Empirical Studies on Growth and Comparative Development

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Evaluation committee

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Summary

The thesis consists of five chapters. Each chapter is self-contained and can be read independently.

Chapter 1 is entitled "Lightning, IT Diffusion and Economic Growth across US States", and is joint work with Thomas Barnebeck Andersen, Jeanet Bentzen and Carl-Johan Dalgaard. In this study we show that a particular feature of climate, the frequency of lightning strikes, is associated with slower growth in labor productivity across the 48 contiguous US states after 1990; before 1990, there is no correlation between growth and lightning. The effect of other climate variables (e.g., temperature, rainfall, and tornadoes) does not conform to this pattern. A viable explanation is that lightning influences IT diffusion. By causing voltage spikes and dips, a higher frequency of ground strikes leads to damaged digital equipment and thus higher IT user costs. Accordingly, lightning density (the number of strikes per square km per year) should adversely affect the speed of IT diffusion. We find that higher lightning densities indeed seem to have slowed IT diffusion across US states, conditional on standard controls. Hence, an increasing macroeconomic sensitivity to lightning may be due to the increasing importance of digital technologies for the growth process.

Chapter 2 is entitled "On the Impact of Digital Technologies on Corruption: Evidence from US States and across Countries", and is also joint work with Thomas Barnebeck Andersen, Jeanet Bentzen and Carl-Johan Dalgaard. In this chapter we analyze whether increased Internet use has reduced the extent of corruption. To examine this question we develop a novel identification strategy for Internet diffusion. As aforementioned, by instigating equipment damage, power disruptions increase the user cost of IT capital, and reduce thereby the speed of Internet diffusion and the spread of computer use. Lightning activity is a natural phenomenon causing power disruptions every year, globally. Using global satellite data and data from ground based lightning detection sensors, we construct lightning density data for a large cross section of countries and for the contiguous US states. Empirically, lightning density is a strong instrument for Internet diffusion. In conformity with OLS estimates, our instrumental variables (IV) estimates show that Internet diffusion has indeed reduced the extent of corruption across countries and across US states.

Chapter 3 is entitled "The Impact of Aid on Bureaucratic Quality: Does the Mode of Delivery Matter?", and is joint work with Rainer Thiele. In this chapter we study the impact of development aid on bureaucratic quality, taking into account the fact that some forms of aid are more likely to affect bureaucratic quality than others. In order to make a causal interpretation of the results, we set up an indentification strategy exploiting the relationship between aid allocation, country size, and rates of infant mortality. Using data for the cross section of countries receiving foreign aid from 1995 to 2005, we find that aid given in the form of grants appears to have impaired the well functioning

of bureaucracies, whereas aid given in the form of loans seems to have had no effect. The negative impact of grants is found to be larger when they are given as budget support rather than as assistance to specific projects or general public programs.

Chapter 4 is entitled "Does Foreign Aid increase Foreign Direct Investment?" and is joint work with Eva Rytter Sunesen. We examine the idea that foreign aid and Foreign Direct Investment (FDI) are complementary sources of finance for development (which is conventionally accepted by the international development community). We show that the theoretical relationship between foreign aid and FDI is ambiguous in the context of a small open economy: aid raises the marginal productivity of capital if it is used to finance inputs that are complementary to capital in the aggregate production process (like public infrastructure or human capital investments), but aid crowds out other private investments if it takes the form of pure physical capital transfers. We turn to an empirical analysis and study the relationship between FDI and different types of development aid. Our results show that aid invested in complementary inputs draws in FDI, while aid invested in physical capital crowds it out. The combined effect of these two types of aid is small but on average positive.

Chapter 5 is entitled "Aid and Sectoral Labor Productivity", and is joint work with Rainer Thiele. This chapter examines empirically the proposition that aid to poor countries is detrimental for external competitiveness, giving rise to Dutch disease type effects. At the aggregate level, aid is found to have a positive effect on output growth. A sectoral decomposition shows that the effect is significant and positive both in the tradables and the nontradables sectors, conditional on a number of growth covariates. The paper thus finds no empirical support for the hypothesis that aid reduces external competitiveness in developing countries. We explore possible reasons, and point to the existence of large idle labor capacity and high levels of dollarization in financial liabilities at the firm level.

Sammenfatning (Summary in Danish)

Afhandlingen består af fem kapitler, som hver især er selvstændige studier og kan læses uafhængigt af hinanden.

Kapitel 1, "Lightning, IT Diffusion and Economic Growth across US States" (Lynnedslag, IT Udbredelse og Økonomisk Vækst på Tværs af Amerikanske Delstater), er forfattet i samarbejde med Thomas Barnebeck Andersen, Jeanet Bentzen og Carl-Johan Dalgaard. I dette studie viser vi, at der efter 1990 er en negativ sammenhæng mellem et bestemt aspekt af klimaet, nemlig frekvensen af lynnedslag, og vækst i arbejdskraftens produktivitet på tværs af de 48 sammenhængende amerikanske delstater; før 1990 er der ingen korrelation mellem lynnedslag og vækst. Effekten af andre klima-relaterede variable (så som temperatur, nedbør og tornadoer) følger ikke same mønster. En plausibel forklaring er at lynnedslag påvirker udbredelsen af informations teknologi (IT). Lynnedslag beskadiger digitalt udstyr ved at forårsage udsving i elektrisk spænding. En højere frekvens af lynnedslag medfører derfor højere IT brugeromkostninger. Man kan derfor forestille sig, at lyn-tæthed (antallet af lyn per kvadratkilometer per år) har en negativ effekt på udbredelsen af IT. Vi finder at højere lyn-tæthed faktisk har bremset udbredelsen af IT på tværs af amerikanske delstater, også når vi betinger på standard-kontrol variable. Dette betyder at en øget, makroøkonomisk følsomhed overfor lynnedslag kan skyldes den stigende vigtighed af digitale teknologier for økonomisk vækst.

Kapitel 2, "On the Impact of Digital Technologies on Corruption: Evidence from US States and Across Countries" (Om Effekten af Digitale Teknologier på Korruption: Resultater fra amerikanske delstater og på tværs af lande), er også skrevet sammen med Thomas Barnebeck Andersen, Jeanet Bentzen og Carl-Johan Dalgaard. I dette kapitel analyserer vi, hvorvidt øget brug af internettet har ført til en reduktion i omfanget af korruption. For at undersøge dette spørgsmål udvikler vi en innovativ identifikations-strategi for effekten af internetbrug. Som ovennævnt, ved at medføre skade på elektronisk udstyr øger strømafbrydelser omkostningerne ved brug af IT, hvilket medfører at brugen af computere og internet vokser langsommere. Lynnedslag er et naturligt fænomen, som medfører mange strømafbrydelser hvert år over hele kloden. Ved brug af verdensomspændende satellit data, samt data fra jord-baserede lyn-sensorer, konstruerer vi lyn-tæthedsdata for et stort antal lande og for de sammenhængende amerikanske delstater. Empirisk viser lyn-tæthed sig at være et stærkt instrument for internet udbredelse. Samstemmende med vores OLS estimater viser vores IV estimater at internet-udbredelse faktisk har reduceret omfanget af korruption på tværs af lande og på tværs af amerikanske delstater.

Kapitel 3 hedder "The Impact of Aid on Bureaucratic Quality: Does the Mode of Delivery Matter?" (Effekten af Udviklingsbistand på Kvalitet i Bureaukratiet: Hvilken Betydning har Leveringsmåden?) og er forfattet i samarbejde med Rainer Thiele. I dette kapitel studerer vi effekten af udviklingsbistand på kvalitet i det offentlige bureaukrati i modtagerlandene. Vi tager højde for det forhold, at nogen former for udviklingsbistand har større potentiale for at påvirke bureaukratisk kvalitet end andre. For at kunne tillægge vores resultater en kausal fortolkning, baserer vi vores identifikationsstrategi på effekterne af befolkningsstørrelse og børnedødelighed på allokering af bistand. Vi anvender data for det tværsnit af lande, som modtog udviklingsbistand mellem 1995 og 2005. Vi finder at bistand givet som "gaver" (dvs. som ikke skal tilbagebetales) ser ud til at have beskadiget bureaukratiernes funktionsevne indenfor den analyserede periode, mens bistand givet i form af lån ikke har nogen effekt. Vi finder også at den negative effekt af gave-bistand er større når bistanden gives som budget-støtte end når den gives som generel eller teknisk assistance til bestemte offentlige projekter og programmer.

Kapitel 4, "Does Foreign Aid increase Foreign Direct Investment?" (Fører Udviklingsbistand til Højere, Direkte, Udenlandske Investeringer?) er skrevet sammen med Eva Rytter Sunesen. Vi undersøger den ide, at udviklingsbistand og direkte, udenlandske investeringer (FDI) er komplementære kilder til finansiering af økonomisk udvikling – et forhold som i det internationale udviklingsmiljø generelt antages at gælde. Vi viser at fra et teoretisk synspunkt er forholdet mellem udviklingsbistand og FDI i en lille åben økonomi er tvetydigt. Bistand øger kapitalens marginalprodukt hvis den bruges til at finansiere inputs som er komplementære til kapital i den aggregerede produktionsproces (så som offentlig infrastruktur og investeringer i human kapital), men bistand fortrænger private investeringer hvis den gives som investering i fysisk kapital. Vi vender os dernæst til en empirisk analyse og studerer forholdet mellem FDI og forskellige typer af udviklingsbistand. Vore resultater viser at bistand investeret i komplementære inputs tiltrækker FDI, mens bistand investeret i fysisk kapital fortrænger det. Den første effekt er en smule stærkere end den sidste, således at den samlede effekt er positiv, men temmelig svag.

Kapitel 5, "Aid and Sectoral Labor Productivity" (Ulandsbistand og sektor-specifik produktivitet i arbejdskraften) er samforfattet med Rainer Thiele. Dette kapital undersøger empirisk den påstand, at bistand til fattige lande er skadelig for international konkurrencedygtighed, fordi bistand medfører effekter at typen "Dutch disease". På det aggregerede niveau finder vi, at bistand har en positiv effekt på økonomisk vækst. En sektor-specifik dekomponering viser at denne effekt er positiv og signifikant både i sektoren for goder, som handles internationalt, og i sektoren for goder, som ikke handles, også når vi betinger på en række faktorer, som er korreleret med økonomisk vækst. Dermed finder papiret ikke nogen støtte for den hypotese, at udviklingsbistand reducerer international konkurrencedygtighed i udviklingslande. Vi undersøger mulige årsager til dette, og peger på tilstedeværelsen af store, ledige arbejdskraftressourcer og på det høje niveau af "dollarisering" i firmaernes finansielle passiver.

Empirical Studies on
Growth and Comparative Development

Lightning, IT Diffusion and Economic Growth across U.S. States*

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Abstract

Empirically, a higher frequency of lightning strikes is associated with slower growth in labor productivity across the 48 contiguous US states after 1990; before 1990 there is no correlation between growth and lightning. Other climate variables (e.g., temperature, rainfall, and tornadoes) do not conform to this pattern. A viable explanation is that lightning influences IT diffusion. By causing voltage spikes and dips, a higher frequency of ground strikes leads to damaged digital equipment and thus higher IT user costs. Accordingly, the flash density (strikes per square km per year) should adversely affect the speed of IT diffusion. We find that lightning indeed seems to have slowed IT diffusion, conditional on standard controls. Hence, an increasing macroeconomic sensitivity to lightning may be due to the increasing importance of digital technologies for the growth process.

Keywords: Climate, IT diffusion, economic growth.
JELClassification: O33, O51, Q54.

1 Introduction

There is compelling evidence to suggest that climate and geography profoundly affected the historical growth record (Diamond, 1997; Olsson and Hibbs, 2005; Putterman, 2008; Asraf and Galor, 2008). Today, climate shocks, like temperature changes, still affect growth in poor countries (Dell et al., 2008). But are climate and geography also important in highly developed economies, where high-tech industry and services are dominant activities?

Some research suggests that geography is still a force to be reckoned with, even in rich places. Access to waterways, for instance, appears to matter (Rappaport and

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Sachs, 2003). However, a geographic characteristic that exhibits a time-invariant impact on prosperity is difficult to disentangle from other slow moving growth determinants that may have evolved under the influence of climate or geography. In particular, climate and geography quite possibly influenced the evolution of economic and political institutions.[1]

The present paper documents that a particular climate related characteristic – lightning activity – exhibits a time-varying impact on growth in the world's leading economy. Studying the growth process across the 48 contiguous US states from 1977 to 2007, we find no impact from lightning on growth prior to about 1990. However, during the post 1990 period there is a strong negative association: states where lightning occurs at higher frequencies have grown relatively more slowly. What can account for an increasing macroeconomic sensitivity to lightning?

In addressing this question one may begin by noting that the 1990s was a period of comparatively rapid US growth; it is the period where the productivity slowdown appears to finally have come to an end. Furthermore, the 1990s is the period during which IT appears to have diffused throughout the US economy at a particularly rapid pace. In fact, IT investment is often seen as a key explanation for the US growth revival (e.g., Jorgenson, 2001). On a state-by-state basis, however, the process of IT diffusion, measured by per capita computers and Internet users, did not proceed at a uniform speed.

An important factor that impinges on IT investment and diffusion is the quality of the power supply. That a high quality power supply is paramount for the digital economy is by now well recognized; as observed in The Economist (2001): "For the average computer or network, the only thing worse than the electricity going out completely is power going out for a second. Every year, millions of dollars are lost to seemingly insignificant power faults that cause assembly lines to freeze, computers to crash and networks to collapse. [...] For more than a century, the reliability of the electricity grid has rested at 99.9% [...] But microprocessor-based controls and computer networks demand at least 99.9999% reliability [...] amounting to only seconds of allowable outages a year." Indeed, a sufficiently large power spike lasting only one millisecond is enough to damage solid state electronics such as microprocessors in computers. Therefore, as a simple matter of physics, an irregularly fluctuating power supply reduces the longevity of IT equipment, and thus increases the user cost of IT capital.

A natural phenomenon that causes irregular voltage fluctuations is lightning activity. Albeit the impulse is of short duration, its size is impressive. Even in the presence of lightning arresters on the power line, peak voltage emanating from a lightning strike can go as high as 5600 V, which far exceeds the threshold for power disruptions

[1]An apparent impact from "diseases" on comparative development may be convoluting the impact from early property rights institutions in former colonies (Acemoglu et al., 2001); the impact of access to waterways, as detected in cross-country data, may also be related to the formation of institutions (Acemoglu et al., 2005).

beyond which connected IT equipment starts being damaged (e.g., Emanuel and McNeil, 1997). Moreover, the influence from lightning is quantitatively important: to this day lightning activity causes around one third of the total number of annual power disruptions in the US (Chisholm and Cummings, 2006). It is therefore very plausible that lightning may importantly have increased IT user costs.[2] Naturally, in places with higher IT user cost one would expect a slower speed of IT diffusion; lightning prone regions may be facing a climate related obstacle to rapid IT diffusion.

Even though a link between lightning and IT diffusion is theoretically plausible, it does not follow that the link is economically important. Nor is it obvious that IT can account for the lightning-growth correlation.

We therefore also study the empirical link between lightning and the spread of computers and Internet across the US. We find that the diffusion of computers and the Internet has progressed at a considerably slower pace in areas characterized by a high frequency of lightning strikes. This link is robust to the inclusion of standard controls for computer diffusion (Caselli and Coleman, 2001). Moreover, lightning ceases to be correlated with growth post 1990, once controls for IT are introduced. While the lightning-IT-growth hypothesis thus seems well founded, other explanations cannot be ruled out a priori.

An alternative explanation is that the correlation between growth and lightning picks up growth effects from global warming. If global warming has caused lightning to increase over time, and simultaneously worked to reduced productivity growth, this could account for the (reduced form) correlation between lightning and growth. We document that this is unlikely to be the explanation for two reasons. First, we show that from 1906 onwards US aggregate lightning is stationary; on a state-by-state basis, we find the same for all save two states. There is thus little evidence to suggest that lightning density is influenced by a global warming induced trend. Second, we attempt to deal with the potential omitted variables problem by controlling directly for climate shocks which also could be induced by climate change. We examine an extensive list of climate variables, including rainfall, temperature, and frequency of tornadoes. None of these variables impacts on the correlation between lightning and state-level growth rates. Nor does any other climate variable exhibit the kind of time-varying impact on growth that we uncover for lightning.

Another potential explanation is that the lightning-growth correlation is picking up "deep determinants" of prosperity that exhibit systematic variation across climate zones, just as lightning does. For instance, settler mortality rates, the extent of slavery and so forth. However, the correlation between lightning and growth is left unaffected

[2]Naturally, the "power problem" may be (partly) addressed, but only at a cost. The acquisition of surge protectors, battery back-up emergency power supply (so-called uninterruptable power supply, UIP) and the adoption of a wireless Internet connection will also increase IT user costs through the price of investment. Hence, whether the equipment is left unprotected or not, more lightning prone areas should face higher IT user cost.

by their inclusion in the growth regression.

In sum, we believe the most likely explanation for the lightning-growth correlation is to be found in the diffusion mechanism. The analysis therefore provides an example of how technological change makes economies increasingly sensitive to certain climate related circumstances. This finding is consistent with the *temperate drift hypothesis* (Acemoglu et al., 2002), which holds that certain climate related variables may influence growth in some states of technology, and not (or in the opposite direction) in others.

The paper is related to the literature that studies technology diffusion; particularly diffusion of computers and the Internet (e.g., Caselli and Coleman, 2001; Beaudry et al., 2006; Chinn and Fairlie, 2007). In line with previous studies, we confirm the importance of human capital for the speed of IT diffusion. However, the key novel finding is that climate related circumstances matter as well: lightning influences IT diffusion. In this sense the paper complements the thesis of Diamond (1997), who argues for an impact of climate on technology diffusion. Yet whereas Diamond argues that climate is important in the context of agricultural technologies, the present paper makes plausible that climate also matters to technology diffusion in high-tech societies.

The analysis proceeds as follows. In the next section we document the lightning-growth link. Then, in Section 3, we discuss likely explanations (IT diffusion, other forms of climatic influence, institutions and integration) for the fact that lightning correlates with growth from about 1990 onwards. Section 4 concludes.

2 Lightning and US growth 1977-2007

This section falls in two subsections. In Section 2.1 we present the data on lightning and discuss its time series properties. In particular, we demonstrate that lightning is stationary; and that, for panel data purposes, lightning is best thought of as a state fixed effect. Next, in Section 2.2, we study the partial correlation between lightning and growth across the US states.

2.1 The Lightning Data

The measure of lightning activity that we employ is the flash density, which captures the number of ground flashes per square km per year. We have obtained information about the flash density from two sources. The first source of information is reports from weather stations around the US. From this source we have yearly observations covering the period 1906-1995 and 40 US states. From about 1950 onwards we have data for 42 states. The second source of information derives from ground censors around the US. This data is a priori much more reliable than the data from weather stations.[3] In

[3] Lightning events recorded at weather stations are based on audibility of thunder (i.e., these are basically recordings of thunder days), whereas ground sensors measure the electromagnetic pulse that emanates from lightning strikes (i.e., these are recordings of actual ground strikes). In the context of IT

addition, it is available for all 48 contiguous states, but it only comes as an average for the period 1996-2005.[4]

In order to understand the data better, we begin by studying its time series properties. Figure 1 shows the time path for aggregate US lightning over the period 1906-95.

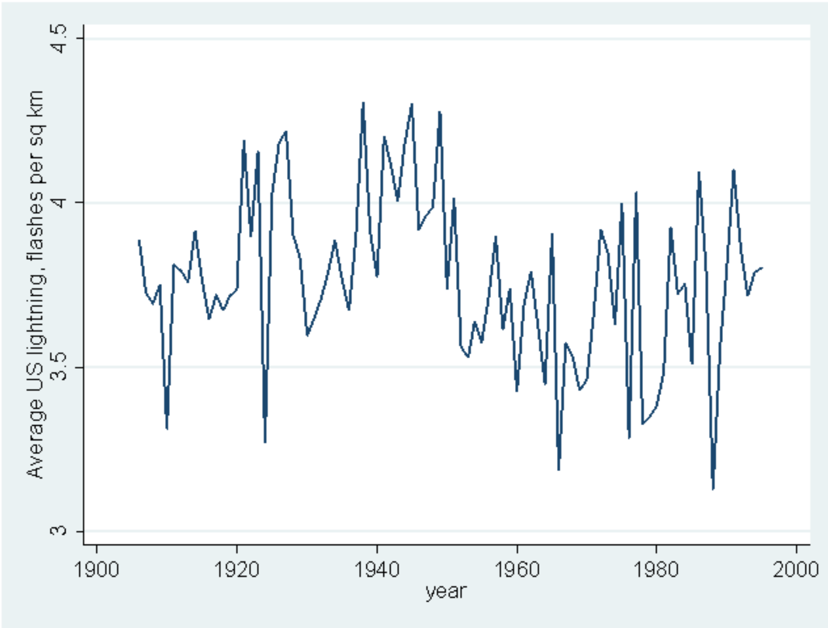

Figure 1: **The average flash density in the US: 40 states**
Source: Lightning observations from weather stations, transformed from thunder days (TD) into flash density (FD) using the formula $FD = 0.04 \times TD^{1.25}$. See Data Appendix for details.
Notes: Only 40 states have complete information for the period 1906-1995. The "left-out" (contiguous) states are Connecticut, Delaware, New Hampshire, New Jersey, Rhode Island, Vermont, Mississippi, and West Virginia. The figure shows the weighted average, where the weight is determined by state size.

The aggregate flash density is calculated as the state-size weighted average over the 40 states with data for this extended period. Visual inspection suggests that there is no clear trend. More formally, to test whether lightning contains a stochastic trend, we use an augmented Dickey-Fuller (DF) test with no deterministic trend. Lag length is selected by minimizing the Schwarz information criterion with a maximum of five lags. For aggregate US lightning the optimal lag length is one and the DF statistic equals -4.516. Hence the presence of a unit root is resoundingly rejected.

At the state level the presence of a unit root is also rejected at the 5% level in 38 of the 40 states, cf. Table 1. In light of the fact that DF tests have low power to reject the null of a unit root, we are in all likelihood safe to conclude that state-level lightning is

diffusion it is ground strikes that matter, and not the type of lightning occurring between clouds, say.

[4]Further details are given in the Data Appendix.

Table 1. Dickey-Fuller tests for unit root in lightning

	test-statistic	p-value	No. obs.	No. lags
Aggregate US	-4.52	0.0000	88	1
Alabama	-5.31	0.0000	88	1
Arizona	-3.38	0.0118	87	2
Arkansas	-8.98	0.0000	89	0
California	-8.40	0.0000	89	0
Colorado	-8.69	0.0000	89	0
Florida	-8.19	0.0000	89	0
Georgia	-8.58	0.0000	89	0
Idaho	-3.48	0.0085	87	2
Illinois	-9.61	0.0000	89	0
Indiana	-8.24	0.0000	89	0
Iowa	-9.42	0.0000	89	0
Kansas	-4.46	0.0002	88	1
Kentucky	-2.94	0.0412	87	2
Louisiana	-4.62	0.0001	88	1
Maine	-2.75	0.0662	87	2
Maryland	-5.32	0.0000	88	1
Massachusetts	-9.25	0.0000	89	0
Michigan	-8.76	0.0000	89	0
Minnesota	-10.28	0.0000	89	0
Missouri	-9.92	0.0000	89	0
Montana	-9.01	0.0000	89	0
Nebraska	-3.64	0.0051	87	2
Nevada	-10.02	0.0000	89	0
New Mexico	-3.58	0.0062	87	2
New York	-4.01	0.0013	88	1
North Carolina	-5.40	0.0000	88	1
North Dakota	-7.84	0.0000	89	0
Ohio	-3.59	0.0059	87	2
Oklahoma	-11.61	0.0000	89	0
Oregon	-7.09	0.0000	89	0
Pennsylvania	-2.20	0.2045	86	3
South Carolina	-8.01	0.0000	89	0
South Dakota	-8.62	0.0000	89	0
Tennessee	-7.32	0.0000	89	0
Texas	-5.45	0.0000	88	1
Utah	-5.55	0.0000	88	1
Virginia	-7.41	0.0000	89	0
Washington	-8.75	0.0000	89	0
Wisconsin	-9.45	0.0000	89	0
Wyoming	-7.71	0.0000	89	0

Notes. The Augmented Dickey-Fuller test with no deterministic trend for each of the 40 states over the period 1906-1995. Lags selected by Schwarz's information criteria. Lightning is average number of flashes per year per square km, measured at weather stations.

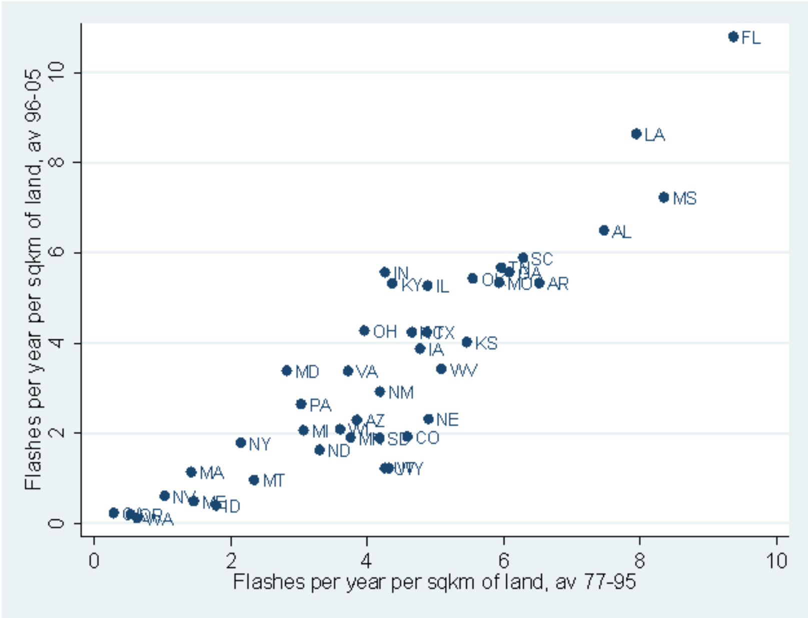

Figure 2: **The average flash density 1977-95 versus 1996-2005: 42 states.**
Sources: 1977-95 based on Thunder days (TD) from weather station observations, converted into flash density (FD) using the formula $FD = 0.04 \times TD^{1.25}$. 1996-2005 data are based on ground detectors. See Appendix for further details. Notes: The correlation is 0.90, and a regression, $FL_{96-05} = a + bFL_{77-95}$ returns: $a = -0.99$, $b = 1.05$, $R^2 = 0.81$.

also stationary.

These findings are of some independent interest in that they suggest that global warming has not interfered with the evolution of lightning trajectories in the US in recent times. In other words, there is little basis for believing that the flash density has exhibited a trend during the last century.

In the analysis below we focus on the period from 1977 onwards, dictated by the availability of data on gross state product. Consequently, it is worth examining the time series properties of the lightning variable during these last few decades of the 20th century.

During this period the flash density is for all practical purposes a fixed effect. In the Appendix (Table A1) we show state-by-state that the residuals obtained from regressing lightning on a constant are serially uncorrelated. That is, deviations of the flash density from time averages are, from a statistical perspective, white noise. To show this formally, we use the Breusch-Godfrey test and a Runs test for serial correlation. By the standards of the Breusch-Godfrey test, we cannot reject the null hypothesis of no serial correlation in 38 states out of 42 states; using the Runs test, we fail to reject the null in 40 states. Importantly, no state obtains a p-value below 0.05 in both tests. This suggests that for the 1977-95 period lightning is best described as a state fixed effect.

(Table A1 in the appendix shows the results of these tests).

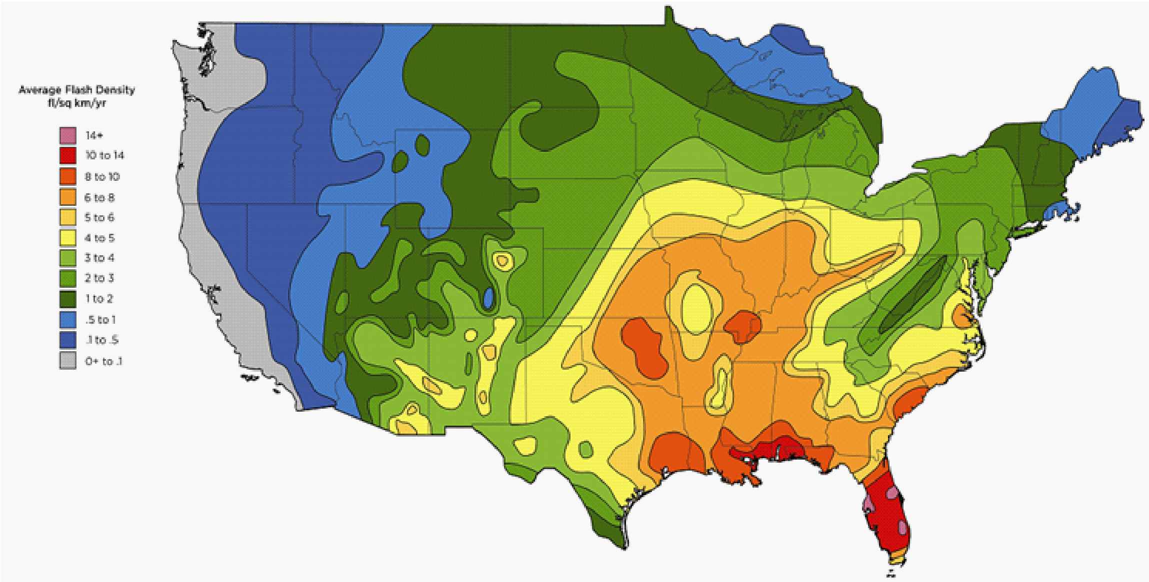

Figure 3: **The distribution of flash densities across the US: 1997-2007.**
Source: Vaisala Group, www.vaisala.com

As remarked above, we have an alternative source of data available to us, which contains information for the 1996-2005 period. How much of a concurrence is there between data for the 1977-95 period and the data covering the end of the 1990s and early years of the 21st century? Figure 2 provides an answer. Eyeballing the figure reveals that the two measures are very similar. In fact, we cannot reject the null that the slope of the line is equal to one. This further corroborates that lightning is a state fixed effect.

These findings have induced us to rely on the data deriving from ground censors in the analysis below. As noted above, this latter lightning data is of a higher quality compared to the measure based on weather stations and it covers more US states. Moreover, since deviations from the average flash density are white noise, we lose no substantive information by resorting to a time invariant measure. Still, it should be stressed that using instead the historical lightning measure based on weather stations (or combining the data) produces the same qualitative results as those reported below. These results are available upon request.

The cross-state distribution of the 1997-2007 data is shown in Figure 3. Summary statistics for the period for which we have data (1996-2005) are provided in Table A2 in the appendix.

There is considerable variation in the flash density across states. At the lower end we find states like Washington, Oregon, and California with less than one strike per square km per year. It is interesting to note that the two states who are world famous for IT, Washington and California, are among the least lightning prone. At the other end of the spectrum we find Florida, Louisiana, and Mississippi with seven strikes or more. It is clear that lightning varies systematically across climate zones. Hence, it is important to check, as we do below, that lightning's correlation with growth is not due to other climate variables like high winds, rainfall and so on.

2.2 The Emergence of a Lightning-Growth Nexus

Figures 4 and 5 show the partial correlation between growth in labor productivity and the flash density, controlling only for initial labor productivity.

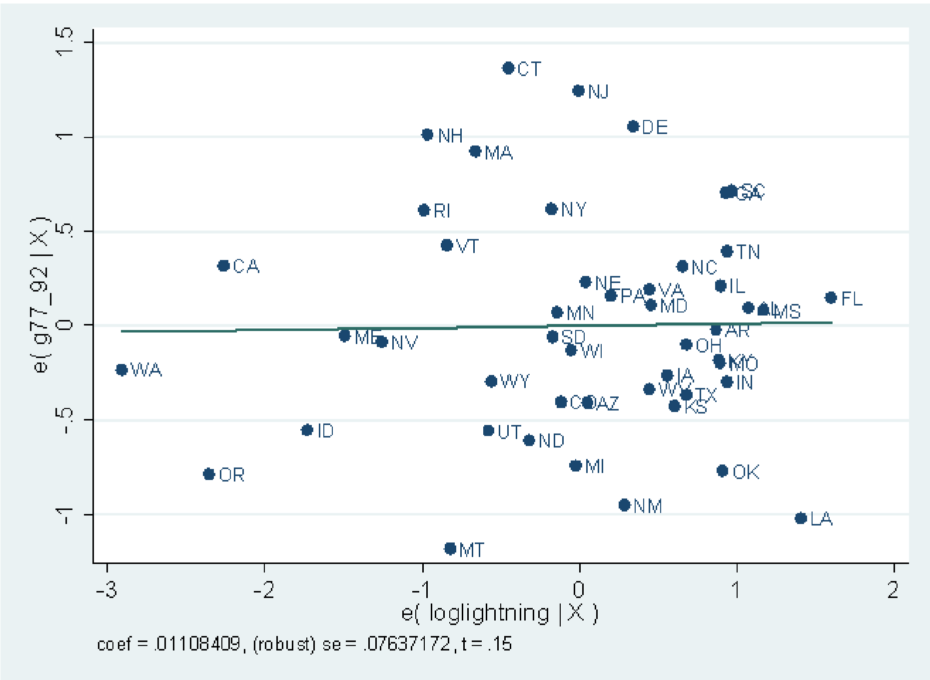

Figure 4: **The correlation between state growth and (log) flash density, conditional on a constant and initial income per worker: 1977-1992.**

We have data on gross state product (GSP) per worker for the period 1977-2007.[5] Hence, for this first exercise we have simply partitioned the data into two equal sized 15 year epochs. As seen from the two figures, there is a marked difference in the partial correlation depending on which sub-period we consider. During the 1977-92 period there is no association between growth and lightning; the (OLS) point estimate is essentially nil. However, in the second sub-period the coefficient for lightning rises twenty

[5]State level data on personal income is also available, and for a longer period. But personal income does not directly speak to productivity. By contrast, GSP per worker is a direct measure of state level labor productivity. Moreover, the GSP per worker series is available in constant chained dollar values, which is an important advantage in the context of dynamic analysis. See the Data Appendix for a description of the GSP per worker series.

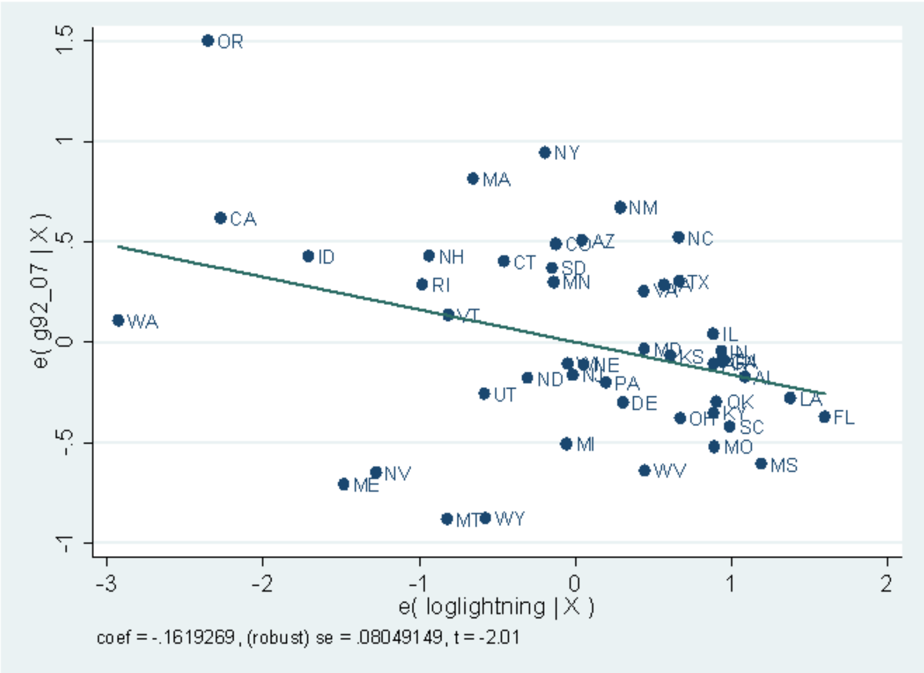

Figure 5: **The correlation between state growth and (log) flash density, conditional on a constant and initial income per worker: 1992-2007.**

fold (in absolute value) and turns statistically significant; places with higher flash density have tended to grow at a slower rate during the 1990s and the first decade of the 21st century.

While this exercise is revealing, there is no particular reason to believe that the lightning-growth correlation emerged precisely in 1992. Hence, to examine the issue in more detail, we study the same partial correlation by running "rolling" regressions over 10 year epochs, starting with 1977-87.[6] That is, we estimate an equation of the following kind:

$$\log\left(\frac{y_{it}}{y_{it-10}}\right) = b_0 + b_1 \log(y_{it-10}) + b_2 \log(lightning_i) + \varepsilon_{it}$$

and examine the evolution of b_2 as t increases. Figure 6 shows the time path for b_2 as well as the associated 95% confidence interval.

In the beginning of the period there is not much of a link between lightning and growth; if anything the partial correlation is positive. As one moves closer to the 1990s the partial correlation starts to turn negative and grows in size (in absolute value). By 1995 the lightning-growth correlation is statistically significant at the 5% level of confidence. As one moves forward in time the partial correlation remains stable and

[6]The exact choice of time horizon does not matter much; below we run regressions with 5, 10, and 15 year epochs that complement the present exercise.

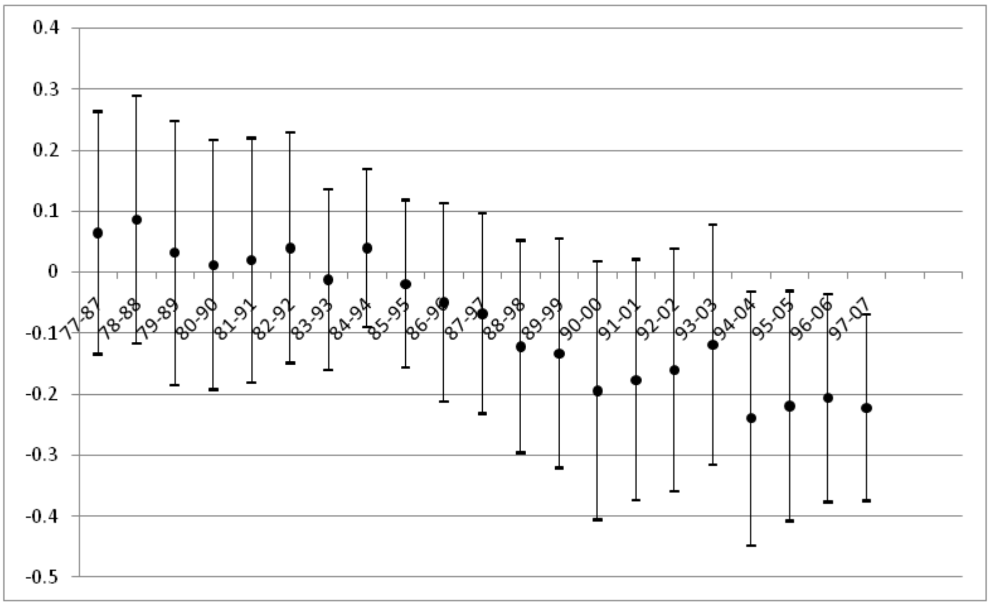

Figure 6: **The lightning-growth nexus: 1977-2007.**
Notes: The figure shows estimates for b_2 (and the associated 95 percent confidence interval) from regressions of the form: $log(y_t) - log(y_{t-10}) = b_0 + b_1 log(y_{t-10}) + b_2 log(lightning) + e$, where y is gross state product per worker and $t = 1987, \ldots, 2007$. 48 states; estimated by OLS.

significant. Hence, this exercise points to the same conclusion as that suggested by Figures 4 and 5: the negative partial correlation between lightning and growth emerged in the 1990s.

Albeit illustrative, both exercises conducted so far are ad hoc in the sense that they do not allow for a formal test of whether the impact from lightning is rising over time. Hence, as a final check, we run panel regressions with period length of 5, 10, and 15 years. The results are reported in Table 3 below.

Since lightning, for all practical purposes, is a fixed effect (cf. Section 2.1), Table 3 reports the results from running pooled OLS regressions. Specifically, we estimate the following equation:

$$\log\left(\frac{y_{it}}{y_{it-T}}\right) = b_0 + b_1 \log(y_{it-T}) + b_{2t} \log(lightning_i) + \mu_t + \varepsilon_{it}$$

where $T = 5, 10, 15$ and b_{2t} accordingly is allowed to vary from period-to-period by way of interaction with time dummies. This way we can track the statistical and economic significance of lightning over time. Note also that we include time dummies independently of lightning, so as to capture a possible secular trend in growth over the period in question.

Turning to the results we find that the impact of lightning increases over time, and

Table 3. Lightning and growth

(1)	5 year periods	**1977-1982**	**1982-1987**	**1987-1992**	**1992-1997**	**1997-2002**	**2002-2007**	Observations	R-squared
		-0.04	0.17	-0.09	-0.04	-0.28**	-0.18*	288	0.20
		[0.10]	[0.16]	[0.09]	[0.12]	[0.11]	[0.09]		
(2)	10 year periods	**1977-1987**		**1987-1997**		**1997-2007**		Observations	R-squared
		0.07		-0.07		-0.22***		144	0.15
		[0.10]		[0.08]		[0.08]			
(3)	15 year periods	**1977-1992**			**1992-2007**			Observations	R-squared
		0.01			-0.16**			96	0.20
		[0.08]			[0.08]				

Notes. Pooled OLS estimates of the coefficient on lightning (b_{2t}). The dependent variable is the yearly growth rate of GSP per worker over periods of 5, 10, and 15 years, respectively. All regressions include a constant, the initial level of (log) real GSP per worker and a full set of time-dummies. Lightning is the (log) average number of flashes per year per square km, measured by flash-detectors. Robust standard errors in brackets, adjusted for clustering at state level. Asterisks ***, **, and * indicate significance at the 1, 5, and 10%, respectively.

turns statistically significant during the 1990s.[7] The significance of lightning is particularly noteworthy as it is obtained for the relatively homogenous sample of US states. As is well known, the growth process for this sample is usually fairly well described by the initial level of income alone, suggesting only modest variation in structural characteristics that impinge upon long-run labor productivity (e.g., Barro and Sala-i-Martin, 1992). As a result, the scope for omitted variable bias contaminating the OLS estimate for lightning is a priori much more limited than, say, in a cross-country setting. However, in the next section we do find that one particular growth determinant renders lightning insignificant: IT penetration.

The impact from lightning is economically significant as well. Consider the results pertaining to the "intermediate case", which involves 10 year epochs. Taken at face value, the point estimate for the 1990s imply that a one standard deviation increase in lightning intensity (about 2.4 flashes per year per sq km) induces a reduction in growth by about 0.2 percentage points , conditional on the level of initial labor productivity and the time effects. This is about 12.5 % of the gap between the 5th percentile and the 95th percentile in the distribution of GSP per worker growth rates for the period 1977-2007 (for the 48 states in our sample). By extension, variation in lightning by four standard deviations (roughly equivalent to moving from the 5th percentile to the 95th percentile in the lightning distribution across US states) can account for about 50% of the "95/5" growth gap.[8] Needless to say, this is a substantial effect.

These results uniformly support the same qualitative conclusion: a macro economic sensitivity to lightning has emerged over time in the US. The question is why?

3 Hypotheses and Explanations

3.1 IT Diffusion

We begin this section by examining the theoretical foundation behind the claim that lightning (or, more appropriately, the flash density) should have an impact on growth via IT diffusion. Subsequently we examine the hypothesis empirically.

Theory: The simple analytics of why lightning matters to IT diffusion.

The simplest way to think about IT diffusion is via basic neoclassical investment theory. That is, IT diffusion occurs in the context of IT capital investments. In what follows we develop a simple model that links the flash density (our independent variable in the

[7] The general time dummies (not reported) corroborate the prior of a revitalization of productivity growth during the 1990s.

[8] Log normality of lightning is not accurate; but on the other hand not terribly misleading either. It does exaggerate the actual variation in lightning slightly; the observed variation is about 7 flashes, compared to the "back-of-the-envelope" calculation implying roughly 9.

regressions above) to IT capital accumulation, and thus IT diffusion and growth in output.

Consider a representative firm producing output, Y, with the technology $F(C)$. C is the stock of IT capital, whereas $F(\cdot)$ is a neoclassical production function, featuring positive and diminishing returns; for simplicity we ignore other inputs in production. The price of output is normalized to one and markets are competitive.

We assume the capital stock cannot be adjusted to its optimal level instantaneously. A reason would be the presence of (convex) installation costs. For simplicity, we ignore adjustment costs in the formal analysis, and assume instead that the IT capital stock simply follows an *ad hoc* adjustment rule capturing whatever frictions that prevent firms from adjusting the capital stock fully.[9]

Specifically, assuming time is continuous, the adjustment rule is $\dot{C} = \lambda(C^* - C)$, where $\dot{C} = dC/dt$ is the instantaneous change in the capital stock, λ is a positive parameter, C^* is the optimal IT capital stock (to be determined below), and C is the current (or initial) stock of IT capital. Hence, in each period the capital stock is mechanically adjusted towards its optimal level.

In the absence of convex adjustment costs, the optimal IT capital stock, C^*, is given by the first order condition from the static profit maximization problem:

$$F'(\mathrm{C}) = u$$

where u is the user cost of capital. Ignoring taxes, the user cost formula is (Hall and Jorgenson, 1967):

$$u = p(r + \delta - \pi),$$

where p is the relative investment price, r is the real rate of return, δ is the depreciation rate of IT capital, and π is the instantaneous rate of change in the relative investment price.

Next, we assume the depreciation rate is increasing in the number of lightning strikes, n, in the surrounding area of the power conductor. That is,

$$\delta = \delta(n),\ \delta'(n) > 0.$$

The basic idea is that lightning strikes lead to power disturbances, which reduce the longevity of IT capital. This assumption has a sound physical foundation. Solid-state electronics, such as computer chips, are constructed to deal with commercial power supply in the form of alternating current. The voltage of the current follows a sine wave with a specific frequency and amplitude. If the sine wave changes frequency or amplitude, this constitutes a power disruption. Digital devices convert alternating cur-

[9]Nothing much is lost by this simplification. The key result obtained below, that the flash density reduces growth, can also be derived invoking convex costs of adjustment, at the costs of more algebra. In the interest of brevity, however, we stick with the simpler model.

rent to direct current with a much reduced voltage; digital processing of information basically works by having transistors turn this voltage on and off at several gigahertz (Kressel, 2007). If the power supply is disrupted, the conversion process may be corrupted which causes damage to the equipment, reducing its longevity.

It is important to appreciate that even extremely short lasting power disruptions are potentially problematic. Voltage disturbances measuring less than one cycle (i.e., 1/60th of a second in the US case) are sufficient to crash and/or destroy servers, computers, and other microprocessor-based devices (Yeager and Stalhkopf, 2000; Electricity Power Research Institute, 2003). A natural phenomenon which damages digital equipment, by producing power disruptions, is lightning activity (e.g., Emanuel and McNeil, 1997; Shim et al., 2000, Ch. 2; Chisholm, 2000). In reduced form then, more lightning strikes to the power supply implies higher IT capital depreciation.[10] We capture the physical links between lightning strikes and equipment damage by assuming $\delta'(n) > 0$.

Finally, the number of strikes, n, per year (per 100 km line length) can be determined as (Chisholm, 2000)

$$n = 3.8 \cdot f \cdot h^{0.45},$$

where f is the flash density and h is the height (in meters) of the conductor above ground. This completes the model.

To see how the flash density impacts on IT diffusion, substitute n into the user cost expression, and invoke the first order condition from profit maximization. Then the optimal IT capital stock, C^*, is given by

$$C^* = \Phi\left(p\left[r + \delta\left\{3.8 \cdot f \cdot h^{0.45}\right\} - \pi\right]\right)$$

where $\Phi = F'^{-1}$. As a consequence, using the adjustment rule, the growth rate of the IT capital stock becomes

$$\frac{\dot{C}}{C} = \lambda \frac{\Phi\left(p\left[r + \delta\left\{3.8 \cdot f \cdot h^{0.45}\right\} - \pi\right]\right)}{C} - \lambda.$$

This expression forms the basis for the following observation:

[10]Note that lightning may enter a firm or household in four principal ways. First, lightning can strike the network of power, phone, and cable television wiring. This network, particularly when elevated, acts as an effective collector of lightning surges. The wiring conducts the surges directly into the residence, and then to the connected equipment. In fact, the initial lightning impulse is so strong that equipment connected to cables up to 2 km away from the site of the strike can be damaged (BSI, 2004). Technically speaking, this is the mechanism we are capturing in the simple model above. Second, when lightning strikes directly to or nearby air conditioners, satellite dishes, exterior lights, etc., the wiring of these devices can carry surges into the residence. Third, lightning may strike nearby objects such as trees, flagpoles, road signs, etc., which are not directly connected to the residence. When this happens, the lightning strike radiates a strong electromagnetic field, which can be picked up by the wiring in the building, producing large voltages that can damage equipment. Finally, lightning can strike directly into the structure of the building. This latter type of strike is extremely rare, even in areas with a high lightning density.

Proposition 1 *Conditional on the initial capital stock, a higher flash density leads to a lower growth rate of the IT capital stock.*

Proof. Since $\Phi' < 0$ and $\lambda > 0$ the result follows immediately from differentiation. QED ■

Hence, in areas with a greater flash density, the speed of IT diffusion - as measured by IT capital accumulation - will proceed at a slower pace. The intuition is that a higher flash density rate increases the frequency of power disturbances, IT capital depreciation, the user cost of IT capital, and thus lowers IT investments. Moreover, as output is increasing in the IT capital stock, $Y = F(C)$, growth in output will similarly tend to be slower in areas with greater lightning activity, conditional on the initial level of output.[11]

It is worth reiterating that firms may take pre-emptive actions so as to reduce the impact of lightning on the cost of capital; this could be done by investing in surge protectors, say. However, the crux of the matter is that this imposes an additional cost to be carried in the context of IT investments; in terms of the model above, it amounts to an increasing investment price, p. Hence, even if we take the likely "pre-emptive measures" into account, more lightning prone areas will tend to feature slower growth in IT capital, and thus slower output growth.

While the above theoretical considerations speak to a direct impact of lightning on IT investment, there could be an important complementary mechanism at work. The choice of firm location may depend on the quality of power supply, and thus lightning. Specifically, it may be the case that IT intensive firms choose to locate in areas where lightning intensity is modest, due to the resulting (slightly) higher power quality. Interestingly, The National Energy Technology Laboratory operated by the US Department of Energy reports that a recent firm level survey had 34% respondents saying that they would shift business operations out of their state if they experienced ten or more unanticipated power disturbances over a quarter of a year.[12] Hence, it seems plausible that this mechanism also could affect comparative IT penetration across US States.

These mechanisms, linking lightning to growth, are likely to have become increasingly important over time for a number of reasons. First, IT capital investments accounted for a substantial part of output growth, starting in the 1990s (e.g., Jorgenson, 2001). Consequently, factors that impact on IT capital accumulation (e.g., the flash density) should also become more important to growth. Second, the 1990s was the era during which the Internet emerged (in the sense of the World Wide Web); a conceivable reason why firms chose to intensify IT investments during the same period.[13] From a

[11]It should be clear that the advocated mechanism is robust in a general equilibrium setting. Through elevated capital depreciation, higher lightning density would work to reduce the long-run (steady state) level of capital per worker in any neoclassical growth model. Hence, conditional on the initial capital stock, growth will be reduced in transition by an increasing flash density.

[12]http://www.netl.doe.gov/moderngrid/

[13]The WWW was launched in 1991 by CERN (the European Organisation for Nuclear Research). See

physical perspective, however, the network connection is another way in which lightning strikes may reach the computer, in the absence of wireless networks (which have not been widespread until very recently). Third, the 1990s saw rapid increases in the computing power of IT equipment. In keeping with Moore's law, processing speed doubled roughly every other year. This is an important propagation mechanism of the lightning-IT investment link. The reason is that the sensitivity of computers to small power distortions increases with the miniaturization of transistors, which is the key to increasing speed in microprocessors (Kressel, 2007).

In sum, these factors would all contribute to increasing the importance of the flash density to IT investments, and thus to growth, during the 1990s. But the question is whether empirically this theory can account for the apparent increasing macroeconomic sensitivity to lightning.

Empirical analysis: Lightning, IT diffusion and Economic Growth.

In order for the above theory to be able to account for the lightning-growth correlation, two things need be true. First, it must be the case that lightning is a strong predictor of IT across the US states. Second, there should be no explanatory power left in lightning vis-à-vis growth, once we control for IT. We examine these two requirements in turn.

In measuring the diffusion of IT capital across the US we employ two measures. Both measures derive from a supplement to the 2003 Current Population Survey, which contained questions about computer and Internet use. The first measure is percentage of households with access to Internet, and the second measure is percentage of households with a PC. A couple of remarks on these data are necessary.

First, we only have one observation for both IT variables. Consequently, we have to settle for cross section regressions. Second, one may question whether there is value in using both variables, since having access to a computer is a prerequisite for the use of the Internet. Yet, the emergence of the WWW is a much more recent technology than the PC, as the former derives from 1991. The personal computer started spreading earlier. Hence, the initial conditions that may matter to the speed of adoption are discernible by time. For instance, whereas educational attainment in the 1970s should influence the spread of the personal computer, the Internet is affected by education levels in the 1990s. Hence, the two empirical models of IT diffusion will have to differ in terms of the "dating" of the right hand side IT diffusion determinants. As a result, we employ both.

A natural point of departure is with the simple correlation between the flash density and the two IT measures for the 48 states in our sample. Figure 7 and 8 depict them.

Visually, the strong negative correlations between the flash density and PC and Internet users, respectively, are immediately obvious. By 2003, states that experience

Hobbes' Internet Timeline v8.2 http://www.zakon.org/robert/internet/timeline/

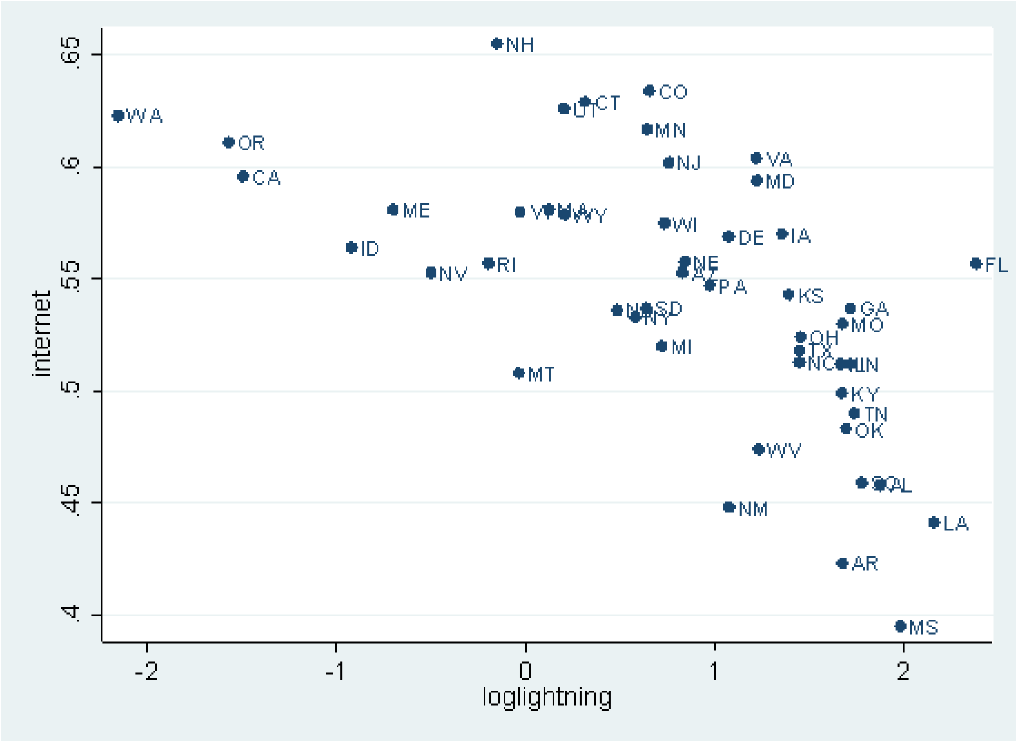

Figure 7: **Lightning versus Internet users per 100 households in 2003.**
Sources: See Data Appendix Notes: The raw correlation between the two series is -0.62.

lightning strikes at a higher frequency also have relatively fewer users of computers and the Internet.

A more systematic approach involves more controls of course. Human capital is probably the first additional determinant of diffusion that comes to mind. The idea that a more educated labor force is able to adopt new technologies more rapidly is an old one, going back at least to the work of Nelson and Phelps (1966). Another natural control is the level of GSP per worker. Aside from being a catch-all control for factors that facilitate diffusion, it can also be motivated as a measure of the "distance to the frontier". A priori the sign of the coefficient assigned to GSP per worker is therefore ambiguous. A positive sign is expected if initially richer areas are able to acquire IT equipment more readily. A negative sign could arise if richer areas, by closer proximity to the technology frontier, are less able to capitalize on "advantages of backwardness".

In addition to labor productivity and human capital, we follow Caselli and Coleman (2001) in choosing relevant additional determinants of IT diffusion (they also include human capital and income per capita). First, we use measures for the composition of production; it seems plausible that IT may spread more rapidly in areas featuring manufacturing rather than agriculture. Second, we employ proxies for global links, measured by international movements of goods and capital, and a measure of

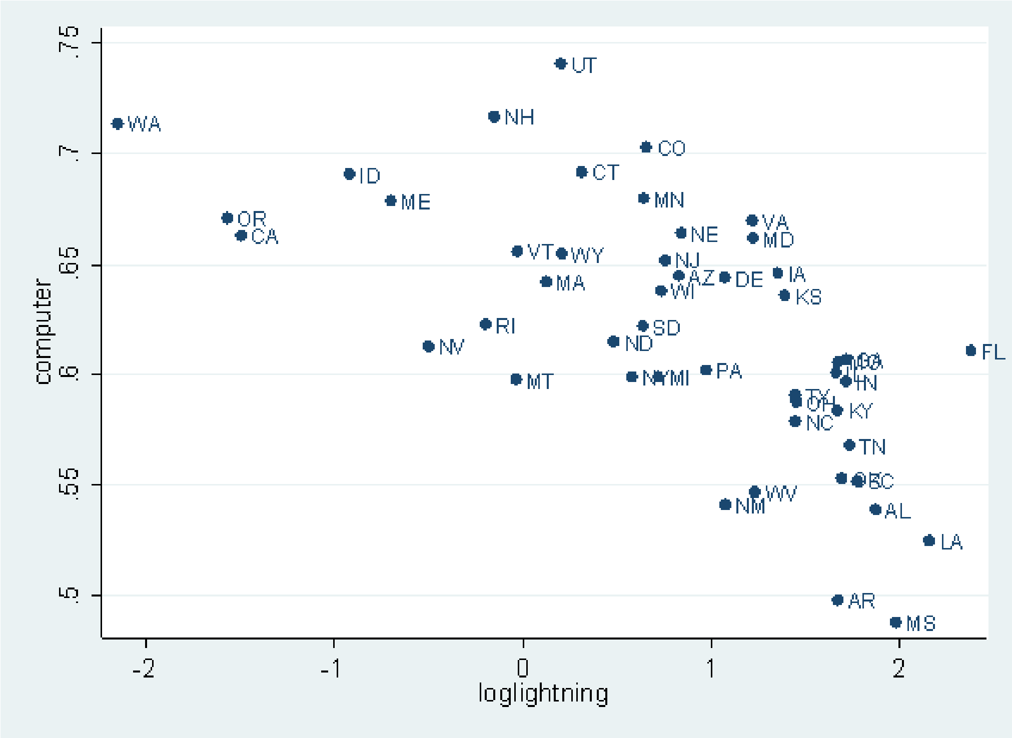

Figure 8: **Lightning versus personal computers per 100 households in 2003.**
Sources: See Data Appendix.

local market size: state population. Third, we employ various historical variables as controls. Caselli and Coleman, studying cross-country data, include a measure of economic institutions, which we are not able to do directly in our US sample. However, by including various plausible historical determinants of productivity (e.g., soldier mortality, the pervasiveness of slavery in the late 19th century, and so on) we hope to pick up the same type of information.[14] Of course, in a US context one would a priori expect cross-state differences in institutional quality to be orders of magnitude smaller than in cross-country data.

In Table 4 we report the results for Internet users; Table 5 contains similar regressions for personal computers. Since PCs emerged in the 1980s we measure the determinants of PC diffusion around 1980 whenever feasible. By contrast, since the WWW emerged in 1990, we measure the same initial conditions around 1990.

In column 1 of Table 4 we examine the simple correlation between Internet users and the flash density; the latter is highly significant and can account for nearly 40% of the variation in Internet users as of 2003. In the next 6 columns we include GSP per worker in 1991 along with various human capital measures. As is clear, most of the human capital variables are highly significant, along side GSP per worker and the

[14]Details on all the data mentioned above are given in the Data Appendix.

Table 4. Lightning and Internet diffusion

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lightning	-3.57***	-3.57***	-3.63***	-1.21***	-2.30***	-2.51***	-3.10***
	[0.61]	[0.62]	[0.63]	[0.44]	[0.46]	[0.52]	[0.53]
(log) Real GSP per worker, 1991		9.95***	8.95**	8.99***	1.66	3.09	3.07
		[3.46]	[3.61]	[2.85]	[3.30]	[3.08]	[4.43]
Enrollment rate, 1991			-11.54				
			[14.57]				
High school degree or higher, 1990				71.04***			
				[9.86]			
Bachelor's degree or higher, 1991					0.86***		
					[0.14]		
College degree or higher, 1998						66.96***	
						[10.12]	
Graduate or professional degree, 1990							125.56***
							[45.86]
Constant	57.18***	-49.77	-28.33	-95.32***	20.97	7.28	15.33
	[0.72]	[37.19]	[44.27]	[31.10]	[34.28]	[32.29]	[45.65]
Observations	48	48	48	48	48	48	48
R-squared	0.38	0.45	0.46	0.73	0.66	0.64	0.54

Table 5. Lightning and computer diffusion

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Lightning	-3.68***	-3.67***	-3.56***	-1.40***	-2.13***	-2.74***	-3.26***
	[0.56]	[0.58]	[0.60]	[0.46]	[0.49]	[0.53]	[0.51]
(log) Real GSP per worker, 1977		2.40	2.81	-1.27	-2.82	-1.44	-1.39
		[3.87]	[4.24]	[3.02]	[3.00]	[3.20]	[3.42]
Enrollment rate, 1980			7.35				
			[17.82]				
High school degree or higher, 1980				47.65***			
				[8.76]			
Bachelor's degree or higher, 1977					4.47***		
					[0.73]		
College degree or higher, 1998						59.70***	
						[10.84]	
Graduate or professional degree, 1990							112.71***
							[35.25]
Constant	64.97***	39.4	28.51	44.79	41.57	65.37*	71.79*
	[0.72]	[41.23]	[53.08]	[31.95]	[31.36]	[34.08]	[36.05]
Observations	48	48	48	48	48	48	48
R-squared	0.43	0.43	0.43	0.63	0.65	0.61	0.52

Notes. OLS estimates. The dependent variable in Table 4 is percentage of households with a personal computer at home in 2003. The dependent variable in Table 5 is the percentage of households with access to the Internet at home in 2003. Lightning is the (log) average number of flashes per year per square km, measured by flash-detectors. The rest of the covariates are described in the Data Appendix. Robust standard errors in brackets. Asterisks ***, **, and * indicate significance at the 1, 5, and 10%, respectively.

Table 4A. Lightning and Internet diffusion - Additional controls

		Economy structure			Trade & Integration				Institutions			Religion	Race & ethnicity			Urbanization	Age structure	
ADDITIONAL CONTROL:		Share of Agricult. in GSP, 1991	Share of Govt. in GSP, 1991	Share of Manufact. in GSP, 1991	(log) Exports per capita, 1991	(log) FDI per capita, 1991	(log) Agric. exports per capita, 1991	(log) Populat., 1991	Soldier mortality, 1829-1854	% of workforce in mining, 1880	% of slavery, 1860	% population attending a church or a sinagogue almost every week, av. 2004-2006	% white population 1990	% black population 1990	% Hispanic origin population 1990	% urban population 1990	% population 15 years or less, 1990	% population 15-64 years, 1990
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
Lightning	-1.21***	-1.20***	-1.20***	-1.30**	-1.08**	-1.22***	-0.90*	-1.29***	-1.22***	-1.39***	-1.38***	-1.25**	-1.07**	-1.05**	-1.29**	-1.19**	-1.07***	-0.89*
	[0.44]	[0.44]	[0.44]	[0.49]	[0.50]	[0.42]	[0.48]	[0.43]	[0.44]	[0.51]	[0.45]	[0.59]	[0.47]	[0.46]	[0.51]	[0.45]	[0.40]	[0.44]
(log) Real GSP per w., 1991	8.99***	6.31*	8.55**	9.13***	7.04**	11.58***	6.30*	7.48***	8.89***	8.02***	9.00***	9.08***	11.86***	10.12***	9.54***	9.70**	5.82**	3.87
	[2.85]	[3.58]	[3.58]	[2.88]	[2.63]	[3.36]	[3.26]	[2.51]	[2.99]	[2.78]	[2.84]	[3.20]	[3.25]	[2.97]	[2.98]	[3.90]	[2.84]	[3.40]
High school or higher, 1990	71.04***	73.91***	71.05***	74.82***	71.87***	68.46***	76.43***	73.15***	72.51***	73.22***	78.65***	70.82***	60.48***	66.26***	71.32***	72.22***	78.05***	77.54***
	[9.86]	[10.14]	[9.97]	[9.84]	[9.93]	[10.82]	[10.98]	[10.20]	[11.11]	[10.26]	[13.60]	[10.25]	[10.23]	[11.89]	[10.21]	[10.92]	[9.77]	[9.24]
ADDITIONAL CONTROL		-29.17	-4.82	10.2	1.05	-1.64	-0.67*	0.63	12.36	-6.63	4.26	3.61	12.90**	-5.7	-8.63	-1.36	-56.37	67.60**
		[21.05]	[25.08]	[8.42]	[1.09]	[1.05]	[0.37]	[0.40]	[55.69]	[5.48]	[4.21]	[28.74]	[6.32]	[8.03]	[6.84]	[4.05]	[33.88]	[30.31]
Constant	-95.32***	-67.95*	-89.88**	-101.40***	-82.24***	-108.72***	-67.62*	-90.08***	-95.62***	-86.05***	-101.52***	-96.48***	-129.23***	-103.40***	-100.95***	-102.91**	-54.21	-89.61***
	[31.10]	[37.59]	[41.30]	[32.75]	[27.97]	[31.50]	[33.83]	[27.52]	[31.52]	[30.82]	[32.78]	[35.55]	[35.88]	[30.59]	[32.34]	[42.51]	[34.32]	[28.36]
Observations	48	48	48	48	48	48	48	48	48	48	48	48	48	48	48	48	48	48
R-squared	0.73	0.74	0.73	0.74	0.74	0.75	0.75	0.74	0.73	0.74	0.74	0.73	0.75	0.73	0.74	0.73	0.76	0.76

Notes. OLS estimates. The dependent variable is the percentage of households with access to the Internet at home in 2003. Lightning is the (log) average number of flashes per year per square km, measured by flash-detectors. The rest of the covariates are described in the Data Appendix. Robust standard errors in brackets. Asterisks ***, **, and * indicate significance at the 1, 5, and 10%, respectively.

Table 5A. Lightning and computer diffusion - Additional controls

		Economy structure			Trade & Integration			Institutions			Religion	Race & ethnicity			Urbanization	Age structure	
ADDITIONAL CONTROL:		Share of Agricult. in GSP, 1977	Share of Govt. in GSP, 1977	Share of Manufact. in GSP, 1977	(log) FDI per capita, 1981	(log) Agricult. exports per capita 1977	(log) Population 1977	Soldier mortality, 1829-1854	% of workforce in mining, 1880	% of slavery, 1860	% population attending a church or a sinagogue almost every week, av. 2004-2006	% white population 1980	% black population 1980	% Hispanic origin population 1980	% urban population 1980	% population 15 years or less, 1980	% population 15-64 years, 1980
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)
Lightning	-1.40***	-1.23**	-1.37***	-1.22**	-1.41***	-0.98*	-1.46***	-1.32***	-1.53***	-1.48***	-1.76***	-1.25***	-1.43***	-1.32***	-1.45***	-1.31***	-1.05**
	[0.46]	[0.50]	[0.48]	[0.47]	[0.48]	[0.51]	[0.51]	[0.43]	[0.49]	[0.50]	[0.58]	[0.40]	[0.51]	[0.44]	[0.47]	[0.43]	[0.49]
(log) Real GSP per w., 1977	-1.27	-4.17	-0.48	-2.35	-0.77	-2.43	-2.6	-0.43	-1.78	-1.19	-0.81	0.97	-1.56	0.08	-3.18	-1.91	-4.45
	[3.02]	[3.56]	[3.13]	[2.67]	[3.37]	[2.90]	[3.78]	[3.46]	[2.85]	[3.14]	[3.34]	[3.38]	[3.61]	[3.07]	[3.81]	[2.85]	[3.25]
High school or higher, 1980	47.65***	51.62***	47.46***	56.43***	47.12***	51.51***	49.54***	43.34***	52.59***	51.64***	47.24***	40.43***	49.08***	51.08***	45.42***	48.82***	50.22***
	[8.76]	[9.60]	[8.45]	[9.11]	[9.41]	[9.38]	[8.72]	[10.66]	[9.27]	[9.95]	[8.95]	[10.37]	[11.27]	[8.74]	[8.60]	[8.70]	[8.74]
ADDITIONAL CONTROL		-23.27	21.38	14.17**	-0.36	-0.57*	0.52	-53.62	-9.61	2.71	29.79	11.25	1.76	-16.51**	3.52	-23.92	50.54
		[14.52]	[20.85]	[6.72]	[0.70]	[0.29]	[0.58]	[62.71]	[6.56]	[4.50]	[27.30]	[7.92]	[10.78]	[7.03]	[3.83]	[38.65]	[50.59]
Constant	44.79	73.69**	33.44	47.24	42.17	56.56*	50.07	39.79	47.54	41.08	37.17	15.93	46.74	28.77	64.25	56.25*	43.49
	[31.95]	[36.21]	[34.36]	[28.32]	[33.58]	[30.29]	[35.53]	[34.27]	[30.40]	[33.80]	[36.31]	[37.64]	[35.60]	[32.76]	[39.42]	[33.30]	[30.93]
Observations	48	48	48	48	48	48	48	48	48	48	48	48	48	48	48	48	48
R-squared	0.63	0.64	0.64	0.67	0.63	0.65	0.64	0.64	0.65	0.63	0.64	0.65	0.63	0.67	0.64	0.64	0.65

Notes. OLS estimates. The dependent variable is percentage of households with a personal computer at home in 2003. Lightning is the (log) average number of flashes per year per square km, measured by flash-detectors. The rest of the covariates are described in the Data Appendix. Robust standard errors in brackets. Asterisks ***, **, and * indicate significance at the 1, 5, and 10%, respectively.

flash density. This is consistent with previous findings (e.g., Caselli and Coleman, 2001; Beaudry et al., 2006). Still, the best fit is obtained when we employ the fraction of state population with a high school diploma or more (column 4); along with the flash density and (log) GSP per worker the three variables can account for three quarters of the variation in Internet users.

In an effort to check for robustness Table 4A introduces additional controls (on top of human capital, income, and lightning), one by one. Nowhere is the influence from the flash density eliminated. Rather, the point estimate appears robust to the inclusion of alternative IT diffusion controls, economically as well as statistically.

Next consider Table 5. Column 1 confirms that lightning is strongly correlated with personal computer users; the R^2 is in fact slightly higher than what is true for Internet users. In general the results for personal computers are rather similar to those involving Internet diffusion. Nevertheless, there are two differences worth remarking on.

First, it appears that the measure of human capital that holds the strongest explanatory power vis-à-vis computers is the fraction of the state population with a bachelor degree or above (BA), rather than the high school variable. The difference in R^2 in the two specifications is marginal though (cf. columns 4 and 5). To ease comparability we have therefore chosen to stick with the high school measure in the context of the robustness checks. But the results are very similar if we used the BA variable instead. Second, initial GSP per worker is not significant in the regressions. Nevertheless, on theoretical grounds we have chosen to keep it in the regressions to follow.

Examining columns 1-17 of Table 5A it is clear that lightning is robust to the inclusion of plausible alternative determinants of diffusion.[15] Again the point estimate for the flash density is very stable. Interestingly, comparing Tables 4A and 5A, one may observe that the size of the coefficient assigned to the flash density is numerically very similar in the two separate specifications. This could be taken to suggest that it is the same basic mechanism that affects both computer and Internet diffusion, in keeping with the theory developed above.

The lightning-IT correlation can obviously not be ascribed to reverse causality. Moreover, since the remaining diffusion determinants are lagged, the risk that endogeneity of these variables is contaminating the OLS estimate for lightning is diminished. However, there is a particular issue which may render the results of the analysis above misleading: clustering.

Lightning density is characterized by a degree of geographical clustering for which reason we need to worry about cluster fixed effects. If cluster fixed effects are uncorrelated with the independent variables, OLS remains consistent but will underestimate the variances of regression parameters. If, on the other hand, cluster fixed effects are correlated with the independent variables, OLS turns inconsistent as well (Cameron

[15]We do not have data on exports sufficiently far back in time so as to allow it to enter in Table 5a. This accounts for the fact that Table 5a is one column smaller than Table 4a.

and Trivedi, 2005). The appropriate remedy depends on the structure of the clusters. With small clusters (i.e., many clusters and few observations within each cluster), the response is fairly straightforward. We can simply obtain cluster robust standard errors on account of independence across clusters.

However, with only 48 states, we face a problem of large clusters (i.e., few clusters and many observations within each cluster), for which reason the OLS cluster robust variance matrix is not a feasible option (Cameron and Trivedi, 2005; Angrist and Pischke, 2009). What we can do instead is to introduce cluster fixed effects via the cluster dummy variables model, as outlined in Cameron and Trivedi (2005). Introducing cluster fixed effects in this way serves to remove the intraclass error correlation that causes the potential inconsistency and the bias in the variance matrix in the first place. In order to implement the cluster dummy variables model, we need to decide on the appropriate clusters. Since there is no a priori cluster partition, we employ two alternative partitions.

The first partition is based on a decomposition of the US power grid. The US has no "national power grid." Instead, the contiguous US states are divided into two main grids, the Eastern Interconnected System and the Western Interconnected System, and a minor grid, namely the Texas Interconnected System. Electric utilities in an interconnection are electrically tied together during normal system conditions and work at a synchronized frequency operating at an average of 60Hz. The Eastern and Western Interconnects have only very limited interconnections with each other, while a few states, including Texas, are linked to both. By construction, this partition ensures cluster independence across the main two grids; i.e., climatic influences on the Western Interconnect will not influence the Eastern Interconnect, and vice versa. Thus, power disturbances are independent across the two main interconnections but dependent within interconnections.

The second partition is simply based on the four US Census Bureau regions, namely Midwest, Northeast, South, and West. Visually, this partition corresponds well with the spatial distribution of lightning (see Figure 3).[16]

Tables 6 and 7 report results from regressions where cluster dummy variables are included. In both tables, column 1 is without dummies, whereas columns 2 and 3 have Census regions and interconnection based dummies, respectively. In both tables, lightning retains significance regardless of the inclusion of cluster dummy variables. However, while the Census based clusters are jointly significant in both tables, the

[16]We did not pursue the issue of cluster fixed effects in Section 2.2 since it is difficult to see how a cluster fixed effect can account for the time varying partial correlation between growth and lightning. Nevertheless, for completeness, we have run the regressions from Table 3 while including the cluster fixed effects as discussed above. For the case of 10 year epochs, results are basically identical to those reported above. The same goes for 5 and 15 year epochs when we rely on interconnection dummies. When we use Census regions and 5 and 15 year epochs, parameters are roughly unchanged but we lose a bit in terms of precision. However, we do obtain significance at the 12 percent level. Overall, there does not seem to be an omitted variables problem on account of cluster effects.

Table 6. Lightning and Internet diffusion
(controlling for cross sectional dependence)

	(1)	(2)	(3)
Lightning	-1.21***	-1.89**	-1.46**
	[0.44]	[0.71]	[0.55]
(log) Real GSP per worker, 1991	8.99***	5.80*	7.12***
	[2.85]	[3.07]	[2.60]
High school degree or higher, 1990	71.04***	91.49***	73.18***
	[9.86]	[14.39]	[10.26]
Constant	-95.32***	-79.30**	-77.34***
	[31.10]	[32.51]	[28.50]
Regional dummies:	no	US Census	NERC
		(4 regions)	(3 regions)
H0: all reg dummies = 0 (pval)		0.03	0.15
Observations	48	48	48
R-squared	0.73	0.79	0.76

Table 7. Lightning and computer diffusion
(controlling for cross sectional dependence)

	(1)	(2)	(3)
Lightning	-1.40***	-2.10***	-1.62***
	[0.46]	[0.66]	[0.52]
(log) Real GSP per worker, 1977	-1.27	-1.8	-1.7
	[3.02]	[2.66]	[3.03]
High school degree or higher, 1980	47.65***	71.90***	53.48***
	[8.76]	[13.74]	[9.02]
Constant	44.79	30.21	44.36
	[31.95]	[27.38]	[32.65]
Regional dummies:	no	US Census	NERC
		(4 regions)	(3 regions)
H0: all reg dummies = 0 (pval)		0.03	0.07
Observations	48	48	48
R-squared	0.63	0.71	0.69

Notes. OLS estimates. The dependent variable in Table 6 is the percentage of households with access to the Internet in 2003. The dependent variable in Table 7 is the percentage of households with a personal computer at home in 2003. Lightning is the (log) average number of flashes per year per square km, measured by flash-detectors. The rest of the covariates are described in the Data Appendix. The set of regional dummies in column (2) is the US Census Bureau's regional division of the country (West, Midwest, Northeast, South), and the regional dummies in column (3) is the major interconnected power systems of the North American Electric Reliability Corporation, NERC (Western, Eastern, Texas). Robust standard errors in brackets. Asterisks ***, **, and * indicate significance at the 1, 5, and 10%, respectively.

interconnection dummies are only significant in Table 7; i.e., when computer use is the dependent variable. It is also interesting to note that standard errors are always larger when cluster dummies are added. This is consistent with OLS underestimating the variances of regression parameters in the presence of cluster dependencies. The point estimates, however, are not statistically different across the columns of each respective table. Consequently, omission of the cluster dummies does not appear to induce an omitted variables bias. In sum, taking cluster issues into account does not impact on our results.

In spite of these checks it is impossible to completely rule out that the partial correlation between lightning and IT could be attributed to one or more omitted variables in the analysis above. Still, a causal interpretation is well founded on theoretical grounds, and the empirical link between IT and lightning is clearly robust to a reasonable set of alternative IT determinants, and cluster fixed effects. Moreover, the point estimate seems stable across specifications. It falls in a reasonably confined interval, no matter which determinant we include on top of human capital and labor productivity. These characteristics provide a reasonable basis for believing the estimates above can be taken to imply that lightning is causally impacting on the speed of IT diffusion.

If we take the parameter estimate for lightning seriously, what is the economic strength of the link? Using the estimate from column 4 in Table 4 we find that a one standard deviation increase in lightning leads to a reduction in Internet users by about 1 percent.[17] In 2003 the states with the lowest Internet penetration (the 5th percentile) had about 44% of the population being able to access the Internet; at the other end of the spectrum (the 95th percentile) about 60% of the population was online. Hence the estimate for lightning implies that a one standard deviation change in lightning can account for about 7% of the 95/5 gap; four standard deviations therefore motivates about 25% of the difference.

The final issue is whether IT can account for the link between growth and lightning. Table 8 shows the relevant regression results. We focus specifically on the 1991-2007 period, as this is the period during which lightning is significantly correlated with growth.

In column 1 of Table 8 the lightning-growth correlation is reproduced. In the following two columns we add the two IT measures. Individually, both are significantly and positively correlated with growth as expected. The interpretation of the two right hand side variables is slightly different though. As noted above, the Internet originated in 1991. As a result, the independent variable can be seen as a proxy for Internet investments over the period; in 1991 the number of Internet users inevitably was close to zero, so the 2003 value effectively captures changes in Internet users over the relevant period. Needless to say the same is not true for PCs, which started diffusing far earlier. If the IT investment rate is the relevant control, the PC variable is therefore measured

[17]Recall, the standard deviation of the flash density variable is 2.4 in our 48 state sample.

Table 8. Growth, lightning, and IT

	(1)	(2)	(3)	(4)	(5)	(6)
(log) Real GSP per worker, 1991	-0.66	-0.82*	-0.99**	-0.76*	-0.92**	-1.19***
	[0.41]	[0.44]	[0.45]	[0.42]	[0.43]	[0.44]
Lightning	-0.16**			-0.09	-0.06	-0.09
	[0.08]			[0.10]	[0.09]	[0.09]
Computer presence, 2003		2.82**		1.72		-7.67
		[1.20]		[1.56]		[4.58]
Internet presence, 2003			3.32***		2.58*	9.65**
			[1.13]		[1.36]	[4.33]
Constant	8.57*	8.39*	10.19**	8.48*	9.85**	13.76***
	[4.40]	[4.52]	[4.57]	[4.45]	[4.44]	[4.79]
Observations	48	48	48	48	48	48
R-squared	0.15	0.15	0.19	0.17	0.20	0.24

Notes. OLS estimates. The dependent variable is the yearly growth rate of GSP per worker over the period 1991-2007. Computer presence is the % of households with a computer at home, and Internet presence is the % of households with access to Internet at home. Lightning is the (log) average number of flashes per year per square km, measured by flash-detectors. Robust standard errors in brackets. Asterisks ***, **, and * indicate significance at the 1, 5, and 10%, respectively.

with error. This may account for the fact that the economic size of the impact of the Internet variable is larger than that of PCs in Table 8.

The key result of the exercise is reported in columns 4-6. When the IT variables are added to the equation, the flash density looses significance. The loss of significance is mainly attributably to a lower point estimate, which essentially is cut in half. A reasonable interpretation is that lightning appears in the growth regression due to its impact on IT diffusion. Column 6 introduces all three variables at once. Despite the obvious multicollinearity in this experiment (which explains the somewhat wobbly behavior of the Internet slope estimates), Internet remains significant: this means that the Internet dominates lightning (and computers) as a predictor of cross state growth rates in the Internet era: 1991 onwards.

We believe the above analysis builds a fairly strong case in favour of the IT diffusion hypothesis; that is, the thesis that lightning appears as a growth determinant in the 1990s due to the growing influence of digital technologies on economic growth.

3.2 Climate Shocks

While the IT diffusion hypothesis is a viable explanation for the lightning-growth correlation, it is not a priori the only plausible one. Perhaps other climate related variables exert an impact on growth, and at the same time happen to be correlated with the flash density.

Table 9. Growth regressions with geographical and climate controls

GEOGRAPHY: (log)	Lightning (flashes/ year/sqkm)	Temperature (C degrees)	Precipitation (cm/year)	Tornado intensity (av EF-scale)	Hail size (cm)	Wind speed (km/h)	Humidity (% moisture in air)	Cloudiness (days/year)	Sunshine (days/year)	Elevation (meters above sea level)	Latitude (degrees)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
(log) Real GSP per w., ini.	-0.72	-0.72	-0.84*	-0.82*	-0.90*	-0.66	-0.66	-0.67	-0.86	-0.83*	-0.70
	[0.45]	[0.46]	[0.44]	[0.44]	[0.46]	[0.47]	[0.45]	[0.44]	[0.52]	[0.46]	[0.45]
GEOGRAPHY $\times t_{78-87}$	0.07	0.05	1.06***	1.83***	-0.56	-0.41***	2.14**	0.89	-1.32	-0.36***	-0.2
	[0.10]	[0.28]	[0.20]	[0.38]	[0.89]	[0.14]	[0.83]	[0.54]	[0.86]	[0.08]	[0.91]
GEOGRAPHY $\times t_{88-97}$	-0.07	0.15	0.25	0.24	-0.54	0.01	-0.95	-0.17	-0.28	-0.02	-0.03
	[0.08]	[0.25]	[0.26]	[0.44]	[0.72]	[0.10]	[0.77]	[0.43]	[0.54]	[0.09]	[0.83]
GEOGRAPHY $\times t_{98-07}$	-0.22***	-0.12	0.08	-0.24	-1.99	0.19	-0.97	-0.33	0.002	0.07	0.73
	[0.08]	[0.23]	[0.18]	[0.32]	[1.22]	[0.43]	[0.70]	[0.41]	[0.70]	[0.07]	[0.55]
Observations	144	144	144	144	144	144	144	144	141	144	144
R-squared	0.15	0.11	0.23	0.21	0.13	0.16	0.15	0.13	0.14	0.23	0.11

Notes. Pooled OLS estimates. The dependent variable is the yearly growth rate of GSP per worker over the periods 1977-1987, 1987-1997, and 1997-2007. All regressions include a constant and a full set of time-dummies. All geographic/climate variables are (log) annual averages taken over periods of 10 years, described in the Data Appendix. Lightning is the (log) average number of flashes per year per square km, measured by flash-detectors. Robust standard errors in brackets, adjusted for clustering at the state level. Asterisks ***, **, and * indicate significance at the 1, 5, and 10%, respectively.

Table 10. Growth regressions with geographical and climate controls, conditional on lightning

GEOGRAPHY: (log)	Lightning (flashes/ year/sqkm)	Temperature (C degrees)	Precipitation (cm/year)	Tornado intensity (av EF-scale)	Hail size (cm)	Wind speed (km/h)	Humidity (% moisture in air)	Cloudiness (days/year)	Sunshine (days/year)	Elevation (meters above sea level)	Latitude (degrees)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
(log) Real GSP p.w., ini.	-0.72	-0.80*	-0.92**	-0.86*	-0.84*	-0.68	-0.65	-0.67	-0.85	-1.04**	-0.75
	[0.45]	[0.47]	[0.43]	[0.43]	[0.48]	[0.46]	[0.45]	[0.45]	[0.52]	[0.47]	[0.46]
Lightning $\times t_{78-87}$	0.07	0.08	-0.07	-0.06	0.12	-0.17	-0.001	0.10	0.11	-0.15	0.09
	[0.10]	[0.11]	[0.14]	[0.12]	[0.12]	[0.14]	[0.14]	[0.12]	[0.11]	[0.12]	[0.13]
Lightning $\times t_{88-97}$	-0.07	-0.13	-0.11	-0.1	-0.06	-0.07	-0.04	-0.07	-0.06	-0.1	-0.12
	[0.08]	[0.08]	[0.09]	[0.10]	[0.08]	[0.09]	[0.09]	[0.08]	[0.07]	[0.10]	[0.09]
Lightning $\times t_{98-07}$	-0.22***	-0.29***	-0.25***	-0.22***	-0.21**	-0.23***	-0.21**	-0.24***	-0.23***	-0.24**	-0.29***
	[0.08]	[0.09]	[0.08]	[0.08]	[0.09]	[0.08]	[0.08]	[0.08]	[0.08]	[0.09]	[0.10]
GEOGRAPHY $\times t_{78-87}$		-0.06	1.12***	1.95***	-0.95	-0.58***	2.15**	0.97*	-1.47*	-0.43***	0.29
		[0.31]	[0.26]	[0.45]	[0.98]	[0.18]	[1.03]	[0.55]	[0.84]	[0.09]	[1.12]
GEOGRAPHY $\times t_{88-97}$		0.33	0.36	0.44	-0.45	-0.03	-0.83	-0.22	-0.2	-0.08	-0.69
		[0.26]	[0.27]	[0.52]	[0.77]	[0.11]	[0.83]	[0.42]	[0.51]	[0.11]	[1.00]
GEOGRAPHY $\times t_{98-07}$		0.37	0.25	-0.04	-0.4	-0.16	-0.28	-0.52	0.33	-0.04	-0.86
		[0.27]	[0.20]	[0.28]	[1.22]	[0.42]	[0.86]	[0.34]	[0.60]	[0.08]	[0.71]
Observations	144	144	144	144	144	144	144	144	141	144	144
R-squared	0.15	0.17	0.29	0.26	0.16	0.22	0.19	0.19	0.19	0.29	0.16

Notes. Pooled OLS estimates. The dependent variable is the yearly growth rate of GSP per worker over the periods 1977-1987, 1987-1997, and 1997-2007. All regressions include a constant and a full set of time-dummies. All geographic/climate variables are (log) annual averages taken over periods of 10 years. Lightning is the (log) average number of flashes per year per square km, measured by flash-detectors. Robust standard errors in brackets, adjusted for clustering at state level. Asterisks ***, **, and * indicate significance at the 1, 5, and 10%, respectively.

To begin with, lightning correlates with various kinds of weather phenomena that arise in the context of thunderstorms. Aside from lightning, thunderstorms produce four weather phenomena: tornadoes, high winds, heavy rainfall, and hailstorms. It seems plausible that these climate variables can induce changes in the growth rate in individual states in their own right. Each of them destroy property (physical capital), people (human capital), or both (Kunkel et al., 1999). By directly affecting the capital-labor ratio, the consequence of, say, a tornado could be changes in growth attributable to transitional dynamics. The nature of the transitional dynamics (i.e., whether growth rises or falls) is unclear, as it may depend on whether the tornado destroys more physical or human capital (e.g., Barro and Sala-i-Martin, 1995, Ch.5).[18] Nevertheless, since the lightning-growth correlation pertains to a relatively short time span (so far), it is hard to rule out that the above reasoning could account for it.

In addition, lightning correlates with temperature; hotter environments usually feature a higher flash density. Temperature has been documented to correlate with economic activity within countries (e.g., Nordhaus, 2006; Dell et al., 2009); therefore, we cannot rule out a priori that the link between lightning and growth is attributable to the intervening influence of temperature.[19]

Hence, in an effort to examine whether climate shocks could account for the lightning-growth correlation, we gathered data on all of the above weather phenomena: temperature, precipitation, tornadoes, hail size, and wind speed. In addition, we obtained data on topography (i.e., elevation) and latitude. The latter is a useful catch-all measure of climate. For good measure, we also obtained data on sunshine, humidity, and cloud cover (albeit it is not entirely clear why these weather phenomena should matter to growth). In total, we have data on ten alternative climate/geography variables; the details on the data are found in the Data Appendix.

With these data in hand, we ask two questions. First, ignoring lightning, do any of these weather phenomena exhibit a correlation with growth which is similar to that of lightning? That is, do any of them appear to become more strongly correlated with growth during the period 1977-2007? Second, taking lightning into account, do any of the above mentioned variables render lightning insignificant?

Tables 9 and 10 report the answer. As the lightning correlation does not depend appreciably on whether we invoke 5, 10 or 15 year epoch length we have chosen to focus on 10 year epochs. Results for 5 and 15 year epochs are similar, and available upon request.

Columns 2-11 of Table 9 examine the potentially time varying impact from each weather variable; column 1 reproduces the lightning regularity from Section 2.2. It is

[18] In a US context one may suspect a relatively larger impact on physical capital compared to human capital; if so climate shocks would tend to instigate a growth acceleration in their aftermath, as a higher marginal product of capital induces firms to invest in physical capital.

[19] Nordhaus (2006) and Dell et al. (2009) document a correlation between temperature and income levels, not growth. In fact, Dell et al. (2008) find that temperature is not correlated with growth in rich places, using cross-country data. Nevertheless, the link seems worth exploring.

plain to see that none of the weather variables exhibit a similar growth correlation as that involving lightning. In fact, it is always the case that the variable in question is either insignificant, or tends to become less correlated with growth over time.

In Columns 2-11 of Table 10 we simultaneously include lightning and the various alternative climate/geography controls. In all cases, lightning remains significantly correlated with growth. In fact, when comparing the point estimate for lightning with or without (column 1) additional controls, it emerges that the point estimate is virtually unaffected.

In sum, these results suggest that the lightning-growth correlation is unlikely to be attributable to other weather phenomena.

3.3 Institutions and Integration

An extensive literature examines the impact from historical factors on long-run development. For instance, variation in colonial strategies seems to have an important impact on institutional developments around the world, thus affecting comparative economic development (e.g., Acemoglu et al., 2001). Similarly, initial relative factor endowments, determined in large part by climate and soil quality, may well have affected long-run development through inequality and human capital promoting institutions (Engerman and Sokoloff, 2002; Galor et al., 2008). Thus, in many instances the initial conditions that may have affected long-run developments are related to climate or geography. In the present context, therefore, it seems possible that the lightning-growth correlation may be picking up the influence from such long-run historical determinants of prosperity. Naturally, the conventional understanding would be that "deep determinants of productivity", e.g. determinants of political and economic institutions, should have a fairly time invariant impact on growth. As a result, it would not be surprising if such determinants do not exert a time varying impact on growth. But whether it is the case or not is obviously an empirical matter.

To examine whether the lightning-growth nexus is attributable to such effects, we obtained data on ten potential determinants of long-run performance for the US. The source of the data is Mitchener and McLean (2003), who examine the determinants of long-run productivity levels across US states. In addition, we collected state-level data on three dimensions of global integration, related to international movements of goods and capital. This leaves us with 13 different potential determinants of labor productivity growth, broadly capturing "institutions, geography and integration" (Rodrik et al., 2004).[20]

As in Section 3.2 we ask whether these determinants, individually, exhibit a time varying impact on growth, and whether their inclusion in the growth regression renders lightning insignificant.

[20] See the Data Appendix for details.

Table 11. Growth regressions with historical controls

HISTORY:		% of workforce in mining, 1880	Average no. of cooling degree days	% of 1860 population in slavery	Access to navigable water	% of 1860 population on large slave plantations	Settler origin:				Average annual soldier mortality in 1829-1838, 1839-1854, %
							English	French	Spanish	Dutch	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
(log) Real GSP per w., init.	-0.72	-0.71	-0.73	-0.68	-0.87*	-0.68	-0.72*	-1.23***	-0.73*	-1.03**	-0.67
	[0.45]	[0.44]	[0.45]	[0.45]	[0.45]	[0.45]	[0.43]	[0.43]	[0.40]	[0.43]	[0.45]
Lightning $\times t_{78-87}$	0.07										
	[0.10]										
Lightning $\times t_{88-97}$	-0.07										
	[0.08]										
Lightning $\times t_{98-07}$	-0.22***										
	[0.08]										
HISTORY $\times t_{78-87}$		-2.95***	-0.004	1.10**	0.74***	1.80*	0.70***	-0.43**	-0.69***	0.65***	-1.90
		[0.68]	[0.01]	[0.54]	[0.20]	[0.91]	[0.18]	[0.21]	[0.19]	[0.20]	[10.49]
HISTORY $\times t_{88-97}$		-0.67	-0.01	-0.61	0.29	-1.21*	0.12	-0.55***	-0.26	0.45*	-5.36
		[0.93]	[0.01]	[0.43]	[0.26]	[0.68]	[0.19]	[0.16]	[0.17]	[0.23]	[8.78]
HISTORY $\times t_{98-07}$		0.84	-0.005	-0.69*	0.05	-1.14**	-0.05	-0.36**	0.02	0.36	-15.99**
		[0.71]	[0.01]	[0.36]	[0.18]	[0.53]	[0.18]	[0.17]	[0.16]	[0.29]	[6.84]
Observations	144	144	144	144	144	144	144	144	144	144	144
R-squared	0.15	0.18	0.11	0.15	0.19	0.16	0.20	0.21	0.21	0.15	0.13

Notes. Pooled OLS estimates. The dependent variable is the yearly growth rate of GSP per worker over the periods 1977-1987, 1987-1997, and 1997-2007. All regressions include a constant and a full set of time-dummies. Lightning is the (log) average number of flashes per year per square km, measured by flash-detectors. All HISTORY variables taken from Mitchener and McLean (2004). Robust standard errors in brackets, adjusted for clustering at state level. Asterisks ***, **, and * indicate significance at the 1, 5, and 10%, respectively.

Table 12. Growth regressions with historical controls and lightning

HISTORY:		% of workforce in mining, 1880	Average no. of cooling degree days	% of 1860 population in slavery	Access to navigable water	% of 1860 population on large slave plantations	Settler origin: English	Settler origin: French	Settler origin: Spanish	Settler origin: Dutch	Average annual soldier mortality in 1829-1838, 1839-1854, %
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
(log) Real GSP per w., init.	-0.72	-0.78*	-0.700	-0.65	-0.96**	-0.66	-0.73	-1.20***	-0.74*	-1.02**	-0.67
	[0.45]	[0.43]	[0.46]	[0.46]	[0.44]	[0.47]	[0.43]	[0.44]	[0.40]	[0.44]	[0.46]
Lightning $\times t_{78-87}$	0.07	-0.06	0.14	-0.06	-0.03	-0.05	0.06	0.12	0.04	0.07	0.11
	[0.10]	[0.13]	[0.11]	[0.13]	[0.13]	[0.12]	[0.10]	[0.10]	[0.10]	[0.10]	[0.11]
Lightning $\times t_{88-97}$	-0.07	-0.11	-0.04	-0.01	-0.11	0.01	-0.07	-0.01	-0.08	-0.06	-0.05
	[0.08]	[0.09]	[0.07]	[0.09]	[0.09]	[0.08]	[0.08]	[0.09]	[0.08]	[0.08]	[0.08]
Lightning $\times t_{98-07}$	-0.22***	-0.23**	-0.33***	-0.23**	-0.25***	-0.22**	-0.22***	-0.20**	-0.22***	-0.22***	-0.19*
	[0.08]	[0.09]	[0.08]	[0.09]	[0.08]	[0.09]	[0.08]	[0.09]	[0.08]	[0.08]	[0.10]
HISTORY $\times t_{78-87}$		-3.18***	-0.01	1.31**	0.77***	2.05**	0.70***	-0.47**	-0.68***	0.66***	-7.63
		[0.87]	[0.01]	[0.63]	[0.24]	[1.02]	[0.19]	[0.21]	[0.20]	[0.21]	[11.70]
HISTORY $\times t_{88-97}$		-1.16	-0.004	-0.58	0.39	-1.24	0.12	-0.55***	-0.27	0.44*	-2.60
		[1.06]	[0.01]	[0.50]	[0.29]	[0.78]	[0.19]	[0.18]	[0.17]	[0.24]	[9.04]
HISTORY $\times t_{98-07}$		-0.15	0.02***	0.07	0.25	-0.03	-0.03	-0.27	-0.01	0.33	-5.79
		[0.97]	[0.01]	[0.44]	[0.22]	[0.68]	[0.16]	[0.18]	[0.15]	[0.28]	[8.42]
Observations	144	144	144	144	144	144	144	144	144	144	144
R-squared	0.15	0.22	0.17	0.18	0.24	0.19	0.24	0.25	0.25	0.20	0.16

Notes. Pooled OLS estimates. The dependent variable is the yearly growth rate of GSP per worker over the periods 1977-1987, 1987-1997, and 1997-2007. All regressions include a constant and a full set of time-dummies. Lightning is the (log) average number of flashes per year per square km, measured by flash-detectors. All HISTORY variables taken from Mitchener and McLean (2004). Robust standard errors in brackets, adjusted for clustering at state level. Asterisks ***, **, and * indicate significance at the 1, 5, and 10%, respectively.

Table 13. Growth regressions with trade & integration controls and lightning

INTEGRATION: (log)		Agricultural exports per capita	Exports per capita	FDI per per capita	Agricultural exports per capita	Exports per capita	FDI per per capita
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(log) Real GSP per w., ini.	-0.72	-1.02**	-1.12**	-0.41	-0.99**	-1.10**	-0.42
	[0.45]	[0.44]	[0.43]	[0.55]	[0.44]	[0.44]	[0.56]
Lightning $\times t_{78-87}$	0.07				0.15*	0.09	0.07
	[0.10]				[0.09]	[0.11]	[0.11]
Lightning $\times t_{88-97}$	-0.07				-0.04	-0.05	-0.06
	[0.08]				[0.08]	[0.09]	[0.08]
Lightning $\times t_{98-07}$	-0.22***				-0.23***	-0.20**	-0.22***
	[0.08]				[0.08]	[0.07]	[0.07]
INTEGRATION $\times t_{78-87}$		-0.18***	0.25	-0.12	-0.20***	0.28	-0.13
		[0.05]	[0.20]	[0.16]	[0.05]	[0.20]	[0.16]
INTEGRATION $\times t_{88-97}$		-0.14**	0.2	-0.13	-0.14**	0.18	-0.11
		[0.06]	[0.22]	[0.18]	[0.06]	[0.23]	[0.19]
INTEGRATION $\times t_{98-07}$		-0.01	0.37***	-0.42***	0.01	0.31**	-0.42***
		[0.05]	[0.12]	[0.12]	[0.05]	[0.13]	[0.13]
Observations	144	144	144	144	144	144	144
R-squared	0.15	0.22	0.16	0.13	0.28	0.19	0.18

Notes. Pooled OLS estimates. The dependent variable is the yearly growth rate of GSP per worker over the periods 1977-1987, 1987-1997, and 1997-2007. All regressions include a constant and a full set of time-dummies. Lightning is the (log) average number of flashes per year per square km, measured by flash-detectors. Robust standard errors in brackets, adjusted for clustering at state level. Asterisks ***, **, and * indicate significance at the 1, 5, and 10%, respectively.

In Table 11 we examine the impact from various historical determinants of productivity one-by-one. Interestingly, in several cases the impact does seem to vary across decades. Of particular note is column 11, which involves soldier mortality rates. Much like lightning the partial correlation seems stronger at the end of the period, compared to the beginning of the 1977-2007 period.

Table 12 includes both lightning and the individual controls. Since the soldier mortality rates is the only variable we have found so far that exhibits a correlation with growth that is qualitatively similar to that of lightning, the results reported in column 11 is of central importance. When both variables enter the growth regression only lightning retains explanatory power. The point estimate for the last period does decline a bit, and the statistical significance of lightning is somewhat reduced. But soldier mortality rates do not statistically dominate lightning in the specification. More broadly, it is once again worth observing how stable the partial correlation between lightning and growth seems to be. Comparing the results reported in column 1 (no historical controls) for lightning to those reported in columns 2-11 it is clear that the coefficient for lightning is quite robust.

Finally, Table 13 examines the potential influence from integration. Of particular note is column 3, from which it is clear that the influence from exports seems to have increased over the period. This result may have a reasonable interpretation. The EU common market (established in the early 1990s), and, perhaps more importantly, the emergence of China and India in world markets may well have left an imprint on growth. A likely path of influence would be trade. This can explain an intensification of the impact from trade in the 1990s, on labor productivity growth, through greater division of labor. While it thus seems that exports have become more important to growth at the end of the 1977-2007 period, column 6 reveals that trade does not account for the lightning-growth correlation.

4 Concluding Remarks

In theory, lightning should impact on IT diffusion. Higher lightning intensity leads to more frequent power disruptions, which in turn reduces the longevity of IT equipment. As a result, by inducing higher IT user cost, a higher lightning frequency should hamper IT investments. By implications, high-tech societies may actually be quite vulnerable to climate shocks. Consistent with the temperate drift hypothesis, technological change may therefore render societies more sensitive to climate phenomena that previously were only of second order importance.

Empirically, we document that lightning activity is negatively correlated with measures of IT diffusion; computers and Internet hook-ups per household. Conditional on standard controls, states with less lightning have adopted IT more rapidly than states where lightning activity is more intensive.

Consistent with a detrimental impact on IT diffusion, we find that states with more lightning have grown slower from about 1990 onwards. This pattern cannot be accounted for by other climate phenomena, nor can it be explained by a time varying influence from deep historical determinants of productivity.

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Appendix: Data description and Tables

Main variables:

1. **Lightning.** Our main measure of lightning density, originating from ground-based flash sensors, is from the US National Lightning Detection Network Database (NLDN). The NLDN consists of more than 100 remote, ground-based lightning sensors, which instantly detect the electromagnetic signals appearing when lightning strikes Earth's surface. The data is available as an average over the period 1996-2005 for the 48 contiguous US states from Vaisala's website: http://www.vaisala.com. We find that lightning is not statistically different from a constant plus white noise (see main text for analysis). Therefore, we extend Vaisala's data to the period 1977-2007. To investigate the time-series properties of lightning, we use data on the number of thunder days (TD) per year by state, available for the period 1901-1995. These data are collected as part of the Climate Change Detection and Attribution Program at the National Oceanic and Atmospheric Administration (NOAA). The raw data comes from 734 cooperative observer stations and 121 first order stations (see Changnon, 2001 for a detailed description). The data consists of monthly and yearly TD totals for 38 US states over the period 1901-1995, 40 states over the period 1906-1995 and 42 states over the period 1951-1995. It is available for purchase from the Midwestern Regional Climate Center: http://mrcc.isws.illinois.edu/prod_serv/tstorm_cd/tstorm1.html. From these data, we calculated the average yearly number of thunder days per state. Ultimately, we are interested in average flash density (FD) by state rather than thunder days per year. FDs are defined as the number of ground strikes per sq km per year. We converted yearly TDs into FDs using the following formula (Chisholm, 2000):
$$FD = 0.04 \times TD^{1.25}.$$

2. **Temperature and Precipitation.** Data from the United States Historical Climatology Network (USHCN) project, developed at NOAA's National Climatic Data Center (NCDC) to assist in the detection of regional climate change across the US. The USHCN project has produced a dataset of daily and monthly records of basic meteorological variables (maximum and minimum temperature, total precipitation, snowfall, and snow depth) from over 1000 stations across the 48 contiguous US states for the period 1900-2006. The precipitation data we use is corrected by USHCN for the presence of outlier daily observations, time of data recording, and time series discontinuities due to random station moves and other station changes. The temperature data we use is additionally corrected for warming biases created by urbanization, and the replacement of liquid-in-glass thermometers by electronic temperature measurement devices during the mid 1980s. We

construct yearly average temperatures (expressed in degrees Celsius) and yearly average precipitation totals (expressed in cm per year) for each state, as simple averages of monthly data from 1221 stations across the country. The data is available at: http://cdiac.ornl.gov/epubs/ndp/ushcn/newushcn.html.

3. **Latitude.** Latitude at the center of the state, calculated from geographic coordinates from the US Board on Geographic Names. The data is available at: http://geonames.usgs.gov/domestic/download_data.htm.

4. **Altitude.** Approximate mean elevation by state. Data source: US Geological Survey, Elevations and Distances in the United States, 1983. Available from the US Census Bureau at: http://www.census.gov/prod/2004pubs/04statab/geo.pdf.

5. **Tornadoes, Wind, and Hail.** The Storm Prediction Center of NOAA's National Weather Service Center provides data for tornadoes, wind, and hail for the period 1950-2007. Data is available for the tornado occurrences and their damage categories in the Enhanced Fujita (EF) scale (assigning 6 levels from 0 to 5). We construct a measure of tornado intensity as the average damage category for all tornado occurrences during a year. For all the estimations, we rescale the EF categories from the original 0 to 5 scale to a 1 to 6 scale. Wind is measured as the yearly average of wind speed, expressed in kilometers per hour. Hail is measured as the average size of hail in centimeters. The data is available at http://www.spc.noaa.gov/climo/historical.html.

6. **Humidity, Sunshine and Cloudiness.** Data from the "Comparative Climatic Data for the United States through 2007", published by NOAA. (Relative) humidity is the average percentage amount of moisture in the air, compared to the maximum amount of moisture that the air can hold at the same temperature and pressure. Cloudiness is measured as the average number of days per year with 8/10 to 10/10 average sky cover (or with 7/8 to 8/8 average sky cover since July 1996). Sunshine is the total time that sunshine reaches the Earth's surface compared to the maximum amount of possible sunshine from sunrise to sunset with clear sky conditions. The data is available at http://www1.ncdc.noaa.gov/pub/data/ccd-data/CCD-2007.pdf.

7. **GSP per worker.** Gross Domestic Product by state (GSP) per worker in chained 2000 US$. US Bureau of Economic Analysis (BEA) offers two series of real GSP. The first is for the period 1977-1997, where industry classification is based on the Standard Industrial Classification (SIC) definitions. The second series covers the period 1997-2007 and relies on industrial classification based on the North American industrial Classification System (NAICS). Both GSP series are available at http://www.bea.gov/regional/gsp/. We build a single measure of real GSP, extending levels of the series based on the SIC system with the yearly growth

rates of the series based on the NAICS. This is equivalent to assuming that from 1997 onwards, the growth rate of GSP per worker calculated with the SIC system equals the growth rate of real GSP calculated with the NAICS definitions.[21] Based on this estimate for real GSP, we construct a yearly series of real GSP per employed worker dividing real GSP by the number of employees per state. The growth rate is measured in percentages. State-by-state data for the number of employed workers is provided by the State Personal Income accounts at the US BEA (available at: http://www.bea.gov/regional/spi).

8. **Computers and Internet.** Percentage of households with computer and percentage of households with Internet access at home in 2003. Data collected in a supplement to the October 2003 US Current Population Survey, available at: http://www.census.gov/population/socdemo/computer/2003/tab01B.xls.

Main variables for IT diffusion:

Human capital. This extended list of human capital variables is downloaded from http://www.allcountries.org.

1. **Enrollment rate:** Public elementary and secondary school enrollment as a percentage of persons 5-17 years old. From "Digest of Education Statistics", National Center of Education Statistics (NCES), Institute of Education Sciences, US Department of Education, http://nces.ed.gov/programs/digest/. Available at: http://www.allcountries.org/uscensus/266publicelementaryandsecondaryschoolenrollment.html.

2. **High school degree or higher:** Persons with a high school degree or higher as a percentage of persons 25 years and over. From "Digest of Education Statistics", National Center of Education Statistics (NCES), Institute of Education Sciences, US Department of Education,
http://nces.ed.gov/programs/digest/d03/tables/dt011.asp.

3. **Bachelor's degree or higher:** Persons with a bachelor's degree or higher as a percentage of persons 25 years and over. Same source as high school degree or higher.

4. **College degree or higher:** Persons with a college degree or higher as a percentage of persons 25 years and over. Same source as high school degree or higher and bachelor's degree or higher.

[21] BEA warns against merging the level of the two series of real GSP directly, since the discontinuity in the industrial classification system will obviously affect level and growth rate estimates. Our choice of merging the growth rates of the two series can be justified recalling both the SIC and the NAICS aim to classify production of all industries in each state, so that the growth rate of both GSP series in levels is comparable. As a check, we computed the correlation between the growth rate of aggregate US GDP and gross domestic income (GDI), since GDP corresponds to the NAICS-definition and GDI corresponds to the SIC-definition (BEA, http://www.bea.gov/regional/gsp/). The correlation is higher than 0.99 for different periods between 1929 and 2007.

5. **Graduate or professional degree:** Persons with a graduate or professional degree as a percentage of persons 25 years and over. Same source as high school degree or higher, bachelor's degree or higher, and college degree or higher.

Additional determinants of IT diffusion:

In addition to human capital, Caselli and Coleman (2001) suggest the following set of determinants of computer technology diffusion across countries: real income, GDP shares of different sectors, stock of human capital, amount of trade, and degree of integration to the world economy. We gathered similar data for US states, described below.

1. **Shares of agriculture production, manufacturing production, and government spending in GSP:** Agriculture, forestry, fishing, and hunting production as % of GSP; Manufacturing production as % of GSP, Total Government spending as % of GSP.

2. **Agricultural exports per capita:** Agricultural exports per capita (US$). Total value of Agricultural exports by state, from US Department of Agriculture, divided by population. Available at: http://www.ers.usda.gov/Data/StateExports/2006/SXHS.xls. Population data from US Census Bureau.

3. **Exports per capita:** Exports per capita (US$). Total exports by state from US Department of Commerce divided by population. Total exports data available for purchase from US Census Bureau at http://www.census.gov/foreign-trade/reference/products/catalog/stateweb.html. Population data from the US Census Bureau. Freely available for the period 1990-1999 at: http://allcountries.org/uscensus/1326_u_s_exports_by_state_of.html.

4. **FDI per capita:** Gross value of Property, Plant, and Equipment (PPE) of Nonbank US Affiliates, per capita (US$). Data on PPE available from US BEA for the period 1999-2006 available at: http://bea.doc.gov/international/xls/all_gross_ppe.xls. For the year 1981 and the period 1990-1997 available at: http://allcountries.org/uscensus/1314_foreign_direct_investment_in_the_u.html. Population data from US Census Bureau.

5. **Institutional and historical determinants of productivity:** All variables are taken from Mitchener and McClean (2003).

   (a) **% workforce in mining, 1880:** Percentage of the workforce employed in mining in 1880.

   (b) **Average no. cooling degree days:** The average number of cooling degree days is computed as the number of days in which the average air temperature rose above 65 degrees Fahrenheit (18 degrees Celsius) times the number of degrees on those days which the average daily air temperature exceeded 65 over the year.

(c) **% of 1860 population in slavery:** The total number of slaves as a percentage of the total population of each state in 1860.

(d) **% of 1860 population on large slave plantations:** The number of slaves owned by slaveholders having more than 20 slaves as a percentage of the total population of each state in 1860.

(e) **Access to navigable water:** An indicator variable that takes the value of one if a state borders the ocean/Great Lake /river, and zero otherwise.

(f) **Settler origin:** A series of indicator variables which take on positive values if a state, prior to statehood, had ties with that colonial power.

(g) **Average annual soldier mortality in 1829-1838, 1839-1854, %:** Soldier mortality rates at the state level are derived using US soldier mortality data for individual forts. Quarterly data were collected by the US Surgeon General and Adjutant General's Offices 1829-1838 and by the US Surgeon General's Office for 1839-1854. Mitchener and McClean obtained the yearly mortality rates by dividing the number of deaths each year by the average annual "mean strength" of soldiers.

6. **Socio-demographic indicators:** Data on religiousness, race and ethnicity, urbanization and age structure of the population; from various sources.

(a) **Church attendance, average 2004-06:** Data from a Gallup Poll analysis, conducted between January 2004 and March 2006, based on responses to the question, "How often do you attend church or synagogue – at least once a week, almost every week, about once a month, seldom, or never?" Data available at: http://www.gallup.com/poll/22579/church-attendance-lowest-new-england-highest-south.aspx#2

(b) **% of white population, black population, and population of Hispanic origin:** Data for race and Hispanic origin for the US, regions, divisions, and states (100-Percent Data). Source: US Census Bureau. Data available at: http://www.census.gov/population/www/documentation/twps0056/tabA-03.xls (for 1980), and http://www.census.gov/population/www/documentation/twps0056/tabA-01.xls (for 1990).

(c) **% of urban population:** Rural and Urban population 1900-1990 (released 1995). Source: US Census Bureau. Available at: http://www.census.gov/population/www/censusdata/files/urpop0090.txt

(d) **% of population 15 years or less, and % of population between 15-64 years:** Population by broad age group. "Demographic Trends in the 20th Century", Table 7, parts D and E. Source: US Census Bureau. Available at: http://www.census.gov/prod/2002pubs/censr-4.pdf

Table A1. Tests for whether lightning is a constant plus white noise

	Breusch-Godfrey test			Runs test	
	test-statistic	p-value	No. lags (a)	test-statistic	p-value
Aggregate US	0.02	0.88	1	0.46	0.65
Alabama	0.61	0.43	1	-0.22	0.82
Arizona	0.16	0.69	1	-0.13	0.90
Arkansas	0.16	0.69	1	1.67	0.09
California	0.48	0.49	1	-0.12	0.91
Colorado	0.12	0.73	1	0.25	0.80
Florida	0.02	0.90	1	-0.70	0.49
Georgia	0.00	0.95	1	0.25	0.80
Idaho	0.02	0.90	1	0.72	0.47
Illinois	0.20	0.65	1	-1.64	0.10
Indiana	1.67	0.20	1	-0.22	0.82
Iowa	0.20	0.66	1	-0.22	0.82
Kansas	0.58	0.44	1	0.84	0.40
Kentucky	0.24	0.62	1	0.25	0.80
Louisiana	0.06	0.81	1	-0.70	0.49
Maine	1.05	0.31	1	0.25	0.80
Maryland	0.01	0.94	1	0.25	0.80
Massachusetts	1.29	0.26	1	0.72	0.47
Michigan	0.33	0.56	1	-0.70	0.49
Minnesota	0.00	0.98	1	-1.64	0.10
Mississippi	0.98	0.32	1	-2.12	0.03
Missouri	0.19	0.66	1	0.36	0.72
Montana	0.71	0.40	1	-2.12	0.03
Nebraska	0.22	0.64	1	-0.70	0.49
Nevada	0.02	0.88	1	0.72	0.47
New Mexico	1.25	0.26	1	-0.22	0.82
New York	7.52	0.02	2	0.36	0.72
North Carolina	0.74	0.39	1	-1.45	0.15
North Dakota	5.30	0.07	2	-0.22	0.82
Ohio	0.03	0.85	1	-0.70	0.49
Oklahoma	2.97	0.09	1	-1.64	0.10
Oregon	0.64	0.42	1	-1.45	0.15
Pennsylvania	5.25	0.07	2	0.72	0.47
South Carolina	0.23	0.63	1	-0.22	0.82
South Dakota	2.93	0.09	1	1.33	0.18
Tennessee	0.22	0.64	1	-0.22	0.82
Texas	3.79	0.05	1	-0.22	0.82
Utah	4.54	0.03	1	-0.70	0.49
Virginia	4.68	0.03	1	-0.22	0.82
Washington	0.48	0.49	1	-0.61	0.54
West Virginia	4.56	0.03	1	0.72	0.47
Wisconsin	0.57	0.45	1	-1.17	0.24
Wyoming	0.09	0.77	1	-0.22	0.82

Notes. The residuals are obtained from regressing lightning on a constant for each of the 42 states over the period 1977-1995. H0: Residuals are not serially correlated. Lightning is average number of flashes per year per square km, measured at weather stations.
(a) Lags selected by Schwarz's information criteria.

Table A2. Summary statistics for the main variables

				Percentiles				
	Obs.	**Mean**	**Std. Dev.**	**99%**	**75%**	**50%**	**25%**	**1%**
Lightning, average 1996-2005 (flashes/year/sq km)	48	3.18	2.39	10.79	5.30	2.48	1.23	0.12
Annual growth rate of real GSP per worker, average 1977-2007 (%)	48	1.07	0.42	1.97	1.37	1.07	0.82	0.10
Internet presence, 2003 (%)	48	54.4	5.9	65.5	58.1	55.0	51.2	39.5
Computer presence, 2003 (%)	48	62.1	5.7	74.1	66.3	61.9	59.0	48.8

Notes. Lightning defined as average number of flashes per year per square km over the period 1995-2006, measured by flash-detectors.

On the Impact of Digital Technologies on Corruption: Evidence from U.S. States and across Countries*

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Abstract

Does increased Internet use reduce corruption? To examine the question we develop a novel identification strategy for Internet diffusion. By instigating equipment damage, power disruptions increase the user cost of IT capital, which in turn lowers the speed of Internet diffusion. A natural phenomenon causing power disruptions is lightning activity. Using global satellite data and data from ground-based lightning detection censors, we construct lightning density data for a large cross section of countries and for the contiguous U.S. states. Empirically, lightning density is a strong instrument for Internet diffusion. In conformity with OLS estimates, IV estimates show that Internet diffusion has indeed reduced the extent of corruption across countries and across U.S. states.

Keywords: Corruption, Internet, Instrumental variables estimation, Lightning.
JEL Classification codes: K4, O1, H0.

1 Introduction

Corruption is commonly perceived to be a major stumbling block on the road to prosperity. Aside from retarding growth (Mauro, 1995), corruption entails "fiscal leakage", which reduces the ability of poor countries to supply public services like schooling and health care (e.g. Reinikka and Svensson, 2004). Put simply, it is a government failure one would like to dispose of.

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The present paper provides evidence that the spread of the Internet has reduced the extent of corruption during the 1990s and early years of the 21st century. Theoretically, there are two fundamental avenues through which the Internet has been able to accomplish this feat. First, the World Wide Web (WWW) is a new major source of information.[1] By disseminating information of official wrong-doing the "risk of detection" inevitably rises for politicians and civil servants. Second, the Internet is the chief vehicle for the provision of E-government worldwide.[2] By allowing citizens direct access to government services online, E-government obviates bureaucrats' role as intermediaries between the government and the public, thus limiting the interaction between potentially corrupt officials and the public. Moreover, online systems, by construction, require standardized rules and procedures. This reduces bureaucratic discretion and increases transparency as compared to the "arbitrariness" available to civil servants when dealing with the public on a case-by-case basis.

The Internet is a new technology, which greatly influences our estimation approach. Figure 1 illustrates its remarkable growth from 1990; in roughly 15 years the number of Web sites has grown by 100 million.

To examine the impact of the WWW we estimate the impact of *changes* in Internet users on *changes* in corruption levels from 1990s to 2006. Using cross-country data as well as data for the 48 contiguous U.S. states we find that increasing Internet penetration lowers corruption. The impact is both economically and statistically significant. The correlation is robust to the inclusion of a large set of corruption determinants, both across countries and states.

In order to establish causality we develop an identification strategy based on finding exogenous variation in the costs of using IT equipment in general, and the Internet in particular. As explained in detail below, computer equipment is highly sensitive to power disruptions: power surges can lead to equipment failure and damage. A higher frequency of power disruptions thereby leads to increasing costs of IT equipment, either by elevating capital depreciation or as a result of additional costs that needs to be borne in order to protect equipment from power disruptions. A natural phenomenon which produces power surges is lightning activity. In fact, one third of all power disruptions in the U.S. are related to lightning activity. We therefore hypothesize that higher lightning intensity increases the costs of using IT equipment and thus lowers the speed of Internet diffusion.

Using global satellite data assembled by the U.S. National Aeronautics and Space

[1] Technically, there is a distinction between the Internet and the World Wide Web (WWW). The latter was launched in 1991 by CERN (the European Organisation for Nuclear Research), whereas the history of "the Internet" arguably is much older. See Hobbes' Internet Timeline v8.2 <http://www.zakon.org/robert/internet/timeline/>. In this paper, we define the Internet/WWW as the network of networks using the TCP/IP/HTTP protocols, which was spawned by the launch of WWW.

[2] More than 80% of all E-government is Internet based (West, 2005). Moreover, in Britain, *Directgov*, an official Web page launced in 2004, aims to contain the entire British government in one place, <http://www.direct.gov.uk/en/index.htm>.

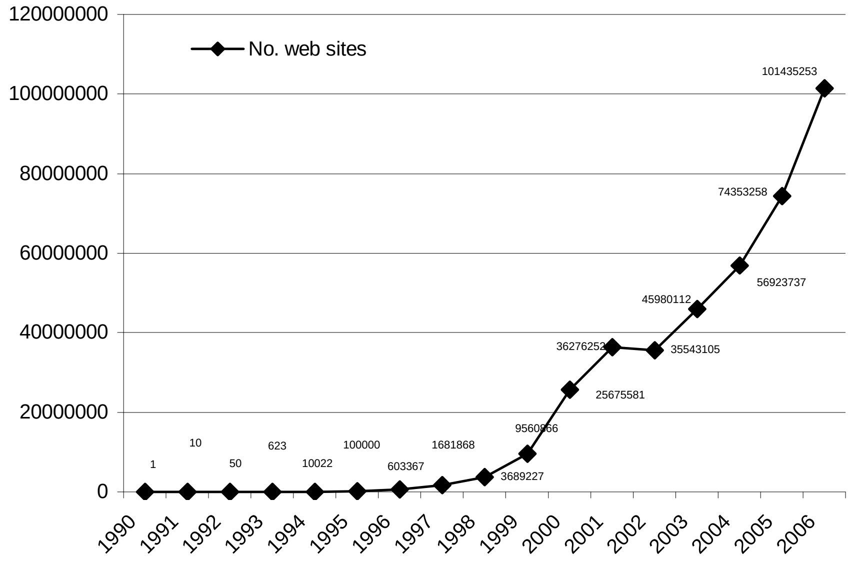

Figure 1: The figure shows the total number of Web sites on the Internet, 1990-2006. Generally, the numbers refer to December 1st in the individual year. *Source*: Hobbes' Internet Timeline v8.2 <http://www.zakon.org/robert/internet/timeline/>.

Administration (NASA) we construct country and state level measures of lightning density. In addition, for the U.S. we also employ lightning data from the U.S. National Lightning Detection Network's ground-based lightning detection censors. We then document, using cross-country data, that greater lightning density is associated with more frequent power disruptions per year, which in turn is associated with a slower speed of Internet diffusion. In addition, using U.S. data, we show that the reduced form relationship between lightning density and corruption does not exist prior to the inception of the WWW; it only exists from 1991 onwards. These falsification tests make probable that the lightning instrument meets the appropriate exclusion restriction: lightning affects corruption only through its impact on Internet penetration. If our instrument were capturing other determinants of corruption, one would expect to see a correlation between the two, both before and after the introduction of the WWW. Moreover, the falsification tests support a causal link between lightning density and the speed of Internet penetration, making the former a viable candidate instrument for the latter. Indeed, lightning density turns out to be a strong instrument for the speed of Internet diffusion across the U.S. states as well as across the world. Our 2SLS estimates confirm the OLS results: rising Internet use over the 1990s reduced corruption.

The present paper is related to the literature which studies the determinants of the level of corruption. Notable contributions include Ades and di Tella (1999), Treisman (2000), Brunetti and Weder (2003), Persson et al. (2003), Glaeser and Saks (2006), and Licht et al. (2007). Conceptually, Brunetti and Weder (2003) is perhaps the closest precursor to the present paper. The authors find a corruption-reducing impact of a free press, and argues that this shows that an independent press works to increase transparency. In the present case we expect the Internet to affect corruption for partly the same reason.[3]

The political economy literature, which studies the impact of information on governance more generally, is also related. This literature suggests that a better informed public serves to discipline the political establishment, thus affecting governance (e.g. Besley and Burgess, 2002; Strömberg, 2004; Reinikka and Svensson, 2004; Eisensee and Strömberg, 2007; Ferraz and Finan, 2008).

Finally, the paper is related to the literature which studies the determinants of the spread of the personal computer (e.g. Caselli and Coleman, 2001) and the Internet (e.g. Chinn and Fairlie, 2007) across countries. This literature has documented a positive impact of GDP per capita and the electricity infrastructure on Internet penetration, and of human capital levels on the adoption of computers. Consequently, the level of Internet penetration should *a priori* be viewed as endogenous.[4]

The paper is structured as follows. In Section 2, we provide some anecdotal evidence on the Internet's merits as an anti-corruption technology. In Section 3, we present our empirical specifications of choice. Section 4 outlines the identification strategy in detail; in particular, we explain how lightning activity impacts on digital equipment, and provides details on the data. Section 4 then provides an analysis of how the Internet has affected corruption across the U.S. states, whereas Section 5 provides cross-country evidence. Section 6 concludes. Extensive robustness testing has been relegated to appendices.

2 Anecdotal Evidence

There is by now an abundance of anecdotal evidence supporting the two aforementioned fundamental avenues through which the Web has been able to reduce corruption. In this section we provide some illustrative examples.

Turning first to the Internet's role as a major source of information, the New York Times (May 24, 2005) reports on the self-appointed Chinese investigative journalist, Li Xinde, who maintains an anti-corruption Web site targeting official corruption in China.[5] This journalist travels around China with a laptop and a digital camera inves-

[3]Evidence to the importance of mass media more broadly, but still in the context of combating corruption, is provided in the interesting study by McMillan and Zoido (2004).

[4]There is strong theoretical basis for believing that the initial adoption of new technologies is endogenous to governance. New technologies may create political as well as economic "losers", for which reason incumbent entrepreneurs and politicians may try to block adoption (Mokyr, 1990; Parente and Prescott, 1999; Acemoglu and Robinson, 2001). It seems plausible that places with widespread corruption, for example, may have adopted the Internet later, due to the influence of politicians, civil servants, or both. A positive impact from governance indicators, or income per capita, on the level of Internet users can be rationalized by this mechanism.

[5]"Death by a Thousand Blogs" by Nicholas D. Kristof. The Washington Post (May 2, 2007) also ran a

tigating official wrongdoing. He then writes about it on his Web site, making sure to leave town before local authorities can arrest him. The Web site has been instrumental in exposing a corruption case involving the deputy mayor of Jining, a large Chinese city. The site featured an investigative report and a series of photos showing the deputy mayor kneeling and crying, begging not to be reported to the police. The deputy mayor was subsequently arrested.[6]

In 2001 an Indian news Web site <www.Tehelka.com> nearly toppled the Indian government after documenting high-level corruption. The reporters posed as arms dealers and documented negotiations with top politicians and bureaucrats over the size of required side payments to get the order; in some instances the reporters even got the delivery of the bribe on camera. Consequently, numerous politicians and top officials had to resign, chief among them the defence minister George Fernandes.[7]

In 2004 judge John Gomery headed a commission investigating a kickback program in which the Canadian Liberal government had given millions to Montreal-based advertising firms. These funds were intended for publicizing certain government programs, but were instead funneled to political allies. While the hearings were public the judge banned the official media from publishing reports on what was being uncovered until a final ruling was made. The official media respected the ban but a Canadian blogger bypassed it by collaborating with a U.S. Web site <http://www.captainsquartersblog.com>.[8] The ensuing public resentment from what was disseminated through the Web site arguably cost the Liberals their parliamentary majority in the June 2004 election.[9]

Undoubtedly, cases like these are now familiar to politicians and civil servants across the world, which serves to lower the incentive to engage in shady dealings.

Turning next to the Internet's role as the chief vehicle for the provision of E-government, consider the OPEN (Online Procedures Enhancement for Civil Applications) system of Seoul Municipality, South Korea, a Web portal which allows citizens online (real-time) monitoring of applications for a variety of municipal licenses (Bhatnager, 2003). The system was initiated based on a recognition that extensive regulations, resulting from a large expansion of the municipal bureaucracy, had increased the scope for corruption. After declaring "war on corruption", OPEN was seen as one element of a broad range of activities aimed at lowering corruption. Besides the abovementioned online

(different) story featuring Li Xinde exposing corruption in China.

[6]Another fascinating story from China concerns the village of Shengyou in Hebei Province, which saw a clash between village peasants and numerous armed thugs sent by property developers to grab their land. A video smuggled out by one of the villagers shows his fellow residents being beaten with staves and shovels. Six villagers were killed and around 50 were wounded. With copies of the video circulating the Internet, the authorities reacted promptly: A mayor and a Communist Party chief of the municipality to which the village belongs were sacked. The official media reported that 22 people had been arrested, including the bosses of a firm contracted by a local state-owned power plant to build a waste-processing plant on the village fields. See "Turning ploughshares into staves", The Economist (June 23, 2005).

[7]See "The Sting That Has India Writhing" by Celia W. Dugger, The New York Times (March 16, 2001).

[8]See "Shivering Mr Martin's timbers", The Economist (April 7, 2004).

[9]See also "Did blogosphere influence vote? Corruption inquiry covered only on Web might have tipped Canadian election" by David Kopel, The Rocky Mountain News (January 28, 2006).

monitoring service, the portal explains the various elements of the anticorruption fight; it displays an anticorruption index; and it informs citizens on rules, regulations, and procedures.

In India, the celebrated "Bhoomi" program from the state of Karnataka represents an illustrative example of how E-government serves to limit the interface between civil servants and the public. Starting in 1998 the programme aimed to computerize land records; by now more than 20 million landholdings belonging to the state's 6.7 million landowners has been registered. Before online registration was available citizens had to seek out village accountants to register, a process which involved considerable delays and the need for bribes to be paid. With the online system there is no longer a need for the official "middlemen" (Bhatnager, 2003).

Practitioners such as Transparency International has argued in its 2003 Global Corruption Report that E-government offers a partial solution to the problem of corruption. It is widely seen as a tool for public sector system transformation, with the potential to transform governmental efficiency, transparency, citizen trust, and even political participation (West, 2008). While this potential has yet to be realized in full, governments do seem to undertake significant investments in setting up E-government systems. Indeed, and despite a dearth of hard data, West (2008) argues (based on an analysis of 1,667 national government Web sites in 198 countries) that 50% of all governments offer Web sites with services that are fully executable online, 96% offer access to publications, and 75% offer access to databases.

3 Specification

Since governance indicators, like the prevalence of corruption, tend to be persistent over time, empirical work on the determinants of corruption usually seeks to explain differences in corruption levels across U.S. states or across countries. In the present case, however, the inevitable choice of focus is on *changes* in corruption. The Internet is a recent phenomenon, and, as such, cannot have affected corruption levels prior to its inception, adoption and widespread use. Consequently, we study whether changes in Internet penetration can explain changes in corruption over the time-period during which the Internet has been in operation.

We will mainly rely on the following parsimonious specification:

$$DC_i = \alpha_0 + \alpha_1 DINTERNET_i + \alpha_2 C_{initial,i} + \varepsilon_i, \quad (1)$$

where DC_i is the change in corruption levels between an initial and a final year, $C_{final,i} - C_{initial,i}$ and $DINTERNET_i$ is the change in Internet penetration, $INTERNET_{final,i} - INTERNET_{initial,i}$. The inclusion of $C_{initial,i}$ makes explicit that changes in corruption almost inevitably is a function of the initial level. For example, a country with no corruption obviously cannot experience reductions in corruption levels. Notice that this also has the virtue of automatically controlling for (a potentially large set of) variables which may affect the level of corruption.

To see the latter point more clearly, observe that (1) is equivalent to the following levels regression with a lagged dependent variable:

$$C_{final,i} = \alpha_0 + \alpha_1 DINTERNET_i + (\alpha_2 + 1)\, C_{initial,i} + \varepsilon_i. \tag{2}$$

Accordingly, all time invariant structural characteristics affecting the *level* of corruption will be picked up by $C_{initial,i}$. This reduces the scope for omitted variable bias significantly.

Of course, while specification (1) reduces the likelihood of omitted variable bias, which translates into $\text{Cov}(DINTERNET_i, \varepsilon_i) \neq 0$, it does not rule it out. *Time-varying* characteristics may be omitted, causing $\text{Cov}(DINTERNET_i, \varepsilon_i) = 0$ to fail. Hence, to check the robustness of the link between changes in Internet penetration and changes in levels of corruption we also invoke specifications of the form:

$$DC_i = \alpha_0 + \alpha_1 DINTERNET_i + \alpha_2 C_{initial,i} + \mathbf{X}'_{initial,i}\boldsymbol{\alpha}_3 + \epsilon_i, \tag{3}$$

where **X** contains additional controls. Standard control variables will include the level of income per capita (or GDP per capita in the cross-country context) as well as the growth rate of the same; the latter in an effort to check for time-varying corruption determinants. When we study the U.S. sample, where we use an outcome based measure of corruption (total convictions), we will also include a measure of state size (i.e, state population) in the set of basic controls to ensure that all "scale effects" are pruned from the data. These controls are employed in both the OLS and 2SLS setting.

In addition to these controls we also examine the robustness of the partial correlation between $DINTERNET_i$ and changes in corruption to the inclusion of a much larger set of corruption determinants. It is worth stressing that adding regressors comes at a cost in terms of an increased risk of measurement error, which may bias all estimates. For this reason, we only use the expanded set of controls to study partial correlations; our robustness analysis documents that the partial correlation between $DINTERNET$ and DC is robust across countries and U.S. states.

In spite of these checks one may worry that the magnitude of the obtained estimate for α_1 could be misleading on account of $\text{Cov}(DINTERNET_i, \epsilon_i) \neq 0$. To address this concern we employ an IV approach. The next section describes our identification strategy in detail.

4 Identification

4.1 Theory

In order to explain changes in Internet use from the time of the Web's inception to present-day, one needs to explain IT investments. In this regard the user cost of IT capital plays a central role. A higher level of user cost implies a lower desired (long-run) IT capital stock, which *ceteris paribus* will imply lower IT investments during adjustment. By implication, the spread of the Internet will occur at a slower pace.

The identification strategy employed in the present paper is therefore to find exogenous determinants of the user costs associated with digital equipment in general, and therefore the Internet in particular. As is well known, the user cost of capital depends, among other things, on the rate of capital depreciation (Hall and Jorgenson, 1967). IT capital depreciation is one important avenue through which our instrument affects the Internet diffusion process.

Digital equipment, such as the computer, is highly sensitive to *power disruptions*. Such disruptions are likely to cause down-time, though sudden power surges may also damage the equipment and randomly destroy or alter data. In other words, power disruptions tend to increase the user cost of IT capital. A natural phenomenon which damages digital equipment, by producing power failures, is *lightning activity* (e.g., Shim et al., 2000 Ch. 2; Chisholm, 2000).

Perhaps surprisingly, the influence from lightning activity is far from trivial. The National Lightning Safety Institute, a nonprofit organization, reports that lightning in the United States alone accounted for more than 100,000 laptop and desktop computer losses in 1997.[10] NASA (2007) points out that the main impetus to lightning research in the late 1960s was the danger of lightning to aerospace vehicles and solid-state electronics used in computers and other electronic devices.[11] In order to understand why lightning is so problematic some remarks on the physics of power supply and distribution are required.

Power plants create commercial electric power with alternating voltage. Alternating voltage, $V(t)$, as a function of time, t, takes the form of a sinusoidal wave, i.e. $V(t) = V_0 \cdot \sin(2\pi \cdot f \cdot t)$, where f is the number of oscillations per second (f is usually expressed in hertz (Hz), where f Hz means f cycles per second). The time required for the pattern to be repeated is the period $T = 1/f$. Whenever this sinusoidal wave changes size, shape, frequency, develops notches, etc., technically, there is a power disturbance.

[10] <http://www.lightningsafety.com/nlsi_lls/nlsi_annual_usa_losses.htm>

[11] Solid-state electronics refers to an electronic device in which electricity flows through solid semiconductor crystals rather than through vacuum tubes. Transistors, made of one or more semiconductors, are at the heart of modern solid-state devices. In the case of integrated circuits, millions of transistors can be involved. Microprocessors are the most complicated integrated circuits. They are composed of millions of transistors that have been configured as thousands of individual digital circuits, each of which performs some specific logic function (see Kressel 2007 for an enjoyable discussion with a historical perspective).

It is important to appreciate that computers to this day remain extremely sensitive to such disturbances. For more than a century the reliability of the electricity grid has rested at 99.9%, or so-called "three nines" reliability. Three-nines reliability permits roughly 9 hours of outages per year. This was sufficient when the economy was built around light bulbs and electric motors. But microprocessor-based controls and computer networks demand at least 99.9999% reliability (or "six nines" reliability).[12] This amounts to only seconds of allowable outages per year.

The reason why such high reliability is required is that equipment based on solid-state electronics, such as computer chips, is constructed to work under conditions of a "clean" sinusoidal wave form. The power supply of a digital device converts the alternating current to direct current with a much reduced voltage. Digital processing of information works by having transistors "turn" this small voltage on and off at several gigahertz (Kressel, 2007). If the wave becomes disturbed or distorted, the conversion process can become corrupted. This may result in equipment failure. In fact, sensitivity to small distortions increases with the miniaturization of transistors, which is the key to increasing speed in microprocessors (Kressel, 2007). Voltage disturbances measuring less than one cycle are sufficient to crash servers, computers, and other microprocessor-based devices (Yeager and Stalhkopf, 2000; Electricity Power Research Institute, 2003). To put it differently, at a 60 Hz frequency (the standard in the U.S.) this means that a power disturbance of a duration less than 1/60th of a second is enough to crash a computer!

By some estimates, lightning is the direct cause of one third of all power quality disturbances in the United States (Chisholm and Cummins, 2006). Moreover, the probability of lightning-caused power interruptions or equipment damage scales linearly with lightning density (Chisholm, 2000; Chisholm and Cummins, 2006).[13] As a result, lightning-caused computer failure is a quite pervasive phenomenon.

Some of the most noted instances of lightning triggered computer failures relate to airports. For example, according to Germany's Federal Office for Information Security (Bundesamt für Sicherheit in der Informationstechnik, BSI), a major German airport experienced a lightning strike very close to the air traffic control tower. Despite the external lightning protection system that had been installed (a lightning conductor), the computerized fire extinguishing system in the IT area was triggered by the incidence, and, as a result, all airport operations were paralyzed for two hours (BSI, 2004). The New York Times (July 3, 1999) reports how air traffic across the northeastern United States was disrupted after lightning hit New England's main air traffic control center

[12]See for example National Energy Technology Laboratory (2003). Yeager and Stalhkopf (2000) actually argue that "nine nines" reliability is needed. See also "The power industry's quest for the high nines", The Economist (March 22, 2001).

[13]This linear scaling can be expressed precisely. Let N_S denote the number of strikes to a conducter per 100 *km* of power line length, h the average height (in meters) of the conducter above ground level, and GFD the ground flash density, then $N_S = 3.8 \cdot GFD \cdot h^{0.45}$ (see Chisholm, 2000).

in Nashua, N.H.[14] After the lightning strike the mainframe computer began reporting error messages to technicians, apparently because devices attached to it were not running properly.

In general, lightning discharges can enter electronic equipment inside a residence in four principal ways (IEEE, 2005). First, lightning can strike the network of power, phone, and cable television wiring. This network, particularly when elevated, acts as an effective collector of lightning surges. The wiring conducts the surges directly into the residence, and then to the connected equipment. In fact, the initial lightning impulse is so strong that equipment connected to cables up to 2 km away from the site of the strike can be damaged.[15] Second, when lightning strikes directly to or nearby air conditioners, satellite dishes, exterior lights, etc., the wiring of these devices can carry surges into the residence. Third, lightning may strike nearby objects such as trees, flagpoles, road signs, etc., which are not directly connected to the residence. When this happens, the lightning strike radiates a strong electromagnetic field, which can be picked up by the wiring in the building, producing large voltages that can damage equipment. Finally, lightning can strike directly into the structure of the building. This latter type of strike is extremely rare, even in areas with a high lightning density.

Accordingly, if computer equipment is left unprotected, high lightning intensity will work so as to increase the user cost of digital capital by elevating the rate of capital depreciation. Of course, steps may be taken to protect the equipment. A high-quality surge protector provides protection against voltage spikes, for example. High-tech companies install generators to supplement their power needs, thereby insuring themselves against power failure. They also add "uninterruptedly power sources" relying on batteries to power computers until generators kick in. These initiatives, however, will in any case increase the costs of acquiring digital equipment, and thereby the user cost of IT capital. The crux of the matter is that if one lives in an environment with a high annual lightning density, this adds to the costs of using modern electronics, including a computer.[16]

In the discussion above we have focused on the impact of power quality on the marginal costs of IT capital. But this is not the only way in which lightning may matter for Internet diffusion. In areas with frequent and prolonged power disruptions and outages, the marginal *benefit* of owning a computer is surely diminished. This mechanism

[14] "Lightning Hits Control Center And Grounds Many Planes" by Matthew L. Wald.

[15] According to BSI (2004, p. 258), with discharges of several hundred thousand volts, lightning strikes can achieve currents of up to 200,000 amperes. This enormous electrical energy is released and dies away within a period of 50-100 microseconds. A lightning strike of this order of magnitude originating from a distance of about 2 km will still cause voltage peaks that are capable of destroying sensitive electronic devices in the power lines of a building.

[16] Besides, the above mentioned protective devises are not necessarily enough to ensure against damage. According to the National Oceanic and Atmospheric Administration (NOAA), a typical surge protector will not protect equipment from a nearby lightning strike (see <http://www.lightningsafety.noaa.gov/indoors.htm>). Generators, in turn, do not react fast enough and can deliver dirty power; batteries are expensive to maintain and may also not react fast enough. See e.g., "The power industry's quest for the high nines", The Economist (March 22, 2001).

will also work to lower the speed of Internet diffusion.

Consequently, lightning matters to Internet diffusion in two ways: by inducing power disturbances, it increases the marginal costs as well as lowers the marginal benefits of IT capital. This is not to say that these two complementary channels necessarily are equally relevant everywhere. In the U.S. sample we would conjecture that the benefit channel is of secondary relevance; power outages and disruptions are likely of too short a duration to seriously impact on the perceived benefits from IT. In the cross-country sample, however, both the cost channel and the benefit channel may be relevant, especially so in poor countries. For this reason we would expect a stronger impact of lightning on Internet penetration in the cross-country context, compared to the U.S. sample.[17]

Against this background we propose lightning density as an instrument for the speed at which Internet use per capita changed over the period in question. Schematically we can express the theory in the following way

$$\text{LIGHTNING DENSITY} \longrightarrow \text{POWER DISTURBANCES} \longrightarrow \text{INTERNET USE}, \quad (4)$$

where the second arrow implicitly subsumes the impact of power disturbances on the costs and benefits of IT capital.

Lightning is certainly exogenous in a deep sense. However, this does not ensure validity of the exclusion restriction. Consider the following example: Climate-related circumstances may map into levels of corruption (indirectly capturing e.g. the resource curse mechanism), and lightning may be more pronounced in some climate zones compared to others. As mentioned in Section 3, however, the inclusion of $C_{initial}$ ensures that time *invariant* determinants of corruption are (implicitly) controlled for; the resource curse mechanism is therefore unlikely to pose a problem *vis-à-vis* the exclusion restriction. Rather, validity of the exclusion restriction requires that lightning has no direct impact on *changes* in corruption over the period under study, *conditional* on the initial level of corruption, $C_{initial}$, and the (initial) level of income per capita. Formally, let z denote the lightning density; validity requires $\text{Cov}(\epsilon, z) = 0$, where ϵ is the error term in (3).

Naturally, with only one instrument a formal test of the exclusion restriction is not feasible. However, informal tests are possible. Hence, in the analysis below we construct various falsification tests designed to shed light on the plausibility of the exclusion restriction.

[17]Some may instinctively believe that the marginal cost of protecting the computer (completely) from lightning also is negligible in the U.S. context. In this regard William A. Chisholm has suggested to us in personal correspondence that a laptop computer with battery and wireless Internet access may indeed be completely protected from lightning at a cost of about $200 extra. This is hardly a trivial expense. However, and more importantly, these measures will not protect the nearby cell-phone tower providing wireless infrastructure. The point being that the cost of wireless service will reflect the local providers' costs in replacing equipment when the tall tower is struck repeatedly by lightning. Hence, either way, lightning density will elevate the costs of being online.

4.2 Data on the Instrument: Lightning density

The raw data for flash densities (flashes per km^2 per year) comes from NASA. The Global Hydrology and Climate Center (GHCC) has designed, constructed, and deployed numerous types of groundbased, airborne, and spacebased sensors to detect lightning activity and to characterize the electrical behavior of thunderstorms as part of their research on atmospheric science. The GHCC's spacebased sensors detect all forms of lightning activity over land and sea 24 hours a day. Thus, such sensors allowed the development of the first global database of lightning activity, which has been used so far for severe storm detection and analysis, and for lightning-atmosphere interaction studies.

In this paper we rely on the data from the Optical Transient Detector (OTD), a space based sensor launched on April 3, 1995. For a period of roughly 5 years the satellite orbited Earth once every 100 minutes at an altitude of 740 km. At any given instant it viewed a 1300 $km \times 1300$ km region of Earth. "Flashes" were determined by comparing the luminance of adjoining frames of OTD optical data. When the difference was larger than a specified threshold value, an "event" was recorded.[18] These satellite-based data are archived and cataloged by the GHCC, where they are also made available, free of charge.[19]

We apply the data from a high-resolution (0.5 degree latitude $\times$ 0.5 degree longitude) grid of total lightning bulk production across the planet, expressed as a flash density, from the completed 5 year OTD mission.[20] Figure 2 provides a world map of the average flash density over the 5 years period.

We construct average flash densities for each country and U.S. state by first mapping the corresponding geographic areas into the lightning data grid and then taking the average of flash densities within each of these areas. The coordinates describing the areas are taken from the GEOnet Names Server (GNS) at the U.S. National Geospatial-Intelligence Agency's (NGA),[21] and the U.S. Board on Geographic Names' (U.S. BGN) database of foreign geographic names and features.[22] We used the GNS database released on October 7, 2008.[23] The GNS data covers the entire planet with the exception of the U.S. and the Antarctica. The area for the U.S. was estimated using geographic features for the 48 contiguous U.S. states, contained in the database released on August 15, 2008 by the Geographic Names Information System at the U.S. BGN.[24]

[18] Basically, these optical sensors use high-speed cameras designed to look for changes in the tops of clouds. By analyzing a narrow wavelength band (near-infrared region of the spectrum) they can spot brief lightning flashes even under daytime conditions.

[19] <http://thunder.msfc.nasa.gov/data/#OTD_DATA>

[20] Available at <ftp://microwave.nsstc.nasa.gov/pub/data/lightning-satellite/lis-otd-climatology/HRFC/LISOTD_HRFC_V2.2.hdf>

[21] <http://earth-info.nga.mil/gns/html/namefiles.htm>

[22] <http://geonames.usgs.gov/domestic/download_data.htm>

[23] Available at <ftp://ftp.nga.mil/pub2/gns_data/geonames_dd_dms_date_20081007.zip>

[24] Available at <http://geonames.usgs.gov/docs/stategaz/NationalFile_20080815.zip>

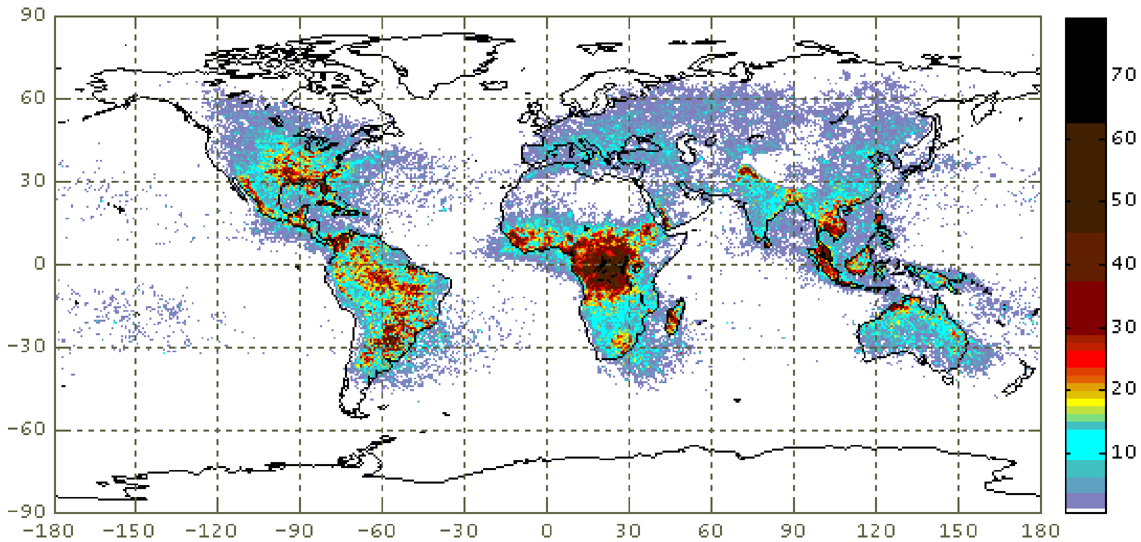

Figure 2: Average flash density (flashes per year per km^2). The figure is constructed using the OTD Global Lightning Distributions for the period April 12, 1995 to December 31, 1999.

More specifically, we follow four steps in order to calculate the average lightning density for each geographic entity: (*i*) we collect coordinates for geographic features that describe the complete land territory of each country; (*ii*) we use these coordinates to establish each feature's reference point on the lightning data grid; (*iii*) we select from these reference points the ones which identify uniquely the area covering each country on the grid; and (*iv*) we compute the simple average of lightning densities over each country's final selection of grid references.

A potential problem with the OTD data is that it only provides observations on *total* lightning events; i.e., intra-cloud, cloud-to-cloud, cloud-to-sky and cloud-to-ground lightning. In other words, OTD data does not separate out *cloud-to-ground* lightning incidences. The pertinent characteristic of lightning in the evaluation of risk to electronic equipment and electric power systems is the cloud-to-ground flash density. Fortunately, since the mid-1980's, it has been possible to measure ground flash density more directly using networks of electromagnetic sensors. Such Lightning Location Systems (LLS) are able to detect individual ground strikes with high spatial and temporal accuracy. However, many parts of the world, particularly the developing world, are not covered by the LLS data.[25] But accurate cloud-to-ground data does exist for the 48 contiguous U.S. states. These cloud-to-ground lightning flashes, which are measured by the U.S. National Lightning Detection Network (NLDN), are provided by Vaisala for the period 1996-2005.[26] NLDN consists of more than 100 remote, ground-based lightning sensors, which instantly detect the electromagnetic signals given off when

[25] In addition, data is not freely available for the parts of the world actually covered.

[26] See <http://www.vaisala.com/thunderstorm>

lightning strikes Earth's surface. Comparing the NLDN ground-based measures with the NASA satelite-based measures for the U.S. provides an indication of the extent to which the total amount of lightning is a reliable indicator of cloud-to-ground lightning.

Figure 3 provides a scatter plot of total lightning against cloud-to-ground lightning for the U.S. sample. Note that by definition total lightning should be at least as large as cloud-to-ground lightning, which is confirmed by the figure; all observations are below the 45 degree line.[27]

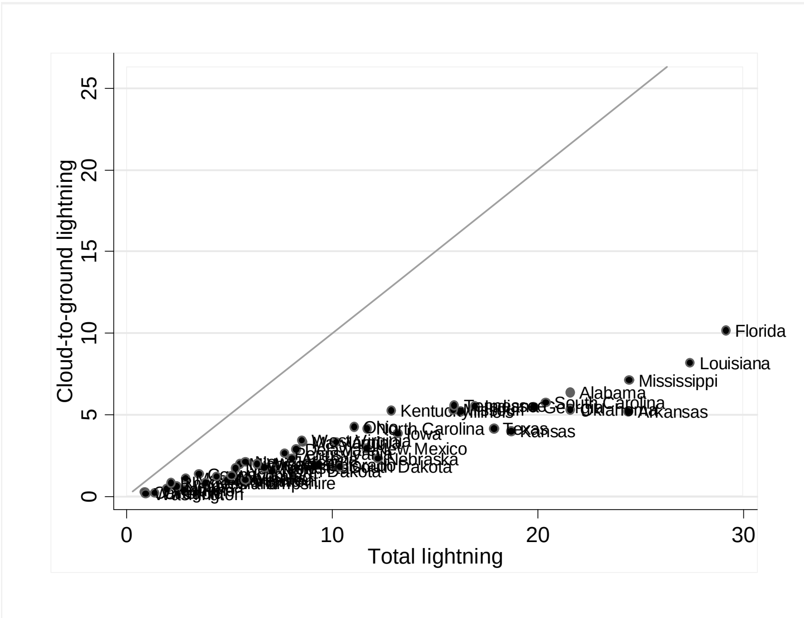

Figure 3: The figure shows a scatter plot of total lightning flash rate density (flashes per km^2 per year) based on the Optical Transient Detector high resolution data (horizontal axis) versus cloud-to-ground flash rate density (flashes per km^2 per year) based on the U.S. National Lightning Detection Network (vertical axis). The full line is the 45-degree line. Number of observations is 48.

Figure 3 shows that there is agreement between the two measures of lightning. With a correlation above 0.95, total lightning is a reasonable proxy for cloud-to-ground lightning in the contiguous U.S. states sample. This can also be expected to be the case in the cross-country data (Chisholm and Cummins, 2006).

5 Cross-State Evidence

The cross-state analysis proceeds as follows. In the next section we provide details on the remaining data used for the analysis. Then, in Section 5.2, we provide evidence

[27]NASA's flash densities of total lightning are calculated for the 1995-1999 period, while Vaisala's cloud-to-ground measures are calculated for the 1996-2005 period. In addition, Vaisala uses $mile^2$ as the area unit; these where converted into km^2 by dividing by the $mile^2$ numbers by 1.609^2.

on the partial correlation between changes in Internet usage and changes in corruption levels. Before we proceed to our 2SLS estimates in Section 5.4 we provide an independent check of the validity of our identification strategy in Section 5.3.[28]

5.1 Data

The corruption data derives from the Justice Department's "Report to Congress on the Activities and Operations of the Public Integrity Section". This publication provides the number of federal, state, and local public officials convicted of a corruption-related crime by state. As argued by Glaeser and Saks (2006), conviction levels capture the extent to which federal prosecutors have charged and convicted public officials for misconduct in each of the U.S. states. There are potential problems with using conviction rates to measure corruption: in corrupt places, the judicial system is itself likely to be corrupt, meaning that fewer people will be charged with corrupt practices. This problem, however, is diminished when focusing on federal convictions, the reason being that the federal judicial system is somewhat isolated from local corruption. Consequently, it should treat people similarly across states (Glaeser and Saks, 2006).

More specifically, the change in corruption convictions, DCC, is calculated as the difference between corruption convictions (CC) in 2006 and 1991:

$$DCC_i = \log(1 + CC_{2006,i}) - \log(1 + CC_{1991,i}). \quad (5)$$

The initial year follows from the fact that the Internet (in the sense of the World Wide Web) was invented (at CERN) in 1990. Positive values for DCC are interpreted as reflecting *increasing* corruption.[29]

The second key variable is Internet users, which we measure as the percentage of households with Internet access. It is based on data collected in a supplement to the October 2003 Current Population Survey (CPS), which included questions about computer and Internet use.[30] The CPS is a multi-stage probability sample with coverage in all states and the District of Columbia. The sample was selected from the 1990 Decennial Census files and is continually updated to account for new residential construction. To obtain the sample the United States is divided into 2,007 geographic areas, and about 60,000 occupied households are eligible for interviews.

Since U.S. corruption data goes back to 1991, the launch date of the WWW, we

[28] With the satellite-based lightning data we are able to include all 50 U.S. states, i.e. add Alaska and Hawaii. Results are very similar. Yet we stick to the 48 state sample because cloud-to-ground lightning data is only available for these 48 states.

[29] Note that an increased use of digital technology will both increase the risk of detection for a corrupt official (the detection technology is improved) as well as lower the incentive to commit corrupt acts. Hence, in theory, increased use of digital technology could increase the number of convictions if the former effect dominates. It might thus seem as if the Internet increases corruption. However, empirically the net effect is negative, implying that the "incentive effect" dominates, as documented below.

[30] Released October 27, 2005, by the U.S. Census Bureau. It is available at <http://www.census.gov/population/socdemo/computer/2003/tab01B.xls>.

define the change in Internet use by state population as

$$DINTERNET_i = INTERNET_{i,2003} - INTERNET_{i,1991} = INTERNET_{i,2003},$$

since $INTERNET_{i,1991} = 0$ for all i (i.e., for all states).

Finally, in some specifications we control for the initial level of income or the growth rate of (real) income per capita between 1991 and 2006. In these instances we employ data on "personal income per capita", which is taken from the State Personal Income Database from U.S. Bureau of Economic Analysis (BEA). Nominal income per capita is deflated using the implicit price deflator for personal consumption expenditures, index year 2000 (also from BEA). The natural log of income per capita in 1991 is denoted as $\log(YCAP_{1991})$; the growth rate is denoted $GYCAP$. Finally, we also control for initial state population, which also derives from BEA. The natural log of initial state population is denoted $\log(POP_{1991})$. Summary statistics are reported in Appendix A.

5.2 Partial Correlations

Table 1 documents the partial correlation between changes in Internet use and changes in corruption for the 48 contiguous U.S. states. The dependent variable is defined by equation (5).

Column 2 estimates equation (1). As is clear, $DINTERNET$ is estimated with high precision in the U.S. sample. With a t-value of 2.37 in column 2 significance holds at roughly the 2% level. The specification (1) accounts for almost 30% of the variation in DCC in the U.S. sample. Adding the growth rate of real income per capita, $GYCAP$, does not overturn the significance of changes in Internet use; neither does adding $\log(POP_{1991})$ and $\log(YCAP_{1991})$, cf. Column 4. Indeed, while significant at conventional levels, the inclusion of $\log(POP_{1991})$ and $\log(YCAP_{1991})$ does not (statistically) change the $DINTERNET$ point estimate. Hence, the remaining variation in DINTERNET, once we have partialled out the effect of initial corruption, is roughly orthogonal to the variation in income per capita and the variation in population size. This finding provides some assurance that a parsimonious specification such as (1) is appropriate for the U.S. sample. Overall, the results reported in Table 1 document a noteworthy partial association between Internet diffusion and changes in corruption convictions from 1991 onwards.

In order to check the robustness of the partial correlation between Internet diffusion and changes in corruption levels more thoroughly we have explored the entire list of determinants of corruption discussed in Maxwell and Winters (2005). They focus on four fundamental traits of U.S. states that should have predictable effects on corruption: (*i*) number of corruptible government bodies, (*ii*) the size of the state, (*iii*) socio-ethnic homogeneity, and, (*iv*) civic-minded and well-informed political cultures. In addition to proxies for these traits the authors consider seven additional control variables, which, besides real income growth, consists of metropolitan population, general tax revenue, direct initiatives, direct initiatives threshold, campaign expenditure restrictions, and finally, open party primaries.[31]

Table 1: Ordinary Least Squares on U.S. states

Dependent variable: DCC	(1)	(2)	(3)	(4)
DINTERNET		-0.058**	-0.058**	-0.089***
		(0.024)	(0.024)	(0.023)
$\log(1+CC_{1991})$	-0.387***	-0.386***	-0.418***	-0.904***
	(0.096)	(0.096)	(0.099)	(0.139)
GYCAP			-1.976	
			(2.218)	
$\log(POP_{1991})$				0.753***
				(0.143)
$\log(YCAP_{1991})$				2.322**
				(0.887)
CONSTANT	1.254***	4.392***	5.167***	-27.450***
	(0.276)	(1.312)	(1.597)	(7.964)
Observations	48	48	48	48
R-squared	0.20	0.29	0.30	0.59
F-test [H0: DINTERNET = -0.058, p-value]		1.000	0.99	0.168

Notes: Dependent variable is the change in log(1+number of corruption convictions), DCC, over the period 1991 to 2006. The explanatory variables are described in the main text. Robust standard errors are reported in parentheses. Asterisks ***,**,* indicate significance at the 1, 5, and 10% level, respectively.

In Appendix B, Table A.2 we report the results from introducing all of these 13 variables (i.e., the ones we have not already controlled for in Table 1) as ancillary controls in the basic specification, one at the time. Overall, the association between changes in Internet use and changes in corruption convictions is robust to the inclusion of the "Maxwell-Winters controls", with one exception: When the percentage of population with high school diploma is introduced into the regression, $DINTERNET$, as well as the high school variable itself, are found to be insignificant. Still, both variables are individually significant when the other variable is excluded, and they are jointly significant when they both enter the regression.

There are several ways to think about the latter result. A proximate explanation is of course multicollinearity. At a deeper level, however, one may speculate as to

[31] We refer to Maxwell and Winters (2005) for a detailed description of the data.

the interpretation of the collinearity of human capital and $DINTERNET$. The most obvious explanation is that technology diffusion occurs more rapidly within skilled populations. Consequently, human capital should be correlated with $DINTERNET$. If the level of human capital does not affect changes in corruption directly, the correlation between the former and the latter is spurious, reflecting the intervening influence from $DINTERNET$. However, it is not possible *a priori* to rule out a direct effect from skills on changes in corruption, which then taints the OLS estimate of $DINTERNET$ on changes in corruption levels. To make further progress we need to move beyond the OLS analysis.

5.3 Falsification Test

According to our identification strategy lightning only affects corruption through Internet penetration. This observation paves the way for a simple falsification test: If indeed lightning density affects changes in corruption levels due to its impact on power supply, IT investments, and thus Internet use, lightning density should be uncorrelated with changes in corruption *prior* to the inception and spread of the Internet. If lightning density *is* correlated with past changes in corruption levels the variable is likely capturing some other (omitted) determinant of corruption. If so, the validity of the lightning instrument should be questioned.

To examine whether this is the case we run the following regression:

$$DCC_i = \alpha_0 + \alpha_1 \log(LIGHTNING)_i + \alpha_2 CC_{initial,i} + X'_{initial,i}\alpha_3 + \varepsilon_i, \tag{6}$$

on two separate periods of time. First, we examine the sign and significance of α_1 on the period 1991-2006. We expect a positive and statistically significant estimate for α_1, capturing a slower spread of the Internet and thus smaller reductions in corruption, *ceteris paribus*. Second, we examine the sign and significance of α_1 on the period 1976-1990, where the initial year is a consequence of data availability on corruption convictions. Here we expect a numerically small and statistically insignificant estimate for α_1.

Table 2 reports results on the falsification test. As shown in columns 1-6 lightning does *not* influence changes in corruption prior to the inception of the WWW. This holds both for the period 1976-1990, where we have missing observations on corruption convictions for three states, and for the 1978-1990, where there are no missing observations. It is worth observing that the numerical size of the point estimate is close to zero; the non-rejection of $\alpha_1 = 0$ is not simply a matter of a more imprecisely estimated parameter.

Columns 7-9 show that lightning exerts a statistically significant influence on corruption after the inception of the WWW. Figures 4 and 5 provide a visual impression of these findings.

Table 2: Falsification test in U.S. sample

Period	1976-1990	1976-1990	1976-1990	1978-1990	1978-1990	1978-1990	1991-2006	1991-2006	1991-2006
Dependent variable: DCC	(1)	(2)	(3)	(4)	(5)	(5)	(7)	(8)	(9)
log(LIGHTNING)	-0.068	-0.027	-0.149	0.019	0.016	-0.036	0.344**	0.278**	0.303**
	(0.183)	(0.110)	(0.134)	(0.198)	(0.113)	(0.130)	(0.145)	(0.109)	(0.126)
$\log(1+CC_{initial})$	-0.388***	-0.954***	-0.941***	-0.352**	-0.930***	-0.939***	-0.452***	-0.916***	-0.926***
	(0.143)	(0.162)	(0.154)	(0.170)	(0.185)	(0.184)	(0.109)	(0.142)	(0.146)
$\log(POP_{initial})$		0.908***	0.983***		0.908***	0.954***		0.833***	0.815***
		(0.185)	(0.204)		(0.153)	(0.186)		(0.147)	(0.154)
$\log(YCAP_{initial})$			-1.794			-0.865			0.417
			(1.266)			(1.224)			(0.814)
CONSTANT	1.654***	11.22***	5.295	1.469***	-11.30	-3.400	0.787**	-10.72***	-14.67*
	(0.313)	(2.733)	(11.18)	(0.358)	(2.235)	(10.61)	(0.347)	(2.051)	(7.808)
Observations	45	45	45	48	48	48	48	48	48
R-squared	0.15	0.49	0.51	0.10	0.44	0.45	0.32	0.54	0.54

Notes: Dependent variable is the change in log(1+number of corruption convictions), DCC, over the stated period. The explanatory variables are described in the main text; and, in particular, log(LIGHTNING) is cloud-to-ground lightning. Robust standard errors are reported in parentheses. Asterisks ***,**,* indicate significance at the 1, 5, and 10% level, respectively.

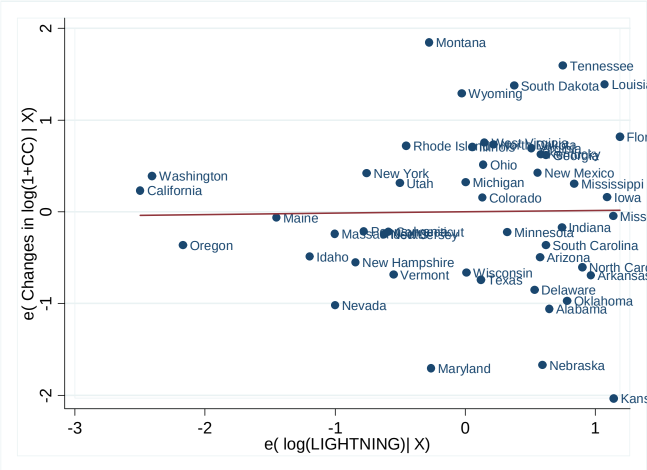

Figure 4: The figure shows the association between lightning and changes in corruption convictions over the 1978-1990 period, with the influence of income per capita and population partialled out.

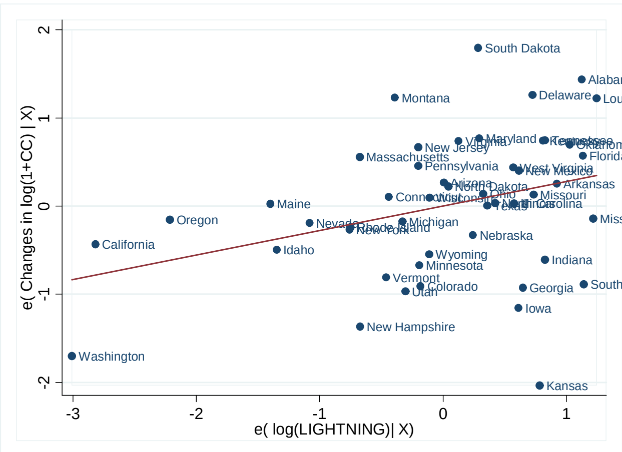

Figure 5: The figure shows the association between lightning and changes in corruption convictions over the 1991-2006 period, with the influence of income per capita and population partialled out.

These results suggests that lightning is *not* picking up omitted (time varying) determinants of corruption. If lightning were picking up time varying determinants of corruption (like human capital) it would be correlated with changes in corruption levels *both* before and after the inception of the WWW.

5.4 IV Estimates

The 2SLS results are reported in Table 3. Inspecting panel A we see that lightning density is negatively related to the diffusion of the Internet across U.S. states, in keeping with the theory underlying our identification strategy. This holds whether we rely on the accurate cloud-to-ground lightning density or, alternatively, the total lightning proxy. In column 3 we include $\log(POP_{1991})$ and $\log(YCAP_{1991})$. Importantly, as seen from panel A, lightning remains a strong instrument despite the inclusion of these additional controls.[32]

Since we are unable to reject homoscedasticity the usual rule-of-thumb for strength of instruments is informative. As the F-values are always above 30, our instruments of choice are strong (Staiger and Stock, 1997).

2SLS yield results that are broadly consistent with OLS. Indeed, testing the equality of OLS and 2SLS via the Hausman test does not allow rejection of the null that DINTERNET is exogenous. We are thus led to conclude that valid inferences can be drawn from OLS in the U.S. sample. Observe, however, that OLS tends to produce numerically smaller point estimates compared with 2SLS. In this sense the OLS results can be viewed as conservative estimates of the impact from the spread of the Internet on changes in corruption.

So what is the *economic* significance of Internet use in combating corruption? To answer this question we begin with the levels specification associated with (2):

$$\log(1 + CC_{2006}) = \alpha_0 + \alpha_1 DINTERNET + (\alpha_2 + 1)\log(1 + CC_{1991}).$$

If we linearize, treating CC_{1991} as a constant, the following simple approximation emerges:

$$\Delta CC_{2006} \simeq \alpha_1 (1 + CC_{2006})\, INTERNET_{2003}, \tag{7}$$

where we have used that $DINTERNET = INTERNET_{2003}$, cf. Section 5.1.

Next, consider the impact of changes in Internet use on corruption for the "typical" U.S. state. The typical U.S. state in terms of median level Internet use is Arizona ($CC_{2006} = 16$ and $INTERNET_{2003} = 55.3$). Suppose that Arizona is moved up to the third quartile in the distribution of Internet users in 2003; this is equivalent to an increase of 2.8 Internet users per 100 people. Using the OLS results reported in column 2 of Table 1 in equation (7) we find $\Delta CC_{2006} \simeq (-0.058) \cdot (1 + 16) \cdot (2.8) = -2.76$. That is, this increase in Internet penetration would reduce the number of corruption convictions by roughly 3 yearly convictions. Put differently, moving up 11 places in the ranking of Internet users (i.e. from the median to the third quartile) would move Arizona down two places in the U.S. state corruption convictions ranking (i.e. from the 58th percentile to the 55th percentile of the corruption convictions distribution in 2006).

[32] Using the total lightning proxy in column 3 provides similar results.

Table 3: Two-Stage Least Squares on U.S. sample

	(1)	(2)	(4)
Dependent variable: DINTERNET	Panel A: First-stage regression		
log(1+CC_{1991})	0.721	0.371	0.102
	(0.526)	(0.505)	(0.650)
log(LIGHTNING) (cloud-to-ground)	-3.801***		-2.384***
	(0.651)		(0.416)
log(LIGHTNING) (total)		-4.338***	
		(0.743)	
log(POP_{1991})			-0.968
			(0.695)
log($YCAP_{1991}$)			24.418***
			(4.276)
CONSTANT	59.554***	62.628***	-171.670***
	(1.378)	(1.684)	(39.576)
F (first-stage) value [H0: log(LIGHTNING) = 0]	34.11	34.10	32.85
Dependent variable: DCC	Panel B: Second-stage regression		
DINTERNET	-0.090**	-0.067*	-0.127**
	(0.036)	(0.037)	(0.051)
log(1+CC_{1991})	-0.386***	-0.386***	-0.914***
	(0.097)	(0.094)	(0.145)
log(POP_{1991})			0.692***
			(0.154)
log($YCAP_{1991}$)			3.518**
			(1.677)
CONSTANT	6.171***	4.918**	-36.459***
	(1.949)	(1.990)	(13.439)
Observations	48	48	48
R-squared	0.26	0.29	0.57
Hausman exogeneity test [H0: $\alpha_{2SLS} - \alpha_{OLS} = 0$, p-value]	0.452	0.797	0.953

Notes: Dependent variable is the change in log(1+number of corruption convictions), DCC, over the period 1996 to 2006. The explanatory variables are described in the main text. Cloud-to-ground lightning is measured using ground-based censors, whereas total lightning is measured via satellites. Robust standard errors are reported in parentheses. Asterisks ***,**,* indicate significance at the 1, 5, and 10% level, respectively.

6 Cross-Country Evidence

In this Section we examine whether the results obtained above appear to generalize to a cross-country sample. After providing details on the data used in the cross-country setting (beyond lightning density), we discuss the partial correlation between changes in corruption and changes in Internet users in Section 6.2. Section 6.3 examines the validity of our instrument, and Section 6.4 contains the 2SLS estimate of the impact of the Internet on corruption.

6.1 Data

Global corruption is measured using the well-known Control of Corruption Index (CCI) compiled by Kaufmann et al. (2007). The CCI measure, which ranges from -2.5 (worst) to 2.5 (best), is available biannually from 1996 to 2002 and annually from 2002 onwards.[33] The CCI indicator attempts to measure "the extent to which public power is exercised for private gain, including both petty and grand forms of corruption as well as capture by elites and private interests" (Kaufmann et al., 2007, p. 4).[34] The indicator is based on a large number of individual data sources, which are then aggregated into one measure by an unobserved components model. This means that the aggregate measure is a weighted average of the underlying individual data sources, with weights reflecting the precision of each of these underlying data sources; this makes the CCI the most comprehensive measure of corruption around.[35] Moreover, by virtue of being a solution to a statistical signal extraction problem, the aggregate CCI indicator is presumably more informative than any individual data source.[36]

The change in corruption levels is calculated as the difference between CCI in 2006 and 1996, i.e.

$$DCCI_i = CCI_{2006,i} - CCI_{1996,i}. \tag{8}$$

Observe that, by definition, positive values for DCCI_i means *less* corruption.

[33]These boundaries correspond to the 0.005 and 0.995 percentiles of the standard normal distribution. For a few countries, country ratings can exceed these boundaries when scores from individual data sources are particularly high or low (Kaufmann et al., 2007).

[34]Some are sceptical concerning the use of perception-based corruption data (Svensson, 2005). Interestingly, however, Olken (2006) has provided novel evidence on the relation between corruption perceptions and a direct measure of corruption in the context of villages across Indonesia. The empirical results show that villagers' perceptions of corruption do appear to be positively (albeit weakly) correlated with a direct "missing expenditure" measure.

[35]The widely reported Corruption Perception Index (CPI) compiled by Transparency International forms part of the CCI measure (see Kaufmann et al., 2007, Table A13). Reasuringly, the simple correlation between CCI and CPI is 0.97. A high correlation is not unexpected since corruption reflects the underlying institutional framework (Svensson, 2005).

[36]Svensson (2005), however, notes that the aggregation procedures used by Kaufmann et al. presumes that subindicator measurement errors are independent across sources. In reality, errors may be correlated since producers of different indices read the same reports and most likely each other's evaluations. If the assumption of independence is relaxed, the gain from aggregating a number of different reports is less clear.

Ideally, we would like to go back to 1991, the launch date of the WWW. Unfortunately, with the preferred CCI measure this is not feasible as 1996 is the earliest year for which the variable is available. However, to check robustness we also run regressions using the ICRG corruption indicator (from the International Country Risk Guide); the ICRG indicator allows us to go back to 1991.

Our key explanatory variable is the number of Internet users per 100 people. Increasingly, the number of Internet users is based on regular surveys. In situations where surveys are not available, an estimate can be derived based on the number of subscribers. Data is compiled by the International Telecommunication Union (ITU), and made available in the World Development Indicators (WDI) 2007. Since global corruption data goes back to 1996, we calculate the change in Internet users over this period:

$$DINTERNET_i = INTERNET_{i,2005} - INTERNET_{i,1996},$$

where 2005 is the most recent year available for Internet users in the WDI (2007). When we examine the period 1991-2006, using the ICRG indicator, we use the approximation that $INTERNET_{i,1991} = 0$, as in analysis of the U.S. states.

Real PPP-adjusted GDP per capita, $YCAP$, for the global sample is taken from WDI (2007). The growth rate of real GDP per capita, $GYCAP$, is calculated over the period 1996-2005 (respectively, 1991-2005), as 2005 is the most recent year with observations on PPP GDP per capita in the WDI (2007). The natural log of real GDP per capita in 1996 is denoted as $\log(YCAP_{1996})$.

Finally, for our cross-country falsification tests of our instrument, we examine each step of the theory underlying our identification strategy. That is, we test whether indeed lightning is associated with more frequent power disruptions, which in turn affects the speed of Internet diffusion within countries. In order to do so, we need data on the frequency of power failures.[37]

The cross-country measure of the frequency of power disruptions is collected by the World Bank, and it is available in WDI (2007) for the years 2002-2006. Specifically, the variable measures the average number of days per year that establishments experience power outages or surges from the public grid. We will use this variable as a proxy for the intensity of power disturbances. This variable, which we will refer to as $OUTAGES$, form part of the World Bank Group's Enterprise Surveys.[38] These surveys cover almost 58,000 firms in 97 countries for the period 2002–2006. For most countries electrical outages are measured only at one point in time during the period 2002-2006. However, for 26 countries there are two observations, one in 2002 and one in 2005. In these cases we use the 2005 values. Summary statistics are provided in Appendix A, Table A.1.

[37] Admittedly, we would have liked to perform these tests on the U.S. sampel as well. However, in spite of our best efforts, we have only been able to obtain data on power failures for a cross section of countries. At the same time, the falsification test employed on U.S. data is not possible in the cross-country setting due to a lack of data on corruption.

[38] <http://www.enterprisesurveys.org/>

6.2 Partial Correlations

Table 4 explores the partial correlation between changes in Internet use and changes in corruption for the cross-country data; the dependent variable is DCCI, defined by equation (8).

Table 4: Ordinary Least Squares on cross-country sample

Dependent variable: DCCI	(1)	(2)	(3)	(4)	(5)	(6)
DINTERNET		0.016***	0.016***	0.008**	0.018***	0.011***
		(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
CCI_{1996}	-0.070**	-0.287***	-0.290***	-0.415***	-0.295***	-0.436***
	(0.028)	(0.065)	(0.066)	(0.065)	(0.061)	(0.066)
GYCAP			0.199			
			(0.148)			
$\log(YCAP_{1996})$				0.279***		0.312***
				(0.062)		(0.071)
CONSTANT	0.001	-0.307***	-0.367***	-2.538***	-0.299***	-2.924***
	(0.043)	(0.087)	(0.091)	(0.509)	(0.096)	(0.593)
Regional dummies	NO	NO	NO	NO	YES	YES
Observations	113	113	105	108	113	108
R-squared	0.03	0.22	0.29	0.38	0.33	0.42
F-test (p-value):						
H0: DINTERNET = 0.016		1.000	0.980	0.0654	0.597	0.210

Notes: Dependent variable is the change in Control of Corruption, DCCI, over the period 1996 to 2006. The explanatory variables are described in the main text. Latin America & the Caribbean is the excluded regional category; the remaing regions are East Asia & Pacific (EAP), Europe & Central Asia (ECA), Latin America & the Caribbean (LAC), Middle East & North Africa (MENA), North America (NA), South Asia (SOA), Sub-Saharan Africa (SSA).Robust standard errors are reported in parentheses. Asterisks ***,**,* indicate significance at the 1, 5, and 10% level, respectively.

Column 2 estimates the parsimonious specification (1), described in Section 3. The sign of DINTERNET is positive, indicating more "control of corruption" (i.e., *less* corruption). Moreover, $DINTERNET$ is estimated with high precision. With a t-value above 4 significance is well below the 1% level. Including the growth rate of real GDP per capita has no impact on the slope estimate associated with $DINTERNET$, cf. column 3. In fact, the growth rate of real GDP per capita holds no explanatory power once we control for initial corruption and changes in Internet use. When we include the log of GDP per capita, $DINTERNET$ remains significant albeit numerically somewhat smaller. Regional dummies does not change the results appreciably either. In fact, the null that the $DINTERNET$ slope estimates in all columns equal 0.016 cannot be rejected at the five percent level. Hence, statistically speaking, adding the log of GDP per capita in 1996 and/or regional dummies makes no difference to the results. This means that the remaining variation in $DINTERNET$, once we have partialled out the effects of initial corruption, is roughly orthogonal to the variation in GDP per capita and/or regional fixed effects. In other words, the positive (partial) association between changes in Internet use and changes in corruption is not driven by omitted factors such as the level of economic development.[39] Consequently, a parsimonious specification such as (1) appears to be appropriate in the cross-country sample. These results are very similar to what we found for the U.S. sample.

Table 5: Ordinary Least Squares on cross-country subsamples

Dep. variable: DCCI	(1)	(2)	(3)	(4)	(5)	(6)	(7)
DINTERNET	0.013***	0.016***	0.016***	0.015***	0.019***	0.016***	0.016**
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.006)
CCI_{1996}	-0.235***	-0.285***	-0.286***	-0.275***	-0.339***	-0.301***	-0.291***
	(0.064)	(0.066)	(0.065)	(0.065)	(0.063)	(0.069)	(0.106)
CONSTANT	-0.221**	-0.305***	-0.306***	-0.334***	-0.334***	-0.325***	-0.317**
	(0.085)	(0.088)	(0.090)	(0.090)	(0.090)	(0.099)	(0.122)
Observations	91	112	110	100	100	90	75
Excluded region	SSA	NA	SOA	MENA	EAP	LAC	ECA
R-squared	0.17	0.22	0.22	0.24	0.28	0.26	0.16
F-test (p-value):							
H0: DINTERNET = 0.016	0.389	0.995	0.990	0.877	0.385	0.895	0.984

Notes: Dependent variable is the change in Control of Corruption, DCCI, over the period 1996 to 2006. The regions are: East Asia & Pacific (EAP), Europe & Central Asia (ECA), Latin America & the Caribbean (LAC), Middle East & North Africa (MENA), North America (NA), South Asia (SOA), Sub-Saharan Africa (SSA). Robust standard errors are reported in parentheses. Asterisks ***, **, * indicate significance at the 1, 5, and 10% level, respectively. The F-test tests the null that the slope estimate of DINTERNET associated with each subsample is equal to the slope estimate associated with the full sample, i.e. column 2 in Table 4.

Naturally, one may still worry about the robustness of the partial correlation. Hence, as a first check we re-estimate the basic regression model omitting each region of the world, one by one. The results are found in Table 5.

As can be inferred from the table the point estimate for $DINTERNET$ is remarkably robust. The last row in Table 5 performs an F-test, where the null is that the slope estimate of $DINTERNET$ associated with each sub-sample is equal to the slope estimate associated with the full sample, i.e. 0.016. The null cannot be rejected at any conventional level of significance.

As another robustness check we have examined the partial correlation between

[39] This is perhaps not surprising as the correlation between CCI_{1996} and $YCAP_{1996}$ is as high as 0.8.

$DCCI$ and $DINTERNET$ while controlling for the set of corruption predictors discussed in a recent survey by Treisman (2007). Specifically, we check robustness to the inclusion of 28 additional corruption determinants, covering (in the terminology of Treisman) three broad categories of determinants: "historical and cultural controls"; "political controls"; and "rents and competition controls". Overall we find that the estimate of $DINTERNET$ is unchanged, once again suggesting the parsimonious specification (1) is appropriate. The details can be found in Appendix C.

Finally, we have examined an alternative corruption indicator, the ICRG index, which also allows us to consider the entire period 1991-2006. The results once again support a strong partial correlation between changes in Internet penetration and changes in corruption levels. The results are found in Appedix D, Table A.6, columns 1-2.

6.3 Falsification Test

The type of falsification test adopted in the U.S. sample is infeasible for the cross-country sample. The reason is that corruption indicators are not available sufficiently far back in time. Hence, in this section we provide an alternative falsification test, which entails checking each element in the causal chain, cf. (4), underlying our identification strategy.[40] While we argue these links are highly plausible on physical grounds, it does not follow that they are quantitatively relevant in an economic context. Moreover, if lightning is uncorrelated with power failures but nevertheless seem to correlate with changes in Internet use, one may question the identification strategy; lightning may then be picking up the influence from other (omitted) corruption determinants.

To proceed, we express the underlying logic of the identification strategy as a system of equations:

$$\log(OUTAGES_i) = \beta_0 + \beta_1 \log(LIGHTNING_i) + \mathbf{X}_O'\gamma_O + v_i, \quad (9)$$
$$DINTERNET_i = \alpha_0 + \alpha_1 \log(OUTAGES_i) + \mathbf{X}_I'\gamma_I + u_i \quad (10)$$

where $\mathbf{X}_O$ and $\mathbf{X}_I$ contain additional controls. The basic question is whether $\beta_1 > 0$ and $\alpha_1 < 0$.

We will run regressions (9) and (10) with and without controls. In the former case $\mathbf{X}_O$ and $\mathbf{X}_I$ equal CCI_{1996} and $\log(YCAP_{1996})$. In principle, a variety of variables could be included in $\mathbf{X}_O$ and $\mathbf{X}_I$. The choice of including initial corruption and initial income per capita as additional controls is made for two reasons. First, governance indicators have previously been shown to affect energy efficiency (Frederikson et al., 2004) as well as Internet use (e.g., Chinn and Fairlie, 2006). Second, and more important in the present context, by controlling for both initial corruption and initial income per capita, these specifications will have direct bearing on the validity of the 2SLS regressions

[40] As noted above, this type of test cannot be performed on U.S. data since the required data on power failures is not available.

in the next section: The identification strategy (implicitly) requires lightning to affect outages, and outages to affect Internet use *conditional* on CCI_{1996} and $\log(YCAP_{1996})$. Consequently, this is the setting where confidence in the validity of the identification strategy, involving lightning as an instrument for changes in Internet use, is required.

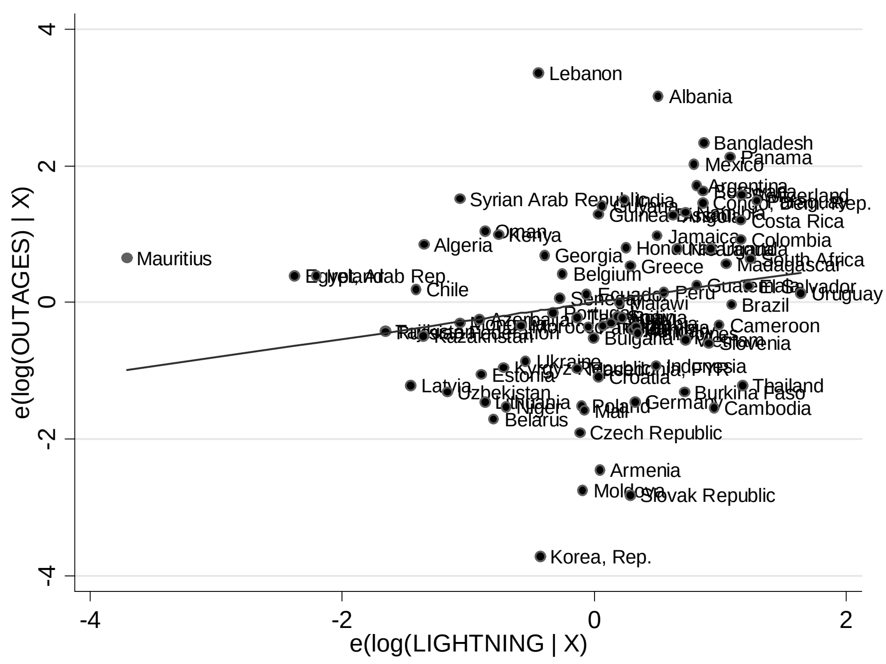

Figure 6: The figure shows the relationship between average lightning density and electrical outages, with the influence of initial corruption and initial income per capita partialled out.

Table 6 reports the results from estimating equations (9) (columns 1-3) and (10) (columns 4-6). In column 1 we report the bivariate association between lightning density and electrical outages and surges. The correlation is both highly significant and positive as required. Moreover, lightning can account for some 12% of the global variation in electrical outages. In column 2 we add CCI_{1996}. The magnitude of the point estimate for lightning declines but remains significant at the 5% level. Adding $\log(YCAP_{1996})$ changes nothing substantially; in particular, lightning now has a *p*-value of 0.053, i.e. is significant at the 10% level.

Figure 6 provides a visual impression of the partial association between $\log(LIGHTNING)$ and $\log(OUTAGES)$, cf. column 3, Table 6. Taking the point estimate at face value, the results imply that an increase in lightning density of one percent increases the number of days where power outages take place by about 0.2-0.3%.

Turning to column 4 of Table 6, we find that a higher intensity of electric power failures hampers the spread of the Internet within countries. Indeed, electrical outages and surges can account for nearly 60% of the total variation in the Internet variable.

Table 6: Falsification test in cross-country sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	log(OUTAGES)	log(OUTAGES)	log(OUTAGES)	DINTERNET	DINTERNET	DINTERNET	DINTERNET	DINTERNET
log(OUTAGES)				-6.219***	-5.284***	-4.132***	-5.194***	4.003***
				(0.768)	(1.007)	(1.180)	(1.018)	(1.202)
log(LIGHTNING)	0.504***	0.350**	0.267*				-0.683	-0.838
	(0.162)	(0.166)	(0.136)				(1.384)	(1.391)
CCI_{1996}		-0.727***	0.154		5.001***	2.199	4.662**	1.751
		(0.193)	(0.210)		(1.744)	(1.628)	(1.984)	(1.967)
log($YCAP_{1996}$)			-1.108***			5.105***		5.169***
			(0.188)			(1.439)		(1.469)
CONSTANT	1.200***	1.165***	10.68***	28.04***	26.79***	-18.63	27.82***	-17.95
	(0.366)	(0.339)	(1.600)	(2.139)	(2.277)	(13.46)	(3.245)	(13.34)
Observations	93	86	84	70	67	66	67	66
R-Squared	0.12	0.24	0.45	0.57	0.61	0.65	0.62	0.66

Notes: Dependent variable in columns (1) to (3) is the log of outages; the dependent variable in columns (4) to (8) is the change in Internet use over the period 1996 to 2006. All variables are described in the main text. Robust standard errors are reported in parentheses. Asterisks ***,**,* indicate significance at the 1, 5, and 10% level, respectively.

Adding CCI_{1996} (column 5) does not change the impression of a first-order impact from outages to the spread of the Internet. In fact, outages appear to have a quantitatively stronger effect on the spread of the Internet than corruption. Specifically, increasing $\log(OUTAGES)$ by one standard-deviation is associated with a reduction in $DINTERNET$ of -0.61 standard-deviation units; in contrast, a similar exercise using CCI_{1996} would increase $DINTERNET$ by 0.29 standard-deviation units.

Adding $\log(YCAP_{1996})$ in column 6 does not change anything qualitatively. Figure 7 provides a visual impression of the strength of the partial association between power failures and changes in Internet use.

Finally, in columns 7 and 8 we add $\log(LIGHTNING)$ as an explanatory variable in equation (10). If lightning density affects the spread of the Internet in ways *unrelated* to electrical outages, we would expect it to be significant conditional on the inclusion of $\log(OUTAGES)$. If so, one may worry that lightning density is proxying for other variables affecting Internet diffusion, which would raise doubts concerning the validity of the exclusion restriction maintained in the next section. Indeed, this amounts to a falsification test of the identification strategy. Fortunately, the instrument passes the test: lightning is not a significant determinant of changes in Internet use over the period in question, *conditional* on the inclusion of power failures. This finding renders probable that the statistical correlation between $\log(LIGHTNING)$ and $DINTERNET$, which we document in the next section, arises due to lightning's influence on the frequency of power failures, which in turn affects the spread of the Internet within countries.

6.4 IV Estimates

Turning now to instrumental variables methods, Table 7 reports 2SLS-based results for the cross-country sample. Panel A of Table 7 provides results from the first-stage regressions in the 2SLS procedure. In accordance with the logic of the identification strategy, the lightning density is negatively related to the diffusion of the Internet in column 1. In column 2, where we have also included the logarithm of real GDP per capita, the lightning instrument turns insignificant; or, more appropriately, the instrument is weak.

The association between average lightning density and the change in Internet use in column 1, once the influence of initial corruption is partialled out, is close to the "rule-of-thumb" value suggested by Staiger and Stock (1997). Specifically, if the F-value associated with the null of zero explanatory power of the instrument is above ten we need not concern ourselves with issues of weak identification: inference based on 2SLS estimates will not be plagued with size distortions. This rule-of-thumb, however, requires that the error variance is homoskedastic (see assumption M, part (c) in Staiger and Stock, 1997). While this assumption is fulfilled in the second-stage regression (cf. Pagan-Hall test), it fails in the first-stage regression (cf. Breusch-Pagan/Cook-Weisberg test). Consequently, while inspection of panel A reveals the F-value is "close" to ten in

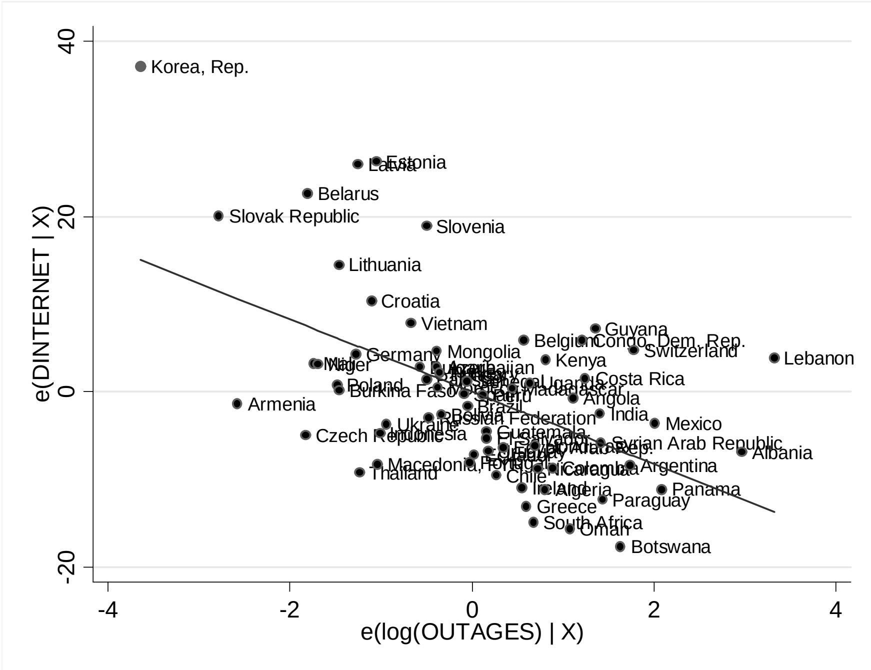

Figure 7: The figure shows the relationship between electrical outages and changes in Internet use, with the influence of initial corruption and initial income per capita partialled out.

column 1, with non-constant error variance it is not entirely clear what this means for the actual size of significance tests.

Turning to panel B, we see that 2SLS yields results that are broadly consistent with OLS, cf. Table 4 and 5. $DINTERNET$ remains significant at 5% in column 1; the slope estimate is roughly twice the size of the corresponding OLS estimates but standard errors also more than double. A formal test of the equality of OLS and 2SLS is the Hausman test. This test, which is reported in panel B in the table, does not allow rejection of the null that $DINTERNET$ is exogenous. Put differently, we do not detect any systematic difference between OLS and 2SLS coefficient estimates. However, if our 2SLS estimates are weakly identified the overall properties of the test are somewhat unclear.

We address the problem of potential size distortions by invoking a method proposed by Chernozhukov and Hansen (2008), which allows us to construct confidence intervals with the correct size regardless of the strength of instruments.[41] Moreover, Chernozhukov and Hansen show how these size-adjusted confidence intervals can easily be made robust to non-spherical disturbances. Finally, the procedure has good

[41] These confidence intervals are weak-identification robust since information about the (partial) correlation between instruments and the endogenous variable is not used.

power properties.

Table 7: Two-Stage Least Squares on cross-country sample

	(1)	(2)
Dependent variable: DINTERNET	Panel A: First-stage regression	
CCI_{1996}	11.21***	4.983***
	(1.428)	(1.629)
$\log(YCAP_{1996})$		9.507***
		(1.127)
log(LIGHTNING)	-3.602***	-2.086
	(1.290)	(1.333)
CONSTANT	25.59***	-58.27***
	(2.634)	(9.889)
F (first-stage) value [H0: log(LIGHTNING) = 0]	7.800	2.450
Dependent variable: DCCI	Panel B: Second-stage regression	
DINTERNET	0.032***	0.042*
	(0.012)	(0.022)
CCI_{1996}	-0.515***	-0.622***
	(0.172)	(0.161)
$\log(YCAP_{1996})$		-0.053
		(0.224)
CONSTANT	-0.682***	-0.346
	(0.227)	(1.499)
Observations	113	108
95% Weak-identification robust confidence intervals for DINTERNET	[0.013, 0.087]	
90% Weak-identification robust confidence intervals for DINTERNET		[0.005, ∞]
Hausman exogeneity test [H0: $\alpha_{2SLS} - \alpha_{OLS} = 0$, p-value]	0.201	0.215

Notes: Dependent variable is the change in Control of Corruption, DCCI, over the period 1996 to 2006. The explanatory variables are described in the main text. Robust standard errors are reported in parentheses. Asterisks ***,**,* indicate significance at the 1, 5, and 10% level, respectively.

Panel B reports these size-adjusted confidence intervals. As is evident from the table, 95% size-adjusted confidence interval deems $DINTERNET$ above zero in column 1. The midpoint of the confidence interval is 0.05, which is above both OLS (0.016) and 2SLS (0.032) estimates. In column 2, where $\log(YCAP_{1996})$ is included and where we face a severe identification problem, the 90% size-adjusted confidence interval provides a sharp lower bound, which is strictly larger than zero; but the confidence interval is upwardly unbounded, i.e. data are not informative about the upper bound.

In Appendix C, Table A6, columns 3 and 4 we perform the above analysis using the

ICRG measure of corruption, which allows us to examine the entire period 1991-2006. The results are statistically stronger than those reported in Table 7, but the basic message is the same. Using OLS to address the economic impact of Internet use on changes in corruption levels is not overly misleading in the cross-country sample. However, OLS will likely provide conservative estimates, as both 2SLS and one midpoint of size-adjusted confidence interval is numerically larger than the OLS estimate.

A final issue concerns economic significance. In the cross-country sample we rely on the levels specification (2), i.e.

$$CCI_{2006} = \alpha_0 + \alpha_1 DINTERNET + (\alpha_2 + 1)\, CCI_{1996}.$$

Using the results from OLS reported in column 2 of Table 4, the effect of an increase in $INTERNET_{2005}$ *holding the initial level of Internet users constant* will be considered. In this case, we have that

$$\Delta CCI_{2006} \simeq \alpha_1 \Delta INTERNET_{2005}. \tag{11}$$

The "typical" country in the cross-country sample is Morocco ($CCI_{2006} = -0.060$ and $INTERNET_{2005} = 15.25$). How would Morocco's corruption ranking change if we increase the number of Internet users in 2005 from the median to the third quartile of the world distribution? This amounts to increasing $INTERNET$ from 15 in one hundred to 35 in one hundred (i.e. to the level of Spain). Inserting in (11) gives $\Delta CCI_{2006} \simeq (0.016) \cdot 20 = 0.32$. That is, it would increase the CCI_{2006} score for Morocco from -0.060 to 0.26. Put differently, moving from the median to the third quartile in terms of Internet users would move the corruption score from the median to roughly the 62nd percentile, which happens to be the level of Italy. While being economically important, the effect does not seem implausible large.

Overall, the results from the cross-section analysis are very similar to the results obtained for the U.S. sample. Internet diffusion is robustly correlated with reductions in the level of corruption and the 2SLS estimates of the impact of the Internet on corruption suggest the association is causal. Moreover, both samples support a simple parsimonious specification; statistically, the 2SLS estimates are equivalent to those obtained by OLS. In sum, these results lend support to the proposition that the Internet has worked so as to lower corruption from 1991 onwards.

7 Concluding remarks

The present analysis renders probable that the Internet is a powerful anti-corruption technology. Indeed, the Internet facilitates the dissemination of information about corrupt behavior, making it more risky for bureaucrats and politicians to take bribes; it also obviates the need for potentially corrupt officials to serve as middlemen between the government and the public; and it has allowed for more transparency in the context of public procurement.

The correlation between changes in Internet penetration and changes in corruption, conditional on the initial level of corruption, is persistent: It holds across the world at large, and across different sub-samples of the world, using best available corruption perception indices as dependent variables; it also holds across U.S. states, using corruption convictions.

In order to examine whether Internet penetration has had a causal impact on corruption, the paper develops a new instrument for the user cost of digital equipment such as computers, and thereby for Internet diffusion. Digital equipment is highly sensitive to power disruptions; and, to a considerable extent, lightning activity causes power disruption around the world. Indeed, according to some calculations, lightning causes some 17,000 computers around the world to crash each second (Yeager and Stahlkopf, 2000).

Using data based on ground-based censors and global satellite data, we construct state and country level measures of average lightning density. Using cross-country data, we document that lightning is highly correlated with electrical outages, and that the latter is associated with a slower pace of Internet penetration. Across the U.S. we show that lightning is correlated with changes in corruption after the emergence of the WWW, but uncorrelated with changes in corruption *prior* to its invention. These findings support the use of lightning density as an instrument for Internet diffusion. Empirically, lightning density turns out to be a strong instrument for Internet diffusion, especially in the U.S. sample. Our 2SLS estimates document that the spread of the Internet has lowered corruption, both in the U.S. and worldwide.

We are unable to reject that the spread of the Internet is exogenous in our specifications, though OLS estimates are considerably lower than the 2SLS counterparts. However, despite remaining conservative, by using OLS estimates in our subsequent calculations, we find the impact of the Internet on corruption to be economically substantial. Accordingly, to the extent that corruption affects economic growth, these findings provide one mechanism by which the Internet may work to spur growth. In this way, our findings also provide a new perspective on the importance of the well-documented global "digital divide".

The identification strategy developed in this paper may prove useful in future research. By providing a strong instrument for Internet use, researchers may be able to make new progress on the impact of computers and the Internet on other outcomes, like the return to education or productivity growth more broadly.

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A Summary Statistics

The summary statistics for the cross-country sample is found in panel A, whereas the similar data for the U.S. is found in panel B.

Table A1: Summary statistics

	Obs.	Mean	Median	Std. dev.	Min	Max
	Panel A: Cross-country sample					
CCI_{2006}	113	0.105	-0.251	1.080	-1.767	2.573
CCI_{1996}	113	0.112	-0.169	1.057	-2.084	2.297
$INTERNET_{2005}$	113	22.56	15.24	21.66	0.208	86.94
$INTERNET_{1996}$	113	1.541	0.196	3.216	0	18.26
GYCAP	105	0.264	0.189	0.287	-0.351	1.416
$YCAP_{1996}$,1000 $	108	9.454	5.980	8.855	0.070	37.76
Average lightning density	113	8.999	6.590	8.617	0.018	44.38
Outages	93	27.50	9.600	46.60	0.040	248.96
	Panel B: U.S. sample					
CC_{2006}	48	20.15	9.50	21.48	0.00	83.00
CC_{1991}	48	13.44	6.00	21.14	0.00	108.00
$INTERNET_{2003}$	48	54.39	55.00	5.88	39.50	65.50
GYCAP	48	0.363	0.357	0.056	0.265	0.580
$YCAP_{1991}$, 1000 $	48	22.53	22.10	3.32	16.43	31.78
POP_{1991}	48	52.22	36.46	56.16	4.59	305.00
Average lightning density	48	10.54	8.58	7.31	0.89	27.34

B Robustness: Additional controls, US sample

Table A.2. Ordinary Least Squares: Additional controls from Maxwell and Winthers (2004)

Dep. variable: DCC	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
DINTERNET	-0.089***	-0.088***	-0.090***	-0.089***	-0.081***	-0.093***	-0.072**	-0.038	-0.088***	-0.090***	-0.102***	-0.089***	-0.096***	-0.084***
	(0.023)	(0.023)	(0.023)	(0.022)	(0.022)	(0.026)	(0.028)	(0.037)	(0.022)	(0.023)	(0.025)	(0.023)	(0.023)	(0.022)
$\log(1+CC_{1991})$	-0.904***	-0.914***	-0.906***	-0.932***	-0.888***	-0.898***	-0.938***	-0.987***	-0.877***	-0.905***	-0.888***	-0.907***	-0.908***	-0.905***
	(0.139)	(0.138)	(0.143)	(0.145)	(0.146)	(0.145)	(0.142)	(0.147)	(0.152)	(0.146)	(0.138)	(0.151)	(0.141)	(0.138)
$\log(POP_{1991})$	0.753***	0.861***	0.732***	0.973***	0.709***	0.747***	0.726***	0.753***	0.648***	0.750***	0.727***	0.758***	0.690***	0.787***
	(0.143)	(0.181)	(0.222)	(0.239)	(0.162)	(0.150)	(0.151)	(0.146)	(0.199)	(0.153)	(0.145)	(0.147)	(0.149)	(0.137)
$\log(YCAP_{1991})$	2.322**	2.260**	2.315**	2.140**	1.603	2.172**	1.920**	2.283***	1.647	2.349**	2.879***	2.284**	2.482***	1.683*
	(0.887)	(0.845)	(0.890)	(0.883)	(0.982)	(1.054)	(0.850)	(0.780)	(1.099)	(1.119)	(1.033)	(0.886)	(0.825)	(0.848)
# corruptible gov. bodies		-0.116												
		(0.091)												
Size of the state			0.005											
			(0.026)											
Very small states				0.427										
				(0.507)										
Socio-ethnic homogeneity					-0.00022									
					(0.00019)									
% college graduates						1.146								
						(4.278)								
Civic involvement							-0.191							
							(0.184)							
% high school diploma								-6.698						
								(4.070)						

Table A.2: Continued

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Metropolitan population									0.008					
									(0.010)					
Tax revenue per capita										-0.00001				
										(0.00025)				
Direct initiatives											0.306			
											(0.261)			
Direct initiatives threshold												0.044		
												(0.269)		
Campaign expenditure restr.													0.454**	
													(0.191)	
Open primaries														0.359
														(0.222)
Constant	-27.45***	-28.31***	-27.09***	-29.13***	-19.38*	-25.96**	-23.49***	-24.59***	-19.75*	-27.65***	-32.08***	-27.17***	-27.82***	-22.04***
	(7.964)	(7.481)	(8.087)	(8.405)	(10.118)	(9.999)	(7.898)	(7.095)	(11.161)	(9.340)	(9.016)	(7.985)	(7.257)	(7.496)
Observations	48	48	48	48	48	48	48	48	48	48	48	48	48	48
R-squared	0.586	0.604	0.586	0.592	0.594	0.587	0.596	0.615	0.592	0.586	0.599	0.586	0.61	0.609
F-test (p-val):	1	0.951	0.992	0.976	0.695	0.901	0.534	0.17	0.96	0.993	0.623	0.991	0.766	0.821
[H0: DINTERNET = - 08939]														

Notes: Dependent variable is the change in log(1+number of corruption convictions), DCC, over the period 1996 to 2006. The explanatory variables are described in the main text and Appendix A.2. Robust standard errors are reported in parentheses. Asterisks ***,**,* indicate significance at the 1, 5, and 10% level, respectively.

C Robustness: Additional controls, Cross-Country sample

In this appendix we conduct a "kitchen sink" type robustness check of all OLS results in the cross-country sample relying on the control variables used in Treisman (2007). Specifically, we include these variables in the specification (3), one at a time, in order to check whether the partial association between changes in Internet use and changes in corruption is influenced by any of these "popular" predictors of corruption.

Treisman (2007) relies on three sets of variables: a set of "historical and cultural controls"; a set of "political controls"; and finally a set of "rents and competition controls".

The set of historical and cultural controls include: percentage Protestants, percentage Catholics, percentage Muslims, British legal origin, French legal origin, German legal origin, Scandinavian legal origin, former British colony, former French colony, and finally former colony of other power except Spain or Portugal.[42] As is evident from Table A.3, the association between changes in Internet use and changes in corruption is robust to the inclusion of these controls in the sense of never turning insignificant. In fact, the point estimate of DINTERNET remains statistically unaffected, cf. *F*-test in Table A.3.

Treisman's political controls[43] include: political rights, democratic since 1950, press freedom, newspaper circulation, presidential democracy, pure plurality system, open-list system, district magnitude, and fiscal decentralization. As is evident from Table A.4, with the exception of newspaper circulation, the association between changes in Internet use and changes in corruption remains is robust to the inclusion of these controls, in the sense of never turning insignificant. Once again, the point estimate of DINTERNET remains statistically unaffected. When newspapers circulation is included, however, both this variable and DINTERNET are insignificant. Yet both variables are significant in this 95-country sample when the other variable is excluded, and they are jointly significant. Put differently, the insignificance of DINTERNET is due to multicollinearity.

Finally, the set of rents and competition controls include: fuel exports, imports, year opened to trade, time required to open a firm, women in government, and the log of the standard deviation of inflation. Inspection of Table A.5 reveals that, with the exception of time required to open firm and the log of the standard deviation of inflation, the association between changes in Internet use and changes in corruption is robust to the inclusion of these controls. When time required to open firm is included, DINTERNET turns insignificant. This is not due to multicollinearity; it is due to the large reduction in sample size, from 108 (with time required to open firm *excluded*) to 69 (with time required to open firm *included*). DINTERNET is insignificant in the 69-country sample

[42]For more information on exact definitions of the variables used in Appendix A.1, we refer to Treisman (2007).

[43]Treisman appears to do a double-counting by treating the percentage of Protestants both as a political and a historical-cultural control. We include it only as a historical-cultural control.

even with time required to open firm excluded, i.e. when estimating column 1 in the reduced sample (point est. 0.006, std. error 0.004). More importantly, time required to open firm is surely affected by corruption (corruption causes longer time required to open firm); but it is in no way clear why it should affect corruption (longer time required to open firm is not likely to cause corruption), in which case it should not be included as a control. These two factors in conjunction, i.e. reduced sample size and endogenous right-hand side variable, implies that we should not attach too much importance to the results in column 5, Table A.5. Finally, DINTERNET is also insignificant in column 8, where the log of the standard deviation of inflation is included. However, Treisman does not use all available information on inflation variability. Once we update his variable using the log of the standard deviation of CPI (log of the standard deviation of the GDP deflator), we can expand our country coverage from 79 to 105 (108). In both cases, DINTERNET turns significant, cf. columns 9-10.

Overall, the results from Tables A.3-A.5 confirm the robustness and stability of the partial association between changes in Internet use and changes in corruption in the cross-country sample.

Table A.3. Ordinary Least Squares: Additional ("historical and cultural") controls from Treisman (2007)

Dependent variable: DCCI	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
DINTERNET	0.008**	0.006*	0.008*	0.009**	0.009**	0.008*	0.010**	0.008*	0.009**	0.009**	0.009**
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
CCI_{1996}	-0.415***	-0.469***	-0.429***	-0.415***	-0.418***	-0.415***	-0.408***	-0.442***	-0.424***	-0.418***	-0.424***
	(0.065)	(0.062)	(0.064)	(0.065)	(0.069)	(0.067)	(0.068)	(0.066)	(0.066)	(0.066)	(0.070)
$\log(YCAP_{1996})$	0.279***	0.313***	0.310***	0.278***	0.276***	0.285***	0.271***	0.291***	0.275***	0.284***	0.277***
	(0.062)	(0.063)	(0.060)	(0.061)	(0.063)	(0.066)	(0.063)	(0.065)	(0.059)	(0.066)	(0.062)
Protestants		0.006***									
		(0.002)									
Catholics			-0.002*								
			(0.001)								
Muslims				0.001							
				(0.001)							
British legal origin					0.075						
					(0.093)						
French legal origin						-0.08					
						(0.079)					
German legal origin							-0.166				
							(0.164)				
Scandinavian legal origin								0.399***			
								(0.142)			

Table A.3. Continued

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
British colony									0.158*		
									(0.091)		
French colony										0.023	
										(0.082)	
Other colony											-0.05
											(0.088)
Constant	-2.538***	-2.856***	-2.733***	-2.563***	-2.539***	-2.536***	-2.485***	-2.639***	-2.548***	-2.586***	-2.526***
	(0.509)	(0.515)	(0.492)	(0.501)	(0.524)	(0.527)	(0.519)	(0.528)	(0.486)	(0.550)	(0.504)
Observations	108	106	107	107	103	103	103	103	107	108	108
R-squared	0.375	0.43	0.392	0.381	0.372	0.374	0.373	0.396	0.394	0.376	0.377
F-test (p-val):	0.999	0.547	0.833	0.917	0.865	0.904	0.755	0.859	0.908	0.99	0.885
[H0: DINTERNET = .00848]											

Dependent variable is the change in Control of Corruption, DCCI, over the period 1996 to 2006. The explanatory variables are described in the main text and in Appendix A.1. Robust standard errors are reported in parentheses. Asterisks ***, **, * indicate significance at the 1, 5, and 10% level, respectively.

Table A.4: Ordinary Least Squares: Additional ("political") controls from Treisman (2007)

Dependent variable: DCCI	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
DINTERNET	0.008^{**}	0.008^{**}	0.008^{**}	0.007^{*}	0.005	0.010^{**}	0.010^{**}	0.008^{**}	0.010^{**}	0.013^{***}
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
CCI_{1996}	-0.415^{***}	-0.438^{***}	-0.455^{***}	-0.476^{***}	-0.365^{***}	-0.363^{***}	-0.390^{***}	-0.388^{***}	-0.401^{***}	-0.318^{***}
	(0.065)	(0.067)	(0.075)	(0.067)	(0.064)	(0.068)	(0.070)	(0.063)	(0.069)	(0.084)
$\log(YCAP_{1996})$	0.279^{***}	0.263^{***}	0.281^{***}	0.268^{***}	0.236^{***}	0.205^{***}	0.231^{***}	0.233^{***}	0.236^{***}	0.159
	(0.062)	(0.065)	(0.063)	(0.063)	(0.065)	(0.065)	(0.070)	(0.070)	(0.064)	(0.108)
Political rights		-0.032								
		(0.024)								
Democratic since 1950			0.167							
			(0.124)							
Press freedom				-0.005^{**}						
				(0.002)						
Newspaper circulation					4×10^{-5}					
					(5×10^{-5})					
Presidential democracy						-0.037				
						(0.122)				
Pure plurality system							-0.006			
							(0.092)			
Open-list system								0.263^{*}		
								(0.154)		

Table A.4: Continued

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
District magnitude									-0.002**	
									(0.001)	
Fiscal decentralization										5×10^{-5}
										(0.003)
Constant	-2.538***	-2.294***	-2.577***	-2.186***	-2.172***	-1.933***	-2.168***	-2.184***	-2.204***	-1.666*
	(0.509)	(0.549)	(0.517)	(0.539)	(0.525)	(0.558)	(0.575)	(0.582)	(0.527)	(0.907)
Observations	108	107	107	107	95	76	97	96	98	52
R-squared	0.375	0.387	0.387	0.416	0.345	0.332	0.337	0.357	0.359	0.411
F-test	0.999	0.914	0.88	0.712	0.442	0.729	0.771	0.933	0.676	0.296
[H0: DINTERNET = .00848 p-val]										

Dependent variable is the change in Control of Corruption, DCCI, over the period 1996 to 2006. The explanatory variables are described in the main text and in Appendix A.1. Robust standard errors are reported in parentheses. Asterisks ***, **, * indicate significance at the 1, 5, and 10% level, respectively.

Table A.5. Ordinary Least Squares: Additional controls ("rents and competition controls") from Treisman (2007)

Dependent variable: DCCI	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
DINTERNET	0.008^{**}	0.010^{**}	0.008^{**}	0.009^{**}	0.004	0.008^{**}	0.009^{**}	0.007	0.008^{**}	0.008^{**}
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
CCI_{1996}	-0.415^{***}	-0.408^{***}	-0.402^{***}	-0.394^{***}	-0.290^{***}	-0.449^{***}	-0.451^{***}	-0.444^{***}	-0.380^{***}	-0.409^{***}
	(0.065)	(0.080)	(0.065)	(0.077)	(0.069)	(0.067)	(0.067)	(0.073)	(0.066)	(0.067)
$\log(YCAP_{1996})$	0.279^{***}	0.251^{***}	0.268^{***}	0.175^{***}	0.149^{**}	0.285^{***}	0.271^{***}	0.265^{***}	0.246^{***}	0.280^{***}
	(0.062)	(0.080)	(0.063)	(0.065)	(0.057)	(0.065)	(0.064)	(0.063)	(0.061)	(0.063)
Fuel exports		0								
		(0.001)								
Imports			0.001							
			(0.001)							
Year opened to trade				-0.004						
				(0.004)						
Time required to open firm					-0.125^{***}					
					(0.039)					
Women in lower house of parliament						0.008				
						(0.005)				
Women in government at ministerial level							0.008^{**}			
							(0.004)			
log(SD of inflation)								-0.056		
								(0.057)		
log(SD of CPI)									0.039	
									(0.224)	
log(SD of GDP deflator)										0.076
										(0.156)

Table A.5: Continued

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Constant	-2.538***	-2.342***	-2.493***	-1.384**	-0.898*	-2.672***	-2.617***	-2.293***	-2.249***	-2.556***
	(0.509)	(0.668)	(0.525)	(0.657)	(0.516)	(0.545)	(0.529)	(0.518)	(0.495)	(0.514)
Observations	108	82	104	95	69	107	88	79	105	108
F-test [H0: DINTERNET = .00848 p-val]	0.999	0.76	0.939	0.857	0.23	0.797	0.925	0.743	0.906	0.967
R-squared	0.375	0.357	0.377	0.335	0.29	0.392	0.419	0.415	0.336	0.376

Dependent variable is the change in Control of Corruption, DCCI, over the period 1996 to 2006. The explanatory variables are described in the main text and in Appendix A.1. Robust standard errors are reported in parentheses. Asterisks ***, **, * indicate significance at the 1, 5, and 10% level, respectively.

D Robustness: Alternative corruption measure, Cross-Country sample

Table A.6. Alternative corruption indicator (ICRG)

Dependent variable: DICRG	(1)	(2)	(3)	(4)
	OLS		2SLS	
$INTERNET_{2005}$	0.036***	0.031***	0.043***	0.054**
	(0.006)	(0.007)	(0.009)	(0.021)
$ICRG_{1991}$	-0.747***	-0.748***	-0.820***	-0.835***
	(0.088)	(0.096)	(0.114)	(0.125)
$\log(YCAP_{1991})$		0.108		-0.196
		(0.123)		(0.284)
CONSTANT	0.981***	0.179	1.079***	2.576
	(0.231)	(0.881)	(0.244)	(2.238)
Observations	102	97	102	97
F (first-stage) value [H0: log(LIGHTNING) = 0]			25.08	6.466
Hausman exogeneity test [H0: $\alpha_{2SLS} - \alpha_{OLS} = 0$, p-value]			0.620	0.582

Notes: The dependent variable is the change in the ICRG's index over the period 1991 to 2005, DICRG. The explanatory variables are described in the main text. Asterisks ***, **, * indicate significance at the 1, 5, and 10% level, respectively.

The Impact of Aid on Bureaucratic Quality: Does the Mode of Delivery Matter?*

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Abstract

We show that the impact of foreign aid on bureaucratic quality in recipient countries varies with the mode of delivery. Specifically, grants are found to impair the functioning of the bureaucracy, whereas loans are not. The negative impact of grants is larger when they are given as budget support rather than as assistance for specific projects or for programs in general.

Keywords: Foreign aid, bureaucratic quality.
JEL classification: F35.

1 Introduction

Donors try to contribute to better institutions in developing countries through substantial technical support, putting a particular emphasis on enabling local bureaucracies to improve their performance and standards. In theory, aid has the potential to raise bureaucratic quality, as it can release governments of binding revenue constraints, enabling them for instance to pay higher salaries to civil servants. Yet, a variety of factors created by the aid process itself, such as the provision of technical assistance that inhibits the recipient government's own capacity and initiative, or the donor fragmentation caused by the presence of multiple agencies and development agendas (Knack and Rahman, 2007), can become serious obstacles to the development of better local bureaucracies.

Surprisingly, only a limited number of studies have analyzed empirically development aid's capacity to promote better bureaucracies. For a sample of 34 African countries, Bräutigam and Knack (2004) find that high levels of aid are associated with declines in the overall quality of governance. Covering a broader cross-section of aid recipients, Knack and Rahman (2007) identify a negative effect of donor fragmentation on bureaucratic quality.

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In this paper we present an empirical assessment of the effects of aid on bureaucratic quality across countries, aiming to contribute to the literature in two respects. First, we focus on an important characteristic of development aid in recent years, namely, the increasing degree in discretion in the use of funds with which donors disburse aid.[1] Second, we focus on disaggregated flows of aid rather than on a single aid aggregate, and thus take explicitly into account the fact that some forms of aid are more likely to affect governance than others.

Our main findings suggest that the impact of foreign aid on bureaucratic quality in recipient countries varies with the mode of delivery. Specifically, grants are found to impair the functioning of the bureaucracy, whereas loans are not. We also find that the negative impact of grants is larger when they are given as budget support rather than as assistance for specific projects or for programs in general.

The remainder of the paper is organized as follows. The data and the method of estimation are discussed in Section 2. Section 3 presents the results, and Section 4 some concluding remarks.

2 Data and Method

Our dependent variable is the average level of bureaucratic quality. We take as a proxy the respective index from the PRS Group's International Country Risk Guide (ICRG).[2] This index is intended to measure institutional strength and the extent to which the bureaucracy tends to minimize revisions of policy when governments change. It gives countries a score ranging from 0 to 4 according to the overall level of bureaucratic quality, as perceived by the population, and measured by a number of independent surveys.[3]

Aid data are taken from the OECD's Creditor Reporting System (CRS).[4] The data refer to actual aid disbursements. Disbursements are to be preferred over aid commitments as the behavior of recipients is more likely to respond to actual transfers of resources rather than to donors' promises. We distinguish between program and project aid, based on the OECD's Development Assistance Committee (DAC) sector codes for aid allocation. Program aid consists of funds for "general budget support", "developmental food aid", "other commodity assistance", and "action related to debt". Project aid comprises investments in social and economic infrastructure, as well as aid

[1]This is many times justified on grounds of giving more substance to the idea of aid *ownership* in recipient countries (see e.g. DFID, 2005

[2]Available at , https://www.prsgroup.com/ICRG.aspx.

[3]Governance indicators are in general imperfect measures; see for example Kaufman and Kray (2008), for a survey and discussion of the challenges remaining in relation to their construction; but also of the characteristics that make them informative and useful for empirical research.

[4]See Appendix A1 for precise definitions and sources of the variables used in the regressions, and Appendix A2 for summary statistics.

to production sectors such as agriculture.[5]

Our analysis covers the years 1995-2005, which coincides with the period in which donors started to increasingly emphasize the crucial importance of aid *ownership* and the quality of governance for development, and the period for which we have access to data on disaggregated aid disbursements from the OECD.

Our basic econometric specification is

$$\Delta bureau_{i,95-05} = \alpha + \beta_0 bureau_{i,95} + \beta_1 \overline{aid}^{t,p}_{i,95-05} + \varepsilon_i, \qquad (1)$$

where $\Delta bureau_{i,95-05}$ is the change in the level of bureaucratic quality in country i over the period 1995–2005, $bureau_{i,95}$ is the initial level of bureaucratic quality, $\overline{aid}^{t,p}_{i,95-05}$ is the average level of type t aid received for purpose p, where $t \in$ {grants, loans}, and $p \in$ {project aid, program aid, budget support}; and ε_i is a zero-mean error term.

This specification reduces to an important extent problems related to omitted variables. Including $bureau_{i,95}$ as a regressor in equation (1) helps to control for a potentially large set of historical slow moving factors explaining differences in the level of bureaucratic quality (like ethnic fractionalization or natural resource endowments, for example); and factors such as unobservable dimensions of culture which might be difficult to account for directly.[6] Controlling for $bureau_{i,95}$ also helps to account for the fact that the variation in average levels of bureaucratic quality across countries depends to a large extent on idiosyncratic initial conditions. Indeed, as shown in Table 1 for the case of total aid, various proxies for institutions, resource endowments, religion, ethno-linguistic fractionalization and geographical location turn out to be insignificant when introduced jointly with $bureau_{i,95}$. [7]

We extend equation (1) to include $\mathbf{X}_{i,95-05}$, a vector of time varying covariates of bureaucratic quality to reduce the number of potentially confounding factors in the identification of β_1:

$$\Delta bureau_{i,95-05} = \alpha + \beta_0 bureau_{i,95} + \beta_1 \overline{aid}^{t,p}_{i,95-05} + \boldsymbol{\beta_2} \mathbf{X}_{i,95-05} + \varepsilon_i. \qquad (2)$$

In our preferred specification, the vector $\mathbf{X}_{i,95-05}$ is represented by the initial level of real GDP per capita. Other regressors suggested in the literature, such as the number

[5]For details concerning the sector codes, see the Appendix.

[6]Notice that we could also rewrite equation (1) as $bureau_{i,05} = \alpha + (\beta - 1) bureau_{i,95} + \beta_1 \overline{aid}^{t,p}_{i,95-05} + \varepsilon_i$, which highlights that all predetermined characteristics of the average level of bureaucratic quality during the period of analysis are controlled for. An alternative strategy to try to account for the effect of individual predetermined confounders (especially time invariant and slow moving unobservable characteristics) is to run the regression with yearly data in first differences or with the variables transformed to deviations from their means. However, this type of strategy would lead to heavily downward-biased estimates, given the high persistence of the level of bureaucratic quality during the period of analysis.

[7]By focusing on changes in bureaucratic quality, regression (1) basically explains variation within countries. Running a regression in averages, $\overline{bureau}_{i,95-05} = \alpha + \beta_1 \overline{aid}^{t,p}_{i,95-05} + \varepsilon_i$, would help to explain variation *between* countries. Estimates based on this approach confirm our results (not shown here, but available on request).

of conflicts the government is involved in (Bräutigam and Knack 2004) and the initial level of human capital (here proxied by enrolment in tertiary education), are not found to have an independent impact on bureaucratic quality. Table 1 shows that the basic negative correlation between bureaucratic quality and total aid, is robust to this type of controls; and also to controlling for of a number of other possible covariates of bureaucratic quality, like socio-demographic conditions (proxied by population size, and measures of the religious landscape); and general geographic conditions (proxied by the percentage of tropical area, latitude of the country; and the initial endowment of natural resources).

The first option to estimate equation (2) is OLS. However, an OLS regression may not allow us to identify β_1 as a causal effect of aid on bureaucratic quality, since causality between aid and bureaucratic quality can run in both directions (see, e.g., Alesina and Weder, 2002, who argue that more corrupt countries receive more aid). To account for this, we estimate equation (2) in a 2SLS framework, using the initial level of population and the mortality rate for children under 5 years as instruments for our aid variables.

Population size is a promising instrument for aid, as Easterly (2009) notes, based on the observation that "there is an exogenous small country bias in aid such that smaller countries get higher aid per capita and higher aid as a ratio to their income" (Easterly, 2009, p. 388). Such a relationship clearly exists, but population size as a regressor does not satisfy the exclusion restriction immediately, since it is possible that it affects directly the dynamics of bureaucratic quality.[8] However, in a recent study, Knack and Azfar (2003) show that the theoretical relationship between country size and the quality of governance is ambiguous, and that the empirical relationship between population and corruption (key feature of bureaucratic quality) is feeble, and highly sensitive to sample selection bias.[9] Therefore, conditional on the initial level of bureaucratic quality and income per capita, our choice of population size as an instrument for aid appears to satisfy the conceptual requirements of relevance and validity.

We supplement population size in the set of instruments, with the rate of mortality for children under 5 years. We make this choice for various reasons. First, population size itself might not capture entirely the idea of allocating aid according to recipients' current needs (which is the approach to aid delivery that the international development community has emphatically been pursuing during the period we study); and

[8]There are a number of studies arguing, for example, that small and more homogeneous populations make good governance easier, see for example Alesina and Spolaore (1998).

[9]Knack and Azfar (2003) argue, in particular, that historical indexes trying to measure corruption cover mostly large nations (corrupt or not), and some of the small nations which are probably less corrupt. (They refer to Transparency International's indexes for the years in the mid-1990s, where about 50 countries, with a median population between 27 and 31 million, were surveyed, *cf* Knack and Azfar, 2003, Table 1.) As coverage of countries increased and corruption and governance indicators started to include several sources of data and information, smaller and more corrupt countries started to appear in the overall sample. This basically explains why the correlation we tend to find now between corruption and population size is much weaker than the positive correlation it is possible to find using old governance indicators.

Table 1: Total Aid and Bureaucratic Quality, OLS

Dependent variable: Δ Bureauc. Quality, 1995-2005	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Bureaucratic Quality, 1995	-0.60***	-0.71***	-0.66***	-0.67***	-0.60***	-0.59***	-0.64***	-0.60***	-0.61***	-0.61***	-0.64***	-0.65***	-0.82***
	[0.091]	[0.089]	[0.088]	[0.096]	[0.093]	[0.092]	[0.098]	[0.10]	[0.094]	[0.094]	[0.095]	[0.11]	[0.094]
Aid/GDP, av. 1995-2005	-0.056***	-0.039***	-0.044***	-0.058***	-0.056***	-0.056***	-0.044***	-0.056***	-0.055***	-0.054***	-0.054***	-0.067***	-0.033**
	[0.012]	[0.012]	[0.012]	[0.012]	[0.012]	[0.012]	[0.014]	[0.013]	[0.012]	[0.012]	[0.013]	[0.014]	[0.015]
Real GDP per capita, 1995		0.044***											0.063**
		[0.012]											[0.025]
School enrolment, 1995			0.011*										0.0027
			[0.0057]										[0.0079]
Corruption, 1985				0.062									-0.029
				[0.071]									[0.075]
Population, 1995					0.00002								
					[0.00031]								
Land area (sq km)						-0.000053*							-1.8E-05
						[0.000030]							[0.000036]
Ethnolinguistic fractionalization							-0.55*						-0.41
							[0.30]						[0.34]
% of catholics, 1980								-0.00046					
								[0.0027]					
% of protestants, 1980								-0.0055					
								[0.0057]					
% of muslims, 1980								-0.0043*					
								[0.0024]					
N. of conflicts, av. 1985-1995									0.024				
									[0.12]				
(Absolute) Latitutde										0.22			
										[0.54]			
% of tropical area											-0.13		
											[0.18]		
Share of natural resources, 1971												-0.23	
												[0.20]	
Constant	1.24***	1.12***	1.13***	1.23***	1.24***	1.27***	1.52***	1.43***	1.24***	1.19***	1.34***	1.41***	1.53***
	[0.20]	[0.20]	[0.22]	[0.28]	[0.20]	[0.20]	[0.23]	[0.27]	[0.20]	[0.24]	[0.24]	[0.24]	[0.30]
Observations	101	101	95	93	101	101	84	100	100	100	93	83	78
R-squared	0.33	0.41	0.40	0.36	0.33	0.34	0.40	0.36	0.33	0.33	0.38	0.36	0.54

Notes: Robust standard errors in brackets. ***, ** and * denote significance at 1, 5 and 10 percent level.

infant mortality is a clear indicator of need of aid. This supports the relevance of infant mortality as an instrument for aid. Second, in a study on the link between institutions of governance and development performance, Campos and Nugent (1999) show that bureaucratic quality has no direct effect on infant mortality rates across countries (see Campos and Nugent, 1999, Table 4). This supports infant mortality's validity as instrument. Third, if population size and infant mortality are valid instruments, and uncorrelated among themselves,[10] then any linear combination of them is valid as well, and in general more efficient than using any of them separately.[11]

As shown in the next section, this pair of instruments passes comfortably the standard tests of strength and relevance in all cases, except when the aid variable refers to loans, where we cannot reject with a high probability that the instruments are weak. We find that infant mortality is a particularly weak instrument for loans. This is perhaps not very surprising though, as loans tend to be given to richer developing countries, where infant mortality arguably is no longer among the most pressing concerns.[12] As an alternative instrument we include the (lagged) level of loan commitments. Loan commitments are highly correlated with loan disbursements (see e.g. Clemens et al., 2004), and should not affect the level of bureaucratic quality directly if sufficient time has passed since commitments were made.

Finally, since the 2SLS estimates do not allow for correct inference on the coefficients when the instruments are weak, we additionally rely on Moreira's (2003) test for the significance of weakly identified coefficients.

3 Results

Table 2 presents the OLS regression results for the preferred specification, which includes aid as the explanatory variable of interest, and the levels of initial bureaucratic quality and initial GDP per capita as controls. Both controls are positively and significantly associated with average bureaucratic quality over the period under consideration. As concerns aid, a clear pattern emerges. First, while project aid does not seem to affect bureaucratic quality, program aid (in general) and aid for budget support (in particular) turn out to exhibit a negative correlation with the level of bureaucratic quality. Second, the effects are statistically significant for total levels of aid and for the part of them being only grants, but not for loans. Third, the impacts of total aid and grants are larger (more negative) when the funds are meant to support the fiscal budget rather than to finance specific projects or more general programs. If we associate grants and aid for general programs with potentially higher degrees of flexibility in the use of

[10]The correlation between them in the sample is 1.5%.

[11]Using both instruments also allows us to test one overidentifting restriction, which is naturally additional useful information to assess the instrumentation strategy's overall validity.

[12]Other indicators of need, such as the share of paved roads equally fail to produce strong and valid instruments.

Table 2: Disaggregated Aid and Bureaucratic Quality, OLS

Dependent variable: Δ Bureaucratic Quality, 1995-2005	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Bureaucratic Quality, 1995	-0.71***	-0.64***	-0.72***	-0.67***	-0.65***	-0.68***	-0.68***	-0.65***	-0.68***	-0.68***	-0.61***	-0.68***
	[0.09]	[0.09]	[0.09]	[0.09]	[0.09]	[0.09]	[0.09]	[0.09]	[0.09]	[0.09]	[0.08]	[0.09]
Real GDP per capita, 1995	0.04***	0.05***	0.04***	0.05***	0.05***	0.05***	0.05***	0.05***	0.05***	0.05***	0.05***	0.05***
	[0.01]	[0.01]	[0.01]	[0.01]	[0.01]	[0.01]	[0.01]	[0.01]	[0.01]	[0.01]	[0.01]	[0.01]
Aid/GDP, av. 1995-2005	-0.04***											
	[0.01]											
Loans		0.02										
		[0.03]										
Grants			-0.04***									
			[0.01]									
Project aid/GDP, av. 1995-2005				-0.03								
				[0.03]								
Loans					0.07							
					[0.12]							
Grants						-0.05						
						[0.03]						
Program aid/GDP, av. 1995-2005							-0.05***					
							[0.02]					
Loans								-0.28				
								[0.17]				
Grants									-0.04***			
									[0.01]			
Budget support aid/GDP, av. 1995-2005										-0.26***		
										[0.09]		
Loans											-1.79***	
											[0.68]	
Grants												-0.23**
												[0.09]
Constant	1.12***	0.77***	1.13***	0.92***	0.78***	0.96***	0.98***	0.88***	0.96***	1.00***	0.84***	0.98***
	[0.20]	[0.17]	[0.19]	[0.19]	[0.17]	[0.19]	[0.19]	[0.18]	[0.18]	[0.19]	[0.16]	[0.19]
Observations	101	101	101	101	101	101	101	101	101	101	101	101
R-squared	0.41	0.37	0.42	0.38	0.37	0.38	0.40	0.39	0.40	0.41	0.42	0.39

Notes: Robust standard errors in brackets. ***, ** and * denote significance at 1, 5 and 10 percent level.

Table 3: Disaggregated Aid and Bureaucratic Quality, 2SLS

Dependent variable: Δ Bureaucratic Quality, 1995-2005	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Bureaucratic Quality, 1995	-0.82***	-0.48**	-0.82***	-0.79***	-0.47***	-0.81***	-0.78***	-0.70***	-0.82***	-0.74***	-0.38*	-0.78***
	[0.11]	[0.22]	[0.10]	[0.11]	[0.15]	[0.11]	[0.11]	[0.12]	[0.12]	[0.09]	[0.20]	[0.10]
Real GDP per capita, 1995	0.03**	0.09*	0.03**	0.02	-0.02	0.02*	0.03*	0.01	0.03*	0.03**	0.00	0.03**
	[0.01]	[0.05]	[0.01]	[0.02]	[0.04]	[0.01]	[0.01]	[0.02]	[0.01]	[0.01]	[0.02]	[0.01]
Aid/GDP, av. 1995-2005	-0.10***											
	[0.03]											
Loans		0.84										
		[1.19]										
Grants			-0.10***									
			[0.03]									
Project aid/GDP, av. 1995-2005				-0.28***								
				[0.09]								
Loans					-3.34*							
					[1.91]							
Grants						-0.26***						
						[0.08]						
Program aid/GDP, av. 1995-2005							-0.22***					
							[0.07]					
Loans								-2.87				
								[1.85]				
Grants									-0.21***			
									[0.07]			
Budget support aid/GDP, av. 1995-2005										-0.80***		
										[0.24]		
Loans											-12.74**	
											[5.99]	
Grants												-0.86***
												[0.27]
Constant	1.64***	-0.21	1.57***	1.77***	1.76***	1.69***	1.56***	1.62***	1.63***	1.43***	1.10***	1.48***
	[0.29]	[1.26]	[0.27]	[0.35]	[0.57]	[0.31]	[0.31]	[0.39]	[0.36]	[0.23]	[0.35]	[0.26]
Observations	100	100	100	100	100	100	100	100	100	100	100	100
R-squared	0.37	0.01	0.41	0.27	0.02	0.33	0.18	0.05	0.14	0.30	0.04	0.30
First-stage F statistic	11.87	4.84	12.44	10.48	3.07	11.35	8.68	3.74	7.65	10.25	2.73	10.10
Sargan/Hansen J stat. overidentification (p-value)	0.60	0.06	0.51	0.87	0.10	0.92	0.64	0.86	0.56	0.69	0.33	0.62

Notes: Robust standard errors in brackets. ***, ** and * denote significance at 1, 5 and 10%. Aid instrumented by population size and under-5 mortality rate in 1995.

Table 4: Loans and Bureaucratic Quality, 2SLS

Dependent variable: Δ Bureaucratic Quality, 1995-2005	(1)	(2)	(3)	(4)
Bureaucratic Quality, 1995	-0.724***	-0.62***	-0.67***	-0.59***
	[0.130]	[0.10]	[0.10]	[0.09]
Real GDP per capita, 1995	0.0399**	0.04*	0.04*	0.04***
	[0.0190]	[0.02]	[0.02]	[0.01]
All Aid Loans/GDP, av. 1995-2005	-0.38			
	[0.313]			
Project aid loans/GDP, v. 1995-2005		-0.45		
		[0.82]		
Program aid loans/GDP, av. 1995-2005			-1.29	
			[1.03]	
Budget Support loans/GDP, av. 1995-2005				-2.59
				[2.02]
Constant	1.248***	0.93***	1.17***	0.86***
	[0.425]	[0.29]	[0.35]	[0.16]
Observations	101	101	101	101
R-squared	0.13	0.14	0.07	0.10
First-stage F statistic	14.44	11.85	10.96	32.80
p-value for Moreira (2003) CLR test	0.10	0.56	0.15	0.22
Confidence interval for Moreira (2003) CLR test	[-2.51, 0.00]	[-3.24, 1.00]	[-9.19, 0.20]	[-6.90, 1.04]

Notes: Robust standard errors in brackets. ***, ** and * denote significance at 1, 5 and 10 percent level. Aid disbursements instrumented by initial aid commitments for each loan category.

funds, our results suggest that the probability of adverse effects from aid varies positively with the degree of discretion that recipients have over the incoming resources.

Table 3 shows the results when we estimate the model by 2SLS, instrumenting aid with initial infant mortality and initial population. The first stage regression in this case reflects well the main findings of the aid allocation literature (e.g. Berthélemy, 2006). That is, given equation (2), the first stage regression explains the level of aid received by a country with the initial level of population (reflecting the fact that smaller countries receive more aid in relative terms), the initial level of infant mortality (representing an important specific need of the recipient country), and the initial level of GDP per capita (which captures, both, initial development conditions, and the idea that donors prefer to give more resources to poorer countries).[13]

[13] None of the standard indicators of donor interest, such as former colonial status or religious affinity, qualified as a potential instrument as none is found to be a significant determinant of aid allocation. This may reflect the fact that we include aid given by multilateral agencies, which according to previous studies is unaffected by conventional donor interest variables (e.g., Nunnenkamp and Thiele 2006).

The most notable result in Table 3 is that the sign of project aid turns significantly negative. This can be rationalized along the line of Knack and Rahman's (2007) finding that project proliferation deteriorates bureaucratic quality, by putting a strain on the absorptive capacity of recipient governments. The instruments we rely on in Table 3 appear as valid (we get high p-values of the Hansen's J-test for overidentification) and fairly strong (F-statistics exceeding the rule-of-thumb threshold of 10), except for the case of loans. The impact of loans on bureaucratic quality continues to be insignificant or weakly significant except for budget support, but inference about this estimate is limited by the presence of weak instruments.

In Table 4 we introduce lagged commitments as an instrument for disbursed loans. This instrument appears to be markedly stronger than those used before. The corresponding 2SLS regression yields an estimate statistically not different from zero, which is corroborated by Moreira's (2003) test. In all cases the confidence interval for the coefficient contains the value zero.

4 Concluding Remarks

This paper analyzes the impact of different forms of aid on bureaucratic quality in recipient countries. The main finding is that grants impair the functioning of the local bureaucracy whereas loans do not. In a similar vein, when investigating tax effort, another important dimension of governance, Gupta et al. (2004) find that grants have a significant negative effect on government revenue while loans have a significant positive effect. Taken together, these results qualify the predominant view that loans are superior to grants as a mode of delivering aid to poor countries.

Grants are found to exhibit the strongest negative effect on bureaucratic quality when they take the form of budget support. Our analysis thus suggests a note of caution about routinely providing aid for budget support.

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A Variable definitions and sources

1. **Aid.** Total ODA disbursements. Source: OECD CRS.

2. **Project aid.** ODA disbursements for (DAC codes):
   (110) education,
   (120) health,
   (130) population policies/programmes and reproductive health,
   (140) water supply and sanitation,
   (150) government and civil society,
   (160) other social infrastructure and services,
   (210) transport and storage,
   (230) energy generation and supply,
   (240) banking and financial services,
   (250) business and other services,
   (311) agriculture,
   (312) forestry,
   (313) fishing,
   (321) industry,
   (322) mineral resources and mining,
   (323) construction,
   (331) trade policy and regulations and trade-related adjustment,
   (332) tourism,
   (400) multisector/cross-cutting,
   (430) other multisector,
   (910) administrative costs of donors.
   Source: OECD CRS.

3. **Program aid.** ODA disbursements for (DAC codes):
   (510) general budget support,
   (520) developmental food aid/food security assistance,
   (600) action relating to debt.
   Source: OECD CRS.

4. **Budget support.** ODA disbursements for Budget support. Part of Program aid. Source: OECD CRS.

5. **Bureaucratic quality.** Bureaucratic Quality Index 0 to 4 points. Measures institutional strength and the extent to which the bureaucracy tends to minimize revisions of policy when governments change. High points (up to 4) are given to countries where the bureaucracy governing without drastic changes in policy or interruptions in government services. These low-risk countries have bureaucracies somewhat autonomous from political pressure and with established mech-

anisms for recruitment and training. To produce this index, the ICRG collects political information and makes political risk assessments on the basis of subjective analysis of the available information.
Source: ICRG.

6. **Corruption.** Index 0 to 6 Points. Assessment of corruption within the political system. The most common form of corruption met directly by business is in the form of demands for special payments and bribes connected with import and export licenses, exchange controls, tax assessments, police protection, loans, etc. Although this measure takes such corruption into account, it is more concerned with actual or potential corruption in the form of excessive patronage, nepotism, job reservations, 'favor-for-favors', secret party funding, and suspiciously close ties between politics and business. Similar to the measure of bureaucratic quality, to produce this index, the ICRG makes a risk assessment on the basis of subjective analysis of the available information.
Source: ICRG.

7. **Real GDP per capita.** Real GDP per capita. Based on Penn World Table's (PWT) 6.2 chain index, obtained by first applying the component growth rates between each pair of consecutive years $t-1$ and t ($t = 1951$ to 2000), to the current price component shares in year to obtain a growth rate for each year. This growth rate for each year t is applied backwards and forwards from 1996, and summed to the constant price net foreign balance to obtain the Chain GDP series.
Source: PWT 6.2.

8. **Population.** Population size (in millions).
Source: WDI 2007.

9. **Land area.** Square km (in 000s).
Source: WDI 2007.

10. **Under 5 mortality rate.** Under-5 mortality rate is the probability that a newborn baby will die before reaching age five, if subject to current age-specific mortality rates. The probability is expressed as a rate per 1,000.
Source: WDI 2007.

11. **Human capital.** Ratio of total enrolment, regardless of age, to the population of the age group that officially corresponds to the level of tertiary education. Tertiary education, whether or not to an advanced research qualification, normally requires, as a minimum condition of admission, the successful completion of education at the secondary level.
Source: WDI 2007.

12. **Ethno-linguistic fractionalization.** Easterly and Levine's (1997) average of ethnolinguistic fractionalization, reflecting the share of the population for whom the language spoken at home is not the official or the most widely used language in the country, and the degree of ethnic fractionalization.
    Source: Toerell et al. (2008).

13. **Share of catholics.** La Porta et al (1999). Catholics as percentage of population in 1980.
    Source: Toerell et al. (2008).

14. **Share of muslims.** La Porta et al (1999). Muslims as percentage of population in 1980.
    Source: Toerell et al. (2008).

15. **Share of protestants.** La Porta et al (1999). Protestants as percentage of population in 1980.
    Source: Toerell et al. (2008).

16. **Absolute latitude (index 0-1).** La Porta et al (1999). The absolute value of the latitude of the capital city, divided by 90 (to take values between 0 and 1).
    Source: Toerell et al. (2008).

17. **Number of conflicts the gov't is involved.** UCDP/PRIO Armed Conflict Dataset (version 3-2005). Number of conflicts in which the government of the country is involved.
    Source: Toerell et al. (2008).

18. **Share of land area in the tropics.** From Sachs and Gallup (1995). Percentage of tropical area in the country.
    Source: CID, Harvard.

19. **Natural resources abundance.** From Sachs and Warner (1997). Share of mineral production in GNP in 1971.
    Source: CID, Harvard.

B Tables and Figure 1

Table A1. Summary statistics

	No. obs.	Mean	Std. Dev.	Min	Max
Bureaucratic Quality index, 1995	101	1.91	0.94	0	3.58
Bureaucratic Quality index, av. 1995-2005	101	1.85	0.85	0	3.93
Δ Bureaucratic Quality index, 1995-2005	101	-0.11	0.88	-3	2
Aid / GDP, av. 1995-2005	101	3.61	5.32	0.002	24.34
Loans	101	0.59	1.53	0	14.49223
Grants	101	3.25	5.12	0.001	24.04
Budget support aid/GDP, av. 1995-2005	101	0.38	0.70	0	3.18
Loans	101	0.04	0.12	0	0.72
Grants	101	0.34	0.65	0	2.90
Project aid/GDP, av. 1995-2005	101	1.77	2.30	0	11.45
Loans	101	0.25	0.43	0	2.83
Grants	101	1.58	2.21	0	11.40
Program aid/GDP, av. 1995-2005	101	1.61	3.23	0	20.35
Loans	101	0.17	0.43	0	3.18
Grants	101	1.70	3.81	0	26.77
Real GDP per capita (000s), 1995	101	6.25	6.85	0.17	30.56
School enrolment, 1995	95	16.81	14.80	0.29	65.95
Corruption index, 1985	93	2.76	1.24	0	6
Population, 1995	101	42.75	151.55	0.28	1204.86
Under-5 mortality rate per thousand,1995	100	83.21	72.60	6	295
Land area (000s sq km)	101	732.61	1364.45	0.32	9327.48
Ethnolinguistic fractionalization	84	0.42	0.32	0	0.89
% of catholics, 1980	100	29.63	36.08	0	97.3
% of protestants, 1980	100	7.97	13.67	0	64.2
% of muslims, 1980	100	31.42	38.65	0	99.5
N. of conflicts, av. 1985-1995	100	0.38	0.63	0	4.8
(Absolute) Latitutde index	100	0.22	0.15	0	0.59
% of tropical area	93	0.64	0.45	0	1
Share of natural resources, 1971	83	0.14	0.26	0	1.51

Table A2. Countries in the sample (WB country codes)

Latin America	
Argentina	(ARG)
Bahamas, The	(BHS)
Bolivia	(BOL)
Brazil	(BRA)
Chile	(CHL)
Colombia	(COL)
Costa Rica	(CRI)
Dominican Republic	(DOM)
Ecuador	(ECU)
Guatemala	(GTM)
Guyana	(GUY)
Honduras	(HND)
Haiti	(HTI)
Jamaica	(JAM)
Mexico	(MEX)
Nicaragua	(NIC)
Panama	(PAN)
Peru	(PER)
Paraguay	(PRY)
El Salvador	(SLV)
Suriname	(SUR)
Trinidad and Tobago	(TTO)
Uruguay	(URY)
Venezuela, RB	(VEN)
Asia	
Bangladesh	(BGD)
Brunei Darussalam	(BRN)
China	(CHN)
Hong Kong, China	(HKG)
Indonesia	(IDN)
India	(IND)
Korea, Rep.	(KOR)
Sri Lanka	(LKA)
Mongolia	(MNG)
Malaysia	(MYS)
Pakistan	(PAK)
Philippines	(PHL)
Papua New Guinea	(PNG)
Singapore	(SGP)
Thailand	(THA)
Vietnam	(VNM)
Eastern Europe	
Albania	(ALB)
Armenia	(ARM)
Azerbaijan	(AZE)
Belarus	(BLR)
Cyprus	(CYP)
Croatia	(HRV)
Kazakhstan	(KAZ)
Moldova	(MDA)
Slovenia	(SVN)
Turkey	(TUR)
Ukraine	(UKR)
Middle East	
United Arab Emirates	(ARE)
Bahrain	(BHR)
Algeria	(DZA)
Egypt, Arab Rep.	(EGY)
Iran, Islamic Rep.	(IRN)
Iraq	(IRQ)
Israel	(ISR)
Jordan	(JOR)
Kuwait	(KWT)
Lebanon	(LBN)
Libya	(LBY)
Morocco	(MAR)
Malta	(MLT)
Oman	(OMN)
Qatar	(QAT)
Saudi Arabia	(SAU)
Syrian Arab Republic	(SYR)
Tunisia	(TUN)
Yemen, Rep.	(YEM)
Africa	
Angola	(AGO)
Burkina Faso	(BFA)
Botswana	(BWA)
Côte d'Ivoire	(CIV)
Cameroon	(CMR)
Congo, Rep.	(COG)
Ethiopia	(ETH)
Gabon	(GAB)
Ghana	(GHA)
Guinea	(GIN)
Gambia, The	(GMB)
Guinea-Bissau	(GNB)
Kenya	(KEN)
Liberia	(LBR)
Madagascar	(MDG)
Mali	(MLI)
Mozambique	(MOZ)
Malawi	(MWI)
Namibia	(NAM)
Niger	(NER)
Nigeria	(NGA)
Sudan	(SDN)
Senegal	(SEN)
Sierra Leone	(SLE)
Togo	(TGO)
Tanzania	(TZA)
Uganda	(UGA)
South Africa	(ZAF)
Congo, Dem. Rep.	(ZAR)
Zambia	(ZMB)
Zimbabwe	(ZWE)

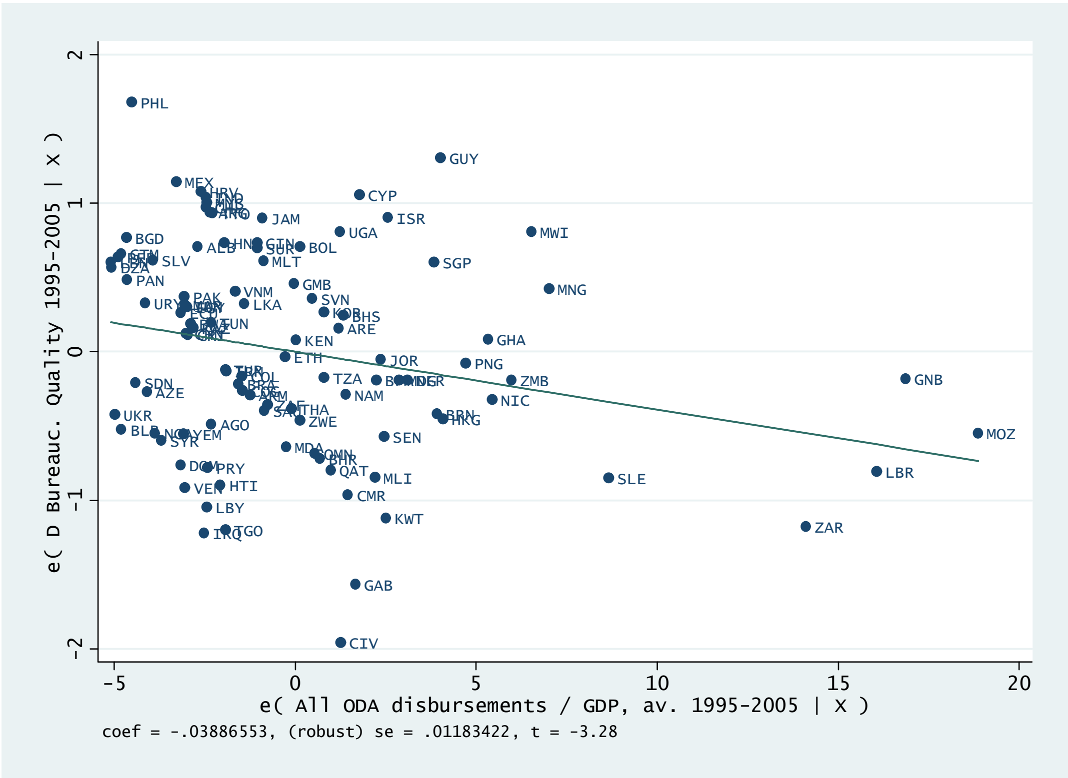

Figure 1: The relationship between average aid receipts and change in Bureaucratic Quality over 1995-2005, controlling for initial levels of Bureaucratic Quality and income per capita. (The slope corresponds to the coefficient of aid/GDP in regression 2 in Table 1).

Does Foreign Aid increase Foreign Direct Investment?*

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Abstract

The notion that foreign aid and foreign direct investment (FDI) are complementary sources of capital is conventional among governments and international cooperation agencies. This paper argues that the notion is incomplete. Within the framework of an open economy Solow model we show that the theoretical relationship between foreign aid and FDI is indeterminate. Aid may raise the marginal productivity of capital by financing complementary inputs, such as public infrastructure projects and human capital investment. However, aid may also crowd out productive private investments if it comes in the shape of physical capital transfers. We therefore turn to an empirical analysis of the relationship between FDI and disaggregated aid flows. Our results strongly support the hypotheses that aid invested in complementary inputs draws in foreign capital while aid invested in physical capital crowds out FDI. The combined effect of these two types of aid is small but on average positive.

Keywords: Aid, foreign direct investment (FDI), open economy Solow model.
JEL classifications: F21, F35, H40, O19.

1 Introduction

The notion that foreign aid and foreign direct investment (FDI) are complementary sources of capital is conventional among governments and international cooperation agencies. For example, a salient point in the UN's 2002 Monterrey Report of the International Conference on Financing for Development is that official development assistance (ODA), trade and foreign direct investment (FDI) are three essential tools for development financing:

> "ODA plays an essential role as a complement to other sources of financing for development, especially in those countries with the least capacity to attract private direct investment. A central challenge, therefore, is to create

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> the necessary domestic and international conditions to facilitate direct investment flows, conducive to achieving national development priorities, to developing countries, particularly Africa, least developed countries, small island developing States, and landlocked developing countries, and also to countries with economies in transition." (UN, 2002, p. 9).

However, the implicit presumption that ODA has a "catalysing" effect on FDI, i.e., that aid and FDI are complements, is by no means evident. For example, Kosack and Tobin (2006) argue that aid and FDI are unrelated, because aid is mainly oriented to support the government budget and finance investments in human capital, while FDI is a private sector decision and relatively more connected to physical capital. In a more general study, Caselli and Feyrer (2007) find that the marginal product of capital (*MPK*) is roughly the same across countries, and one of the implications is that increasing aid inflows to developing countries will lower the *MPK* in these economies and will tend to be fully offset by outflows of other types of capital investments (p. 540). If this is the case, aid and FDI are clearly closer to being substitutes rather than being complements.

This paper provides a unified framework for assessing the relative merit of these different claims. We set up an open-economy Solow model with perfect capital mobility that distinguishes between aid directed towards complementary factors of production and aid invested in physical capital. The distinction serves to illustrate, on the one hand, that aid invested in complementary factors increases *MPK* in the recipient country, which tends to draw in additional foreign resources, and thus helps to sustain a higher level of capital over time. For example, aid can ease important bottlenecks in poor countries by financing public infrastructure and human capital investments that would not have been undertaken by private actors (due to the free-riding problem in financing public goods), nor by public agents (because of the budgetary constraints that prevent aid-recipient governments from undertaking this type of investments). On the other hand, the model also shows that foreign aid invested in physical capital directly competes with other types of capital, and thus replaces investments that private actors would have undertaken anyway. In this case, capital mobility and rate-of-return equalisation across countries will give rise to a flight of other types of capital after an aid flow has been received.

The theoretical model provides a number of results and testable predictions. First, for a given level of domestic saving, aid invested in physical capital crowds out other types of foreign investments in physical capital, one for one. Second, aid invested in complementary factors of production has an ambiguous effect on FDI. The logic of the ambiguity is that, while an increase in complementary factors increases *MPK*, the productivity increase also raises income, domestic savings and domestic investments, which tends to lower *MPK* and thus to crowd out foreign investments. These two findings suggest that the overall impact of aid on FDI is ambiguous and that the composition of aid matters. Finally, the relationship between complementary aid and FDI is unlikely to be linear, so scale effects from this type of aid should be taken into account.

We take the implications of our theoretical model to the data utilising a panel of 84 countries over the period 1970-2001. We find a large and positive effect of aid invested in complementary factors, while aid invested in physical capital has a negative impact on FDI. Although the combined impact of these two types of aid on FDI remains positive, our results imply that more aid should be directed towards inputs complementary to physical capital to optimise the return on aid. The results are robust to (1) a broader definition of complementary aid than that adopted in our benchmark estimations, (2) to allowing for imperfect capital mobility, and (3) to including other traditional FDI determinants.

The paper is structured as follows. Section 2 reviews the scarce empirical literature on FDI and aid. Section 3 introduces the theoretical model of FDI and aid building on an open economy Solow model with perfect capital mobility. Section 4 discusses relevant econometric issues and presents the data. Section 5 shows the results, and Section 6 tests their robustness. Section 7 sums up and discusses policy implications.

2 Literature Review

The relationship between aid and FDI is controversial and empirical results remain inconclusive. To our knowledge, only four papers explicitly analyse the relationship between aid and FDI. Harms and Lutz (2006) and Karakaplan *et al.* (2005) analyse the question for a broad sample of developing countries. Karakaplan *et al.* (2005) find that aid has a negative direct effect on FDI and that both good governance and financial market development significantly improve the impact of aid on subsequent flows of FDI. Harms and Lutz (2006), on the other hand, find that once they control for the regulatory burden in the host country, aid works as a complement to FDI and, surprisingly, that the catalysing effect of foreign aid is stronger in countries that are characterised by an unfavourable institutional environment.

The two case studies based on Japanese FDI and aid flows in Kimura and Todo (2007) and Blaise (2005) also find incongruent results. While Blaise (2005) finds positive effects of aid to infrastructure projects, Kimura and Todo (2007) find no positive infrastructure effect, no negative rent-seeking effect but a positive vanguard effect (arising when foreign aid from a particular donor country promotes FDI from the same country but not from other countries).

This paper argues that the mixed results can be explained by the high level of aggregation of the aid variable. While Karakaplan *et al.* (2005) include only overall ODA, Harms and Lutz (2006) also distinguish between grants, technical cooperation grants, as well as bilateral and multilateral aid. However, it remains unclear why one would expect foreign investors to react differently to these sources of aid. Kimura and Todo (2007) apply the idea of different types of aid, but construct their proxies relying only on data for aid commitments and they only separate out aid to physical infrastructure.

3 A Theoretical Model of FDI and Aid

A general shortcoming in the empirical literature is the lack of consensus on the specification of the FDI relation, and none of the existing empirical papers on aid and FDI are supported by a theoretical model. This paper closes this gap by proposing a Solow model for a small open economy to model the main characteristics of the relationship between aid and FDI.[1]

We assume a Cobb-Douglas production function where GDP per capita, y, is given by

$$y = Ak^{\alpha}, \tag{1}$$

where k is the stock of physical capital per capita, $\frac{K}{L}$, α is a constant and A denotes total factor productivity.

We assume that the total flow of foreign aid, AID, can be split into aid invested in complementary factors, AID_A, and aid invested in physical capital, AID_K, where $AID = AID_A + AID_K$. AID_A by nature raises the marginal productivity of all production factors that are complementary to physical capital.[2] For example, infrastructure investments lead to the interconnection of markets (Easterly and Levine, 1999), while investments in human capital improve technology adoption. AID_K, on the other hand, enters the production function only through its effect on physical capital accumulation, and has no (augmenting) effect on total factor productivity.[3]

To model this explicitly, we first assume that complementary aid has an augmenting effect on all production factors that are complementary to physical capital, and we thus allow the flow of AID_A to increase the existing stock (A_0) of A in the economy:

$$A = A_0 + AID_A. \tag{2}$$

Allowing complementary aid to have a direct impact on A is a shorthand for the idea that AID_A has an augmenting effect on any production factor other than k (e.g. human capital, public investments, new technology, etc.) and, thus, it is able to increase – ultimately– the MPK.

Second, we assume an open economy.[4] Accordingly, in per capita terms, capital

[1]One exception is Beladi and Oladi (2007) who analyse the question in a general equilibrium setting where all foreign aid is used to finance public goods.

[2]The argument of complementarity between public and private investment is generalised by Clarida (1993) and Chatterjee et al. (2003). Reinikka and Svensson (2002) find empirical support for the importance of complementary public capital for foreign investors.

[3]We thus allow part of foreign aid to be productivity enhancing while FDI brings no spillovers. In reality, all capital transfers might contain some knowledge transfer but the assumption is made to keep the model simple and tractable.

[4]In line with Sørensen and Witta-Jacobsen (2006, Ch. 4) and Turnovsky (2000).

equipment can be financed by (i) domestic savings ($S = sy$, where s is a given savings rate), (ii) foreign direct investments (fdi) and (iii) the inflow of aid invested in physical capital (aid_K). Then capital accumulation per capita is given by

$$\dot{k} = sy + fdi + aid_K - (n + \delta)k, \tag{3}$$

where n is the population growth rate and δ is a fixed depreciation rate.

With perfect capital mobility, the world real rate of return, r^w, pins down at any point in time the net return to capital ($MPK - \delta$), and thus

$$r^w = MPK - \delta = A\alpha k^{\alpha-1} - \delta. \tag{4}$$

According to (4), the steady state level of k at any point in time is given by

$$k^* = \left[\frac{A\alpha}{r}\right]^{\frac{1}{1-\alpha}}, \tag{5}$$

where r is defined as a gross world real rate of return, $r^w + \delta$.

Rewriting (3) taking (5) as given, the flow of FDI per capita is determined as the residual

$$fdi = -aid_K - sy^* + (n + \delta)k^*, \tag{6}$$

where $y^* = Ak^{*\alpha}$.

At a first glance, (6) seems to support the Caselli and Feyrer (2007) conjecture that aid and FDI are substitutes: for a given level of domestic savings, equalisation between MPK and r requires an increase in foreign aid to be accommodated by a proportional reduction in FDI:

$$\frac{\partial fdi}{\partial aid_K} = -1. \tag{7}$$

However, this finding only holds for aid invested in physical capital. The effect of complementary aid, on the other hand, has two components:

$$\frac{\partial fdi}{\partial aid_A} = -s\frac{\partial y^*}{\partial aid_A} + (n + \delta)\frac{\partial k^*}{\partial aid_A}. \tag{8}$$

First, since

$$s\frac{\partial y^*}{\partial aid_A} = s\frac{\partial\left(Ak^{*\alpha}\right)}{\partial aid_A} = s\left[Lk^{*\alpha} + A\alpha k^{*\alpha-1}\frac{\partial k^*}{\partial aid_A}\right] > 0, \tag{9}$$

complementary aid has a positive effect on domestic savings and thus on domestically financed capital investments. This result comes from the fact that aid_A shifts the production function thereby raising the steady state levels of income and domestic savings. Given the assumption of MPK equalisation in (4), the corresponding increase in domestically financed investments causes a proportional reduction in the need for FDI of the size $-s\frac{\partial y^*}{\partial aid_A}$.

Also, since

$$\frac{\partial k^*}{\partial aid_A} = \frac{\partial}{\partial aid_A}\left(\left[\frac{A\alpha}{r}\right]^{\frac{1}{1-\alpha}}\right) = \frac{1}{1-\alpha}\left[\frac{A\alpha}{r}\right]^{\frac{\alpha}{1-\alpha}}\frac{L\alpha}{r} > 0, \tag{10}$$

we see that complementary aid has a positive effect on the steady state capital stock. This finding is based on the augmenting effect of aid_A, which raises MPK and thus allows the recipient country to increase its capital stock without experiencing a counterbalancing capital flight. That is, for a fixed s, aid-financed investments in complementary factors allow a sustainable increase in FDI equal to $(n+\delta)\frac{\partial k^*}{\partial aid_A}$.

This model holds then several implications that should be taken into account when assessing the empirical relationship between aid and FDI. First, the effect of total aid on FDI is ambiguous:

$$\frac{\partial fdi}{\partial aid} = \frac{\partial fdi}{\partial aid_K} + \frac{\partial fdi}{\partial aid_A} = -1 - s\frac{\partial y^*}{\partial aid_A} + (n+\delta)\frac{\partial k^*}{\partial aid_A} \gtrless 0, \tag{11}$$

because we expect aid to production sectors to have a negative effect on FDI, but the effect of complementary aid is indeterminate. Second, from equations (9) and (10), since the marginal effect of complementary aid on FDI includes the level of aid itself, the relationship between complementary aid and FDI is not linear. In particular, there are scale effects from complementary aid that should be taken into account. Since $-s\frac{\partial y^*}{\partial aid_A}$ and $(n+\delta)\frac{\partial k^*}{\partial aid_A}$ work in opposite directions, the sign of the second order effects will also be indeterminate and will need to be assessed empirically. Third, the model stresses the need to take all sources of capital into account, and it is therefore essential to include domestic savings as an additional explanatory variable in the empirical FDI analysis. To our knowledge, this has not been done before.

4 Econometric Issues

In a panel setting, the econometric interpretation of the aid-FDI relationship is

$$fdi_{it} = \beta_0 + \beta_1 A^0_{it} + \beta_2 n_{it} + \beta_3 S_{it} + \beta_4 aid^K_{it} + \beta_5 aid^A_{it} + \beta_6\left(aid^A_{it}\right)^2 + u_{it}, \tag{12}$$

where fdi_{it} is FDI per capita in country i during period t, A^0_{it} is the overall productivity level at the beginning of period t, n_{it} is population growth, S_{it} is domestic savings per capita, aid^K_{it} is aid invested in physical capital, and aid^A_{it} is aid invested in complementary factors. The square of aid^A_{it} is included in (12) to control for the scale effects of complementary aid.

We expect β_1 to be positive since a high productivity level gives a high steady state level of capital, β_2 should be positive since a fast growing population lowers the per capita capital stock and thus allows for an increase in FDI per capita, and β_3 should be negative since high domestic saving lowers the need for foreign capital. From equation (7) we know that aid_K crowds out foreign investments one-to-one, $\beta_4 = -1$, whereas the effect of aid_A (β_5 and β_6) is indeterminate. Since data on total productivity is unavailable, the next section will discuss the strategy used to identify A^0_{it} empirically.

4.1 Productivity

Since data on the initial productivity level (A^0_{it}) is unavailable, we need to find valid proxies. In the first case, we use pooled OLS (POLS) and estimate

$$fdi_{it} = \alpha_t + \beta_0 + \beta_1 n_{it} + \beta_2 S_{it} + \beta_3 aid^K_{it} + \beta_4 aid^A_{it} + \beta_5 \left(aid^A_{it}\right)^2 + u_{it}, \tag{13}$$

where α_t is a time-specific constant that captures common productivity shocks at time t. However, not all countries start out with the same initial conditions and we thus allow also for cross sectional differences in productivity by including time-invariant country-specific fixed effects, α_i,

$$fdi_{it} = \alpha_t + \alpha_i + \beta_0 + \beta_1 n_{it} + \beta_2 S_{it} + \beta_3 aid^K_{it} + \beta_4 aid^A_{it} + \beta_5 \left(aid^A_{it}\right)^2 + u_{it}. \tag{14}$$

This equation can be estimated consistently and efficiently with a fixed effects model (FE). However, if productivity evolves unequally across countries over time, regression (14) leaves out important information. We therefore extend the list of variables to include a lagged dependent variable, which captures time-moving country-specific factors as well as agglomeration effects,

$$fdi_{it} = \alpha_t + \alpha_i + \beta_0 + \beta_1 fdi_{it-1} + \beta_2 n_{it} + \beta_3 S_{it} + \beta_4 aid^K_{it} + \beta_5 aid^A_{it} + \beta_6 \left(aid^A_{it}\right)^2 + u_{it}. \tag{15}$$

Equation (15) can be estimated consistently and efficiently using the Arellano and Bond (1991) Generalised Method of Moments (GMM) estimator. It is important to notice that including a lagged dependent variable also reduces the need to control for other FDI determinants. All estimators use standard errors that are robust to arbitrary heteroskedasticity as well as intra-group correlation (clustering).

4.2 Endogeneity

We need to consider the possible endogeneity of aid in estimating the above equations, since all estimators are consistent only if all explanatory variables are exogenous. Aid would be endogenous, for example, if donors systematically disburse more resources to those countries that are neglected by private foreign investors (Harms and Lutz, 2006). We therefore estimate (13)–(15) following the instrumentation strategy in Hansen and Tarp (2000, 2001), Dalgaard and Hansen (2001) and Dalgaard *et al.* (2004).

The first set of instruments accounts for donors' overall preference for granting more aid to countries with smaller populations and lower levels of income per capita and thus includes (lagged) interactions between levels of aid and (i) the size of population and (ii) the initial level of GDP per capita in the recipient country. We also include the lagged level of aid to account for persistency in other determinants of aid as well as a dummy variable for African countries in the CFA franc zone to capture particular donors' strategic interests.

Tests confirm the validity of our instruments, and the Durbin-Wu-Hausman test finds that the aid variables should be treated as endogenous in the FDI relation. All the results reported in the next section are therefore based on Instrumental Variables (IV) methods.

4.3 Data

The dependent variable, fdi_{it}, is net FDI inflows in constant US dollars from the UNCTAD Foreign Direct Investment database, divided by the population to control for country size. The main explanatory variables are the population growth rate and savings per capita from the WDI (2005).

The aid variables are based on total net flows of official aid disbursements reported in the OECD/DAC database. Since data on sectoral disbursements are available only after 1990, the measure of per capita aid flows to sector s, aid_{it}^{s}, is constructed using sectoral commitments as a proxy for sectoral disbursements. In particular, we follow Clemens *et al.* (2004) and Thiele *et al.* (2006) and assume that the proportion of aid actually disbursed to sector s is equal to the proportion of aid committed to sector s, and hence that

$$aid_{it}^{s} \approx \frac{commit_{it}^{s}}{\sum_s commit_{it}^{s}} \sum_s aid_{it}^{s}, \tag{16}$$

where $commit_{it}^{s}$ is the amount of ODA commitments to sector s. Approximating sectoral disbursements with sectoral commitments may cause some concerns due to differences in definitions and statistical record (see Clemens *et al.*, 2004, for more details). However, according to Odedokun (2003) and Clemens *et al.* (2004) this problem is likely to be small since disbursements and commitments (both on the aggregate and sectoral levels) are highly correlated. Also, annual discrepancies are likely to be larger than averages, and we thus average the data over five-year intervals.

Aid is decomposed into two broad categories according to its purpose of investment:

- Aid invested in complementary inputs: aid oriented to social infrastructure (such as education, health, and water supply projects) and economic infrastructure (such as energy, transportation and communications projects).

- Aid invested in physical capital: contributions to directly productive sectors (such as agriculture, manufacturing, trade, banking and tourism projects).

These two aid categories capture the main characteristics of aid_A and aid_K: aid invested in complementary factors is intended to generate positive spillover effects (public goods, inputs complementary to physical capital) whereas aid invested in physical capital has a more narrow purpose and could more easily have been undertaken by private investors. Other sectoral aid categories (like multisector support, programme assistance, debt reorganisation, emergency assistance and unallocated types of aid) are excluded from the analysis since they are primarily oriented to provide fiscal budget support in the recipient country.[5]

5 Results

Figure 1 in the Appendix shows the partial correlation between FDI and aid invested in physical capital. While there seems to be a negative relationship between the two variables, it is difficult to assess if there is full crowding out from the downwards sloping line (that is, to assess if the slope is -1). Figure 2 depicts the partial correlation between FDI and aid invested in complementary goods. The figure clearly indicates that the two variables are positively correlated and that the relationship might not be linear. However, the exact predictions from the theoretical model can only be tested in a more comprehensive framework where country-specific characteristics capture the cross-sectional heteroskedasticity clearly prevalent in the figures.

Results from estimating equations (13)–(15) for a sample of 84 countries using five-year intervals are reported in Table 1. Independently of the chosen estimator, our results strongly support the notion that aid invested in complementary factors has a catalysing effect on FDI. This means that the short-run replacement effect of aid_A on FDI is outweighed by the positive effect that complementary aid has on the long-run levels of income and capital per capita. A Hausman test confirms the significance of fixed effects, and the highly significant lagged dependent variable suggests that we should rely on the consistent and efficient Arellano and Bond (1991) GMM estimator in our further analysis. When the time series are persistent, the first-difference GMM (GMM-DIF) estimator is poorly behaved since under such conditions lagged levels of the variables are only weak instruments for subsequent first-differences. We therefore rely on the system GMM (GMM-SYS) estimator suggested by Arellano and Bover (1995) and Blundell and Bond (1998). All variables are treated as endogenous, which means that instruments should be lagged two periods or more to be valid.

The results in column (4) in Table 1 show that, for a given domestic savings rate, one aid dollar invested in complementary factors draws in 1.24 dollars of FDI, both in per

[5]Section 6 includes a test for robusteness of the results with respect to the definition of complementary aid, and a note about the changes in the results when variables possibly correlated with aid_A are included in the regressions.

Table 1: FDI and Foreign Aid

	(1) POLS	(2) FE	(3) GMM-DIF	(4) GMM-SYS	(5) GMM-SYS
aid_K	-0.59	-1.56^{***}	-0.77^{***}	-0.94^{***}	-0.88^{***}
	[0.8]	[0.3]	[0.2]	[0.2]	[0.2]
aid_A	1.67^{***}	1.71^{***}	1.34^{***}	1.24^{***}	1.07^{***}
	[0.5]	[0.2]	[0.2]	[0.2]	[0.2]
aid_A, squared	-0.0028^{***}	-0.0012^{*}	-0.0015^{***}	-0.0015^{***}	-0.0013^{***}
	[0.0006]	[0.0007]	[0.0002]	[0.0002]	[0.0002]
Savings, sy	29.7^{*}	-32.3	1.11	20.5^{***}	-20.2
	[16]	[24]	[23]	[7.8]	[17]
Pop. growth, n	-7.26^{**}	-0.97	1.05	-3.7	-2.19
	[3.5]	[1.4]	[1.6]	[2.5]	[1.8]
fdi_{t-1}			0.045	0.40^{***}	0.38^{***}
			[0.1]	[0.09]	[0.1]
GDP per capita					13.7^{***}
					[4.1]
Constant	12.9	.	.	5.04	1.14
	[14]	.	.	[8.5]	[6.0]
Observations	289	277	217	289	289
R^2	0.11	0.08	.	.	.
N. countries	84	72	76	84	84
Model specification tests:					
Hansen-Sargan overid.	(0.21)	(0.88)	(0.15)	(0.34)	(0.79)
Underid.	(0.0028)	(0.0)	.	.	.
Cragg-Donald F	(0.0021)	(0.0)	.	.	.
Anderson F joint sig F	(0.0)	(0.0)	.	.	.
DWH p	(0.071)	(0.0026)	.	.	.
AR(1)	.	.	(0.00)	(0.21)	(0.75)
AR(2)	.	.	(0.77)	.	.
Hypothesis tests on marginal effects evaluated at the median:					
ME of $aid_K = -1$	0.41	-0.56	0.23	0.06	0.12
	[0.83]	[0.30]	[0.19]	[0.25]	[0.21]
ME of $aid > 0$	0.96^{**}	0.10	0.50^{***}	0.24^{***}	0.13^{**}
	[0.52]	[0.37]	[0.13]	[0.09]	[0.07]
ME of $aid_A > 0$	1.55^{***}	1.66^{***}	1.27^{***}	1.18^{***}	1.02^{***}
	[0.51]	[0.18]	[0.16]	[0.22]	[0.22]

Notes. ***, **, and * denote significance at 1, 5, and 10% levels. Robust standard errors in brackets, p-values in parentheses. The dependent variable is FDI per capita. All regressions include time dummies. Aid variables are instrumented with own lags, interactions with GDP per capita, log(pop) and a FRZ dummy.

capita terms. The square of complementary aid is negative and significant, suggesting that the "savings" effect described in equation (9) dominates for sufficiently high levels of aid_A. Evaluated at the median of the sample, our results indicate that the marginal effect of aid_A on fdi is 1.18, and a Wald test confirms it to be significantly positive. Having specified a dynamic model, we can calculate the long run effect of aid_A by assuming a that the level of FDI per capita is the same in every period. Evaluating at the median, we find that one additional aid dollar per capita invested in complementary factors draws in 1.97 (1.18/0.6) dollars of FDI per capita in the long run. We conclude from this that aid_A generates important short run as well as long run benefits for foreign investors. The results also confirm the crowding out effect of aid invested in physical capital, since one aid dollar per capita invested in physical capital replaces 0.94 dollars of fdi, which accumulate to 1.57 dollars in the long run (0.94/0.6).

The effect of population growth is insignificant throughout the analysis. But, contrary to the prediction from our model, we find a positive rather than a negative effect of domestic savings on fdi. A plausible explanation is that foreign investors look explicitly at data on national savings when making their investment decisions and interpret a high s as a signal of sustained growth history and good economic prospects.[6] To adjust for this positive externality we include GDP per capita in column (5). Adjusting for the purchasing power of the population leaves savings insignificant and negative, which suggest that once we correct for the positive signalling effect of a high saving rate, domestic and foreign capital are substitutes as suggested by the theoretical model.

Finally, we perform some tests of hypothesis and present the results at the bottom of the Table. We test the Caselli and Feyrer (2007) conjecture that aid invested in physical capital replaces FDI one for one. The Wald tests show that we cannot reject its validity in most of the cases. We also find that the combined effect of aid_A and aid_K is significantly positive and between 0.21 and 0.24 (evaluated at the median of the sample), which implies that the substitution effect of aid_K is more than outweighed by the positive effects of aid_A on fdi in a typical country. If the marginal effects are evaluated at the mean instead of the median, our conclusions remain the same.

6 Robustness

In light of the important policy implications arising from our results, it is necessary to ensure that these results are robust to correcting for possible misspecifications in the empirical relationship between FDI and aid. We carry out three basic checks for robustness of our empirical findings.

[6]This is in line with evidence showing that the households with the highest lifetime incomes are the ones with highest lifetime saving rates (Carroll, 2000), and that higher growth rates lead to higher savings rates (Carroll, Overland and Weil, 2000; Loayza, Schmidt-Hebbel and Servén, 2000).

6.1 Technical Assistance

The grouping of aid variables could be questioned. In particular, aid in this paper does not include Technical Cooperation Grants (TCGs), which contribute to development primarily through education and training. Since TCGs consist of activities involving the supply of human resources or actions targeted on human resources (education, training, and advice) one could easily argue that TCGs would have the same impact as aid invested in complementary factors. In the Appendix (Table A1) we therefore replicate the specifications from Table 1 using an extended definition of aid_A that includes also TCGs from the OECD database. Although there is a slight drop in the size of the coefficients, the results from Table 1 carry over.

6.2 Imperfect Capital Mobility

If mobility of capital is imperfect, MPK should be allowed to deviate from the gross world interest rate by a risk-premium, ρ, that reflects idiosyncratic country characteristics. In this case, the first-order condition in (4) should read

$$r + \rho = \text{MPK}, \tag{17}$$

and the capital stock in (5) should be redefined accordingly:

$$k^* = \left[\frac{A\alpha}{r+\rho}\right]^{\frac{1}{1-\alpha}}. \tag{18}$$

While this renders the effect of aid invested in physical capital unchanged, the effect of complementary aid becomes somewhat more complicated. The risk premium impact FDI directly through (18) but, given that

$$\frac{\partial k^*}{\partial aid_A} = \frac{\partial}{\partial aid_A}\left(\left[\frac{A\alpha}{r+\rho}\right]^{\frac{1}{1-\alpha}}\right) = \frac{1}{1-\alpha}\left[\frac{A\alpha}{r+\rho}\right]^{\frac{\alpha}{1-\alpha}}\frac{L\alpha}{r+\rho}, \tag{19}$$

the marginal effect of aid_A will also depend on the risk premium and thus on country-specific characteristics. To capture this econometrically, we include the risk premium level and its interaction with aid_A, and estimate

$$\begin{aligned} fdi_{it} &= \alpha_t + \alpha_i + \beta_0 + \beta_1 n_{it} + \beta_2 S_{it} + \beta_3 aid_{it}^K + \beta_4 aid_{it}^A + \beta_5\left(aid_{it}^A\right)^2 \\ &\quad + \beta_6\rho_{it} + \beta_7\left(aid_{it}^A \times \rho_{it}\right) + u_{it}. \end{aligned} \tag{20}$$

β_6 and β_7 are expected to be negative because higher risk reduces country i's attractiveness as an investment location.

To capture the risk premium we include the overall International Country Risk

Table 2: FDI and Foreign Aid — Political Risk

Risk measure:	ICRG index (1)	Govt. stab. (2)	Socio-ec. condit. (3)	Investm. profile (4)	Internal conflict (5)	External conflict (6)	Political corrupt. (7)	Military in politics (8)	Religious tensions (9)	Law and Order (10)	Ethnic tensions (11)	Democ. account. (12)	Bureauc. quality (13)
		Political risk											
aid_K	−0.78•	−0.69**	−0.80•	−0.68**	−0.99•	−0.96•	−0.92•	−0.65**	−0.96•	−1.00•	−0.98•	−0.87•	−0.71**
	[0.3]	[0.3]	[0.2]	[0.3]	[0.2]	[0.3]	[0.3]	[0.3]	[0.2]	[0.2]	[0.3]	[0.3]	[0.3]
aid_A	0.34	0.48	0.41*	0.36	1.32•	1.30•	1.06*	0.71*	1.57•	1.24•	1.55•	1.14•	0.92**
	[0.7]	[0.6]	[0.2]	[0.5]	[0.2]	[0.5]	[0.6]	[0.4]	[0.2]	[0.4]	[0.3]	[0.3]	[0.4]
aid_A, squared	−0.0016•	−0.0015•	−0.0016•	−0.0015•	−0.0016•	−0.0015•	−0.0014•	−0.0015•	−0.0014•	−0.0016•	−0.0015•	−0.0014•	−0.0015•
	[0.0002]	[0.0002]	[0.0002]	[0.0002]	[0.0003]	[0.0002]	[0.0003]	[0.0002]	[0.0002]	[0.0002]	[0.0002]	[0.0002]	[0.0002]
$aid_A \times$Risk	0.014	0.094**	0.13•	0.11**	0.00088	−0.0025	0.062	0.13**	−0.083•	0.038	−0.056	0.013	0.14**
	[0.009]	[0.04]	[0.02]	[0.04]	[0.03]	[0.04]	[0.2]	[0.06]	[0.02]	[0.09]	[0.05]	[0.05]	[0.06]
Risk	−0.1	−4.56	−0.6	−0.6	−2.01	4.77*	0.83	−4.59	5.94*	−10.9*	0.8	8.58**	−4.21
	[0.6]	[4.0]	[3.3]	[3.6]	[1.9]	[2.6]	[7.0]	[3.5]	[3.1]	[5.7]	[3.6]	[4.2]	[4.5]
fdi_{t-1}	0.41•	0.41•	0.41•	0.43•	0.43•	0.42•	0.40•	0.45•	0.39•	0.43•	0.42•	0.42•	0.39•
	[0.10]	[0.09]	[0.10]	[0.09]	[0.09]	[0.09]	[0.1]	[0.10]	[0.1]	[0.10]	[0.10]	[0.1]	[0.09]
Savings, sy	16.3*	21.7•	21.2**	20.4•	24.0•	18.2**	22.0**	19.5**	26.0•	26.7•	22.1**	21.2•	22.2**
	[8.2]	[7.9]	[10]	[7.4]	[7.8]	[8.0]	[8.9]	[7.9]	[8.4]	[8.4]	[8.6]	[6.5]	[8.9]
Pop. gr., n	−4.99*	−4.23	−8.74•	−5.79**	−5.61**	−5.30**	−6.03**	−5.13*	−7.10**	−6.29**	−7.98•	−4.09*	−6.11**
	[2.8]	[2.5]	[3.1]	[2.6]	[2.8]	[2.6]	[2.9]	[2.9]	[2.8]	[2.9]	[3.0]	[2.5]	[2.7]
Observations	233	231	231	231	231	231	231	231	231	231	231	231	231
N. Countries	72	72	72	72	72	72	72	72	72	72	72	72	72
Sargan test	(1.00)	(0.98)	(0.97)	(0.99)	(0.98)	(0.97)	(0.99)	(0.98)	(0.97)	(0.96)	(0.98)	(0.96)	(0.99)
AR(1)	(0.91)	(0.81)	(0.53)	(0.79)	(0.69)	(0.76)	(0.70)	(0.98)	(0.43)	(0.59)	(0.59)	(0.71)	(0.91)
Hypothesis tests on marginal effects evaluated at the median:													
ME $aid_K = -1$	0.22	0.31	0.20	0.32	0.01**	0.04	0.08	0.35	0.04	0.00•	0.02*	0.13	0.29
	[0.26]	[0.34]	[0.20]	[0.28]	[0.23]	[0.28]	[0.28]	[0.31]	[0.23]	[0.25]	[0.25]	[0.26]	[0.34]
ME of $aid > 0$	0.35•	0.48•	0.22•	0.29•	0.27•	0.26**	0.26•	0.40•	0.14**	0.29•	0.28•	0.25•	0.43•
	[0.09]	[0.14]	[0.05]	[0.06]	[0.07]	[0.14]	[0.11]	[0.12]	[0.08]	[0.11]	[0.08]	[0.08]	[0.12]
ME $aid_A > 0$	1.12•	1.16•	1.02•	0.97•	1.26•	1.21•	1.19•	1.04•	1.10•	1.28•	1.26•	1.12•	1.13•
	[0.22]	[0.23]	[0.21]	[0.26]	[0.23]	[0.24]	[0.25]	[0.24]	[0.25]	[0.21]	[0.25]	[0.25]	[0.26]

Notes. Robust standard errors in brackets and p-values in parentheses. • p<0.01, ** p<0.05, * p<0.1. The dependent variable is FDI per capita. All regressions include a constant and time dummies. External instruments for the aid variables are interactions with log(population), GDP per capita and a FRZ dummy.

Guide rating as well as its three subcategories of risk: political, financial and economic.[7] All risk variables are treated as endogenous. In general, lower political risk is associated with higher levels of overall accountability, stability and institutional quality in the political process. In particular, from the ICRG rankings, political risk is lower (1) the higher the government stability, (2) the better the socioeconomic conditions and the investment profile, (3) the lower the number of internal conflicts, external conflicts and political corruption, (4) the lower the military is involved in politics, (5) the lower the religious and the ethnic tensions, (6) the higher the prevalence of law and order, and (7) the larger the degrees of democratic accountability and bureaucratic quality. Results from estimating (20) including these political risk measures are reported in Table 2.[8]

The political risk variable enters only significantly in four cases. Relative absence of external conflict, low level of religious tensions and a high level of democratic accountability suggest all a lower risk premium and tend to attract foreign investors. However, the prevalence of law and order shows a negative impact on FDI inflows (significant only at the 10% level, though). This counter intuitive result might be due to the fact that we have already accounted for domestic savings, which will be highly correlated with this risk variable: countries characterised by high prevalence of law and order tend to have higher domestic saving.

The interactions between complementary aid and the political risk indicator are more often significant, and the results suggest that government stability, favourable socioeconomic conditions, an attractive investment profile, low military interference in politics and better bureaucratic quality are all supportive of a high steady-state level of capital. Although the results show a negative impact of the interaction between aid_A and the index for low degree of religious tensions, the net marginal effect on FDI remains positive.

Table 3 presents similar estimations taking into account different economic and financial risk measures. The economic risk variables reflect the macroeconomic situation and the economic advancement of the host country: GDP per capita, real GDP growth, inflation, the budget balance as a share of GDP and the current account as a share of GDP. The financial risk variables assess a country's ability to finance its official, commercial and trade debt obligations: external debt as a share of GDP, debt service as a share of exports, the current account as a share of export, international liquidity as months of import cover and exchange rate stability (calculated here as the annual change in the real exchange rate).[9] Results in Table 3 keep our overall conclusions un-

[7]In order to detect significant effects of aid on FDI, Karakaplan et al. (2005) and Harms and Lutz (2006) use aid interacted with the Kaufmann et al. (2005) governance indicators to capture differences in government effectiveness.

[8]For the results in Table 2, a high value of the different political-risk measures is associated with a low overall political risk, and hence, a high value of the different risk measures should have a positive effect on fdi.

[9]Similar to the case of the political risk indexes, all these different measures reflect lower overall levels of economic and financial risk.

Table 3: FDI and Foreign Aid — Economic and Financial Risks

	Economic risk					Financial risk				
Risk measure:	GDP per capita (1)	GDP growth (2)	Inflation rate (3)	Budget balance (4)	Curr. Acc. balance (5)	Foreign debt to GDP (6)	Foreign debt service to exp. (7)	Curr. Acc. to exports (8)	Reserves to imp. months (9)	Exch. rate stability (10)
aid_K	−0.94***	−0.89***	−0.92***	−1.14***	−0.81***	−0.89***	−0.94***	−0.89***	−0.93***	−0.95***
	[0.2]	[0.2]	[0.3]	[0.2]	[0.3]	[0.2]	[0.3]	[0.3]	[0.2]	[0.3]
aid_A	1.14***	1.06***	1.25***	1.32***	1.17***	1.52***	1.28***	1.20***	1.09***	1.29***
	[0.2]	[0.2]	[0.2]	[0.2]	[0.2]	[0.3]	[0.3]	[0.2]	[0.3]	[0.2]
aid_A, squared	−0.0014***	−0.0014***	−0.0015***	−0.0015***	−0.0016***	−0.0015***	−0.0015***	−0.0015***	−0.0014***	−0.0015***
	[0.0002]	[0.0002]	[0.0002]	[0.0001]	[0.0002]	[0.0002]	[0.0002]	[0.0002]	[0.0002]	[0.0002]
$aid_A \times$Risk	0.019	0.028***	−0.23	−2.53***	−0.027*	−0.39**	0.0023	−0.58	0.042	−0.38
	[0.03]	[0.009]	[0.2]	[0.5]	[0.01]	[0.2]	[0.005]	[0.7]	[0.04]	[0.3]
Risk	11.9***	−1.62	9.16	13	0.46	17.0**	0.45	11.9	−6.20***	13.7
	[4.5]	[1.1]	[7.7]	[101]	[0.6]	[7.4]	[0.4]	[20]	[1.3]	[9.8]
fdi_{t-1}	0.39***	0.41***	0.43***	0.30***	0.43***	0.41***	0.47***	0.41***	0.39***	0.44***
	[0.1]	[0.1]	[0.10]	[0.06]	[0.1]	[0.1]	[0.1]	[0.1]	[0.1]	[0.10]
Savings, sy	−17.3	24.2***	19.8**	18.2**	19.8***	22.8**	14.5	20.7**	27.7***	19.0**
	[18]	[8.9]	[7.9]	[8.1]	[7.4]	[9.0]	[9.4]	[8.2]	[9.0]	[7.5]
Pop. gr., n	−4.28*	−6.72**	−5.72*	−7.85***	−6.71**	−9.21***	−4.85*	−6.11**	−7.38**	−5.18*
	[2.3]	[2.6]	[2.9]	[2.8]	[3.1]	[3.3]	[2.6]	[2.9]	[2.8]	[2.8]
Observations	233	233	229	203	223	229	219	223	218	233
N. Countries	72	72	71	65	72	70	70	72	70	72
Sargan test	(0.00)	(1.00)	(1.00)	(0.00)	(1.00)	(0.00)	(1.00)	(0.00)	(1.00)	(1.00)
AR(1)	(0.51)	(0.50)	(0.90)	(0.63)	(0.88)	(0.72)	(0.84)	(0.82)	(0.73)	(0.91)
Hypothesis tests on marginal effects evaluated at the median:										
ME $aid_K = -1$	0.06	0.11	0.08	−0.14	0.19	0.11	0.06	0.11	0.07	0.05
	[0.20]	[0.22]	[0.26]	[0.16]	[0.25]	[0.22]	[0.25]	[0.26]	[0.22]	[0.26]
ME of $aid > 0$	0.15*	0.21***	0.25***	0.17***	0.37***	0.34***	0.32***	0.32***	0.25***	0.24***
	[0.11]	[0.05]	[0.09]	[0.06]	[0.10]	[0.07]	[0.07]	[0.10]	[0.07]	[0.09]
ME of $aid_A > 0$	1.10***	1.09***	1.17***	1.31***	1.19***	1.23***	1.26***	1.20***	1.17***	1.19***
	[0.19]	[0.22]	[0.21]	[0.15]	[0.20]	[0.22]	[0.23]	[0.22]	[0.20]	[0.22]

Notes. Robust standard errors in brackets and p-values in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is FDI per capita. All regressions include a constant and time dummies. External instruments for the aid variables are interactions with log(population), GDP per capita and a FRZ dummy.

changed. It is interesting to note, however, that the political risk variables seem to be more important to foreign investors than the economic and financial risk variables.

6.3 Omitted Variables

Tables 2 and 3 show a positive impact from the savings rate on fdi. We adjust for this in Tables 4 and 5 including the level of GDP per capita in the regressions. As in Table 1, the effect of savings disappears and it is captured by the level of GDP per capita, which supports our results previously suggesting the existence of positive externalities from s to fdi.

However, it is important to notice that once we adjust for the risk of investing abroad by including various proxies for the risk premium, population growth turns out to have a significantly *negative* impact on fdi in both Tables 2 and 3. One explanation might be that a fast growing population is attractive for the efficiency-seeking investor but that the quality of the abundant labour in some countries might be too poor to attract foreign investors. In this case, a fast growing population might instead cause social tensions and excessive burdens on the public system, which will tend to scare away foreign investors rather than draw in more investments.[10] We therefore add the primary school enrolment rate from the World Development Indicators (2005) in Tables 4 and 5, to take the quality of the labour force and the level of development into account.[11] In many cases, the adjustment for the quality of the labour force means that population growth no longer enters significant and in the remaining cases it reduces the size of the initially negative effect on fdi. It is interesting noticing that the adjustment for the level of human capital reduces the size of the effect of aid_A on fdi. This means that the aid_A variable is picking up the information that we intend, and thus substantiates our choice and definition of different types of aid.

Finally, while our empirical specification includes both variables predicted by our theoretical model as well as a rich specification of idiosyncratic country characteristics, there might be additional variables that play a role in the allocation choice of foreign investors. To test for this, further regressions included measures of market potential (regional dummies, urban population and rural population), factor market characteristics (size of the labour force, average years of schooling) and market access (openness, number of vehicles, transportation network density, telephone lines and rail lines). None of them turned out significant or to have a qualitative impact on our results. These results are available upon request.

[10]This is in line with Mankiw, Romer and Weil's (1992) point that a higher population growth rate implies lower per capita human capital levels and thus lower MPK levels. This will have a negative impact on FDI.

[11]The data on school enrolment is highly unbalanced, so we interpolated within countries to fill in gaps, and extended the series with the first and the last values to complete the extremes. The correlation between the original and the transformed series is above 0.98 in both cases.

Table 4: FDI and Foreign Aid — Political Risk (extended model)

Risk measure:	ICRG index (1)	Political risk: Govt. stab. (2)	Socio-ec. condit. (3)	Investm. profile (4)	Internal conflict (5)	External conflict (6)	Political corrupt. (7)	Military in politics (8)	Religious tensions (9)	Law and Order (10)	Ethnic tensions (11)	Democ. account. (12)	Bureauc. quality (13)
aid_K	−0.77•	−0.66**	−0.78•	−0.66•	−0.93•	−0.93•	−0.89•	−0.68**	−0.93•	−1.00•	−0.91•	−0.85•	−0.71**
	[0.2]	[0.3]	[0.1]	[0.2]	[0.2]	[0.2]	[0.2]	[0.3]	[0.2]	[0.2]	[0.2]	[0.2]	[0.3]
aid_A	0.31	0.43	0.37*	0.28	1.16•	1.22•	1.03*	0.70**	1.43•	1.28•	1.37•	1.05•	0.85**
	[0.6]	[0.5]	[0.2]	[0.4]	[0.2]	[0.4]	[0.6]	[0.3]	[0.2]	[0.3]	[0.3]	[0.3]	[0.3]
aid_A, squared	−0.0015•	−0.0014•	−0.0015•	−0.0014•	−0.0014•	−0.0014•	−0.0013•	−0.0014•	−0.0014•	−0.0015•	−0.0014•	−0.0014•	−0.0014•
	[0.0002]	[0.0002]	[0.0002]	[0.0002]	[0.0002]	[0.0002]	[0.0003]	[0.0002]	[0.0002]	[0.0002]	[0.0002]	[0.0002]	[0.0002]
$aid_A \times$Risk	0.013	0.082**	0.13•	0.10•	0.0028	−0.0059	0.032	0.11*	−0.064•	−0.018	−0.045	0.016	0.12**
	[0.009]	[0.03]	[0.02]	[0.03]	[0.03]	[0.03]	[0.2]	[0.06]	[0.02]	[0.07]	[0.04]	[0.05]	[0.06]
Risk	−0.25	−4.02	0.22	−1.81	−3.19	2.5	−1.06	−6.69**	2.43	−10.9**	−6.71*	5.48	−7.14*
	[0.6]	[3.2]	[2.5]	[3.1]	[2.1]	[2.4]	[4.4]	[3.3]	[2.9]	[4.4]	[3.9]	[3.4]	[4.2]
fdi_{t-1}	0.39•	0.39•	0.39•	0.41•	0.41•	0.40•	0.39•	0.43•	0.39•	0.43•	0.40•	0.41•	0.38•
	[0.1]	[0.1]	[0.1]	[0.1]	[0.1]	[0.1]	[0.1]	[0.1]	[0.1]	[0.1]	[0.1]	[0.1]	[0.1]
Savings, sy	−18.1	−5.86	−16.8	−10.7	−10.7	−14.8	−14.1	−12.4	−8.53	−9.07	−19.7	−7.25	−7.2
	[18]	[17]	[20]	[17]	[17]	[18]	[19]	[18]	[20]	[17]	[18]	[18]	[19]
Pop. gr., n	−3.8	−3.37	−8.15•	−4.85*	−4.05	−5.30*	−4.58	−4.71	−6.15**	−5.99**	−6.33*	−4.54*	−5.02*
	[2.6]	[2.5]	[2.9]	[2.6]	[2.8]	[2.9]	[3.0]	[2.9]	[2.9]	[3.0]	[3.2]	[2.5]	[2.8]
GDP p. cap.	11.8**	8.75**	12.1•	10.3**	11.7•	11.6•	11.7•	11.3**	10.9**	12.5•	15.2•	9.53**	9.81**
	[4.6]	[4.1]	[4.4]	[4.2]	[4.0]	[4.3]	[4.1]	[5.0]	[4.5]	[4.0]	[4.8]	[4.4]	[4.6]
Prim. school.	0.061	0.18	−0.015	0.096	0.14	−0.031	0.12	0.14	0.012	−0.0003	0.085	−0.037	0.18
	[0.2]	[0.2]	[0.2]	[0.2]	[0.2]	[0.2]	[0.2]	[0.2]	[0.2]	[0.2]	[0.2]	[0.2]	[0.2]
Observations	233	231	231	231	231	231	231	231	231	231	231	231	231
N. Countries	72	72	72	72	72	72	72	72	72	72	72	72	72
Sargan test	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)	(0.67)	(1.00)	(0.00)	(1.00)	(1.00)	(1.00)
AR(1)	(0.36)	(0.29)	(0.44)	(0.40)	(0.73)	(0.57)	(0.62)	(0.37)	(0.88)	(0.95)	(0.81)	(0.67)	(0.49)
Hypothesis tests on marginal effects evaluated at the median:													
ME $aid_K = -1$	0.23	0.34	0.22	0.34	0.07	0.07	0.11	0.32	0.07	0.00•	0.09	0.15	0.29
	[0.21]	[0.29]	[0.15]	[0.22]	[0.19]	[0.24]	[0.25]	[0.27]	[0.20]	[0.23]	[0.21]	[0.20]	[0.28]
ME of $aid > 0$	0.27•	0.37•	0.15•	0.21•	0.19•	0.17*	0.17**	0.29•	0.12**	0.17**	0.22•	0.20•	0.32•
	[0.09]	[0.12]	[0.06]	[0.06]	[0.08]	[0.12]	[0.11]	[0.13]	[0.07]	[0.10]	[0.07]	[0.07]	[0.12]
ME $aid_A > 0$	1.03•	1.03•	0.93•	0.87•	1.12•	1.10•	1.06•	0.97•	1.05•	1.16•	1.13•	1.05•	1.03•
	[0.19]	[0.22]	[0.19]	[0.23]	[0.21]	[0.22]	[0.22]	[0.22]	[0.23]	[0.20]	[0.22]	[0.21]	[0.24]

Notes. Robust standard errors in brackets and p-values in parentheses. • p<0.01, ** p<0.05, * p<0.1. The dependent variable is FDI per capita. All regressions include a constant and time dummies. External instruments for the aid variables are interactions with log(population), GDP per capita and a FRZ dummy.

Table 5: FDI and Foreign Aid — Economic and Financial Risks (extended model)

	Economic risk					Financial risk				
Risk measure:	GDP per capita (1)	GDP growth (2)	Inflation rate (3)	Budget balance (4)	Curr. Acc. balance (5)	Foreign debt to GDP (6)	Foreign debt service to exp. (7)	Curr. Acc. to exports (8)	Reserves to imp. months (9)	Exch. Rate stability (10)
aid_K	−0.92***	−0.87***	−0.92***	−1.06***	−0.80***	−0.89***	−0.96***	−0.89***	−0.91***	−0.95***
	[0.2]	[0.2]	[0.2]	[0.2]	[0.2]	[0.2]	[0.2]	[0.2]	[0.2]	[0.2]
aid_A	1.10***	1.00***	1.17***	1.18***	1.07***	1.37***	1.24***	1.11***	1.02***	1.18***
	[0.2]	[0.2]	[0.2]	[0.2]	[0.2]	[0.2]	[0.3]	[0.2]	[0.3]	[0.2]
aid_A, squared	−0.0014***	−0.0013***	−0.0014***	−0.0014***	−0.0015***	−0.0014***	−0.0014***	−0.0014***	−0.0013***	−0.0014***
	[0.0002]	[0.0002]	[0.0002]	[0.0001]	[0.0002]	[0.0002]	[0.0002]	[0.0002]	[0.0002]	[0.0002]
$aid_A \times$Risk	0.021	0.016**	−0.34**	−1.81***	−0.029**	−0.32**	−0.0026	−0.42	0.035	−0.52**
	[0.03]	[0.008]	[0.2]	[0.6]	[0.01]	[0.1]	[0.005]	[0.5]	[0.04]	[0.2]
Risk	11.8***	0.74	1.05	32.1	0.75	13.9**	0.2	13.4	−5.34***	0.92
	[4.3]	[0.7]	[5.9]	[76]	[0.5]	[6.8]	[0.3]	[16]	[1.2]	[5.9]
fdi_{t-1}	0.39***	0.39***	0.42***	0.27***	0.42***	0.39***	0.46***	0.40***	0.38***	0.42***
	[0.1]	[0.1]	[0.1]	[0.06]	[0.1]	[0.1]	[0.1]	[0.1]	[0.1]	[0.1]
Savings, sy	−17.2	−19.3	−19.4	−18.8	−21.1	−11.6	−16.9	−18.8	−6.7	−21.5
	[18]	[18]	[18]	[18]	[15]	[19]	[17]	[17]	[14]	[17]
Pop. gr., n	−4.19	−4.90*	−3.92	−5.74**	−4.32	−7.85**	−5.51*	−4.18	−5.07*	−4.13
	[2.8]	[2.6]	[2.5]	[2.6]	[2.7]	[3.2]	[2.8]	[2.6]	[2.8]	[2.5]
GDP per capita		13.9***	12.8***	11.2**	13.0***	11.9**	12.5***	12.8***	10.8***	13.3***
		[4.1]	[4.3]	[4.4]	[3.6]	[4.8]	[3.8]	[3.9]	[3.2]	[4.2]
Prim. schooling	0.036	0.0092	0.063	0.17	0.016	−0.052	−0.11	−0.026	0.13	0.099
	[0.2]	[0.2]	[0.1]	[0.2]	[0.2]	[0.2]	[0.2]	[0.2]	[0.2]	[0.1]
Observations	233	233	229	203	223	229	219	223	218	233
N. Countries	72	72	71	65	72	70	70	72	70	72
Sargan test	(1.00)	(1.00)	(1.00)	(0.00)	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)	(1.00)
AR(1)	(0.49)	(0.66)	(0.36)	(0.95)	(0.28)	(0.62)	(0.39)	(0.48)	(0.66)	(0.35)
Hypothesis tests on marginal effects evaluated at the median:										
ME of $aid_K = -1$	0.08	0.13	0.08	−0.06	0.20	0.11	0.04	0.11	0.09	0.06
	[0.20]	[0.18]	[0.21]	[0.16]	[0.19]	[0.18]	[0.22]	[0.21]	[0.20]	[0.21]
ME of $aid > 0$	0.14	0.13***	0.16***	0.11**	0.29***	0.24***	0.17***	0.21***	0.16***	0.13**
	[0.11]	[0.05]	[0.07]	[0.06]	[0.08]	[0.08]	[0.06]	[0.07]	[0.06]	[0.07]
ME of $aid_A > 0$	1.07***	1.00***	1.08***	1.17***	1.10***	1.12***	1.13***	1.11***	1.08***	1.07***
	[0.19]	[0.21]	[0.20]	[0.16]	[0.18]	[0.20]	[0.22]	[0.20]	[0.20]	[0.20]

Notes. Robust standard errors in brackets and p-values in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is FDI per capita. All regressions include a constant and time dummies. External instruments for the aid variables are interactions with log(population), GDP per capita and a FRZ dummy.

7 Conclusion

Due to its potential to transfer knowledge and technology, create jobs, boost overall productivity, and enhance competitiveness and entrepreneurship, attracting FDI to developing countries is essential to contribute to economic growth, development and poverty reduction. Given the emphasis on using ODA as a vehicle for creating a private sector enabling environment, the question of whether or not aid flows induce significantly more FDI inflows becomes an important and relevant question not only on its own right but also as an essential element in the aid effectiveness debate.

The results strongly support the hypotheses that aid invested in inputs complementary to physical capital draws in foreign capital, while aid directly invested in physical capital crowds out private foreign investments. While the impact of the two types of aid together is positive, an important policy implication is that the composition of foreign aid matters and that more aid should be directed towards complementary inputs. Such investments improve the absorption capacity of the recipient country and increase MPK in the host country, which allows it to accumulate more foreign capital without experiencing a drop in domestic investments or a flight of foreign capital.

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Appendix: Figures and Tables

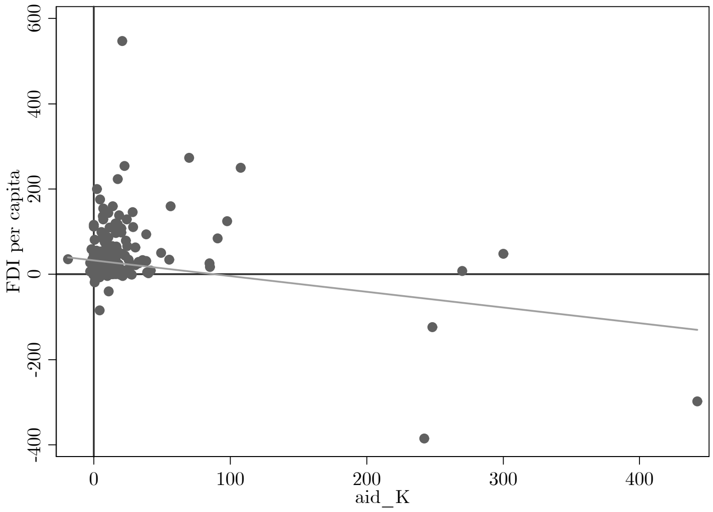

Figure 1: FDI and Aid to Physical Capital (aid_K)

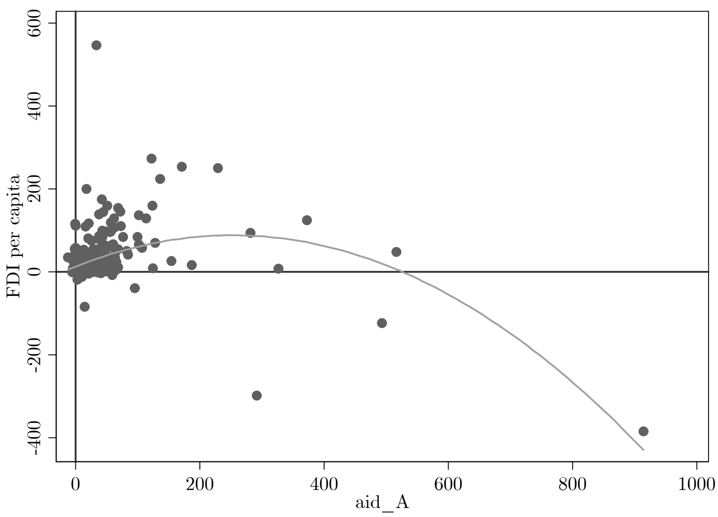

Figure 2: FDI and Aid to Complementary Factors (aid_A)

Table A1: FDI and Foreign Aid - Alternative Definition of aid_A

	(1) POLS	(2) FE	(3) GMM-DIF	(4) GMM-SYS	(5) GMM-SYS
aid_K	-0.29	-1.47^{***}	-0.71^{***}	-0.75^{***}	-0.74^{***}
	[0.8]	[0.2]	[0.2]	[0.3]	[0.2]
$aid_A^{\dagger}$	1.09^{***}	1.65^{***}	1.33^{***}	0.97^{***}	0.87^{***}
	[0.4]	[0.2]	[0.2]	[0.2]	[0.2]
$aid_A^{\dagger}$, squared	-0.0020^{***}	-0.0012^{**}	-0.0015^{***}	-0.0012^{***}	-0.0011^{***}
	[0.0004]	[0.0006]	[0.0002]	[0.0002]	[0.0002]
Savings, sy	38.5^{**}	-25.8	6.62	26.5^{**}	-20.3
	[17]	[22]	[20]	[10]	[18]
Pop. growth, n	-8.91^{**}	-1.7	0.52	-5.50^{*}	-3.18
	[4.1]	[1.4]	[1.4]	[3.2]	[2.3]
fdi_{t-1}			0.018	0.37^{***}	0.36^{***}
			[0.1]	[0.1]	[0.1]
GDP per capita					15.4^{***}
					[4.3]
Constant	18.8	.	.	5.73	-0.99
	[13]	.	.	[10]	[6.7]
Observations	289	277	217	289	289
R^2	0.11	0.08	.	.	.
N. countries	84	72	76	84	84
Model specification tests:					
Hansen-Sargan overid.	(0.12)	(0.53)	(0.11)	(0.30)	(0.86)
Underid.	(0.0017)	(0.0)	.	.	.
Cragg-Donald F	(0.0013)	(0.0)	.	.	.
Anderson F joint sig F	(0.0)	(0.0)	.	.	.
DWH p	(0.17)	(0.0018)	.	.	.
AR(1)	.	.	(0.00)	(0.12)	(0.54)
AR(2)	.	.	(0.69)	.	.
Hypothesis tests on marginal effects evaluated at the median:					
ME of $aid_K = -1$	0.71	-0.47	0.29	0.25	0.26
	[0.80]	[0.24]	[0.19]	[0.28]	[0.22]
ME of $aid > 0$	0.71	0.13	0.56^{***}	0.17^{*}	0.07
	[0.57]	[0.33]	[0.15]	[0.11]	[0.08]
ME of $aid_A^{\dagger} > 0$	1.00^{***}	1.60^{***}	1.26^{***}	0.92^{***}	0.82^{***}
	[0.35]	[0.14]	[0.16]	[0.23]	[0.21]

Notes. *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors in brackets, p-values in parentheses. The dependent variable is FDI per capita. All regressions include time dummies. Aid variables are instrumented with own lags, interactions with GDP per capita, log(pop) and a FRZ dummy. $aid_A^{\dagger}$ is defined as aid_A + technical cooperation grants.

Table A2: Partial Correlations - Main Variables

	fdi	aid_K	aid_A	$aid_A^\dagger$	n	sy
fdi	1					
aid_K	−0.24	1				
aid_A	−0.16	0.79	1			
$aid_A^\dagger$	−0.15	0.78	0.99	1		
n	−0.24	0.03	−0.05	−0.02	1	
sy	0.35	0.12	0.19	0.17	−0.18	1

Table A3: Partial Correlations - Economic Risk Measures

	GDP per capita	GDP growth	Inflation rate	Budget balance	Curr. Acc. balance
GDP per capita	1				
GDP growth	−0.08	1			
Inflation rate	0.14	−0.23	1		
Budget balance	0.12	0.18	−0.20	1	
Curr. Acc. balance	0.22	0.19	−0.24	0.25	1

Table A4: Partial Correlations - Financial Risk Measures

	Foreign debt	For. debt service	Curr. Acc. to exports	Reserves to imp. months	Exch. R. stab.
Foreign debt	1				
For. debt service	0.14	1			
Curr. Acc. to exports	−0.56	−0.17	1		
Reserves to imp. months	−0.24	−0.05	0.22	1	
Exch. R. stab.	0.30	0.28	−0.26	−0.01	1

Table A5: Partial Correlations - Political Risk Measures

	ICRG index	Govt. stab.	Socio-ec. condit.	Investm. profile	Internal conflict	External conflict	Political corrup.	Military in politics	Religious tensions	Law and Order	Ethnic tensions	Democ. account.	Bureauc. quality
ICRG index	1												
Govt. stab.	0.68	1											
Socio-ec. condit.	0.36	−0.10	1										
Investm. profile	0.70	0.66	0.29	1									
Internal conflict	0.72	0.51	0.25	0.48	1								
External conflict	0.53	0.35	0.03	0.32	0.46	1							
Political corrup.	0.30	0.10	0.19	0.14	0.29	0.05	1						
Military in politics	0.55	0.31	0.19	0.40	0.52	0.26	0.42	1					
Religious tensions	0.34	0.17	0.04	0.21	0.39	0.32	0.29	0.30	1				
Law and Order	0.63	0.48	0.19	0.40	0.65	0.25	0.35	0.42	0.23	1			
Ethnic tensions	0.47	0.28	0.06	0.23	0.57	0.27	0.33	0.37	0.41	0.42	1		
Democ. account.	0.33	0.18	−0.05	0.25	0.21	0.28	0.35	0.44	0.14	0.16	0.20	1	
Bureauc. quality	0.47	0.23	0.32	0.30	0.29	0.11	0.40	0.47	0.04	0.34	0.19	0.35	1

Table A6: Summary statistics

	Obs	Median	Mean	Std. dev.	Min.	Max
Main variables:						
fdi	289	9.8	27.2	64.1	−384.9	547.0
aid_K	289	6.8	15.9	41.9	−18.7	442.1
aid_A	289	21.6	40.1	79.6	−12.2	914.4
$aid_A^\dagger$	287	32.5	50.3	82.2	−7.0	926.0
n	289	2.3	2.2	1.0	−5.1	7.0
sy	289	0.1	0.3	0.5	−0.4	3.2
Political risk measures:						
ICRG index	233	60.3	60.1	10.3	27.6	80.6
Govt. stab.	232	8.0	7.6	2.2	2.3	12.0
Socio-ec. condit.	232	5.0	4.9	1.5	1	9
Investm. profile	232	6.1	6.3	1.8	1.2	11
Internal conflict	232	8.2	7.8	2.4	0.4	12.0
External conflict	232	9.8	9.4	2.0	2.3	12.0
Political corrup.	232	3.0	2.8	0.9	0	5
Military in politics	232	3.0	3.1	1.5	0	6
Religious tensions	232	5.0	4.3	1.4	0	6
Law and Order	232	3.0	3.2	1.1	1	6
Ethnic tensions	232	4.0	3.9	1.4	0	6
Democ. account.	232	3.3	3.3	1.2	0	6
Bureauc. quality	232	2.0	1.7	0.9	0	3.5
Economic risk measures:						
GDP per capita	289	1.1	1.5	1.7	0.1	9.1
GDP growth	289	3.7	3.7	3.1	−11.5	15.7
Inflation rate	280	0.1	0.2	0.3	0.0	2.8
Budget balance	242	0.0	0.0	0.0	−0.4	0.2
Curr. Acc. balance	274	−3.1	−3.2	5.9	−31.2	20.1
Financial risk measures:						
Foreign debt	285	0.6	0.7	0.6	0.1	7.0
For. debt service	270	18.8	19.7	13.5	1.3	84.5
Curr. Acc. to exports	274	−0.1	−0.2	0.3	−1.6	0.4
Reserves to imp. months	268	3.3	3.8	3.1	0.1	26.0
Exch. R. stab.	289	0.1	0.2	0.4	−1.7	2.8

Table A7: Sample

		75-79	80-84	85-89	90-94	95-99	00-01			75-79	80-84	85-89	90-94	95-99	00-01
ALB	Albania						×	MNG	Mongolia					×	×
ARG	Argentina		×	×	×	×	×	MOZ	Mozambique				×	×	×
ARM	Armenia						×	MRT	Mauritania				×	×	×
BDI	Burundi				×	×	×	MUS	Mauritius				×	×	×
BEN	Benin		×	×	×	×	×	MWI	Malawi						×
BFA	Burkina Faso	×	×	×	×	×		MYS	Malaysia			×	×	×	×
BGD	Bangladesh					×	×	NAM	Namibia					×	×
BGR	Bulgaria						×	NER	Niger	×	×	×	×	×	×
BOL	Bolivia			×	×	×	×	NGA	Nigeria		×			×	×
BRA	Brazil	×	×	×	×	×	×	NIC	Nicaragua			×	×	×	×
BWA	Botswana		×	×	×	×	×	NPL	Nepal	×	×	×	×	×	×
CAF	Central African Rep.		×	×	×	×	×	OMN	Oman					×	×
CHL	Chile		×	×	×	×	×	PAK	Pakistan	×	×	×	×	×	
CHN	China			×	×	×	×	PAN	Panama			×	×	×	×
CIV	Côte d'Ivoire				×	×		PER	Peru			×	×	×	×
CMR	Cameroon	×	×	×	×	×	×	PHL	Philippines		×	×	×	×	×
COG	Congo, Rep.				×	×	×	PRY	Paraguay		×	×	×	×	×
COL	Colombia	×	×	×	×	×	×	ROM	Romania					×	
CRI	Costa Rica		×	×	×	×	×	RUS	Russia					×	
DOM	Dominican Republic					×	×	RWA	Rwanda		×	×	×	×	×
DZA	Algeria	×	×	×	×	×	×	SAU	Saudi Arabia					×	×
ECU	Ecuador			×	×	×		SDN	Sudan	×	×	×		×	×
EGY	Egypt		×	×	×	×		SEN	Senegal	×	×	×	×	×	×
ETH	Ethiopia		×	×	×	×	×	SLV	El Salvador					×	×
GHA	Ghana					×	×	SYR	Syria				×	×	×
GTM	Guatemala			×	×	×	×	TCD	Chad		×	×	×	×	×
HND	Honduras			×	×	×	×	TGO	Togo				×	×	×
HRV	Croatia						×	THA	Thailand		×	×	×	×	×
HTI	Haiti			×	×	×	×	TJK	Tajikistan					×	
IDN	Indonesia			×	×	×	×	TTO	Trinidad & Tobago						×
IND	India	×	×	×	×	×	×	TUN	Tunisia	×	×	×	×	×	×
IRN	Iran					×	×	TUR	Turkey			×	×	×	×
JAM	Jamaica					×	×	TZA	Tanzania				×	×	×
JOR	Jordan	×	×	×	×	×	×	UGA	Uganda					×	×
KAZ	Kazakstan					×	×	UKR	Ukraine						×
KEN	Kenya		×	×	×	×	×	URY	Uruguay						×
KHM	Cambodia						×	UZB	Uzbekistan					×	×
LAO	Laos					×	×	VEN	Venezuela				×	×	×
LKA	Sri Lanka				×	×	×	VNM	Viet Nam			×	×	×	
MAR	Morocco				×	×	×	YEM	Yemen				×	×	×
MEX	Mexico		×	×	×	×	×	ZAF	South Africa					×	×
MLI	Mali				×	×	×	ZWE	Zimbabwe			×	×	×	×

Aid and Sectoral Labor Productivity*

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December 5, 2008

Abstract

The paper examines empirically the proposition that aid to poor countries is detrimental for external competitiveness, giving rise to Dutch disease type effects. At the aggregate level, aid is found to have a positive effect on growth of labour productivity. A sectoral decomposition shows that the effect is significant and positive both in the tradables and the nontradables sectors. The paper thus finds no empirical support for the hypothesis that aid reduces external competitiveness in developing countries. Possible reasons are the existence of large idle labour capacity and high levels of dollarization in financial liabilities at the firm level.

Keywords: Foreign aid, sectoral growth, Dutch disease.
JEL classifications: F35, O47.

1 Introduction

The empirical literature on the macroeconomic effects of aid has produced contrasting results. Among the recent studies, some authors claim that the effects of aid on growth and development have been historically very close to zero (Rajan and Subramanian, 2008), or even negative (Easterly, 2007); while others (like Clemens, Radelet and Bhavnani, 2004; Roodman, 2007) conclude that on average aid has had a positive effect on growth. Given the actual size of aid transfers, these mixed findings are to some extent disappointing. But, as Bourguignon and Sundberg (2007) indicate, the mixed evidence is probably not surprising "[...] given the heterogeneity of aid motives, the limitations of the tools of analysis, and the complex causality chain linking external aid to final outcomes." (Bourguignon and Sundberg, 2007, p. 316).

One way in which most of the recent empirical studies reflect the fact that the causality chain linking aid to outcomes is complex, is the common conclusion that

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the effects of aid are highly dependent on idiosyncratic characteristics of the recipient countries. This is also the starting point of this paper, where we reformulate the fundamental question on aid's effectiveness in a way that helps to identify specific mechanisms at work, and the individual country characteristics that matter.

Some papers already go along this line by trying to identify factors directly *undermining* aid's capacity to increase growth. The arguments advanced in these papers basically belong to one of two different strands. The first is concentrated on the negative incentives and effects that aid may have on the institutional quality of the recipient countries (see for example Rajan and Subramanian, 2007; Bräutigam and Knack, 2004). The basic idea is that foreign aid may reduce the pressure to embark on necessary institutional reforms in the recipient countries. Some of the reasons are that aid appears as a windfall of resources alleviating structural deficits for irresponsible fiscal authorities, or that aid tends to spur corruption and competition for the rents it might create among special-interest groups. [1]

The second strand is related to possible detrimental macroeconomic effects of aid (see for example Adam and Bevan, 2006; Gupta, Powell and Yang, 2006; de Renzio, 2007). The main argument in these studies is that the capacity to "absorb" the aid influx in an effective manner can be severely restricted. The combination of a lack of absorption capacity and a relatively large size of aid receipts can translate into inflationary pressures and a tendency of the domestic currency to appreciate. This hurts exportable sectors directly, and may affect aggregate output growth and employment if the effects are sustained over a certain period.[2]

This paper belongs to the second strand. The main question we pose is to which extent foreign aid to poor countries has effectively limited the growth of exportable sectors. In particular, the paper presents an empirical assessment of the effects of aid on growth rates of average labor productivity at the aggregate level, and a disaggregation of this effect between the impact on the tradable (exportable) sector and the nontradable sector. A comprehensive empirical analysis of this issue at the cross-country level is absent in the existing literature on foreign aid. The currently available estimates of the impact of aid on sectoral competitiveness and labor productivity rely on highly stylized simulations (Adam and Bevan, 2006), have been concentrated only on the manufacturing sector (Rajan and Subramanian, 2005), or on the agricultural and industrial sector (Feeny and Ouattara, 2009).

Our contribution with this paper is intended to be twofold. First, we provide estimates of the effects of aid on growth of average labor productivity in sectors producing most of the tradable (exportable) goods and in those producing most of the nontradable goods. The main results show significant positive effects of aid on labor productivity growth in both sectors. They are robust to different econometric specifications, con-

[1] See for example Svensson (2000) and Djankov, Montalvo and Reynal-Querol (2006).

[2] The prospect of effects like these may rise even further, as donors have promised a substantial scaling-up of financial aid to the poorest countries.

ditioning factors, and the endogenous nature of the relationship between donors' aid disbursements and recipients' economic growth.

In general, our results contrast with those of similar previous studies. Thus, in a second step, we explore mechanisms that provide a plausible explanation for the main findings. More precisely, the paper shows that a large inflow of aid might be beneficial for growth of average labor productivity when the recipient country has high levels of dollarization of financial liabilities. A possible reason is that firms indebted mostly in foreign currency benefit from an overvalued domestic currency, because that reduces their financial costs and burdens. This type of positive effects on firms' balance-sheets might be substantial in highly dollarized economies and, therefore, might allow firms, and the economy, to grow faster.

The remainder of the paper is organized as follows. Section 2 reviews the link between aid and external competitiveness and explains how we analyze it. Section 3 discusses methodological issues. Section 4 presents the empirical results, and Section 5 concludes.

2 Aid, real appreciation and external competitiveness

Rajan and Subramanian (2005) ask why is it so difficult to find a robust effect of aid on growth, and argue that the potential benefits of aid are reduced by adverse effects on competitiveness. They present evidence indicating that aid inflows cause overvaluation of the real exchange rate (RER) in the recipient country, and that this reduces competitiveness in the exporting sectors, as reflected by "systematic adverse effects on growth, wages, and employment in labor intensive and export sectors" (Rajan and Subramanian, 2005, p. 22).

The overall idea resembles closely the mechanics of the Dutch disease problem after an influx of any type of foreign resources. The basic Dutch disease model is set up on an economy producing two types of goods, the traded and the nontraded. Quantity and price of the nontraded good are set by the intersection of a downward sloping demand curve and an upward sloping supply curve; while the quantity of the traded good is set by its demand, at the price given exogenously by the world market.[3] In this simple model, a large inflow of foreign resources (aid for instance) has two important effects. First, it tends to make the value of the domestic currency stronger vis-a-vis the foreign currency (that is, the domestic nominal exchange rate gets appreciated). Second, it tends to expand the demand for both goods; which raises the price of nontraded vs the traded goods (given that the latter is given by the world market), and consequently tends to increase the domestic rate of inflation. When both of these effects (the nominal appreciation and the increase in domestic inflation) are combined, the price of nontraded goods increases in real terms compared to the price of traded

[3]The model is fully described in Elbadawi et al (2007) and Nkusu (2004), for example.

goods (or, equivalently, the RER gets appreciated, or overvalued).

RER appreciation is detrimental for growth in the traded sector if wages and other production costs do not adjust downwards. If the slowdown in the traded sector is long-lasting, it may also retard growth in the whole economy, especially if the production of tradable goods exhibits has side effects for the whole economy –such as the adoption of new technologies and the opening of new markets.

Among the papers that have analyzed the case of Dutch disease and RER overvaluation after an influx of foreign aid, some show that the Dutch disease effects of aid are small (for example Prati, Sahay and Tressel, 2003), while others suggest that they are potentially larger (for example Elbadawi et al., 2007). However, there is a certain consensus among these studies on two specific points. The first is that a windfall of foreign aid does cause RER overvaluation. The reason is that the negative effects of aid on the RER are difficult to avoid completely, given that recipient countries tend to be limited in their ability to contain the RER overvaluation (for example, with contractionary monetary policies), or to expand domestic supply (for example, due to problems of absorptive capacity).[4]

The second point of consensus is that this aid-induced RER overvaluation tends to be present mainly during the short run. This happens because, after the aid inflow has been received, the economy has the possibility of effectively expanding the domestic supply over the medium and longer run. An expansion in domestic supply can happen, for example, when aid is used to build infrastructure such as new rural roads that tend to benefit relatively more the nontradables sector. This helps to contain the tendency of the domestic price level to increase, and the tendency of the RER to get overvalued over the long run.

With this background, this paper presents an econometric assessment of the effects of aid on growth rates of average labor productivity at the aggregate level, and a sectoral disaggregation of this effect, distinguishing the effect of aid on the tradable from the nontradable sectors. Our hypothesis is that, if aid causes real appreciation and a reduction in external competitiveness, that is Dutch disease type of problems, an inflow of aid should have a negative effect on growth of sectors producing most of the tradable goods, and a positive effect on sectors producing most of the nontradable goods.

Contrary to previous papers in the literature, our results show no evidence of Dutch disease type effects. Hence, we explore some mechanisms that might account for this finding. The exploration is based on the theoretical work from Nkusu (2004), who argues that developing countries may exhibit some particular characteristics that reduce the probability of having RER overvaluation and Dutch disease type of problems after an influx of aid, or characteristics that might even allow them to benefit when the RER gets overvalued. One of these characteristics is the existence of idle capacity. When a

[4]Killick and Foster (2007) argue that the increasing aid to Africa, for example, will be difficult to manage for the recipient countries in ways that "do not disadvantage producers of tradeable goods, and the private sector generally".

developing country with idle capacities receives foreign aid resources, the associated expansion in aggregate demand can be met relatively fast by an expansion of aggregate supply. This reduces the upward pressures on the level of inflation and, thus, also reduces the pressure for RER overvaluation.

A second characteristic reducing risks related to Dutch disease is that production in developing countries is typically highly dependent on imported inputs. This implies that, with input costs largely denominated in foreign currency, a RER overvaluation unambiguously lowers total costs of production. We explore this idea by testing the hypothesis that high levels of dollarization of financial liabilities may offset the negative effects of real appreciation induced by aid, supporting positive, rather than negative, effects of aid on growth.

In our empirical analysis we proceed in four steps. First we seek to identify the marginal effect of aid on growth in aggregate average labor productivity, defined as the average growth rate of output (GDP or Total Value Added) per worker, and denoted by g. This is made along the lines of the empirical aid-growth literature. That is, the basic model we estimate is

$$g = f(a,\ p,\ d,\ \mathbf{\Gamma},\ \mathbf{Z}), \quad (1)$$

which is a regression of growth in output per worker on the size of aid effectively disbursed, a, the direct effects of macro policies' quality, p, geographical country-specific determinants, d, conditional effects of a, p and d on g (captured by a vector $\mathbf{\Gamma}$ containing interaction terms between a, p and d, with the level of aid disbursed),[5] and a set of control for other covariates of aggregate average labor productivity growth, $\mathbf{Z}$.

The second step in the empirical analysis involves a sectoral decomposition of the aggregate effect. We estimate the same type of model, but using measures of growth of productivity in the tradable (exportable) and the nontradable sectors, g_s:

$$g_s = f(a,\ p,\ d,\ \mathbf{\Gamma},\ \mathbf{Z}_s), \quad (2)$$

where $s \in \{\text{tradables, nontradables}\}$ and $\mathbf{Z}_s$ is a vector of covariates of sectoral growth of labor productivity.

These first two steps are aimed to give an answer to the question of whether foreign aid causes a relative loss of external competitiveness (Dutch disease) or not.[6]

[5] In terms of traditional aid-growth models (as in Dalgaard et al., 2004, for example), these interaction effects correspond to Burnside and Dollar's (2000) claim that aid works with reasonable policies ($\partial^2 g/\partial a \partial p > 0$); Hansen and Tarp's (2000) suggestion that aid exhibits diminishing returns ($\partial^2 g/\partial a^2 < 0$); and Dalgaard, Hansen and Tarp's (2004) finding of higher aid effectiveness with better geographic/climatic conditions ($\partial^2 g/\partial a \partial d > 0$).

[6] A reviewer commented that it is not possible to learn if aid causes Dutch disease by focusing on *differences in labor productivity across sectors*; and that it would be more appropriate in this case to analyze *differences in sectoral shares*. The main argument is that aid may affect not only the level of sectoral output, but also the level of sectoral employment (as an example, if aid causes agricultural labor productivity to increase, it would be difficult to say if it was because aid had a positive effect on agricultural output, or a negative effect on agricultural employment). To check this, we run our main regressions in terms

The following steps are aimed to explore possible reasons. In particular, we analyze if financial conditions and the composition of debt are a relevant part of the explanation. The next steps require then (*a*) extending the aggregate models in (1) and (2) to control for the characteristics of debt in the aid-recipient countries, and (*b*) decomposing again this effect into its sectoral components. Accordingly, we estimate

$$g = f(a,\ p,\ d,\ \mathbf{\Gamma},\ \mathbf{Z},\ \mathbf{Z}^f) \tag{3}$$

and

$$g_s = f(a,\ p,\ d,\ \mathbf{\Gamma},\ \mathbf{Z}_s,\ \mathbf{Z}^f), \tag{4}$$

where $\mathbf{Z}^f$ is a vector including financial characteristics in the country, in particular characteristics of debt in the country, and and variables controlling for the RER evolution.

3 Method and data

3.1 Econometric specification

The basic econometric specification for the model in (1) is

$$g_{it} = (a_{it}\ p_{it}\ d_i)\left(\beta_a\ \beta_p\ \beta_d\right)' + a_{it} \times (a_{it}\ p_{it}\ d_i)\left(\beta_{aa}\ \beta_{ap}\ \beta_{ad}\right)' + \mathbf{Z}_{it}'\boldsymbol{\beta}_{\mathbf{Z}} + \boldsymbol{\tau}'\boldsymbol{\beta}_\tau + \varepsilon_{it}, \tag{5}$$

where g_{it} is a measure of growth in output per worker (or growth in average labor productivity) in country i during period t; a_{it}, is the size of effective aid in terms of GDP;[7] p_{it} is the Burnside and Dollar (2000) index of good macro policies; d_i is a measure of structural characteristics (Dalgaard et al., 2004), proxied by the share of tropical area in the country from Gallup, Sachs and Mellinger (1999); τ is a vector of time-dummies (to control for common shocks); and ε_{it} is a zero-mean error component. $\mathbf{Z}_{it}$ is a vector containing other exogenous determinants of output per worker growth, specifically:

of sectoral output instead of sectoral labor productivity (to partial out the effect of changes in employment); and found the same results. For the rest of the paper, we choose to keep the focus on sectoral labor productivity instead of sectoral levels or sectoral shares, for various reasons. First, analysis of Dutch disease requires trying to approximate differences in *competitiveness* across sectors, rather than only differences in the *relative sizes* of different sectors (and differences in output per worker across sectors are more helpful in this sense than differences sectoral levels or shares). Second, put in a growth perspective, the determining factor of the sectoral composition of the economy is necessarily some measure of sectoral productivity: seeing sectors of the economy shrinking or expanding over time critically depends on the trajectory of productivity in each sector (put differently, only sectors that learn to produce better goods or services more efficiently are the ones that can expand sustainably). Third, when we explore posible reasons behind our main findings, we find theoretical reasons in the literature to focus on variables linked to costs and performance of firms. That implies that, if we aim to understand not only if aid causes Dutch disease but also why, we need to to pay attention to factors that are relevant for firms, and differences in productivity of any factor (labor for instance) are probably one of the first type of proxies that come to mind.

[7]Effective aid is defined as the grant equivalent of official disbursements constructed by Chang, Fernandez-Arias and Serven (1998), calculated as the sum of official grants and the grant element in concessional loans.

(*a*) the degree of financial depth, measured as the (lagged) ratio of M2 to GDP, (*b*) the ICRG index of institutional quality, trying to reflect security of private property and enforceability of contracts,[8] (*c*) the level of output per worker at the beginning of every period t, (*d*) the degree of ethno-linguistic fractionalization in the country (Easterly and Levine, 1997), (*e*) the number of conflicts in which the government is involved (UCDP/PRIO, 2006), and (*f*) an interaction term between these last two.

In a similar way, the econometric specification for the sectoral decomposition proposed in equation (2), is

$$g_{it,s} = (a_{it}\ p_{it}\ d_i)\left(\lambda_a\ \lambda_p\ \lambda_d\right)' + a_{it} \times (a_{it}\ p_{it}\ d_i)\left(\lambda_{aa}\ \lambda_{ap}\ \lambda_{ad}\right)' + \mathbf{Z}'_{it,s}\boldsymbol{\lambda}_{\mathbf{Z}_s} + \boldsymbol{\tau}'\boldsymbol{\lambda}_\tau + \varepsilon^s_{it}, \quad (6)$$

where $g_{it,s}$ is a measure of output per worker in sector s, $s \in \{\text{tradables, nontradables}\}$, and the new estimated coefficients are the λ's.

To estimate the models in (3) and (4), which are extensions of the previous two regressions meant to identify as directly as possible the presence of RER overvaluation and Dutch disease, it is necessary to extend the vector $\mathbf{Z}_{it}$ with variables reflecting the evolution of the RER and the characteristics of debt in the recipient country (in particular those related to the level of dollarization of financial liabilities). The variables considered for this extension are (*a*) the rate of RER devaluation (and the square of it), which helps to control for the effects that the RER has directly on growth,[9], (*b*) the amount of external debt measured as a proportion of GDP, and (*c*) the currency composition of the external debt.

The central econometric concern for the estimation of all these regressions is the endogenous character of aid: aid disbursements are obviously determined to some extent by the recipient country's growth process itself (Berthélemy, 2006; Nunnenkamp and Thiele, 2006). All the recent empirical literature on foreign aid effectiveness has turned to the use of instrumental variables (IV) to address the problem of endogeneity. We follow this line and perform two-stage least squares (2SLS) estimations, using the set of instruments in Dalgaard and Hansen (2001) and Dalgaard, Hansen and Tarp (2004).[10] Given that our 2SLS estimates ran into statistical problems (as we will discuss

[8]The ICRG indexes are available since 1984. When we use this measure directly, our sample gets reduced by half. We extended the series backwards to complete the sample, and use this extended series throughout. The ICRG index is in either case significant, and the overall, results in both cases remain qualitatively the same. This reflects the fact institutional quality, as captured by these indexes at least, tends to vary slowly in time. For instance, the change for the median country, among the 140 covered between 1984 and 2001, has been only 1.3%.

[9]The square of the RER devaluation is included to model formally the idea that there exists an "equilibrium RER", or a RER level that keeps the balance between keeping exports competitive and keeping the level of inflation controlled. See for example Elbadawi et al. (2007, footnote 1, p. 1).

[10]These are Aid/GDP, lagged; (Aid/GDP) squared, lagged; (Policy × Aid/GDP), lagged; Policy × (log Initial GDP per capita); Policy × (log Initial GDP per capita) squared; Policy × (log Population); and a dummy for countries in the Central Francophone Africa zone. The instrumentation strategy is fully described and motivated in detail in Dalgaard, Hansen and Tarp (2004), but in general it is aimed to reflect donors' overall preference to send aid to the smallest and poorest countries, those with better macro policies and to account for some strategic interests of donors in specific groups of countries (former

in detail below), we also applied the GMM-DIF estimator suggested by Arellano and Bond (1991) and the GMM-SYS estimator proposed by Arellano and Bover (1995) and Blundell and Bond (1998).

3.2 Data on sectoral labor productivity

Our estimations require measures of average labor productivity in the tradable (exportable) and the nontradable sectors. We build these measures with data from the World Bank's World Development Indicators (WDI) on labor force participation, sectoral employment and sectoral real Value Added (defined as the net output of a sector –measured in constant USD– after adding up all outputs and subtracting intermediate inputs).

The proxies we constructed for labor productivity in the tradable (exportable) sector are (*a*) the non-services GDP per worker and (*b*) the sum of Agricultural and Industrial Value Added per worker. The proxy constructed for the nontradables sector is based on Value Added per worker in the Services sector.

This distinction between tradables and nontradables sectors is made under the assumptions that the overall production in the economy comes from activity in agriculture, industry and the services sectors; and that production of nontradables is concentrated in the services sector, while production of tradables takes place primarily in the agricultural and industrial sectors. This assumption is supported by the survey in Tica and Družić (2006, Table 1), who review a large number of empirical papers analyzing the effects of productivity gaps on terms of trade, and report that none of those studies treats the services sector as producing tradables (the 58 papers reviewed by them treat production of tradables as taking place in the agricultural sector, or the industrial sector, or both). The argument gathers strength when it is placed in the context of developing countries, where trade in agricultural and manufacturing goods (containing for example exports of raw agricultural commodities, agroindustrial products, minerals, and so forth) tends to be much higher than trade in services.

The WDI provide a measure of real Value Added per worker in the Agricultural sector, but not in the other sectors. We therefore constructed proxies for Value Added per worker in the Industrial and Services sectors. An important point is that an accurate labor productivity measure requires an estimate of the number of workers actually employed in the different sectors, rather than estimates of the number of workers in the labor force or the potential number of workers in each sector. Accordingly, we construct series for the effective sectoral allocation of labor based on sectoral employment data from the WDI. The series for sectoral employment were built based on interpolations of the employment data in the Agricultural, Industrial and Services sectors, and completed to fill gaps towards the end of the sample period under the assumption that

colonies, important trade partners, or political allies, for example).

the sectoral distribution of employment kept stable over time.[11] For all the estimations in the remainder of the paper then, the proxies used for levels of sectoral labor productivity levels are measures of real Value Added in the Agricultural, the Industrial and the Services sectors, divided by the estimated number of employed workers in each corresponding sector.

The sample covers a group of 69 developing economies over 40 years, the period between 1962 and 2001. All the variables were averaged over periods of 4 years, to capture the evolution of trends rather than the incidence of cycles, and to make the results comparable to those in previous empirical studies. Our sample does not go beyond 2001 because many of the variables used in the regressions could not be updated further than that for many countries in the sample.

Summary statistics of our variables are displayed in Appendix Table A1, and the description of the countries and periods considered is given in Table A2.

4 Results

4.1 Impact of aid in the aggregate

The estimated effects of aid on growth of aggregate average labor productivity, that is the results of regression (5), are displayed in Table 1. Column 1 contains the 2SLS estimates.

The coefficients of interest (the coefficients on the aid variable and the 3 aid-interaction terms) show that aid has a positive and direct impact on growth of aggregate labor productivity, and that these benefits can be increased in countries with good policies, whereas they are lowered in countries where the amount of tropical area is large (Dalgaard et al., 2004). This last finding can be interpreted as indicating that aid effectiveness is limited in countries where location and climate are disadvantageous or, in particular, in countries with large a amount of tropics, where the burden of diseases is larger (as suggested by Gallup et al., 1999, for example) or where growth in agricultural productivity is restricted (see Masters and Wiebe, 2000, for example).

The rest of the coefficients show that good policies, as captures by the policy index, are good for growth by themselves. Initial conditions appear to matter (the coefficient on the initial level of output per worker is significant), sound institutions (measured by the ICRG index) have a positive correlation on growth, and a high number of conflicts does not seem to be correlated to higher rates of growth, just as do not either more ethnical division in the country. The only puzzling effect in column 1 is the negative and significant effect of financial depth (measured by the lagged ratio of money

[11] As a control, we constructed a series for the sectoral composition of the labour force assuming that for any given country, a larger fraction of the labour force was concentrated in sectors where the production of Value Added was higher. This change of measure yields qualitatively equivalent results to those reported in the following tables.

Table 1: Aid and labor productivity

Dependent variable:	Growth in GDP per worker				
	2SLS (1)	2SLS/FE (2)	GMM-DIF (3)	GMM-SYS (4)	GMM-SYS (5)
Aid/GDP	3.05***	4.75**	0.30	2.62***	2.67***
	[1.0]	[2.2]	[0.8]	[0.9]	[0.9]
(Aid/GDP), squared	−0.0055	0.038	−0.058**	−0.038**	−0.036**
	[0.04]	[0.06]	[0.03]	[0.02]	[0.01]
(Aid/GDP)×Policy	0.17**	−0.1	−0.11**	−0.038	−0.038
	[0.08]	[0.09]	[0.05]	[0.06]	[0.06]
(Aid/GDP)×Tropical area	−3.20***	−5.29**	0.035	−2.32**	−2.38***
	[0.9]	[2.3]	[0.8]	[0.9]	[0.9]
Policy index	0.69***	1.19***	1.28***	1.06***	1.07***
	[0.2]	[0.2]	[0.2]	[0.2]	[0.2]
Tropical area	−0.55			−0.94**	−0.90**
	[0.5]			[0.5]	[0.5]
(log) Initial GDP per worker	−0.49*	−1.90**	−6.06***	−0.82***	−0.76***
	[0.3]	[0.7]	[1.3]	[0.3]	[0.2]
ICRG institutions index	0.36***	0.25	0.25	0.40***	0.41***
	[0.09]	[0.2]	[0.2]	[0.09]	[0.10]
Fin. depth (M2/GDP), lagged	−2.36**	−1.79	−3.54	−3.51***	−3.41***
	[1.1]	[1.6]	[2.2]	[1.2]	[1.2]
Ethnic fractionalization	−0.42			−0.36	
	[0.9]			[1.0]	
Number of conflicts	−1.01*	−1.91**	−0.73	−1.21**	
	[0.6]	[1.0]	[0.9]	[0.6]	
Ethnic fract.×Conflicts	1.38*	2.58**	0.34	1.36*	
	[0.8]	[1.2]	[1.5]	[0.8]	
Observations	460	459	457	505	505
Number of countries	69	68	69	69	69
Specification tests:					
Sargan test, overidentification	(0.79)	(0.92)	(0.99)	(0.99)	(0.99)
Cragg-Donald test, underidentification	(0.0)	(0.0018)	.	.	.
Anderson-Rubin test, joint signif.	(0.062)	(0.23)	.	.	.
Durbin-Wu-Hausman test, endogeneity	(0.022)	(0.024)	.	.	.
Hausman test for Fixed Effects	.	(0.36)	.	.	.
Total number of instruments	26	20	137	179	151
AR(1)	.	.	(0.0027)	(0.0023)	(0.0025)
AR(2)	.	.	(0.55)	(0.72)	(0.71)
Hypothesis tests on the marginal effect (ME) of aid:					
ME of aid > 0 (mean)	1.05***	1.18*	0.11	0.96***	0.97***
	[0.43]	[0.8]	[0.29]	[0.36]	[0.31]
ME of aid > 0 (*country A*)	3.34***	4.36**	0.051	2.45***	2.50***
	[0.96]	[2.05]	[0.72]	[0.92]	[0.84]
ME of aid > 0 (*country B*)	0.08	−0.59	0.035	0.15	0.15
	[0.32]	[0.63]	[0.21]	[0.16]	[0.15]

Notes. Robust standard errors in brackets, p-values in parentheses. *** , ** and * denote significance at 1, 5, and 10%. Each observation is an average over a period of 4 years. Data from 1962 to 2001. Regressions include a constant, time-dummies and a dummy for countries in the Sub-Sahara and the East Asia regions. *Country A* is a hypothetical country with a *good* policies (policy index at the 75th percentile), receiving little aid (25th percentile) and located outside the tropics (25th percentile of the tropical area variable distribution. *Country B* is a hypothetical country with a *bad* policies (policy index at the 25th percentile), receiving large amounts of aid (75th percentile) and located in the tropics (75th percentile of the tropical area variable distribution.

and deposits, M2, to GDP). This can be due to an omitted variable bias, since a high M2/GDP ratio might be correlated with high levels of other sources of foreign capital (for example foreign bonds, or external debt), which act as substitutes of aid to some extent.

The last part of column 1 in Table 3 shows a test for the hypothesis that the marginal effect (ME) of aid, defined as $\frac{\partial g}{\partial a}$, is positive. Given that the ME of aid we can estimate is

$$\frac{\partial g_{it}}{\partial aid_{it}} = \beta_a + 2\,\beta_{aa}\,aid_{it} + \beta_{ap}\,p_{it} + \beta_{ad}\,d_i, \tag{7}$$

it is necessary to choose a fixed point to estimate the marginal effect. The most obvious point is the mean of the different variables composing it (that is, the mean levels of aid, the macro policy index, and the percentage of tropical area in the country). However, Figures 1 and 2 show that the distributions for aid and the share of tropical area are highly asymmetric.

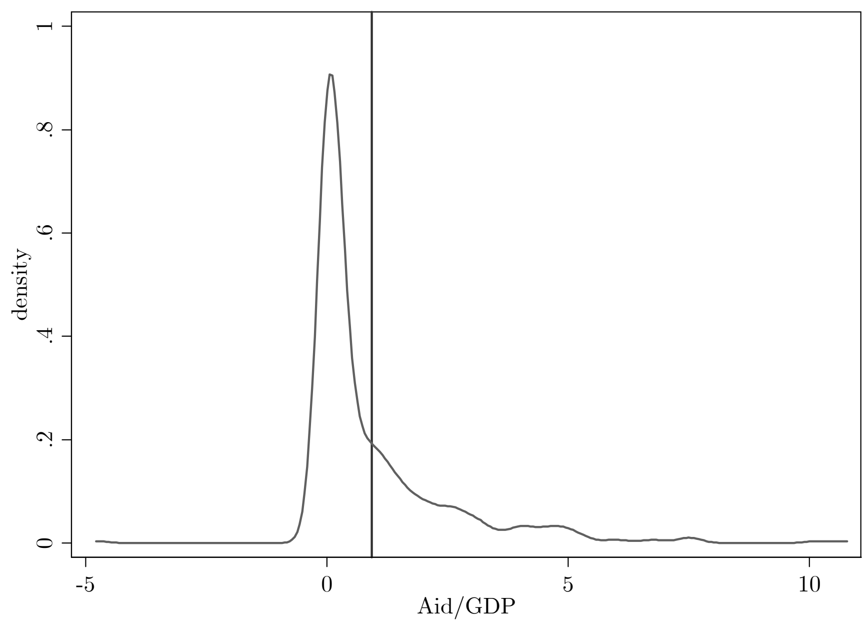

Figure 1: Aid/GDP %, density plot

Figure 1 shows that the distribution of aid is skewed to the left and has a relatively fatter right-hand side tail. This reflects the fact that countries receive in general some (relatively low level of) aid; but, with a certain frequency, countries receive a much larger amount of aid, for example in the form humanitarian aid after a natural disaster, or for the reconstruction of an area after a period of conflict. Figure 2 shows the distribution for the amount of tropical area in the countries in the sample, which is bimodal, and reflects the fact that most of the aid-recipients are located either in highly tropical areas, or in places that are considerably far from the tropical lands. This is

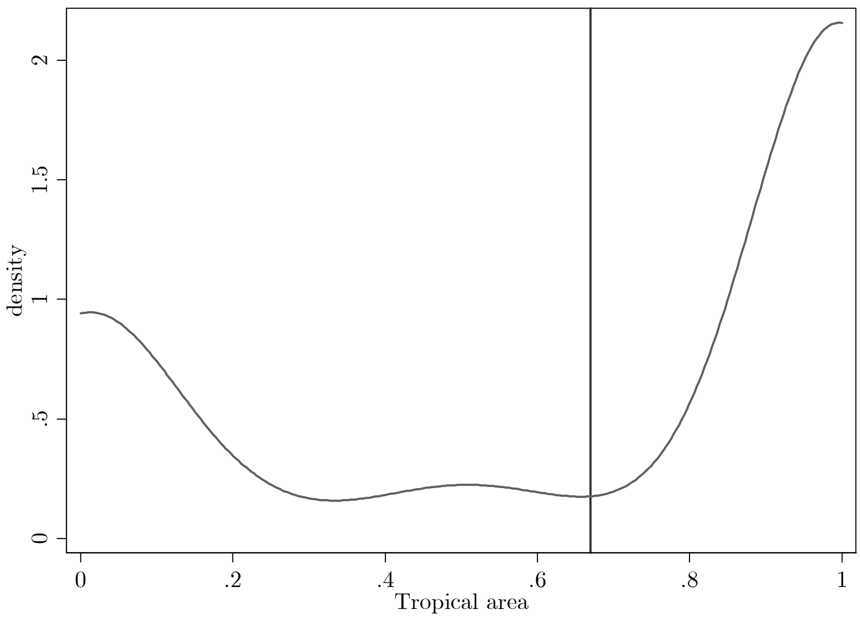

Figure 2: Tropical area index, density plot

relevant because, with this type of distributions, the average country may not reflect the most typical characteristics, and evaluating the marginal effect at the mean can be misleading.

Therefore, Table 1 also presents the marginal effects of aid evaluated at two other points. The first is defined as a hypothetical *country A*, exhibiting a high level for the quality-of-policy index (at the 75th percentile of the Burnside-Dollar policy index distribution); and also receiving a low amount of aid (equivalent to the level at the 25th percentile in the aid distribution), and being located outside the tropics (at the 25th percentile in the distribution of tropical area). The second hypothetical point of evaluation is a hypothetical *country B*, exhibiting low level for the index of good macro policies (25th percentile); and also receiving large amounts of aid (75th percentile), and located in a very tropical area (75th percentile). These two definitions are completely arbitrary, but can be understood as an approximation of the upper and lower bounds for the marginal effect of aid in a *typical* country.

It turns out that aid has a net positive and highly significant effect in the average country. The effect is equally significant, but 3 times larger for a *country A* (with relatively more favorable conditions), and statistically not different from zero for a *country B* (with less favorable conditions).

As for the validity of the 2SLS regressions, the tests on the quality of instrumentation show overall good results: (1) a high p-value for the Sargan test of overidentification does not allow to reject the hypothesis that the set of instruments employed are valid, (2) a low p-value for the Cragg-Donald test of underidentification implies that the instruments are not weak, (3) the low p-value for the Anderson-Rubin test for joint significance of the endogenous regressors cannot reject the hypothesis that the instruments are jointly significant, and (4) the low p-value for the Durbin-Wu-Hausman test of endogeneity of the instruments allows to reject the hypothesis that the endogenous regressors can be treated as exogenous, indicating that the use of IV in this specification is appropriate.

However, even though the specification is comprehensive with respect to the main determinants of growth, it is possible that there are important unobserved individual country characteristics that are being omitted in the regression. To control for this, Column 2 in Table 1 presents the same regression as in Column 1, but estimates the model including individual country fixed effects. The results are relatively similar to the ones in Column 1 regarding the significance of the coefficients of interest, the tests on the quality of instrumentation, and the significance of the marginal effect of aid evaluated at different points. From the statistic for the (Hausman) test of fixed-effects, it is not possible to reject the hypothesis that the country fixed effects are significant and, thus, correctly included in Column 2.

This poses a serious problem for these first two regressions. The reason is that if both the unobserved country characteristics and the lagged level of the dependent variable are significant and belong into the regression, then the 2SLS regression in Column 1 is misspecified (because it lacks the fixed-effects terms), and the 2SLS/FE regression in Column 2 gives inconsistent estimates (because differencing the data and estimating the regression with the lagged level of the dependent variable on the right-hand side introduces correlation between one of the regressors and the error term, by construction).

An appropriate estimator for a panel data model where (*a*) unobserved individual fixed effects are relevant and (*b*) a lagged level of the dependent variable needs to be included as a regressor, is the GMM-DIF estimator. This method estimates the regression in first differences, and instruments the lagged differences in the right-hand side with lagged levels of the variables. The number of lags to be included can be chosen arbitrarily, but following Roodman (2009), we try to reduce the problem of "too many" instruments by restricting them to be only the ones starting from the second.

Column 3 in Table 1 presents the regression with the GMM-DIF estimator. The results are quite different from the ones before. In particular, aid now has no significant effect on growth. Column 3 reports however a high coefficient for the lagged level of the dependent variable, which reflects a high level of autocorrelation. As shown by Arellano and Bover (1995), lagged levels are weak instruments of current differences when the series are highly persistent. The autocorrelation tests displayed in column 3 show high AR(1) for the error term in the model in differences (which is expected to happen), but high AR(2) as well, supporting the conjecture that the dependent variable is highly persistent. In this case, first-differences cannot be strongly instrumented by lagged levels.

The alternative is to use the GMM-SYS estimator, which supplements the model in differences with equations in levels and instruments based on lagged differences. The GMM-SYS estimates suggest that aid has a positive effect on growth, that the effect is not dependent on the quality of macro policies but on the type of structural country characteristics, and that the overall effect operates with diminishing returns. The estimates of the marginal effects of aid are similar to those in column 1. A higher p-value for the absence of AR(2) shows an improvement in the quality of estimation and instrumentation compared to the GMM-DIF estimation in column 3.

Finally, Column 5 in Table 1 drops the variables associated with country conflict and division; this helps to reduce problems of multicollinearity and the total number of instruments, does not change the significance of the rest of the variables in the model, and allows a more precise estimation of the marginal effects. We therefore regard it as the preferred econometric specification.

4.2 Sectoral disaggregation of the impact of aid

Table 2 presents the sectoral decomposition of the aggregate effect of aid on growth. To provide a benchmark, column 1 shows again the aggregate effects of aid on growth (corresponding to Column 5 in Table 1, the preferred specification for the aggregate model). Columns 2 and 3 contain estimates for the sectors likely to be producing most of the exportable goods (the tradables sector). The proxy for output in the tradables sector considered in Column 2 is output in the non-Services sector, and the one considered in Column 3 is the aggregation of Value Added in the Agricultural and the Industrial sectors.

The most important difference between these two regressions is that column 3 shows a significant coefficient for the aid-squared term, while column 2 does not. But the rest of the coefficients of interest are similar in size and significance in the two specifications. Despite the difference in the coefficient on the aid-squared term, the overall conclusion from the marginal effects remains the same: aid is shown to have a positive marginal effect in the tradables sector in the average country, a three times higher effect in countries with more favorable conditions, and no significant effect in countries with less favorable conditions.

Column 4 shows the estimates of the effects of aid in the Services sector, which is taken as a proxy for the sector producing most of the nontradable goods in a developing economy. This column reveals a more independent impact from aid compared to Columns 2 and 3 in the sense that the interaction term with tropical area is no longer significant. Evaluated at the mean, the size of the marginal effects turns out to be roughly equal in services and non-services sectors. For a *country A* the size of the marginal effect is even lower than in columns 2 and 3, while for a *country B* it becomes

Table 2: Aid and growth in labor productivity: Sectoral decomposition

Dependent variable: (growth in)	GDP per worker (1)	Non-Services GDP per worker (2)	Agric. & Indus. VA per worker (3)	Services VA per worker (4)	Agricultural VA per worker (5)	Industrial VA per worker (6)
Aid/GDP	2.67***	2.67***	3.14***	1.94**	−0.62	4.15***
	[0.9]	[0.9]	[0.8]	[0.8]	[5.5]	[1.2]
(Aid/GDP), squared	−0.036**	−0.025	−0.052***	−0.073*	−0.59	−0.031
	[0.01]	[0.02]	[0.02]	[0.04]	[0.5]	[0.03]
(Aid/GDP)×Policy	−0.038	−0.014	−0.018	−0.17	0.19	0.0067
	[0.06]	[0.06]	[0.07]	[0.2]	[0.4]	[0.1]
(Aid/GDP)×Tropical area	−2.38***	−2.61***	−2.89***	−0.83	7.31	−3.94***
	[0.9]	[0.9]	[0.8]	[1.0]	[6.1]	[1.2]
Policy index	1.07***	0.94***	0.93***	1.71***	1.13	1.48***
	[0.2]	[0.2]	[0.2]	[0.5]	[0.9]	[0.4]
Tropical area	−0.90**	−0.31	−0.13	−2.25**	−1.98	−0.46
	[0.5]	[0.5]	[0.5]	[0.9]	[2.3]	[0.7]
(log) Initial level dep. var.	−0.76***	−0.57**	−0.42	−1.55***	−0.52	−1.13***
	[0.2]	[0.3]	[0.3]	[0.4]	[3.6]	[0.3]
ICRG institutions index	0.41***	0.42***	0.37***	0.53***	−0.97	0.49***
	[0.10]	[0.10]	[0.10]	[0.2]	[0.8]	[0.1]
Fin. depth (M2/GDP), lagged	−3.41***	−1.98	−2.4	−5.69***	−4.00	−4.57**
	[1.2]	[1.5]	[1.5]	[2.1]	[7.4]	[2.1]
Observations	505	505	504	505	504	504
Number of countries	69	69	69	69	69	69
Number of instruments	176	176	176	176	176	176
AR(1)	(0.0025)	(0.034)	(0.03)	(0.014)	(0.19)	(0.000099)
AR(2)	(0.71)	(0.4)	(0.35)	(0.5)	(0.27)	(0.32)
ME of aid > 0 (mean)	0.97***	0.86***	1.09***	1.10***	3.36	1.46***
	[0.31]	[0.34]	[0.32]	[0.40]	[3.71]	[0.45]
ME of aid > 0 (*country A*)	2.50***	2.54***	3.00***	1.53**	0.05	4.03***
	[0.84]	[0.85]	[0.77]	[0.76]	[4.84]	[1.13]
ME of aid > 0 (*country B*)	0.15	−0.02	0.10	0.70**	5.51*	0.14
	[0.15]	[0.20]	[0.20]	[0.43]	[4.20]	[0.26]

Notes. Robust standard errors in brackets, p-values in parentheses. ***, **, and * denote significance at 1, 5, and 10%. All the regressions display GMM-SYS estimates, include a constant term, time-dummies and dummies for countriews in Sub-Sahara and East Asia regions. Each observation is an average over a period of 4 years. Data from 1962 to 2001. *Country A* is a hypothetical country with a *good* policies (policy index at the 75th percentile), receiving little aid (25th percentile) and located outside the tropics (25th percentile of the tropical area variable distribution. *Country B* is a hypothetical country with a *bad* policies (policy index at the 25th percentile), receiving large amounts of aid (75th percentile) and located in the tropics (75th percentile of the tropical area variable distribution.

significantly positive.

Overall, these findings do not point to systematic differences in the impact of aid on tradable and nontradable production. This is the main result of the paper. It can be interpreted as providing empirical evidence against the hypothesis that aid is detrimental for external competitiveness and growth in average labor productivity, or that aid causes Dutch disease. If aid was a cause of Dutch disease, two "symptoms" after an inflow of aid would have to be a decline in the growth of the exportable (or tradables) sector, and a relative increase in the growth rate of the nontradables sector. The estimates of the marginal effects of aid in Columns 1-4 of Table 2 suggest the opposite: evaluated at the mean (and at the *country A* levels, where estimates are statistically significant), aid does not seem to cause a slowdown in the exportables sector compared to the aggregate level, nor an acceleration of the nontradables sector compared to the aggregate level.

Decomposing further the effect in column 3 (that is, decomposing the proxy for the tradables sector), Columns 5 and 6 indicate that the positive effects of aid on growth of the exportables sector actually come from the Industrial sector rather than the Agricultural sector. This implies that the external competitiveness of manufactures, minerals and agroindustrial products (all activities within the Industrial sector) is unlikely to deteriorate in response to an inflow of foreign aid.

4.3 Real appreciation and the absence of Dutch Disease

Further analysis of the link between aid, growth and Dutch disease requires accounting for variables that play a central role in this relationship. Columns 2 and 3 in Table 3 account for the effects of changes in the RER. The rate of RER devaluation is treated as endogenous within the model. After controlling for it, the marginal effects of aid estimated drop to half the size of the ones in the preferred specification (reproduced in Column 1). The square of RER devaluation is included in Column 3 to capture the idea that countries tend to benefit from a devaluated RER (because that tends to increase exports' competitiveness), but that after a certain point a too fast rate of RER devaluation can be passed to higher inflation rates, which starts to limit the (initial) benefits of RER devaluation.

Columns 4 and 5 in Table 3 account for the degree of external indebtedness and the currency composition of debt. Column 4 introduces the ratio of external debt to GDP, and Column 5 the percentage of dollarization in the country's overall level of debt, as a measure of the currency composition of the debt and the country's financial exposure to changes in the exchange rate.

From columns 2 to 5 it can be seen that the variables measuring RER devaluation, the ratio of external debt to GDP and the measure of debt's currency composition are marginally significant or not significant individually. However, when all these new variables are included at the same time, three of them become highly significant, and

Table 3: Aid and labor productivity: Financial effects

Dependent variable:	Growth in GDP per worker					
	(1)	(2)	(3)	(4)	(5)	(6)
Aid/GDP	2.67***	1.52***	1.68***	2.67***	2.22**	1.52***
	[0.9]	[0.5]	[0.5]	[0.9]	[0.9]	[0.5]
(Aid/GDP), squared	−0.036**	−0.016	−0.0065	−0.020**	−0.022**	−0.023**
	[0.01]	[0.01]	[0.01]	[0.009]	[0.009]	[0.009]
(Aid/GDP)×Policy	−0.038	−0.083	−0.055	−0.13*	−0.018	−0.15**
	[0.06]	[0.06]	[0.07]	[0.08]	[0.06]	[0.08]
(Aid/GDP)×Tropical area	−2.38***	−1.48***	−1.81***	−2.33***	−2.10**	−1.28**
	[0.9]	[0.5]	[0.5]	[0.9]	[0.9]	[0.5]
Policy index	1.07***	1.10***	0.89***	1.04***	1.04***	0.95***
	[0.2]	[0.2]	[0.2]	[0.2]	[0.2]	[0.2]
Tropical area	−0.90**	−0.72	−0.58	−0.87	−1.11**	−0.87*
	[0.5]	[0.5]	[0.5]	[0.5]	[0.5]	[0.5]
(log) Initial GDP per worker	−0.76***	−0.89***	−0.84***	−0.98***	−0.98***	−1.11***
	[0.2]	[0.2]	[0.2]	[0.2]	[0.3]	[0.2]
ICRG institutions index	0.41***	0.41***	0.41***	0.47***	0.46***	0.53***
	[0.10]	[0.10]	[0.10]	[0.1]	[0.1]	[0.1]
Fin. depth (M2/GDP), lagged	−3.41***	−2.06*	−2.05*	−3.64**	−3.74**	−2.17
	[1.2]	[1.2]	[1.2]	[1.5]	[1.6]	[1.4]
Real exch. rate devaluation		0.099	0.45***			0.42***
		[0.1]	[0.2]			[0.1]
Real exch. rate dev., squared			−0.0097***			−0.0091***
			[0.003]			[0.002]
External debt/GDP				−0.0070*		−0.0077**
				[0.004]		[0.004]
Debt currency composition (%USD)					0.40	1.62*
					[1.3]	[1.0]
Observations	505	427	427	402	409	339
Number of countries	69	65	65	60	61	57
Number of instruments	176	220	264	200	200	303
AR(1)	(0.0025)	(0.0096)	(0.013)	(0.0084)	(0.0083)	(0.035)
AR(2)	(0.71)	(0.62)	(0.81)	(0.49)	(0.49)	(0.54)
ME of aid > 0 (mean)	0.97***	0.44**	0.42***	0.74***	0.61***	0.37***
	[0.31]	[0.2]	[0.19]	[0.28]	[0.27]	[0.17]
ME of aid > 0 (*country A*)	2.50***	1.29***	1.51***	1.20***	1.20***	0.60**
	[0.84]	[0.48]	[0.48]	[0.45]	[0.49]	[0.29]
ME of aid > 0 (*country B*)	0.15	−0.12	−0.22	0.10	0.023	−0.024
	[0.15]	[0.12]	[0.12]	[0.11]	[0.11]	[0.11]

Notes. Robust standard errors in brackets, p-values in parentheses. ***, **, and * denote significance at 1, 5, and 10%. All regressions display GMM-SYS estimates, include a constant term, time-dummies, dummies for countries in the Sub-Sahara and East Asia regions. Each observation is an average over a period of 4 years. Data from 1962 to 2001. *Country A* is a hypothetical country with a *good* policies (policy index at the 75th percentile), receiving little aid (25th percentile) and located outside the tropics (25th percentile of the tropical area variable distribution. *Country B* is a hypothetical country with a *bad* policies (policy index at the 25th percentile), receiving large amounts of aid (75th percentile) and located in the tropics (75th percentile of the tropical area variable distribution.

Table 4: Aid, sectoral growth in labor productivity and financial effects

Dependent variable:	GDP per worker (1)	Non-Services GDP per worker (2)	Agric. & Indus. VA per worker (3)	Services VA per worker (4)	Agricultural VA per worker (5)	Industrial VA per worker (6)
Aid/GDP	1.52***	1.62**	1.98***	1.58*	5.25	3.04***
	[0.5]	[0.7]	[0.7]	[0.9]	[6.1]	[1.0]
(Aid/GDP), squared	−0.023**	−0.012	−0.037*	−0.054	−0.35	0.0096
	[0.009]	[0.02]	[0.02]	[0.04]	[0.4]	[0.05]
(Aid/GDP)×Policy	−0.15**	−0.11	−0.15*	−0.34**	0.034	−0.26*
	[0.08]	[0.08]	[0.09]	[0.1]	[0.4]	[0.2]
(Aid/GDP)×Tropical area	−1.28**	−1.64**	−1.79***	−0.46	−1.74	−2.67***
	[0.5]	[0.7]	[0.7]	[0.8]	[3.9]	[1.0]
Policy index	0.95***	0.84***	0.90***	1.43***	1.47	1.40***
	[0.2]	[0.2]	[0.2]	[0.4]	[1.4]	[0.4]
Tropical area	−0.87*	−0.31	−0.14	−2.42**	1.42	−0.2
	[0.5]	[0.7]	[0.6]	[0.9]	[3.2]	[1.0]
(log) Initial level dep. var.	−1.11***	−0.98***	−0.89***	−1.57***	−2.53	−1.52***
	[0.2]	[0.2]	[0.2]	[0.3]	[2.3]	[0.3]
ICRG institutions index	0.53***	0.51***	0.47***	0.60***	−1.38	0.49***
	[0.1]	[0.1]	[0.1]	[0.2]	[1.4]	[0.2]
Fin. depth (M2/GDP), lagged	−2.17	−0.88	−1.39	−5.75**	−11.7	−4.14
	[1.4]	[1.6]	[1.8]	[2.7]	[15]	[2.8]
Real exch. rate devaluation	0.42***	0.22	0.32	0.46**	−0.27	0.32
	[0.1]	[0.2]	[0.2]	[0.2]	[0.8]	[0.3]
Real exch. rate dev., squared	−0.0091***	−0.0071**	−0.0086***	−0.0093**	0.0027	−0.0079
	[0.002]	[0.003]	[0.003]	[0.004]	[0.01]	[0.005]
Ext. Debt/GDP	−0.0077**	−0.0073*	−0.0068	−0.018**	0.0026	−0.017**
	[0.004]	[0.004]	[0.004]	[0.008]	[0.02]	[0.007]
Debt currency comp. (%USD)	1.62*	2.43*	2.33*	3.28	−7.01	1.16
	[1.0]	[1.3]	[1.3]	[2.9]	[7.2]	[1.7]
AR(1)	(0.035)	(0.086)	(0.072)	(0.04)	(0.29)	(0.0011)
AR(2)	(0.54)	(0.37)	(0.3)	(0.54)	(0.31)	(0.18)
ME of aid > 0 (mean)	0.37***	0.27	0.42**	0.76**	3.25	0.81***
	[0.17]	[0.24]	[0.26]	[0.47]	[3.69]	[0.31]
ME of aid > 0 (*country A*)	0.6**	0.62*	0.81**	0.63	4.46	1.23***
	[0.29]	[0.39]	[0.4]	[0.52]	[5.03]	[0.55]
ME of aid > 0 (*country B*)	−0.024	−0.21	−0.12	0.51	2.59	0.032
	[0.11]	[0.18]	[0.2]	[0.44]	[3.16]	[0.27]

Notes. Robust standard errors in brackets, p-values in parentheses. ***, **. and * denote significance at 1, 5, and 10%. All the regressions display GMM-SYS estimates, include a constant term, time-dummies and dummies for countriews in Sub-Sahara and East Asia regions. Each observation is an average over a period of 4 years. Data from 1962 to 2001. 339 observations. 57 countries. 303 instruments. *Country A* is a hypothetical country with a *good* policies (policy index at the 75th percentile), receiving little aid (25th percentile) and located outside the tropics (25th percentile of the tropical area variable distribution. *Country B* is a hypothetical country with a *bad* policies (policy index at the 25th percentile), receiving large amounts of aid (75th percentile) and located in the tropics (75th percentile of the tropical area variable distribution.

the fourth marginally significant (Column 6).

All the additional variables in regression 6 in Table 3 are treated as endogenous. This specification therefore provides a good basis to decompose the effect of aid on growth and implement a more thorough test of the relationship between aid, growth and Dutch disease. It is reproduced as a baseline regression in column 1 in Table 4, where the results of the sectoral decomposition are displayed.

Table 4, similar to Table 2, provides no support for the case that aid causes Dutch disease: in Columns 3 and 6, for example, growth in sectors producing most of the exportables is shown to be positively affected by aid, and with a marginal effect larger than in the overall economy. Again it is the Industrial sector where the impact appears to be strongest These findings strengthen our evidence on the absence of Dutch disease type of problems caused by aid, because they turn out to be robust to the inclusion of the effects of changes in the RER and other relevant financial variables.

A second interesting finding from Table 4 is the statistical significance of variables that reflect the exposure of the aid-recipient economies to changes in the RER, because it can be seen as suggestive evidence in support of positive balance-sheet effects in aid-recipient economies. The currency composition of foreign debt is significant only in sectors that are producing most of the exportable goods, suggesting that the positive balance-sheet effects are more likely to benefit firms producing these (exportable) goods. When this is combined with results from other studies showing that aid causes real appreciation, the results in Table 4 suggest that one reason for aid not creating Dutch disease type of problems might be that firms in those economies benefit from (possibly aid-induced) appreciations of the RER, and this might probably be partially due to having an important share of their debt denominated in foreign currency.[12]

5 Summary and Conclusion

This paper presents an empirical assessment of the hypothesis that aid is detrimental for external competitiveness and growth in average labor productivity in the recipient countries. This evidence is based on a sectoral decomposition of the effects of aid on aggregate growth, and on an extension of the typical aid-growth econometric specification to control for the effects of (*a*) changes in the RER and (*b*) financial characteristics of the debt in the different sectors. The findings are robust to different specifications, conditioning factors, and the endogenous nature of aid disbursements.

Our main results point to the absence of Dutch Disease effects: aid is found to have a positive marginal effect on growth of output per worker, at the aggregate level, and in both the tradable and the nontradable sectors. One possible explanation might be

[12]This evidence also opens space for discussion and revision of the finding that RER overvaluation has contractionary effects (as suggested for example by Shi, 2006), and tends to support the importance of balance-sheet effects mentioned in the literature on the contractionary effects of devaluations (see Frankel, 2005, for a recent survey on the topic).

the existence of idle capacity in the recipient countries, which can help to promptly meet the increase in aggregate demand caused by aid inflows. Another explanation, explored empirically in the paper, is that Dutch disease type of problems need not materialize in an aid-recipient economy because firms in the tradable sector might benefit from an aid-induced RER appreciation. This would be the case if their debt is denominated mostly in foreign currency. We find evidence suggesting that the sectors producing most of the exportable goods tend to benefit from aid, even after controlling for the level of real appreciation that aid might cause and the amount of debt denominated in foreign currency.

Taken together, the finding that aid may cause RER appreciation but not Dutch disease type of problems, and the finding that the marginal effects of aid in countries with *bad* policies and *weak* structural characteristics are close to zero but not negative, suggest that the effectiveness of aid depends much more on the ability of donors to reduce the negative incentives associated with the use of foreign aid in the recipient countries, rather than on the ability to control the macroeconomic type of problems supposedly undermining the effects of foreign aid on growth.

In practice, our findings suggest that the success of the planned scaling-up of aid to the poorest countries does not depend so much on whether the resources are spent or absorbed by local governments (in the sense of Killick and Foster, 2007), nor on the limitations that donors and agencies put on them to guarantee the right use of the aid resources, but rather on whether it is possible to find a way to create or maintain good incentives in the recipient countries, and to overcome structural bottlenecks such as low agricultural productivity in tropical areas.

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Table A1: Summary statistics

		Obs.	Mean	p.25	p.50	p.75	Std. dev.	Min.	Max.
GDP per capita	growth rate	505	1.41	−0.50	1.68	3.22	3.18	−12.68	10.60
	level	505	3,546.1	1,108.9	2,102.2	4,098.1	3,900.6	225.8	20,535.6
GDP per worker	growth rate	505	1.09	−0.82	1.38	2.83	3.13	−12.49	9.73
	level	505	8,229.8	984.4	3,410.0	7,870.3	12,759.9	195.8	73,613.2
Non-Services GDP per worker	growth rate	505	1.35	−1.03	1.43	3.46	4.30	−16.35	34.28
	level	505	9,394.9	889.7	3,599.9	8,597.4	15,661.9	137.0	117,040.3
Agri.&Indust. VA per worker	growth rate	504	1.23	−1.05	1.40	3.33	4.35	−18.29	34.15
	level	504	7,794.3	791.4	3,350.2	7,565.6	12,033.9	123.9	81,051.6
Services VA per worker	growth rate	505	1.17	−1.10	1.08	3.33	4.82	−24.20	53.38
	level	505	8,450.6	1,869.6	3,810.5	7,922.1	11,865.0	152.9	67,612.8
Agricultural VA per worker	growth rate	504	3.22	−1.53	0.84	2.99	38.51	−47.16	724.87
	level	504	11,708.3	496.8	1,789.1	10,615.9	33,081.3	106.6	283,446.9
Industrial VA per worker	growth rate	504	1.73	−1.44	1.72	4.63	5.48	−23.43	25.73
	level	504	9,786.9	2,190.9	4,400.0	10,945.2	13,228.8	297.6	90,083.8
Aid/GDP (%)		505	0.93	0.01	0.23	1.23	1.54	−4.78	10.78
Policy index		505	1.62	1.43	1.70	2.26	0.99	−6.58	2.80
Tropical area index		505	0.67	0.04	1	1	0.44	0	1
Franco-zone dummy		505	0.093	0	0	0	0.29	0	1
Ethnic fractionalization index		505	0.448	0.14	0.5	0.72	0.30	0	0.93
Assasinations number		505	0.349	0	0	0.25	0.75	0	7
ICRG institutional quality index		505	4.949	3.37	4.99	6.11	2.36	0.56	10
Financial depth (M2/GDP), lagged		505	0.311	0.18	0.25	0.40	0.19	0.04	1.30
Sub-Saharan Africa		505	0.269	0	0	1	0.44	0	1
East Asia		505	0.125	0	0	0	0.33	0	1
Real exhange rate devaluation (% change)		427	6.43	0.0046	0.013	0.062	65.68	.000014	1206.4
External debt/GDP (%)		402	61.99	28.94	48.89	76.10	59.43	0	768.15
Long term debt, currency composition (%USD)		409	0.480	0.34	0.48	0.63	0.20	0	0.98

Table A2: Sample

		66-69	70-73	74-77	78-81	82-85	86-89	90-93	94-97	98-01
Argentina	ARG		×	×	×	×	×	×	×	×
Australia	AUS			×	×	×	×	×	×	×
Burkina Faso	BFA		×	×	×	×	×	×	×	×
Bulgaria	BGR							×	×	×
Bolivia	BOL		×	×	×	×	×	×	×	×
Brazil	BRA	×	×	×	×	×	×	×	×	×
Botswana	BWA			×	×	×	×	×	×	×
Canada	CAN			×	×	×	×	×	×	×
Chile	CHL	×	×	×	×	×	×	×	×	×
China	CHN						×	×	×	×
Cote d'Ivoire	CIV				×	×	×	×	×	×
Cameroon	CMR			×	×	×	×	×	×	×
Congo, Rep.	COG						×	×	×	×
Colombia	COL	×	×	×	×	×	×	×	×	×
Costa Rica	CRI		×	×	×	×	×	×	×	×
Denmark	DNK	×	×	×	×	×	×	×	×	×
Dominican Rep.	DOM					×	×	×	×	×
Algeria	DZA								×	×
Ecuador	ECU	×	×	×	×	×	×	×	×	×
Egypt, Arab Rep.	EGY			×	×	×	×	×	×	×
Ethiopia	ETH					×	×	×	×	×
Gabon	GAB					×	×	×		
Ghana	GHA	×	×	×	×	×	×	×	×	×
Gambia, The	GMB		×	×	×	×	×	×		
Guatemala	GTM	×	×	×	×	×	×	×	×	×
Honduras	HND	×	×	×	×	×	×	×	×	×
Haiti	HTI	×	×	×	×	×	×	×	×	×
Hungary	HUN					×	×	×	×	×
Indonesia	IDN		×	×	×	×	×	×	×	×
India	IND	×	×	×	×	×	×	×	×	×
Iran, Islamic Rep.	IRN			×	×	×	×	×	×	×
Jamaica	JAM			×	×	×	×	×	×	×
Japan	JPN		×	×	×	×	×	×		
Kenya	KEN		×	×	×	×	×	×	×	×
Korea, Rep.	KOR		×	×	×	×	×	×	×	

		66-69	70-73	74-77	78-81	82-85	86-89	90-93	94-97	98-01
Sri Lanka	LKA	×	×	×	×	×	×	×	×	×
Morocco	MAR	×	×	×	×	×	×	×	×	×
Madagascar	MDG		×	×	×	×	×	×	×	×
Mexico	MEX		×	×	×	×	×	×	×	×
Mali	MLI						×	×	×	×
Malaysia	MYS		×	×	×	×	×	×	×	×
Nigeria	NGA	×	×	×	×	×	×	×	×	×
Nicaragua	NIC		×	×	×	×	×	×	×	×
Norway	NOR			×	×	×	×	×	×	×
New Zealand	NZL				×	×	×	×	×	×
Pakistan	PAK	×	×	×	×	×	×	×	×	×
Peru	PER	×	×	×	×	×	×	×	×	×
Philippines	PHL	×	×	×	×	×	×	×	×	×
Papua New Guinea	PNG					×	×	×	×	×
Poland	POL								×	×
Paraguay	PRY	×	×	×	×	×	×	×	×	×
Romania	ROM							×	×	×
Senegal	SEN		×	×	×	×	×	×	×	×
Singapore	SGP	×	×	×	×	×	×	×	×	×
Sierra Leone	SLE	×	×	×	×	×	×	×	×	×
El Salvador	SLV	×	×	×	×	×	×	×	×	×
Togo	TGO			×	×	×	×	×	×	×
Thailand	THA	×	×	×	×	×	×	×	×	×
Trinidad & Tobago	TTO			×	×	×	×	×	×	
Tunisia	TUN		×	×	×	×	×	×	×	×
Turkey	TUR		×	×	×	×	×	×	×	×
Tanzania	TZA							×	×	×
Uganda	UGA					×	×	×	×	×
Uruguay	URY	×	×	×	×	×	×	×	×	×
United States	USA			×	×	×	×	×	×	×
Venezuela, RB	VEN	×	×	×	×	×	×	×	×	×
South Africa	ZAF							×	×	×
Congo, Dem. Rep.	ZAR		×	×	×	×	×	×	×	
Zambia	ZMB	×	×	×	×	×	×	×	×	×