The economic impact of a health shock on poor households: The case of VL in eastern India

Jaikishan Desai and Rhonda Sarnoff

June 2011

I. Introduction

An unexpected illness or injury can impose an enormous economic burden on poor uninsured households. Higher healthcare expenditures and a possible loss in income lowers economic well-being in the immediate term, but the impact can persist if the disruption in a household's income-expenditure balance is achieved with a significant alteration in its asset-liabilities portfolio which alters future earning capability; this could be via increased indebtedness, sales of productive assets, and erosion of human capital formation via school dropouts.

Empirical estimates of the economic impact of health shocks are limited. Most empirical studies deal with the immediate and direct burden imposed by a disease, and only rarely tackle the extended impact on earnings and well-being. This might be due to some inherent difficulties in estimating the causal relationship between a poverty-related low-incidence event (disease and injury), which cannot be randomized and econometrically identified, and economic well-being which is influenced by several other factors which are difficult to control.

In this paper we use panel data to examine the economic impact of one particular type of health shock – a disease - amongst poor households in India. Visceral leishmaniasis (VL), known as *kala azar* in India, is a vector-borne disease caused by the parasite *Leishmania donovani* and is endemic in the northeastern part of the Indian sub-continent and several other countries in the Middle East, Northern Africa, and South America. It is transmitted by the bite of a sand fly, a tiny insect smaller than a mosquito, and it is estimated that worldwide there are 500,000 new cases each year [1]. The disease requires specialized diagnosis and treatment that can last a few weeks during which the affected person, and those caring for them can suffer a loss in income. The economic burden can be particularly severe because extended dormancy, and similarity of symptoms with other diseases can easily lead to delayed diagnosis and prolonged treatment.

Data for this study were collected as part of a large probabilistic incidence survey of 15,178 households, in which potential cases were invited for a clinical interview, and cases confirmed on the basis of case history, and medical records. The study of economic impact was an add-on component to the incidence survey and took the confirmed VL cases as the starting point and interviewed the constituent households in December 2006 and March/April 2007. During this "baseline survey" data were collected on illness and treatment experience and household economic functioning during the 12 months preceding the interview. A group of comparison households from the same villages were also interviewed in the same survey. These two groups of households were re-interviewed twice and similar data on illness experience and economic functioning were collected over a 16-month period. The resulting panel of VL-affected households and comparison households is used to estimate the immediate and direct impact of the disease on treatment expenditures and household income, and a more extended, indirect impact on economic well-being over the 16-month follow-up period.

The paper is organized as follows. In section II we review the literature on the economic impact of health shocks, and in section III describe the surveys and data. Thereafter, in section IV we outline the estimation approach used in this study. Section V presents the results of our analyses, and in Section VI we discuss the results. Section VII presents some concluding observations.

II. Literature

Several studies have estimated the economic burden imposed by infectious diseases [2-4]. Some of these are macro studies which use estimates of prevalence from one study area, along with estimates of direct treatment costs and indirect costs (days lost to work, and income loss), to estimate the national burden of a disease[5]. Others use household-specific data to estimate the costs of specific diseases, but almost all of these are cross-sectional studies based.

It is widely recognized that the economic impact of aS disease is not constrained to its immediate effect on household expenditures and income, but can extend well into the future with fundamental changes in household indebtedness, earnings capabilities, and well-being[6]. Empirical estimates of these extended impacts are, however, few and far between. Other than the well-known Kigera longitudinal study on the economic impact of AIDS in eastern Africa, the only other longitudinal study we are aware of is Adamstaff (2007) which estimates the impact of three types of health shocks on in Vietnam[7]. Neither of these studies address low-incidence infectious diseases (like VL) and neither examines the impact on consumption-based measures of economic well-being.

III. Data

This study took the opportunity to piggy-back on a large "incidence" survey aimed at developing population-level estimates of the incidence of VL in East Champaran district, Bihar (India).[1] The incidence survey employed a stratified multi-stage design and interviewed 14,223 households in two rounds, the first (Round 1) in December 2006 and the second (Round 2) in April-May 2007. "A trained interviewer visited each selected household and asked the household head or a responsible adult whether any of the household members was currently suffering from VL, experiencing a fever for more than 2 weeks, had been diagnosed with VL, died from VL, or died from an illness with a fever lasting

[1] A map of the district and state is provided in the Appendix.

longer than 2 weeks in the last 12 months. All individuals who met at least one of these criteria were considered possible VL cases. The possible cases, or informants (if the case had died or was unavailable), were invited for a clinical interview conducted the same day by the survey team's medical doctor" (Das et al. 2010: p 6). The initial screening identified 471 possible cases of VL and of the 450 who reported for the clinical interview, VL was diagnosed in 227 individuals. These cases belonged to 194 households, hereafter referred to as VL households.

A baseline household survey was conducted soon after the clinical interview, and interviewers were able to contact 182 of the 194 VL households. In order to measure the immediate and direct impact of the disease detailed data was collected on disease experience, treatment expenses, and income loss associated with the disease. In keeping with the way cases were identified in the incidence survey, the reference period for these data was 12 months.[2] The survey also collected data on household composition, schooling, income, expenditures, assets, and debt, and the reference period for most of these measures was the 12 months preceding the baseline survey. The contemporaneous measurement of disease experience and household economic functioning is unavoidable when cases are identified retrospectively. The important implication though is that without pre-disease data, and no independent source of variation in disease incidence, it is not possible to identify the immediate - indirect - economic impact of the disease on economic well-being.

In each of the administrative blocks of the study area a (quasi) random sample of comparison households – without a VL case during the 12-month reference period - was selected from the same villages as the clinically-identified VL cases; this ensures that the comparison group is comparable in terms of endemicity of the disease. Four households were selected from each village in the high-incidence stratum, three per village in the medium-incidence stratum, and two per village in the low-incidence stratum. No specific criteria were used for selecting these households; selection was based on interviewer judgment of similarity in outward appearance (dwelling size and condition, and living conditions). The two groups of households are largely comparable though there are some distinct differences in household age-composition, and credit market engagement; we discuss these in detail in Section V.

Two follow-up surveys, eight months apart, sought to re-interview all 182 households with a VL case, and the 91 comparison households without a VL case. Only six households were lost to follow up,

[2] For those who had ongoing treatment at the time of the baseline survey, and those who received additional treatment after the baseline survey post-baseline data was collected in the follow-up surveys.

and so we have a final sample of 267 households, 178 with at least one VL case and 89 without a case during the baseline survey reference period.[3]

IV. Methods

Estimating the direct impact of a particular disease - in terms of treatment expenses and income lost because of sick days – is, in principle, straightforward because it is possible to ask disease-specific questions.[4] Our estimates of direct impact are based on detailed questions on all treatments, and income loss during each illness episode, and indicate that the economic shock resulting from the disease is sizable, though highly varied; we discuss these results in the following section.

Estimating the causal (indirect) impact of a disease on economic well-being poses a greater challenge, mainly due to difficulties in estimating causal relationships with observational data, but further complicated by the nature of the causal factor of interest (disease). We provide a brief overview of the issues related to estimating the causal impact of diseases,[5] and then outline the empirical strategy adopted in this study.

The potential outcomes approach, developed in a series of papers by Rubin,[8-11] and termed the Rubin Causal Model,[12] provides a useful framework for outlining the challenges associated with estimating the causal effect of VL. In the binary form of the model a causal factor is represented by D_i, which equals 1 if an individual experiences the factor and 0 if he/she does not; in the evaluation literature the causal factor is treatment of some sort, in our case it is the disease VL. Associated with the two states of D_i are potential outcomes $Y_i(1)$ and $Y_i(0)$, the former representing the outcome an individual would have if they experienced the causal factor ($D_i = 1$) and the latter the outcome they would have if they did not experience the causal factor ($D_i = 0$). The individual effect of the causal factor is typically represented as the difference between these two potential outcomes ($\tau_i = Y_i(1) - Y_i(0)$), but since, at a point in time, both outcomes cannot be observed for the same individual, it is impossible to estimate the individual effect of D_i; this is known as the "fundamental problem of causal inference."[12] Attention, therefore, turns to estimating average effects, which can be unconditional ($E[Y_i(1) - Y_i(0)]$), or

[3] During the follow-up surveys 17 new cases of VL were reported by the sample households, of which only one was from the comparison group, and that too in the second follow-up survey. Unlike the baseline survey, where household-reported VL cases were clinically interviewed for final case confirmation, these new cases were not clinically screened.

[4] In practice, determining the disease-specificity of treatment expenses can be complicated by simultaneous occurrence of other diseases (comorbidity), and estimating income loss is difficult when the affected individual and their household are self-employed.

[5] The following section covers only the most basic material and draws heavily on the comprehensive review of the program evaluation literature in Imbens and Woolridge (2009).

conditional on experiencing the causal factor (E[$Y_i(1)$ - $Y_i(0)$| D_i=1]) or not experiencing it (E[$Y_i(1)$ - $Y_i(0)$| D_i=1]).

The realized outcome for an individual, Y_i, is distinct from the two potential outcomes, but can be written as a combination of the two:

$$Y_i = Y_i(0)\, D_i + Y_i(0)\ (1 - D_i) \quad = \begin{cases} Y_i(0) \ \ if\ D_i = 0 \\ Y_i(1) \ \ if\ D_i = 1 \end{cases}$$

Since what we observe for an individual is the only one of the two potential outcomes, and thus the average effect for the treated group, i.e. E[$Y_i(1)$ | D_i=1], and the average effect for the untreated, i.e. E[$Y_i(0)$ | D_i=0] the real question is whether these observed sample group means are sufficient for estimating what is not observed and thus inferring the average treatment effects for all (relevant) individuals. Much depends on the mechanism by which individuals are assigned to the two groups, the resulting balance in characteristics relevant to the outcome of interest in the two groups, and the relationship between assignment and potential outcomes.

Randomization, by its very nature, implies that assignment is unrelated to potential outcomes and in sufficiently large samples results in an even balancing of covariates in the two groups. In this case the observed average effect in one group can serve as a reasonable estimate of the unobserved average effect in the other group, and we can estimate the average treatment effect (E[$Y_i(1)$ - $Y_i(0)$]) without bias and consistently.

Observational studies, most often, do not have the luxury of randomization and so the challenge, in many ways, is to mimic, as closely as possible, a randomized design. If we can assume that the assignment to treatment is independent of potential outcomes and there is sufficient overlap in the distribution of characteristics (of the unit of analysis) in the two groups, then we can proceed to estimate the average treatment effect subject to these assumptions. The two assumptions are referred to as uncounfoundedness and overlap and together are termed strong ignorability[13]. We can state these assumptions as follows:

Unconfoundedness assumption: $D_i \perp (Y_i(0), Y_i(1) \mid X_i)$

Overlap assumption: $0 < pr(D_i = 1 \mid X_i = x < 1,\ for\ all\ x$

Another assumption, often not stated as explicitly, but implicit in most statistical analyses of observational data is that there is no interaction between the units of analysis. In statistics this is termed the Stable-Unit-Treatment-Value-Assumption, or SUTVA[14]. With some types of treatments, and for some causal effects this might be a reasonable assumption but when we are interested in the causal effect of an infectious disease in an endemic area the assumption is tenuous, at best[15]. One way to reduce the role of interactions is to aggregate units of analysis and in this study we do that by assessing the economic impact of VL on households. While this accounts for within-household interactions, it does not fully address the endemicity of the disease in the study area, which remains a limitation of our study.

Several methods have been developed to estimate causal relationships in observational data and Imbens and Woolridge provide an excellent review of the literature, along with recommendations on the choice of estimators [16]. In this study we use a series of estimators based on propensity score matching, covariate matching, and a difference-in-difference estimator. The difference-in-difference estimator ignores the panel dimension of the data and simply compares the difference in means of the outcome variable in VL households between surveys with the same difference in comparison households. It relies on differencing to control for observed and unobserved correlates of VL, and is thus based on the assumptions underlying difference-in-difference models but also subject to their limitations[17]. To the extent that its assumptions are valid for this study, the estimator is powerful because it is based on an ordinary least regression of differenced data, and thus unbiased, consistent, and efficient. We use it as a benchmark to compare estimates with the other matching estimators which necessarily involve trading off bias reduction and efficiency.

The outcome of interest in this study is real per-adult equivalent expenditures (over 6 months), a consumption-based measure that takes into account differences in household age composition, and the economies of scale that are known to be important for measuring standard of living in resource-sharing households[18-20]. We use three specifications of this measure: (1) log of real per-adult equivalent expenditures in the second follow-up survey, (2) the difference between the real per-adult equivalent expenditures in the second follow-up survey and the baseline survey, and (3) the difference in log of real per-adult equivalent expenditures in the two surveys. The differenced outcome measures amount to employing a combination approach - with difference in difference at the analysis stage and covariate matching for data preparation - and is thus similar to the estimator used by Heckman, Ichimura, and Todd[21].

V. Results

In the baseline survey, 209 individuals were identified as having had VL in the 12 months preceding the survey. Retrospective identification of cases means that there is variation in onset, duration, and length of impact. Twenty-one individuals (10 percent) had ongoing treatment at the time of the baseline survey,[6] another 20 received treatment after the baseline survey, and 13 had died before the baseline interview. Our estimates of treatment expenses and income loss are based on data from all three surveys so there is no truncation of estimates for cases with ongoing treatment and post-baseline treatment.

The mean age of those who had VL was 24 years, and 57 percent were males. Fifty-one percent of the cases were adults, and 25 percent were the head of their household. Since very few women and children are engaged in income-generating activities in the study areas, this demographic pattern implies substantial variation in the potential for income loss due to VL (Table 1).

The 178 households with one or more VL cases (in the baseline survey) tend to be large with, on average, seven individuals, of whom four are 18 years or younger. These households are poor by most any measure. Three-quarters live in thatched dwellings with mud floors, have no toilet facilities, and draw drinking water from a tubewell. They own few consumer durable assets of value. The most striking indicator of their standard of living is annual per-capita expenditure, a widely used indicator of the standard of living that is based on detailed data on food (25 items) and non-food expenditures (clothing, fuel, housing, education, health, etc). Mean per-capita expenditure for a VL household over a six-month period is Rs 5134 ($122) which amounts to only about Rs 28 per person per day, and is well below the dollar-a-day indicator used to gauge absolute poverty around the world. An alternative measure, per-adult equivalent expenditure, better accounts for differences in age composition and economies of scale, and the mean value of this measure, at Rs 10,203 ($243) is almost twice the per-capita estimate.[7]

VL households' livelihood is very closely tied to agriculture: 58 percent reported "own-farm activities" and 73 percent reported casual labor as sources of household income, and these sources are cited as the most important income source by 35 percent and 44 percent (respectively) of households. Forty-seven percent of VL households own land, but land holdings are small, on average only 1.22 acres. Mean value of land owned is Rs 78,910 ($1879) making land the primary marketable asset owned by

[6] Data on treatment expenses and lost income were also collected in the follow-up surveys thus producing a complete picture of the disease experience of households.
[7] We use Bihar-specific scaling factors (for age composition and economies of scale) from [20].

these households. About one-half of VL households also own some livestock, with about a third owning draught animals (cows, bulls, buffaloes) and a third owning goats and sheep.

Tables 2 and 3 present data on a larger set of household characteristics from all three surveys for both VL households and comparison (nonVL) households. We do not discuss these results in any detail except to note that notwithstanding some demographic differences, the two groups of households are quite comparable in terms of living conditions, economic well-being, assets, and household income sources. It is also worth noting that except for the expenditure-based well-being measures, which are the outcomes of interest in this paper and thus discussed in greater detail later, most other measured characteristics are reasonably stable across the three surveys. It, therefore, seems reasonable to assume that many of these characteristics, while measured post-disease, are of a more "structural" nature and likely to reflect underlying features even before the disease experience. This is important because our baseline survey is not a pre-disease measurement, and so we are constrained in the choice of variables for measuring covariate overlap for the matching methods we use later in the paper.

a. Immediate economic impact:

The immediate economic impact of *VL* consists of the unforeseen increase in health care expenditures, and the loss in income due to work days lost to illness. These can vary quite a bit because infection (via the bite of a sand fly) can easily go unnoticed, initial symptoms are fairly general, and disease severity varies. Compounding these disease-specific factors are variations due to the choice of healthcare providers, and resulting diagnosis, treatment, and response to treatment.

Mean treatment expense per individual is Rs 5482 ($134), but a quarter of all individuals spend no more than Rs 2396 ($58) and a quarter have treatment expenses in excess of Rs 7100 ($173). All individuals have some treatment expenses but not all experience loss in income because children and women make up almost two-thirds of all *VL* cases, and less than one-half of adult women are engaged in income-earning activities. Consequently mean (overall) income loss per VL case is Rs 2738 ($65), even though amongst those who did lose income the mean loss is Rs 7631 ($182).[8] Besides income lost by those who are directly affected by *VL*, care-givers can also experience a loss in income. Self-reported – indirect - income loss is observed for 29 percent of affected individuals, and on average amounts to Rs 430 ($10); this tends to be somewhat higher if the (*VL*) affected individual is young and female.

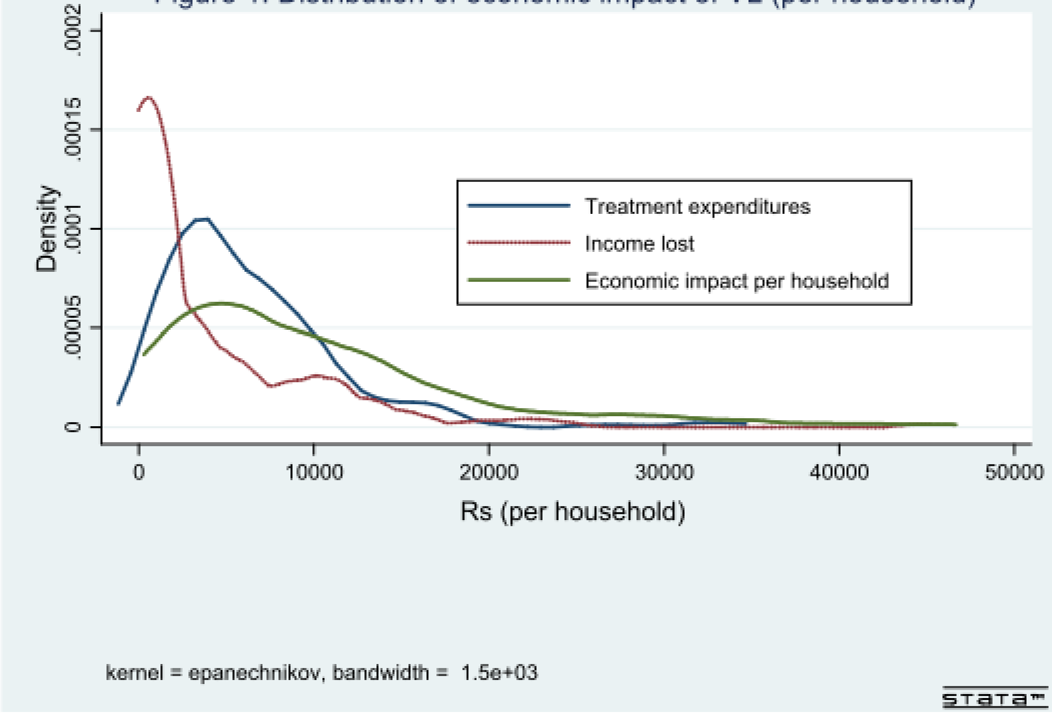
[8] This implies that the average income loss per day was Rs 53 ($1.29), which is very comparable with the daily wage rate for casual labor during this time period.

The immediate – direct - economic impact of *VL* is the sum of unexpected expenses on treatment and the loss in income resulting from the illness. With mean treatment expenses per individual equaling Rs 5482 ($134), and mean income loss amounting to Rs 2739 ($67), the immediate economic impact of *VL* on an individual is, on average, Rs 8220 ($200).

The household level impact of the disease is the sum of individual-level impacts, and any other income lost by those who care for household members affected by *VL*. The 209 individuals who had *VL* belong to 178 households, with 87 percent of households having one *VL* case during the baseline survey reference period, and 13 percent more than one. The mean economic impact of *VL* on a household is Rs 10,158 ($248), of which about 63 percent is due to treatment expenses (Rs 6,436: $139), and 37 percent a result of income loss (Rs 3,721: $91).

Considering how poor these households are, the immediate economic shock is substantial. One way to contextualize the magnitude of the impact is to compare it with mean monthly household expenditure of a *VL* household of seven individuals (Rs 5,295: $129), and mean monthly food expenditures (Rs 3,019: $74). The mean economic impact of *VL* on a household is equivalent to about two months total expenditure and three months expenditure on food.

Figure 1: Distribution of economic impact of VL (per household)

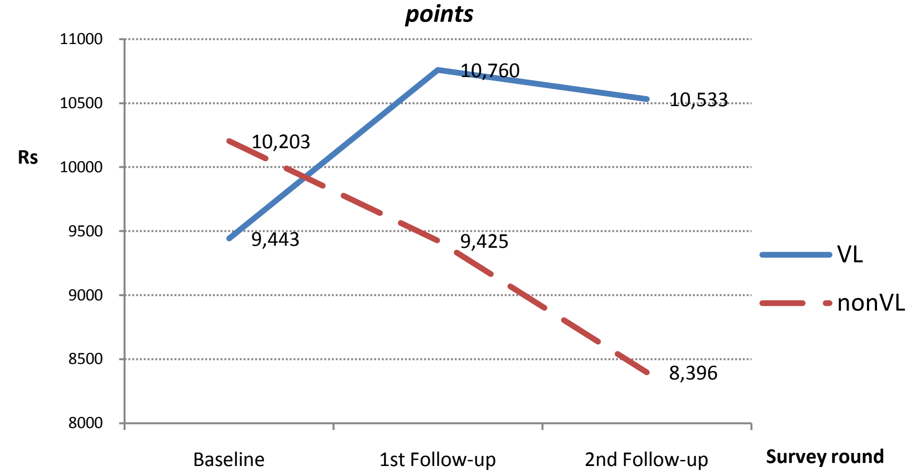


b. Impact of VL on economic well-being

The unexpected increase in VL treatment expenses and loss in income due to lost work days has an obvious, adverse impact on well-being but the magnitude of this effect depends on a household's ability to cope with the health shock. For some who are able to reallocate resources, draw on savings, and borrow from the credit market the impact might be a transitory drop in well-being, but for those who are resource-constrained the impact might be more sustained, and even devastating. In this section we use detailed data on household consumption expenditures to examine the impact of VL on economic well-being over a 16-month period.

Figure 2 displays the means of per-adult equivalent expenditures at the three survey points for VL and nonVL households, and it is readily apparent that starting with very similar levels of living standards during the baseline survey's reference period, there was increasing divergence over the 16

Figure 2: Mean real per-adult equivalent expenditures at three survey points



months of the study period. In nonVL households, between the baseline survey and the first follow-up survey, per-adult equivalent expenditure increased by 14 percent, while VL households experienced a decline of 8 percent. During the next eight months, nonVL households also experienced a small decline of two percent,[9] but the decline in VL households was much higher at 11 percent. Taken together the nonVL households experienced a net 12 percent increase in well-being while VL households experienced

[9] The decline in well-being in nonVL households, quite coincidentally, is related to an unusual increase in healthcare expenditures in this group. The increase in mean expenditures is not due to outliers, and also not due to VL cases, but more likely a reflection of the precarious condition of poor households who repeatedly experience health shocks. We do not pursue this aspect of the data in this paper except to note that the resource reallocation patterns experienced in the nonVL group in the second follow-up survey mirror those observed in the VL group during the baseline survey.

a decline in living standards of 18 percent. We next examine whether this pattern can be interpreted causally – as the effect of VL on the economic well-being of households.

In order to ascribe a causal inference to the pattern displayed by Figure 2, we first examine differences between disease-affected (VL) households and the comparison group to determine whether the two groups are comparable in terms of pre-disease characteristics,[10] particularly those related to the outcome of interest (economic well-being). Since the baseline survey was conducted after initial disease impact, most variables measured in the baseline survey reflect either survey-date conditions or those prevailing over the same reference period as the disease; in both cases they are, potentially, affected by the disease experience. But Tables 2 and 3 also show that most characteristics of household also display a fairly stable pattern across the three surveys, which suggests that at least some (baseline) survey-date variables represent underlying structural characteristics. For formally comparing the two groups, we have selected variables that are least likely to have been affected by VL, and thus most closely reflect pre-disease conditions. Table 4 presents means, standard deviations, and normalized differences (between the two groups) for these variables.[11] Following Imbens and Woolridge's rule of thumb we focus particular attention on normalized differences greater than one-fourth, but also comment on differences greater than one-tenth[16].

Table 4 shows that while the two groups of households are similar in many ways, there are large differences along some dimensions that suggest the groups are not well-balanced in terms of at least some observable covariates. VL households have, on average, one more young (15 years and younger) household member than comparison households, and this translates to a difference in household size; the normalized difference for the former is 0.38, and for the latter 0.265.[12] The two groups are also very different in their credit market engagement. Only 16 percent of VL households borrowed for

[10] Since the comparison group of households was selected (quasi) randomly from the same villages as the VL households, and all data were collected with the same survey instrument geographical and measurement comparability is ensured (Heckman, Ichimura, Todd 1997).

[11] Normalized difference Δ_x is the difference in sample means of the two groups divided by the square root of the sum of the two sample variances $\Delta_x = \frac{\bar{X}_1 - \bar{X}_0}{\sqrt{S_1^2 + S_0^2}}$

[12] We examined differences in various age composition variables and selected this age cutoff because employment data indicate that, in all three surveys, labor force participation rates of those above 15 are distinctly different from those 15 and younger.

consumption purposes prior to the illness,[13] while 60 percent of comparison households did the same; the normalized difference is 69 percent. Differences in borrowing for production and treatment purposes are smaller but also above, the more conservative, threshold of 0.10, and thus indicative of a generally lower level of credit market engagement amongst VL households. It is worth noting that data for the later surveys do not indicate such a sharp difference in borrowing patterns (Table 2). VL households are also less likely to own land, have less acreage of cultivable land, and be more likely to have wage income.[14] These patterns are consistent with the observed caste differences between the two groups: VL households also more likely to be from scheduled castes and other backward castes, which are social groups at the bottom of the socioeconomic ladder in India. While the normalized differences in these variables, along with the difference in household head's marital status, are all lower than 0.25, together they point to a lower physical resource base in VL households; an index based on a principal components analysis of these resource variables has a normalized difference of 0.14.

Ignoring imbalance in the distribution of observable covariates has the potential of increasing the bias in average treatment effects so we undertake "non-parametric preprocessing" of the data to improve overlap in covariate distributions [22]. We employ propensity score-based matching, and covariate matching to obtain more comparable matched samples to estimate the average treatment effect, and average treatment effect on treated effect of VL on three outcome measures: (1) log of real per-adult equivalent expenditures in the second follow-up survey, (2) the difference between the real per-adult equivalent expenditures in the second follow-up survey and the baseline survey, and (3) the difference in log of real per-adult equivalent expenditures in the two surveys.[15] These three outcome measures reflect different ways of capturing the treatment effect and in effect also amount to slightly different treatment effect estimators (see Section IV).

In addition to the two covariate matching strategies we also employ a difference-in-difference model which ignores the (household) panel dimension of the data and compares temporal differences between the VL and nonVL group without any matching-based adjustment of the groups; treatment

[13] Pre and post-illness borrowing was determined by comparing the loan uptake date with the date for the first episode of sustained high fever (typically the first obvious symptom of VL). This necessarily introduces an observational (duration) bias into the comparison of the two groups because with varied timing of onset of disease during the 12-month reference period, the observation period for pre-illness loans is necessarily shorter for VL households; for nonVL households it is the entire 12 months. We try to minimize the bias by using only dummy variables for the loan purpose, instead of other available information (on number, amount, and source of all loans) but it is quite possible that the measures are still influenced by recall bias.

[14] These variables, in particular acreage, could be affected by the causal factor (illness) but data from the three surveys suggest that there were no land sales during the study period, and income earning sources were also relatively stable.

[15] Details on the effects of matching on covariate imbalance are available from the authors on request.

effect is calculate with and without post-differencing covariate adjustment. This model serves as a useful benchmark for assessing the value of matching estimators.

Figure 2 shows that during the baseline survey's reference period, the mean difference in real per-adult equivalent expenditures in VL households was Rs 760 higher than that in nonVL households. Over the next 16 months VL households' standard of living declined while that of nonVL households increased and by the second follow-up survey the mean difference in real per-adult equivalent expenditures in VL households was Rs 2137 lower than that in nonVL households. A naïve estimator of treatment effect, which is simply the difference in means at baseline and second follow-up thus equals Rs 2897, and this is essentially the difference-in-difference coefficient shown in Table 6. It is interesting that post-differencing covariate adjustment does not alter the estimate, which is statistically significant at 1 percent. Not only is the estimator significant the magnitude of the estimate is sizable: the mean difference-in-difference in living standards of Rs 2897 represents a 28 percent decline from the baseline survey level.[16]

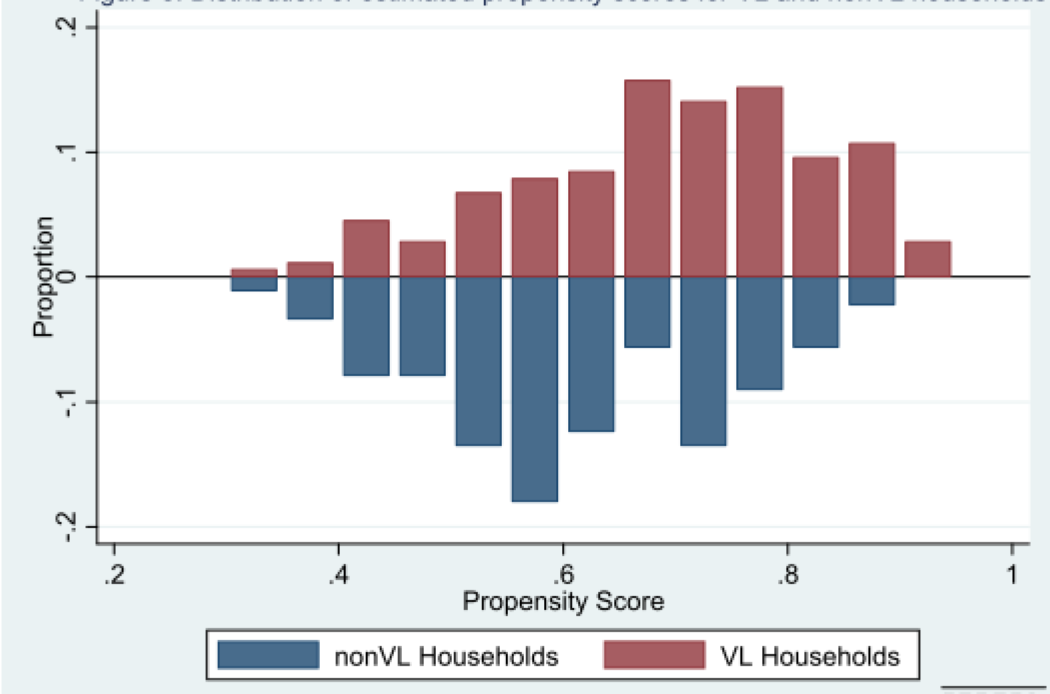
For the propensity score matching methods a propensity score is estimated and Figure 3 shows that except for the tails of the distribution,[17] there is sufficient overlap in the propensity scores to justify using these methods. We follow the literature and trim 10 percent of the sample at both ends of the distribution [23]. Results for three propensity score matching estimators – stratification,[18] kernel regression, and nearest neighbor matching - are presented in Table 5, and average treatment on treated effects of all three are significant at 1 percent for all three outcome measures. While the stratification estimator shows a mean effect in terms of differences in real per-adult equivalent expenditures (Rs 2699) similar to that estimated with the difference in difference model (Rs 2900), the size of the treatment effect obtained with the other two estimators is substantially lower (Rs 1733). We have not attempted to determine the extent of bias reduction achieved by each of these estimators,[19] but note

[16] A similar, but smaller, difference (17 percent of baseline consumption) emerges in real per-adult equivalent food expenditures (results available on request).
[17] We employed a logistic regression specification to estimate the propensity score and experimented with several specifications to achieve the best balance in the estimated propensity score in the two groups. Stata's (version 11) pscore procedure was used to estimate the propensity score. Results of these regressions are available from the authors on request.
[18] The atts Stata program, written by Becker and Ichino, is used for the stratification model, and the psmatch2 Stata program, written by Leuven and Sianesi (http://ideas.repec.org/c/boc/bocode/s432001.html), is used to estimate the kernel and nearest neighbor treatment effect models. Results of the other estimators available in psmatch2 produce very similar results; results available from the author on request.
[19] Table 6 presents covariate bias reduction achieved when using a propensity score based nearest neighbor matching approach. It can be seen that this matching strategy reduces bias some covariates, but increases bias in others.

that the current literature suggests that, other things equal, a stratification estimator with five equal-sized strata is able to reduce bias by 95 percent; we use four strata as this leads to the best balancing of covariates within strata.

Finally we turn to Abadie and Imben's bias-adjusted covariate matching estimator.[24] Table 5 shows that the average treatment effect and the average treatment on treated effect estimated by this model are almost identical to the difference-in-difference model, and very similar to the propensity score stratification model.

Figure 3: Distribution of estimated propensity scores for VL and nonVL households



These results suggest that even though the two groups are very different in terms of their demographic composition, and somewhat different along a few other dimensions these factors do not have a strong bearing on the estimated average treatment effect of VL. The estimated average treatment effect in most of the matching models is similar to that obtained with the more efficient difference in difference model.

VI. Conclusion

It is widely recognized that health shocks can radically alter the life trajectory of poor households, and in developing countries these are largely linked to infectious diseases. Estimates of the impact of health shocks are, however, few and far between and the few studies that have been conducted on this important issue have little information beyond the immediate expense burden imposed by diseases. We thus have little empirical information to determine the extent to which health shocks impact a household's well-being over an extended period of time. Against this background this study fills a large gap in the literature and provides estimates of the causal impact of an infectious disease on the economic well-being of poor households in eastern India.

We estimate the extended economic impact of VL by exploiting panel data on a sample of households with clinically identified cases of VL, and a comparable sample of households without a VL case from the same villages. Our estimates suggest that visceral leishmaniasis has a large and significant impact on household well-being, both in the immediate term, and over an extended (16-month) stretch of time. These estimates are consistent across different model specifications, and while we have not tested between these different specifications, it appears that the simplest estimate based on comparing outcome means at different survey points is quite robust to more sophisticated specifications. This has an important implication for the design of future studies on the economic impact of a health shock because our selection of a comparison sample on households without a VL case from the same villages as the VL cases is practical, and low-cost, and does not appear to compromise the findings.

References

1. Singh, S.P., et al., *The epidemiology of Leishmania donovani infection in high transmission foci in India.* Tropical Medicine and International Health, 2010. **15**(Suppl. 2): p. 12-20.
2. Sharma, D.A., et al., *The economic impact of visceral leishmaniasis on households in Bangladesh.* Tropical Medicine and International Health, 2006. **11**(5): p. 757–764.
3. Sundar, S., et al., *Household cost-of-illness of visceral leishmaniasis in Bihar, India.* Tropical Medicine and International Health, 2010. **15**(Suppl. 2): p. 50-54.
4. Russel, S., *The economic burden of illness for households in developing countries: A review of studies focusing on malaria, tuberculosis and human immunodeficiency virus/acquired immunodeficiency syndrome.* American Journal of Tropical Medicine and Hygiene, 2004. **71**(Suppl 2): p. 147-155.
5. Ramaiah, K.D., et al., *The economic burden of lymphatic filarsis in India.* Parasitology Today, 2000: p. 251-53.
6. Skoufias, E., *Economic crises and natural disasters: Coping strategies and policy implications.* World Development, 2003. **31**(7): p. 1087-1102.
7. Wagstaff, A., *The economic consequeces of health shocks: Evidence from Vietnam.* Journal of Health Economics, 2007. **26**: p. 82-100.
8. Rubin, D.B., *The Use of Matched Sampling and Regression Adjustment to Remove Bias in Observational Studies.* Biometrics, 1973. **29**(1): p. 184-203.
9. Rubin, D.B., *Matching to Remove Bias in Observational Studies.* Biometrics, 1973. **29**(1): p. 159–83.
10. Rubin, D.B., *Estimating causal effects of treatments in randomized and nonrandomized studies.* Journal of Educational Psychology, 1974. **66**(5): p. 688-701.
11. Rubin, D.B., *Statistics and Causal Inference: Comment: Which Ifs Have Causal Answers.* Journal of the American Statistical Association, 1986. **81**(396): p. 961-962.
12. Holland, P.W., *Statistics and Causal Inference.* Journal of the American Statistical Association, 1986. **81**(396): p. 945-960.
13. Rosenbaum, P.R. and D.B. Rubin, *The central role of the propensity score in observational studies for causal effects.* Biometrika, 1983. **70**(1): p. 41-55.
14. Rubin, D.B., *Bayesian inference for causal effects: The role of randomization.* Annals of Statistics, 1978. **61**(1): p. 34-58.
15. Halloran, M.E. and C.J. Struchiner, *Causal Inference in Infectious Diseases.* Epidemiology, 1995. **6**(2): p. 142-151.
16. Imbens, G.W. and J.M. Wooldridge, *Recent Developments in the Econometrics of Program Evaluation.* Journal of Economic Literature, 2009. **47**(1): p. 5-86.
17. Bertrand, M., E. Duflo, and S. Mullainathan, *How Much Should We Trust Differences-in-Differences Estimates?* The Quarterly Journal of Economics, 2004. **119**(1): p. 249-275.
18. Lanjouw, P. and M. Ravallion, *Widowhood and poverty in rural India: Some inferences from household survey data.* Journal of Development Economics, 1995. **105**: p. 1415-1434.

19. Deaton, A., *The analysis of household surveys*. 1997, Baltimore: The Johns Hopkins University Press.
20. Meenakshi, J.V. and R. Ray, *Impact of household size and family composition on poverty in rural India.* Journal of Policy Modeling, 2002. **24**: p. 539-559.
21. Heckman, J.J., H. Ichimura, and P.E. Todd, *Matching as an Econometric Evaluation Estimator: Evidence from Evaluating a Job Training Programme.* The Review of Economic Studies, 1997. **64**(4): p. 605-654.
22. Ho, D.E., et al., *Matching as Nonparametric Preprocessing for Reducing Model Dependence in Parametric Causal Inference.* Political Analysis, 2007. **15**(3): p. 199-236.
23. Crump, R.K., et al., *Dealing with limited overlap in estimation of average treatment effects.* Biometrika, 2009. **96**(1): p. 187-199.
24. Abadie, A., et al., *Implementing matching estimators for average treatment effects in Stata.* Stata Journal, 2004. **4**(3): p. 290-311.

Appendix

Districts of Bihar state in India (with study site circled)

Table 1: Characteristics of individuals who had visceral leishmaniasis during baseline survey reference period, and their households

	Mean	*S.D.*
Individuals who had visceral leishmaniasis during baseline survey period		
Mean age in years	23.9	17.7
Proportion adult (>18 years)	0.51	0.5
Proportion male	0.57	0.49
Proportion head of household	0.25	0.43
No of individuals	209	
Household characeristics		
Household size	6.81	2.65
No. of household members 15 years old and younger	3.24	1.93
No. of household members older than 15 years	2.46	1.39
Proportion scheduled castes or other backward castes (OBCs)	0.89	0.31
Proportion of households with at least 1 person older than 15 years with some schooling	0.38	0.49
Proportion who own watches	0.33	0.47
Proportion who own radio, cassete player	0.08	0.28
Proportion who own bicycle	0.60	0.49
Proportion who live in a solid or semi-solid dwelling	0.75	0.43
Proportion whose main source of drinking water is a Tubewell	0.9607	0.1949
Household's sources of income (proportion who income from…..)		
Own farm activities	0.58	0.49
Casual labour (farm and non-farm)	0.73	0.45
Collection/foraging (of food, wood)	0.20	0.40
Remittances	0.35	0.48
Proportion of households who own land	0.47	0.50
Proportion of households who cultivated land	0.61	0.49
Mean acres owned per household (n=83)	1.22	2.11
Mean acres cultivated (n=108)	1.40	1.55
Mean value of land owned by a household (Rs)	78,910	157,684
Mean value of livestock owned by a household (Rs)	3,573	4,995
Per-capita expenditures (6 months)	5,134	3716.41
Per-adult equivalent expenditures (6 months)	10,203	6083.66
Share of food in total household expenditures	0.58	0.15
Share of cereals in total food expenditures	0.51	0.14
No. of households	178	

Table 2: Income sources, productive assets, and labor market effort of households at three survey points

	VL households			*Non-VL households*		
	Baseline	*1st Follow-Up*	*2nd Follow-Up*	*Baseline*	*1st Follow-Up*	*2nd Follow-Up*
Household's sources of income (% of all households)						
Own farm activities	58.4	64.6	55.6	62.9	68.5	64.0
Casual labour (farm and non-farm)	73.0	79.8	74.7	64.0	57.3	59.6
Salaried employment	3.4	1.7	2.2	3.4	2.2	2.2
Personal (jajmani) services	5.6	3.9	1.7	3.4	3.4	3.4
Petty business/trade/manufacturing	8.4	11.2	9.6	9.0	7.9	10.1
Collection/foraging (of food, wood)	19.7	52.2	61.2	12.4	37.1	58.4
Remittances	35.4	42.1	44.9	33.7	41.6	42.7
Other income sources	6.2	12.9	7.3	4.5	14.6	9.0
No. of households	178	178	178	89	89	89
Land ownership						
Percent who own land	46.6	43.8	41.0	55.1	56.2	53.9
Mean acres owned by household	1.22	1.19	1.15	1.57	1.62	1.96
No. of households who own land	83	78	73	49	50	48
Land cultivation						
Percent who cultivated land	60.7	62.9	57.3	67.4	68.5	66.3
Mean acres cultivated by household	1.40	1.25	1.22	1.68	1.66	1.71
No. of households who cultivated land	108	112	102	60	61	59
Ownership of livestock						
Percent who own animals	53.4	41.0	49.4	60.7	57.3	56.2
No. of households who own livestock	95	73	88	54	51	50
Labour market participation of all household members						
Mean Total Hours worked (household memb	1622	1649	1684	1498	1474	1403
Mean Total Days worked by household meml	241	265	269	218	253	235
Mean Total Days lost to work	45	25	23	24	22	24
Number of households	178	178	178	89	89	89
Credit market engagement						
Percent of households who took a loan for treatment	72.5	24.7	23.6	18.0	29.2	25.8
Percent of households who took a loan	82.0	46.6	44.9	56.2	51.7	55.1
Mean Value of assets owned by a household (Rs)						
Land	169,497	235,300	181,348	227,590	246,614	275,217
Livestock	5,630	5,796	4,584	5,495	5,459	4,440
All production assets (tools, livestock, business assets, land)	83,975	106,833	78,617	131,663	143,862	154,444
No. of households	178	178	178	89	89	89

Table 3: Household demographics, housing conditions, assets and standard of living at three survey points

	VL households			*Non-VL households*		
	Baseline	*1st Follow-Up*	*2nd Follow-Up*	*Baseline*	*1st Follow-Up*	*2nd Follow-Up*
Demographics (mean no. per household)						
Children 0-5 years in household	1.35	1.35	1.26	1.02	1.04	1.08
Children 6-10 years in household	1.49	1.47	1.48	0.81	1.01	0.87
Household size	6.81	6.67	6.79	5.61	5.70	5.72
Housing characteristics						
Type of dwelling: Semi Pucca/Pucca (%)	24.7	30.3	25.9	30.3	38.2	41.6
Type of floor of dwelling : Brick, Cement, etc (%)	3.9	2.8	1.1	11.2	6.7	6.7
Main source of drinking water: Tubewell (%)	96.1	97.2	97.8	94.4	97.8	97.8
Toilet facilities: No latrine (%)	96.6	98.3	98.3	94.4	96.6	92.1
Main source of lighting: Gobar gas (%)	98.9	98.9	99.4	98.9	97.8	96.6
Ownership of consumer durables (% who own)						
Watch	33.1	19.7	25.8	34.8	23.6	30.3
Radio, cassette player	8.4	8.4	11.2	11.2	14.6	13.5
Bicycle	59.6	52.2	56.2	62.9	57.3	55.1
Mean Value (Rs) of consumer assets owned by household						
Consumer durables	697	688	719	1,623	1,408	1,581
Dwelling	31,184	35,294	47,001	36,625	45,687	66,419
All consumption assets	31,881	35,982	47,720	38,247	47,095	67,999
Mean Household expenditures (Rs)						
Food	36,231	17,860	15,834	30,175	18,769	15,934
Non-food	7,615	4,758	3,982	8,705	4,526	4,285
Education	1,143	897	797	1,012	619	643
Housing (rent)	2,245	1,271	1,692	2,637	1,645	2,391
Health (excluding VL expenses)	8,183	4,943	3,957	11,854	5,028	6,317
VL - test, treatment	8,120	496	53	-	-	28
Total	63,537	30,224	26,315	54,383	30,586	29,600
Expenditure-based well-being measures (6 months)						
Per-capita expenditures	5,134	4,670	4,211	5,103	5,842	5,682
Per-adult equivalent expenditures	10,203	9,425	8,376	9,443	10,760	10,533
Per-adult equivalent food expenditures	5,848	5,634	5,096	5,411	6,535	5,641
Share of food in total household expenditures (%)	58.4	64.3	63.1	63.0	63.5	57.0
Share of cereals in total food expenditures (%)	51.1	52.5	50.4	46.6	47.4	47.4
No. of households	178	178	178	89	89	89

Note: Reference period for household expenditures was 12 months for baseline survey and 6 months for follow up surveys

Table 4: Means, standard deviations and normalized differences of potentially pre-disease household characteristics in VL and nonVL households

	VL households		*NonVL households*		*Normalized Difference*
	Mean	*SD*	*Mean*	*SD*	
No. of household members 15 and younger	**3.2360**	**1.9282**	**2.2472**	**1.7404**	**0.3807**
No. of household members over 15	2.4551	1.3903	2.5393	1.4227	-0.0424
No. of household members over 15 with some schooling	0.6292	1.0127	0.7753	1.1751	-0.0942
No. of non-nuclear household members	1.3146	2.0890	1.0787	1.5756	0.0902
No. of household members (total)	**5.6910**	**2.4333**	**4.7865**	**2.3907**	**0.2652**
Dummy: Muslim	0.1517	0.3597	0.1798	0.3862	-0.0532
Dummy: Caste general	0.1067	0.3097	0.1461	0.3552	-0.0835
Dummy: Scheduled caste	0.4775	0.5009	0.3596	0.4826	0.1696
Dummy: Other backward caste (OBC)	0.4157	0.4942	0.4944	0.5028	-0.1116
Dummy: Head female	0.0955	0.2947	0.1124	0.3176	-0.0389
Age of household head (years)	43.7416	11.7353	43.5506	14.0430	0.0104
Dummy: Head currently married	0.8820	0.3235	0.9438	0.2316	-0.1553
Dummy: House of durable materials	0.7528	0.4326	0.6966	0.4623	0.0887
Dummy: House floor material - mud	0.9607	0.1949	0.8876	0.3176	0.1960
Dummy: Drinking water source - tubewell	0.9607	0.1949	0.9438	0.2316	0.0557
Dummy: Toilet - no latrine	0.9663	0.1810	0.9438	0.2316	0.0765
Dummy: Household gets income from own farm activities	0.5843	0.4942	0.6292	0.4858	-0.0649
Dummy: Household gets wage income	0.7303	0.4450	0.6404	0.4826	0.1369
Dummy: Household owns land	0.4663	0.5003	0.5506	0.5003	-0.1191
Acres of agricultural land owned	0.5671	1.5618	0.8655	1.6071	-0.1331
Dummy: Household raises livestock	0.8034	0.3986	0.7865	0.4121	0.0294
Dummy: Pre-illness loan for production	0.0449	0.2078	0.1124	0.3176	-0.1776
Dummy: Pre-illness loan for consumption	**0.1685**	**0.3754**	**0.5955**	**0.4936**	**-0.6885**
Dummy: Pre-illness loan for treatment	0.2360	0.4258	0.3146	0.4670	-0.1245
Resource index (principal components based)	-0.068	0.913	0.135	0.889	0.1590
Number of households	178		89		

Table 5: Average treatment on treated, and average treatment effects for various propensity score methods, nearest neighbour covariate matching, and difference in difference models

Propensity score methods

	Average Treatment on Treated (ATT) Effect			
	Coef.	Std. Err.	t-statistic	
Stratification (4 strata)				
Difference in Log(Real per-adult equivalent expenditures)	-0.279	0.083	-3.370	***
Difference in Real per-adult equivalent expenditures	-2699	916	-2.945	***
Log Real per-adult equivalent expenditures in 2nd follow-up sur	-0.162	0.060	-2.702	***
Kernel estimator (with 10 percent trimming)				
Difference in Log(Real per-adult equivalent expenditures)	-0.151	0.085	-3.030	***
Difference in Real per-adult equivalent expenditures	-1733	695	-2.430	***
Log Real per-adult equivalent expenditures in 2nd follow-up sur	8.966	9.141	-2.720	***
Nearest neighbour matching (with 10 percent trimming)				
Difference in Log(Real per-adult equivalent expenditures)	-0.151	0.138	-3.350	***
Difference in Real per-adult equivalent expenditures	-1733	1398	-2.940	***
Log Real per-adult equivalent expenditures in 2nd follow-up sur	8.966	9.147	-2.600	***

Covariate matching

	Average Treatment on Treated (ATT) Effect			
	Coef.	Std. Err.	t-statistic	
Abadie-Imbens estimator with robust errors for heteroscedasticity				
Difference in Log(Real per-adult equivalent expenditures)	-0.268	0.077	-3.480	***
Difference in Real per-adult equivalent expenditures	-2848	917	-3.110	***
Log Real per-adult equivalent expenditures in 2nd follow-up sur	-0.124	0.057	-2.170	**
	Average Treatment Effect (ATE)			
Abadie-Imbens estimator with robust errors for heteroscedasticity				
Difference in Log(Real per-adult equivalent expenditures)	-0.276	0.079	-3.520	***
Difference in Real per-adult equivalent expenditures	-2829	899	-3.150	***
Log Real per-adult equivalent expenditures in 2nd follow-up sur	-0.131	0.061	-2.170	**

Difference-in-difference

	Coef.	Std. Err.	t-statistic	
No covariate adjustment				
Difference in Log(Real per-adult equivalent expenditures)	-0.262	0.081	-3.250	***
Difference in Real per-adult equivalent expenditures	-2900	930	-3.110	***
Covariate adjustment				
Difference in Log(Real per-adult equivalent expenditures)	-0.262	0.078	-3.390	***
Difference in Real per-adult equivalent expenditures	-2900	962	-3.010	***

Table 6: Bias reduction due to propensity score nearest neighbour matching

		Means			
		VL	*NonVL*	*% Bias*	*% Reduction in bias*
No. of household members 15 and younger	Unmatched	3.236	2.2472	53.8	
	Matched	2.9317	2.9658	-1.9	96.5
No. of household members over 15	Unmatched	2.4551	2.5393	-6.0	
	Matched	2.4348	2.5823	-10.5	-75.1
No. of household members over 15 with some schooling	Unmatched	0.62921	0.77528	-13.3	
	Matched	0.62112	0.73137	-10.1	24.5
Dummy: Muslim	Unmatched	0.15169	0.17978	-7.5	
	Matched	0.14907	0.15839	-2.5	66.8
No. of household members (total)	Unmatched	5.691	4.7865	37.5	
		5.3665	5.5481	-7.5	79.9
No. of non-nuclear household members	Unmatched	1.3146	1.0787	12.8	
	Matched	1.1925	1.0497	7.7	39.5
Dummy: Scheduled caste	Unmatched	0.47753	0.35955	24.0	
	Matched	0.46584	0.4177	9.8	59.2
Dummy: Other backward caste (OBC)	Unmatched	0.41573	0.49438	-15.8	
	Matched	0.42236	0.45652	-6.9	56.6
Dummy: Head female	Unmatched	0.09551	0.11236	-5.5	
	Matched	0.09938	0.07764	7.1	-29.0
Age of household head (years)	Unmatched	43.742	43.551	1.5	
	Matched	43.925	43.97	-0.3	76.4
Dummy: Head currently married	Unmatched	0.88202	0.94382	-22.0	
	Matched	0.86957	0.95807	-31.5	-43.2
Dummy: Household gets income from own farm activities	Unmatched	0.58427	0.62921	-9.2	
	Matched	0.57143	0.71118	-28.5	210.9
Dummy: Household gets wage income	Unmatched	0.73034	0.64045	19.4	
	Matched	0.7205	0.65373	14.4	25.7
Dummy: Household owns land	Unmatched	0.46629	0.55056	-16.8	
	Matched	0.43478	0.59783	-32.6	-93.5
Dummy: Household raises livestock	Unmatched	0.80337	0.78652	4.2	
	Matched	0.78882	0.8354	-11.5	176.4
Dummy: Drinking water source - tubewell	Unmatched	0.96067	0.94382	7.9	
	Matched	0.95652	0.94255	6.5	17.1
Dummy: Toilet - no latrine	Unmatched	0.96629	0.94382	10.8	
	Matched	0.96273	0.94565	8.2	24.0
Acres of agricultural land owned	Unmatched	0.56713	0.86547	-18.8	
	Matched	0.55708	0.89378	-21.2	-12.9
Dummy: House floor material - mud	Unmatched	0.96067	0.88764	27.7	
	Matched	0.95652	0.8618	35.9	-29.7
Dummy: House of durable materials	Unmatched	0.75281	0.69663	12.5	
	Matched	0.73913	0.69876	9.0	28.1
Dummy: Pre-illness loan for production	Unmatched	0.04494	0.11236	-25.1	
	Matched	0.03727	0.12267	-31.8	-26.7
Dummy: Pre-illness loan for consumption	Unmatched	0.16854	0.59551	-97.4	
	Matched	0.17391	0.61646	-100.9	-3.6
Dummy: Pre-illness loan for treatment	Unmatched	0.23596	0.31461	-17.6	
	Matched	0.25466	0.23602	4.2	76.3
Resource index (principal components based)	Unmatched	-0.06133	0.12266	-20.4	
	Matched	-0.04235	0.02881	-7.9	61.3