only the coefficient for the impact of lame ducks on highway expenditures reaches conventional levels of statistical significance, providing some evidence that governors that cannot be re-elected may decrease infrastructure expenditures. However, the effect is not robust to using highway capital outlays rather than total expenditures as dependent variable.

6.7.3 Election cycle

In a final extension, I investigate whether infrastructure expenditures are subject to a distinct pattern over the election cycle. The literature on opportunistic behavior of politicians suggests that governments might be tempted to increase expenditures shortly before elections in order to stimulate the economy (see, e.g., Persson and Tabellini, 2000, Chapter 16), or to directly gain popularity among certain groups of voters.

I test for such a positive pre-election impact using dummy variables for each year over the gubernatorial election cycle. The first year of a new government serves as baseline (omitted category). As the majority of states follows a 4-year election cycle, we would expect the strongest impact on expenditures during the 4th year. We might also see an increase in year 2 given that some states follow a 2-year election cycle, and governments in states with 4-year cycles might increase expenditures ahead of mid-term elections.

Table 6.9 presents the corresponding results. Given that the analysis focuses on expenditure fluctuations during election cycles, the regressions do not control for the contributions from the federal government. As explained in Section 6.3, apportionments of the federal government are often paid in the form of matching funds, and states have some flexibility in the timing of spending these apportionments. Therefore, state governments may have the incentive to target these funds towards years before elections.

The results in Table 6.9 provide evidence for an election cycle effect for highway expenditures. Expenditures for highways (Column 1) are systematically higher in the year prior to elections, suggesting that governments may be inclined to choose the timing of these expenditures opportunistically. The evidence is even stronger for highway capital outlays as dependent variable. In contrast, I find no evidence for a similar effect for the other infrastructure categories.

6.8 Conclusion

In this chapter, I have examined the impact of parties and political competition on infrastructure provision based on detailed government expenditure data from U.S. states over the period 1970-2000. Previous studies have relied on aggregate measures of infrastructure – in particular total capital outlays – and therefore do not provide a granular picture of the role governments play in the process of provisioning infrastructure. I consider expenditures on four different categories of transportation and social infrastructure and test whether divided governments raise these expenditures compared to situations with unified control, as the public discussions on infrastructure's bi-partisan character might suggest.

In line with the hypothesis, the results provide evidence that divided state governments increase expenditures for highways by 2-3% compared to unified Democratic control. With an increase of close to 6%, the effect is more pronounced for capital outlays than for total expenditures. In contrast, expenditures for air transport, hospitals and parks and recreation facilities do not show systematic spending patterns between the different types of government. An analysis of aggregate state expenditures suggests that the observed increase of highway expenditures does not simply reflect the inability of divided governments to manage their budget.

As an extension, I consider an alternative measure of political competition based on the vote share of parties in state-level elections. The results show that the increase in highway expenditures is the result of the division of government rather than of increased electoral competition. I also test for the influence of additional institutional features – taxation or expenditure limits and gubernatorial term limits – but find little impact on infrastructure expenditures. Thus, the results provide no evidence for the claim that expenditure limits may discourage investments in infrastructure.

The analysis in this chapter focuses on the level of government expenditures. As more data on infrastructure quality becomes available going forward, it will be interesting to also examine the efficiency of such expenditures. It seems likely that the degree of political competition may also affect the quality of infrastructure investments undertaken, particularly in light of so-called pork-barrel expenditures that are being spent to benefit specific lobby groups or constituencies.

Table 6.8: Extension 2: Institutional features

Total expenditures, yearly panel, 1970-2000				
	(1)	(2)	(3)	(4)
Dep. variable	Highways	Air Transport	Hospitals	Parks
True Rep. control	-0.00133	0.123	-0.0417	0.0738
	(0.0255)	(0.136)	(0.0326)	(0.0710)
Divided government	0.0235	0.0728	0.00366	0.0512
	(0.0160)	(0.0969)	(0.0226)	(0.0388)
Taxation or exp. limits	-0.00976	0.250*	-0.000856	0.0224
	(0.0305)	(0.140)	(0.0471)	(0.0647)
Gub. term limit legislation	-0.0160	-0.199	0.0187	-0.0450
	(0.0528)	(0.131)	(0.0595)	(0.105)
Gov. = lame duck	-0.0228*	-0.0420	0.0202	0.00162
	(0.0115)	(0.110)	(0.0181)	(0.0280)
Share aged 65+	-1.392	12.73	-4.275	-8.605
	(2.867)	(15.05)	(3.289)	(10.53)
Share aged 5-17	-1.416	3.658	-1.854	1.886
	(0.934)	(4.825)	(1.306)	(2.409)
Log Income per capita	0.647*	0.0173	0.179	2.260***
	(0.333)	(1.097)	(0.352)	(0.693)
Log Population	0.389	5.556**	1.031	-2.349
	(0.585)	(2.411)	(0.789)	(1.907)
Population growth	-0.679	4.290	-1.333	3.360
	(1.173)	(4.833)	(1.320)	(2.910)
Urban pop. share	0.625	4.686	0.561	-3.447
	(0.945)	(4.219)	(1.864)	(2.796)
Unemployment rate	-1.550**	-0.450	-2.040**	1.334
	(0.643)	(3.025)	(0.892)	(2.973)
Union membership rate	-0.0692	-0.206	0.507	-0.225
	(0.297)	(2.005)	(0.376)	(0.839)
Log Fed. highways contr.	0.264***			
	(0.0586)			
Log Fed. air transport contr.		0.183***		
		(0.0370)		
Log Fed. health & hospitals contr.			0.00359	
			(0.0384)	
Log Fed. other nat. resources contr.				0.0410*
				(0.0218)
Constant	36.80**	0.275	6.525	-1.032
	(15.58)	(51.49)	(16.73)	(29.46)
State dummies	Yes	Yes	Yes	Yes
Time dummies	Yes	Yes	Yes	Yes
State-specific linear time trend	Yes	Yes	Yes	Yes
Observations	1,455	787	1,456	1,156
R-squared	0.983	0.917	0.984	0.942

Robust standard errors, clustered by state, in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The dependent variables are expressed as log state expenditures in year 2000 USD terms. Alaska, Hawaii and Nebraska are excluded from the analysis.

Table 6.9: Extension 3: Election cycle

Total expenditures, yearly panel, 1970-2000

	(1)	(2)	(3)	(4)
Dep. variable	Highways	Air Transport	Hospitals	Parks
True Rep. control	0.0153	0.0645	-0.0404	0.0122
	(0.0301)	(0.150)	(0.0325)	(0.0897)
Divided government	0.0376*	-0.00490	0.00411	0.0508
	(0.0202)	(0.0938)	(0.0231)	(0.0381)
Cycle Year 2	-0.00483	0.0233	-0.00481	-0.00984
	(0.00741)	(0.0515)	(0.00634)	(0.0197)
Cycle Year 3	0.00766	-0.0512	0.00736	-0.00197
	(0.00755)	(0.0431)	(0.00868)	(0.0187)
Cycle Year 4	0.0164**	0.00453	0.00838	0.00521
	(0.00779)	(0.0492)	(0.00904)	(0.0175)
Share aged 65+	0.139	10.85	-4.145	-9.375
	(3.334)	(12.30)	(3.314)	(10.72)
Share aged 5-17	-1.884	0.837	-1.876	1.640
	(1.141)	(6.456)	(1.304)	(1.611)
Log Income per capita	0.644*	0.402	0.190	2.392***
	(0.350)	(1.218)	(0.361)	(0.752)
Log Population	0.699	4.093	1.012	-2.652
	(0.542)	(2.673)	(0.774)	(2.018)
Population growth	-0.810	0.683	-1.336	2.597
	(1.300)	(4.768)	(1.368)	(3.051)
Urban pop. share	1.224	1.898	0.549	-3.142
	(1.186)	(4.785)	(1.844)	(2.745)
Unemployment rate	-1.383	1.738	-2.027**	1.451
	(0.839)	(5.121)	(0.891)	(3.027)
Union membership rate	-0.115	2.054	0.491	-0.434
	(0.329)	(1.991)	(0.375)	(0.868)
Constant	46.10**	-28.18	6.214	2.684
	(19.85)	(48.49)	(16.09)	(32.32)
State dummies	Yes	Yes	Yes	Yes
Time dummies	Yes	Yes	Yes	Yes
State-specific linear time trend	Yes	Yes	Yes	Yes
Observations	1,457	1,247	1,456	1,174
R-squared	0.978	0.861	0.984	0.942

Robust standard errors, clustered by state, in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Note: The dependent variables are expressed as log state expenditures in year 2000 USD terms. Alaska, Hawaii and Nebraska are excluded from the analysis.

Chapter 7

Conclusion

In mature economies, new infrastructure generally does not raise aggregate output and productivity in the short to medium run, but is likely to affect the spatial allocation of economic actors.

This is the key finding of Chapters 2-5 of this doctoral thesis, based on an analysis of the local and regional effects of the Swiss highway network and the impact of the roll-out of broadband infrastructure across the OECD. This result is consistent with the view of infrastructure as baking powder of the economy[1], with growth depending on a large number of ingredients, and infrastructure being only one of them – even though a key one. From the empirical analysis and the discussion, a few general implications for future infrastructure investments in mature economies emerge.

First, infrastructure should be understood as an enabler of economic growth – not as a driver. Enhancing infrastructure networks to accommodate economic development should therefore be policymakers' main concern. In contrast, expectations that infrastructure investments lead to a push of the economy beyond their immediate stimulus effect from construction work may be overly optimistic. Infrastructure is a necessary condition for growth – but by far not a sufficient one. This finding is not new (see, e.g., Frey, 1979), but seems to be often forgotten in practice.

Second, even though a new infrastructure may have little impact on aggregate economic variables such as income per capita in the short and medium run, it often has important effects in other dimensions that affect social welfare. These effects include gains, such as shorter commuting times, cheaper communication, or reduced search and transaction costs. The internet telephony service provider Skype, for instance, did not generate a large amount of revenues in the early years, but likely considerable consumer benefits by offering free (long-distance) communication. At the same time, infrastructure may also have consequences that decrease people's utility, for instance in the form of negative noise externalities from new highways or airports. These effects beyond pure output gains need to be taken into account

[1] I am indebted to Jan Mischke for pointing out this analogy.

in the cost-benefit analysis of infrastructure investments.

Third, the advent of a new infrastructure typically creates winners and losers. A new telecommunications technology (e.g., broadband) may render an older one (e.g., dial-up internet) obsolete. A new highway can make certain regions more attractive, at the detriment of others. These heterogeneities of the impact may have important implications for the political viability of a new project. Do many people benefit, and a few lose? Or do many lose, and only a few benefit? Policymakers should consider such aspects explicitly in order to avoid unintended consequences.

Last but not least, the finding that infrastructure investments do not raise income per capita in the short and medium run does not imply that these investments cannot be efficient. In the empirical analysis, the removal of a severe bottleneck would show up as the strongest impact. For the economy, however, it may be more efficient to ensure that bottlenecks do not emerge in the first place, rather than removing them later on. If capacity is increased timely, the short- and medium-term returns to infrastructure may seem much smaller than they actually are. Therefore, the finding of little short- and medium-term benefits should not be taken as argument that too much investment in infrastructure is undertaken.

These implications apply to mature economies. For developing countries, where a lack of sufficient infrastructure still constitutes a frequent barrier to economic growth, the effect of infrastructure investments on growth may be different. Roberts et al. (2012), for instance, provide evidence that the construction of the Chinese expressway network led to a rise in income. Storeygard (2013) highlights the importance of transport costs for income differences across Sub-Saharan African cities. In a similar way, other authors document a positive impact of the provision of telecommunications services (e.g., Jensen, 2007) and energy (e.g., Andersen & Dalgaard, 2013). Not surprisingly, investments in infrastructure therefore play a crucial role in development programs of organizations such as the World Bank. According to Crescenzi and Rodríguez-Pose (2012, p. 488), roughly 20 percent of World Bank lending in recent years has been allocated to transport infrastructure alone. This share is larger than the sum of resources allocated to health, education, and social services combined.

What are the key determinants of infrastructure provision? This is the question underlying Chapter 6, with a focus on political and institutional factors. The results based on data on U.S. states suggest that parties and other institutional features such as term limits for governors and state taxation or expenditure limits do not have a major influence on economic and social infrastructure expenditures. In contrast, political competition in the form of divided government is positively associated with higher investments in highways, a core economic infrastructure.

Again, it is likely that institutions are much more relevant for infrastructure provision in less developed countries. In particular weak states that lack sufficient fiscal capacity may fall short of providing an adequate level of infrastructure (see, e.g., Acemoglu, 2005, or Besley & Persson, 2011). But also countries at similar stages of development show large differences in terms of infrastructure provision. China, for instance, invests large amounts in infrastructure despite major shortcomings in its political institutions. It even outperformed the leading OECD countries in terms of infrastructure spending in recent years (see McKinsey Global Institute, 2013). In contrast, Latin American countries fail to make substantial investments in infrastructure. Understanding the causes of such differences will be an important research priority going forward.

Decision-making on investments in new or existing infrastructure will always remain risky, given the long-term horizon and lumpy nature of such investments. The research carried out within the scope of this dissertation suggests that these decisions should not be guided by narrow monetary cost-benefit considerations alone. Chances that investments in infrastructure such as a highways or telecommunication networks lead to measurable short-term increases in economic output beyond demand stimulus and spatial relocation of economic activity are relatively low. Still they may create substantial benefits for society – in the long run, and in forms that are not captured by traditional output statistics, but are equally important for social welfare.

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