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Randomizing the "Last Mile"

A Methodological Note on Using a Voucher-Based Approach to Assess the Impact of Infrastructure Projects

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ABSTRACT

This methodological note discusses the potential and limits of using voucher-based experiments to randomly evaluate the micro-level impact of infrastructures on households' well-being. We argue that such methods are policy relevant, statistically robust, and ethically correct. A number of conditions regarding the vouchers' design and level, as well as allocation methods and household sampling, must be taken into account, however. We illustrate the discussion with an ongoing voucher-based impact evaluation of a rural electrification program in Ethiopia.

Keywords: infrastructure, impact, vouchers, Ethiopia

1. INTRODUCTION

With US$18.7 billion spent in 2004, infrastructure is by far the most important sector in Overseas Development Assistance;[1] yet the actual impact of infrastructures on development outcomes remains unclear. While a few macro-level studies have attempted to establish causal relationships, they usually lack the appropriate data to provide context-specific recommendations for infrastructure expansion strategies.[2] Studies relying on aggregated household data look more promising, although specification issues can lead to contradictory results.[3]

In contrast, micro-level studies can provide specific insights into how households' behavioral responses and local environment can condition the impact of infrastructure on well-being. A number of methodological issues arise from these studies, however. One source of bias relates to cluster-level (community, district, or market) estimates and stems from the fact that infrastructure placement is not done randomly, but rather results from the combination of technical feasibility, cost minimization, social targeting, and political considerations. A simple comparison of outcomes in clusters with and without infrastructure projects would then lead to biased estimates—the *placement* bias.

Several recent papers have made important progress in accounting for placement biases, using various empirical strategies. Some use instrumental variables based on geographic characteristics that determine the suitability of terrains for a given infrastructure (such as Duflo and Pande (2007) for dams, Dinkelman (2008) for electric lines, and Batzilis et al. (2010) for cellular phone towers); others rely on propensity score matching estimators combined with difference-in-difference approaches to control for observable and time-invariant unobservable determinants of placement and outcomes (van de Walle and Mu 2007 for rural roads, for example). Still others use the progressive rollout of infrastructure programs to compare areas already treated to nearby areas to be served later (see Jensen 2007; Aker 2008; and Goyal (2010) for cell phones).[4]

A second source of bias relates to individual-level estimates and is linked to the impact conditioned on an individual's actual use of the infrastructure service. Among those with access to the service, not all individuals will be placing phone calls or connecting to the electrical grid, as they may not all be able to afford it or expect the same benefits from it. Comparing "users" to "nonusers" may also lead to biased impact estimates—the *self-selection* bias.

Fewer studies have attempted to overcome such biases, despite the importance of assets and related behavior in measuring and understanding infrastructures' impact on well-being. For instance, Jalan and Ravallion (2003) use propensity score matching to show that the effects of piped water on diarrheal disease in children is significantly affected by women's education status, resulting in limited impact of the water infrastructure among poorer households. In another study, Jacoby (2000) uses the distance from market to predict the increased land value implied by rural roads, finding that benefits of roads are neither large nor targeted enough to significantly reduce income inequality.

While they make significant contributions, these approaches depend on the—mostly untestable—conditional independence assumptions that rest at the core of the estimators used. To account for this, one may recommend that fully randomized treatment be implemented at the household level. However, given

[1] Within the World Bank alone, support to infrastructure typically represents 40–50 percent of all engagements (World Bank 2006). In its latest report on infrastructure development in Sub-Saharan Africa, it calls for $930 billion to be invested in the continent's infrastructure over 10 years (World Bank 2009). Finally, according to a recent announcement by its president, Robert B. Zoellick, the World Bank Group plans to increase infrastructure investments to $45 billion over the next three years—an increase of $15 billion over the three years preceding the financial crisis.

[2] See Straub (2008) for a recent review of 64 studies.

[3] See Fay et al. 2005; Ravallion 2007; and Fay et al. (2007) for discussion of a recent controversy.

[4] Another potential source of identification may rely on the quality of the infrastructure being placed. For instance, the study by Olken (2007) investigates how different and randomly allocated corruption monitoring approaches significantly affected the quality of the road being built. This could later be used as a source of exogeneity to assess the effect of road quality on households' and communities' well-being.

the nonrival nature of most infrastructures and their overall known positive effects, randomizing households' participation within a given community would likely raise practical and ethical concerns.

Instead, this note discusses random allocation of encouragements toward use, rather than access. It is often the case that users must cover connection costs to the infrastructure, such as drop-down wires or pipes, meters, and so forth, usually referred to as the *last mile costs*. Voucher approaches can therefore be designed to partially cover these costs, offering several types of advantages. Analytically, the method is akin to instrumental variable approaches, although the satisfaction of an exclusion restriction assumption is built within the design of the study. Ethically, this approach is less exposed to criticism as the project is not denied to anyone, only further encouraged to some. Finally, from a policy perspective, encouragement approaches can be used to investigate adoption behavior and assess optimal levels and targeting for connection subsidies—sometimes called "smart subsidies"(see Barnes and Halpern 2000)—as well as the impact on well-being. While vouchers have been used in other sectors (such as education, food stamps, neighborhood relocation, and lately in fertilizers), to our knowledge, this approach has never been applied to infrastructures in developing countries. This paper discusses the potential and pitfalls of such an experiment. We use an ongoing voucher-based impact evaluation of a rural electrification program in Ethiopia as an illustration. Our hope here is limited to the provision of a methodological discussion that can contribute to ongoing designs of impact evaluations of infrastructure projects. A forthcoming companion paper will discuss the study's obtained results.

2. SETTINGS FOR A VOUCHER-BASED IMPACT EVALUATION OF RURAL ELECTRIFICATION IN ETHIOPIA

In recent years, Ethiopia has engaged in important grid extension programs to raise electrification of rural households from its current rate of 1 percent. Connection costs are prohibitive, however, ranging from US$50–100, in a country where 80 percent of the population lives on less than $2 a day. In 2007, a study on "removing barriers to connection" was planned, part of which relied on a pilot experiment through voucher distribution. The pilot utilized pre- and post-electrification surveys of 20 villages to be electrified by the national electricity company.

The pilot relied on the following conceptual framework. Consider a community where an electrical line has just been installed and where each household is now responsible for covering the cost of the *last mile* (the drop-down wire from the closest pole that holds the main line to the house). We aim to assess the effect of household i's electrical connection, C_i , on a measure of its well-being, Y_i (wealth, for instance). $C_i = 1$ if the household does connect, and $C_i = 0$ otherwise. Because, at any given time, household i cannot be observed both with and without an electrical connection, one is limited to comparing connected households to unconnected ones. Unless accounted for, however, self-selection biases will affect the obtained estimate. For instance, if richer households are more likely to connect given the cost of the last mile, one will likely overstate the effect of electrification on wealth.

Now consider that a number of vouchers are randomly allocated to a subset of households in the community. Vouchers can only be used to partially cover the costs of the last mile. Let $Z_i = 1$ if household i has been assigned a voucher and $Z_i = 0$ otherwise. Because vouchers are only incentives for connection, there may not be a one-to-one relationship between voucher allocation and household i's decision to connect. This imperfect compliance therefore violates the usual *ignorability of treatment* assumption, such that standard average treatment effects may not be estimated. Imbens and Angrist (1994); Angrist and Imbens (1995); and Angrist, Imbens, and Rubin (1996) show, however, that a Local Average Treatment Effect (LATE) equivalent to the instrumental variable estimator may be estimated. This LATE is only identified for the households positively affected by vouchers in their decision to connect (the *compliers*):

$$\text{LATE} = E[Y_i(C=1) - Y_i(C=0)|\text{complier}] = \frac{E[Y_i|Z_i=1]-E[Y_i|Z_i=0]}{E[C_i|Z_i=1]-E[C_i|Z_i=0]} = \beta^{IV}. \quad (1)$$

The LATE is only defined under a set of assumptions. We discuss below the validity of these assumptions and ways to ensure validity in the context set forth above.

Assumption 1

Exclusion restriction: $Y(Z,C) = Y(Z',C), \forall Z, Z' \text{and } \forall C$.

The identification of LATE thus relies on the absence of a relationship between vouchers and outcome apart from the vouchers' effects on connection decisions. If violated, the causal effect of connection on income will not be estimated but rather the combined effect of voucher and connection on income.

Even randomized voucher allocation may not suffice to ensure that Assumption 1 is satisfied if postdistribution allocation can occur. If, for instance, vouchers can be exchanged or sold later, it is likely that households without vouchers but with high expected returns from electricity will be willing to buy vouchers from others; thus, the obtained LATE would be biased correspondingly. To ensure that this is not the case, efforts must be made to ensure that vouchers are in fact nontransferable. To do this, we used a rather complex design involving watermarks, official stamps, and unique serial numbers to reduce the risk of voucher falsification. Clear instructions regarding the nontransferability of the voucher were given both in writing (on the voucher) and verbally at the time of distribution. Further, the voucher recipient's name, national ID number, and address were written on the voucher at the time of distribution.

Even then, postallocation exchange cannot be entirely ruled out. The extent to which this may bias the results can, however, be tested, at least partially. As shown in Angrist, Imbens, and Rubin (1996), biases due to violation of the exclusion restriction are inversely proportional to the *power* of the instrument. By varying the level of vouchers (for example, high incentive, medium incentive, low incentive, and no incentive), one may therefore test for differences in the obtained LATE estimates—a significant difference being indicative of a violation of the exclusion restriction. For this reason, two levels of vouchers were distributed in the Ethiopian study: 10 percent and 20 percent discount.[5]

Assumption 2

$$\textit{Strict monotonicity}: C_i(Z=1) \geq C_i(Z=0), \forall i.$$ [6]

The LATE is only estimated for those households for which the vouchers actually affected the decision to connect—the compliers. In fact, neither those households that would have connected in any case (the *always takers*) nor those voucher recipients who chose not to connect (the *never takers*) contribute to the estimate of the impact. To be interpretable, however, households that have decided *not* to connect *because* they received a voucher—the *defiers*—must not exist.

Although unlikely, the existence of *defiers* should not be entirely ruled out. One could imagine situations in which a social stigma is associated with the use of a voucher or in which suspicion exists regarding the allocation, such that some voucher recipients choose not to connect at all in order to clearly establish their nonutilization of the voucher. Clear and transparent (public) allocation of vouchers is then necessary. In Ethiopia, for instance, a list of all household heads in the village was first established, using official registries; each was assigned a number. On the designated day (announced several days in advance), a public lottery was organized in which one of the attendees was asked to draw blindly, one by one from a container, the number of winning households.

Assumption 3

$$\textit{Stable Treatment Unit Value}: E\left[Y_i \middle| C_i, C_j = 1\right] = E[Y_i|C_i], \forall i, j.$$

If a connection exerts positive (negative) spillovers on a compliant but nonelectrified neighbors' well-being, the LATE will be biased downward (upward). Importantly, these spillovers (or contamination of the control group) can affect either a subset of households or the entire village. Examples of this include effects on others' income via local growth and general equilibrium effects.[7]

The existence and magnitude of such effects is difficult to assess in very small and compact communities, where they may be particularly salient. In larger communities, however, one can test for these effects by assessing whether distance from connected households helps explain evolution of nonconnected households' well-being.[8] For this purpose, a sufficiently large number of households should be sampled in each community so as to ensure that measures of distance to connected neighbors are relatively accurate. Further, GPS coordinates should be collected for each surveyed household in order to enable computation of distance.

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[5] Because of budget constraints, only two levels of vouchers could be utilized, and we chose the most directly policy-relevant voucher, in line with what the Ethiopian power utility foresaw for later policies (see below).

[6] This assumption is sometimes referred to as the no defier assumption.

[7] In fact, one caveat to most randomized evaluations is that they estimate partial equilibrium treatment effects, which may differ from general equilibrium treatment effects (Heckman, Lochner, and Taber 1998).

[8] One could also rely on comparison of the situation of nonconnected households to "similar ones" in nearby and comparable nonelectrified communities. Propensity score matching methods (optimally, combined with a difference-in-difference estimator) may be used for this purpose. However, the placement biases discussed previously would remain an issue.

In the present study, the survey covered 90 households, which represents an average of 10 percent of the population per village and of which a subset received a voucher toward partially covering the connection costs. Household questionnaires included the standard set of demographics and income-consumption expenditure modules, along with specific modules dedicated to energy usage and time allocation and GPS coordinates for each house in order to measure distance between households and identify the potential positive (negative) spillovers.

Assumption 4

$$\text{No interference: } E[C_i|Z_i, C_{-i}] = E[C_i|Z_i], \forall i.$$

A related issue may also arise at the connection stage. If a household's decision to connect is significantly influenced by others' decisions to do so, a situation referred to as *endogenous spillovers* in Manski (1993), the LATE estimator may be biased (Sobel 2006; Rosenbaum 2007). Therefore, one must also assume that voucher recipients' decisions to connect are not influenced by the fact that some of their fellow villagers are willing to do so.

One may decide to control for such biases by including the proportion of households in the vicinity that have chosen to connect among the regressors.[9] As noted by Manski (1993), however, such estimation may suffer from reflection problems, and parameters will not be identified unless further exclusion restrictions can be assumed. Vouchers may help in this regard, using the intensity of voucher recipients in the vicinity as an instrumental variable for C_{-i} (Duflo and Kremer 2003; Angrist and Krueger 2001) This can be further enhanced by varying the intensity of voucher distribution across communities. In the present study, the number of vouchers to be allocated in each village was randomly drawn between a maximum (64) and a minimum (48), such that each of the 20 "treated" villages had a different intensity of vouchers. Once this number was reached, an additional number of households were drawn to obtain the final sample size of 90 households per village.

Further Consideration: Level of Discount

Size of the complier population is significant when estimating the impact of an infrastructure project, as it gives the effectively usable sample size and the study's statistical power thereof. As shown in Imbens and Rubin (1997), it is relatively straightforward to estimate the proportion of compliers relative to always takers and never takers, given the random allocation of vouchers. For the purposes of this study, the proportion of connected households among those who did not receive a voucher is a good approximation of the population of always takers among the voucher recipients—under a no-interference assumption, that is. The proportion of compliers among those voucher recipients who connected, π, can thus be assessed by

$$\pi = \frac{\frac{E[C_i=1,Z_i=1]}{E[C_i=1,Z_i=1]+E[C_i=0,Z_i=1]} - \frac{E[C_i=1,Z_i=0]}{E[C_i=1,Z_i=0]+E[C_i=0,Z_i=0]}}{E[C_i=1,Z_i=1]} \tag{2}$$

When it is applied to the population of all voucher recipients who did connect, π provides the effective sample size of the study that can be utilized to compute power calculations. Assuming negative price elasticity of connection, however, one finds a trade-off between the study's capacity to detect impacts—the minimum detectable effect—and the sample's representation of the program's population under normal conditions.

In fact, increasing the discount provided by the voucher leads to a greater population of compliers—at the extreme, it is likely that a 100 percent subsidy would lead to a never-taker population close to zero—and therefore it diminishes the minimum detectable effect of the study. However, one

[9] Either the entire community or a subset of it, based on a geographical or social distance criteria.

should note from the above discussion that the impact is estimated on the very population that would not have connected under normal conditions—by definition, the compliers only connect *because* they are given a voucher. Thus, if there is a positive correlation between initial wealth and one's willingness to pay for connection, the population of compliers on which impact will be assessed is likely to be less wealthy than its always-taker counterpart—the population that connects under normal conditions. For this reason, lower incentives should lead to a population of compliers that is closer in its characteristics to always takers.

Two sources of information may be utilized to partially solve this dilemma. First, initial assessments of price elasticity of connection—based on other studies or on contingent valuation questionnaires administered at baseline—can help assess the likely effect of vouchers on connection rates and hence provide an initial estimate of π. Second, voucher levels can be designed in line with potential for future pricing policies: while a 100 percent discount is not likely to be replicated on a large-scale basis, it may be the case that the electricity agency is considering future discount policies within some range. In such a case, the impact will be estimated on a population representative of future discount beneficiaries. Together, this range, the estimated π, and the minimum detectable effect sought by the study provide the necessary information to calculate optimal size of the overall sample.

In the current study, the levels of vouchers were jointly decided with Ethiopia's Power Corporation and effectively corresponded to the range of price subsidy options considered for poorer populations. Further, households reported important demand for electricity, and price barriers were deemed to be the most important. Finally, households were asked whether they would connect if the connection price was established at level *x*, with *x* being a household-specific number randomly drawn from four different prices between US$30–45. A simple ordinary least squares (OLS) estimate showed a relatively strong elasticity: a $1 increase in price was roughly related to a 1 percent decrease in the probability that one would connect. Together, this information ensured that the 1,800 households surveyed (20 x 90) were deemed sufficient to detect impacts of reasonable magnitude.

In Table 2.1, we report the sample distribution across villages electrified between survey rounds 1 and 2.

Table 2.1—Connection and voucher allocation in 10 Ethiopian villages

Village ID	1	2	3	4	5	6	7	8	9	10	Total
# hh surveyed	81	83	87	81	85	88	82	62	78	75	804
% of vouchers	61.7	53.0	55.2	69.1	79.3	81.8	68.3	62.9	59.0	57.3	65.0
Of which											
% 10%	56.0	34.8	38.6	46.4	47.7	47.7	51.9	61.5	57.8	70.8	50.7
% 20%	44.0	65.2	61.4	53.6	52.3	52.3	48.1	38.5	42.2	29.2	49.2
% hh connected	43.2	42.2	28.7	23.5	46.0	34.1	35.4	59.7	44.9	26.7	37.9
Of which											
% with voucher	52.0	50.0	41.7	25.0	50.7	36.1	32.1	79.5	41.3	37.2	43.4
% without voucher	29.0	33.3	12.8	20.0	27.8	25.0	42.3	26.1	50.0	12.5	27.8
Test of diff (1-2): p-val	0.04	0.12	0.00	0.62	0.08	0.39	0.37	0.00	0.45	0.01	0.00

Source: Authors' calculation from Ethiopia Rural Electrification Survey.

We also report sample size, voucher allocation, and connection rates for each of the effectively electrified villages.[10] As shown, the number of vouchers distributed varies within each village, as does their face value (10 or 20 percent of the household-specific connection price). The second part of the table reports connection rates in villages, showing substantial heterogeneity across villages, from 23 percent of the households sampled to nearly 60 percent. As shown by simple tests of differences between households with and households without vouchers, such financial encouragements do carry significant positive incentives for households' connection, which in turn positively affects the power of the study.

[10] The reported sample differs from the original sample of 90 households per village due to the absence of any adult in the household at the time of the first or second survey round.

3. CONCLUSION

Overall, we found that implementation of the voucher scheme raised no major difficulties within the communities. With sufficient explanation, a clearly transparent selection process, and a close follow-up with the local offices of the national electricity company, no major obstacles were found. The main problem we faced was that, due to technical reasons, 10 of the 20 planned villages could not be electrified during the course of the study, affecting the study's overall statistical power. A last recommendation is therefore to ensure that selected study sites do not raise any technical difficulties for connection—although this comes at a cost of focusing on the impact of electricity for very specific villages.

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