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Energy, Economic Growth, and Poverty Reduction

A LITERATURE REVIEW

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Energy, Economic Growth, and Poverty Reduction: A Literature Review

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Abbreviations

ADB	Asian Development Bank
ARI	acute respiratory infection
CGE	computable general equilibrium
CPI	consumer price index
DALYs	disability-adjusted life years
FCFA	franc Communauté Financiere Africaine
GDP	gross domestic product
GMM	generalized method of moments
IEA	International Energy Agency
IO	input-output
IV	instrumental variable
kWh	kilowatt-hour
LSMS	Living Standards Measurement Study
MENA	Middle East and North Africa
OECD	Organisation for Economic Co-operation and Development
OLS	ordinary least squares
PC	principal components
PM _{2.5}	particulate matter with diameter smaller than 2.5 micrometers
PM ₁₀	particulate matter with diameter smaller than 10 micrometers
PSM	propensity score matching
Rs	Rupees
WTP	willingness to pay

Chapter 1: Overview

Introduction

The objective of this paper is to help project teams better articulate the link between their projects and the World Bank Group's twin corporate goals of reducing poverty and boosting shared prosperity. Based on an extensive literature review, this report examines what type of empirical evidence exists to support linking the project interventions to the twin goals. There are many studies citing a strong link between energy, economic growth, and poverty reduction. However, not all of them are robust, and not distinguishing robust studies from flawed ones risks making exaggerated or inaccurate claims about the benefits of an energy project. The detailed review of the published studies contained in this report is intended to point teams to reliable studies and caution against citing results from studies with serious methodological or data problems.

Delivery of reliable modern energy services contributes to poverty reduction and shared prosperity—indirectly through its contribution to economic growth, and directly by enriching the lives of the beneficiaries of such services. For many WBG projects, indirect channels are more important than the direct ones. The argument for indirect contribution rests on the link between growth and poverty reduction, and between energy and growth in turn.

There is ample evidence in the literature that faster economic growth is associated with faster poverty reduction. Using a sample of developing countries, Ravallion and Chen (1997) estimated that, on average, a 1-percent increase in mean income or consumption expenditures in the population reduced the proportion of people living below the poverty line by 3 percent, while *Attacking Poverty* (World Bank 2000) found the elasticity of poverty reduction with respect to growth to be about 2 rather than 3. As expected, there is a great degree of heterogeneity across countries.

To explain the observed heterogeneity in the relationship between economic growth and poverty reduction, Bourguignon (2003) refined the econometric model formulation and proposed specifications that greatly increased the goodness of fit. His revised models link economic growth and poverty reduction, and the analysis supports the postulate that a lesser level of development and a higher level of inequality reduce the growth elasticity of poverty, a point reinforced by Ravallion (2012) who examined data on poverty measures from 90 developing countries. Bourguignon posits that it may be reasonable to argue that an effective long-run policy of poverty reduction should rely primarily on sustained growth, but also that reducing inequality would increase the growth elasticity of poverty reduction, thereby accelerating poverty reduction for a given rate of economic growth.

The purpose of this review of studies linking energy to certain aspects of economic growth is to provide World Bank teams with a guide to those studies that can provide valuable insights into the likely benefits from particular types of energy sector projects. The review is limited to those studies that have carried out statistical analysis in order to assess the significance and magnitude of any links identified.¹ There are

¹ In assessing whether an estimated coefficient is significantly different from zero, a 5-percent test is most commonly used, meaning that there is a 1-in-20 chance that a statistically insignificant coefficient is erroneously judged to be significant. The higher the percentage, the easier it is to find coefficients that are statistically significant, but the lower the confidence level. The results reported here are based on a 5-percent test.

many published studies relating to links between energy and economic growth but not all are reliable. Technical errors are common, and lessons drawn from such studies are not a solid guide to the links being investigated. This review assesses a large number of studies by asking whether

- the theoretical framework underpinning the study is sound;
- the technical basis of investigation to measure quantitative links (econometric/statistical/survey techniques employed) is rigorous;
- the results are plausible; and
- the interpretation of the result is sound.

Many of the problems that arise in the studies reviewed relate to the use of incorrect specification or inappropriate econometric techniques. Alternative approaches are available and have been used in some studies. To understand the circumstances under which some common problems occur and the nature of the econometric solutions adopted, annex 1 explains in simple terms such problems and solutions.

This study reviewed about 200 journal articles and working papers from a variety of institutions, mainly from the last decade. Earlier studies tended to employ methodologies that have since been shown to be flawed by advances in econometrics. Where appropriate, earlier literature was also consulted.

Because the purpose of the review is to identify studies that provide reliable evidence on the links between energy use and economic outcomes, it was necessary to look at both macro-economic and micro-economic studies. The following links were studied in detail:

- Between infrastructure and gross domestic product (GDP)
- Between energy use and GDP
- Between power outages and the performance of businesses
- Between a household's connection to electricity supply and various economic outcomes (income, employment, education etc.).

The first two categories examine possible linkage at a macro-economic standpoint, while the second two carry out the investigation at the micro-economic level of the firms or households that may be affected by a change in outages or connection. The former requires the use of economy-level data and researchers typically rely on whatever relevant published material is available. Micro-economic analysis requires data from surveys of households or businesses. Some studies actually collect such data, but in order to work with a large sample, as is desirable for this type of analysis, it is usually necessary to rely on official surveys, which may be several years old and not have included all the questions that would be desirable for investigations carried out at a later date and for a different purpose than that for which the survey was originally designed. Data availability can be a factor in limiting the reliability of results obtained from studies in many countries.

The review focuses largely on the electricity sector. The number of studies relating fuels used by households and small- and medium-size enterprises—another area of interest to the World Bank—to economic outcomes is small, and the use of formal econometric type modeling to test for the significance and magnitude of such interventions is extremely limited. Under these circumstances, little reliable guidance to teams on a quantitative link between interventions and economic variables linked to

economic growth or poverty reduction could be provided from an analysis of this literature. This study provides a brief description of non-electric household energy in the last chapter.

This overview chapter summarizes the findings of the study. Because many studies employed advanced econometric techniques, and because methodological flaws found in many studies invalidate their findings, the rigor of methodological approaches is discussed in some detail in the chapters that follow. Annex 1, which treats these methodological issues, should ideally be read before proceeding to the next four chapters. Each of chapters 2–5 begins with a summary, followed by a table of the main results of the key studies and their strengths and weaknesses, more detailed discussion of the reviewed studies, and an assessment. Each chapter can be read as a stand-alone chapter. Chapters 2–5 can be highly technical in some places. The intention is to serve as a reference for teams wishing to find out more about the studies and their findings, as well as explain why some studies cannot be used to explain the link between energy and economic outcomes.

Infrastructure and growth

There is general agreement that infrastructure is an important contributor to the growth of an economy. For many developing countries, shortages of infrastructure are seen as acting as a brake on economic growth. Because the power sector is a major component of infrastructure, a demonstrated link between infrastructure and growth supports the hypothesis that power sector infrastructure is linked to growth. Straub (2008a, 2008b) and Calderón and Servén (2014) survey the issues that arise in studying the links between infrastructure provision and growth.

Another reason for examining infrastructure as a whole is the existence of complementarities. The benefits of electricity supplied to a hospital would be greatly reduced if there are no paved roads connecting patients to the hospital, if the hospital had no access to clean water, and if there were poor telephone connections between the hospital and patients. Because of these complementarities, it would be easier to demonstrate a link between a package of interventions—for example, provision of reliable infrastructure such as electricity, telecommunications, transport, and water—to the level of economic output.

Many studies link the amount of infrastructure and the *level of GDP* of an economy through a production function relationship, in which infrastructure is included together with capital and employment in the determination of GDP, while others look for empirical relations between the level of infrastructure and *GDP growth*. In both cases, the definition and measurement of infrastructure is crucial and a number of different approaches have been used, thus generating a variety of results depending on the approach.

In the broadest sense infrastructure relates to electricity, gas, telecommunications, transport (road and rail), water supply, sanitation, and sewerage, although limitations on data availability often restrict attention to a subset of these sectors. Some studies have attempted to measure infrastructure by public capital, but this includes things such as schools, hospitals, and public housing, which are different from the traditional concept of infrastructure. At the same time private stocks of infrastructure may be important (especially for power and telecommunications) and their omission could understate the amount and growth of infrastructure and ascribe too much importance to the influence of public infrastructure on GDP.

Two recent studies are representative of the approaches taken to quantify the link between the stock of infrastructure and GDP, while avoiding econometric problems that have reduced the reliability of results from certain other studies.

- Calderón and Servén (2010a) analyzed the effects of the quantity and quality of aggregate infrastructure indicators on the growth rate of economies, and on the degree of income inequality. Growth rates were measured over five-year intervals between 1960 and 2005 for 97 countries. The infrastructure sectors included were power, telecommunications, and roads; other sectors were not included because of lack of adequate data. A number of other explanatory (control) variables² were also included. The coefficients of the aggregated infrastructure variables were significant, as were some of other explanatory variables. Multiplying actual changes in infrastructure by the coefficients obtained, the model indicated that, on average, between 1991–1995 and 2001–2005, annual world growth increased by 1.6 percentage points due to the increase in infrastructure, of which 1.1 percentage points were due to the accumulation of infrastructure stocks, and 0.5 percentage points to the increase in quality. The largest contribution of infrastructure to economic growth was in South Asia, where it reached 2.6 percentage points, of which quantity accounted for 1.6 percentage points. Sub-Saharan Africa experienced an increase of 0.7 percentage points, of which 1.2 percentage points were due to increasing quantity, while decreasing quality was responsible for a fall of 0.5 percentage points. Tests on the relation to the degree of inequality, as measured by a Gini coefficient,³ showed that during a similar period the model indicated that globally the increase in infrastructure development was related to a decline of three percentage points in the Gini coefficient, of which two percentage points were due to quantity and one percentage point was due to quality.
- Calderón, Moral-Benito, and Servén (2011) followed the production function approach in which the level of GDP was related to human capital, physical capital, and a measure of infrastructure similar to that constructed by Calderón and Servén (2010a). Annual data for the period 1960 to 2000 for 88 countries were used. All three variables were found to be significant and the elasticities of GDP with respect to labor, capital, and infrastructure were 0.1, 0.34, and 0.08, respectively. There was little evidence that there was heterogeneity of the infrastructure elasticity across countries.

Both studies measured the stock of infrastructure in the power sector by the installed generation capacity. Including transmission and distribution capacity in the index of the total infrastructure of the power sector would be desirable but lack of suitable data made this impossible. It is likely that expansion of all components of the power sector would have followed similar paths, making generation capacity a reasonable proxy. The omission of the load factor also may have introduced measurement errors into the infrastructure quantity variable because some developing countries have installed capacity far in excess of what is operational in practice. Measurement errors can lead to bias in the estimated coefficients of the infrastructure variables and hence reduce the reliability of the results obtained.

² Explanatory variables are independent variables appearing on the right-hand side of the equation. In this report, when the explanatory variable is the energy variable of interest (for example, electricity connection or power outages), all other explanatory variables are alternatively called control variables.

³ A Gini coefficient of 0 represents complete equality, and 1 complete inequality with the entire country's wealth belonging to a single person.

The above shortcomings notwithstanding, the results obtained provide strong support for the hypothesis that the stock of infrastructure is a determinant of the rate of economic growth of countries, and by extension that the size of the power sector is a factor in determining the growth and level of GDP. The first study also provided solid evidence that the quality of infrastructure has an effect on the rate of growth of an economy, and that the quality of the power sector output has a substantial role in the overall effect of quality.

The first study also demonstrated a link from infrastructure to inequality. Increases in the quantity and quality of infrastructure were associated with a reduction in inequality. Although the analysis did not provide evidence on which group within the economy was benefitting most from infrastructure improvements, it is likely that the reduction in inequality was a result of a relative improvement of lower-income groups, rather than a relative worsening of upper-income groups.

Energy and growth

It is generally accepted that an adequate supply of reliable energy is essential for economic development. The establishment of a link between increased energy use and the growth of an economy is relevant for many energy sector projects. Projects designed to increase capacity, whether of generation, transmission, or distribution of electricity (including increasing access), or that develop oil or gas deposits, are intended to have as one consequence the facilitation of increased energy production and consumption. If increased energy use leads to increased economic growth of an economy, to the extent that increased economic growth leads to poverty reduction, there is a link between these energy projects and poverty reduction. Hence the existence of a link from energy consumption to increased economic growth indicates benefits beyond those for the direct consumers of the increased energy supply.

The link(s) between the use of energy and the output (GDP) of an economy has been the subject of an extensive academic literature. For the more recent studies the aim has been to test whether higher energy use leads to GDP growth, or GDP growth leads to more energy consumption, or both links coexist, or no causal relation exists between the two variables. The energy-growth linkage has a key difference from the infrastructure-growth linkage in the previous section. The infrastructure-GDP link supposes that the capital stock of the power sector helps determine the level of GDP in the economy. A decrease in the consumption of electricity (as might be caused by a recession) would not be expected to lead to a decrease in GDP because there would be no fall in the amount of infrastructure. By contrast, the use of energy usually includes oil and gas as well as electricity, and the formulation of the model implies that as energy use falls the level of GDP will fall. Energy has been measured in a variety of ways. Studies limiting energy to the power sector have used megawatt-hours, while those including oil and gas have used megajoules, British thermal units, or tons of oil equivalent.

Some authors have interpreted the “growth” hypothesis (increased energy use leads to increased GDP) to imply that energy conservation leads to a fall in GDP. This is in fact confusing a shift of the production function caused by the technical progress (less energy required to produce the same output) with a shift along the production function in which a decline in energy use (caused by factors such as an energy price shock) leads to a fall in GDP. It appears that there are no econometric studies attempting to distinguish these two effects.

More recent studies have taken into account two important methodological issues. First the possibility of two-way causality requires special estimation techniques, and a variety of approaches have been used to do so. Second, both energy use and GDP are non-stationary (they have grown strongly) in most economies, requiring particular econometric techniques to ensure that purely spurious correlations due to a common third factor driving the growth of both series are not used as evidence of causal links between them.

Despite the widespread recognition of these two issues, it has become apparent that in the literature there is a complete lack of agreement concerning the nature of the causal link (if any) between energy and GDP. This point was made by Ozturk (2010) in a survey of literature published in the leading energy journals, and was reinforced by a meta-analysis of 158 articles by Kalimeris, Richardson, and Bithas (2014). The latter found that each of the four possible patterns of causal links had been identified on a roughly equal number of occasions, and that there was no systematic correlation between the causal pattern identified and the methodological approach adopted. Payne reported similar findings for the links between energy consumption and growth (2010a) and between electricity consumption and growth (2010b). A simple correlation between energy (or electricity) consumption and GDP, such as is presented by McKinsey (Castellano et al. 2015), cannot be taken to necessarily support the view that increasing energy use will increase GDP. The causation may be entirely in the reverse direction: increased GDP leads to increased energy use. Only full specification and estimation of both possible links can establish their relative importance.

An issue that has been widely ignored is that of omitted variables. Studies that claim to be testing for the possible simultaneous presence of a production function relation in which higher energy use contributes to GDP growth and a demand function relation in which GDP growth results in higher energy consumption should include all possible major determinants of both relations. Omission of a significant variable leads to bias and possible misidentification of the causal pattern. In the light of this possibility it would be expected that the production function would have included capital and labor variables as well as energy as inputs to the determination of GDP, and that the demand function would have included the relative energy price as well as GDP in the determinants of the use of energy. To evaluate the prevalence of this possible omitted variables bias, the present study surveyed all the articles included by Ozturk (2010) and subsequent publications in the leading energy journals. Out of these 136 studies, 126 applied some form of testing for the direction of causality, and 116 applied testing and estimation techniques that allowed for non-stationarity of the data. However, only 3 articles tested for the direction of causality, allowed for non-stationarity, and included possible major explanatory variables for both the production and demand function relationships (see annex 1). Two of these three studies had other specification problems, leaving the single study by Stern and Enflo (2013) using Swedish data as a reliable guide to the energy-GDP relationship. The Swedish study was based on 150 years of data and indicated that the direction of causality was energy use affecting economic output over the full sample period, while economic output affected energy use in recent smaller samples. Relative energy prices had a significant negative link to both energy use and GDP. Extrapolation of these results to the current experience in developing countries should be undertaken with caution because the type of economy studied and the length of the time period used were quite different.

These findings show that it would be easy to identify a number of studies that claim to support the hypothesis that greater energy consumption drives GDP growth much more than GDP growth drives

energy consumption. However, virtually all these studies had failed to include the key variables in both such relations—instead they would have included only the labor and capital inputs, or only the relative price of energy. As a result they cannot be relied upon to provide a reliable assessment of the significance of the link from energy to GDP.

Power outages

Power outages occur in all countries, but they are frequent and long lasting in many developing countries. It is universally accepted that outages—also referred to as shortages, blackouts, load shedding, loss of load, or unserved energy—result in losses to the economy, and the key policy question relates to the magnitude of the adverse effects. There are other ways in which power quality can decline, such as voltage dips and swells (UNEP 2012), with attendant adverse effects. However, there appears to have been no statistical testing for the effects of power quality changes other than that due to outages.

Although power outages affect all consumers, the main emphasis in studying their effects has been on agriculture and industry. In countries where pumping for irrigation is common and farmers are connected to the grid, outages can result in loss of irrigation and damage to crops, with a corresponding loss of income to the farmers. Electricity is a major input for a number of industries, and its shortages affect firm behavior. Certain newer, high-tech industries are highly dependent on a guaranteed level of power quality, and poor-quality power supply either acts as a constraint on the emergence of these industries in a particular economy, or else forces firms to adopt expensive alternative methods of power supply.

There is an extensive and varied literature on the estimation of the costs associated with outages in an economy, and this can be used to provide insights into the potential benefits from projects that reduce outages, such as more generation or transmission capacity, pricing schemes to reduce peak loads to a level that can be supplied by the existing system, or other investment upgrades that improve the quality of power supply.

Power outages affect households that are already connected to the grid, and may also discourage connection for households that are not yet connected. Most households do not have recourse to backup generation, and for them power outages would mean resorting to other forms of lighting such as candles or kerosene lamps, with lower efficiency and convenience, and the benefits associated with access to electricity—such as longer hours for study for children and powering home businesses—will be lessened (Abdullah and Mariel 2010; Chakravorty, Pelli, and Marchand 2013).

The effects on businesses depend on their reaction, if any, to the existence of power outages. Several different responses (coping strategies) have been analyzed by various authors. These strategies, some of which can be combined, include

- doing nothing and accept the lower sales, revenues, and costs that ensue;
- installing some backup generation, but not sufficient to mitigate all grid power shortages;
- installing sufficient backup generation to mitigate all grid shortages;
- increasing the throughput rate during periods when grid power is available, or increasing operating time when power is available (working weekends), even at the expense of higher per-unit cost of supply;

- changing the nature of the business by switching from making high energy-intensity intermediate inputs to buying them from suppliers facing lower energy costs;
- improving energy efficiency by investing in non-energy inputs; and
- exiting from the business.

These possibilities highlight the difference between *gross* and *net* costs of power outages (UNEP 2012). The gross loss is that felt directly as a result of a power outage before taking into account any coping response. It would correspond to the value of lost sales and the damage to the plant or equipment cost caused by the outage. Net costs start with gross costs, add the capital and operating costs of the coping strategy, less savings on inputs not needed and any recovery of sales due to the coping actions.

The effects of power shortages on the economy have been evaluated using a number of different methods:

1. *Regression modelling* where an outcome variable (such as total costs, income, or productivity) is related to the duration and frequency of outages. This provides a test of the significance of the hypothesis that outages have adverse effects, and permits a quantification of their effects for the data set in question.
2. *Direct-loss approach* where users are asked to evaluate the losses they have sustained from power outages, or would sustain from a hypothetical outage situation. This provides a quantification of the effects of the outage experienced, but does not allow significance testing, and is dependent on recall and the ability of respondents to take into account all costs and coping actions involved. It is necessary to distinguish between gross losses and net losses that include adjustment through coping actions.
3. *Indirect-cost approach* where the costs of installing and operating backup generation is calculated, and applied to those firms that have adopted this solution. Costs of backup generation include the annualized capital cost of the backup plant used, and costs of operating such a plant (fuel and maintenance costs) for the period of the blackout. Foster and Steinbuks (2009) provide a detailed example for the calculation of indirect costs. Indirect-cost calculations should also take into account unrecovered losses where the backup generation is unable to offset all the loss in power from the grid (Oseni and Pollitt 2013).
4. *Willingness to pay (WTP)*, which asks users how much consumers would be willing to pay to be offered a defined improvement in the quality of the power supply. Where consumers have already invested in physical capital (backup generator), their answer would relate only to the extra running costs they are incurring and hence underestimate the total effects of the outages.

The results are implausible in some studies where calculations were based on survey data. For example, a study of Cameroon (Diboma and Tatietsse 2013) estimated that the cost of backup generation was \$4.4 per kilowatt-hour (kWh), while the tariff was \$0.15/kWh. Other studies, such as Foster and Steinbuks (2009), found all estimated costs of backup generation in 19 African countries to be \$0.74/kWh or lower, which are in line with the general level of costs for backup generation. Clearly, the calculation for Cameroon has to be regarded as unreliable until further evaluation demonstrates otherwise. A second example comes from Bose et al. (2006), who calculated the cost of backup generation in India to be \$0.06/kWh, while the grid tariff was \$0.10/kWh. Were backup generation to be so much cheaper than the grid, it would be rational to use self-generation much more widely. Both studies were based on surveys of firms and suggest that survey methods can lead to misleading results.

Three studies stand out as providing quantitative results based on a sound methodology and yielding plausible values for the costs of outages as measured in the particular study:

- Iimi (2011), using regression based on firm-level data, analyzed the effects on total costs of production of the various factor inputs and also the frequency and duration of outages in 26 countries in Eastern Europe and Central Asia. A 1-percent increase in the frequency of outages was associated with a 0.7-percent increase in total costs for given input levels, while a 1-percent increase in the duration of the average outage led to a 1.3-percent increase in costs. Statistical tests showed that small firms were not any more affected by power outages than large firms, although a similar analysis for the water sector showed that small firms were more affected by outages.
- Foster and Steinbuks (2009) studied the costs and benefits of backup generation in 19 African countries. The study provided a detailed account of how the costs of self-generated electricity were calculated, and the results ranged from US\$0.13/kWh to US\$0.74/kWh.
- The World Bank (2001) analyzed agricultural power supply in Andhra Pradesh and Harayana, and quantified its effects on farm incomes in Harayana in 1999. Three sources of inadequate power supply were quantified: (i) the availability of power through the rostering arrangements used to limit total supply; (ii) unscheduled outages during the roster periods; and (iii) transformer burnouts due to over-loading, poor maintenance, or lightning strikes. Farm incomes in Harayana were regressed on a number of variables including measures of these three factors. For medium to large farmers, statistically significant coefficients indicated that an increase of 1 day per year lost to transformer burnout cost US\$107, while an extra hour per day of unscheduled outage cost US\$658. The estimated willingness to pay by these farmers to reduce unscheduled outages by 25 percent was about 15 percent of their base income. No statistically significant results were found for small to marginal farmers.

These studies provide valuable reference points for considering the costs of outages, even though it is clear that these will tend to be country-specific. The results of Iimi (2011) indicated that frequency and duration of outages have different cost elasticities, suggesting that the results from studies that focus either on frequency alone, or on the hours of total lost power per unit time, may have a specification error, resulting in biased estimates of the effects of outages on costs. The papers by Reinikka and Svensson (2002), Allcot, Collard-Wexler, and O'Connell (2014), and Fisher-Vanden, Mansur, and Wang (2012) all introduced useful approaches to the issue of how firms cope with outages, but reduced the reliability of the quantitative results by failing to distinguish between the duration and frequency of such outages.

Survey-based results, especially those attempting to distinguish direct costs, indirect costs, and the willingness to pay, were disappointing. Results were often not credible and appeared to depend heavily on the exact form in which questions were asked. Direct-loss estimates were open to misinterpretation by respondents, memory lapses, and a temptation to overstate the effects of outages.

Some recent articles broadened the discussion of possible coping strategies to include choosing to back up only part of normal power demand (Oseni and Pollitt 2013), choosing to buy rather than make energy-intensive intermediate inputs (Fisher-Vanden, Mansur, and Wang 2012), or altering the pace of throughput and hours worked when grid power is available (Alam 2013). These possibilities need to be considered when calculating the net losses from outages.

Access

A number of studies have attempted to estimate the benefits of electrification on households or small businesses. There are many possible paths by which the use of electricity or other modern fuels might benefit households (Khandker, Barnes, and Samead 2013) and analysis has focused on the estimation of the effects on outcome variables—income, total household expenditures, employment, or various dimensions of education, such as time spent at home studying or the school enrollment rate.

The use of household survey data allows for the inclusion of a large number of factors that might influence the outcome variables, and most of these can be assumed to be exogenous—that is, they could affect the outcome variables but are not affected by them. Classic examples of household-specific factors are the age, gender, or the education level of the head of household.

A second group of factors are common to all households in a village or commune, such as the presence of an all-weather road, school, or the distance to local market, which vary from village to village. Where these factors are measured they can be added to the list of explanatory variables. If not, one way of accounting for them is to introduce a “fixed effect” for each village, whereby these common factors are assumed to affect every household in the same village by the same amount, but the effects may vary across villages.

Earlier studies assumed that the outcome variable of interest would be affected by these household- and village-level variables, including the household’s electrification status, and carried out ordinary least squares (OLS) estimation of the coefficients of the explanatory variables. The coefficient on the electrification variable was then assumed to measure the increase in income or any other outcome variable enabled by electrification.

More recently, a number of studies have focused on the possibility that the electrification status of a household is endogenous: that is, not only does it affect income, but the level of income determines whether or not the household is electrified. This can come about by a “placement effect,” in which the electric utility shows preference for providing electricity first to higher-income villages (because more households are likely to connect, hence lowering per-unit costs). It can also come about because where a village has access to electricity (for example, the village has been connected to the grid), the households willing to connect are those with higher incomes (especially if connection charges are not fully subsidized), producing a selection bias. Two recent studies have introduced the hypothesis that non-connected households in villages where there is access to grid electricity have benefited from this electrification. A statistically significant effect of access for non-connected households was identified, suggesting that the total benefits of electrification may have been underestimated in previous studies.

The effect of such endogeneity is to impart an upward bias to the estimation of (in other words, overstate) the effects of electrification on income, so that studies not taking this endogeneity into account do not provide reliable estimates of the benefits of electrification. Alternative methods of estimation are required and three approaches have been used: instrumental variable (IV) estimation, propensity score matching (PSM), and panel data analysis allowing for heterogeneity between households. Studies using these methods have found clear evidence that the electrification status of households is endogenous, and that ignoring such endogeneity can over-estimate benefits.

A study of firms in Benin by Peters, Vance, and Harsdorff (2008) showed that electrification of a village was followed by the creation of certain electricity-reliant firms. These had significantly higher profits than non-reliant firms in areas with and without access to electricity. Non-reliant and connected firms in areas with access performed no better than similar firms in areas without access.

Several studies of the effects of electrification on households measured the effect on income or consumption. Kumar and Rauniyar (2011) found that farm income in Bhutan was unaffected but that non-farm income increased by 63 percent; Khandker et al. (2012) found that non-farm income in India rose by 70 percent; and a study of Vietnam by Khandker, Barnes, and Samad (2013) showed total income increasing by 28 percent. Consumption also increased significantly in some studies: Khandker et al. (2012) estimated an increase of 18 percent in India, and Khandker, Barnes, and Samad (2013) reported a 23-percent increase in Vietnam. Van de Walle et al. (2013) reported only a 7-percent increase in India for connected households, but estimated that unconnected households in villages where there was access also had consumption increasing by 1 percent a year due to the electrification of the village. These results suggest that electrification does result in an increase in household income (or consumption as a proxy), but that the magnitude varies considerably from country to country. An important finding is that unconnected households in villages where there is access to grid electricity also exhibit some increase in consumption.

The effects on education and employment generally have indicated a variety of effects. A study by Grogan and Sadanad (2013) found that women in Nicaragua were 23 percent more likely to work while there was no change for men; Dinkelman (2011) found female employment in rural Kwazulu-Natal in South Africa increased by 30 percent with no significant effect for men; Khandker et al. (2012) found that women in India were 17 percent more likely to work, with no significant effect for men. However, van de Walle et al. (2013) found the reverse situation in their study of India—male labor supply increased by about 16 days a year while there was no significant effect for female labor. All studies indicated that there was an increase in household employment following electrification, and in the majority of cases this was for women only. However, the very detailed study by van de Walle et al. (2013) found the reverse. Without further work that is able to explain these different results it is reasonable to conclude that electrification increases employment, but not that this will be confined to females.

Kumar and Rauniyar (2011) estimated that electricity connection in Bhutan increased the time spent in schooling by 0.54 years, and the time spent on homework by 10 minutes a day. Khandker et al. (2012) found that in India there were significant increases for boys enrollment (6 percent), study time at home (1.4 hours/week), and years of education completed (0.3 years), and for girls enrollment (7 percent), study time at home (1.6 hours/week), and years of education completed (0.5). Van de Walle et al. (2013) found significant increases in India in enrollment (9 percent) and completion rates for girls (9 percent) but not for boys. Khandker, Barnes, and Samad (2013) found completion rates for education in Vietnam were significant for boys (0.1 years) and girls (0.9 years), while enrollment rates were insignificant for both. The latter two studies allowed for connection of the household and also for non-connected households when the village or the commune had access to the grid. This group of studies supports the view that electrification leads to more education as measured by enrollment, years of education completed, or both. In addition two studies indicate that more time is spent on studying at home. Again there is variation among countries as to the magnitude of these effects and there is no direct evidence within these studies on how increased education leads to increased income.

The variations in results obtained may be in part due to different specifications of the models used to explain the outcome variables and to different estimation techniques. However, it is also very likely that there are substantial differences among countries. Caution should therefore be exercised in extrapolating results.

A related issue is that of household air pollution. Households that cook with solid fuels (wood, charcoal, coal) are exposed to high levels of indoor air pollution, of which fine particulate matter (particles smaller than 2.5 microns in diameter) is especially harmful to the health of those in close proximity to the cooking source. Policies to reduce household air pollution are receiving increased attention, but there are a number of difficulties in evaluating the benefits of doing so. Duflo, Greenstone, and Hanna (2008) provide a valuable literature review arranged around four questions:

- How is indoor air pollution linked to fuel types and cooking stove technologies?
- How is health linked to levels of indoor air pollution?
- How is economic productivity of the household linked to health issues caused by indoor air pollution?
- What policies are available to reduce levels of indoor air pollution?

These questions can be tackled at the individual project level or at a national or even global level. Ezzati and Kammen (2002) studied in great detail the incidence of household air pollution in 55 households in Kenya and used health monitoring over a two-year period to assess the health effects of the levels of pollution found in different circumstances. They estimated, for example, that the introduction of a ceramic woodstove, not requiring any shift in fuel, would reduce the level of acute respiratory infection (ARI) by 25 percent for children under the age of four, while the combination of cooking outside with an improved stove would reduce ARI by 65 percent for females between the age of 5 and 14. This study was limited by measuring household air pollution by particulate matter smaller than 10 microns (PM_{10}) in diameter rather than more relevant 2.5 microns.

Two recent studies examined the global burden of disease caused by air pollution from household use of solid fuels in 1990, 2005, and 2010. Both expressed the burden of disease in terms of mortality (deaths per year) and morbidity (disability-adjusted life years, or DALYs).

Lim et al. (2012) considered solid fuel use for both cooking and heating, and found that household pollution from solid fuel use in 2010 was the fourth most serious global cause of the burden of disease, accounting for 3.5 million deaths and 111 million DALYs. In South Asia and most of Africa it was the most serious or second most serious risk factor.

Chafe et al. (2014) studied the contribution of household air pollution from cooking to ambient air pollution. They estimated that about 12 percent of population-exposure weighted average ambient $PM_{2.5}$ globally was attributable to household use of solid fuel cooking, and in Sub-Saharan Africa the share was as high as 37 percent in 2010. South Asia had a share of 26 percent, but the overall level of ambient $PM_{2.5}$ was far higher. Worldwide household cooking in 2010 resulted in an estimated 370,000 deaths and 9.9 million DALYs from ambient air pollution. The vast majority of deaths occurred in South Asia and East Asia, while the number of deaths in sub-Saharan Africa was comparatively small. However, between 2005 and 2010, the number of deaths and the morbidity actually increased in Sub-Saharan Africa, while declining in East Asia.

Chapter 2: Infrastructure and growth

Summary of findings

The effects of increased infrastructure on an economy have been the subject of intense investigation, particularly for developed countries (Straub 2008a, 2008b, Calderón and Servén 2014). For developing countries many studies have focused on a single component of infrastructure, but recently some studies have taken a broader view of the components of infrastructure and have related a measure of aggregate infrastructure to the economic growth of the economy. A study by Urrunaga and Aparicio (2012) used a similar approach to explore the relation between infrastructure provision and economic growth at a regional level within Peru. An important feature of more recent work has been the distinction between the quantity and quality of infrastructure, reflecting observations that poor quality of infrastructure appears to play a role in limiting the growth of economies.

As an extension of this work some studies have also investigated the relationship between the level of infrastructure and the degree of income inequality in an economy, with the aim of testing the hypothesis that an increase in the provision of infrastructure leads to a reduction in inequality through the widening of access of the poor to these infrastructure services.

Two distinct specifications of the effects of infrastructure are found in the literature: (i) a production function approach in which the GDP of an economy is related to factor inputs (typically labor and capital) and also to the level of infrastructure; and (ii) a growth regression in which the rate of growth is related to the level of infrastructure and other control variables. Both approaches are included in the studies reviewed. The relatively small literature on the effects of total infrastructure provision on GDP in developing countries led to the selection of six studies published in recent years as representative of this section of the energy literature. Table 1 summarizes the results of the studies analyzed and indicates an assessment of their value in providing usable information. Two studies stand out as providing solid results and reflecting these two different approaches.

- Calderón and Servén (2010a)⁴ analyzed the effects of the quantity and quality of aggregate infrastructure indicators on the growth rate of economies, and on the degree of income inequality. Growth rates were measured over five-year intervals between 1960 and 2005 for 97 countries. The infrastructure sectors included were power, telecommunications, and roads—other sectors were not included because of lack of adequate data. For these three sectors aggregate indices of quantity and of quality were constructed, and the generalized method of moments (GMM) was used to estimate the dynamic panel structure involved in the model. A number of control variables were also included. The coefficients of the aggregated infrastructure variables were significant, as were some of the control variables. Multiplying actual changes in infrastructure by the coefficients obtained, the model indicated that, on average, between 1991–1995 and 2001–2005, annual world growth increased by 1.6 percentage points due to the increase in infrastructure, of which 1.1 percentage points were due to the accumulation of infrastructure stocks and 0.5 percentage points to the increase in quality. The largest contribution was in South Asia, where it reached 2.6 percentage points, of which quantity accounted for 1.6 percentage

⁴ Calderón and Servén (2010b) presented almost identical results focused on drawing detailed implications for Latin America rather than for Sub-Saharan Africa as was done in Calderón and Servén (2010a).

points. Sub-Saharan Africa experienced an increase of 0.7 percentage points, of which 1.2 percentage points were due to quantity, while poor quality was responsible for a fall of 0.5 percentage points. Tests on the relation to the degree of inequality, as measured by a Gini coefficient, showed that during a similar period the model indicated that globally the increase in infrastructure development was related to a decline of three percentage points in the Gini coefficient, of which two percentage points were due to quantity and one percentage point was due to quality, while for Sub-Saharan Africa larger infrastructure stocks reduced the Gini coefficient by three percentage points, and the worsening quality of infrastructure services increased the Gini coefficient by one percentage point.

- Calderón, Moral-Benito, and Servén (2011) followed the production function approach in which the level of GDP was related to human capital, physical capital, and a measure of infrastructure similar to that constructed by Calderón and Servén (2010a). A dynamic panel approach was used with annual data from 1960 to 2000 for 88 countries. The model was tested for stationarity and the variables were found to be cointegrated. The explanatory variables were found to be weakly exogenous, thus confirming the relation between the variables as a production function. All three variables were found to be significant and the elasticities of GDP with respect to labor, capital, and infrastructure were 0.1, 0.34, and 0.08 respectively. Given the share of electricity in the overall infrastructure quantity index, these imply that the elasticity of GDP with respect to electricity is about 0.03. As an illustration of the importance as well as statistical significance of the results, the authors calculated that an increase in the level of total infrastructure from the cross-country median value in 2000 (similar to that in Tunisia) to that of the 75th percentile would have resulted in a 7.7 percent increase in GDP per worker in the entire economy (not worker employed in the infrastructure sectors).

Table 1: Survey of results on infrastructure from papers selected

Authors	Countries and time period	Method	Findings	Robustness of results
Calderón and Servén (2010a)	97 countries. Non-overlapping 5-year averages from 1960 to 2005	GMM-IV estimation. Growth related to infrastructure quantity and quality indices	Quantity and quality of infrastructure have significant effects on increasing growth rates and on reducing inequality. Over a ten-year period the increase in the quantity of infrastructure was estimated to add 1.1 percentage points to average global annual GDP growth while increased quality added 0.5 percentage points. The Gini coefficient was estimated to have fallen by 3 percentage points	Solid methodology. Results provide plausible values for effects of infrastructure quantity and quality. Tests showed no evidence of heterogeneity of infrastructure effects.

			due to the impact of infrastructure.	
Calderón, Moral-Benito, and Servén (2011)	88 countries. Annual data 1960–2000.	After testing for stationarity and cointegration, estimated production function relation of GDP to inputs (including quantity of infrastructure) using heterogeneous panel technique.	Quantity of infrastructure has a significant effect on GDP: elasticity is 0.08. The elasticity with respect to electricity is about 0.03. Extensive testing showed no heterogeneity of the elasticity.	Solid methodology taking account of non-stationarity and possible endogeneity of explanatory variables. Quality of infrastructure was not included and there was no discussion of the implications of this omission.
Seneviratne and Sun (2013)	76 countries, annual data from 1980 to 2010.	Regression of Gini coefficient on infrastructure quantity and quality aggregate indices and control variables. Variables were measured as deviations from country means.	Quantity and quality of infrastructure both decrease inequality significantly. The elasticity of the Gini coefficient with respect to quantity is -0.18 and for quality is -0.19.	Useful update of Calderón and Servén (2010a). Did not test for effect of infrastructure on growth or GDP. Used lagged values of infrastructure variables to control for endogeneity bias, rather than more advanced techniques as in Calderón and Servén.
Sahoo, Dash, and Nataraj (2012)	China. Annual 1975–2007.	Production function including infrastructure aggregate index. After testing for non-stationarity and cointegration, estimation by GMM.	All coefficients significant except labor. Elasticity of GDP with respect to infrastructure 0.36. Infrastructure sector level elasticities were all between 0.1 and 0.16, and that for power consumption 0.16.	Useful study for a single economy. Not all components of infrastructure index looked plausible and may induce bias. Capital stock was proxied by private and public investment. Aggregate infrastructure elasticity appears high by comparison with global studies.
Seethepalli, Bramati, and Veredas (2008)	16 countries in East Asia. Annual data from 1985 to 2004.	Production function including individual infrastructure variables aggregated over 5-year intervals. Pooled regression with control and infrastructure variables entered one at a time.	Labor and capital are not significant when different infrastructure variables are included. Infrastructure variables are highly significant and GDP elasticities range from 0.55 for roads to 1.0 for electricity and 5.5 for telecoms.	Methodology is not robust. Measurement of capital by gross domestic fixed capital formation and labor by an education level variable and use of least squares in the face of problems of non-stationarity and endogeneity may be distorting results.

Additional control variables affect infrastructure elasticities but not significance of labor and capital.	Elasticities seem much too high to be credible.
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Source: Authors' analysis of the cited papers.

Introduction

Even more than is the case with energy, there is general agreement that infrastructure is an important contributor to the growth of an economy. For many developing countries, shortages of infrastructure are seen as acting as a brake on economic development, and country-level strategies pay close attention to remedying deficits in the supply of infrastructure. Because the power sector is one of the components of infrastructure, a link between infrastructure and growth supports the hypothesis that energy is linked to growth.

Another reason for examining infrastructure is complementarities. The benefits of electricity supplied to a hospital would be greatly reduced if there are no paved roads connecting patients to the hospital, if the hospital had no access to clean water, and if there were poor telephone connections between the hospital and patients. Because of these complementarities, it would be easier to demonstrate a link between a package of interventions—for example, provision of reliable infrastructure such as electricity, telecommunications, transport, natural gas, and water.

There is a correspondingly large literature that attempts to assess the contribution of infrastructure to growth, and these studies face similar methodological issues (see annex 1) to those of the energy-growth nexus. There are additional issues created by the definition and nature of infrastructure itself. A review of these issues is given by Straub (2008a, 2008b), Estache and Garsous (2012), and Calderón and Servén (2014).

Many studies link the amount of infrastructure and the *level of GDP* of an economy through a production function relationship, in which infrastructure is included together with capital and labor in the determination of GDP, while others look for empirical relations between infrastructure and *GDP growth*. In both the definition and measurement of infrastructure is crucial and a number of different approaches have been used, thus generating a variety of results depending on the approach followed.

The definition of infrastructure

In the broadest sense infrastructure relates to electricity, gas, telecommunications, transport (road and rail), water supply, sanitation, and sewerage, although limitations on data availability often restrict attention to a subset of these sectors.

Some studies have attempted to measure infrastructure by public capital, but this includes things such as schools, hospitals, and public housing, which are different from the traditional concept of infrastructure. At the same time private stocks of infrastructure may be important (especially for power and telecoms) and their omission could understate the amount and growth of infrastructure.

The measurement of infrastructure can be made in terms of the expenditure on the items identified or by physical stocks. Where procurement is inefficient, or even corrupt, public expenditure can overstate the public stock of infrastructure.

Bearing in mind these issues, recent studies have tended to use physical stocks of infrastructure, both public and private, as the variable for testing the hypothesis that increased infrastructure leads to increased growth of an economy or to a higher level of GDP.

Methodological issues

The basic approaches to estimating the effect of infrastructure on growth are either to estimate an aggregate production function or to use an empirical growth regression. Both lead to a number of methodological problems that have been recognized and addressed in recent literature.

- The multidimensional nature of infrastructure. Physical infrastructure relates to the combined effect of several individual components (for example, transport, telecommunications, and power) so that relating growth to a single indicator is likely to result in a biased estimator. However, entering several different variables into the estimated relation may result in rather imprecise estimates of the contributions of the individual components (Seethepalli, Bramati and Veredas 2008). A number of recent studies have found a way to compromise between these problems by taking a weighted average of the infrastructure indicators used for the different sectors. The first principal component (PC) of these series chooses the weights so that the constructed indicator is as highly correlated with the individual series as possible. This approach was used by Calderón and Servén (2010a, 2010b), Calderón, Moral-Benito, and Servén (2011), Sahoo, Dash, and Nataraj (2012), and Senerviratne and Sun (2013). Its acceptability as an average was tested by Calderón and Servén (2010a) by comparing the results using the PC with those obtained by entering the variables separately but simultaneously into the model. The authors found that the unrestricted and the restricted PC estimates were not significantly different. Using principal components to construct indices of infrastructure quantity and quality faces two problems. First, only power, telecommunications, and roads were included, leaving the possibility that some other important factors (water, other transport) may have been omitted, producing biased estimates of the impact of infrastructure. Second, the principal component is constructed so as to be correlated with the variables included. Collapsing three variables into one inevitably loses some information and may omit exactly the aspects that are most highly correlated with growth.
- The quality of the infrastructure. Although information on the quality of infrastructure is limited, it is desirable to make some allowance for it so that cases with a large quantity but poor quality can be distinguished from a case with similar quantity but high quality. Calderón and Servén (2010a, 2010b) and Senerviratne and Sun (2013) include an infrastructure quality index calculated as the PC of individual-sector quality indices.
- The measurement of infrastructure quantities. The recent studies of the infrastructure-growth link have used a cross-section or panel approach. Both require that data be available for a large number of countries. Because the availability of physical stocks of infrastructure is limited, compromises have to be made between including a wider range of sectors and

including only those sectors for which a satisfactory indicator is available. Table 2 shows the choices made in six recent studies, all of which drew their data from the World Development Indicators.

Table 2: Indicators of infrastructure quantities and qualities in selected studies

Sector	Seethepalli, Bramati and Veredas 2008	Calderón and Servén 2010a, 2010b	Calderón, Moral-Benito and Servén 2011	Sahoo, Dash, and Nataraj 2012	Senerviratne and Sun 2013
Quantity indicators					
1. Power	Consumption in kWh per capita	Generation capacity in megawatts per worker	Generation capacity in megawatts per worker	Consumption in kWh per capita	Consumption in kWh per capita
2. Telecoms	Main plus mobile lines per capita	Main plus mobile lines per worker	Main plus mobile lines per worker	Main plus mobile lines per capita	Main plus mobile plus internet users per 100 people
3. Roads	Kilometers of paved roads per capita	Kilometers of roads per square kilometer of surface area	Kilometers of roads per worker	Length of paved roads as percentage of total roads	Kilometers of road per 100 square kilometers of land area
4. Water	% of population with access to improved water source	—	—	—	—
5. Sanitation	% of population with access to improved facility	—	—	—	—
6. Energy use	—	—	—	Consumption in kilograms oil equivalent per capita	—
7. Rail	—	—	—	Kilometers per capita	—
8. Air transport of freight	—	—	—	Millions tons per kilometer flown	—
Included in composite indicator (PC)					
	—	1+2+3	1+2+3	1+2+3+6+7+8	1+2+3
Quality indicators					
9. Power	—	Transmission and distribution losses as % of total output	—	—	Transmission and distribution losses as % of total output
10. Telecoms	—	Waiting time for installation of line	—	—	—

Sector	Seethepalli, Bramati and Veredas 2008	Calderón and Servén 2010a, 2010b	Calderón, Moral-Benito and Servén 2011	Sahoo, Dash, and Nataraj 2012	Senerviratne and Sun 2013
11. Roads	—	Share of paved roads in total road network	—	—	Share of paved roads in total road network
Included in composite indicator (PC)					
	X	9+10+11	X	X	9+11
Number of countries	20	97	88	1	16
Data period	1985–2004	1960–2005	1960–2000	1975–2007	1980–2010
Estimation method	Pooled regression with 5-year average data points	Panel IVs with 5-year average data points	Dynamic heterogeneous panel using pooled mean group estimator	Dynamic panel with generalized method of moments estimator	Pooled regression with 5-year average data points

Source: papers cited.
— = not used.

Certain features of data measurement should be noted. The omission of some infrastructure sectors, notably water and non-road transport, is largely due to the lack of data in many countries. For included variables some of the studies do not give justifications for the particular measures chosen.

- Calderón and Servén (2010a) and Senerviratne and Sun (2013) scale the length of the road network by the area of the country (or the area of arable land) in order to remove the effects of differences in the size of countries. Other studies scale by population.
- The quantity of infrastructure in power was measured by generation capacity or by the consumption of power (per capita). Total capacity might be thought to include transmission and generation assets, but these would be difficult to aggregate with generation in physical units. Furthermore, there is likely to be high correlations between transmission or distribution assets and generation assets, because they have to be scaled to support the maximum demand on the system. However, in many countries the load factor is low and there is substantial unusable capacity, leading to the total infrastructure being overstated.
- The single study that included a measure of non-road transportation (Sahoo, Dash, and Nataraj 2012) used a narrow measure of rail infrastructure (rail length), thus omitting rolling stock considerations. The measure for air freight (tons carried per kilometer flown) reflects the average size of the aircraft used, but does not represent the size of the fleet. The variable for roads (paved roads as percentage of the total) is in effect a quality variable rather than a quantity variable.
- **Simultaneity.** Although infrastructure as measured by physical capital reflects decisions made over many prior years, there is still the possibility that economies that have high current GDP also have had high GDP in the past. Such a trend would lead to a link between infrastructure and GDP that reflects the decision of high-income countries to have a high level of infrastructure. This

simultaneous link between the series means that a simple regression of GDP on infrastructure would give a biased estimate of the effect of increasing infrastructure. The study by Seethepalli, Bramati and Veredas (2008) ignored this issue, while Calderón, and Servén (2010a, 2010b) used an instrumental variable (IV) approach. Internal instruments (lags of explanatory variables) were coupled with external instruments (urban population and population density). Sahoo, Dash, and Nataraj (2012) and Calderón, Moral-Benito, and Servén (2011) use dynamic panel estimates and established that there is a single direction of causation through the estimation of the cointegration equation. Senerviratne and Sun (2013) allowed for possible endogeneity of the level of infrastructure in the equation explaining the level of the Gini coefficient by using only lagged values of the infrastructure variables. It is also important to note that the use of gross domestic fixed capital as a proxy for the capital stock in the estimation of a production function is more likely to lead to simultaneous bias, because the capital stock is largely predetermined in the current period. The studies by Seethepalli, Bramati and Veredas (2008), and Sahoo, Dash, and Nataraj (2012) used gross domestic fixed capital formation rather than capital stock data.

- Non-stationarity. The use of time series data in all the above studies raises the issue of non-stationarity. Where the number of countries in the sample is much larger than the number of observations in each country, as is the case where non-overlapping five-year averages are used (Seethepalli, Bramati and Veredas 2008; Calderón and Servén 2010a), the likelihood of spurious correlation is reduced. The use of dynamic panel analysis allowed two of the studies to correct for the effects of non-stationarity. Calderón, Moral-Benito, and Servén (2011) introduced the stock of physical capital and a measure of secondary education as the other variables in the production function that was tested for stationarity and cointegration, while Sahoo et al (2012) used the total labor force and the sum of private and public investment.
- Heterogeneity. The inclusion of quality variables can be seen as a method of correcting for a form of heterogeneity that would not otherwise be accounted for. In addition, country-specific unobservable effects are allowed for by the use of the appropriate panel fixed-effects model (Calderón and Servén 2010a; Calderón, Moral-Benito, and Servén 2011; Senerviratne and Sun 2013). Seethepalli, Bramati, and Veredas (2008) included some additional variables in their growth equations: quality of governance, share of private participation in infrastructure investment, country income level, extent of rural-urban inequality of access to infrastructure services, and a dummy variable for island economies. Calderón and Servén (2010a, 2010b) used as explanatory variables initial GDP per worker, secondary enrollment in education, private domestic credit as a percentage of GDP, trade volume as a percentage of GDP, inflation rate, government consumption as a percentage of GDP, political risk index, and the difference of the terms of trade between successive years.

Despite the variations in data and estimation method, all the studies concluded that infrastructure is a statistically significant determinant of GDP growth. The studies are not directly comparable when assessing the magnitude of the infrastructure effect on growth.

In their estimation of the production function, without extra explanatory variables, Seethepalli, Bramati, and Veredas (2008) found that each of the five components of infrastructure had a positive and significant effect on growth. Elasticities of GDP with respect to infrastructure were very high, ranging from 5.5 for telecoms to 1.0 for electricity and 0.6 for roads. The addition of the explanatory variables, one at a time, tended to reduce the size of the elasticities, and that for electricity varied between 0.8 and 2.2.

The studies by Calderón and Servén (2010a, 2010b) found that the quantity and quality of infrastructure were both significant and positive. Of the explanatory variables, only initial GDP per worker taken over the country as a whole (negative coefficient) and secondary education (positive coefficient) were significant. Because the study linked the level of the infrastructure indicator to the growth of GDP, the coefficient could not be interpreted as an elasticity. Multiplying actual changes in infrastructure by the coefficients obtained, the model indicated that, on average, between 1991–1995 and 2001–2005, annual world growth increased by 1.6 percentage points due to the increase in infrastructure, of which 1.1 percentage points were due to the accumulation of infrastructure stocks, and 0.5 percentage points to the increase in quality. The largest contribution of infrastructure to GDP growth was in South Asia, where it reached 2.6 percentage points, of which quantity accounted for 1.6 percentage points. Sub-Saharan Africa experienced an increase of 0.7 percentage points, of which 1.2 percentage points were due to quantity while poor quality was responsible for a fall of 0.5 percentage points. Tests on the relation to the degree of income inequality, as measured by a Gini coefficient, showed that during a similar period the model indicated that the increase in infrastructure development was related to a three percentage point decline in the Gini coefficient globally, of which two percentage points were due to quantity and one to quality. That is, the increase in infrastructure led to a reduction in the measure of inequality within the economy.

Calderón, Moral-Benito, and Servén (2011) estimated the model in a form in which the coefficients were estimates of the elasticities. That of the capital stock was 0.34 and that of human capital was 0.1, both similar to values previously noted in the production function literature, while that of the infrastructure index was 0.08. These results proved robust to variations in specification, and there was little evidence of cross-country heterogeneity in the elasticity of GDP with respect to infrastructure. Because the coefficient of electricity generation capacity in the PC was 0.35, the elasticity of GDP with respect to power was about 0.03. Because a reduction in outages is equivalent to an increase in capacity, it would also be possible to use this estimated elasticity to generate a crude estimate of the effect of a reduction in outages on the economy.

Sahoo, Dash, and Nataraj (2012) found that the GDP elasticity of infrastructure as a whole for China was significant and positive with a value of 0.36, while those for individual infrastructure sectors ranged from 0.09 for roads to 0.16 for electricity.

Seneviratne and Sun (2013) focused solely on the effect of infrastructure quantity and quality on inequality and found that the elasticity of inequality (as measured by a Gini coefficient) was -0.18 with respect to the quantity of infrastructure and -0.19 with respect to quality.

Urrunaga and Aparicio (2012) used data from the 24 regions of Peru for the period 1980–2009 and related regional growth to three infrastructure variables (electricity, telecommunications, and roads) as well the economically active population adjusted for human capital, and the non-infrastructure capital stock. All three infrastructure variables were significant in the differences model, and the elasticity of output with respect to electricity was 0.09. The elasticity with respect to human capital was 0.47, while that with respect to non-infrastructure capital was 0.11. The overall goodness of fit was very low, suggesting that there were important factors explaining the change in GDP that had not been included in the model. Data limitations prevented the authors from including other important infrastructure variables (water and sanitation, port and airport capacity, and broadband infrastructure). In addition the study noted that the infrastructure index, obtained via addition of standardized scores for the different components, did not

allow for complementarity between inputs. Despite these problems the study provides evidence to support the hypothesis that regional variations in infrastructure contribute to regional differences in growth.

A recent study by Warner (2014) examined the links between public investment and the rate of growth of GDP for a number of developing countries. A novel feature of the study was the focus on “boom” episodes in which the ratio of public investment to GDP was unusually high for several years. The use of these episodes increased the probability that the ratio was exogenously determined—the government of the country in question had made a conscious decision to increase public investment, usually with the goal of increasing the growth rate of the economy. Using data from 126 countries spanning 1960–2011, Warner was able to identify 21 countries for which there were identifiable public investment boom episodes. For these the annual growth of real GDP per capita was regressed using OLS on current and lagged values of the ratio of public investment to GDP relative to a baseline non-boom value, and a number of other explanatory variables. If public investment affects growth it would be expected that lagged value would be significant because of the time required to complete projects. Warner found only a very weak correlation between the current public investment/GDP ratio and the growth of GDP, while lagged value were all insignificant. A number of sensitivity tests were carried out and all confirmed the lack of impact of public investment booms on long term economic growth. The study used a combination of time series and cross-section data but did not address the issue of heterogeneity between countries, except to note that Ethiopia was an outlier. The very low values of the multiple correlation coefficient obtained—less than 0.2 for the principal variants tried—raise doubts that all factors explaining the growth rate were included in the equations.

Some key differences between the Warner study and the studies reviewed above mean that at this stage the results obtained by the former cannot be taken to refute the finding of the latter that infrastructure quantity has a positive effect on the growth rate. The measurement of the principal explanatory variable is different—Warner used financial values of government public investment (or public capital), while the other studies used physical measures of public plus private capital for the main infrastructure sectors. A number of factors can lead to differences between these two approaches. First, private capital, especially for power and telecoms, can be substantial. Second, financial values can overstate the physical stock when there are procurement and project execution inefficiencies. Third, for different countries the definition of what is included in public investment can vary. These measurement problems, coupled with the lack of any attempt to adjust the estimation technique for the cross-section time-series nature of the data and possible heterogeneity, limit the inferences that can be drawn at this stage from the finding that public investment booms do not appear to increase the growth of GDP.

Assessment

The various studies reviewed all confirmed that infrastructure has a significant effect on GDP or the growth of GDP. Furthermore, those studies that have distinguished between the quantity and quality of infrastructure found that both had significant effects. The studies that had taken the most care with variable specification and econometric methodology provided robust evidence that the contribution of infrastructure to growth or the level of GDP was significant.

Other studies where there are questions concerning the data used, the econometric methodology, or both produced estimates of the effects of infrastructure that were significant but appeared unrealistically large.

Further work on these specifications would be needed before such values could be accepted or extrapolated to other situations.

Studies that investigated the possibility that the elasticity of GDP with respect to infrastructure varied from country to country did not find any evidence of heterogeneity. Fixed-effect dummy variables were used in most cases and were adequate for dealing with differences among countries. This gives more credence to the extrapolation of estimated value of the GDP elasticity with respect to infrastructure from global studies to individual countries.

The extrapolation of the aggregate infrastructure elasticity to the power sector elasticity uses the share of power infrastructure in the PC for quantity. There is a possible issue with this approach. Because of data limitations, the overall index is based only on a subset of indicators—adding more indicators could reduce the weight of the power sector in the PC and hence the derived elasticity. None of the studies surveyed discussed the implications of basing the analysis on a subset of infrastructure indicators.

The studies surveyed did not attempt to directly link the provision of infrastructure to the effects on low-income households. The benefits identified come from the increase in GDP and the way in which higher GDP is translated into benefits for the poor. The studies that tested the relationship between infrastructure provision and the degree of inequality in an economy focused on an aggregate measure of inequality—the Gini coefficient. A reduction in the Gini coefficient could occur reductions of incomes of better-off groups and with no increases in incomes of the poorest groups. Inequality may be reduced through a reduction in poverty, but this needs to be established before it can be claimed that increasing the quantity and quality of infrastructure benefits lower-income households.

Chapter 3: Energy and growth

Summary of findings

The link(s) between the use of energy and GDP has been the subject of an extensive academic literature. For the more recent studies the aim has been to test whether energy causes growth (a change in energy use leads to a change in output), or GDP growth causes energy use to grow, or both links coexist, or that there is no causal relation between the two variables.

More recent studies have taken into account two important methodological issues. First the possibility of two-way causality requires special estimation techniques, and a variety of approaches have been used to do so. Second, both energy use and GDP have grown strongly in most economies, making the basic data non-stationary and requiring particular econometric technique to ensure that purely spurious correlations between the series not be used as evidence of causal links between them.

Despite the widespread recognition of these two issues it has become apparent that there is a complete lack of agreement concerning the nature of the causal link (if any) between energy and GDP. This point was made by Ozturk (2010) in a survey of the literature published in the leading energy journals, and was reinforced by a meta-analysis of 158 articles by Kalimeris, Richardson, and Bithas (2014). The latter found that the four possible patterns of causal links had been identified on a roughly equal number of occasions, and that there was no systematic correlation between the causal pattern identified and the methodological approach adopted. A similar finding for the link(s) between electricity consumption and GDP was reported in the survey by Payne (2010a). A simple correlation between energy (or electricity) consumption and GDP, such as is presented in Castellano et al. (2015), cannot be taken to necessarily support the view that increasing energy use will create economic output—the causation may be entirely in the reverse direction, namely it is increased GDP that leads to increased energy use. Only full specification and estimation of both possible links can establish their relative importance.

An issue that has been widely ignored is that of omitted variables. Studies that claim to be testing for the possible simultaneous presence of a production function relation (higher energy consumption contributes to GDP growth) and a demand function relation (higher GDP leads to greater energy consumption) should include all possible major determinants of both relations. Omission of a significant variable leads to bias and possible misidentification of the causal pattern (see annex 1). In the light of this possibility it is important to include capital and labor variables as well as energy as inputs to the determination of GDP in the production function, and the relative energy price as well as GDP in the determinants of the use of energy in the demand function, but many studies have not done so. To evaluate the prevalence of this possible omitted variables bias, a survey of all the articles included by Ozturk (2010), and subsequent publications, largely those in the leading energy journals, was carried out. The accompanying Excel file contains all the details. Out of these 136 studies, 126 applied some form of testing for the direction of causality, and 116 applied testing and estimation techniques that accounted for non-stationarity of the data. However, only three articles tested for the direction of causality, allowed for non-stationarity, and included possible major explanatory variables for both the production and demand function relationships. Furthermore, two of these three studies included other data measurement problems. The study by Eggoh, Bangake, and Rault (2011) on African countries has problems with the specification and measurement of

the explanatory variables used, while the study by Azlina (2012) on Malaysia contains no information on how any of the variables were specified and measured.

The conclusion of this review of this strand of the literature has to be that there is at present no reliable statistical evidence that energy consumption drives economic growth. Many studies find that this is not the case, possibly because of serious econometric problems, and that those few studies that have avoided such problems are not sufficiently reliable to be taken as a group providing solid evidence to be cited. Instead, further work is required to establish a coherent link between the increase in energy use and the increase of GDP in an economy.

The link from energy to growth

It is generally believed that an adequate supply of reliable energy is considered essential for economic development. The establishment of a link between increased energy use and the growth of an economy is relevant for many energy sector projects. Projects designed to increase capacity, whether of generation, transmission, or distribution of electricity (including increasing access), or that develop oil or gas deposits, are intended to have as one consequence the facilitation of increased energy production and consumption. If increased energy use leads to increased output (growth) of an economy, then to the extent that increased economic output leads to poverty reduction, there is a link between these energy projects and poverty reduction. Hence, the existence of a link from energy consumption to increased output of the economy provides an important justification for undertaking such projects beyond the benefits occurring to the direct consumers of this increased energy supply.

Recognition of this link and its importance in development strategy has led to a vast literature, mainly statistical in nature, attempting to demonstrate the existence and significance of the link. However, data limitations and the methodological problems that are discussed below mean that much of this literature is unreliable as a guide to the link, and care has to be taken in selecting evidence to support the argument that increased energy use leads to an increased output of the economy.

The growth hypothesis centers on the quantity of energy used in an economy, and this has implications for the measurement of energy in statistical testing. To aggregate all forms of energy it is conventional to convert all sources of energy consumed to tons of oil-equivalent (or British thermal units) and then to work either in per capita terms or absolute terms, depending on whether economic output, as measured by GDP, is to be measured in per capita or absolute (real) terms. This approach fails to capture the efficiency of energy use (including the amount of usable energy consumed, as in stoves, or the amount of electricity produced in power plant) or the quality of energy supply.

Some studies have chosen to focus solely on the consumption of electricity (Wolde-Rufael 2006; Chen, Kuo, and Chen 2007; Yoo and Kwak 2010; Yoo and Lee 2010; Welle-Strand et al. 2012; Ouedraogo 2013a, 2013b; Altintas and Kum 2013) thus ignoring the effect (if any) of the consumption of oil and gas outside of the power sector, while certain studies have attempted to find links from the use of individual fuels to the growth of output (Ashgar 2008; Ngepah 2011; Kum, Ocal, and Aslan 2012).

Viewing domestic energy use as a determinant of GDP means that exports of energy are excluded from the measurement of aggregate energy supply to the economy, while imports are included. This treatment of traded energy is important for oil and gas, which is extensively exported by some major hydrocarbon

producers. Data on the consumption of energy is usually taken from either the International Energy Agency (IEA) or the Energy Information Administration of the U.S. Department of Energy, while data on GDP are often taken from World Development Indicators.

As will be explained below, testing for a link between energy use and growth can also involve testing for a negative link from energy prices to energy use and also for a direct negative link of energy prices to growth. Where a direct link from energy prices to growth can be established this supports the hypothesis that energy projects designed to reduce the price of energy through efficiency gains can have a positive effect on growth and hence a beneficial effect on poverty reduction.⁵ A wide variety of projects may have as their goal or partial goal the reduction of energy prices through efficiency improvements. These might include projects to improve the generation mix and reduce the use of higher-cost fuels, and projects to improve sector efficiency (especially technical and commercial losses in the power sector). However, the majority of studies do not have available disaggregated data on energy prices, or even an average energy price, and instead use the consumer price index or the international price of crude oil as proxies for domestic energy prices.

The nature of the link

This so-called “growth” hypothesis that an increase in aggregate energy use leads to economic growth⁶ is based on the idea of extending the classical production function, in which aggregate capital and labor combine to produce output, and in which technical progress shifts this relationship over time. Energy is seen as a separate factor of production with distinctly different effects on economic output from those of capital and labor. Firms wishing to increase output will use more labor and capital and also more energy. As with traditional aggregate production function estimation, it is assumed that despite variations between sectors in the relation between inputs and outputs, there exists a stable aggregate relation between aggregate energy use and aggregate output. However, it can be seen that energy sold for final consumption (liquefied petroleum gas for household cooking, gasoline for household transportation) does not contribute to increasing output, so that aggregate energy use overstates the amount of energy linked to growth through a production function.

Energy can be linked to increased output in a separate way. Where there are unforeseen energy shortages (outages in the power sector) firms may find that they are unable to produce their planned output. Projects that increase energy supply (through the reduction of technical losses or incremental generation capacity) could then allow output to increase, without requiring the other factors of production to increase at the same time. Studies of the aggregate link between energy use and economic output have not attempted to incorporate the effect of changing constraints on energy supply.

This framework for linking the consumption and use of energy to the output of the economy covers a wide range of energy projects. As well as those designed to directly produce more energy, there are projects that facilitate increased energy production. For example, power sector reform that leads to more efficient operation of the sector (for example, by reducing commercial or technical losses) is often

⁵ The introduction of energy subsidies would lower prices but the long run effects of financing the subsidies would themselves have negative effects on economic output.

⁶ Although this is referred to as the growth hypothesis, it is important to recognize that it does not relate the level of energy use to the growth rate of an economy—were it to do so economies with ever increasing levels of energy use would experience ever higher growth rates which is manifestly not the case.

required in order to address shortages of energy supply. However, there are certain projects that fit less easily into the category of increasing net energy use. Projects that improve energy efficiency, such as those to improve insulation, are designed to reduce the consumption of energy but will lead to a growth in output by freeing up resources for other projects. Some studies (Ozturk 2010; Eggoh, Bangake and Rault 2011; Kahsai et al. 2012) generalize this line of reasoning by arguing that the growth hypothesis implies that energy conservation measures will lead to a reduction in the output of the economy. This appears to assume that conservation and an increase in energy efficiency move the economy back along the production function, while in fact it represents a shift of the production function in which the same output can be obtained from fewer inputs. However, there appears to be no econometric study that tests separately for these two effects.

Methodological issues in linking energy to output

The literature on what is termed the energy-GDP nexus is extensive, and much of the more recent literature also reviews earlier studies. There is almost universal agreement that such studies have not been able to come to a definite conclusion on the question of whether an increase in energy use contributes to economic growth. Ozturk (2010) conducted a literature survey of the articles published up to that date and provided insights as to why there is so little agreement among these studies. His survey covered both single-country studies (38 for energy and 26 for electricity) and multi-country studies (26 for energy and 8 for electricity). They showed no consistent pattern. For seven countries (including India, Malaysia, and Turkey) there are multiple studies using different data periods and different equation specifications, and even these found no consistent patterns for individual countries. More recent articles (for example, Akkemik and Göksal 2012) that include surveys of other studies reach similar conclusions. Kalimeris, Richardson, and Bithas (2014) carried out a meta-analysis of 158 published articles in an attempt to see whether the different conclusions concerning the link from energy use to output depends on the methodology of the studies in question.

There are a number of methodological issues in the estimation of the effect of increased energy consumption on output. The failure to appreciate one or more of these issues has led to contradictory estimates of the sign and size of this link, and may explain why there has been so little agreement among studies. Studies that ignore one or more of these issues may well be unreliable, although the extent of any such bias will be unknown unless re-specification and re-estimation of the model using the same data framework is undertaken.

- **Simultaneity.** It has long been recognized that there may also be a reverse link from output to energy, reflecting an aggregate demand equation. Generally it is assumed that an increase in income (output) in an economy will lead to an increase in the demand for energy, although shifts in relative demand between sectors (for example, from manufactures to services) may cause the energy intensity of an economy to fall. This is equivalent to recognizing that the income elasticity of demand for energy may be less than unity—but it would be rare for the elasticity to become negative at the level of an aggregate economy. Hence a positive correlation between energy and GDP could be due to either a causal link from energy to growth, or from growth to energy, or both. This possibility means that it is necessary to use a methodology that can separate the two directions of causation and evaluate their relative importance. Studies that claim to look at just the “growth” model, without taking the other possibility into account, cannot be relied upon.

Virtually all recent studies discuss the issue of simultaneity and attempt to allow for its existence. Studies such as those by Welle-Strand et al. (2012) that concentrate on a single direction of “causation” cannot be assumed to have evaluated that link correctly, because the reverse link, if it exists, would result in biased estimates.

- Non-stationarity. Where time series data are used for testing for the relationship(s) between energy and output it is evident that both series are subject to strongly increasing trends. If both are determined by a third factor, also trend-dominated, they could appear to be significantly correlated even if there is no actual link between them. Special econometric techniques are required to ensure that what is being measured is not a spurious correlation. The series have to be checked to see whether they are stationary (non-trend dominated) and, if not, whether they are cointegrated—a linear combination of the series in question is stationary, as would be expected to be the case if they are causally linked. The treatment of non-stationarity can be achieved by a variety of tests and approaches but is now a feature of virtually all studies concerning the energy-growth link(s). Studies such as those by Ngepah (2011) and Welle-Strand (2012) that do not check for non-stationarity of the main data series or work through a known cointegrated relation are at risk of producing spurious correlations between the variables investigated.
- Short time series and panel estimation. The statistical tests for the presence of non-stationary series and their cointegration can have low power when there are few observations. The annual data available, particularly for earlier studies involving lower income countries, may be limited to 20 to 30 observations—the study by Wesseh and Zoumara (2012) for Liberia had 29 observations while the study by Stern and Enflo (2013) for Sweden had 150 observations. As a means of increasing the number of degrees of freedom it has become common to combine data from different countries in panel estimation. Studies using data for up to 100 countries with perhaps 30 observations per country have been carried out (Yoo and Lee 2010; Narayan and Popp 2012; Akkemik and Göksal 2012; Apergis and Tang 2013). As well as providing the large samples required for reliable estimate this has the advantage of increasing the variation of the data, because differences among countries tend to be much larger than the changes within a country over time. However, as discussed below, many panel studies have assumed that the crucial coefficient (that is, the effect of energy on growth) is the same for all countries and constant over time.
- Omitted variables. A pervasive shortcoming of the studies testing for the presence and nature of the energy-growth link is that of the omission of other key explanatory variables that may be affecting the relationship. For example, if energy is linked to output by a production function, it is to be expected that at a minimum capital and labor will also affect output. Ignoring these factors in the statistical estimation may bias the value of the coefficient on energy to an extent dependent on the correlation between energy and the omitted variables. Similarly, in the link from output to energy (a demand-type function) the price of energy could also be expected to be a determinant of energy consumption. Failure to include the energy price may lead to bias. Even though many studies recognize the possibility of bi-directional links between energy and GDP, and indeed test for its presence, most include either only production function variables (capital, labor) or only demand variables (energy prices), but not all of them. Claims that there is no link from GDP to

energy when price has not been included in the estimation, or that there is no link from energy to GDP when other factor inputs have not been included, have to be regarded as unproven. As an extreme example, the study by Narayan and Popp (2012) did not include capital, labor, or energy prices in the testing of the bi-directional relationship and found *negative* demand relations between GDP and energy for all the different regional groups of countries included, a highly improbable and questionable result.

A few studies have suggested other explanatory variables that should be included in the tests for the links between energy and GDP, but these are less well-founded in theory and their omission from other studies cannot be regarded as leading them to be inevitably flawed. For example, Liddle (2013) and Apergis and Tang (2013) included urbanization as a shift factor in the production function through spillover effects and economies of scale, while Altintas and Kum (2013) include exports as a factor determining GDP.⁷

- **Measurement errors.** The measurement of energy use and output is straightforward and few variations have occurred in the literature. However, since other variables need to be included in the statistical testing the measurement of these variables also becomes important—measurement errors themselves can lead to further bias. Two measurement problems are commonly encountered.
 - *The capital stock* is rarely available on a time series basis for developing countries, although it may be for high-income countries (Stern and Enflo 2013). Most studies have therefore used Gross Fixed Capital Formation (investment) as a proxy for capital (Eggoh, Bangake and Rault 2011; Akkemik and Göksal 2012) rather than use a perpetual inventory method to estimate the capital stock itself. The use of the investment series, however, may be problematical. If capital is cointegrated with output, it is unlikely that its difference over time (investment) will be. This may lead to rejection of an important variable in the estimation of the production function and hence to biased estimation of the energy link.
 - *The price of energy* is not available for most countries other than for members of the Organisation for Economic Co-operation and Development (OECD) where the IEA publishes prices for different forms of energy (Belke, Dobnik, and Dreger 2011). Some studies have used the consumer price index (CPI) as a proxy (Eggoh, Bangake, and Rault 2011; Altintas and Kum 2013) while others have used the international price of crude oil expressed in dollars (Ouedraogo 2013b). Both of these approaches are problematical. As pointed out by Stern and Enflo (2013) the price of energy should be measured relative to a general price index within the country in order to allow for substitution in demand between energy and other commodities. Proxying this price ratio by the CPI (or GDP price deflator) introduces a measurement error into the variable that can result in biased estimation. For example, if the price of energy were perfectly correlated with the CPI, as is implicitly desirable for those seeing the CPI as a proxy for the energy price, the ratio

⁷ In their classic study of the determinants of economic growth, Levine and Renelt (1992) found that the only robust links were between growth of GDP and the ratio of investment to GDP, and between the investment share and the ratio of international trade to GDP. The former is equivalent to the link between the change in output and the level of investment (change in the capital stock). This type of relation is tested in much of the energy-growth literature.

would be constant and hence insignificant, while there might be a spurious correlation between movements in the CPI and the use of energy.

In studies that have measured output in constant U.S. dollars (as is common practice with panel data) energy prices should also be measured in dollars. However, the CPI is not corrected for exchange rate movements. The international price of crude oil, as is used in a number of studies, is measured in dollars but does not reflect a relative price movement. Also, the crude oil price does not represent the general movement of all domestic energy prices, even allowing for exchange rate movements.

- **Heterogeneity.** Traditional panel estimation used in multi-country studies includes so-called fixed effects, which correspond to allowing the intercept of the relation being different for each country by creating a 0/1 dummy variable which is 1 for the country in question and 0 for all others. This incorporates any variable that explains differences among countries but that is constant over time for each country. However, it assumes that the slopes (coefficients on the explanatory variables) are the same across all countries. The assumption that the coefficient of the effect of a unit change in energy consumption on the output of the economy (or the effect of a unit change in output has the same effect on energy demand) is the same for every country may be too restrictive, and imposing such a restriction can lead to unreliable estimates of the effects under investigation. Some studies have disaggregated their panels into groups of similar countries either by region (Narayan and Popp 2012) or by income level (Kahsai et al.2012; Liddle 2013) while others have used newer, more flexible econometric techniques that allow for heterogeneity (Akkemik and Göksal 2012).

Evidence linking energy and growth

The survey by Ozturk (2010) and the meta-analysis by Kalimeris, Richardson, and Bithas (2014) were supplemented by further analysis carried out for the purposes of this review. For this the studies covered by Ozturk were augmented by other studies identified as having been published since 2010 in the main journals concerned with energy use and consumption issues, producing a list of 136 studies investigating the energy-growth link.⁸

The meta-analysis study of Kalimeris, Richardson, and Bithas (2014) identified 158 studies and, where these studies carried out separate analysis of a groups of countries or of individual countries, 686 cases were derived where tests for the direction of causality between energy and GDP had been carried out. Each study was described by six attributes related to methodological features of the analysis, in order to test whether the results obtained on the direction of causality were correlated with the approach adopted. The attributes chosen for analysis were

- length of study period in years (4 categories);
- economic development level of country(ies) (5 categories);
- one or more countries (2 categories);
- econometric methodology for testing causality (7 categories);

⁸ This exercise was carried out before the publication of the meta-analysis in 2014. It was judged to be unnecessary to redo the analysis using the list of studies from the meta-analysis, because the two lists overlapped to a large extent, and the findings in each were so unambiguous.

- energy input source (7 categories); and
- energy measurement method (7 categories).

A tabulation of causality result against the attributes showed that 193 cases indicated causality flowing from energy to GDP; 163 indicated causality flowing from GDP to energy; 174 indicated bi-directional causality; and 155 indicated no causality in either direction. These results confirmed Ozturk's observation that there was no clear evidence for any particular causal relation between energy and growth when a wide range of studies are considered together.

In order to test whether particular combinations of categories of the attributes lead to particular causal patterns, a multinomial logit analysis of these four outcomes related to the various attributes, expressed as dummy variables, showed that the econometric methodology and the energy measurement attributes were significant but that there was no evidence that individual categories of these attributes were significantly associated with any given causal pattern.

The majority of these studies included in the meta-analysis tested for stationarity and adjusted the estimation technique accordingly, and most also explicitly recognized and tested for the possibility that the causal link could exist in two directions. However, a striking feature of most of these studies is that, having hypothesized that causality could flow from energy to GDP (production function), from GDP to energy (demand function), or both, variables relating to only one of these relations were including in the testing. Some studies testing for two-way causality focused only on the demand function and included energy prices as an extra variable (shift factor) but not labor or capital, while others focused only on the production function. If two-way causality existed, it would have been necessary to include variables that determine both links. Omitting variables that are actually significant leads to estimation bias, making conclusions based on a partial specification of the two-way causal model unreliable for assessing the significance of the individual causal links. With this in mind the survey carried out for the present study categorized articles under three attributes:

- Testing for causal links
- Testing and adjusting for non-stationarity
- Including both production side variables (capital and labor) and demand side variables (energy price) in initial testing.

Of the 136 articles analyzed, 126 applied some form of causality testing methodology, 116 applied test and estimation techniques allowing for non-stationarity, and three studies tested for causality, allowed for non-stationarity, and included capital, labor and energy price variables (see the accompanying Excel file for a list of these studies and the attributes identified). All other causality tests ignored variables from either the production function or demand function specifications, even though they recognized the possibility that both might exist. The omission of these variables that are likely to have a large inter-temporal and inter-country variation could well lead to substantial bias in testing and estimation, and lead to conclusions that are not well-founded. The doubt about the reliability of the results is reinforced by the wide divergence of conclusions reached about the direction of causality. There may be other problems, particularly those of heterogeneity, that were not systematically addressed in many of these studies, but the omitted variable problem presents a minimum that should be borne in mind in future testing and in using the results generated by these studies.

Another recent meta-analysis of the link between energy consumption and GDP was carried out by Menegaki (2014), who focused on the production function relation between energy and GDP as investigated in 51 studies. The elasticity of GDP with respect to energy from each study was regressed on a number of variables, all of which were in a zero/one form except for the elasticity of GDP with respect to capital. The 22 explanatory variables included were divided into several groups:

- General study characteristics (year of publication, panel or time series, tests for structural breaks)
- Method of analysis with respect to treatment of cointegration (5 alternatives)
- Country grouping of study (region, length of time period, number of countries in the study)
- Variables in long-run relationship (inclusion of electricity in total energy, prices of goods, carbon dioxide emissions, labor variable, elasticity of capital)
- Causality (bidirectional causality identified by the study).

The principal finding of this study was that the elasticity of GDP with respect to energy (including electricity) was positively and significantly related to the elasticity of GDP with respect to capital—a 1 percent increase in the capital elasticity was associated with a 0.85 percent increase in the energy elasticity. Four of the dummy variables were significant (including two of those for the method of analysis). The study did not give a detailed explanation of why the variables chosen might be expected to affect the size of the energy elasticity of GDP—in particular there was no mention of whether capital and energy are expected to be complementary inputs (as the meta-regression found) or substitutes. Other possibly important variables, such as the quantity of labor input or the relative price of energy, were not included and hence the possibility of bias due to the omission of factors in bidirectional causality limits the reliability of the results found.

A recent study by Bruns, Gross, and Stern (2014) introduced techniques of meta-analysis to examine the presence of two-way causality between energy and output. The authors took data from 72 studies covering 574 growth-to-energy statistics and 568 energy-to-growth statistics. The study tested for publication bias (the tendency for only those studies with significant results to be accepted for publication) and for mis-specification bias (the tendency for authors to over-fit models by adding too many lagged values). The resulting test procedure was unable to uncover significant two-way causality. The study also allowed for the inclusion or exclusion of key explanatory (control) variables in the different studies. The study found significant evidence for the hypothesis that growth increases energy use, and that this effect was stronger when an energy price variable had been included in the original study. Adding capital to the energy-to-growth equation in fact tended to reduce the significance of this link.

The three studies that satisfy the criteria of testing for causality, allowing for non-stationarity and including both production function and energy demand variables are described below.

Stern and Enflo (2013)

This study stands alone in using a very lengthy time series of data for a single country, Sweden. As the authors note, the use of a data period stretching from 1850 to 2000 increases the chance that there are shifts in crucial coefficients over time. The central purpose of the study was to test the causality was from energy to growth or the reverse. The conclusion was that tests were sensitive to variable definition, choice of additional explanatory variables in the model, sample periods, and the introduction of structural breaks.

The overall conclusion was that the causality was from energy to GDP over the full sample period, while it was from GDP to energy in recent years. Energy prices also had a negative link to both energy use and GDP.

The study used two different measures of output (gross domestic output and GDP), capital, labor, two measures of energy use (one allowing for changes in the productivity of energy over time), and two different energy prices, both measured relative to the GDP deflator (one being the general energy price and the other the price of crude oil expressed in local currency).

The first step of the analysis tested for non-stationarity with and without an allowance for structural breaks. The dates of the structural breaks were first specified exogenously and then were determined by the data. A variety of tests supported the hypothesis that all the data series were non-stationary (had unit roots) while the timing of structural breaks did not show any consistent pattern.

The second step was the estimation of separate equations for production (output, energy, capital, and labor) and for demand (energy, output, and energy price). The former indicated that there was causality from energy to GDP in the full period and from 1900 to 2000, but that the causality was from GDP to energy from 1950 to 2000. The latter indicated that energy price was linked to energy demand for all periods, while GDP growth drove increasing energy use only for the 1950–2000 sub-period. Virtually all other studies have chosen one or other of these two equations for causality testing rather than investigating both.

The third step was to estimate the long-run relation between the variables based on cointegration analysis. Using a variety of models with different variables, different sub-periods, and with structural breaks in 1916 and 1973, there was strong evidence for cointegration. In the estimation of the long-run relationships normalized on energy, capital was not significant in any model, while energy prices were significant in all models, and labor and output were significant in some models. Estimates of the long-run coefficients were not presented for the relationships normalized on output.

The results of Stern and Enflo emphasized the sensitivity of tests to data periods and data measurement. However, they provide strong support for the argument that including all the major variables in the analysis is essential—there are strong a priori reasons for their inclusion and omitting some or all of these variables can lead to very different results for estimation and testing. The relevance of these results to other countries lies in the lessons generated on how the analysis should be approached. In particular the use of a very long time series meets concerns about small samples and their effect on the power of statistical tests, but at the same time is more open to possibilities of shifts in the relationships.

Eggoh, Bangake, and Rault (2011)

These authors carried out panel estimation for 21 African countries using data from 1970 to 2006. They first reviewed other studies that had investigated the relation between energy and growth in Africa, providing useful insights as to why a wide variation in results had been obtained. The variables included in their study are real GDP in U.S. dollars, energy use in kilograms of oil equivalent per capita, CPI, total labor force, and real gross fixed capital formation, all measured in logarithms. The effect of using energy per capita, while other variables were not so deflated, introduces a specification error and may lead to bias of estimated coefficients, while the shortcomings of using the CPI and the gross fixed capital formation

have been noted previously. The estimation involved some tests and techniques not yet common and can be regarded as providing a more flexible approach to identifying the nature of the links between the series. The first step of the examination of the relationship between the series was the testing for the order of integration of the series. All five series were found to be non-stationary but their first differences were stationary.

The second step used panel cointegration tests to examine whether a long-run relationship existed among the variables. An important innovation in this respect was the allowance for the presence of a limited number of structural breaks in the relationship—ignoring the presence of any breaks that do exist can lead to mis-specification of the long-run relationship. The null hypothesis of no cointegration was rejected for all countries, and oil importers and oil exporters were treated separately.⁹ Two structural breaks were identified for most countries, one occurring in the mid- to late 1970s, and the other in the mid- to late 1990s.

The third step of the analysis tested for the existence of a long-run relationship between the five variables of interest using dynamic OLS in order to correct for endogeneity. All long-run coefficients were significant and positive for both energy importers and exporters.¹⁰ When all countries were grouped together, energy and labor had similar-size links to GDP, while capital was substantially more important. For energy exporters the link to capital was much weaker, while those to energy and labor were substantially higher. The reverse was true for energy importers, where capital was much more strongly linked to GDP. In all cases the link to prices was small. A test of equality between exporters and importers rejected the hypothesis that the magnitude of the long-run elasticities was equal for these two groups, but within each group the hypothesis of equality was not rejected. The significance of the long-run coefficients does not establish the direction(s) of causality but rather the existence of a stable relationship between the variables of interest.

The final step in the analysis was the estimation of short and long-run causality tests (Table 3). The short-term effects are the sum of the lagged coefficients and correspond to the elasticity of a change in the explanatory variable on the outcome variable, while the error correction term measures the speed at which the variables return to their long-run equilibrium relation. For the aggregate of all countries every variable had a significant short-run effect on growth, and this was confirmed for exporters and for importers. Energy, labor, and capital all had positive short-run effects on growth, while prices had a small negative effect. The energy elasticity of growth was 0.34, indicating that a one-percent increase in the total use of energy would result in a 0.34-percent increase in output.

⁹ Energy exporters include Algeria, Cameroon, Republic of Congo, Côte d'Ivoire, Arab Republic of Egypt, Gabon, Libya, Nigeria, South Africa, and Sudan. Energy importers included Benin, Ethiopia, Ghana, Kenya, Morocco, Senegal, Tanzania, Togo, Tunisia, Zambia, and Zimbabwe.

¹⁰ Long run does not refer to a temporal effect but rather that the two way link between the series has been included in the calculation.

Table 3: Sources of causality for GDP and energy in African countries

All countries	Short run					Adjustment
	ΔY	ΔE	ΔP	ΔL	ΔK	ECT
ΔY	–	0.34*	- 0.02*	0.28*	0.41*	-0.12*
ΔE	0.18*	–	-0.09*	0.02	0.48*	-0.28*
Energy exporters						
ΔY	–	0.30*	-0.03*	0.17*	0.61*	-0.19*
ΔE	0.14*	–	-0.05*	-0.96*	0.05	-0.17*
Energy importers						
ΔY	–	0.39*	-0.04*	0.44*	0.51*	-0.07*
ΔE	0.19*	–	-0.11*	0.07	0.03	-0.39*

Source: Eggoh, Bangake, and Rault (2011).

ECT = error correction term. Δ = difference in output (Y), energy (E), energy price (P), labor (L), and capital (K).

*=significant at 5%.

In the energy equation GDP had a significant positive effect while prices had a significant negative effect. Labor was not significant for energy exporters, and capital was not significant for either importers or exporters. The error correction variables were all significant and negative (as expected), indicating that if GDP (or energy) were above the equilibrium level implied by the explanatory variables, it will tend to fall back toward the equilibrium.

These results, based on a full specification of the possible links between energy and growth and using flexible and sophisticated estimation techniques, suggests a number of conclusions:

- For the sample of countries chosen, a long-run relationship between energy and growth was shown to be significant.
- The long-run relation was found to be significantly different for energy exporters and energy importers, although within each group there was equality.
- Estimation of the causal structure showed bidirectional causality as indicated by the short-run coefficients in Table 3. Capital and labor were significant in the growth equation, and price was significant in the energy equation. This confirms that studies omitting some or all of these variables are likely to yield misleading results.
- The key coefficient for the purpose of evaluating the link from energy to growth is the short-run elasticity of energy in the growth equation. A value of 0.34 taken over all countries, with 0.30 for exporters and 0.39 for importers, indicates the importance of energy as a factor in the determination of the output of these economies. It has to be remembered that energy is measured per capita, while GDP is in absolute terms. Over short periods the growth in population may be small compared to the growth in the use of energy so that the elasticity may be a reasonable approximation to that of energy itself.
- Price was found to have a small but significant negative effect in the growth equation with elasticities of -0.03 and -0.04 for exporters and importers, respectively. These results support the hypothesis that projects to lower energy prices through efficiency improvements can have positive effects on growth. The price elasticity was much smaller than the energy elasticity, suggesting that energy quantity improvements are likely to have a larger effect than energy price improvements on the growth of the economy

- A number of questions still remain and further work may shed light on these issues. One is the measurement of the variables. The use of energy per capita produces a variable growing less rapidly than energy itself and this may affect the results; prices are represented by the CPI, which is likely to have moved differently from the real price of energy measured in domestic currency. Capital is measured by gross fixed capital formation and the effect of this approximation has not been evaluated in the literature. Finally, the evidence that the elasticities vary by country groupings determined by whether or not the country is an energy exporter suggests that other factors may also affect the magnitude of the energy elasticity.

Azlina (2012)

This study investigated the causal links between energy and GDP for Malaysia using data from 1960 to 2009. The author recognized that the possible existence of a demand relation and a production function relation required the inclusion of explanatory variables from both. The demand function included the energy price and the share of industry in GDP (as a measure of the structure of the economy), while the production function initially included capital and labor. The article provided no details on how these variables were measured, creating some uncertainty about the reliability of the results.

The first stage of the analysis tested for non-stationarity. All variables except labor were integrated of order one. Dropping labor and testing for cointegration between all the other variables showed that there were two cointegrating relationships between these variables. The associated vector error correction model imposed a number of constraints to reflect the assumed structure of the model. In the demand equation the coefficient of capital was restricted to zero, while in the production function equation the coefficients of energy prices and industrial structure were set equal to zero. Using the Granger testing methodology it was concluded that neither equation showed short-run causality between energy and GDP, but in the long run there was a causal link from GDP to energy but not from energy to GDP.

Computable general equilibrium and input-output models

A different approach to understanding the effects of investment in the power sector is involved in the use of computable general equilibrium (CGE) and input-output (IO) models. These techniques do not base their results on a “before and after” or a “with and without” analysis of actual past data to evaluate the link between the investment and various outcomes such as GDP or employment. Instead, they take a known project (possibly at the planning or construction stages) and use relationships based on previous data to make a prediction of the change in GDP or employment resulting. Since the predicted figures are not matched against actual outcomes, there is no direct check on the reliability of the prediction. Instead emphasis is placed on the realism and flexibility of the model used. In short, these approaches do not demonstrate a link between energy and growth; instead they assume one and can adjust the assumptions used in the modelling to demonstrate a desired result.

Computable general equilibrium approach

CGE models are capable of incorporating a wide range of possible responses to an initial policy change (the project under consideration) but are not evaluated with respect to the actual outcomes observed or from a series of similar episodes in the past. CGE models typically incorporate an IO structure to represent industry-level effects but embed it within a social accounting matrix formulation that

incorporates flows between all sectors in the economy. A study by the Asian Development Bank (ADB 2003) used a CGE model to simulate the effects of three hypothetical policy changes that were similar to ADB assistance to Indonesia. These included a ten-percent supply shock to power sector capacity (to represent various power sector loans), a two-percent improvement of the operating efficiency of the state power utility (representing the effects of efficiency-directed loans), and a ten-percent tariff increase representing the impact of policy advice. The CGE model was used to simulate the effects of these changes on GDP, exports, imports, employment, and the consumer price index.

The ten-percent increase in power supply capacity and two-percent increase in operating efficiency were simulated to increase GDP by 0.13 percent and 0.03 percent, respectively, while the increase in employment effects were about half as large as those of GDP. The authors commented that small effects calculated by the CGE model were due to a number of reasons: (i) the underlying model was primarily designed to describe how different industries and consumers react to the policy change; (ii) the shares of electricity in the IO data used in the model were very small (only about one percent of costs); (iii) the model incorporated only operating costs at a sector level but not the impact of investment expenditures; and (iv) the impacts did not represent the cumulative effect of the ADB's policy assistance, but only the annual effects. Furthermore, the model represented a static situation and did not incorporate the transient economic costs that arise from power supply interruptions stemming from poor reliability or non-availability.

The use of a CGE model provides valuable insights on the relative importance of different feedbacks within the economy as calibrated in the coefficients used in the model, but it does not provide a way to evaluate the reliability of the approach.

Input-output approach

A number of studies, mainly in developed countries, have used IO tables to quantify the link between investment in an energy project and the additional total domestic output and employment created (World Bank 2011). IO analysis divides the economy into a number of sectors and traces links between sectors in order to understand the total effects on all sectors of a change in demands for the output of a given sector. The key assumption for IO analysis is that there are fixed ratios between extra spending on a project (e.g. investment in a transmission line) and the purchases of inputs required to produce this extra output. It is recognized that these inputs (e.g. steel, energy) themselves require inputs, also in fixed proportions. Labor is also an input and there is assumed to be a fixed ratio between spending on inputs and the expenditure on labor to supply these inputs. In an "open" IO system the extra wage income from all stages of production accrues to households but does not lead to further spending on goods or services, while in a "closed" system households with extra income spend part of it on goods and services.

The effect of extra spending on a project is conventionally measured by value added, or gross output, or employment created, and the goal of IO analysis is to compute the total effect on these variables, allowing for all links between outputs and inputs, where the links are direct, indirect, and induced.

Direct effects are those due to the expenditure on the project itself and can stretch over several years. The project costs of a transmission line would include all spending on domestically produced components and building during the construction phase of the project, as well as organization and maintenance expenditures spread over its operating life. The direct effects are usually well understood and accurately

estimated since they relate solely to the project itself and would need to be estimated for the purposes of project appraisal. The main problem is to allocate such costs to the various sectors identified in the IO table available.

Indirect effects are those brought about by the need to purchase inputs to make the extra outputs required by the initial direct expenditures. Suppose that steel pylons are domestically produced and are purchased as part of the initial project expenditure of the transmission project. To produce more pylons more steel and energy will be required, and these in turn will require other inputs, spreading the effects throughout the economy but in smaller and smaller increments. The sum of all such effects aggregated over all sectors is the indirect effect. The ratio of the total of direct plus indirect effects to the direct effect is known as a type I multiplier. To calculate this multiplier it is assumed that there is a constant ratio between a unit expenditure in a given sector and the expenditure on each input required to produce the output in that sector (the IO coefficients). These coefficients are the key to the analysis. Several factors can result in their being unreliable and producing misleading estimates of the indirect effects.

- **Coefficients are out-of-date.** IO tables are compiled from industrial survey data and in most developing countries data are collected at infrequent intervals. Where the structure of the economy and technology used are changing, the coefficients would be expected to change. Basing calculations on inputs required to produce unit output using data that are several years or even a decade old could result in a substantial error.
- **Sector definitions are inappropriate.** IO tables, especially in lower-income countries, are usually based on relatively few, highly aggregated, sectors, perhaps no more than twenty, whereas those in advanced economies may use more than one hundred sectors. This presents a difficulty for the analysis of a particular project because the sector to which a project has to be assigned may have IO ratios very different from those of the project itself. For example, much of the expenditure of a transmission project would have to be assigned to (say) the metal goods sector (pylons) or to the building sector (construction). The ratios for these sectors are averages over a wide range of activities that may not reflect the particular features of the transmission project.
- **Input use may change as substitution occurs because of demand pressures brought about by the project.** A large project may make heavy demands on the supply capability of certain input sectors. As a result relative prices may change and the use of that input may alter. IO tables make no allowance for such changes, nor for changes in coefficients that have been brought about by external shocks experienced since the time at which the coefficients were estimated. In effect, IO analysis assumes that each input has an infinite elasticity of supply so that any level of incremental demand can be met at the going price, thus keeping the IO coefficient constant. If marginal costs of supply increase for any reason, such as the existence of physical bottlenecks or labor shortages, then the model is no longer valid.

These factors can combine to produce substantial inaccuracies in the calculation of the indirect effects, but there is no way to test their reliability, given that there is no more-up-to-date and disaggregated IO table available against which to make a comparison.

Induced effects result from “closing” the IO table by assuming that household incomes received as extra wages from the various direct and indirect links are themselves spent, thus generating further demands for

goods and services. Flows to the government as extra household taxes paid, and to foreign firms through the purchase of imported items, need to be taken into account. It is generally assumed that the extra spending on the different sectors is in the same proportions as observed in the year for which the IO table was compiled. The injection of this household spending back into the economy creates further rounds of direct and indirect effects in a classic multiplier process. The ratio of the sum of induced, indirect, and direct to direct effects is known as a type II multiplier. Type II is often substantially larger than the type I multiplier.

A guide to the order of magnitude of a project-based type II multiplier is provided by a conventional Keynesian-type investment multiplier. A macro-economy is in essence a single aggregate sector and increased investment spending is expected to result in increased GDP (value added) and employment. Many experts have argued that the Keynesian multiplier should be near zero for a number of reasons. One important consideration is that increased investment spending does not take place in a vacuum. If firms spend more on one project they are likely to spend less on other projects because they do not have access to unlimited and costless finance. Investment is a choice between activities and, unless a foreign firm is making the investment, there is likely to be some offsetting adjustment which will bring in its turn a reduction in output and employment. Similarly, government financing of projects will lead to some adjustment of spending or taxation plans that will result in a reduction of demand. IO models do not make allowance for such countervailing activities and hence are likely to overstate the impact of an initial investment.

Four further problems arise with the calculation of type II multipliers, leading to further possible lack of reliability.

- **IO models do not allow for input substitutability.** The larger effects seen with the use of type II multipliers make it increasingly likely that some substitution due to relative price shifts may take place. If so, the IO coefficients will be misleading. Further, where the IO table is based on data from several years earlier, relative prices and wages may have changed substantially, making it difficult to compare current expenditures with historical expenditures as incorporated in the table.
- **The calculation of employment effects depends on the wage data available.** The IO tables used are often based on expenditure data, so that employment effects are initially seen as expenditures on employment. To convert these into numbers of people employed requires wage rate data and these should correspond at least to the sectors under consideration. For some economies it will not be possible to obtain current wage rate data disaggregated on the same basis as the IO table, and this can result in misleading values for the employment created.
- **Household consumption behavior may differ as between the short run and long run.** Households receiving extra income as a result of new investment may treat such income differently from established sources of income. They may either be cautious and save a higher fraction than normal, or decide that the income is a windfall and spend a higher than normal fraction. Detailed analysis of household behavior is required before it is safe to assume that spending patterns (as regards both the fraction saved and the fractions spent on different goods) are the same as when the IO table was compiled.
- **Job creation is assumed to arise purely in response to investment.** If restrictive labor regulations are deterring job creation and there is a reform, if a new dynamic education minister

overhauls the education system, or if there is a fundamental reform of the police force, dramatically cutting crime and drawing more investment in response, there may not be a large increase in spending but there could be a large response in job creation as a result. IO analysis does not catch the potential impact of any of these reform measures on employment creation. Attempts to attribute all job creation to the initial investment change will be unreliable when there are other policy shifts.

The larger the project relative to the size of the economy the more likely it is that IO coefficients will be unreliable. Any inelasticity in supply will be more important for larger projects—bottlenecks that are minor for a small project may be of major concern for larger projects. This observation warns against attempts to analyze the total effects of all investments made in a sector during a given period through the use of IO tables.

A study by the International Finance Corporation (IFC 2012) identified five categories of employment created by investment in transmission projects in India and Bhutan and used a variety of methods, including IO analysis, for predicting the associated employment effects:

1. *Direct effects.* The jobs created to construct the transmission line and to operate and maintain it over its economic life (44,000 person-years).
2. *Indirect effects.* The jobs created by sectors supplying the extra inputs required for the construction and operating phases of the project (55,000 person-years).
3. *Induced effects.* Jobs created to meet the extra consumption of goods and services arising from the extra income received by workers benefiting from the direct and indirect effects (144,000 person-years).
4. *Supply effects on economic growth.* Jobs created as increased energy supply made possible by the project leads to increased economic growth (up to 450,000 person-years).
5. *Supply effects from reducing bottlenecks caused by power outages.* Jobs created as firms expand output to meet existing demand in reaction to the removal of a production constraint (up to 9,600 person-years).

The direct jobs were estimated from project details that furnished estimated employment for construction, operation, and maintenance. This calculation was likely to be accurate since project costs (and hence employment) would be carefully estimated before undertaking the investment.

Indirect effects relied on the coefficients of an IO table that provided fixed ratios between the physical inputs to the project (steel, energy) and the supply sectors (iron, petroleum refining). Care had to be taken to identify which inputs would have to be imported and hence would not create domestic jobs.

Induced effects were calculated by making assumptions about the propensity of households to spend extra wages (resulting from the extra direct and indirect employment generated by the project). Care had to be taken to allow for leakages to imports, to direct taxes, and to savings. However, there was no discussion of the nature of household consumption and saving decisions. Saving from incomes seen as transitory, as may be the case for construction workers, may be quite different from saving from a steady stream of income (from operations and maintenance employment). In addition, the IO model did not take into account the likelihood that government or private sector companies that are part financing such a project will reduce their spending elsewhere—investment projects are choices between alternatives and are not

simply extra expenditure without consequence for budgets. These considerations are particularly important since estimates of induced effects are often larger than the direct and indirect effects.

The link from extra energy supply to employment comes from arguing that there is a link from energy consumption to economic growth, and that transmission supply makes higher electricity consumption possible. Statistical analysis was used to evaluate this link and to check for the nature of the causal link between electricity and employment in India (whether incremental electricity consumption increases employment, or incremental employment increases electricity consumption, or both pathways exist). As discussed in this literature review there are a number of problems involved in testing for the direction of causality, including allowing for the principal determinants of both the possible production link and the demand link. The study did not include production-side variables (capital and labor) or demand-side variables (relative electricity price), thus leaving open the possibility of mis-specification bias. The conclusion of the study that in India there is no link from employment to electricity use (the demand link) is questionable, given the number of studies that have found a link from GDP to energy and electricity. It is possible that a more fully specified model would have reached different conclusions. The presence of a demand link would tend to reduce the size of the production link and hence the estimate of job creation. The study then estimated the elasticity of employment with respect to electricity demand using a simple regression in which formal employment was regressed on electricity consumption and GDP for the whole economy. The resulting elasticity of 0.53 has to be treated with caution for a number of reasons:

1. No standard errors were provided so that the statistical significance cannot be assessed.
2. The coefficient on GDP was negative. The authors explain this by arguing the very rapid growth of informal employment is positively related to GDP, so that total employment (if it could be observed) would be positively correlated with GDP. Without further supporting evidence this explanation is possible but scarcely convincing.
3. No other determinants of employment were included in the regression and this raises the possibilities of specification bias impacting the employment elasticity.

The final category of employment creation was for those firms that were affected by power blackouts because of transmission constraints. It was assumed that the removal of these constraints would result in these firms increasing their production proportionate to the lost supply of power. This number was based on a survey asking firms to provide estimates of the percentage of sales lost due to power outages. As discussed in the section of this review on power outages, the question needs to distinguish between direct losses experienced prior to any coping activities (such as installation of backup generation) and net losses. The restoration of power supply may enable costs of production to be reduced by avoiding the operating costs of backup generation, but the capital costs of backup generation are still payable during the life of the plant. The employment effects of switching from backup generation to grid supply may be small when there is little loss of supply that is not mitigated by coping measures.

This approach is narrower than the literature discussed above analyzing the links between total energy consumption and GDP in an economy, but does assume that such links exist at a project level. Induced employment from the project is created by the increase in household incomes generating a rise in the demand for goods and hence a rise in employment, while the increase in electricity capacity leads to macro-economic growth.

The above study also highlights the limitations of IO analysis as a forecasting tool. No actual data are collected after the event that makes it possible to validate the assumptions made about the operation of the IO model and the magnitudes of the effects, nor are there a set of previous studies of similar projects in the same countries that could be analyzed statistically to estimate the average effect per dollar of spending. Further, even if data are collected, attribution will be difficult, because it is not the sheer number of additional jobs created that matters, but additional jobs relative to the counterfactual, which is challenging to pin down, given many other developments in the market and the economy during the intervening years.

Assessment

There has been a substantial effort to test for the existence of causal links between energy use and GDP. Recent meta-analysis of this literature by Kalimeris, Richardson, and Bithas (2014), based on 158 studies, indicated that each of the four possible causal links had been “demonstrated” in a comparable number of studies. Further analysis showed that the chance of a particular pattern of causality being accepted was not correlated with the choice of approach measured over six attributes related to the methodology followed in each case. Importantly the econometric methodology adopted did not appear to favor any particular causal pattern.

An analysis of the review of the literature by Ozturk (2010) led to the conclusion that a possible weakness of these studies was the failure to include variables determining demand and production, apart from energy and GDP. Given that omitted variables are known to create bias in estimation and hence lead to incorrect inferences this shortcoming might contribute to the lack of coherence of results on causality testing. The present study therefore surveyed 136 papers to check how many publications had included basic statistical tests and core control variables. Each paper was checked to see if it had tested for causality and allowed for non-stationarity of the variables. Out of those that had, only three were found to have included both demand-side determinants (energy prices) and production-side determinants (capital and labor). These studies were for very different cases—Sweden over 150 years, Malaysia from 1960, and 21 African countries from 1970. The direction of causality established was not the same across the three studies; in the testing for cointegration (required to establish the presence of long-run relationships between the variables) energy prices as well as capital and labor (except in the study on Malaysia) were significant. This pointed to the need to include variables linked to both possible relationships. The failure to do so in the vast majority of studies may have led to omitted variable bias and incorrect inferences about the nature of the causal links.

However, the study on Africa contained a number of possible specification errors, and the study on Malaysia provided no information on variable measurement, limiting reliability for establishing solid conclusions for other work. Clearly more work is needed that takes into account all major determining variables for both the demand equation and the production function equation.

In reviewing this literature it is notable that the issue of reliability of supply was not addressed. Unlike the literature relating infrastructure provision to economic growth, no regressions that separately introduce the quality of energy supply as well as the quantity were explored.

Chapter 4: Power outages

Summary of findings

The impact of power shortages on the economy has been evaluated using a number of different methods. Studies have focused particularly on the poor quality of service (frequent, prolonged outages and large voltage fluctuations damaging equipment). The methods found in the survey discussed in this chapter are the following:

- *Regression modelling* where an impacted (outcome) variable (total costs, income, or productivity) is related to the duration and frequency of outages. This provides a test of the significance of the hypothesis that outages are important, and permits a quantification of their effects for the data set in question.
- *Direct-loss approach* where users are asked to evaluate the losses they have sustained from power outages, or would sustain from a hypothetical outage situation. This provides a quantification of the effects of the outage experienced, but does not allow significance testing, and is dependent on recall and the ability of respondents to take into account all costs and coping actions involved. It is necessary to distinguish between gross losses (the total loss caused by the lack of power from the grid) and net losses that include adjustment through coping actions such as the use of backup generation. The net loss is adjusted by any outage-related savings (wages not paid or material not used because of non-functioning of plant) and by outage-related costs (damage to materials or plant, costs of reprocessing material, costs to restart equipment). A full description is provided in UNEP (2012).
- *Indirect-cost approach* where the costs of installing and operating backup generation is calculated, and applied to those firms that have adopted this solution. Costs of backup generation include the annualized capital cost of the backup plant used, and costs of operating such plant for the period of the blackout (fuel and maintenance costs). Foster and Steinbuks (2009) provide a detailed example for the calculation of indirect costs.
- *Willingness to pay (WTP)*, which asks users how much consumers would be willing to pay to be offered a defined improvement in the quality of the power supply. Where consumers have already invested in physical capital (backup generator), their answer would relate only to the extra running costs they are incurring, thereby underestimating the total effects of the outages.

As well as the net costs to business users of electricity caused by outages, there are also indirect costs to the economy caused by the links from supply industries to downstream producers. Most studies of the costs of outages ignore such indirect effects but the analysis of Pakistan by the Institute of Public Policy (2009) quoted a study suggesting that the value-added multiplier from the industrial sector (affected by load shedding) to other sectors was 0.34, giving some indication of how much costs might also be shifted downstream.

The results of the quantitative studies surveyed are summarized in Table 4 and an assessment of their value in providing usable information in other circumstances is indicated. Three studies stand out as providing quantitative results based on a sound methodology and yielding plausible values for the costs of outages as measured in the particular study:

- Iimi (2011) analyzed the impact on total costs of production of the various factor inputs and also the frequency and duration of outages in 26 countries in Eastern Europe and Central Asia. A one-percent increase in the frequency of outages was associated with a 0.7-percent increase in total costs for given input levels, while a one-percent increase in the duration of the average outage led to a 1.3-percent increase in costs. Tests showed that small firms were not any more affected by power outages than large firms, although a similar analysis for the water sector showed that small firms were more affected by outages.
- Foster and Steinbuks (2009) studied the costs and benefits of backup generation in 19 African countries. The study provided a detailed account of how the costs of self-generated electricity were calculated, and the results ranged from US\$0.13/kWh to US\$0.74/kWh.
- The World Bank (2001) analyzed agricultural power supply in Andhra Pradesh and Harayana, and quantified its effects on farm incomes in Harayana in 1999. Three sources of inadequate power supply were quantified: (i) the availability of power through the rostering arrangements used to limit total supply; (ii) unscheduled outages during the roster periods; and (iii) transformer burnouts due to over-loading, poor maintenance, or lightning strikes. Farm incomes in Harayana were regressed on a number of variables, including measures of these three factors. For medium to large farmers, statistically significant coefficients indicated that an increase of 1 day per year lost to transformer burnout cost US\$107, while an extra hour per day of unscheduled outage cost US\$658. The estimated willingness to pay by these farmers to reduce unscheduled outages by 25 percent was about 15 percent of their base income. No statistically significant results were found for small to marginal farmers.

Table 4: Survey of results on outages from papers selected

Authors	Countries	Method	Findings	Robustness of results
Iimi (2011)	26 ECA countries	Firm-level regression	Both frequency and duration of outages had significant effect on costs. A 1% increase in frequency was associated with an increase of total costs by 0.7% with other inputs constant, while a 1% increase in the average duration of outages would have increased costs by 1.3%.	Solid methodology. Results provide plausible values and indicate importance of duration relative to frequency.
Reinikka and Svensson (2002)	Uganda	Probit analysis of decision to install backup generation; least squares analysis of effects of outages and backup generation on private investment of sample of firms	The level of outages had a significant negative effect on decision to own backup generation, and for firms without backup generation a 1% increase in number of days with interrupted power supply results in a 0.45% decrease in the investment rate.	Valuable model of the decision process under which firms may decide to acquire backup generation. Statistical results are plausible and significant. The failure to distinguish between frequency and duration of outages may result in some bias, depending on how these two measures are distributed over the sample of firms.
Foster and	19 African	Survey of firms	Calculation of cost of backup	Valuable data on indirect

Authors	Countries	Method	Findings	Robustness of results
Steinbuks (2009).	countries	to permit calculation of country-average effects	generation yields a range between US\$0.13 and US\$0.74/kWh. In all but one case, this cost is well above the cost of grid electricity. Figures presented for the value of lost load ranged between US\$13/hour and US\$1,140/hour, with 13 countries experiencing costs of more than US\$100/hour.	costs in a number of countries, together with detailed explanation of methodology. Figures for the value of lost load do not seem to have the right units, but if intended to be kWh, then they are much higher than those found elsewhere, because industries with costs of more than US\$50/kWh are unusual.
Oseni and Pollitt (2013)	11 countries in Africa	Two-limit Tobit model to estimate costs of unmitigated outages for firms that had insufficient backup generation for outages.	Demand for backup capacity was greater for firms with higher loads, greater size, export promotion strategies, and internet usage. Total outage costs (both mitigated and unmitigated) ranged between US\$0.62 and US\$3.32/kWh, of which unmitigated costs accounted for 50–60% of the total.	Valuable model in terms of dealing with the distinction between mitigated and unmitigated costs for firms with some backup generation. Results heavily dependent on the method of estimating amount of backup generation used.
World Bank (2001)	Haryana, India	Farm incomes and regression	Income of medium and large farms significantly affected by unscheduled outages and transformer burnout: an increase of 1 day per year lost to transformer burnout cost Rs 4,600 (US\$107), and an extra hour a day of unscheduled outage cost Rs 28,300 (US\$658). The estimated WTP for these farmers to reduce unscheduled outages by 25% was about 15% of their base income. Unreliable power had no significant impact on the income of small and marginal farmers.	Valuable results for the special case of agriculture in India where pumping for irrigation is important.
Allcott, Collard-Wexler, and O’Connell (2014)	India	Regression model of changes in firm-level output as a function of changes in outages	Shortages at 2005 levels (7.1% national average) were estimated to increase input costs between 0.13 and 0.5% of revenues and lower revenues by 4.8%.	A valuable study for its full articulation of a model of firm adjustments to shortages, and the use of a very large data set. Shortages were measured at a state year average level and could not distinguish frequency from duration, nor differences in outages experienced between individual firms in the same state and industry.
Alam (2013)	India	Regression model impact of outages on steel	10% increase in outages lowered output and profits of steel mills by about 10%, but for rice mills	The use of a proxy variable—average nighttime illumination as measured by

Authors	Countries	Method	Findings	Robustness of results
		and rice mill behavior.	output was little affected and profits fell only 5%, because they switched to a higher rate of throughput to compensate for lost time of operation.	satellite—to stand for outages would need further investigation before it can be relied upon not to introduce measurement bias into the regression equations.
Fisher-Vanden, Mansur, and Wang (2012)	China	Regression model of effects of power outages on total production costs allowing for other inputs for industrial enterprises.	Firms in regions of greater outages decreased factor share of electricity and increased share of materials and capital, with no increased use of self-generation. This suggested a tendency to buy rather than make energy-intensive intermediate inputs and to increase energy efficiency.	The measure of power outages was not ideal because it did not directly measure frequency and duration, was constant for all firms in each of the six regions of China, and exhibited similar trend growth in each region. The methodology is valuable in the introduction of a coping strategy of outsourcing energy-intensive intermediate inputs.
Diboma and Tatiense (2013)	Cameroon	Survey of firms' assessment of cost of hypothetical outage situations	Direct loss per hour of interruption estimated to cost €3.6 (US\$4.6)/kWh if scheduled outage, and €5.4 (US\$6.9) if unscheduled. Indirect cost of backup generation estimated to cost €3.4 (US\$4.4)/kWh, but other figures quoted indicate only €0.63 (US\$0.81). Tariff was €0.12 (US\$0.15)/kWh.	Limited value because indirect costs are significantly higher than values found elsewhere.
Bose, Shukla, Srivastava, and Yaron (2006)	Karnataka, India	Based on data from a survey of businesses	Direct loss Rs 22 (US\$0.51)/kWh, indirect cost of backup generation Rs 2.6 (US\$0.06)/kWh, and WTP Rs 4.9 (US\$0.11)/kWh. Grid tariff Rs 4.3 (US\$0.10)/kWh.	Values of questionable robustness because all three costs are much lower than impacts of outages calculated in other studies. In particular, if indirect costs were so much lower than grid costs it would not be worthwhile to use the grid at all.
Abdullah and Mariel (2010)	Kenya	Multinomial logit and random coefficient logit using household survey data of WTP	Both duration and frequency were significant in households' choices between hypothetical alternatives. WTP calculation does not specify what alternative households were asked to consider ("improve service reliability").	Limited value for results because of lack of clarity in the write-up. Contains useful methodological example of dealing with unobserved heterogeneity in econometrics.
Siddiqui, Jalil, Nasir, Malik, and Khalid. (2011)	Pakistan	Survey of businesses	Calculated output loss on basis of proportion of working day with outages, and scaled by different numbers of months with blackouts. Scaled up to obtain national figures. Losses	Limited value except where data are scarce. The assumption of losses being proportional to number of hours of outages is strong and does not allow for any form

Authors	Countries	Method	Findings	Robustness of results
Moyo (2013)	5 SSA countries	Firm-level regression	for industrial sector ranged between 12 and 37 % of output. 1 extra day of outage per month led to a small rise in output for firms with backup generators, and a 31% fall in output for firms without generators	of coping or for savings in running costs during outages. Limited. Magnitude of effect of increased number of outages a month is too large to be credible—3 extra days would imply almost complete loss of a month's worth of output for firms without backup generation.
Andersen and Dalgaard (2013)	39 SSA countries	Macroeconomic regression	Outage variable significant in explaining long-run growth rate. 1% increase in number of outages decreased long-term GDP per capita by 2.9% but only half the long-run effect was achieved after 100 years	Limited value because of idiosyncratic specification and very slow adjustment of the growth rate to its new equilibrium following the change in outages.
Cissokho and Seck (2013)	Senegal	Firm-level data envelope analysis and regression	Calculation of firm efficiencies shows the average firm efficiency was extremely low compared with the best in the sample. Regression of efficiencies on duration and frequency was insignificant or had wrong sign.	Limited. The extremely low level of average relative efficiencies suggests that calculation was specific to Senegal. The lack of statistical significance of outage variables may well be caused by the way in which firm efficiency was calculated.
Chakravorty, Pelli, and Marchand (2014)	India	Rural households survey data using regression	Connection to electricity had significant effect on income, leading to an average increase of 180%. The quality of supply combining connection and outages was significant and predicted that connection to a high-quality (low-outage) source of power would increase income by 260%.	Limited. The effects of connection are implausibly high, and the method of measuring outages is too restrictive to allow an effective test of their effects.
Kaseke and Hosking (2012)	Zimbabwe	Mining companies survey data using regression	Direct costs of load shedding per kWh varied by mineral type between US\$2 for vermiculite and US\$62 for asbestos. Duration was found to have a negative sign in a regression of total outage costs on a number of variables, while frequency was insignificant.	Limited. The results varied strongly among minerals, and the regression model was not consistent with results found elsewhere that costs increase with duration and frequency of outages.
Adenikinju (2005)	Nigeria	Survey of firms using marginal costs of unsupplied power based on revealed preference	Survey showed that 93% of backup generation resulted in mitigated outage costs being very much larger than unmitigated costs. Regression of outage costs on a number of factors failed to find significant	Limited. The level of backup generation was so high that it would be difficult to extrapolate costs of outages to other countries. Failure of regression suggests there may be specification or

Authors	Countries	Method	Findings	Robustness of results
		approach. Regression of outage costs on number of variables.	effects for duration or frequency, and only the level of electricity consumption was significant.	measurement errors.
Jyoti, Ozbaflı, and Jenkins. (2006)	Nepal	Survey of cost of outages for three firms.	Separate cost calculations were undertaken for unannounced (failure) and announced outages (load shedding), but for two firms these were identical for each of the three years analyzed. These ranged between US\$0.13 and \$0.28/kWh, while for the third firm the costs were about US\$1.00/kWh	Limited. The small sample and the finding of identical costs for announced and unannounced outages for two of the firms suggests that the results cannot be safely extrapolated to other countries.
Pasha and Saleem (2012)	Pakistan	Survey of households and calculation of costs of outages	Estimated that total outage costs amounted to almost 7% of household expenditure at a rate of US\$0.25/kWh	Valuable discussion of various approaches to estimating costs of outages for households. Proposed a new model in which loss of welfare caused by outages was estimated by a WTP questionnaire. This new approach, integrating direct costs and WTP, requires more evaluation in other situations before being adopted as a reliable tool.

Source: Authors' analysis of the cited papers.

Power outages and their effects

Power outages occur in all countries, but in certain developing countries they are frequent and on average last long. It is universally accepted that outages—also referred to as shortages, blackouts, load shedding, loss of load, or unserved energy—result in losses to the economy, and one important policy question relates to the magnitude of the adverse effects. There are other ways in which power quality can decline, such as voltage dips and swells (UNEP 2012), with attendant adverse effects, but there do not appear to be studies that have tested for the significance of any effects on outcome variables.

Although power outages affect all consumers, the main focus in studying their effects has been on agriculture and industry. In countries where pumping for irrigation is common and farmers are connected to the grid, outages can result in loss of irrigation and damage to crops, with a corresponding loss of income to the farmers. The case of India is notable because the large subsidies given to power consumption have led to extensive reliance on pumping and cultivation of water-intensive crops such as sugarcane.

Electricity is a major input for a number of industries, and its shortages affect firm behavior. Certain newer, high-tech industries are highly dependent on a guaranteed level of power quality, and poor-quality

power supply either acts as a constraint on the emergence of these industries in a particular economy, or else forces firms to adopt expensive alternative methods of power supply.

There is an extensive and varied literature on the estimation of the costs associated with outages in an economy, providing insights into the potential benefits from projects that reduce outages, such as more generation or transmission capacity, pricing schemes to reduce peak loads to a level that can be supplied by the existing system, or other investment upgrades that improve the quality of power supply. There is an asymmetry between the costs incurred by recent experiences of outages and the benefits of reducing such outages. Once users have adopted coping strategies that include incurring capital costs (such as the purchase of backup generation), these are fixed for the life of the equipment and it might be uneconomic to remove them upon improvement of the power supply to the point where backup generation is no longer needed. Such a scenario points to situations in which the additional costs imposed by outages could be much higher than the benefits obtained from their removal.

The nature of the links from outages to economic losses

Power outages affect households already connected to the grid, and may also discourage connection for households that are not yet connected. Most households do not have recourse to backup generation, and for them power outages would mean resorting to other forms of lighting such as candles or kerosene lamps, with lower efficiency and convenience, and the benefits associated with access to electricity—such as longer hours for study for children and powering home businesses—will be lessened (Abdullah and Mariel 2010; Chakravorty, Pelli, and Marchand 2013). The factors affected by the increase of the “intensive” margin (the amount of electricity for connected households) and the increase of the “extensive” margin (the number of households connected) are similar, so that policies to reduce outages and policies to increase connections have similar qualitative effects on household income and household welfare. However, the quantitative effects can be different—satisfying basic lighting needs through connection is more important than not being able to use a television or fan whenever desired.

Strategies for households to cope with outages do not appear to be widely studied in the literature, possibly because the only strategy available for most households is to return to a pre-connection use of energy sources (candles, batteries, kerosene lamps). In extreme cases of persistent power outages, such as in Nigeria, a substantial number of better-off households have invested in self-generation with its attendant capital and fuel costs.

The effects of power outages on businesses depend on whether they adopt any coping strategy, and if so on the extent to which this strategy replaces the power lost as a result of the blackout. The study by Alby, Dethier, and Straub (2012) provides a framework for analyzing the decision on whether a firm will decide to invest in backup generation in the face of persistent outages. Some firms will not adopt any coping strategy and will lose all the output affected by the loss of power. Some firms will invest in backup generation and will aim to compensate for part or all of the output lost due to the outage. The economics of these decisions depend on the amount of the loss of output and the savings in input costs from non-production; the capital cost of the backup power of the chosen size and the running costs for the amount of production due to the operation of the backup generator; and the costs and benefits of other coping strategies, such as working overtime to catch up part or all of the lost output. A detailed list of the types of losses due to outages and various coping strategies is given in UNEP (2012).

As well as the net costs to business users of electricity caused by outages, there are also indirect costs to the economy caused by the links from the affected industries to downstream producers. A delay or cancellation of an input, caused by a power outage, can affect the output and profitability of the downstream industries. Most studies of the costs of outages ignore such indirect effects but the analysis of Pakistan by the Institute of Public Policy (2009) quoted a study suggesting that the value-added multiplier from the industrial sector (affected by load shedding) to other sectors was 0.34. The study also calculated the macroeconomic employment and export effects of the outages. It did not calculate the direct effects of these outages on the non-industrial sectors, making the estimate of total loss of value added (2 percent of GDP) conservative.

Methodology and results

Various approaches have been developed to quantify the cost of outages to the user and to the economy. The studies are divided between econometric modelling, where hypotheses on the effects of outages are tested, and survey approaches where the effects are calculated conditional on a set of specified assumptions. The studies also vary from economy-level (Andersen and Dalgaard 2011) to econometric studies based on firm-level data on the relation between productivity and power quality (Escribano, Guasch, and Pena 2010) and to survey-based analysis of outages and their costs (Foster and Steinbuks 2009). Each method has its own strengths and weaknesses, which should be borne in mind when relating empirical results to a potential project in the energy sector designed to reduce the level of outages. This range of approaches makes it difficult to compare results from various studies—the variables measured are often different, and the questions asked in surveys also are often context-driven.

Macroeconomic linkage

The link between power quality and the growth of an economy is discussed in the section of the note on infrastructure and growth. Calderón and Servén (2010a, 2010b) and Seneviratne and Sun (2013) constructed indices of infrastructure quantity and infrastructure quality, and linked them to the growth of economies. The level of outages was a factor in determining power quality but not included in the aggregated indices constructed by these studies. Instead only transmission and distribution losses were used, presumably because lack of economy-wide data for many countries.

Andersen and Dalgaard (2013) related the average annual growth rate of economies over a 12-year period to the number of outages in a typical month for 39 Sub-Saharan African countries. Noting that the number of outages can be endogenous (affected by the growth rate of the economy), they used an instrumental variable (IV) estimator. The instrument chosen was a measure of the number of lightning strikes. The basis for this choice was the fact that lightning damage accounts for about two thirds of over-voltage damage to electricity networks in South Africa, and that over-voltage accounts for about one third of outage incidents. No other structural variables were included in the regression except some natural resource factors (coastal, precipitation, temperature, absolute latitude) that may be correlated with the chosen IV. The outage variable was significant and negative. However, the lack of structural variables such as capital and labor inputs means that omitted variables bias is a possibility. The coefficient and significance of the outage variable could be quite different in a more traditional formulation of an economic growth equation. Furthermore, the outage variable itself may be subject to measurement error. The average annual growth rate over the period 1995–2007 may be affected by outage levels and

durations over the whole of that period, rather than simply by the frequency in a typical month taken from World Bank Enterprise Survey data for 2011. The econometric results showed that a 1-percent increase in outages (as measured by the number of outages per month) decreased the long-run GDP per capita by 2.9 percent. However, this result was obtained within the context of a dynamic adjustment model where convergence to a new equilibrium is extremely slow, and half the long-run effect is achieved only after 100 years. The idiosyncratic specification of the model and the omission of key variables make this study of limited interest for a direct comparison with other studies.

Micro-econometric growth linkage

A number of studies have used a variety of approaches to investigate the effects of power outages on firms' performance through the use of econometric modelling.

Moyo (2013) related firm output to capital, labor, material, and a measure of power quality and other productivity control variables (firm age, foreign ownership dummy, country and sectoral dummies) for 1,598 firms in five countries in Sub-Saharan Africa. These countries experienced between 2 (South Africa) and 12 (Tanzania) outages per month. Various measures for power quality were tested, including number of days per month with an outage, number of hours without power per day, percentage of output lost due to power outages in a year, and ownership of a backup generator. Estimation was by OLS for both a pooled cross-section model and for country- and sector-specific models. The aggregate model assumes homogeneity of reaction to outages across countries and sectors, while the disaggregated models allow for some heterogeneity. The use of power outages in hours per day and in sales loss per year were more often negative and significant than the frequency of outages per month, suggesting that duration has larger effects than frequency. However, the use of OLS to estimate a production function where labor and capital are likely to be endogenous limits the reliability of the results. A one-unit increase in duration was estimated to cause output for all firms to fall by 7.6 percent. Firms that had a backup generator actually showed a net positive effect for the outage and generator effects combined, while firms without a backup generator were estimated to experience a 31-percent fall in output for a one-unit increase in the average duration of outages—an increase of 3 days a month would result in virtually all output being lost, even though the original number of days of outages varied only between 2 and 12 a month for the countries in the sample. The lack of plausibility of these results suggests that the estimation method was not reliable.

Imi (2011) analyzed the impact of infrastructure quality on the cost of production for 4,000 firms in 26 countries in the European and Central Asia region. In these countries access to power, water, and telecoms was high but the quality of service was variable. For each of the three sectors there were two measures of quality: annual frequency and daily duration of service suspensions obtained from Business Environment and Enterprise Performance Surveys. The model used is a trans-logarithmic cost function using data on the value of output and the costs of inputs (labor, energy, and other expenses) and the six measures of infrastructure quality. Estimation was by the “seemingly unrelated” regression technique and by stochastic-frontier analysis. The former assumes that resources have been allocated efficiently, while the latter allows for technical inefficiency with the efficient frontier defined by the best performances among the sample of firms. The mean number of days when there was an outage was 8.7, and the mean duration of each outage was 2.1 hours. The cost elasticities with respect to infrastructure quality estimated from the regressions were calculated: that for the frequency of power outages was 0.007 and for duration was 0.013, both being positive (as expected) and significant. The implication that outages of shorter

duration but greater frequency are less damaging than longer but less frequent ones is relevant for the design of planned outage programs. If all outages were eliminated total costs could have been reduced by about 1.3 percent. Tests for the relation of the cost elasticity of infrastructure quality to firm size were not significant for electricity, although it was found that small firms were more affected by water shortages. Sector disaggregation of the cost functions showed that continuous power was particularly valuable for construction, manufacturing, aggregate transport, hotels, and restaurants. The coefficients of the main variables were all significant and the elasticities were small, but the overall effects on the total costs of production were substantial. Estimation using stochastic-frontier analysis yielded broadly similar results, which is reassuring given the vulnerability of the latter method to outlying data. In economies where the average frequency and duration of outages are much larger, the study points to large adverse effects on total costs with corresponding loss of output. The data demands for this approach to measuring the impact of power outages are substantial, requiring cost data for outputs and all inputs, as well as firm-level information on outages.

Reinikka and Svensson (2002) analyzed the effects of poor quality of public capital, as experienced through power outages, on the investment made in private capital. Their model predicted that high levels of outages would lead to lower levels of private sector investment. Firms could decide to cope with this situation by installing complementary capital (backup generators) themselves. However, this capital is less productive than investment in the public sector would have been. The model was tested by using data on a sample of 171 firms in Uganda in 1997. The mean number of days per year when there was an outage was 89 with a standard deviation of 69 days, indicating wide variability between firms. The 69 firms that owned a generator had a mean number of days of outage of 102, versus 80 for those without a generator. The model estimated two equations, one being a probit equation in which the ownership (or not) of a generator was related to the number of lost days and a number of exogenous explanatory variables (including the size and age of the firm), while the second equation related the rate of investment in non-generation plant to a non-linear formulation, in which generator ownership and the number of lost days were the key variables. The probit equation indicated a significant and large effect of outages. A firm with one standard deviation fewer days of uninterrupted supply from the grid (relative to the average firm) would have a probability of 60 percent that it owns backup generation—about 20 percent higher than the average firm in the sample.

The investment regression indicated a significant difference in behavior (conditional on the probability of outages) between firms with installed generators and those without. For firms lacking backup generation a 1-percent increase in the number of days of interrupted supply resulted in a 0.45-percent reduction in the rate of investment. For firms with backup generation an increase in outages did not have a statistically significant effect on investment. Two issues related to possible specification errors for the estimated models should be noted. First, as pointed out by the authors, there is a possibility that the number of interrupted days is endogenous. If particular power lines are known to be more reliable than others (because of certain priority customers) then firms may choose to locate so as to be supplied by such lines. If this effect is strongest for firms with production processes more sensitive to reliability of supply, then this sorting process would bias estimates towards zero. The strong negative correlation found between investment and outages in a least squares regression would then be reinforced if there is such a selection bias. A second issue was the measurement of outages. This study was unable to separate frequency and duration and assumed that any outage, however short, was as important as an outage of several hours. The study by Iimi (2011) demonstrated the possibility that the cost elasticities with respect to frequency and

duration may differ. If the average duration per day differed between firms in some systematic fashion, there would be measurement error in the variable used for outages.

Fisher-Vanden, Mansur, and Wang (2012) analyzed the response of firms to electricity shortages in China based on 45,000 observations of industrial-firm-level data between 1999 and 2004. The formal model of firm behavior used to study the effects of power shortages on firm behavior was a cost function that allowed for four types of response to such shortages:

1. Decreased productivity (blackouts increasing total costs)
2. Increased use of self-generation with substitution of non-electric energy and capital for electricity
3. Outsourcing by purchasing intermediate inputs that make intensive use of electricity rather than making them
4. Increasing energy efficiency by reducing electricity use and increasing capital.

The authors used a translogarithmic cost function that related total production costs to factor prices of fixed assets, labor, materials, electricity, other energy, the gross value of output, and a measure of electricity scarcity. The measure of electricity scarcity was an annual grid-level statistic that related thermal generation to thermal capacity, adjusted by scheduled and forced outage rates. The lack of interconnection between the six regional grids allowed this statistic to measure reliability within a region. Fixed effects for firms and industry \times year were also included. Because of the possibility that scarcity was endogenous (being caused by greater industrial activity) IVs (grid-level heating and cooling degree days) were also used.

The principal results indicated that in regions of greater power shortages firms decreased the factor shares of electricity and increased the share of materials, while there was no evidence of a significant increase in self-generation. Firms facing greater power shortages also became more capital-intensive. This, coupled with the decrease in energy use, suggested that enterprises may have improved their energy efficiency. The overall effect of blackouts, proxied by the scarcity measure, was estimated to increase cost in the range of 2–20 percent, primarily due to factor substitution.

The results of this study were crucially dependent on the measurement of blackouts. The measure constructed was constant across all firms in each of the six regions. Furthermore, each measure showed rather similar trend growth over the six years of the sample. Alternative measures of scarcity, including hydropower capacity or peak hourly utilization rates, showed similar trends. In addition, one region had data on the duration and frequency of outages. An outage measure of the annual megawatt-hours curtailed based on these additional data was correlated only modestly with the main measure of power shortages. Further, there was no discussion as to whether all firms within a region or of a particular industrial category suffered the same annual outages. Because of the distinct nature of the Chinese economy it is not possible to rely on the quantitative findings of this study for insights into the effects of power outages in other economies. The emphasis on an evaluation of the importance of different methods of coping with outages by power suppliers, and the methodology developed, provide valuable tools for further analysis of this topic.

Allcott, Collard-Wexler, and O'Connell (2014) constructed a formal model to understand the role of shortages in production decision for manufacturing firms in India. Output was modelled as Leontief (strictly proportional) in electricity and a Cobb-Douglas aggregate of materials, labor and capital,

allowing electricity to be a binding constraint. The data unit was the firm in a given year. Inputs were either fixed before the current year (capital), semi-flexible in that they could be modified at the beginning of the year but not varied within the year (labor), or fully flexible in that they could be modified within the year (materials and electricity). Firms had the choice to invest (or not) in self-generation (which costs more than grid electricity when available). The authors summarized the principal theoretical insights of the model as follows:

Shortages have very different effects on firms with vs. without generators. Firms that use generators face an increase in electricity costs (*input cost effect*). This enters the profit function like an output tax and thus reduces demand for other inputs (*the output tax effect*). Even if these firms never stop production during shortages, productivity is lower due to the *input variation effect*: using different bundles of fully flexible inputs during outage vs. non-outage periods is less efficient than having a constant flow of production. Firms without generators are shut down during shortages, which reduces output and causes waste of non-storable inputs (*the shutdown effect*). The waste reduces demand for non-storable inputs when firms foresee periods of higher shortages (*the shutdown tax effect*).

The model was tested with two sets of data. The first was a case study of large textile manufacturers facing weekly pre-scheduled “power holidays.” Each of the sample of 22 firms had backup generation. For this set of firms the effect of power holidays was small: energy costs rose by 0.22 percent of revenues, physical output fell by 1.1 percent, and productivity dropped only by 0.05 percent because 95 percent of inputs (labor and materials) could be flexibly adjusted on power holidays.

The second set of data was taken from the Annual Survey of Industries between 1992–93 and 2010–2011, yielding more than 600,000 plant-by-year observations. From these observations differences over time (mainly one year changes) in various outcome variables were related to changes in outages, industry-by-year, and state dummy variables. To allow for the possible endogeneity of outages (improvements in economic conditions within a state could increase productivity and output, leading to an increase in shortages) an IV was constructed that caused shortages to vary but was otherwise unrelated to the manufacturing sector—this variable captured hydropower production in the year and overall generation capacity addition in the previous year.

The principal results obtained from the IV estimation were that for plants that own generators a one-percentage-point increase in shortages increased the share of self-generated electricity by 0.57 percentage points, which raised inputs costs by between 0.02 percent and 0.07 percent, indicating that the input-cost effect imposed on firms with generators is relatively small. Across all plants a one-percentage-point increase in shortages decreased revenues by 0.68 percent. Based on a nationwide average shortage of 7.1 percent (the actual value in 2005) the model estimated that input costs were increased between 0.13 and 0.5 percent of revenues and that revenue loss was 4.8 percent.

The authors extended the testing in a number of ways that confirmed the importance of the IVs, differences between industries, and the importance of scale economies in self-generation. Several conclusions were drawn from the study:

- Electricity shortages are a large drag on Indian manufacturing, of the order of 5 percent of revenue.

- Shortages affect productivity much less than revenue, and shortages alone are unlikely to explain much of the productivity gap between firms in developing and developed countries.
- Shortages have heterogeneous effects across firms with and without generators and with high vs. low electric intensity. Relatedly, because of economies of scale in self-generation, small plants are less likely to own a generator, meaning that shortages have much stronger effects on small plants.

The use of shortage variables based on annual state-level data—for the ratio of assessed demand in the absence of shortages to the actual quantity supplied—provided variation between years and between states. The study did not distinguish duration from frequency nor did it investigate whether firms within the same state experienced differences in outages over a year, and it is possible that further disaggregation of shortage data, were it to be available, could modify the results obtained.

In a seminar paper posted on the internet, Alam (2013) investigated how firms in different industries cope with power outages. The study used plant data from the Indian Annual Survey of Industries between 1999 and 2010. The steel and rice milling industries were selected for detailed analysis of how these firms adjust production practices in the face of outages. Interviews with industry showed that the steel industry had little alternative but to reduce production. They can install backup generation but this is largely used for safely shutting down the plant in response to an outage—backup generation on the scale required to deliver the electricity required by such power-intensive processes would be uneconomic. Steel firms can adjust production hours when outages are announced and adjust their production schedule accordingly—perhaps operating 24 hours per day during weekends to make up lost time. Rice milling, which is a seasonal activity, has a technological flexibility unavailable to steel. The production process can be accelerated and more produced when power is available. However, this accelerated production comes with a higher fraction of waste product and increases per-unit costs.

Linear regression models related various annual outcome variables to a measure of outages specific to the district and time period, fixed industry effects, district-year fixed effects and district-level rainfall year totals that can influence the price of rice as well as the possibility of outages. The measurement of the power outage variable was innovative and was based on satellite data measuring average visible light. This provided a yearly measure of light intensity at night. Nighttime light intensity is related to the extent of any outages, and Alam argued that day and nighttime outages are highly correlated in India. This use of a proxy for the key exogenous variable could lead to estimation bias. If light intensity varies over time or between districts in a way not associated with outages and not simply modelled by district and time fixed effects, systematic measurement error could affect the results. The use of an average light intensity measure combines both duration and frequency dimensions of outages into a single measure and cannot be used to test for a difference in their statistical significance.

The study found that a 10-percent increase in power outages resulted in steel mills using 10 percent less grid electricity, while rice mills used 4.9 percent less. Profits of steel mills fell by 8.5 percent, while those of rice mills did not fall significantly. Further tests confirmed that rice mills were better able to adjust to power outages in terms of number of hours operated. This study provides useful insights into how the nature of an industry and its technology may affect its ability to cope with power outages. A study of the effects on micro and small industries of frequent and unannounced blackouts in Ghana (Braithwaite and

Amponsah 2012) also indicated that the costs and coping actions were related to the nature of the industry involved.

A study by Cissokho and Seck (2013) related firms' productivity to various inputs using data envelopment analysis on a sample of 528 businesses in Senegal in 2013. Measures of technical, scale, and cost efficiency were constructed for each firm by comparing individual inputs and output to the best values in the sample. These efficiency measures were regressed on a set of variables related to the quality of electricity supply, the characteristics of the firms, and their environment. For the sample the average number of outages per month was 26, with an average duration of 2.3 hours. Given the high frequency of power supply interruption, it is not surprising that 90 percent of firms surveyed owned backup generators. The results from the data envelope analysis were extreme—the average cost efficiency was 6 percent of the corresponding best performance in the sample, and the average technical efficiency was 2 percent, the great majority of firms thus being far inside the efficient production function boundary. In the second stage the regression of these estimated cost efficiencies on the frequency of outages yielded an insignificant coefficient, while that of duration was positive and significant. The authors explain this unexpected result by arguing that firms had learned to deal with outages by becoming more cost-efficient. The extremely low average efficiencies observed from the data envelope analysis suggest that the data are dominated by one or two very efficient firms, making all others appear highly inefficient. This, combined with the lack of significance of the frequency variable and the positive sign for the duration variable, suggests that the results are too specific to the data in Senegal to give usable pointers for other studies.

The World Bank in 1999 carried out a very detailed recall survey of 1,659 farmers in Haryana, India (World Bank 2001). A two-stage approach was used. In the first stage an explanation for the choice of irrigation technology was analyzed (diesel, electricity, both, or neither) through the use of a multinomial logit formulation. At the second stage the determinants of net income for a given technology choice were analyzed through the use of OLS augmented by the Mills' ratio for that technology choice from the first stage. Attention was given to sample selection and endogeneity and different formulations were tried in order to test for robustness. A large number of the farmers surveyed used backup generation to help with crop irrigation. The costs of the backup generation included not only capital and fuel cost but also maintenance and repair costs—motors burnt out frequently and needed to be rewound. The econometric model related net farm income of pump-owning farmers to a number of variables, including the number of days lost due to transformer burnout, average power availability per day through rostering, unscheduled power cuts in hours per day, and motor burnout frequency. In addition a number of farm- and region-specific factors were included. For small farmers the outage variables were all insignificant, but for medium and large farmers the days lost due to transformer burnouts and unscheduled power cuts were negative and significant. The results are shown in Table 5, and indicate that outages have a significant effect on medium and large farmers but not on marginal or small farmers. The authors suggest that the latter may be more vulnerable to shocks and hence make larger precautionary adjustments (larger-size pumps or crop choices). For the medium to large farms an increase of 1 day per year lost to transformer burnout cost Rs 4,600 (US\$107), and an extra hour a day of unscheduled outage cost Rs 28,300 (US\$658).

Table 5: Impact of power supply conditions on short-run net farm income of electric pump owners in Haryana in 1999 (Rs 1,000)

Variable	Marginal and small farmers	Medium and large farmers
Days lost to transformer burnout (days/year)	0.51	-4.6**
Power availability (hours/day)	9.47*	-1.57
Unscheduled cuts (hours/day)	5.3	-28.3**
Motor burnout frequency	10.3	1.3

Source: World Bank 2001.

Note: * denotes coefficient significance at 10%; ** at 5 %.

Based on the econometric results, estimates of farmers' willingness to pay for improved reliability were calculated for both the short term (irrigation technology remains constant) and medium term (irrigation technology could change). The results are shown in Table 6. Generally the medium- and large-scale farmers were willing to pay more for improvements to the quality of power supply, while marginal and small farmers might have overinvested in electric pumps. It is noteworthy that the medium- and large-scale farmers were not willing to pay for greater power availability, but valued reduced outages much more. In the WTP calculation the medium and large farms were willing to pay an amount equal to at least 10 percent of base income to obtain a 25-percent improvement in reliability of power.

Table 6: Farmer willingness to pay for improvements in power supply indicators in Haryana in 1999 (Rs)

Reform scenario	Medium term			Short term		
	Marginal and small	Medium and large	Average	Marginal and small	Medium and large	Average
Base year incomes	33,400	119,000	94,500	33,400	119,000	94,500
Increase in power availability by 1 hour/day for the year	14,000–14,200	ns	4,000–4,100	9,400–9,700	ns	2,500–2,800
25% increase in reliability	8,600–8,700	14,900–19,400	13,100–16,300	ns	14,900–19,400	10,600–13,800
25% decrease in days lost due to transformer burnout	ns	10,900–11,800	7,800–8,400	ns	10,900–11,800	7,800–8,400
25% decrease in frequency of motor burnouts	244	462	399	244	462	382

Source: World Bank 2001.

Note: ns indicates that effect not significant in the net income regression.

Chakravorty, Pelli, and Marhand (2014) used survey data from 1994 and 2005 for 10,000 households in rural India to investigate the impact of connection (extensive margin) and power outages (intensive margin) on rural incomes. Because the allocation of connections to villages may be endogenous with respect to household incomes, the authors proposed two IVs for the electrification decision. The first was the district-level variation in land elevation—setting up a network is cheaper on flat land—while the second was the density of transmission cables in the district relative to the national average. The higher the density the lower will be the cost of connection to a village, while transmission density is exogenous being determined by the federal government. The quality of power was characterized as high if power was supplied for at least 18 hours a day and low if less than 18 hours a day. The quality variable combined both connection and outage information—it took a value of zero when the household was not

connected, 0.5 when it received low-quality supply, and 1.0 when the quality was high. An IV estimation of the impact of grid connection on household income introduced control variables (household size, number of children, assets, occupation, and village and year fixed effects). Households that were connected were estimated to have 180 percent higher income than those that did not. The second stage of the model related household income to the quality of power variable as well as the other controls. If the quality variable increased from zero (no connection) to unity (high-quality supply), the results imply that income would increase by 260 percent, while if the household were connected to poor-quality supply income would be 130 percent higher. Moving from low-quality to high-quality power supply would increase income by 130 percent. These results are too extreme to be credible. In addition, the measurement of quality as a dichotomous value, depending on whether more or less than 18 hours a day supply was available, is too crude to allow extrapolation to other situations.

Escibano, Guasch, and Pena (2010) investigated the effects of infrastructure quality on total factor productivity for 26 countries in Africa based on investment climate surveys for manufacturing firms between 1999 and 2005. The approach adopted was to include a wide range of investment climate variables: infrastructure quality (electricity, water, telecoms, and transport); red tape, corruption, and crime; financial and corporate governance; quality, innovation, and labor skills; and other firm-level control variables. The total factor productivity was estimated using the Solow residual obtained via the estimation of a production function related to labor, materials, and capital. In order to address the problem of endogeneity of the inputs, a long list of observed firm-specific fixed effects coming from the investment climate surveys was used as a proxy for the unobserved firm-specific fixed effects. Further adjustments were made to allow for simultaneity between the total factor productivity and the infrastructure variables. Estimation and specification followed the general-to-specific approach—the initial set included 90 explanatory variables, which were then removed one at a time until the remainder were significant. With respect to the power sector a number of quality variables were included: own-generation, own power infrastructure excluding generators, percentage of electricity used that was self-generated, annual cost of generator fuel as percentage of annual sales, average cost of electricity from the grid, dummy for equipment damage by power fluctuations, total number of outages, average duration of outages, percentage of sales lost, average number of power fluctuations, average duration of power fluctuations, and the number of days to obtain electricity connection. A large number of results were provided, both in aggregate and by country. For countries with high income-growth, infrastructure quality had a low impact on the total factor productivity, but for low-growth countries poor infrastructure quality had a large negative effect, suggesting substantial bottlenecks in the economy. Among the factors that most influenced the average total factor productivity was poor quality of electricity provision, which affected mainly lower-income countries, while allocative efficiency was also most affected by poor-quality electricity provision. With so many variables and countries the study presented only summary findings, so that detailed follow-up on the role of individual variables would be required to draw specific lessons on their importance.

Survey-based evidence on outage impacts

The most common approach to evaluating the effects of power outages on customers has been the use of surveys of consumers. There are three distinct approaches, although two can be combined into a composite analysis:

- The *direct loss* approach asks respondents what their sales losses from outages in a given period were. There are two problems with this approach if followed to the letter, as appears to be the case in several studies. First, the accuracy of response depends on the accuracy of recall. Where the period under investigation is lengthy, perhaps a year, it may be difficult to identify all episodes of power outages and their duration and associated loss in output. Second, the way the question is posed does not necessarily distinguish the initial loss of output and the recovery of some or all of the output through coping mechanisms. Using backup generation has its own costs but would be used only if it conferred a net benefit relative to doing nothing. Similarly, the use of overtime would reduce the net losses suffered. Only by making an explicit distinction between the gross loss of output due to the initial outage and the net loss once coping actions are taken into account would an accurate picture of the effects of outages be obtained. Studies using this approach have tended to conclude that it overestimates the cost of outages because respondents exaggerate the effect of the losses, but the high costs so calculated may be because the respondents concentrate on gross and not net losses.
- The *indirect cost* approach quantifies the extra costs of backup generation, such as the capital costs of the generator, maintenance, and fuel costs. It is unclear in some studies whether they take into account the fact that not all potential sales loss may be made up because backup generators may not be large enough for all the electricity needed. If all lost sales are made up, the extra cost of backup generation is the cost of the outage for firms coping in this way. This approach is acknowledged to ignore the fact that not all firms have backup generation. For some firms it may be better to cope through other means, or even to plan to exit the industry in the face of persistent outages and capital costs of generation that are too high relative to their resources, as in the model of Alby, Dethier, and Straub (2012).
- The *willingness-to-pay* approach asks respondents how much they would be willing to pay in order to avoid the possibility of outages. In principle, the answer should be based on the net costs of coping actions presently undertaken. Firms would be able to save the operating costs of backup generation, but not sunk capital costs, and would have extra sales above any shortfall that was occurring due to the partial nature of the coping process. This approach highlights the difference between what the outage has cost (including capital costs of backup generation) and the reduction in costs that could be achieved if the outage were to disappear. The way in which the questionnaire is designed is crucial to ascertain the maximum WTP of each respondent.

In summary, the above three approaches are likely to generate different valuations of the cost of outages and this is reflected in some of the studies that have used one or more approaches.

Bose et al. (2006) used all three approaches to estimate the costs of unserved power in the agricultural and industrial sectors in the Indian state of Karnataka. A survey of 500 manufacturing units and 900 farmers was carried out in 1999. The production-loss approach identified the loss of output due to the non-availability of electricity supply. The authors reported that many respondents in the manufacturing sector did not report the net loss (allowing for scrap values and production losses that are saved by adopting coping strategies) but only the gross loss. The indirect-cost approach included the annualized capital cost of backup generation, maintenance, and fuel costs, but appeared to be applied only to respondents who invested in backup generation—it was the cost per unit of backup power generated rather than the average cost of a unit of lost power. To estimate the WTP for respondents the authors reviewed alternative approaches to questionnaire design and decided to use a “bidding game” approach, starting with a high

price and reducing it until the respondent indicated they would be willing to pay the amount. Some respondents were not willing to pay anything to avoid outages, and these were recorded as zero when constructing the average WTP. Because many respondents had already invested in the cost of backup generation, their WTP was correspondingly lower. For the manufacturing sector the average production loss per kWh was Rs 22 (US\$ 0.51), the indirect cost of captive generation was Rs 2.6 (US\$0.06), and the WTP was Rs 4.9 (US\$0.11). For this sector the average tariff was Rs 4.3 (US\$0.10). For the agricultural sector the production loss approach per kWh was Rs 3.6 (US\$0.08) and the cost of running an irrigation pumpset (diesel or electric) was Rs 2 (US\$0.05). The WTP approach indicated that farmers were not willing to pay more than they were already doing, possibly reflecting the belief that subsidies would always be available. The results for the indirect cost of running a backup generator, even though they included capital and operating costs, appear to be much too low—it would be cheaper to self-generate than to use grid electricity. This result casts some doubt on the study findings.

Abdullah and Mariel (2010) surveyed 200 households in Kenya to ascertain their WTP for three alternative scenarios. The three scenarios offered two improved situations and the status quo: (i) 5 planned outages a month and average duration of 3 hours, (ii) 5 planned outages and average duration of 2 hours, and (iii), status quo, with 6 outages a month and average duration of 6 hours. These also differed by price (above the existing charge) and the type of distribution provider. The choice between three alternatives was modelled using a multinomial logit distribution in which the explanatory variables included cost, frequency of outage, duration of outage, type of distribution provider (private or community), and a number of socio-economic variables. All the variables were significant, apart from the type of service provider. However, the multinomial logit itself is related to preferences between the three alternatives offered and does not provide a valuation of the differences. The model was extended by using a random-coefficient logit specification that allowed for heterogeneity between individuals. Based on the latter model the WTP for specified socio-economic characteristics was simulated, but the authors did not specify exactly what was being offered in the WTP calculation—whether this was for a complete absence of outages or the reduction of one outage per month. This innovative approach required detailed knowledge of the technical literature to estimate the random coefficient variant and to simulate the WTP from the results. The use of a limited menu of choices is problematic in that it may not represent what could actually be offered to consumers. The method completely avoids using information on the actual losses suffered or the costs of any coping action that has been taken. Finally, because what options households are being offered in the WTP calculation are unspecified (the text merely mentions that it is “to improve service reliability”), it is not possible to interpret the WTP figures obtained from the use of the random-coefficient logit model.

Siddiqui et al. (2011) surveyed 339 firms in four cities in Punjab in Pakistan in 2007. The central variable was the loss of labor hours per day (categorized by industry). The results showed that 30 percent reported no loss or one hour lost per day, while more than 50 percent reported three or more hours lost per day. A separate question on the increase in the cost of production found that three quarters of the firms opted for some form of alternative energy arrangements (mainly standby generators). The average cost increase for the sample was 26 percent. A simple calculation was undertaken to quantify the costs of unserved energy in terms of output loss. The number of hours lost per day was compared to the assumed shift length per day in order to estimate the percentage of daily output lost, on the assumption that losses continued at the same level for a certain number of months. Shift lengths of 8 to 12 hours were considered (biggest losses occurring for a given loss of hours when the shift is shortest), and the number of months during the year

in which such outages occurred was evaluated for periods of 6 to 12 months. The study then aggregated estimates of output losses in the industrial sectors from the main provinces to yield an estimate that the total lost output nationally was between 12 and 37 percent of industrial value added. The authors recognized that there were coping costs and that many industries were able to reduce the losses implied by the number of hours of grid power outages. However, these were not taken into account in the calculation of losses to the economy and as a result the figure for such losses is likely to have been substantially overestimated. The assumption that output losses are proportional to the percentage of the working days in which outages occur is too simple to be used in any case unless there are no other data available. This approach also makes no distinction between the frequency of outages and the average duration of an outage; the only factor that is significant is the total number of hours lost.

Diboma and Tatietsse (2013) surveyed 70 industrial companies in Cameroon in 2009. The sample included a mix of firms with and without backup generation. To make an estimate of the direct effects of power outages they asked respondents to quantify the various costs of six different scenarios. These included both announced and unannounced 1-, 2- and 4-hour outages. The costs included the value of lost production from outages (costs of restarting, damage to materials and plant, and costs of operating backup), as summarized in Table 7.

Table 7: Direct cost of interruption to power supply in Cameroon (€/kWh)

Duration	Interruption with advance notice	Interruption without advance notice
1 hour	3.6	5.4
2 hours	2.5	3.5
4 hours	2.0	2.5

Source: Diboma and Tatietsse 2013.

The highest cost per kWh was €5.4 (US\$6.9) for an unannounced 1-hour outage, and the lowest cost per kWh was €2.0 (US\$2.6) for a 4-hour announced outage. In addition the authors estimated the costs of backup generation including all capital and running costs, based on estimated operating times for such generation. The average cost was €3.4 (US\$4.4) per kWh of unsupplied energy. The treatment of capital costs in terms of expected life and interest rates was not provided and hence the calculation for the cost per hour supplied by captive generation cannot be assessed. The direct-cost calculation took into account the operating costs of backup generation but not the capital costs, underestimating the total net cost of power outages. These figures have to be compared to the grid cost of supply of €0.12 (US\$0.15)/kWh. The authors also commented that the cost of self-generation for industry was €0.63 (US\$0.80)/kWh, much lower than the indirect cost estimated in the study and raising questions about the derivation of the figures.

Foster and Steinbuks (2009) analyzed the decision to invest in self-generation for countries in Africa using data drawn from the World Electric Power Plant Database and the World Bank's Business Enterprise Surveys. As part of this analysis they carried out an evaluation of the costs and benefits of self-generation, and this provided a measure of the costs of power outages. The authors pointed out that a competitive, risk-neutral firm will maximize expected profits by equating at the margin the expected cost of generating 1 kWh of its own power to the expected gain due to that kWh. The gain consists of the continued production (even if partial) that self-generating makes possible and the avoided damage to equipment that might have been caused by a power failure. Because the expected marginal gain from a

self-generated kWh is also the expected marginal loss from the kWh not supplied by the utility, the marginal cost of self-generated power is an estimate of the marginal cost of an outage. This approach combines the value of loss of output (if any) and the costs of coping with the outage. The authors estimated the unit cost of self-generated electricity, taking into account the way in which capital costs vary with the size of diesel generators and assuming an internal rate of return of 10 percent and an average generator life of 20 years. In the absence of information on the total duration of outages, the frequency of outages was multiplied by an assumed average duration of eight hours per day. The results, shown in Table 8 for the country-level cost of self-generation, indicated that generally it was much more expensive than electricity from the grid. The results taken across countries show a large measure of similarity and can be compared to indirect cost estimates presented in other papers. They are much lower than the value reported by Diboma and Tatietsé of US\$4.4/kWh for Cameroon.

Table 8: Comparative Costs of Self-generated and Publicly Supplied Electricity in Africa (US\$/kWh)

Country	Average total cost of self-generation	Price of kWh purchased from grid	Country	Average total cost of self-generation	Price of kWh purchased from grid
Algeria	0.15	0.03	Mali	0.52	0.17
Benin	0.46	0.12	Mauritius	0.61	0.14
Burkina Faso	0.74	0.21	Morocco	0.62	0.08
Cameroon	0.46	0.12	Niger	0.41	0.23
Cape Verde	0.50	0.17	Senegal	0.34	0.16
Egypt, Arab Republic	0.30	0.04	South Africa	0.54	0.04
Eritrea	0.13	0.11	Tanzania	0.29	0.09
Kenya	0.29	0.10	Uganda	0.44	0.09
Madagascar	0.39	—	Zambia	0.45	0.04
Malawi	0.50	0.05			

Source: Foster and Steinbuks 2009.

— Not available.

The authors also present figures for the losses due to outages (“lost load”) for firms with and without backup generators. For firms without backup generators these range from US\$13/hour in Senegal to US\$1,140/hour in South Africa (with 13 countries having values in excess of US\$100/hour). For firms with backup generators the value of lost load ranges between US\$2 and US\$444. The units are unusual and the results are difficult to interpret, unless the units were intended to read kWh instead of hour. If kWh, the results are markedly outside the international experience of other countries where values rarely exceed US\$50/kWh for even the sectors most dependent on grid electric power.

Kaseke and Hosking (2012) used the direct assessment method to estimate the costs of load shedding on the mining sector in Zimbabwe. Firms numbering 120 of various sizes and mining various minerals were surveyed in 2008. Statistics on the frequency and duration of load shedding were collected, as well as other characteristics of the firms. Some firms had an arrangement with the power utility for uninterrupted supply, but in practice this could not be guaranteed. For mines without this arrangement there were on average six outages per week lasting an average of eight hours, while those mines with the arrangement experienced an average of two outages a week with a two-hour duration. The cost of load shedding was calculated from the costs of lost output, labor costs, loss of material, restart costs, and damage to

equipment, and was estimated to amount on average to US\$31/kWh. No apparent adjustment for backup costs was made and the authors did not discuss coping strategies. The costs varied strongly across minerals, ranging from US\$2/kWh for vermiculite to US\$61/kWh for asbestos. Total direct costs of outages were regressed on the total number of outages, the average duration of outages, total hours lost to outages, total operational hours, capacity of the mine, revenue income, electricity expenditure, employment, dummy for uninterrupted supply arrangement, and dummy for backup equipment. The product of the first two explanatory variables is equal to the third variable, so that redundancy is built into the equation. The coefficient of the duration variable turns out to be negative while the frequency and backup dummy variables are insignificant. The regression would need to be re-specified before the results could be judged for their relevance. The wide variation in costs of outages across minerals suggests that care would have to be taken in extrapolating the results to mining operations elsewhere.

A similar study of the Nigerian power sector by Adenikinju (2005) estimated costs of outages from a survey of firms. Costs of backup generation were adjusted for scale and varying discount rates. The study showed that mitigated costs of outages dominated unmitigated costs because of the extremely high level of backup generation observed (93% of firms owned backup and mitigated 87% of power outages). A regression of outage cost on a number of explanatory variables was unable to find a statistically significant relation between outage costs and the duration and frequency of outages, despite the very high level of outages. Only size-related variables (electricity consumption and employment) were significant. The possibility of specification error or measurement error may explain the lack of significance of the key variables. The extremely high level of backup generation observed in the Nigeria case also limits the reliability of extrapolating the negative finding of a lack of relation between outage costs and the extent of outages to other economies.

A study by Jyoti, Ozbaflı, and Jenkins (2006) of the costs of power outages in Nepal was based on data on frequency and duration of outages experienced by three firms. The data allowed a distinction to be made between unannounced outages (failure) and announced outages (load-shedding), and the costs were calculated for each based on the direct costs experienced. These costs per kWh varied over time and across firms, but for two of the three firms the kWh cost of failure was equal to the cost of load-shedding in each of the three years analyzed. This surprising result suggests that coping costs had not been fully integrated into the calculation. For these two firms the costs of outages ranged between US\$0.13 (2005 prices) and US\$0.28/kWh, while for the third firm the costs of announced and unannounced outages were about US\$1/kWh. The results from this study are of limited value for extrapolation because of the small sample and the finding that announced and unannounced costs were the same in many instances.

Oseni and Pollitt (2013) revisit the estimation of the costs of backup generation in Africa by focusing on unmitigated costs even for those firms that have some backup generation. Based on the approach by Beenstock et al. (1997) they construct an expression for unmitigated losses due to incomplete backup. At the optimum degree of backup the firm will equate the costs of a marginal kW that is not backed up to the marginal cost of backup. The optimal demand for backup is derived assuming the marginal cost of backup is constant and there is an exponential loss-distribution function. There are three outcomes from this type of model:

1. Below a certain threshold the firm decides not to invest in backup and all losses are unmitigated.
2. The firm decides to invest in limited backup so that there are still some unmitigated losses felt by those activities within the firm that are not supplied by backup.

3. There is complete backup and no unmitigated losses exist.

This structure implies that the mean outage loss is a censored variable, both below zero and above some value at which only complete backup is viable.

The model assumes that mean outage loss is a function of various factors that differentiate between firms, including firm's load, size, export promotion (proxied by ISO certificate holding, and dummies for the use of the internet for the firm's operation, sector, and country). The study needs to estimate the extent to which backup generation is used as well as grid electricity, and it does so by assuming values for total outages of the grid and comparing known firm expenditures on electricity (from survey data) with grid prices, information on backup ownership, and the extent of outages experienced by the firm.

A two-limit Tobit model was applied to 5,920 firm observations from 11 African countries in 2007. Of these 3,767 were censored from below (no backup) and 457 were censored from above (fully backed up). The upper threshold was estimated to be 0.85—that is, because of indivisibilities and installation costs, firms prefer to fully invest in backup once it would be worth investing in at least 85 percent backup. The results also indicate that the demand for backup increases with size, export promotion, load, and the use of the internet. The model was then used to calculate unmitigated losses for firms with incomplete backup. These ranged from US\$0.12/kWh for small firms in Mozambique to US\$3.20/kWh for large firms in Nigeria. Similarly firms operating at international quality standard suffered unmitigated costs per kWh between US\$0.47 and US\$3.00, while those not at international standard suffered unmitigated costs of between US\$0.20 and US\$2.52. Using these estimates and information on total outages the study showed that the total expected cost per kWh (including both mitigated and unmitigated elements) ranged between US\$0.62 in Zambia and US\$3.32 in Nigeria, and that unmitigated costs accounted for a substantial proportion of total outage costs (ranging between 46 percent and 72 percent).

These results indicate that when calculating the cost of backup generation for firms it is necessary to take into account the extent of unmitigated costs. Given the very large number of firms in Africa that still have no backup, policies to encourage the use of backup need to take into account the likely extent of unmitigated demand, as well as the nature of the firm. A further calculation compared the costs of outages with costs of grid supply under a cost-reflective tariff (removing subsidies that make actual grid prices very low in many countries). Outage costs were significantly above the true costs of grid supply, suggesting that countries that reformed their pricing might be able to attract sufficient grid investment that would make backup generation unprofitable and unnecessary.

The methodology of this study was dependent on the accuracy of estimating the total level of outage faced by firms and the extent to which backup generation was used, with respect both to frequency, duration, and kWh. In addition the model assumed that the marginal cost of backup generation was constant with no economies of scale to the size of the backup plant.

Pasha and Saleem (2012) analyzed the impact and cost of power load shedding on domestic consumers in Pakistan. They reviewed a wide range of approaches to valuing the costs of unserved power including the cost of self-generation approach, the value of lost-leisure approach, and the willingness-to-pay approach. They proposed a new approach to calculating the costs of outages to households by combining the costs of any backup generation or uninterrupted power storage with a measure of the costs of those activities that cannot be carried out because of the outage (allowing for the extent of self-generation). Instead of the

loss of output as suffered by a firm, there is a loss of utility because households have to reorganize their activities and this is valued by a willingness-to-pay estimate derived from a household survey. They calculated that total outage costs were almost seven percent of total household consumer expenditure, and amounted to a cost of US\$0.25/kWh. The novelty of the approach indicates that further work needs to be undertaken on the evaluation of the costs to households of power outages. Combining direct and indirect costs with a WTP element opens the approach to the weaknesses of consumer survey responses that may provide misleading valuations of the actual costs to the household.

Assessment

A wide variety of approaches have been used to estimate the costs of power outages, partly because the aims of the studies have been different and partly because what is considered a loss has varied. Studies based on survey data, where households or businesses have been asked about the effects of outages, have not always been clear about what is meant by “lost output” or what is included in the costs incurred in coping with the outage. The studies by Foster and Steinbuks (2009) and UNEP (2012) give guidance on the full range of costs to be included and the treatment of lost production (both gross from the initial outage and net after coping strategies have been allowed for). Part of the variation in outage costs calculated by different methods (direct losses, indirect costs, and WTP) in studies such as Bose et al. (2006) can be attributed to the different assumptions underlying the evaluation of these alternative calculations.

Studies that have approached the estimation of outage costs for households or firms through a regression-type approach that related income (sales) to the level of outages, as well as socio-economic and other control variables based on survey data, face the issue of endogeneity. Incomes may be lower where outages are high, but outages may also be high where incomes are low because of poor payment discipline, leading to low revenue from these customers. Parallel to the literature relating the net economic benefits to whether a household is connected to the grid, the choice of IVs is crucial. Chakravorty et al. (2014) propose two such variables—one already established in the literature (the variation in land elevation for the village or district) and the other an innovation (the density of transmission lines) and these appear to be satisfactory.

The models that have related the total factor productivity to the quality of power supplied have distinguished between the frequency and the average duration of outages, allowing for the possibility of giving different weights in the estimated model. The study by Iimi (2011) is particularly interesting in this regard, because the estimation of a firm-level cost function gives sensible results and does not run into major specification problems. For the countries of Eastern Europe and Central Asia the duration of power outages was more important than their frequency—the elasticity of costs with respect to the former was about twice that of the latter. This finding has policy implications in terms of managing announced outages where there is some control over the frequency and duration. Given the heterogeneity of effects found at sector and country level, it is clear that such elasticities cannot be assumed for different regions or industries but should be re-estimated with the relevant data.

It is evident from the results found from this group of papers that only a few studies can be used with confidence as reference points for quantitative assessments of the costs of power outages. Methodological issues reduce the reliability of some studies, while unusually high or low values call into question the validity of other studies.

Some studies present methodological approaches that could be of interest for future detailed work on this topic. The treatment of the endogeneity of outages, parallel to that in the literature on the impact of grid connection, is crucial. If outages are directed to areas of low incomes, there would be a two-way link between outages and incomes and least squares regression would yield biased estimates of the effect of outages on incomes. Instrumental variable estimation can avoid this problem if it is possible to identify an instrument that is uncorrelated with income but is correlated with the degree of outages. Chakravorty et al. (2014) suggested the use of district-level density of transmission cables as an instrument on the grounds that in India it is determined at the federal level and would not be determined by local income considerations, while areas with more transmission are likely to receive higher quality power. This instrument yielded very different results from the use of OLS and, although the equation obtained was not plausible, the proposed instrument may provide a useful addition to other instruments that have been used to deal with the endogeneity of outages and connections. The paper by Abdullah and Mariel (2010) explored the possibility that there is unobserved heterogeneity between households with respect to the outage coefficients (after allowing for observed socio-economic differences, such as household size). They used a random coefficient logit model, and found that the results were improved. A WTP simulation was based on this model, but because the exact improvement in supply that was simulated was not specified, it is difficult to interpret the results.

Various studies have concentrated on particular aspects of coping strategies. Fisher-Vanden, Mansur, and Wang (2012) allowed for the possibility that firms buy rather than make those intermediate inputs that are energy-intensive, while Oseni and Pollitt (2013) focused on modelling the decision on the amount of backup generation (if any) to install. Alam (2013) introduced industry-specific factors that can lead to outages being mitigated by running the plant more intensively, albeit at higher cost. Allcott, Collard-Wexler, and O'Connell (2014) developed a production function model, in which grid electricity was a binding constraint and which led to a number of theoretical findings on the different effects of shortages on firms with and without generators.

Chapter 5: Access

Summary

A number of studies have attempted to estimate the benefits of electrification on households or small businesses. There are many possible paths by which the use of electricity or other modern fuels might benefit households (Kooijman-van Dijk and Clancy 2010; Khandker, Barnes, and Samead 2013) and analysis has focused on the estimation of the effects on outcome variables—income, total household expenditures, employment, or various dimensions of education such as time spent at home studying or the school enrollment rate.

The use of household survey data allows for the inclusion of a large number of factors that might influence the outcome variables, and most of these can be assumed to be exogenous—that is, they could affect the outcome variables but are not affected by them. Classic examples of household-specific factors are the age, gender, or the education level of the head of household.

A second group of factors are common to all households in a village or commune, such as the presence of an all-weather road, school, or the distance to local market, which vary from village to village. Where these factors are measured they can be added to the list of explanatory variables. If not, one way of accounting for them is to introduce a “fixed effect” for each village, whereby these common factors are assumed to affect every household in the same village by the same amount, but the effects may vary across villages.

Earlier studies assumed that the outcome variable of interest would be affected by these household- and village-level variables, including the household’s electrification status, and carried out OLS estimation of the coefficients of the explanatory variables. The coefficient on the electrification variable then was assumed to measure the increase in income or any other outcome variable enabled by electrification.

More recently, a number of studies have focused on the possibility that the electrification status of a household is endogenous: not only does it affect income, but the level of income determines whether or not the household is electrified. This can come about by a “placement effect,” in which the electric utility shows preference for providing electricity first to higher-income villages (because more households are likely to connect, hence lowering per-unit costs). It can also come about because where a village has access to electricity (for example, the village has been connected to the grid), the households willing to connect are those with higher incomes (especially if connection charges are not fully subsidized), producing a selection bias.

The impact of such endogeneity is to impart an upward bias to the estimation of the effects of electrification on income, so that studies not taking this endogeneity into account do not provide reliable estimates of the benefits of electrification. Alternative methods of estimation are required and three approaches have been used: instrumental variable (IV) estimation, propensity score matching (PSM), and panel data analysis allowing for heterogeneity between households.

Studies using these methods have found clear evidence that the electrification status of households is endogenous and that ignoring this leads to overestimation of benefits. There is general agreement that income and consumption are increased due to the connection to electricity. The effects on employment

can be measured in a number of ways (effects on men and women for hours worked, wage rates, participation) and they appear to be country- and situation-specific. Effects on education for boys and for girls in terms of enrollment, completion rates, and time spent on homework are generally positive, but again these vary among countries. Two recent studies have shown that unconnected households in villages where there is access show benefits from other households' electrification, and it is important to take this effect into account when attempting an evaluation of the benefits of programs to increase rural electrification.

A study by Peters, Vance, and Harsdorff (2008) of firms in Benin showed that electrification of a village was followed by the creation of certain electricity-reliant firms. These had significantly higher profits than non-reliant firms in areas with and without access to electricity. Non-reliant and connected firms in areas with access performed no better than similar firms in areas without access.

Several studies of the effects of electrification on households measured the effect on income. Kumar and Rauniyar (2011) found that in Bhutan farm income was unaffected but that non-farm income increased by 63 percent; Khandker et al. (2012) found that non-farm income rose by 70 percent in India; and a study by Khandker, Barnes, and Samad (2013) showed total income in Vietnam increasing by 28 percent. Consumption also increased significantly in some studies: Khandker et al. estimated an increase of 18 percent in India, and Khandker, Barnes, and Samad reported a 23-percent increase in Vietnam. Van de Walle et al. (2013) reported only a 7-percent increase in India for connected households, but estimated that unconnected households in villages where there was access also had consumption increasing by 1 percent a year following the electrification of the village.

The effects on education on time allocation and employment generally have indicted a variety of effects. In Nicaragua a study by Grogan and Sadanad (2013) found that women were 23 percent more likely to work while there was no change for men; Dinkelman (2011) in a study of South Africa found female employment increased by 30 percent with no significant effect for men; Khandker et al. (2013) found in India that women were 17 percent more likely to work, with no significant effect for men. However, van de Walle et al. (2013) found the reverse situation in their study of India—male labor supply increased by about 16 days a year while there was no significant effect for female labor.

Kumar and Rauniyar (2011) estimated that electricity connection in Bhutan increased the time spent in schooling by 0.54 years, and the time spent on homework by 10 minutes a day. Khandker et al. (2012) found that enrollment, study time at home, and years of education completed in India all increased significantly for both boys and girls. Van de Walle et al. (2013) found significant increases in enrollment and completion rates for girls but not for boys in India. Khandker et al. (2013) found completion rates for education in Vietnam were significant for boys and girls, while enrollment rates were insignificant for both.

As explained below some of the variations in results can be attributed to differences in estimation technique and economic specification, but country differences also appear to be important. Table 9 summarizes the main results and approaches used in some recent studies that have recognized the presence of endogeneity to varying degrees.

Table 9: Survey of results on effects of electrification

Authors	Country and time period	Method	Findings	Robustness of results
Peters, Vance, and Harsdorff (2010)	Benin 2008	PSM applied to profit levels of small rural businesses	Following electrification of a village certain electricity-reliant firms were created. These had substantially larger profits than non-reliant firms in areas with and without access. Non-reliant and connected firms in regions with access did not perform significantly better than firms with similar characteristics (propensity scores) in the non-access region.	The results were obtained using an appropriate methodology and several variants of approach. The negative result that non-reliant firms did not benefit from connection is important. The methodology depends on the ability to identify firms in the non-access region that would have connected had access been available. The probit model used requires that explanatory variables not be caused by the act of connection. One possible avenue reducing the robustness of the results is that, for firms created after connection, the investment level used to start the firm (one of the explanatory variables) might have been affected by the existence of access through some form of externality.
Grogan and Sadanand (2013)	Nicaragua 1998	Tobit estimation of rural household time allocated to different activities without IVs. Bivariate probit estimation linking employment to electrification.	The Tobit model found that women and men switched the time allocation from work for family agriculture to work for a salary. No significant reduction in time collecting firewood for women. The study found that women were 23% more likely to work following connection to electricity, while there was no difference in the propensity of men to work.	The equation linking time allocation to electrification did not allow for endogeneity and produced implausible results. The equation concerned with employment allowed for endogeneity and provided evidence that electrification had led to greater employment by women, but its reliability is limited because of possible weaknesses in variable selection. The choice of instruments (village population density and average gradient of land in the village) are suitable to deal with placement bias, but the authors did not address the issue that richer households (those with a higher number of working members) would be more inclined to connect once access was available. Only 12 explanatory variables were used to model the effect of electrification on employment.
Kumar and	Bhutan	PSM applied to	Total income and farm	The use of a variety of PSM

Rauniyar (2011)	2010	income and education of rural households	income were unaffected by electrification but non-farm income increased by 63%, time spent in schooling increased by 0.54 years, and time spent studying at home increased by 10 minutes a day.	techniques provided a check on the robustness of the results. The failure of the authors to distinguish between access and connection made it difficult to interpret the approach followed. Certain village-level variables (e.g., access to an all-weather road) that have been used in other studies were omitted and may have led to an over-estimation of the benefits of electrification.
Dinkelman (2011)	South Africa (2011)	IV applied to rural households in 1996 and 2001 to investigate effects on employment rate. Fixed-effects panel model applied to magisterial level data to test effects of percentage electrified on wages, hours worked, and earnings.	Female employment increased by about 9 percentage points (30% above baseline value) following electrification, but there was no significant effect on male employment. Female employment rose faster where the poverty rate, the female/male ratio, or both were higher. At magisterial level the only significant effect of electrification was on male monthly earnings.	The method used was appropriate and the results appear robust. However, two features particular to the data set make it difficult to extrapolate results to other countries: (i) once a community was selected for electrification, every household was connected free of charge, so that study compared only inter-community differences; (ii) virtually no agricultural employment existed in the area chosen, seriously restricting the employment choice. The community-level analysis used a plausible IV (average land gradient) to deal with placement bias, and the community level exogenous variables were also appropriate. The small number of magisterial districts (38) may help to explain why the other results were generally negative.
Khandker, Samad, Ali, and Barnes (2012)	India 2005	IV applied to rural households to estimate effects of electrification on income, employment, and education	Most outcome variables were significantly affected by electrification. Per capita consumption increased by 18%, labor supply of women increased by 17%, and non-farm income increased by 70%. Enrollment, study time at home, and years of education completed were all significant for boys and for girls.	The choice of IV concentrated on selection bias by using the percentage of households electrified in the village. The instruments did not address the issue of placement bias whereby richer villages were more likely to have been electrified. Further, as argued by van de Walle et al. (2013), non-electrified households in a village with access may benefit from externalities from others' use of electricity, thereby rendering the study's IV invalid. The complete list of exogenous variables used was not given, and not all

				estimated coefficients were reported, making it difficult to know which factors, apart from electricity, contributed to the outcome variables.
Van de Walle, Ravallion, Mendiratta, and Koolwal (2013)	India 1981/82 and 1998/99	IV applied to rural households to estimate effects of electrification on education, labor supply, and consumption expenditure.	Study separated effects on those households connected and those unconnected in villages where there was access to electricity. Consumption expenditure increased by 7%, which was lower than OLS value of 11%, thereby supporting endogeneity assumption. Unconnected households also increased consumption 1% a year from access being provided to village. Male labor supply increased by an average of 16 days a year but there were no significant effects on female labor supply. Enrollment in school and completion rates were significant for girls but not for boys.	A detailed model and plausible choice of IV suggest that results obtained are reliable. Used two IVs (state of electrification in opening year, and physical distance to power generating plants). Argued that IV used by Khandker et al. (2012) was invalid and showed that its use with their own data and model would have produced insignificant results. Used an extensive set of exogenous control variables.
Khandker, Barnes, and Samad (2013)	Vietnam 2002 and 2005	Difference equation based on 2-wave panel of rural households to estimate effects of electrification on income and education. Allowed for time-invariant and time-variant heterogeneity. Distinguished between household-level connection and commune-level access.	Using a 5% significance test and the time-variant model, electricity connection increased income for connected households by 28%, and consumption by 23%. Commune connection (testing for externalities to unconnected households) showed no effect on income but a 48% effect on consumption expenditure. Non-farm income was insignificant in all variants. The time variant model also showed significant effects for completion rates of education for both boys and girls, but not for enrollment rates.	The use of a model allowing for time-variant heterogeneity generates some significant results but does not provide a consistent picture of the effects of electrification: consumption increases strongly for unconnected households in villages with access while income did not increase; school completion rates increased for unconnected households in villages with access, while for households with connections they did not increase. The assumption used to model time-variant heterogeneity—that the observed electrification status in the opening year is independent of the change in status during the 3 years of the panel—may be questionable. The number of control variables (about 20) at household and commune level is substantially smaller than in some other studies.

Source: Authors' analysis of the cited papers.

Background

Energy policies in developing countries have as a major target the increase in the number of households using modern forms of energy. Globally, the *Sustainable Energy for All* initiative sets universal access to modern energy as one of its 2030 targets. As with other consumer goods, the balance between the benefits from consumption and the costs of purchasing energy determines its uptake. The costs of these forms of energy are far better understood than the benefits, which arise through a number of channels and require detailed analysis for quantification.

In terms of government policy, electrification has received much greater attention than modern energy for household cooking and heating, in part because electricity has many important uses outside of homes. Attempts at economic valuation of the benefits of using modern energy have similarly focused on electrification.

Studies on the benefits of electrification for households often use national household surveys. While lack of reliability of electricity supply in many developing countries means that electricity is not always available when needed, there are enough situations where households switch entirely from other forms of energy for lighting (such as kerosene lamps) and powering appliances (batteries) to electricity once they are connected to the electricity grid. The situation is far less clear cut with respect to cooking and heating. Even those who report that their primary source of energy for cooking is a gaseous fuel may continue to use solid biomass in parallel, especially in rural areas, in a usage pattern referred to in the literature as fuel stacking (Masera, Saatkamp, and Kammen 2000). Fuel stacking makes quantification of the benefits—especially health benefits—of the use of modern energy for cooking and heating challenging, requiring more specialized surveys than national household surveys such as those for the Living Standard Measurement Studies (LSMS). Customized surveys have included measurements of ambient concentrations of health-damaging pollutants such as fine particulate matter and collection of data on symptoms of respiratory and other illnesses associated with household air pollution. Data are collected from a relatively small sample of households to examine potential health effects (an example being Bates et al. 2013); time savings; and fuel savings where advanced combustion stoves for solid fuels are also more fuel-efficient, although greater fuel efficiency is far from being synonymous with clean fuel combustion (Smith 2002). The findings are generally situation-specific and cannot be generalized.

Unlike examination of electrification, virtually no study on cooking has attempted to quantify the impact of adoption of modern energy on income or total household expenditures. For these reasons, the rest of this chapter focuses mainly on economic valuation of the benefits of electrification. Electrification overlaps with modern energy for cooking and heating to the extent that electrified households have the option of cooking and heating with electricity, which has been historically important in countries such as South Africa.

At an aggregate level electricity is usually treated similarly to other consumer goods—demand is a function of prices and income. However, at a household level there are three aspects of electricity supply that need to be taken into account in understanding consumer behavior, and these are particularly relevant in rural areas, where the key decision is whether or not to consume electricity rather than how much electricity to consume.

- In many developing countries, the electricity grid does not extend to all households. Off-grid electricity may be an option but may be more expensive. Where grid electricity is available (referred to as there being access hereafter), some households may choose not to be connected because the benefits of doing so are perceived to be less than the costs. Hence, a distinction is made between *access* and *connection* in this chapter: the former depends on the decisions of the supplying body in extending transmission and distribution lines to various locations, while the latter depends on household decisions in those areas that have been provided with access. Policies to increase the uptake of electricity need to increase access and encourage connection by households once electricity becomes available.
- Grid electricity has fixed and variable costs. The fixed costs include the initial connection charge and monthly metering and service fees, while the variable cost depends on the tariff structure for residential consumers: increasing or decreasing block tariffs, volume differentiated tariffs, and time-of-day pricing. The cost structure, with a large fixed cost element, means that households need to calculate the benefits of connection over a number of years and compare them with the discounted value of the total purchase costs. The fact that purchase costs cannot be made very small simply by consuming very limited quantities of electricity is important in understanding why certain households choose not to consume electricity.
- Unlike many consumer goods, electricity is not purchased for the direct benefits arising from its consumption, but rather because it drives various appliances that provide various benefits to the household. Some benefits, such as using radios, television, refrigerators, and fans, may be felt largely as improvements in welfare (although there may be further induced benefits). Other uses—such as the ability to use an electric sewing machine, or effective lighting—can change behavior in ways that can lead to higher incomes and better education. Tracing all the pathways by which the adoption of electricity can affect a household is complex (Khandker, Barnes, and Samead 2013) and requires data on individual households and their use of electricity for various activities over a number of years, because the benefits take time to be fully captured. Accordingly, most studies focus on the relation between the decision to be connected and the final outcomes (income, employment, education).

A number of studies have looked in detail at benefits of using electricity on measurable outcome variables, such as income, employment, or education. Estimating the difference in such outcomes with and without an electricity connection raises important methodological issues that have been tackled in a variety of ways and these are discussed below. Kooijman-van Dijk and Clancy (2010) in a study of the effects of electrification in Bolivia, Tanzania, and Vietnam discuss a number of questions about the effects of electrification (such as “Does the presence of electricity stimulate production? Does an increase in productivity lead to a decrease in poverty in terms of financial capital?”). No quantitative evidence was cited so that the results of their investigation are impressionistic and cannot be evaluated for reliability or significance.

A related issue is that of household air pollution. Households that cook with solid fuels (wood, charcoal, coal) are exposed to high levels of indoor air pollution, of which fine particulate matter (smaller than 2.5 microns in diameter) is especially harmful to the health of those in close proximity to the cooking source. Policies to reduce household air pollution are receiving increased attention, but there are a number of

difficulties in evaluating the benefits of doing so. Duflo, Greenstone, and Hanna (2008) provide a valuable literature review arranged around four questions:

- How is indoor air pollution linked to fuel types and cooking stove technologies?
- How is health linked to levels of indoor air pollution?
- How is economic productivity of the household linked to health issues caused by indoor air pollution?
- What policies are available to reduce levels of indoor air pollution?

The answers to these questions require statistical analysis similar to that used to evaluate the effects of electricity connection on income, employment and education, and are discussed below.

Methodological issues

The main question asked in micro-economic studies of electricity use concerns the incremental benefits from adopting electricity. Tracing the changes in household behavior from the time it started using electricity may require a survey, not just of consumption of electricity and of income, but of time use, education undertaken, etc., for several years. Such surveys would be difficult to organize, and for this reason studies tracing the benefits of a given policy-induced change in energy choices on individual households are rarely undertaken.

Rather than attempt “before and after” analysis, most studies have attempted to quantify the benefits of electrification through a “with and without” comparison. The income, education, or time budget of those households that have been connected and those that have not been connected can be compared using survey data, usually based on a cross-section of households at a given point in time.

A recent publication (GIZ 2013) describes the structure of the testing procedure in which the variable is related to electrification. The simplest comparison is that of the mean income for a group of households with electricity compared to mean income for a group of households without electricity. However, this comparison is usually not a reliable guide to the effect connection has on income levels for three reasons.

1. Income is affected by many factors, and not just by electricity connection, and ignoring them could overstate or understate the effect of electricity connection. Soil quality, the existence of a road to the nearest market, the demographic features of households, and education levels are some examples. The standard approach to allowing for the influence of other factors would be to use a regression model in which income (as an example of a outcome variable) is related to those variables that may be determinants of inter-household differences in income, and a dummy (zero/one) variable to allow for the electrification status of the household. This approach would yield unbiased estimates of the impact of electrification on income, provided that none of the explanatory variables are themselves determined by income. However, the electrification status itself may have been determined by pre-existing income levels, as explained in the next two bullets. If so, OLS estimation will be biased.
2. Where survey data for the cross-section analysis are taken from different villages, some with access to the grid and others without, the general income level of the village is likely to influence the decision on which villages to electrify. Where incomes are higher more households would be likely to connect and the unit cost of the local distribution network would be reduced. Better-off

communities may also be more effective in lobbying for grid extension. This link between pre-existing income levels and the provision of electricity access leads to a further reason for an upward bias in OLS estimation.

3. Within a village with access, some households may have decided not to pay the connection fee. If that decision was dependent on their income level, as seems likely, the electrified households would tend to have higher incomes whether or not electrification actually raised incomes post-electrification. This provides a further reason for OLS estimation to show an upward bias on the effects of electrification on income.

The second and third sources of bias will generally exist at the same time, although there can be circumstances when only one is present. Studies such as that by the World Bank (2002) on the benefits of rural electrification in the Philippines do not allow for these sources of bias and hence provide results that are not reliable.

There have been three distinct approaches to avoiding the bias that could result from using an OLS estimation to measure the effect of connection on income:

1. Instrumental variable (IV) estimation attempts to remove the reverse link from income to the electrification status by modelling that part of the decision to be connected that is not dependent on the level of income.
2. Propensity score matching (PSM) attempts to identify those households in the non-access region that would have chosen to connect had electricity been available and compare their incomes with those that had chosen to be electrified where access was available. This removes the bias due to the inclusion of households whose income was a factor in the decision not to connect.
3. Panel data estimation uses differences of household outcomes over time between surveys as a function of differences in electrification and of other explanatory variables. Under certain circumstances this regression in differences provides unbiased estimates of the effects of electrification on the outcome variables.

IV estimation requires the identification of a variable or variables that are correlated with the explanatory variable of interest (electrification status of the household) but are not affected by the outcome variable, the level of income in this case. This in effect models that part of the electrification decision that is not influenced by income. With respect to the decision to extend access to a village, various physical features of the village, such as the average gradient of the land around the village (the underlying assumption being that the steeper the gradient, the more costly it would be to extend the grid), may have been important. With respect to a household's decision to connect, other household features (age, education level) might be considered independent of the pre-existing income and hence suitable as IVs.

The distinction between access and connection is important for choosing IVs. Most studies of the effects of electrification are concerned with the effects of connection, rather than access. The basic hypothesis has been that access alone makes no difference to a household unless it decides to connect. However, a recent study by van de Walle et al. (2013) produced evidence suggesting that unconnected households benefited from the connection of a neighbor, so that access can affect outcomes even in the absence of connection. In choosing IVs to model the exogenous component of the electrification status of a household, some studies have included instruments relevant only to the decision to provide access to the

village where the household is located, while others have included instruments relevant only to the household's decision to connect when access is available. In fact both types of instruments should be included. Correcting for the tendency of utilities to supply villages with higher-than-average incomes would not distinguish households within the village that had connected because of their higher income from those with lower income that had not connected. If all households in a village selected for electrification were connected—because the government provided subsidies to do so or because all households had sufficiently high income—the instruments would need to correct only for the difference between villages. Where data are drawn from several villages or regions, some of which are electrified, there can exist other village-level advantages that can increase income. These are often modelled using a village fixed effects—a 0/1 dummy variable that is one for the village in question and zero for all other villages. These effects are assumed to be equal for all households within the village, but potentially different from village to village.

In both cases it is not possible to directly test the nature of the instruments' relation to income, and their selection relies on a priori reasoning. The higher the correlation between the instrument and the electrification status, the better the instrument, provided that the basic assumption that it is not influenced by income is valid—a strong correlation between income and the instrument could merely be picking up the fact that higher existing income caused the level of the instrument.¹¹ Statistical tests can reveal not only whether the coefficients of the IV model are significantly different from zero, but also whether they are significantly different from those of the OLS regression that was thought to be biased. The latter can be regarded as a necessary but not sufficient condition for accepting the IV.¹²

PSM techniques compare connected households in the access region to those households in the non-access region that would have connected had they been given the chance. The simplest approach is to base the comparison on the mean outcome for the two groups. From the unconnected and connected in the access region, a probit model—type of regression where the outcome variable can take only two values—can be estimated in which the 0/1 dummy variable for connection (0 for no connection and 1 for connection) is related to variables thought to affect the connection decision and the outcome variable (income level), but which are not affected by the household's connection status. The statistically significant coefficients in the probit model for connection are applied to the values for these variables in the non-access region and those individuals with a predicted score of more than 0.5 are assumed to be those that would have connected if given the opportunity. The mean income level for this hypothetically connected group can be compared with the mean income of the connected group in the access region—the difference is then a measure of the effects of connection on income. As with IV estimation, the results

¹¹ Where there are more instruments available than variables that require instrumenting it is possible to carry out a test of the independence of the instruments and errors in the structural equation. A Sargan/Hansen test of over-identifying restrictions can be used to test whether all the instruments are exogenous, conditional on there being as many exogenous instruments as there are variables that require instrumenting (Söderbom 2009).

¹² A similar issue arises with studies that attempt to model the factors determining whether or not the household has connected to electricity. High income increases the likelihood that the household will choose to be connected if access is available, but connection itself can increase income. If measurement of connection and income is undertaken well after the actual time of connection, as is often the case with survey data, there will be simultaneity bias and instrumental variable estimation should be used rather than OLS regression. In this case the instruments selected should be correlated with income but should not be caused by the connection status of the household. If income at the time of connection were available it can be argued that this could not be affected by connection and hence would be a valid instrument. However, as long as many households were connected at different points in time, the use of existing income would require data collected over several years, and such data are rarely available.

depend on the validity of the assumptions made for the probit analysis. If the explanatory variables in the probit equation are determined in part by the connection status of the household, the prediction values for the unconnected group will be biased because they over- or understate the income level without connection. Extensions of this approach can be used to make comparisons between the two groups of individual households with similar characteristics relevant to the connection decision.

A key limitation of this approach is that the two groups of villages or regions selected for matching should differ with respect to income (or education or health) only because of the levels of the variables included in the probit analysis or because of electricity access. If they differed for other reasons, such as the availability of an asphalt road that could help raise incomes, the difference between incomes for the two matched groups is not due just to electrification. Selection of similar villages for comparison therefore becomes important.

The literature has noted that unobserved factors lead different households to connect at different times even when access is available, and these lead to bias when OLS is used in the simple regression model relating the outcome to electrification status. Using differences over time from panel data effectively relates changes in the outcome variable to changes in the explanatory variables, changes in electrification, and changes in the unobserved components. However, if the unobserved heterogeneity factor were constant over time, the use of differences removes this factor and the regression in differences is unbiased. A further adjustment can be made to deal with time-variant heterogeneity when it can be assumed that the form of time variation is such that certain initial characteristics are related to the time-variant component.

The analysis of the relationships between household air pollution and health, and between health and income, employment, and education raises similar methodological problems to studies on the effects of access. First, the specification of the link between levels of household air pollution and the outcome on health requires accurate measurement of the level of household air pollution and the health outcome variables. Second, the nature of the link between household air pollution and health outcomes needs to be understood and estimated. For example, is the health effects due to chronic exposure over a number of years, to the distance from the source of household air pollution, and to the age or gender of the respondent? A simple measurement of an average level of pollution at the time of the survey may introduce measurement error into the relation to be estimated. A third issue is the possible endogeneity of the level of household air pollution. As remarked by Duflo et al. (2008), observational studies linking health and household air pollution may include individuals who have taken measures to reduce household air pollution because they are wealthier, or better educated. In such a case regressing income on household air pollution blurs the two effects and would produce a biased estimate of the effects of household air pollution. As well as the small number of studies linking health in general to productivity there are some studies linking health in general to education, and these can be used to support the argument that reducing household air pollution would lead to an improvement in education.

Evidence on the effects of increased energy access and connection

Earlier studies that did not allow for the possibility that the connection of certain households to the grid was endogenous cannot be regarded as providing unbiased quantification of the benefits of electrification. Hence, this chapter focuses on recent studies that have explicitly taken account of possible endogeneity. In regression analysis, endogeneity occurs when the explanatory variable is correlated with the error term

in regression. If there is endogeneity, the regression coefficient in OLS estimation is biased. Endogeneity can occur as a result of simultaneity (two variables, such as connection to electricity and income, affecting each other), omitted variables, and measurement error.

Peters, Vance, and Harsdorff (2010)

These authors focused on the benefits of electrification as measured by the profits of small-scale manufacturers in rural Benin in 2008. They surveyed two groups of five villages, chosen for general comparability. Comparability criteria included distance from the capital, asphalt/dirt road usable throughout the year, population, presence of a secondary school, existence of a regular market, and access to micro-finance services. One group lay within an area that had been electrified (between 3 and 7 years prior to the survey) while the other had not been electrified. Within the electrified area (“access region”) there were 146 firms, and there were 130 manufacturers in the non-access region. In the access region 79 firms were created after electricity became available, and 20 of these firms indicated that they required a connection for their operation, referred to as electricity-reliant firms. Reliant firms included welders, saw mills, and printing shops, serving unoccupied niches in the market. The mean monthly profits for these different groups of firms are shown in Table 10.

Table 10: Average monthly profits of different groups of firms in rural Benin (FCFA)

	Non-access region	Access region	Energy reliant in access region	Non-reliant firms in access region			
Type of firm	All	All	Newly created	Newly created		Pre-existing	
Number of firms	130	146	20	59		67	
Connection status	na	na	Connected	Connected	Unconnected	Connected	Unconnected
Number of firms	130	146	20	16	43	23	44
Monthly profits ^a	73,560	87,100	197,620	80,680	55,340	67,960	80,220

Source: Peters, Vance, and Harsdorff 2010.

na = not applicable.

a. US\$1 = 509 FCFA at the time of the survey.

The simple difference in mean profits for firms in the access and non-access regions was not statistically significant, but the difference in profits in the access region between connected (118,500 FCFA) and unconnected (68,178 FCFA) firms was statistically significant using a conventional Student’s t test. However, this comparison cannot be used as a measure of the benefits due to electrification, because firms with better performance may have been the ones that decided to become connected when the opportunity presented itself. This potential simultaneity bias was addressed by the authors using a comparison described below between similar firms in the access and non-access regions.

A very striking finding was that within the access region firms not reliant on electricity—whether they were created before or after village electrification and whether or not they were connected—were significantly less profitable than reliant firms. The difference in mean profits between non-reliant firms

that were connected and non-reliant firms that were not connected in the access region was not statistically significant.

To compare profits between the access and non-access regions the authors had to identify firms that were similar in their characteristics so that they could be regarded as equally likely to adopt electrification once offered. A probit analysis was carried out for the non-reliant firms in the access region to identify the factors associated with the decision to be connected. The coefficients for the entrepreneur's age and investment capital used for firm creation were both positive and significant. The coefficients of this model were applied to the data for firms in the non-access region and those that achieved a propensity score (predicted probability of connecting) of greater than 0.5 were assumed to be those that would connect if given the opportunity. If actual connection led to an increase in income it is expected that mean income for those in the access area would be higher than in the non-access area. The mean profits for this latter group of firms were larger than the mean for the connected firms in the access region, but the difference was not statistically significant. Further comparisons between these two groups using PSM techniques confirmed that there was no significant difference between the profit levels of these two groups of non-reliant firms, all of which were small in size.

This study reached two important conclusions. First, there was no evidence that firms not reliant on electricity performed better in the access region than in the non-access region, even though a substantial number had chosen to be connected to the grid. Second, access to electricity made possible the creation of electricity-reliant firms and these performed significantly better than non-reliant firms in both the access and non-access regions. The authors also pointed out that this analysis ignored crowding-out effects whereby the entry of new firms reduced employment and profits in existing firms. The results also highlighted the fact that merely having access to electricity did not mean that all businesses would choose to be connected. There were a substantial number of pre-existing and newly created firms in the electrified villages that were unconnected for a number of years after the date of village electrification.

The validity of the approach depends on the specification and performance of the probit model. The explanatory variables should not be influenced by the connection status, but should affect the decision to connect. The better the goodness of fit of the probit, the more likely the selection criterion used (predicted outcome greater than 0.5) will be accurate. The pseudo R^2 was 0.24, which generally indicates a satisfactory goodness-of-fit. The variables selected—the value of investment used for firm creation and the age of the entrepreneur—are likely to affect the decision to connect and the level of profits, but it is less clear that they are not influenced by the level of profits. For firms that had been established before access became available the level of investment to create the firm is clearly independent of connection, but for firms formed after access became available the level of investment used to establish the firm may have been affected by the existence of access—households benefitting from connection could have saved and borrowed more, creating a causal link between connection and profits. Since nearly as many connected firms were created after access became available as existed prior to access, this possibility weakens the robustness of the results. The selection of villages for comparability between the two groups, based on a number of criteria, gives some reassurance that results have not been biased by selecting villages where other important factors would be at work in determining profits for the access region relative to the non-access region.

Grogan and Sadanand (2013)

These authors focused on the allocation of time to various activities and how this was affected by the electrification status of the household. Starting from Gronau's theory of the allocation of time (Gronau 1977) they noted that electrification has two effects on the budget frontier for goods and leisure. There is an increase in productivity made possible by the use of electric appliances, but this alone would imply that women (assuming they are the appliance users) substitute out of work, increasing the time they spend on leisure. However, electrification can also increase time allocated to work by increasing effective hours available for work. Data on rural households taken from a LSMS carried out in Nicaragua in 1998 was used to measure the difference in time allocation between households with and without electricity. Simple comparisons between such households showed that 23 percent of women and 97 percent of men in households without electricity had some employment outside the home, while 41 percent of women and 93 percent of men in households with electricity had some employment outside of the home. For women this appeared to suggest that electrification significantly increased the time spent working outside the home and hence cash income. However, a simple comparison of mean effects for the two groups may be biased if those chosen to be offered electrification, or who chose to accept electrification if offered, had a greater opportunity to work and hence higher income.

The authors first estimated a model in which the time allocated to each of five activities—family agriculture, family non-agriculture, salaried work, cooking, and firewood collection—was related to a number of household-level explanatory variables (age, education, local birth, number of children, possession of water pipe, and dirt floor), the electrification status of the household, the distance to a highway, and county-level fixed effects. A Tobit model—a statistical model in which the outcome variable (time allocated to the above five activities in this case) is non-negative—was used and the results in terms of the change in time allocations are shown in Table 11.

Table 11: Electrification and change in time use in rural Nicaragua (minutes/day)

Activity	Women	Men
Family agriculture	-317*	-175*
Family non-agriculture	68*	201
Salaried work	242*	313*
Cooking	-7	-2
Firewood collection	-44	-65*

Source: Grogan and Sadanand 2012.

Significance level: * = 5%.

For both women and men connection to the grid was associated with a large reduction in time spent on family agriculture, and a large increase in salaried work. Time spent on collecting firewood decreased significantly for men but not women, but the change was small compared to the shift out of domestic agriculture and into salaried employment. The estimation of this Tobit function did not make allowance for the possibility that the connection to the grid is endogenous with respect to time spent on the various activities. For example, households with a large amount of salaried work and much less time spent on family agriculture may have been more likely to connect, and this is a more plausible relationship than

one in which connection enables households to increase time spent on salaried work and reduce time spent on agriculture, while making only a small change in time spent on firewood collection.

To allow for the possibility of simultaneity between electrification and employment, the authors constructed IVs for the electrification status of the household in 2005. For each municipality the population density in 1971 and the mean slope gradient of the land were constructed. The higher the population density the lower the costs of extending the grid, while the steeper the slope the greater the costs of grid extension. The assumption is that both variables are correlated with the electrification status of a household but not with the household's income or its choice of time allocation. To account for the possibility that land gradient might reflect agricultural productivity (which would have employment opportunity implications) and population density non-farm employment opportunities and corresponding wage levels, the study included additional explanatory variables intended to capture local labor market conditions, such as the mean monthly earnings of rural males in the municipality.

The two instruments are plausibly related to the decision to provide access to the village. It is, however, less clear if they capture the factors determining a household's decision to connect. If all households in a selected location were automatically connected, the instruments could plausibly be assumed to be independent of household characteristics and correlated with the household's electrification status. But if households had to pay for connection, as was the case in Nicaragua, those with electricity would tend to have been better off and likely to have a higher proportion of women working than those without a connection. The instruments selected do not reflect factors influencing a household's decision to connect.

A recursive bivariate probit model—in which the propensity of an individual to undertake paid work is a function of explanatory variables including the electrification status of the household, and the latter in turn is estimated using an equation containing the two instruments—had a statistically significant coefficient associated with electrification but only for women. Women were 23 percent more likely to work if the household was electrified, but there was no significant effect for the likelihood that men would undertake more paid work.

Two factors limit the robustness of the results obtained. First, the choice of IVs did not address the possibility that households chose to be connected because of their income or their existing time allocation, thus leaving the possibility of endogeneity. Second, the number of explanatory variables was much smaller than in other studies, leaving open the possibility of omitted variable bias.

Kumar and Rauniyar (2011)

The authors estimated the effects of electrification on income and education on rural households in Bhutan in 2010, allowing for observed and unobserved selection biases. To address the possibility of high-income households preferentially selecting electrification, the study adopted a PSM approach using data from certain villages that had been electrified and other villages that had not been electrified. In their description of the data the authors did not distinguish between access and connection and used the term “access” as synonymous with connection. A logit model—which is substantially similar to probit, with a binary outcome variable, the electrification status of the household, in this case—was constructed, where explanatory variables were selected under the assumption that they would affect the decision to have connection and the outcome of that decision, but that they were not affected by connection. These included household-related variables such as gender, household size, age and marital status of household

head, and amount of land owned, and two village-related variables, village population and the distance to the nearest dzonkhag (district) headquarter. Based on the estimated logit model, a number of different approaches to PSM analysis were carried out for different outcome variables, through which similar households in villages with and without electricity were compared. Total income and farm income were not significantly affected by electrification, but non-farm income was significantly higher where the household was electrified. The number of years of school and study time at home were significantly greater where there was electrification. A weighted least squares technique where the weights were related to the propensity scores from the logit model indicated that electrification increased non-farm income by 63 percent, the time spent in schooling by 0.54 years, and the time spent studying at home by 10 minutes a day.

As with other similar studies the validity of the results rests on the assumptions made about the variables used to estimate the logit relationship. The asset type variables (land ownership, livestock ownership) might have been correlated with income and thus could have influenced the decision to be electrified. They might also have been affected by the effects of electrification, although this is not likely in the short run. In fact, land ownership and livestock ownership were insignificant and their inclusion would not have imparted a bias to the results. Omission of certain village-level variables, such as access to an all-weather road, raises the possibility that the economic outcomes attributed to electrification may have been overstated.

Dinkelman (2011)

This study estimated the effects of rural electrification on household employment growth following a mass roll-out program in South Africa. The government set an aggressive target of electrifying 300,000 households a year in its National Electrification Programme and the national utility, ESKOM, began connecting new households in 1995, fully subsidizing the connections and meeting the annual target in most years. A unique feature of this program was that all households within a community to which access had been extended were automatically connected. However, the community-level selection (that is, which communities to electrify) was not random. The order in which communities were selected may have depended on political pressures and cost considerations. Factors affecting costs of connection include distance of the community from the grid, household density, and land gradient. While the first two may well be influenced by economic opportunities present in the community (and thus would be unsuitable as IVs) the latter is not likely to be directly influenced by employment growth and thus was selected as an IV to circumvent possible endogenous placement bias in the electrification status variable.

A two-wave panel dataset was constructed using 1996 and 2001 census data, and data were aggregated to rural ex-homeland communities,¹³ most having fewer than 900 households. Communities were located within one of ten administrative districts, which tended to have the characteristics of a local labor market. The electrification status of the community at the two dates was identified from ESKOM records. Simple tabulation indicated that in 1996 there were significant differences between communities without electricity and those that had been connected under the National Electrification Programme. About 20 percent of these communities in the sample area were connected between 1996 and 2001.

¹³ The apartheid government established homelands, territories set aside for black South Africans. While they were independent or semi-independent on paper, all homelands were impoverished in practice. KwaZulu was given partial autonomy. Homelands were abolished at the end of apartheid.

The main outcome variable analyzed was the employment-to-population rate of African women and men aged 15 to 59. These rates were very low, and were falling during the period studied in the electrified areas. Unlike rural areas in many other countries, agricultural employment was almost non-existent in the ex-homeland areas studied. The fall in employment observed in electrified areas was not attributed to the presence of electrification, but rather to the broad changes in the South African labor market at that time.

The formal statistical analysis related the change in the community employment rate between the two dates to the change in electrification status (if any), district trend effect (dummy variables), other community-level explanatory variables, and an unobserved factor including the community trend effect. Because the unobserved factor is likely to be correlated with the change in electrification status, OLS estimation would have been produced biased results. Instead, IV estimation based on the average land gradient was used. The group of explanatory variables for the electrification status of a community included household density; fraction of households below the poverty line; distances to the grid, to a road, and to a town; fraction of adult men and women with completed high school certificates; share of female-headed households; and the female/male adult sex ratio.

A first-stage relation of the assignment of electrification to the community during the period indicated that the coefficient for land gradient was significant and negative, even when all the explanatory variables were added. However, the correlation was low, making land gradient a weak instrument. The second-stage equation related the employment rate to the fitted value for electrification status from the first equation and the other explanatory variables. For the male employment rate no significant changes were related to electrification based on the IV equations, but for female employment electrification resulted in a significant increase in employment of about 9 percentage points, or about 30 percent above the baseline value.¹⁴ The female employment rate also rose faster where the poverty rate was higher, and where the adult female/male ratio was higher. The IV estimates of the employment effect were also larger than those obtained from an OLS equation. Had the employment rates in steep and flat areas evolved differently even in the absence of new electricity, the gradient IV would have been invalid. The author tested for this possibility indirectly using data just for households that were electrified prior to 1996. Gradient was found to be unrelated to employment change for this group, adding plausibility to the assumption that it was generally unrelated to employment growth.

The study also reported regressions relating household change in uses of energy following electrification. These results, based on IV estimation, indicated that there was a 63-percentage point increase in the number of households using electricity for lighting, a 28-percentage point decrease in the number using wood for cooking, and a 23-percentage point increase in the number using electricity for cooking. These results were much larger than the values obtained from OLS estimation, which showed that lighting with electricity was estimated to increase by only 22 percentage points and cooking with electricity only 6-percentage points. One issue limits the reliability of these results. The observed large differences between OLS and IV would be expected if the possible endogeneity of the electrification variable would lead to a downward bias, rather than the upward bias expected when the outcome variable is income. The author provides no reason why the OLS estimation should under-estimate the benefits of electrification.

¹⁴ Table 4 of the paper shows that electrification was significant for female employment only at 10% level using a standard confidence interval, but because gradient was a weak instrument, the author used an Anderson-Rubin confidence interval and found that at 5% the coefficient was statistically different from zero.

Household survey data from 1995, 1997, 1999, and 2001 were used to construct aggregate variables for 38 magisterial districts. The variables of interest included employment rates, hourly wages, weekly hours worked, and monthly earnings. These were analyzed using OLS and a fixed-effects panel model in which these variables were related to the variation in the electrification rate within the magisterial district, and trends in explanatory variables in the magisterial districts. In the fixed-effects model the electrification rate was significant and positive only for the effect on male monthly earnings. All other variables, for both men and women, were insignificant. OLS estimates were significant and positive for hours of work for both men and women, as well as for male earnings. The small sample may be one reason why the panel approach was unable to provide significant evidence on the effects of rural electrification.

This study contained valuable insights on how to approach the analysis of the effects of electrification on households, and suggested strong evidence that it increased female employment. The IV chosen (the average land gradient for the community) was plausibly independent of the income level, and results using it were significant and different from those that ignored the possible placement bias. However, two features limit the direct comparison of this study with studies of other countries. First, because all households were connected once access was extended, community-level aggregates were used, but they may smooth out important inter-household differences that would need to be modeled in other studies focusing on household-level data. Second, the peculiar economic situation of Kwazulu-Natal, where there was very little agricultural employment, provides a very different set of opportunities and labor markets from those found in many other rural areas of low-income countries, where farm labor may be dominant.

Khandker, Samad, Ali, and Barnes (2012)

This study focused on the impact of electrification on rural households in India as measured by a human development survey carried out in 2005. As well as household-level data, the survey covered key features of villages in which the households were located. The aim of the study was to measure the benefit of electrification on a variety of outcomes: several measures of income and household expenditures, education, employment, kerosene consumption, time allocated to studying at home and fuel collection, and poverty headcount.

Recognizing that households were not randomly connected¹⁵ when access was available and that villages were not randomly selected for electrification, the authors used a strategy similar to that of Dinkelman (2011). The outcome variables were related to the electrification status of the household, observable household characteristics, unobservable household- and village-level characteristics, and district fixed effects. Ignoring the effects of observed and unobserved characteristics (such as an area's productive potential or a household's ability to perceive returns to investment) on a household's decision to be connected and using an OLS model is likely to yield biased estimates. To address endogeneity, a second equation is used to instrument the household's electrification decision. Electrification status is related to the exogenous variables used in the first stage and to instruments that affect the decision to connect but do not directly affect the outcome variables of interest.

The authors proposed as instruments (i) the proportion of households in the village that have electricity as a measure of peer pressure to be connected; and (ii) the interaction of this variable with household-level variables: amount of agricultural land and the age, sex, and education of the head of the household. The

¹⁵ The authors use "access" to denote that the household is connected to the grid.

authors argued that these instruments were likely to be independent of the outcomes of the target variables (for example, proportion of connected households should not directly affect the income of a given household). Tests of instrument relevance (strength of correlation with the electrification variable) and over-identification (lack of correlation with error terms in the basic treatment equation) were carried out and used by the authors to support the choice of instruments. The principal results are shown in Table 12.

Table 12: Household electrification effects on education, employment, and income

Outcome variable	Impact of electricity connection
School enrollment percentage (5-18 years)	
Boys	0.060*
Girls	0.074*
Study time at home (hours/week)	
Boys	1.36*
Girls	1.58*
Completed schooling (years)	
Boys	0.28*
Girls	0.49*
Log men's labor supply (hours/month)	0.015*
Log women's labor supply (hours/month)	0.17*
Log per capita farm income (rupees/month)	0.40
Log per capita non-farm income (rupees/month)	0.69*
Log per capita total expenditure (rupees/month)	0.18*

Source: Khandker et al. 2012.

Note: Marginal effects are reported. Equations included household variables, village characteristics, and district fixed effects, and estimation was by IVs.

Significance level: * = 5%.

The results support the hypotheses that electrification increased the time spent on acquiring education in terms of enrollment, time studying, and completing schooling. Labor supply of women increased by 17 percent in terms of hours worked, while that for men increased by only 1.5 percent. Non-farm income per capita increased by nearly 70 percent, while farm income did not increase significantly.

The authors emphasized that access to reliable power is important. In their sample, villages without power outages had a connection rate of 81 percent, while villages with more than 20 hours of outages a day had a connection rate of about 40 percent. These results were confirmed in the first-stage regressions for the IVs where the average availability of electricity in hours/day was significant and positively related to the connection variable. However, the variable denoting availability of power was not recorded as being entered into the second stage and its possible effects on the outcome variables were not quantified.

This study, based on a very large sample of households, reported large effects of electrification on income, education, and employment. There are two important issues with the approach used. First, the discussion of the choice of instrument focuses entirely on the decision of the households to connect if there is access (percentage of households in village electrified) and ignores the placement bias that would be caused by preferential assignment of access to villages with higher incomes, more education, and higher employment. If high connection rates were themselves determined by income levels, the instrument would have been endogenous and the resulting estimates biased. The second issue has been

highlighted by subsequent work by van de Walle et al. (2013). These authors argued that when a neighboring household is connected, that could bring benefits to an unconnected household in the same community. The greater the number of households connected in a village, the more likely that unconnected households have a connected neighbor and the higher would be the benefits of the average unconnected household. The marginal benefits of connection to a connected household are then reduced. These two issues suggest that the estimated effects of electrification may be biased upwards. A further problem with interpretation is that the full list of exogenous (explanatory) variables used in the IV estimation was not provided, but only examples of types of variable. Some key variables, such as the existence of paved roads, were included, but not all coefficients and standard errors were reported. This illustrates the lack of emphasis on reaching a common understanding on those factors apart from electrification that influence the outcome variables.

Van de Walle, Ravallion, Mendiratta, and Koolwal (2013)

This study used panel data from the 1981/82 and 1998/99 Rural Economic and Demographic Surveys of India. These surveys provided information on education, labor supply, consumption expenditure, and household characteristics. It was possible to construct an indicator variable of whether the household was connected to the grid, and the surveys also collected village-level data on community access to facilities and infrastructure, wages, consumption, land ownership, crop yield, and population characteristics, and information on the year in which electricity access for the village became available.

Using an extension of a time allocation model, the study distinguished between internal benefits to the household from its own electrification, and external benefits from village electrification. The latter included benefits to all households irrespective of whether they were connected (such as public lighting) and benefits to unconnected households depending on whether other households were connected (visiting them to share in lighting, etc.).

The hypothesis that the state of electrification of the village provided benefits to unconnected households required alternative IVs. The state of electrification in the first survey was argued to be exogenous to the change in the electrification status in the 17 intervening years between the two surveys and the ensuing change in outcomes, and this provided a first IV. The authors also argued that access to electricity depends in part on physical proximity to power generating plants, which does not influence outcomes independently of electrification and the other explanatory variables, and this provided a second IV.

A first-stage regression of the change in the state of electrification on household-level explanatory variables, village characteristics, the IVs, and district fixed effects was used to feed into the second-stage regressions that related the various outcome variables to the explanatory variables using IV estimation. Some of the main results are shown in Table 13.

Table 13: Impacts of household and village electrification on consumption, labor supply, and schooling (rural households in India)

Outcomes	Change in household electrification	Years of village electrification times household not electrified
Total consumption expenditure per capita (log)	0.067*	0.010*

Days of regular wage work of women (per year)	-4.72	0.22
Days of regular wage work of men (per year)	16.60*	0.88
Days of casual wage work of women (per year)	6.12	0.01
Days of casual wage work of men (per year)	-10.42	-1.14
Share of children 5-18 in school	0.082*	0.001
Share of girls 5-18 in school	0.094*	0.008
Share of boys 5-18 in school	0.073	-0.013
Girls mean school years as share of maximum possible years	0.092*	0.005
Boys mean school years as share of maximum possible years	0.002	-0.004

Source: Van de Walle et al. 2013.

Estimation using IV and including controls and district fixed effects. The coefficient associated with the years of electrification multiplying a 1/0 dummy for unconnected households gives the impact of village electrification on the outcome variables for unconnected households.

Significance level: * = 5%.

The surveys did not provide data on income, but aggregate consumption was estimated to have increased by 7 percent as a result of electrification. This value was much lower than the 11 percent estimated by OLS, suggesting a sizeable bias due to endogenous acquisition of electricity by more wealthy families. A second and important result was that for households without electricity there was an annualized gain in consumption expenditure of 1 percent following electricity becoming available in the village. These results were contrasted with those of Khandker et al. (2012) who also analyzed rural households in India; their model estimated that the effect on consumer expenditure of electrification by household acquiring a connection was about 18 percent. Van de Walle et al. argued that this large difference was in part due to the choice of IVs and the inclusion of an effect for unconnected households in a village with access. They tested this conjecture by re-estimating their own model using an IV based on the proportion of households that were connected. This yielded a negative but insignificant coefficient on total consumption expenditure.¹⁶ The authors concluded that the choice of instrument is crucial—the proportion of connected households can be endogenous and correlated with income and total consumption expenditure, thus rendering the exclusion restriction invalid.

The effects of electrification on labor supply indicated that only regular work days for men increased significantly (17 days per year), while female labor supply for regular and for part-time work did not change significantly. For schooling there was no significant change for boys, but for girls the enrollment rate rose by about 9 percent and the length of time that girls stayed at school also increased significantly.

Overall this study made a strong case that access to electricity in a village has benefits for those with and without connections. The estimation of the benefits for those connected was sensitive to the choice of IVs used to avoid selection and placement bias in the relation between income (consumption expenditure) and connection to the grid.

A further difference between van de Walle et al. (2013) and Khandker et al. (2012) was the range of explanatory variables in both the first- and second-stage equations. The former used about 60 variables

¹⁶ Van de Walle et al. also discussed the impact of omitting district fixed effects, but the cited version of Khandker et al. explicitly included these.

(excluding the district-level fixed effects), while the latter mentioned about 20 variables (excluding the district-level fixed effects). As van de Walle et al. note, more attention could be paid to the selection of explanatory variables once there is a firmer understanding of the nature of the links between electrification and the outcome variables.

Khandker, Barnes, and Samad (2013)

This study used panel data on 1,120 rural households in Vietnam carried out in 2002 and 2005. Because of the rapid pace of the electrification program, the percentage of households connected in the sample rose from 26 percent to 80 percent in just three years, and all 42 communes covered in the sample had access by 2005. An important aspect of the Vietnam power supply was its reliability—on average power was available in rural areas for 23 hours a day, and there were only two days of power failure a month.

The approach used to evaluate the income and education benefits was to start from a model in which benefits at a given time depended on observed household characteristics, observed commune characteristics, the electrification status of the commune, the electrification status of the household, and unobserved characteristics. This structure is rich enough to distinguish benefits to an unconnected household in a commune with access from benefits to a connected household or to a household in a commune with no access. However, taking data only from the two years does not distinguish between benefits that accrue immediately on electrification from those that occur after a certain passage of time. For example a household electrified in 2005 (the second of the panel years) might not see the full income benefits of electrification for several years.

As with other studies, the problem is that the likelihood of a commune to be electrified, or a household in a commune with access to be electrified, depends on income, producing a simultaneous relation between income and electricity connection. The authors address potential endogeneity by adopting a difference equation specification based on the changes between the initial and final dates of the panel data. Assuming that the factors that influence a particular commune or household to connect at a given time remain the same over the period of the panel, the change in these factors would be zero for the commune or household in question and the bias from omitting these unobserved factors would disappear. It is possible that this heterogeneity is time-variant, and the authors allow for such possibility by including observed characteristics from the initial survey year. Such an approach is widely used, but is perhaps less robust when the years of the panel surveys are very close, as is the case in this study of Vietnam.

The model used about 20 household characteristics (including age, education, dependency ratio, landholding, non-land assets of various types, livestock assets, and employment status) and a few commune-level variables (motorable roads, mobile phone tower, prices of common food items). Results for income and consumer expenditure from the time-invariant and time-variant models are shown in Table 14. The model with time-invariant heterogeneity indicates that household electrification increased total income by about 21 percent at 5-percent significance, but that no other form of income or consumer expenditure showed significant increases. This is in contrast to other studies that found non-farm income to increase with electrification.

Table 14: Household fixed effects estimates of rural electrification impacts on household economic outcomes in Vietnam (‘000 Vietnamese dong per capita per year)

Electrification status	Log total income	Log total nonfarm income	Log total consumption expenditure
Model 1: time-invariant heterogeneity			
Household connection	0.21*	0.29	0.13
Commune connection	-0.05	-0.17	0.18
Model 2: time-variant heterogeneity			
Household connection	0.28*	0.28	0.23*
Commune connection	0.10	0.03	0.48*

Source: Khandker, Barnes, and Samad 2013.

Significance level: * = 5%.

The model with time-variant heterogeneity indicated a larger and statistically significant effect on total income for household connection, but commune connection was still insignificant for all categories of income except farm income (not shown in Table 14). Consumption expenditure showed a statistically significant increase for household connection and a larger and significant increase for commune connection. Commune connection was strong and statistically significant for total consumption expenditure, but in the absence of significant effects on total income this cannot be attributed to externalities providing opportunities for improved income generation.

Tests on education outcome variables were carried out in a similar fashion using both the time-invariant and time-variant approaches. The results shown in Table 15 indicated that school enrollment rates for girls were affected by household connection, but that commune connection had no significant effect. The number of completed school years showed significant commune effects for boys and girls in the time-variant model, and significant effects for boys of household connection. When the time-invariant model was used there was a significant household connection effect for girls. If a 10-percent significance test were used, household grid connection was significant for all categories in both models except for girls' completion of school years, but commune connection was still insignificant in the time-invariant model for both boys and girls.

Table 15: Household fixed-effects estimates of rural electrification impacts on educational outcomes in Vietnam (ages 5–18)

Electrification variable	School enrollment rate		Completed school years	
	Boys	Girls	Boys	Girls
Model 1: time-invariant heterogeneity				
Household grid connection	0.082	0.095*	0.163	0.076*
Commune grid connection	0.032	0.047	0.393*	0.251
Model 2: time-variant heterogeneity				
Household grid connection	0.063*	0.090*	0.116*	0.031
Commune grid connection	0.034	-0.069	0.131*	0.898*

Source: Khandker, Barnes, and Samad 2013.

Significance level: * = 5%.

Non-electric household energy

In many developing countries cooking uses firewood and other forms of biomass, especially in rural areas. Although there are markets for charcoal or firewood, the fuel is often collected. Collection can be time intensive and is carried out predominantly by women and children, thus placing a constraint on the amount of time available for work and education, respectively. Cooking with biomass, particularly when carried out indoors, is also associated with high levels of mortality and morbidity caused by the inhalation of fine particulate matter.

Many studies have discussed these issues and have urged policies to find ways of encouraging households to switch their cooking fuel or to improve their cooking practices. The hypothesis is that a fuel switch would lead to increased income as women switch from collecting firewood to employment, to improved education for those children who are similarly collecting firewood and who will be able to have more time to study for longer time at home, and to improved health as the rates of mortality and morbidity decline. The quantification of these effects requires careful econometric/statistical modeling for establishing causality. The literature on the impact of fuel switching on time use and female earnings is very thin, and it is useful to review a parallel study on water collection.

The link between indoor cooking with solid fuels and increased mortality and morbidity has been investigated at a household level and at a country level. Numerous studies have linked household air pollution to various illnesses: lower respiratory infections, ischaemic heart disease, cerebrovascular disease, chronic obstructive pulmonary disease, cataracts, and trachea, bronchus, and lung cancers (Lim et al. 2012). At the national and global levels, the numbers of premature deaths and illness episodes have been modeled and calculated by modeling ambient concentrations of pollutants and applying modeled dose-response relationships. Some studies have measured ambient concentrations and recorded health symptoms to correlate them. Examples of both approaches are described below.

Dinkelman (2011)

Dinkelman (2011) did not have data on time allocated to various activities, but did have data on whether the household used electricity for lighting, electricity for cooking, or wood for cooking. The change in the proportions of households in a community that experienced each of these outcomes was regressed on the electrification variable and other explanatory (control) variables. IV estimation was used to avoid possible placement bias. The results indicated large shifts toward using electricity. Use of electric lighting rose by 63 percentage points, wood use for cooking fell by 27 percentage points, and cooking with electricity rose by 23 percentage points. Combining these results with the previously highlighted finding that female employment rose following electrification suggests that electrification affects the rural labor market by freeing women's time from firewood collection and releasing them for employment. However, this cannot be taken to be a necessary outcome of rural electrification. It is widely recognized that even after being connected to the grid households often still cook with firewood and fuel collection remains an important activity, making time savings relatively small. Another factor is that employment depends on labor market opportunities, which can be scarce in many areas where fuel collection is prevalent.

Koolwal and van de Walle (2010)

This study focused on the effects of connection to water supplies on households in eight countries. The relevance of this study is the potential impact of freed-up time from not having to collect water. Such time savings are analogous to those from fuel switching (from firewood to a purchased fuel) or from reduced firewood consumption (through stove efficiency improvement) requiring less fuel collection. Considerable care was taken to deal with the endogeneity of households' decision to connect to water supply. The results showed that for female labor there was no evidence that connection to water infrastructure led to an increase in female participation in income-earning, market-based activities. However, there was some evidence that enrollment in school increased for both boys and girls in Morocco, Nepal, Pakistan, and the Republic of Yemen. There were also improvements in child health as measured by height-for-age scores for girls in the Republic of Yemen and weight-for-age scores in Malawi.

Lim et al. (2012)

This large study calculated the burden of disease for 67 risk factors using data from 187 countries aggregated into 21 regions for 1990, 2005, and 2010. The burden of disease was measured by mortality (deaths per year) and morbidity (DALYs). Data were presented for men and women separately, 20 age groups, and the aggregate population.

The risk factor relevant to the present literature review is that of household air pollution from the use of solid fuels for cooking and heating. The outcomes that were related to the exposure to this risk factor were lower respiratory infections; trachea, bronchus and lung cancers; ischemic heart disease; cerebrovascular disease; and chronic obstructive pulmonary disease. Exposure-response functions were used to link household air pollution to these health effects.

The study showed that household pollution from solid fuel use in 2010 was the fourth most serious global cause of the burden of disease, accounting for 3.5 million deaths and 111 million DALYs. In South Asia and most of Africa it was the most serious or second most serious risk factor. Further insights were obtained by an analysis of age-specific burdens, and for the changing pattern of the burden over the period between 1990 and 2010. The study also noted that household air pollution is an important contributor to ambient (outdoor) pollution and might account for 16 percent of the global burden from ambient particulate matter pollution.

Chafe et al. (2014)

The purpose of this study was to estimate country by country and at a global level how much household air pollution from household cooking, as measured by $PM_{2.5}$, contributed to ambient air pollution and hence to the burden of disease. Models were used to calculate the fraction of $PM_{2.5}$ household emissions attributable to cooking, and then the fraction of ambient $PM_{2.5}$ emissions due to household emissions for 1990, 2005, and 2010 for 170 countries. The study did not include emissions from heating with solid fuels.

Globally, about 12 percent of population-exposure weighted average ambient $PM_{2.5}$ was attributable to household use of solid fuel cooking, and in Sub-Saharan Africa the share was as high as 37 percent in 2010. South Asia had a share of 26 percent, but the overall level of ambient $PM_{2.5}$ was far higher.

Using published estimates of the burden of disease due to ambient PM_{2.5} levels and scaling by the share of household emissions from cooking in ambient emissions yielded estimates of the burden of disease from ambient pollution due to indoor household cooking with solid fuels. The study estimated that the burden of disease due to this risk factor resulted in 370,000 deaths and 9.9 million DALYs in 2010. The vast majority of deaths occurred in South Asia and East Asia, while the number of deaths in sub-Saharan Africa was comparatively small. Between 2005 and 2010, the number of deaths and the morbidity actually increased in Sub-Saharan Africa, while declining in East Asia. These figures can be combined with the results obtained by Lim et al. (2012) to give the totals from indoor and ambient pollution due to the household use of solid fuels.

The authors noted the difficulties with obtaining data on a standardized basis and point to two issues that may have led to an underestimate of the effects on health. First, household emissions vary seasonally, resulting in a higher mortality risk during the heating season, as noted in China. Second, household emissions are likely to have a higher average intake fraction than most forms of ambient air pollution because of the long hours spent in close proximity to emission sources.

Ezzati and Kammen (2002)

The authors carried out continuous real-time monitoring of indoor air pollution of 55 households, randomly selected among villages and fuel types, over a 200-day period in Kenya. The study measured PM₁₀ at different locations within the dwelling and constructed profiles of exposure for each individual based on the combination of time-activity budgets, spatial dispersion, and daily and day-to-day exposure variability. This allowed for the temporal variability of stove emissions during the day and the closeness of certain family members to the stove. These data were used to calculate various exposure indices.

Health data for the same households were collected via a series of visits by trained nurses at weekly intervals over a two-year period. Data on symptoms and treatments were recorded and the study calculated the fraction of time individuals were diagnosed with ARI.

The study then estimated a linear risk model for the relation between the ARI rate and the exposure index, controlling for other explanatory variables such as age, gender, type of village agriculture, smoking, and the number of people in the household. Allowance was made for the effects of health treatments provided during the collection of data. ARI was found to be an increasing function of the average daily exposure to PM₁₀, but the rate of increase declined for exposure above 1000–2000 micrograms per cubic meter.

The study was able to investigate possible exposure reduction as a result of four environmental interventions:

- Changing fuel from wood to charcoal
- Changing stove technology from traditional open fire to improved (ceramic) woodstove
- Changing location of cooking from inside to outside the house
- Changing location and type of stove together.

Based on the survey evidence the authors were able to calculate the indices of exposure under these alternative scenarios by age group and gender. They found that the largest relative reductions in exposure occurred for adult and young women, but that the only intervention that would reduce exposure to levels of the same order of magnitude as international standards was the switch from wood to charcoal.

Combining these estimated reductions in exposure with the estimated exposure-response relationships provided estimates of the health effects of these different interventions. For example, the introduction of a ceramic woodstove, not requiring any shift in fuel, was estimated to reduce ARI by 25 percent for children under 4 years of age, while the combination of cooking outside with an improved stove was estimated to reduce ARI by 65 percent for females between the age of 5 and 14.

The study provided valuable insights on methodology linking household air pollution to health effects and derived estimates of health benefits linked to difference policy interventions. In order to compare the economic effectiveness of alternative policies it would be necessary not only to estimate the costs of the different policies, but also the economic value of the different health outcomes. Two points should be noted about the study. First, the sample is very small, so that once the various explanatory variables are added to the risk model the precision of the exposure-response relation is limited. Second, the study concentrated on the measurement of PM₁₀, while recent concern has focused on PM_{2.5} considered to have a much more damaging effect on health. This may constitute a measurement error that could lead to biased estimates and incorrect policy evaluations.

Pope, Díaz, Smith-Sivertsen, Lie, Bakke, Balmes, Smith, and Bruce (2015)

The study was carried out over two years from 2002 to 2004 in rural Guatemala. The study compared respiratory outcomes among 504 women from indigenous communities using improved chimney stoves versus traditional cookstoves burning firewood in a randomized trial. The analysis included 456 women with data from post-intervention surveys, including interviews at 6, 12, and 18 months (respiratory symptoms), and spirometry and carbon monoxide in exhaled breath measurements. Personal carbon monoxide was measured at variable times during the study and associations between carbon monoxide concentrations and respiratory health were estimated. The study did not measure particulate matter, which is more resource-intensive to monitor. Respiratory symptoms (cough, phlegm, wheeze, or chest tightness) during the previous 6 months were positively associated with breath carbon monoxide measured at the same time of symptom reporting and with average personal concentrations during the follow-up period. The authors point to several limitations of the study and caution care in interpreting the results, but conclude that the results provide further support for the effects of exposures to household air pollution on airway inflammation.

Assessment

A recent group of studies has estimated the effects of electrification on household incomes, employment, and education undertaken, while allowing for the possibility that the connection status of the households is endogenous. Earlier studies failed to take this endogeneity into account and hence their results may have been biased, overstating the benefits of electrification.

Although these recent studies have noted that there are many channels through which electrification could increase income (and other outcome variables of interest), they generally do not attempt to estimate the contribution of each of these channels, but rather the total impact on these outcome variables.

All three approaches described above need to make prior assumptions that allow the estimation technique to avoid the bias due to endogeneity of the decision to be electrified, and if these assumptions are incorrect then the resulting estimates continue to be biased. For example, the review of Khandker et al.

(2012) by van de Walle et al. (2013) showed that changing the assumptions about the endogeneity of different possible instruments can make a very large difference to the results obtained. It is therefore important to discuss these assumptions in detail and justify their use.

A feature of these studies based on household survey data is that there are a large number of exogenous variables that might be significant contributors to the outcome of the variable under investigation. Studies in different countries may have access to different lists of such variables, making it difficult to make a direct comparison of studies in different countries and carried out at different times. Under these circumstances, different studies would not be expected to arrive at similar quantitative conclusions about the effect of electrification on incomes. The most that can be expected is that similar qualitative results be obtained if similar approaches are used.

The review of the literature in this chapter suggests that electrification has beneficial effects on income (or consumption), on employment, and on education. However, there is less agreement on exactly which components of these outcome variables are significantly influenced by electrification. For example, some studies have found that non-farm income increased significantly, while others using a similar—but not identical—approach found no evidence that non-farm income increased. Enrollment rates in schools were significant in some studies but not in others; female employment rates increased significantly in certain studies, but not in all. Three factors may be leading to these differences:

- The estimation technique and assumptions underpinning it may be different between studies.
- The list of variables included to explain the outcome variables may differ between studies.
- Countries differ in their specific situation with respect to the outcome variables.

Comparisons between studies need to bear in mind all three of these factors before making assessments of the inferences to be drawn from them.

Studies linking household air pollution to health effects provide insight at the project level, as in the study for Kenya, and globally. The former type of study provides material for assessing the relative impact of different policy interventions, while the latter can be used to assess the relative importance of the health problem under review. None of the studies discussed the final step mentioned by Duflo et al. (2008) of estimating the economic cost of the health outcomes.

Annex 1: Econometric problems met in evaluation of links between energy and economic benefits

The purpose of this annex is to provide an explanation of certain technical terms and arguments used in the evaluation of different studies using econometric techniques. It is intended for readers not entirely familiar with various techniques in econometrics, and avoids a precise mathematical formulation of the issues in favor of descriptions that are not precise in a formal sense but that do convey the general sense of the approaches used in formal mathematical discussions.

Ordinary least squares, bias, and instrumental variables

The starting point for many estimates of the relationship between energy use and various economic outputs is a linear regression model in which the outcome (left-hand side) variable (such as income, employment, or education completed) is assumed to be determined by a the sum of various explanatory (right-hand-side or regressor) variables (such as electrification status, age, gender) each multiplied by its coefficient (parameter). The formulation of the linear model recognizes that the outcome variable will be determined not only by the specified explanatory variables but also by an unknown and unobserved error term.

The method of OLS estimates the values of the coefficients that minimize the aggregate sum of differences between the outcome variable data points and the values of the outcome variable that would be predicted by those estimated coefficients multiplied by the explanatory variables; equivalently it minimizes the estimates of the error terms (the residuals).

OLS can be shown to have a number of desirable properties if certain assumptions hold. The key property is that of unbiasedness—the technique gives values that on average would equal the true coefficients if the same model were to be estimated with the same explanatory variables but with repeated samples of the error terms. Related to this concept is that of consistency—the estimation technique gives a value that approaches the true value as the number of observations becomes sufficiently large.

Properties of unbiasedness or consistency require that the explanatory variables included in the model specification are independent of (in other words uncorrelated with) the error term in the underlying model. The stronger the correlation between the explanatory variable and the error term, the greater would be the bias (the difference between the true value of the coefficient and the average of estimated values) of the estimates for the coefficients of the included explanatory variables. Three conditions give rise to a lack of independence between the explanatory variables and the error term:

1. Omitted variables. The omission from the model specified of important explanatory variables means that these are in fact included in the error term. Where such variables are themselves correlated with the explanatory variables included in the regression model, OLS will be biased and inconsistent. The stronger the correlation between the omitted variable and the included explanatory variable the larger will be the bias.
2. Errors in variables. Where one or more explanatory variable is measured with error it is possible that the OLS estimates will be biased. The existence and magnitude of the bias depends on the

nature of the measurement error. Where the measurement error is uncorrelated with the true value of the explanatory variable it will be correlated with the measured value and this will lead to bias for OLS estimation. In this case the larger the average measurement error the greater will be the bias.

3. Simultaneity. If an explanatory variable is partly determined by the outcome variable—as, for example, when prices and quantities of a market are linked through both the supply and demand functions—then that variable will be correlated with the error term, and the explanatory variable is said to be endogenous. An OLS estimate of the coefficient of the explanatory variable will produce a weighted average of the two links between the outcome variable and the explanatory variable, rather than an unbiased estimate of the “one-way” causal link from explanatory variable to outcome variable.

The presence of any of these problems leads to unreliable estimates when OLS is used, and econometricians have developed tools to avoid such problems. The first step is to use economic theory to check for the potential presence of these problems. If a possible problem is identified, various options for improved estimation are available.

The failure to include important explanatory variables in an estimated equation is countered by initially including the most plausible determinants of a given outcome variable. Significance testing can reveal those explanatory variables for which the estimated coefficient is not significantly different from zero (using standard hypothesis testing techniques) and, if necessary, the model can be re-estimated omitting such variables that appear not to be significant. Models of household behavior based on survey data are able to include a large number of socio-economic characteristics in order to include important determinants of the outcome variable under consideration.

Where plausible explanatory variables are omitted from the beginning, there must be doubt about the unbiasedness and reliability of the coefficients that are estimated. This problem is demonstrated in the literature testing for the links between energy and GDP via the production function, and energy and GDP via a demand function. The former would suggest that other factor inputs (labor and capital) should also be included in the production function, while the latter would suggest that the relative price of energy should also be included in the demand function. Models that recognized the possibility of the simultaneous existence of both links could have been expected to include both the factor inputs and the relative energy price. Where one or other was omitted, the results cannot be relied upon to give accurate estimates of the significance of the two causal links.

Errors in variables are most likely to exist in survey data, where individual household responses may be colored by imperfect recall or by a desire to exaggerate problems that the survey is addressing. Surveys are usually designed to reduce or eliminate such problems but when they do exist and are recognized as potentially present, then it is possible to use the technique of instrumental variables (IV) estimation, providing that a suitable instrument can be identified. The instrument should be correlated with the “true” value of the variable in question, but not with the measurement error. If economic theory can identify such an IV, a two-stage procedure can be used. First regress (by OLS) the problematic explanatory variable on the IV, and use the estimated coefficient from this relation to construct a “predicted” value of the explanatory variable (the estimated coefficient times the IV). At the second stage regress the outcome variable on this predicted value to obtain a consistent estimate of the coefficient linking the outcome

variable and the explanatory variable. The idea behind this approach is to construct a variable to represent that part of the explanatory variable not measured with error.

The problem of simultaneity is met in many economic contexts—the explanatory variable linked to the outcome variable of interest is itself in part determined by the outcome variable and is then endogenous (determined within) to the model being investigated. OLS will give biased estimates of the coefficient of interest and can overstate the benefits of policies to increase the explanatory variable. Instrumental variables, if such can be identified, offer a solution to this problem. The case of rural electrification illustrates the principles involved.

It is hypothesized that electrification, together with a number of other factors, increases the incomes of rural households. A regression model would relate income to the electrification status of the households in the survey and to the values of the other factors. However, analysis of government electrification policies may indicate that the limited amount of rural access has been targeted to those villages where the average income was highest, because in the absence of complete connection subsidies the highest connection rates (and lowest costs of electrification per household) will be found in villages with higher incomes. Further, within villages, households with higher incomes will be those most likely to pay the connection fee once access is provided. The connection status of the households is endogenous (determined within the model) in that not only does electrification help to determine the level of income, but the level of income also determines whether the household is connected if there is access. Hence, even if connection to the grid made no difference to income, there would still be a correlation between those with electricity and their income levels. OLS carried out on this model would overstate the benefits from electrification on income.

The solution to this problem is to find an IV that is related to the electrification status of the households but is not determined by the income level of the households—the IV is exogenous with respect to the basic model. A number of suggestions have been made in this context. For example, the gradient of the land around the village is likely to affect whether the utility has provided access (steep gradients cost more to supply and would be less likely to be supplied in the earlier stages of an electrification program) but would not be affected by the level of income in the village. Or, the distance to the nearest generation supply point would give a natural sequence of access, with villages nearest to supply points being the first to be given access, and again this is likely to be independent of the level of income in the village.

If such instruments are available and are plausibly argued to be independent of the level of incomes in the village, IV estimation can be used. In the first stage the electrification status of the households is regressed on the IV(s) and the other exogenous variables in the model. The coefficients obtained in this equation are multiplied by the explanatory variables used (including the IV) to give a “fitted” or predicted value for the electrification variable. A second-stage regression of the outcome variable on the fitted value of the electrification variable and all other explanatory variables is carried out to yield an estimate of the effect of electrification on income. Provided the assumption about the independence of the IV is correct, this approach provides a consistent estimate of the required coefficient.

Probit and logit models

This class of models is concerned with situations where the observations on the response variable are binary—that is, they take a “yes/no” or a 1/0 form. An example of such a variable would be given by the answer to the survey question, “do you possess a backup generator?” The model is designed to estimate

the importance of the various factors that are thought to explain the ownership of backup generation. The model assumes that there is a unobserved “latent” variable that is determined by an observed variable(s) multiplied by a coefficient(s) plus an unobserved error term. If the latent variable is greater than a threshold value, then the firm chooses to own a backup generator, while if the latent variable is less than the threshold, it will not choose to own backup. The latent variable could be thought of as the *net* costs of outage without backup (that is, losses from not being able to run business, minus wages, electricity costs, and other costs that did not have to be paid). If costs are positive then purchase backup, while if they are negative do nothing. The costs will depend on a number of observed factors, such as the type of product sold, the characteristics of the local market, and the severity of outages. These terms are weighted by coefficients indicating their relative importance. It is the purpose of the study to estimate these coefficients.

The probability that a given firm has backup generation (outcome variable is 1) is then the probability that the error term plus the observed determinant of the choice function is positive (latent variable is positive). This depends on the probability distribution of the errors. The sum of all probabilities up to a given value (the cumulative distribution function) measures the probability that the error term is less than the threshold.

The Probit model hypothesizes that the errors follow a normal distribution so that the probability of observing each outcome can be expressed as a given non-linear function of the coefficient(s) attached to the explanatory variable(s). Maximum likelihood estimation is used to obtain consistent estimates of the coefficient. If the errors are thought to follow a logistic distribution, the non-linear function will be different and maximum likelihood will give different values of the estimated coefficient. This case is referred to as a logit model, and in practice the results from making the two different assumptions are similar.

The Tobit model is closely related to probit models. It assumes the latent variable is determined by an observed variable(s) multiplied by a coefficient(s) plus an error term, and that the observed outcome variable is equal to the latent variable when the latent variable is greater than the threshold value, but when the latent variable is less than the threshold the observed value will equal the threshold value. The latter step introduces the cumulative error probability distribution to allow for all probabilities that the errors will result in the latent variable being less than the threshold. This model could be applied to data in which the survey questionnaire asked what amount of normal power demand could be covered by backup generation. Firms that were above the threshold level would have positive numbers, while those below the threshold would all return zero. Maximum likelihood estimation based on the assumption of an underlying error distribution is used to obtain the coefficient estimates.

Non-stationary data, integration and cointegration

Data sets that are based on time series of observation (such as annual observations) present a particular difficulty for estimation. Many series exhibit growth over time (rather than varying up and down around a mean value) and this growth is often due to some external driving variable. Where this driving variable influences two or more series, it is possible that these series would exhibit a very high correlation between them while there was in fact no direct causal link between them. A regression of one series on the other would appear to be highly significant.

Where the data are dominated by trend movements and do not fluctuate around some constant value, they are said to be non-stationary, and some special estimation procedures have to be used in order to be assured that the results not be spurious.

The first step in this procedure is to check whether the series are stationary or not, and the usual procedure is to carry out unit root tests. If the first difference of a series (current value minus previous value) is stationary then the series has a unit root and is said to be integrated of order one. A variety of such tests have been developed.

It is possible that a linear combination of non-stationary series is itself stationary, implying that there is a long-run relation between the two series, and such series are said to be cointegrated. Several tests of possible cointegration between two series that are integrated of the same order have been developed (Engle-Granger or Johansen tests).

Without needing to go into detail on these testing procedures, their implications for studying the relationships between time series are clear:

- Trend-dominated series can give rise to spurious results and large coefficient bias.
- It is necessary to check first for stationarity of each series in a hypothesized relation.
- If the outcome variable and explanatory variable are integrated of different order (for example, one is stationary and the other has a unit root), there cannot be a meaningful long-run relationship between them.
- If the variables are integrated of the same order, it is necessary to check that they are cointegrated, in which case there can be a meaningful long-term relation between them.

Models built around cointegrated series can also allow for short-run links in which a shock to one series can be immediately transmitted to the other series with further gradual changes, resulting in a long-term relationship between the two.

Panel data

Studies of the energy-growth link and the infrastructure-growth link have used panel data in an attempt to increase the number of observations available for estimation and to broaden the applicability of the results to a wide range of countries. Panel data combines data from several time periods (years or five-year averages in the studies covered in this review) for each of a number of countries. A balanced panel contains observations for every country for the same set of years in each case.

The estimation of a set of panel data raises some issues not normally found with time series on a single country. There can be effects common to a country or common to a year and these can be allowed for using fixed or random effects specifications. These effectively allow the intercept in the model to be different for each country (but the same for each time period) and different for each time period (but the same for each country). Omission of such fixed effects can leave out important sources of variation and lead to biased estimation. Because of the large differences among countries it is to be expected that the error terms corresponding to different countries (and to different time periods) will have substantially different variances, and to obtain the most reliable coefficient estimates this must be allowed for using a technique based on generalized least squares.

A further complication is that the time series elements of the panel may be non-stationary, requiring that cointegration tests to deal with this problem. Where the number of countries is large and the number of time periods is small, this problems is less severe. Dynamic panel analysis also introduces the possibility that outcome variables are partly determined by their own outcomes in earlier periods, and this requires special estimation techniques.

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