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Does Community Driven Development Work? Evidence from Senegal

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Abstract

Community Driven Development (CDD) programs are an extremely important component of the World Bank's portfolio in the developing world, representing close to $7 billion in 2003, yet solid empirical evidence on their impact is relatively scarce, especially for Subsaharan Africa. In this paper, we consider the impact on access to basic services, household expenditures and child anthropometrics of the PNIR (Programme National d'Infrastructures Rurales) CDD project in Senegal using a unique multidimensional panel dataset on rural households that we followed over a two-year period. Using a variety of estimation procedures, including instrumental variables, and working at different levels of aggregation, we find statistically significant and quantitatively important effects of the program on access by villagers to clean water and health services, as well as on standard measures of child malnutrition. The latter effects are particularly important for children in poor households. We also find that it is completed income-generating agricultural infrastructure projects, as well as enhanced primary educational opportunities, that significantly increase household expenditures per capita, whereas health and hydraulic projects do not, suggesting that completed projects in this CDD program improve child health in part through income effects. The identification strategy we adopt in order to assess the impact of completed projects on beneficiary welfare highlights the importance of the role played by village chiefs and sub-regional politics in determining which eligible villages receive projects and which villages do not.

Keywords: Impact evaluation, Community Driven Development, Multidimensional panel data models.

JEL Classification numbers: O19, H43, I12, I38.

1 Introduction

Community Driven Development (CDD) is *very* big business. In 2003 alone, it represented $7 billion in World Bank commitments (Mansuri and Rao (2004)).[1] Given the absolute magnitude of CDD programs, as well as their very important share of development assistance at the global level, and given that it is unlikely that their importance will decline in the near future, it is of considerable interest to know whether, and how, they work.

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[1] In presenting this figure, we are being slightly over-simplistic by lumping together CBD and CDD; note that the figure may represent an upper bound given that it includes projects only a component of which is CDD.

There is a growing controversy surrounding CDD programs, spurred by the presumption that they are not as "bottom up" as they are meant to be. Indeed, critics of CDD, as well as of similar "participative" approaches, argue that they are not community-driven or -based at all, and that they essentially furnish a thinly-disguised veil behind which local elites or opportunistic development entrepreneurs hijack resources that never reach their intended recipients (Platteau and Gaspart (2003)). The "elite capture" view of CDD operations has also been coupled with the critique that no existing evaluations of CDD programs allow one to identify any significant gain to their participative element, with respect to "standard", top-down alternatives (Mansuri and Rao (2004)).[2] Our paper contributes to a growing body of literature dealing with decentralized development, though our focus is on the impact on beneficiary welfare that can be directly ascribed to a CDD program, rather than on the political economy aspects. Key references in this literature include work by Bardhan and Mookherjee (2000, 2005, 2006a, 2006b), Foster and Rosenzweig (2004), Besley and Burgess (2001, 2002), and Besley and Coate (2003). In contrast to this corpus of work, which is essentially inspired by the Indian experience, our paper provides rare microeconometric evidence in an African context, and focuses on disentangling the impact that can be attributed directly to "treatment", to use program evaluation parlance, by CDD.

The empirical approach of the paper is three-pronged. First, we study the impact of treatment by the program on the accessibility of basic services, household expenditures and child anthropometrics, using a quasi-experimental approach in which geographical units treated by the program were matched, based on the explicit criteria used by the program initiators to establish deployment, with equivalent geographical units that were not treated. This provides us a with an estimate of the impact of the "intent to treat".

Second, we provide instrumental variables estimates of the impact of *completed* projects on the household and child response variables, using an identification strategy based on the workings of elite capture at the village level and its interraction with the efforts deployed by a given village to obtain a completed project, as measured by the opinions expressed by village chiefs. This allows us to assess the magnitude of the impact of "treatment on the treated".

Third, we use instrumental variables methods to estimate the impact of completed projects within geographical units that eventually get treated by the program (who therefore act as their own controls), where our identification strategy is augmented to include instrumental variables based on various measures of the political power at the sub-regional level of individual villages. This section of the paper thereby highlights the importance of what we hold to be a neglected aspect of CDD operations, and which we have christened as the "village capture" phenomenon.

Our empirical results, whether they are based on quasi-experimental methods or on instrumental variables estimates, suggest (i) that residing in a PNIR-CDD eligible area (without necessarily being in a village that receives a completed project *per se*) significantly improves access to clean water and health facilities, above and beyond what is being furnished by existing and/or alternative programs; (ii) that residing in a PNIR-eligible area significantly reduces the prevalence of underweight and stunted children, with this effect being particularly pronounced for children residing in poor households; (iii) that residing in a village that received a completed agricultural or educational infrastructure project significantly increases household expenditures per capita and improves the nutritional status of children;. (iv) that the role played by village chiefs and by local democratic politics at the sub-regional level is a key determinant of which villages receive projects and which villages do not, in the context of this particular CDD program; and finally (v) that, while not

[2] Wassenich and Whiteside (2003) and Rawlings, Sherburne-Benz, and Van Domelen (2004) provide assessments of current Bank practices in terms of impact evaluation of CDD programs.

denying that elite capture may obtain in the context of PNIR in Senegal, it remains that the poor appear to be the biggest beneficiaries, implying that "village capture", and the interlinkage between the attribution of projects and local, sub-regional politics, is the real issue. This final point implies that the elite capture critique may have been slightly exagerated, whereas the "village capture" phenomenon has been unduly neglected.[3]

2 The context

A countrywide consultative process was undertaken in Senegal in 1996 and revealed that the priority needs of the rural population were primarily improved access roads, drinking water, access to health and education services, and improved economic opportunities in rural areas. The population also expressed a strong desire to participate in the key decisions affecting local development, and to assume an increased share in the funding of local development plans.[4]

Within the context of its overall development strategy, the Senegalese government drafted, with the participation of civil society, a Letter of Decentralized Rural Development Policy (LPDRD, to use its acronym in French).[5] The LPDRD set out a long-term strategy designed to promote sustainable and equitable economic growth in the rural sector, as a means for effective rural poverty reduction. The key objectives of the strategy were to ensure effective implementation of the government's decentralization policy; promote partnerships between the various actors involved in the participatory local development planning process to facilitate the broadening of the decision-making platform; ensure an increased and predictable flow of resources for investments in community-based social and economic infrastructure; and strengthen the capacity of rural communities to assume full responsibility for local development planning and implementation.[6]

The World Bank- and IFAD-initiated *Programme national d'infrastructures rurales* ("National Rural Infrastructures Program", henceforth, PNIR) constitutes one of the keystones of this strategy, and operates at the level of the smallest sub-regional administrative unit in Senegal —the *Communauté rurale* ("rural community", henceforth, CR). An average CR includes 42 villages (the number varies between 3 and 132 villages over the 320 CRs in Senegal), and has a population of 13,391 souls (std. = 12,799). 90 CRs were chosen from among the poorest in the nine rural regions of Senegal for treatment by the PNIR. 78% of the poor in Senegal live in rural areas, where the average incidence of poverty is about 40%, as compared with 16% in urban areas. The rural population to benefit from the project is estimated at nearly two million people, more than half of whom are currently poor.

One of the major goals of the PNIR is to operationalize decentralized rural development processes, including matched grant funding aimed at providing target rural communities with basic social and economic infrastructure. In theory, the project is designed to support the decentralization and fiscal reform processes; strengthen the capacity of CRs and local governments to plan, prioritize, manage, and maintain community-based infrastructure; and provide funding for demand-driven community-based rural infrastructure that is managed in a sustainable way. It is hoped that the resulting community infrastructure, combined with

[3]A companion paper, which uses detailed political data on the makeup of the local councils, goes some way towards fleshing out this viewpoint in a rigorous empirical framework.

[4]This section is based in part on IFAD (1999).

[5]See Senegal (1999).

[6]There are a large number of poverty alleviation programmes in rural Senegal. Most are based on decentralized and participatory approaches, in which community investments are demand-driven. In this context, the Canadian International Development Agency (CIDA) is spearheading efforts to decentralize fiscal and financial management procedures. Bilateral donors, such as France and Germany, the European Union, the UNDP and others, are funding or plan to fund other decentralized rural development programmes.

improvements in the access of communities to the national road network, will revitalize the local economy and provide enhanced opportunities for income and employment generation.

The project's participatory processes for identification of needs, priority setting, decision-making and management are, in theory, designed to ensure that the infrastructures to be funded correspond to the highest priorities of each rural community; and that they will benefit the majority of its population. A central tenet in project design is ensuring the proper representation of vulnerable and/or marginalized groups (the young, women, and specific castes) in the identification, design and implementation of community development plans. The formal inclusion of these groups in the local community development committee (*Commmité de concertation et de gestion* —CCG), and in the microproject implementation and maintenance committees, is supposed to enhance responsiveness to the needs of these groups, and to ensure that the local elites do not monopolize project benefits. The effective participation of these groups is, again in theory, part of the eligibility criteria for funding. The menu of eligible infrastructures includes agricultural infrastructure, health, educational and sanitary facilities, potable water and access roads. The long-term vision of the PNIR is one of CRs planning and managing their own development programmes, and mobilizing the necessary financial resources.

The timing of treatment by the PNIR was determined in 2002, during our initial involvement in the project, and the planned deployment of the program, despite sometimes intense political pressure from local officials, underwent almost no changes. Treatment was explicitly determined on the basis of five indices at the CR level, attributing a score from 0 to 100 based on the proportion of the population with access to water, a health center, a school, a road, and a market. Based on these indices, 90 CRs were chosen for treatment out of a total of 320.

In order to construct our main counterfactual in a quasi-experimental manner, we therefore selected our control group CRs by running a simple probit where the dependent variable took on the value 1 when the CR had been chosen to be treated by the PNIR, and zero otherwise. The explanatory variables, in addition to regional dummies, were the five indices utilized by the program initiators. We then selected 18 treated CRs, which we matched with 18 control CRs based on the predicted probability of treatment. This amounts to propensity score matching at the CR level.[7] These 36 CRs were chosen amongst those included in the 2001 ESAM2 survey in order to allow us to test the parallel trends assumption between ESAM2 and our own baseline (more on this below). The timing of treatment is presented in Table 1, along with the number of completed projects, by type of infrastructure.[8] Note that 4 of the CRs that are in the control group at $t = 1$ become eligible for treatment at $t = 2$: this provides some variation along the time dimension in the post-baseline periods.

[7] Note that it was straightforward to match CRs based on the estimated propensity score, given that the program initiators essentially used a lexicographic procedure (beginning with access to water, followed by access to health, and so forth), whereas our probit procedure uncovered the implicit weights placed by the authorities on each of the five access indices.

[8] The 18/18 split between treated and non-treated CRs corresponds to $t = 1$ —our second survey ($t = 0$ corresponds to our baseline). Of the 18 CRs initially in the control group, 4 received treatment at $t = 2$. Treatment at the CR level corresponds to a bundle of services, and the potential economies of scale in service delivery that can be obtained through multisectoral interventions have been stressed by Fay, Leipziger, Wodon, and Yepes (2005) on the basis of a cross-country regression framework that exploits within-country variation between asset quintiles (they highlight the positive interaction effect associated with a multiplicative health×infrastructure variable). See also Chong and Hentschel (2003) on bundling of services in Peru, and Jalan and Ravallion (2003) on the interaction between infrastructure and health knowledge in reducing child diarrhea in India.

3 Basic results: treatment by the PNIR

At the lowest level of disaggregation, our specification is given by the panel regression:

$$y_{civjt} = T_{jt}\gamma + x'_{civjt}\theta + \varepsilon_{civjt}, \tag{1}$$

where $c = 1, ..., C$ denotes children, $i = 1, ..., I$ denotes households, $v = 1, ..., V$ denotes villages, $j = 1, ..., J$ denotes CRs and $t = 0, ..., T$ denotes time periods; y_{civjt} denotes the response variable, T_{jt} is a dummy variable that is equal to 1 if CR j is treated by the PNIR in period t and 0 otherwise, x_{civjt} is a matrix of covariates that always includes period dummies in order to account for common shocks that affect all observations in a given period, and ε_{civjt} is a disturbance term that we shall decompose in various manners depending upon the context. In increasing order of aggregation, our response variables are constituted by child anthropometrics (for children aged between 0 and 36 months), household expenditures per capita, and access to various types of basic infrastructure by the village community.

Our basic purpose is to estimate the magnitude of the average treatment effect (ATE), also known as the "intent to treat", given by the parameter γ, as well as its associated standard error. When the unit of observation is the household, for example, the specification given in (1) will correspond to a panel regression where the disturbance term will account for time-invariant household-specific effects, thereby yielding what is essentially a difference-in-differences (henceforth, DD) estimator.[9]

Since treatment by the PNIR is defined at a higher level of aggregation than the response variables, it is essential to adjust standard errors for clustering (Moulton (1986), Moulton (1990)). Failure to do so will result in downward-biased standard errors that lead to the possibility of spuriously identifying a statistically significant effect of treatment.. As such, all of the standard errors presented below, since observations are at a level of aggregation lower than that of a CR, are clustered at the CR level.[10]

Table 2 compares the distributions of the 4 response variables (household expenditures per capita and three standard anthropometric indicators for children) in our *baseline survey* ($t = 0$) and confirms that there is no statistically significant difference between households or children living in CRs that are eventually treated (over the following 2 years) and those that will not be. This is true whether we consider means, or whether we consider the entire distribution of the response variables using the Bartlett or Kolmogorov-Smirnoff test statistics. This is a first indication that the quasi-experimental approach used to select our counterfactual CRs will not bias our results either in favor or against identifying effects of treatment by the PNIR.

Descriptive statistics on the full sample over the five rounds of our surveys ($t = 0$ to $t = 4$) are provided in Table 3. The households in the villages considered here are particularly poor, even by Senegalese standards: mean expenditures per capita (which include an estimate of the opportunity value of home-produced and consumed agricultural output), over a 4 month period, are equal to FCFA 13,614, which is roughly equivalent to $US 0.23 per household member per day. Even expressed in adult-equivalent terms, the corresponding figure is $US 0.28. Households are large —almost 11 members on average— and a surprizingly high number of heads, given their mean age (53) are literate (35.9%). The villages in the sample are relatively large (1,113 inhabitants), and are overwhelmingly *not* connected to the national electricity grid (74.8%).

The anthropometric results for children reveal better average performance for girls than for boys, a fact

[9] A similar approach is adopted by Alderman, Hoogeveen, and Rossi (2006), who consider the effect of the *Partage* program on child malnutrition using the four rounds of the Kagera (Tanzania) LSMS survey.

[10] On this topic, see also Donald and Lang (2004).

that has often been noted in Subsaharan Africa over the past 40 years, as noted by Svedberg (1990). There is significant heterogeneity when one breaks down the averages by age category, with a tendency for the mean z−scores to be better for very small children (0 to 12 months). Note also that intra-household heterogeneity in child anthropometrics is important, as is intra-child heterogeneity, a fact that will be important, in terms of identification, given our use in what follows of within-household and within-child estimation procedures.

Figures 1, 2, 3 and 4 provide kernel density estimates that represent the unconditional distributions over the five sample periods of log expenditures per capita and three different anthropometric measures of child health, for households living in PNIR-treated and control-group CRs. With respect to households residing in control-group CRs, the unconditional distribution of log expenditures per capita appears to be shifted slightly to the right for treated households (especially towards the middle of the distribution), and a much more noticeable shift to the right is apparent in the distribution of the weight-for-age z−scores (WAZ) for children who reside in treated CRs. The same would appear to be true for the distribution of weight-for-height (WHZ), with the shift in the distribution of the height-for-age z−scores (HAZ) being much less noticeable.

These graphic results are considered more explicitly on a period-by-period basis in Table 4, which provides simple tests of the difference in the unconditional means of the response variables, between treated and control group CRs. In unconditional terms, expenditures per capita are significantly greater in PNIR-treated households than in control-CR households at $t = 3$ and $t = 4$. For height-for-age and weight-for-age, children in PNIR-treated CRs have significantly better anthropometric outcomes at $t = 4$ (for WAZ, this is also true at $t = 3$), whereas there is no statistically significant difference in terms of weight-for-height. Of course these results are purely suggestive of the impact of the PNIR on household expenditures and child malnutrition, in that they do not control for any source of time varying or time-invariant heterogeneity.

3.1 Household expenditures

In analyzing the impact of the PNIR on the logarithm of household expenditures per capita, our basic specification is given by:

$$y_{ijt} = T_{jt}\gamma + x'_{ijt}\theta + \varepsilon_{ijt}, \tag{2}$$

$$\varepsilon_{ijt} = \lambda_i + \eta_{ijt}, \tag{3}$$

where λ_i denotes household-specific effects. Our broadest sample is an unbalanced panel consisting of 756 households, distributed in 71 villages in 36 CRs, and observed at least over 2 periods, yielding 3,458 observations. Of these, 1,956 are eligible at one time or another for treatment by the PNIR program. Note that the within-household estimator also sweeps out any village- or CR-specific effects. Results are presented in the upper portion of the first column of Table 5. The estimated average treatment effect (ATE) corresponds to an increase of 4.2% in household expenditures per capita, but with a standard error that renders this effect statistically indistinguishable from zero ($s.e. = 0.08$).

Note, as is typical in a poor African country, that there were a number of other relevant programmes being implemented concurrently with PNIR, and which could affect our response variables. In order to control for the effects of these programs, we collected information on the presence of other programs/interventions in the villages. At a descriptive level, there is indeed cross-sectional variation (i.e. across villages at a given time period) of different interventions, as one would expect. However, these interventions were time-invariant (usually at the CR level of aggregation) over the 2 years of the dataset, and since we control

for household-specific effects (or child-specific effects in the child anthropometric results reported later), the interventions in question are *de facto* controlled for.[11] Similarly, we also collected information on the presence in each village of local initiatives, such as producer associations and cooperatives, microfinance institutions, or women's group: again, in the presence of household- or child-specific effects these variables drop out of the specification, and are therefore not contaminating the treatment effects (or lack thereof) that we attribute to the PNIR.

In order to see whether the insignificant average effect hides any heterogeneity, we then consider the subsample of households which are observed in our baseline survey ($t = 0$), and divide households into three expenditure classes, corresponding to the poor (the first quintile), the "middle class" (corresponding to quintiles 2, 3 and 4), and the rich (the top quintile), based upon their expenditures per capita at $t = 0$. This yields a *balanced* subsample of 572 households (2,860 observations, of which 1,597 are treated) *which we follow over all 5 periods*. We then estimate our basic household expenditures specification separately on each of these three classes of households, whose identities are therefore *constant* over time.[12] Results are presented in the lower part of Table 5 (column 1), and confirm the absence of statistically significant effects on expenditures per capita.[13]

None of these results change appreciably when we replace expenditures per capita with total household expenditures, or with expenditures per adult equivalent. Similarly, results are the same when variables are expressed in levels instead of in logarithms.[14]

3.2 Child anthropometrics

We consider three measures of child health: the z-scores for weight-for-age (WAZ), height-for-age (HAZ), and weight-for-height (WHZ). Each observation corresponds to a child, aged between 0 and 36 months, followed over at least two periods, yielding the panel specification:

$$y_{cijt} = T_{jt}\gamma + x'_{cijt}\theta + \varepsilon_{cijt}, \quad (4)$$

$$\varepsilon_{cijt} = \lambda_c + \eta_{cijt}, \quad (5)$$

[11]Another way of putting this is that, once we use our fixed-effects specifications, the variables that account for other interventions drop out of the specification. The upshot is that our estimated treatment effects are attributable to the PNIR, and are not being contaminated by the presence of other programs. Of course, we also verified that there was no other program which was *perfectly collinear* with our PNIR eligibility dummy, since this would have rendered identification of the effects attributable to the PNIR impossible.

[12]Note that it is essential that the identities of the households be constant over time, and that the expenditure classes be defined exogenously in terms of the initial period. A multiplicative specification in which the PNIR treatment dummy would be multiplied by an expenditure class dummy is inconsistent, since households can move between expenditure classes from one period to the next, and the right-hand-side treatment variables would then be correlated with the response variable by construction. Work in progress makes use of the panel quantile regression estimator developed by Koenker (2004), to whom we are grateful for providing us with his code.

[13]The estimated ATE on this subsample is equal to a 2.5% increase in household expenditures per capita, which is statistically indistinguishable from zero ($s.e. = 0.08$).

[14]An additional check of the absence of an effect of PNIR eligibility on expenditures per capita can be had by exploiting between-household and between-village variation and estimating a three-dimensional variance components model, where one replaces (3) with a nested specification: $\varepsilon_{ivt} = \nu_v + \lambda_{iv} + \eta_{ivt}$, where $\nu_v \sim i.i.d\ (0, \sigma_v^2)$ denotes the vth unobservable village-specific effect and $\lambda_{iv} \sim i.i.d\ (0, \sigma_\lambda^2)$ denotes the nested effect of the ith household within the vth village; the remainder disturbance, η_{ivt}, is assumed to be $i.i.d\ (0, \sigma_\eta^2)$. Results are very similar when one replaces this with a household-CR nested specification that takes the form $\varepsilon_{ijt} = \nu_j + \lambda_{ij} + \eta_{ijt}$. In order to estimate the variance components, we implemented both a Wallace and Hussain (1969) and a Wansbeek and Kapteyn (1989) estimator (see Baltagi, Song, and Jung (2001) for a discussion of their relative merits, as well as more sophisticated alternatives). Again, we find no statistically significant impact of the PNIR on log expenditures per capita, on average, and when we estimate separately over our three initial expenditure classes, and the appropriate Hausman tests do not reject any of the specifications. Moreover, the σ_v^2 and σ_λ^2 are found to be relatively small with respect to σ_η^2 indicating that it is time-varying household-village effects that are driving our findings. These results are available upon request.

where λ_c denotes child-specific effects. The within-child estimator will control for household, village, and CR specific effects, and will of course account for any unobserved child-specific time-invariant heterogeneity.[15] There are 1,000 children in our sample, who belong to 498 households (these constitute a subset of the 756 households considered earlier). Given that a few children are observed for more than 2 periods, our sample consists of 2,069 observations, of which 1,116 are treated by the PNIR.

WAZ is a measure of short-term malnutrition and may vary in the short-run as a result of transitory income and health shocks; it is also referred to as *underweight*. HAZ, also referred to as *stunting*, is a measure of long-term malnutrition, and will reflect the cumulative impact of disease spells and income shocks over time. WHZ, also known as *wasting*, is a measure of short-term malnutrition that combines the weight and height metrics. Our purpose in assessing the impact of the PNIR program on these variables is certainly not to argue that CDD programs are the best or even a good manner of addressing the issue of child malnutrition. Rather, our purpose is to examine the impact of a CDD program on alternative measures of household welfare that may, in addition, reflect changes in the intra-household allocation of resources induced by treatment.[16] Note that, especially in West Africa, and especially among poorer people in rural areas, there can be sharp seasonal differences in nutritional status.. Body weights in some parts of West Africa have been known to vary by 10 per cent over a year. We account for these potential seasonality effects in all specifications presented in the paper by including period-specific dummies.

Results are presented in columns 2 to 4 of Table 5. In column 2, we consider the effect of the PNIR on WAZ, which we find to be statistically indistinguishable from zero. For HAZ, on the other hand, the ATE is of 0.408 standard deviations and is statistically significant at the usual levels of confidence, with an associated standard error of 0.15. Note also that most of this effect appears to stem from the impact of the PNIR on girls (the female-specific coefficient is equal to 0.484, $s.e. = 0.21$ —the male-specific coefficient is statistically indistinguishable from zero at the usual levels of confidence), and older children (the coefficient associated with the 24 to 36 month age category is equal to 0.516, $s.e. = 0.15$).[17] The impact of PNIR eligibility on WAZ is also statistically significant for this age category. Taken in conjunction with the absence of significant effects of treatment on expenditures per capita, these results indicate that improvements in the welfare of some household members do obtain as a consequence of residing in a PNIR-eligible CR, but that they do *not* appear to be caused by an increase in household expenditures per capita.[18]

As shown by the results presented in the lower portion of Table 5, in which we restrict our attention to the balanced subsample of children belonging to households which we observe in our baseline survey and which we follow over the following two years, the average effect obscurs significant differences across expenditure classes, just as was the case for the sex and the age of the child.[19] For the WAZ indicator, the ATE for poor families is 4 times greater than the average effect (the associated coefficient is equal to 0.941, $s.e. = 0.36$).

[15] See Behrman and Hoddinott (2005) for an example of the use of child-specific effects in identifying the impact of program treatment (the Mexican PROGRESA, in their case) on child malnutrition.

[16] In particular, given that CDD programs may enhance the participation of women, it is likely that one of the channels through they affect child health is by strengthening the relative "voice" of women within households. If this is the case, treatment may improve child anthropometric status even if total household expenditures remain unchanged.

[17] Though somewhat surprizing at first sight, note that the average WAZ scores are the *worst* for children in the 24 to 36 months age class (see Table 3), and that the HAZ scores are the second worst among the three age categories, for older children. If there are diminishing marginal returns to treatment as nutritional status improves, the WAZ result is less surprizing than one might think.

[18] There are no statistically significant effects of the PNIR on WHZ. Note that the WAZ, HAZ and WHZ equations may be correlated, see Morales, Aguilar, and Calzadilla (2004), and that it may be possible to improve on the efficiency of estimation by taking this into account.

[19] This subsample is constituted by 817 children, who belong to 391 households (these constitute a subset of the 572 households considered earlier in the balanced household subsample). In terms of quantile regression methods alluded to earlier (in foonote 12), the only study that we are aware of that studies the determinants of stunting in children is Borooah (2005).

In contrast, the WAZ of children in middle class and rich households are unaffected by the PNIR. The same can be said for HAZ, where the impact on children from poor households is twice the average effect, whereas children in the upper four quintiles of the baseline expenditure per capita distribution are unaffected. These results suggest, despite the absence of targetting in the PNIR, that it is the children of poor families who appear to benefit the most from the program, by dint of the simple fact that the marginal benefits to a given improvement in village infrastructure will be greater for the poor than for the rich or the middle class.

3.3 Access to basic services

Our village level surveys collected information concerning the access of villagers to four basic services: drinking water, health services, a primary school, and a paved road. Our response variable y^k_{vjt} takes on the value 1 when basic infrastructure k is available to villagers within the village, and zero otherwise.

In order to identify the impact on the accessibility of basic services attributable to treatment by the PNIR, we consider the following village-level linear probability model:[20]

$$y^k_{vjt} = T_{jt}\gamma + x'_{vjt}\theta_k + \varepsilon_{vjt}, \tag{6}$$

$$\varepsilon_{vjt} = \lambda_v + \eta_{vjt}, \tag{7}$$

where λ_v is a village-specific effect. Results are presented in Table 6. Treatment by the PNIR increases the probability that villagers will have access to drinking water within the village by 22.4% ($s.e. = 0.06$), whereas the corrresponding increase for access to basic health services (constituted by a "case de santé") is 24.1% ($s.e. = 0.09$). If we assess village access to health services on the basis of a "poste de santé", a much larger structure than the "case de santé" which is meant to serve several villages, and code the variable to equal 1 if the "poste de santé" is either within the village or within 5km, the corresponding coefficient indicates an ATE of 15.3% ($s.e. = 0.07$). For all of the results on access to basic services presented in Table 6, the point estimates are roughly the same and the standard errors slightly smaller when we restrict ourselves to a balanced panel consisting of the 60 villages that are observed over each of the five time periods. Finally, despite the important road-construction component of the PNIR program, we find no significant effects in terms of access to a paved road. This is due to two reasons. First, on a general level, it is likely that the late implementation of this component of the program (with respect to the timing of our surveys) renders it difficult to identify any significant effect over the 2003-2005 period. Second, no road construction appears amongst the completed projects in the villages treated by the PNIR in our sample.

3.4 Robustness

While the test statistics presented in Table 2 did not reject the null hypothesis that the distributions of our household and child-specific response variables were the same for "eventually" treated and control observations *in our baseline survey* (thus supporting the validity of the quasi-experimental construction of our control CRs), a number of other concerns could significantly bias our results. In order to assess the robustness of our findings, we therefore consider whether: (i) the "parallel trends" assumption is verified; (ii) serial correlation issues significantly bias our standard errors (in all likelihood downwards, in the case of the child anthropometrics results); (iii) the inclusion of time-varying child- or household-specific covariates significantly alters our results; and (iv) whether we obtain roughly the same results using a non-parametric

[20] Results are similar if we use a village fixed effects (conditional) logit specification.

propensity score matching approach.[21]

3.4.1 The parallel trends assumption

A key assumption, on which our results are based, is that our counterfactual is properly constructed. In particular, it is essential not only that the treated group and the control group be statistically indistinguishable in the baseline survey, but also that they would have *evolved over time* in the same manner, in the absence of the program. Though one cannot test this hypothesis directly over the entire sample, the availability of data on the same households *prior* to our baseline survey allows us to test the "parallel trends" assumption. These data are constituted by the ESAM2 survey, carried out 2 years prior to our baseline, and upon which we based our sampling scheme for this purpose.[22]

In order to test the parallel trends assumption, we artificially code the observations in our baseline survey ($t = 0$) that will eventually be treated over the following two years ($t = 1$ to $t = 4$) as if they were treated at $t = 0$.[23] Combining our baseline survey ($t = 0$) with the ESAM2 data on the same households ($t = -1$) yields a balanced panel dataset of 1,400 observations (700 households), of which 474 are "treated" at $t = 0$. We then implement a simple DD estimator where the initial period is given by ESAM2 ($t = -1$) and the final period is given by our baseline ($t = 0$).[24] Finding a statistically significant effect of this "placebo" treatment would imply rejection of the parallel trends assumption in that it would indicate a significant divergence in the evolution over time of our response variables between the treated and control households. If, for example, the "treated" households were systematically *improving* in terms of their response variables between $t = -1$ and $t = 0$, with the control households' response variables remaining unchanged on average, the positive impact of treatment that we uncovered for a number of anthropometric response variables between $t = 0$ and $t = 4$ could be entirely spurious. A similar spurious finding of a significant effect of treatment by the PNIR would occur if the control observations' situation were systematically *worsening* over time, with the "treated" observations' response variables remaining stable. As such, failure to reject the parallel trends assumption is crucial in terms of the credibility of our empirical findings concerning the impact of the intent to treat.

The results of a series of tests of the parallel trends assumption are presented in Table 7. As should be clear, the parallel trends assumption is not rejected, on average, be it for log expenditures per capita or for our three measures of child anthropometrics. Moreover, the parallel trends assumption is not rejected for log expenditures per capita and for child anthropometrics, even when we estimate separately over the three different initial expenditure classes. Similarly, when we disaggregate the impact of the "placebo" PNIR treatment by sex and by age category for the anthropometric indicators, there is no statistically significant effect.

To the extent that the non-rejection of the parallel trends hypothesis supports the assumption that treated and control CRs would have evolved in a similar manner over the two years of our surveys in the absence of the PNIR, our estimates of the effects of treatment would appear not to be systematically biased

[21]Note that all of the results reported in this paper are robust to enumerator effects. Given that our surveys were carried out in "paperless" format using pocket PCs (which allowed us to significantly reduce the cost of collecting our panel data), we were easily able to track the performance of each enumerator. The inclusion of enumerator-specific effects changes nothing in our results.

[22]The ESAM2 (*Enquête sénégalaise auprès des ménages*) survey is essentially a Senegalese LSMS.

[23]Results are the same if we only consider those CRs that are treated at $t = 1$.

[24]Note that we must restrict our attention to a specification that includes household-specific effects in that there are no children that we can follow over the 2 year period that separates ESAM2 and our baseline. Our child sample is constituted by 837 children belonging to 450 households that have children aged between 0 and 36 months over both surveys, of which 232 children are "treated" at $t = 0$.

either upwards or downwards.

3.4.2 Serial correlation

As forcefully argued by Bertrand, Duflo, and Mullainathan (2004), (positive) serial correlation can significantly bias standard errors downwards (even when intra-CR cluster effects have been accounted for), raising the possibility that statistically significant effects may be erroneously attributed to treatment. In order to assess whether our results were subject to this problem (particularly those results pertaining to WAZ and HAZ), we re-estimated our basic specification in terms of simple DD estimators restricted to two periods.[25]

Results are presented in Tables 8 and 9. For log expenditures per capita, little changes with respect to the results presented in Table 5: irrespective of the final period that is chosen, the ATE of the PNIR is always statistically indistinguishable from zero, and this remains true when we estimate separately over each of the three initial expenditure classes.[26]

For the WAZ indicator of child anthropometrics, taking the $t = 4$ minus $t = 0$ case as an example, the ATE is statistically indistinguishable from zero, (as in Table 5), while the effect on the WAZ of children in poor households almost doubles in size with respect to the results presented in Table 5 (to 1.720, *s.e.* = 0.60), and remains statistically significant at the usual levels of confidence.

For HAZ, on the other hand, the results reveal that the standard error associated with the ATE reported in Table 5 (which implied a statistically significant impact of the PNIR on HAZ, on average) was underestimated: though the point estimate of the ATE for each DD is similar in magnitude to the average effect over 5 periods, the reported standard errors yield simple DD estimates of the ATE that are *not* statistically significant, at the usual levels of confidence. This is not surprizing for the HAZ measure of child anthropometrics, in that, in contrast to WAZ, it will exhibit a good deal of persistence over time because of its cumulative reflection of spells of malnutrition. On the other hand, and though the standard error increases substantially, this phenomenon is not sufficient to eliminate the statistically significant effect of the PNIR on the HAZ of children from poor households. Indeed, if we take the $t = 0$ to $t = 4$ DD as our preferred specification, the point estimate increase substantially with respect to the effects reported in Table 5, reaching 1.528 (*s.e.* = 0.65).

Thus, while there is some evidence that serial correlation biases the standard errors for the effect of the PNIR on HAZ presented in Table 5 downwards, the $t = 0$ to $t = 4$ DD results confirm that while treatment by the PNIR does not affect expenditures per capita, it does significantly improves the nutritional status of children living in poor households.

3.4.3 Covariates and alternative specifications

Another test of the robustness of our findings involves studying the effect on our results of the inclusion of a number of time-varying child, household and village characteristics, as well as considering alternative

[25] Note that an alternative approach involves the GLS estimator proposed by Hausman and Kuersteiner (2004), who show (using Montecarlo simulations) that it is not optimal in terms of efficiency to *entirely* discard the temporal dimension of the data, though a degree of temporal aggregation may be desirable.

[26] An alternative approach to dealing with the serial correlation issue that can be applied to the expenditure per capita data involves rewriting our basic equation in terms of a dynamic panel specification that includes a lagged-dependent variable: $y_{ijt} = \alpha y_{ijt-1} + T_{jt}\gamma + x'_{ijt}\theta + \varepsilon_{ijt}$, where the disturbance term continues to be decomposed as in (3). Since the within-household estimator is no longer appropriate, because y_{ijt-1} will be correlated by construction with the household-specific effect if the time dimension is finite (reasonable asymptotics in the present context let T be finite and I be large), one must resort to instrumental variables. Application of the usual difference-GMM or system-GMM estimators (see e.g. Arellano (2003)) yields no evidence of a statistically significant impact of the PNIR on expenditures per capita, be it either on average, or when we consider our three initial expenditure classes separately. Results are available upon request.

specifications for our anthropometric response variables. Covariates include child age, the age and literacy status of the household head, the population of the village, whether the village is connected to the electricity grid.[27] While inclusion of these time-varying covariates changes the point estimates somewhat, it does not affect the basic story in which treatment by the PNIR has no statistically significant effect on household expenditures per capita, while it reduces the prevalence of underweight and stunted children, with these effects being particularly important in poor households.[28] For example, the ATE of the PNIR on the weight-for-age z−score of children living in households that were poor in our baseline survey is equal to 0.859. ($s.e.$ = 0.38), when we include the full set of covariates, while the corresponing number for HAZ is 0.786 ($s.e.$ = 0.39); for log expenditures per capita, the corresponding figure is 0.033 ($s.e.$ = 0.08).[29]

An additional check of our results involves transforming the anthropometric response variables into dichotomous variables that equal 1 when the z−score falls below −1.5 —an indication of a moderate level of malnutrition for each of the indicators— and zero otherwise, and applying a linear probability model (results are similar if we use a fixed effects (conditional) logit specification): For the WAZ of children in poor households, for example, the point estimate indicates that treatment by the PNIR reduces the probability that a child will be underweight by 30.0%, with the corresponding figure for stunting being 23.4%.[30]

3.4.4 Propensity score matching estimates

As a final check on our results concerning the impact of PNIR eligibility, Table 10 presents propensity score matching (PSM) estimates of the impact of treatment by the PNIR. Note that these results are not directly comparable with those reported in Table 5. This is because the PSM approach is non-parametric (which is an advantage with respect to regression approaches) and because the common support restriction results in the estimated treatment effects being computed on a subsample of individuals that varies according to the matching procedure being implemented. We use various matching procedures, and report results that correspond to the nearest neighbor, radius, stratification and kernel approaches. The results presented in column 1 of Table 10 confirm that there is no impact of PNIR eligibility on log household expenditures per capita. In contrast, the results presented in column 2 show that PNIR-eligibility has a statistically significant impact on WAZ (the exception being the results of the nearest neighbor procedure), with the point estimates varying between 0.212 ($s.e.$ = 0.069) and 0.472 ($s.e.$ = 0.131). The lower bound of these estimates is in line with the results reported in column 2 of Table 5, although the standard errors are smaller. For HAZ, on the other hand, the PSM results reported in column 3 of Table 10 indicate that there is no statistically significant impact of PNIR eligibility: this is in contrast to the results reported in column 3 of Table 5, where we found a statistically significant and quantitatively important impact of PNIR eligibility on HAZ. Finally, note, in contrast again with the results presented in Table 5 (column 4), that the PSM approach finds a statistically significant impact of PNIR eligibility on WHZ. Though there are some differences with respect to the results presented in Table 5, the basic story that emerges from the PSM estimates is very similar: eligibility for treatment by the PNIR has no effect on household expenditures per capita, while it significantly improves the nutritional status of small children.

[27] As mentioned earlier, our fixed effects specifications already control for various types of community-based initiatives, as well as other program interventions.

[28] A number of results are interesting in and of themselves, but do not constitute the focus of the paper. For example, we uncover the usual U-shaped effect of age on z-scores; see Thomas, Strauss, and Henriques (1992). The full results which include covariates are available upon request.

[29] These results are not reported in the interest of brevity but are, of course, available upon request.

[30] Once again, these results are available upon request.

4 Instrumental variables estimates of the impact of completed PNIR projects

We now turn to estimating the impact of *completed PNIR projects*, on household and child welfare, in contrast to treatment at the CR level, which is essentially akin to eligibility. In the standard Manski (1996) terminology, this corresponds to "the effect of treatment on the treated", as opposed to "the intent to treat". The distinction is important for two reasons. First, while eligibility of the inhabitants of a village for the PNIR occurs at the CR level, the implementation of actual infrastructure projects is village-specific. In other words, there are numerous villages within PNIR-treated CRs that have received *no* infrastructure projects at all. Second, while being eligible for the PNIR is clearly exogenous in that it depends solely on the village's physical location within a treated CR, actually obtaining an infrastructure project is not exogenously determined, and is likely to be correlated with observable and unobservable village characteristics. Insofar as we shall be identifying the effect of completed projects using a within-household or within-child estimator, time-invariant village-specific unobservables are controlled for. On the other hand, there may be unobservable village-specific and time-*varying* factors that simultaneously affect the response variable and the probability that an infrastructure project gets completed in a given village. In this case, the within-household or within-child estimators used so far would result in inconsistent parameter estimates.

4.1 The full sample

We begin by considering the same sample of household as in part 3, which includes 22 CRs that are treated by $t = 4$ and 16 control CRs. The estimated effect of completed projects is thus the difference in the response variables between households or children that live in villages that receive a project, and those that do not, where the counterfactual includes households and children that reside in control CRs (which are not eligible for PNIR projects), as well as those that reside in villages that become eligible by $t = 4$ but that do not receive a completed project. As with treatment by the PNIR at the CR level, identification is thus achieved through both cross-sectional and time-series variability (see Table 1 for the timing of completed projects, by type).

4.1.1 Identification strategy

The process by which a PNIR project actually gets identified and formulated at the village-level, transmitted to the *Conseil rural*, and implemented suggests that a number of village characteristics may constitute admissible instruments. For this to be the case, the variables in question must (i) have no direct effect on the welfare of the households residing in the village (so as to be orthogonal with respect to the structural equation's disturbance term) and (ii) be correlated with the likelihood of the village obtaining a PNIR project (i.e., the instruments must be sufficiently "strong"). A first obvious instrument with which to identify the impact of completed projects is eligibility *per se* (Imbens and Angrist (1994)), as given by the PNIR treatment dummy that has been considered up until now. The results presented in part 3 can also be thought of as reduced forms in which one of the instrumental variables used to identify the effect of completed projects is entered directly into the structural equation.

Apart from eligibility for the PNIR, our identification strategy here is based upon the *opinions* expressed by the village chief, a key player in terms of the setting of village priorities and of the urgency with which potential project proposals will be formulated and followed up on. In particular, because of the partici-

patory process inherent to CDD through which marginalized groups are supposed to gain voice in village decisionmaking, it is possible that there are divergences between the village chief's opinions and actual conditions in the village, and that these divergences will be amplified by the CDD process. The success of a village in obtaining a project will then depend in part upon the outcome of the interaction between the village chief and the villagers. As such, the reduced forms which explain the likelihood of a village taking delivery of a completed project are the result of the interaction between the elite capture process alluded to in the introduction and the reaction of the villagers within the CDD context, though they do not of course constitute a formal test of its existence.

We begin by considering the correspondance between village priorities, as perceived by the chief, and those types of projects that are eligible for PNIR funding. We construct a dummy variable that is equal to 1 when the main priority of the village, *as identified by the chief*, is compatible with the menu of projects that are eligible under PNIR funding. If the village chief identifies a village priority that is compatible with funding by the PNIR (agricultural infrastructure, health, educational and sanitary facilities, potable water and access roads) and is able to influence the choice of project that gets transmitted by the village to the *Conseil rural*, then one would expect this to increase the likelihood of the village obtaining a project.

Of course, it is possible that the opinions concerning village priorities expressed by the chief correspond to the actual situation in the village in terms of available infrastructure, though our fieldwork leads us to favor a "pet project" view of the opinions of village chiefs in Senegal. Consider the four main types of rural infrastructure focused on by the PNIR which turn up as completed projects in our dataset: agriculture, water, health and schooling. If the priority identified by the village chief systematically corresponds to the type of infrastructure lacking in the village, then our instrument would be suspect, as it could be correlated with the structural equation's disturbance term. In order to ascertain whether this is the case, Table 12 presents a linear probability regression, with period dummies and village-specific fixed effects, in which the dependent variable is equal to one when the village chief identifies water as being the main priority in the village, and the explanatory variable is a dummy variable that is equal to one when the villagers do not have access to water (column 1). We do the same for access to a school and for access to a health center (columns 2 and 3).[31] In all three cases, there is *no* evidence of a statistically significant correlation between the village chief's opinion and the absence of the infrastructure in question in the village. While this does not conclusively establish that the chief's priority for the village is uncorrelated with the disturbance term in the structural equation, it suggests that the likelihood of this being the case is low. Moreover, it suggests that a degree of elite capture could obtain in the villages in our sample in that the chief's opinion is unrelated on average to the actual priorities of the villages.

Our second instrumental variable is given by the chief's expectations concerning the future evolution of economic conditions in the village, which may potentially affect the effort furnished by villagers in proposing and following up on funding requests. We construct a dummy variable which is equal to 1 when the village chief's expectation is that economic conditions in the village will deteriorate during the next 5 years. *A priori*, the urgency attached to formulating a request for a PNIR project should be increasing in the expected deterioration of economic conditions, if the chief's perception is the key factor that determines the drive of villagers in submitting proposals. On the other hand, and again because of the participatory nature of the PNIR process that is theoretically designed to run counter to traditional power structures, it may be the case that the village chief's opinions are systematically discounted in collective decisionmaking, and that

[31] Note that we cannot carry out this procedure for agricultural infrastructure since village chiefs never identified agricultural projects as being a priority for the village.

the opposite phenomenon will obtain. Another, independent mechanism through which an expectation of deteriorating economic conditions could decrease the likelihood of receiving a project is if such an opinion reflects the perception that *current* conditions are particularly good (in relative terms) and can only get worse: if current conditions are perceived as being particularly good (by the chief and the village's population as well), this may decrease the urgency with which projects are formulated and submitted, thereby decreasing the likelihood of taking delivery of a completed project. Though there is no reason *a priori* for the chief's expectations concerning the future to be correlated with unobservables that would affect current household expenditures or child health (especially once time-invariant heterogeneity is controled for), it is important, for the IV in question to furnish some modicum of identification, that the chief's opinion concerning the future be correlated with the opinions of the villagers (whether this correlation is positive or negative is immaterial from the statistical standpoint, but interesting from the social standpoint).

Though we cannot directly test the correspondance between the village chief's expectations concerning the future and those of the village inhabitants, we can assess the coherence of their views concerning the *past*. If we regress a dummy variable that is equal to 1 when the chief believes that the economic situation has improved over the past 5 years on the *proportion* of the village population that believes the same, while allowing for village-specific effects and period dummies (see column 4 of Table 12), the estimated coefficient is positive and statistically significant at the usual levels of confidence. This indicates, at least as far as the past is concerned, that the chief's opinions concerning economic conditions are in line with those of the villagers.

Our third instrumental variable is based on the chief's perceptions concerning the likely form that will be taken by the villagers' contribution to an eventual PNIR project, since financial and in-kind (including labor) participation by the villagers is a requisite for PNIR funding. We construct a dummy variable that is equal to 1 when the village chief is of the opinion that villagers will be willing to contribute only labor to the implementation of a PNIR infrastructre project in the village, as opposed to labor and money —money alone occurs very rarely. A contribution by the villagers of between 5 and 20% (depending upon the type of infrastructure) of each project's costs is a key aspect of the PNIR's implementation process, and the willingness of the villagers to contribute financially is likely to affect the likelihood of them being successful in taking delivery of a completed project.

If the chief's perception of the villagers corresponds to their actual opinions and subsequent acts, one would expect the "only labor contribution" dummy to decrease the probability of the village receiving a project. On the other hand, the opposite would be true if the chief's opinions do not reflect the true preferences of the villagers, or if the CDD process *per se* leads to a heightened willingness on the part of villagers to contribute financially, of which the village chief is not aware.

For this instrument to be admissible, it must of course be the case that it is not correlated with income shocks to the village that could affect household expenditures or child health. That this exclusion restriction is likely to be satisfied, at least in terms of the perceptions of the chief, is illustrated in column 5 of Table 12 by the *lack of correlation* betwen the chief's perception that the villagers would be willing to contribute only labor and his perception of whether the village is poor.

Summary statistics for the three village chief IVs, for the full sample, are presented in the uppermost portion of Table 11.

4.1.2 Results

Let P_{vjt} be a dummy variable that takes on the value 1 when a PNIR project has been completed in village v, in CR j, at time t, and 0 otherwise. Consider estimating the log expenditure per capita equation given in (2), where we replace T_{jt} by P_{vjt}:

$$y_{ijt} = P_{vjt}\delta + x'_{ijt}\theta + \varepsilon_{ijt}, \tag{8}$$

and where P_{vjt} is instrumented using the excluded IVs discussed in section 4.1.1. Results for the full sample are presented in the upper portion of Table 13, while the lower parts of the same Table considers the impact of completed PNIR projects by initial expenditure class, using the balanced sample.[32]

For all of the child anthropometrics IV results presented in Table 13 we our IVs are given by PNIR eligibility, the chief's expectations concerning the future evolution of economic conditions in the village, and the chief's belief that villagers will only be willing to contribute labor, since in no case does this instrument set lead to the rejection of the overidentifying restrictions (none of the corresponding p−values reported in Table 13 are below 0.465).[33] For the household expenditures per capita results, on the other hand, we confine ourselves to PNIR eligibility and the chief's identification of PNIR-eligible projects as village priorities. Adding the two remaining excluded IVs did not change the point estimates appreciably, but did result in the rejection of the overidentifying restrictions.

For purposes of comparison, the first line of the results of Table 13 presents an estimate of the impact of completed PNIR projects *without* instrumenting.[34] For log expenditures per capita, the point estimate is more than 4 times the magnitude of the corresponding effect of the intent to treat (see column 1 of Table 5), and is statistically significant at the usual levels of confidence. Moving to the IV estimates in the second line of the Table decreases the point estimate somewhat, but also increases the standard error sufficiently for the result to be no longer significant at the usual levels of confidence. Considering the balanced sample and disaggregating by initial expenditure class reveals no statistically significant impact on household expenditures of completed projects when one instruments; indeed, for households that are initially in the lowest quintile of the distribution, the effect on household expenditures per capita appears to be negative. Thus, whether we consider eligibility for treatment (as in part 3) or completed projects, the evidence suggests that the PNIR taken as a whole has little if any impact on household expenditures per capita. This conclusion will be nuanced once we consider, in the final section of the paper, the impact of the PNIR by project type.

In columns 3 and 4 of Table 13 (second line of results), we present IV estimates, which control for child-specific effects, of the impact of completed projects on WAZ and HAZ. The IV estimates of the impact of treatment on the treated are twice as large as those for the intent to treat presented in Table 5, and they are statistically significant at the usual levels of confidence (the estimate is marginally significant for WAZ). The same is true when we move to the balanced sample and consider the impact of completed PNIR projects on the WAZ and HAZ of children living in households that are poor in our baseline survey. As with the intent to treat, no statistically significant effects of completed projects can be detected for wasting (WHZ).

[32] The sample sizes are slightly smaller than for the corresponding least squares results reported in Table 5 because of a very limited number of village chiefs who did not express clear opinions concerning the three variables considered.

[33] If we include the chief's identification of PNIR-eligible projects as village priorities, the p−value of the OID test falls, but the results remains very similar.

[34] In passing, it is worth noting that our results that rely on child-specific effects without instrumenting, presented here (and in Table 5), are based on the same methodology adopted by Behrman and Hoddinott (2005) to study the impact on child height of treatment by the *papilla* component of the Mexican PROGRESA program.

The upshot is that completed PNIR projects significantly improve the nutritional status of children, and that this effect is particularly important for children living in poor households. Moreover, to the extent that one can compare the magnitude of the effect of the intent to treat (Table 5) and the effect of treatment on the treated (Table 13), with the latter being roughly twice the size of the former, the story that emerges is that the gains to CDD operations in Senegal do not accrue solely on the basis of completed projects: simply residing in a PNIR-eligible CR brings statistically significant benefits in terms of child health (perhaps because of spillovers from neighbouring villages that receive completed projects), with completed projects yielding additional improvements.

Note, as indicated by the Shea (1997) R^2 and F-statistics from the "partialled out" reduced forms presented alongside the reduced forms in Table 14, that there is some evidence that a "weak instruments problem" could bias conventional 2SLS-based results for a number of our specifications.[35] On the other hand, Cruz and Moreira (2005) have shown that these tests can be extremely poor indicators of instrument weakness, and F-statistics below the usual cutoff value of 10 do not necessarily indicate that a weak instruments problem is present.

If we move to a more rigorous, and distributionally-motivated, test statistic of instrument strength (in contrast to the "rules of thumb" based on the partial R^2 or F-statistics), given by the Hahn and Hausman (2002a) m_2 test of instrument validity (which itself is based on the bias-adjusted 2SLS or Nagar (1959) estimator proposed by Donald and Newey (2001)), we do not reject the *joint* null of instrument orthogonality (with respect to the disturbance term in the structural equation) and instrument strength.[36] Though this provides convincing evidence that our identification strategy is valid, we prefer to base our inference on the Fuller (1977) estimator (with the "Fuller constant" set equal to 1), since this estimator possesses finite moments for all values of the "concentration parameter" associated with the reduced forms, as well as good small sample properties. Moreover, Hahn, Hausman, and Kuersteiner (2004) have provided extensive Montecarlo experiment results that show that this estimator performs well when compared to other prominent IV estimators, under weak instruments. Results based on conventional 2SLS or the LIML estimator are similar to those presented in Table 13, as are the standard errors if one bases them on the Bekker (1994) formula.[37] The upshot is that instrument orthogonality *per se* does not appear to be an issue, in that the tests of the overidentifying restrictions do not reject, while even if a mild weak instruments problem were present (and the Hahn-Hausman test suggests that it is *not*), it can be adequately dealt with by resorting to the Fuller estimator.

The reduced forms that underly the results presented in Table 13 are interesting in and of themselves in terms of what they tell us concerning those factors that determine why certain villages obtain PNIR projects and others do not. They are also of independent interest in that they describe the outcome of the interaction between village chiefs and villagers in terms of obtaining a project. They are presented in Table 14.[38] Consider the reduced forms that correspond to the child anthropometrics results with child-specific

[35] On the weak instruments problem, see the surveys by Stock, Wright, and Yogo (2002) and Hahn and Hausman (2003), and an excellent short primer on the ensuing biases by Hahn and Hausman (2002b). It is important to note that we are able to eliminate all traces of a weak instruments problem (more on this below) by varying the instrument set according to the equation being estimated. In the interests of coherence between the different specifications, however, we prefer to present results that are all based on the same instrument set.

[36] Asymptotic properties of the test are presented in Hausman, Stock, and Yogo (2005).

[37] For conciseness we do not present these results which are, of course, available upon request.

[38] Village-, household- and child-level controls are included in the structural equation results presented in Table 13, (and, of course, in the reduced forms presented in Table 14), where applicable, and are given by the covariates discussed in section 3.4.3. Results, in terms of the point estimates and associated standard errors, are almost invariant to the inclusion or exclusion of these covariates, be it in the structural equations or in the reduced forms. Note that village population has a positive and statistically significant impact on the probability of taking delivery of a completed PNIR project, indicating that there a

effects presented in columns 3 and 4. Two aspects of the results are worthy of note. First, the village chief expecting economic conditions to deteriorate in the future significantly decreases the probability of the village obtaining a completed PNIR project. Second, when the village chief is of the opinion that villagers will be willing to contribute only labor to a PNIR project, the likelihood of receiving a completed PNIR project increases (by 8.1 to 9.2% on average). Thus, village chiefs may systematically err in their assessment of the willingness of villagers to contribute financially, or the CDD process may lead to grass roots mobilization that is particularly strong in villages whose inhabitants, in the opinion of the chief, would not have been willing to contribute money.

We provide a partial test of the mobilization argument in column 6 of Table 12 (column 7 carries out the same test with additional instruments that will be discussed below), where we consider those factors that affect the emergence of a functional *Comité de Concertation et de Gestion* (CCG) —a CR-level institution one of whose purposes is to identify and formulate project proposals which are then forwarded to the *Conseil rural*, and which only exists (if at all) within PNIR-treated CRs. The existence of a CCG is a *sine qua non* for obtaining PNIR funding, and is a good indicator of the level of political mobilization achieved in villages throughout the CR as a result of the CDD process. Moreover, its interactions with traditional village authorities, such as the chief, are likely to have non-negligible consequences in terms of the likelihood of a village taking delivery of a PNIR project. As should be clear from the results presented in column 6 of Table 12, the probability of a CCG emerging in a village is uncorrelated with the chief believing that the villagers will be willing to contribute only labor. Similarly, there is no relationship between the chief expecting the future to be less than rosy and our indicator of grassroots political mobilization. Finally, there is no relationship between the chief identifying priorities that are PNIR-compatible, and the likelihood of a CCG emerging. The mechanism through which our village chief IVs affect the probability of a project being completed therefore does *not* appear to be based on political mobilization induced by the CDD process. On the other hand, the identification being provided by the excluded IVs may stem from elite capture effects, though it is difficult to see how to test for them explicitly.

4.2 CRs that are eventually treated

We now turn to the effect on household and child welfare of completed projects for the subsample of households that belong to CRs that ultimately become eligible for treatment by the PNIR (a total of 22 CRs). Though this reduces the size of the counterfactual sample (the 14 control CRs that never become eligible are not included), the PNIR program resulted in the strengthening of a number of institutions that are potentially important in determining delivery of a completed project, yielding additional instruments with which to identify the impact of treatment on the treated. In particular, detailed data pertaining to the makeup of the *Conseil rural* are available for these CRs. Note that the identification of the impact of completed projects on household expenditures or child health in this context is still achieved both through time-series and cross-section variation in the pattern of completed projects, within the 22 CRs that eventually become eligible for PNIR treatment, although most of the identification comes from the 18 CRs that become eligible at $t = 1$. In addition to considering the impact of completed PNIR projects in general, we also disaggregate according to the form taken by the project. The four main categories of projects that we consider are (i) agricultural projects, which usually involve income-generating activities, (ii) health projects, (iii) potable water projects and (iv) educational projects. Identifying the type of project that contributes to household welfare is an important step towards understanding the mechanism(s) through which CDD operates.

significant bias in favor of large villages.

4.2.1 Identification strategy

Our identification strategy in this section is based on the politics of the *Conseil rural*, and the leverage that each village enjoys within this institution. Constructing additional instruments that will vary between villages in a given CR is particularly important for the sample of CRs that are eventually treated by $t = 4$ because it is likely that most of the identification furnished by the village chief IVs stems from differences between these 22 CRs and the control CRs that never become eligible.

Our identification strategy is based on variations in the stock of political capital enjoyed by different villages within the *Conseil rural* of the CRs in which they are located. The intuition is as follows: the *Conseil rural* in a CR is one of the main institutional actors that determines whether PNIR projects proposed by various villages within the CR obtain PNIR funding, and it is the *Conseil rural* that must arbitrate between the competing claims of several villages. The *Conseil rural* is constituted by individuals who originate from different villages within the CR.[39] Our hypothesis is that the probability of a village obtaining a project, when it is eligible (i.e. when the village is within a CR that is treated by the PNIR), is an increasing function of its "stock of political capital" within the *Conseil rural*.

Given the vibrant nature of party politics in Senegal at the sub-regional level, we construct two instruments on the basis of this intuition. First, we consider whether at least one of the village's councilors has a party affiliation that corresponds to the party which controls the *greatest number* of seats on the *Conseil rural* (the party in question may therefore not possess an absolute majority). On the one hand, it is possible that belonging to the political party that controls the largest block of votes within the *Conseil rural* may increase the political leverage of the village's councilors. On the other hand, standard political economy arguments suggest that one might uncover a "dictatorship of the minority" effect, in which belonging to a minority group increases one's power through one's ability to *block* proposals. Note that there are a total of 16 different political parties represented in the *Conseil ruraux* in our dataset, though 2 —the ruling Liberal party of President Wade, and the Socialist party of former President Diouf— are by far the most important. Second, we consider the same variable, but defined in terms of the *absolute majority* on the *Conseil rural*, when such a majority exists.[40]

A third political variable that we use in order to achieve identification is constituted by the number of female villagers on the *Conseil rural*: given the relative rarity of female *Conseil rural* members, it is likely that their presence on the *Conseil rural* will have an impact on the likelihood of projects being attributed to a given village. In particular, since the philosophy that underlies CDD programs such as the PNIR is to provide "voice" to disenfranchised groups, such as women, one would expect the number of female villagers sent to the *Conseil rural* to significantly increase the likelihood of the village receiving a completed PNIR project.

For all of these politically-motivated instrumental variables, the exclusion restrictions are that they do not have any effect on expenditures per capita or child health within the village, apart from the indirect effect that obtains through the probability that the village receives a PNIR project. Given that we control

[39] See Senegal (1998) for the institutional details.

[40] An alternative manner of using the information concerning the makeup of the *conseils ruraux* is to compute an index of the political power of each village, the most commonly used indices being those developed by Shapley and Shubik (1954) and Banzhaf (1965). Based on the concept of the value of an n-person cooperative weighted voting game, power indices, which are sometimes referred to as *semivalues* (Dubey, Neyman, and Weber (1981)), measure each village's *a priori* possibilities of influencing the outcome of a vote in the *conseil rural*. The Shapley-Shubik index, for example, represents the expected number of times a given player (village) will be in a *pivotal* position, where being pivotal means that one's defection from a winning coalition would turn it into a losing one, and assumes that all permutations (i.e. vote sequences) are equally probable. The Banzhaf index, on the other hand, assumes that all coalitions are equiprobable. Results do not change appreciably when we use these measures of the political power enjoyed by a given village within the *Conseil rural*.

for household- or child-specific effects, these exclusion restrictions are robust to time-invariant unobservables that would affect both the political instruments and household income or child health. For our exclusion restrictions on these IVs to be invalid, one would therefore need time-varying shocks to affect both the probability of obtaining a project at the village level and the political makeup of the *Conseil rural* at the CR level. Though possible, such a configuration strikes us as being highly unlikely, especially given that the main features of the makeup of the various *Conseil ruraux* in our sample were determined in the 2001 local elections, two years prior to our baseline survey.[41] Summary statistics on the three additional excluded IVs are reported in the lower portion of Table 11.

Note also that village political power at the *Conseil rural* level could be used to attract other programmes which might then confound our identification of the effect attributable to treatment by the PNIR. At the risk of becoming repetitive (see the discussion in section 3.1), this last point is entirely possible and indeed likely, but given that the other interventions were time-invariant at the village level in our dataset, the household- or child-specific effects (village-specific effects would suffice and are subsumed under the fixed effects at lower levels of aggregation) already purge this potential confounding factor. Indeed, they do so even in the first-stage reduced forms in our IV procedures. Given the progressive phasing-in of PNIR eligibility and village projects, and the variation over time in the political capital of the villages, this collinearity does not apply to the PNIR projects and allows one to identify their effects.

4.2.2 Results

Results for the completion of *any* type of PNIR project are presented in Table 15, with none of the specifications being rejected by the tests of the overidentifying restrictions or the Hahn-Hausman m_2 test statistic. We present results based on the Fuller estimator.

Contrary to the results presented in Table 13, there is evidence, once the political capital IVs come into play, of a statistically significant effect of completed PNIR projects on household expenditures per capita. As shown in column 1 of Table 15, a completed PNIR project in the village increases per capita household expenditures by 65% (*s.e.* = 0.16), with this effect being highly significant. The IV estimate presented in column 2 of Table 15 for WAZ is slightly larger than the corresponding estimate from Table 13, and is marginally significant at the usual levels of confidence. For HAZ, on the other hand, the IV point estimate presented in column 3 of Table 15 is much smaller than the corresponding coefficient in Table 13, and is estimated quite imprecisely. Finally, in contrast to the result presented in Table 13, a completed PNIR project is found (see column 4 of Table 15) to increase WHZ by 1.242 standard deviations, with this last effect being marginally significant.

When we consider the balanced sample and focus on children living in households that were poor in our baseline survey, the estimated impact of completed PNIR projects is large and marginally significant for WAZ, with no effect on HAZ. Again, as with the full sample results, one noticeable difference between the results presented in Tables 13 and 15 is that there is a marginally significant effect of completed projects on

[41] We also considered the existence of a CCG as an additional IV (see the discussion at the end of section 4.1.2). Though estimates based on this additional instrument significantly reduced the standard errors associated with completed PNIR projects, we prefer not to base our discussion on these results in that the underlying exclusion restriction that renders it valid is much more tentative than that for the *conseil rural* based instruments. In particular, time varying shocks that affect the probability of obtaining a project and simultaneously affect the probability of having a functional CCG would render our results invalid. On the other hand, if the unobserved heterogeneity that affects the existence of a CCG is time-invariant, then our results based on this additional IV would be valid. Note that the tests of the overidentifying restrictions are not rejected when we include this additional IV. Since this CR-level political instruments does not appear to be correlated with the disturbance term of the structural equation, this gives us additional confidence in the validity of the CR level political IVs that we do use. The results in question are available upon request.

the WHZ of children living in poor households (see the lowermost portion of column 4 of Table 15).

The reduced form equations corresponding to these estimates are presented in Table 16, and highlight the importance of our politically-based IVs in terms of identifying the impact of completed PNIR projects. The village political capital instruments exhibit a great deal of variability within the 22 CR sample, and are highly significant determinants of the probability of a village obtaining a PNIR project. Two points are worth noting.

First, the probability of a village receiving a project is significantly decreased when there is a villager who is part of the largest party on the *Conseil rural*, whereas the opposite is true when one has a villager who is a member of the majority party. This indicates that being part of an absolute majority increases one's political capital in terms of obtaining a PNIR project, whereas there is evidence for a "dictatorship of the minority" when a party enjoying a plurality is the reference group. Thus, while village-level politics undoubtedly matter —in terms of the interaction between the village chief and the population— the influence that the village enjoys at the *Conseil rural* level is also an important determinant, among villages that eventually become eligible for treatment by the PNIR, of who receives PNIR projects and who does not. Indeed, when one includes the village chief IVs in the reduced forms presented in Table 16, they are not statistically significant: inter-village politics at the *Conseil rural* level would therefore appear to trump politics at the intra-village level in terms of who gets projects and who does not, which is perhaps not at all surprizing (these results are not presented but are available upon request). The results of these reduced form equations lead us to prefer a "village capture" view of CDD operations, over the "elite capture" view that is often espoused by critics of CDD.

Second, the number of female villagers on the *Conseil rural* significantly increases, *ceteris paribus*, the likelihood of a village receiving a completed project.[42] This result strongly suggests that while the elite capture phenomenon may be present, it is tempered by the "voice" given to women in the CDD decision-making process. Moreover, this role is not simple window-dressing, in that a stronger female presence on the *Conseil rural* does increase the likelihood of a village receiving a PNIR project. On the other hand, the female village councilors may be manipulated by traditional village elites in order to further the latter's interests, though there is no obvious means of testing whether this is or is not the case.

4.2.3 Disagregating by type of infrastructure

In Table 17 we present IV results which disaggregate the impact of completed PNIR projects by project type. The upper portion of the Table provides least squares results (without instrumenting) for comparison purposes. We consider four types of projects: agricultural, educational, health and hydraulic (potable water). The corresponding reduced form estimations are presented in Table 18. Our results are based on our three political capital IVs, as well as two of our village chief IVs: we include the former since we have found them (in the previous section) to be key determinants of which villages get a project and which do not, whereas the village chief IVs should affect the choice by villages of the *type* of PNIR project.

Two main findings stand out in the IV (Fuller estimator) results reported in Table 17.[43] First, it is agricultural (income generating) PNIR projects that appear to be driving the significant results presented

[42] Note that we can reproduce most of our results by instrumenting using the political capital of *other* villages within the same CR (the exogeneity of the IVs is then, in our humble opinion, really no longer an issue: these results are available upon request). In this case, the probability of a given village receiving a project is a decreasing function of the political capital of other villages within the same CR.

[43] Given that we now have more than one jointly endogenous right-hand-side variable, we test the joint null of instrument orthogonality and instrument strength using the slightly more complicated Hahn and Hausman (2002a) m_3 test of instrument validity, in contrast to the m_2 tests presented earlier.

in Table 15. Whether we consider log expenditures per capita,WAZ or WHZ, it is this type of completed PNIR project that has a positive, quantitatively important, and statistically significant impact. Second, health, water and educational projects do not have a statistically significant impact on child health, although completed educational projects do significantly increase log expenditures per capita. These findings suggest that it is in part through increases in household expenditures, rendered possible by successful income-generating agricultural projects, that this particular CDD program improves the welfare of its beneficiaries.

In interpreting our results, it has been pointed out to us that we do not consider possible negative externalities.[44] That is, we do not mention the possible diversion of resources (teachers, health staff, textbooks, medicines) from other communities to the treatment ones. If this occurred, it is argued, it needs to be counted and will offset any benefits from treatment that we have identified. In our opinion, this is an interesting question, which brings one back to the fundamental issue of what the relevant counterfactual is. At the conceptual level, if one says that villages that did not get projects (because, for example, they did not have the necessary political clout) suffered a loss, then their loss is exactly what they could have expected to gain had they had sufficient political capital. Another way of putting this is to say that factoring in the losses of villages that did not get projects amounts to double-counting. Indeed it is the variation in political capital among villages and over time that induces the exogenous variation in the likelihood of receiving a PNIR project that renders identification possible.

A related issue involves spillovers from villages that receive a completed PNIR project to villages within the same PNIR-eligible CR that do not. In this case, the "intent to treat results" presented in section 3 of the paper already include the inter-village spillovers (within a given CR) given that eligibility for treatment obtains at the CR level of aggregation.

5 Concluding remarks

In this paper, we have studied the impact of a major CDD project in Senegal on the welfare of the beneficiaries, using both household expenditures and child anthropometrics as response variables. The evidence we have marshalled broadly suggests, at least as far as the PNIR is concerned, that CDD infrastructure programs can improve the nutritional status of children in households that reside in eligible areas, irrespective of whether the village they live in receives a completed project or not. This constitutes tentative evidence for a putative CDD effect, in that it is often argued that the participatory nature of such programs generates social dynamics that can improve beneficiary welfare, in particular by giving "voice" to hitherto disenfranchised groups. Given that we have shown that the PNIR has improved access to clean water and healthcare facilities, and that it may be this improved access that lies in part behind the improvement in child anthropometrics, it seems reasonable to conclude that the program has been a success. In particular, the PNIR appears to have been particularly successful in improving the nutritional status of children in poor households. In addition, our instrumental variables results suggest that *completed* PNIR projects in the areas of agricultural and educational infrastructure (but not in the areas of health and potable water) significantly increase household expenditures per capita. Though it is not possible to disentangle the "pure" CDD effect from the effect stemming from completed infrastructure projects *per se*, our results do suggest that both channels may be responsible for the observed improvement in child nutritional status, our preferred measure of household welfare.

While the findings in terms of the impact of the PNIR on beneficiaries are important in terms of assessing

[44] We thank Robert Chambers for raising this particular issue.

the effectiveness of CDD programs of its type, the identification strategy we adopted in order to pinpoint the effects of completed projects highlighted the importance of local politics. On the one hand, the role played by village chiefs, as well as their interaction with the population at large in the context of CDD, warrants much more analysis. This is particularly important in that the village-level institutions that are often created alongside CDD, and which are meant to harness the voice of hitherto excluded groups, are not well understood, especially in formal quantitative terms. On the other hand, we have highlighted the paramount role played by sub-regional politics (the *Conseil rural*, in the Senegalese case), and focused on how the ability of individual villages to affect decisionmaking processes at this level of government directly influences their likelihood of obtaining a completed CDD project. Another way of putting this is that if sub-regional government does not give adequate voice to the villages it is meant to represent, there may be "village-capture" in terms of the allocation of projects, and this phenomenon may well be just as important as the elite capture that is the focus of many critiques of CDD.

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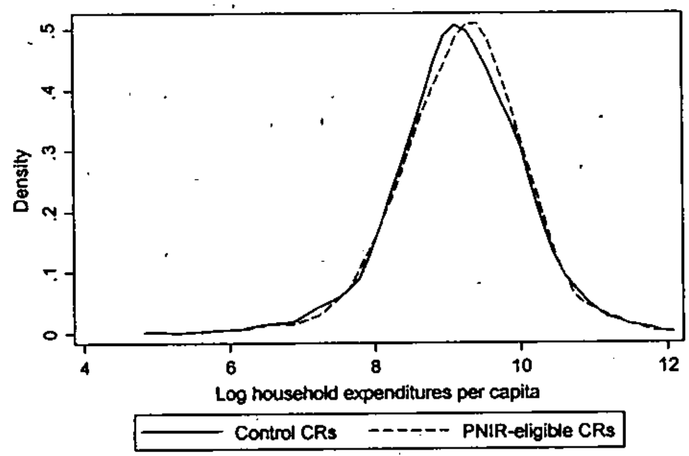

Figure 1: Kernel density estimates of the distributions of log expenditures per capita for households residing in PNIR-treated and control CRs, pooling observations over the 5 periods.

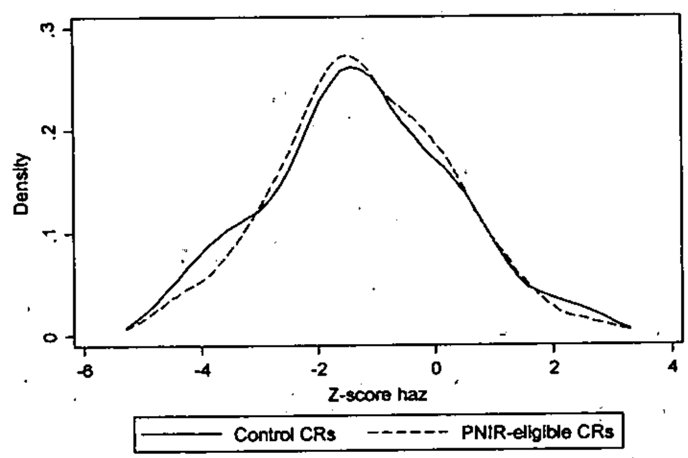

Figure 2: Kernel density estimates of the distributions of height-for-age $z-$scores (stunting) for children belonging to households residing in PNIR-treated and control CRs, pooling observations over the 5 periods.

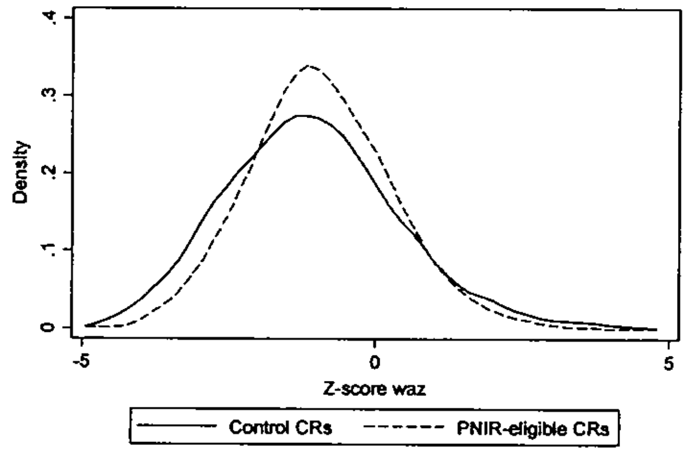

Figure 3: Kernel density estimates of the distributions of weight-for-age z−scores for children belonging to households residing in PNIR-treated and control CRs, pooling observations over the 5 periods.

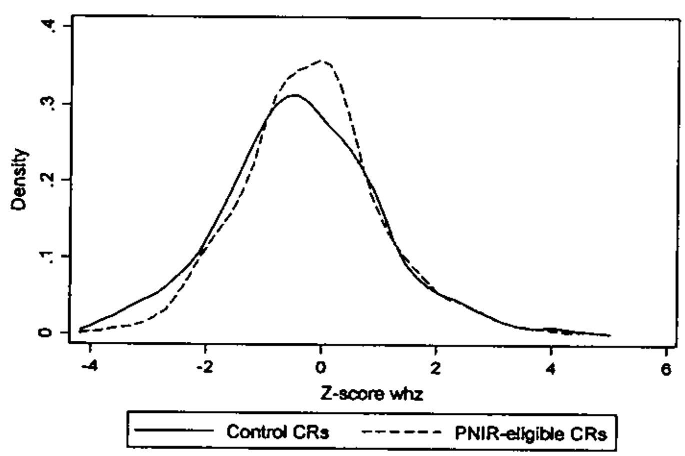

Figure 4: Kernel density estimates of the distributions of weight-for-height z−scores (wasting) for children belonging to households residing in PNIR-treated and control CRs, pooling observations over the 5 periods.

	t	Treated (PNIR-eligible) CRs	Non-treated (control) CRs	Completed projects				
				total	agriculture	water	health	school
Jun. 2003	0 (baseline)	0	0	0	0	0	0	0
Jan. 2004	1	18	18	11	1	1	1	8
Jun. 2004	2	22	14	24	5	3	6	10
Jan. 2005	3	22	14	27	5	3	9	10
Jun. 2005	4	22	14	30	5	4	9	12

Table 1: The timing of treatment and completed projects (cumulative), by project type, June 2003 to June 2005.

Response variables	Mean (standard deviation)		H_0 : no difference in means [p−value]	H_0 : equality of distributions	
	Treated (PNIR-eligible) CRs	Non-treated (control) CRs		Bartlett [p−value]	Kolmogorov [p−value]
Log household expenditures per capita	9.20 (0.95)	9.21 (0.93)	−0.01 [0.94]	0.11 [0.73]	0.04 [0.94]
Height-for-age z−score (HAZ)	−0.91 (1.67)	−1.16 (1.65)	0.24 [0.12]	0.03 [0.86]	0.09 [0.42]
Weight-for-age z−score (WAZ)	−0.84 (1.58)	−1.10 (1.68)	0.26 [0.12]	0.66 [0.41]	0.10 [0.23]
Weight-for-height z−score (WHZ)	−0.34 (1.55)	−0.47 (1.52)	0.12 [0.42]	0.08 [0.76]	0.06 [0.89]

Table 2: Baseline survey, $t = 0$. Testing the null that the distributions of the response variables are identical between households/children in CRs that will eventually (over the four subsequent rounds of surveys) be treated and control group CRs. Tests of the equality of means, Bartlett and Kolmogorov-Smirnov tests of the equality of the distributions.

	Mean	Min	Max	Standard deviation: Total	Within village	Within household	Within child
Child characteristics							
Height-for-age z−score	−1.25	−4.99	3.00	1.54	1.48	1.16	0.80
male	−1.28	−4.99	3.00	1.63	1.51	1.10	0.81
female	−1.22	−4.95	2.94	1.45	1.36	1.02	0.79
0-12 months	−0.77	−4.96	2.98	1.52	1.41	1.01	0.73
12-24 months	−1.54	−4.99	3.00	1.60	1.46	0.91	0.49
24-36 months	−1.38	−4.88	2.60	1.39	1.25	0.75	0.37
Weight-for-age z−score	−1.00	−4.89	4.54	1.34	1.28	0.99	0.66
male	−1.06	−4.40	4.54	1.38	1.28	0.91	0.65
female	−0.93	−4.89	4.54	1.30	1.20	0.84	0.67
0-12 months	−0.33	−4.89	4.54	1.43	1.31	0.91	0.68
12-24 months	−1.25	−4.62	2.93	1.21	1.12	0.71	0.32
24-36 months	−1.34	−4.68	1.76	1.17	1.04	0.62	0.31
Weight-for-height z−score	−0.24	−3.96	4.79	1.31	1.24	0.99	0.69
male	−0.33	−3.96	4.13	1.26	1.15	0.81	0.64
female	−0.14	−3.81	4.79	1.35	1.24	0.93	0.74
0-12 months	0.31	−3.96	4.79	1.43	1.30	0.79	0.48
12-24 months	−0.44	−3.90	3.70	1.27	1.17	0.73	0.46
24-36 months	−0.54	−3.65	3.79	1.03	0.92	0.56	0.32
Age (months)	18.44	0.10	36.99	10.07	9.87	8.49	5.38
Female	0.491	0	1	0.500	0.479	0.327	0
Household characteristics							
Expenditures per capita	13,708	142	152,500	13,175	12,728	10,138	
Age of head	53	17	92	14.1	13.0	2.4	
Household size	10.7	1	34	4.9	4.4	1.1	
Head literate	0.360	0	1	0.480	0.452	0.266	
Female head	0.130	0	1	0.336	0.314	0.075	
Ethnic group of head:							
Wolof	0.479	0	1	0.499	0.334	0.021	
Pular	0.285	0	1	0.451	0.311	0.021	
Serer	0.161	0	1	0.367	0.226	0.015	
Diola	0.022	0	1	0.149	0.059	0.000	
Other	0.017	0	1	0.132	0.121	0.015	
Village characteristics							
Population of village	1,331	135	10,046	1,538	273		
Electricity in village	0.252	0	1	0.434	0.137		
Literacy program in village	0.527	0	1	0.499	0.371		

Table 3: Summary statistics on the full sample: 5 time periods, 36 CRs, 71 villages. For child characteristics, the sample is constituted by 1,000 children that can be followed over at least two periods, living in 498 households yielding a total of 2,069 observations. For household characteristics, the sample is constituted by 756 households that can be followed over at least 2 periods (the sample is larger because a number of households have no children aged between 0 and 36 months) yielding a total of 3,458 observations.

Time periods	Expenditures per capita (FCFA) (std. error)	Child anthropometrics z−scores: Height-for-age HAZ (std. error)	Weight-for-age WAZ (std. error)	Weight-for-height WHZ (std. error)
$t = 1$: Households/children in:				
Household/children in:				
treated CRs	13,637 (521)	−1.416 (0.097)	−1.105 (0.075)	−0.225 (0.080)
control CRs	13,378 (799)	−1.588 (0.129)	−1.246 (0.114)	−0.300 (0.100)
p−value of difference	0.786	0.293	0.305	0.559
$t = 2$: Households/children in:				
treated CRs	13,558 (597)	−1.314 (0.092)	−1.020 (0.079)	−0.213 (0.080)
control CRs	12,580 (956)	−1.378 (0.133)	−1.174 (0.113)	−0.360 (0.109)
p−value of difference	0.385	0.692	0.268	0.283
$t = 3$: Households/children in:				
treated CRs	12,343 (512)	−1.260 (0.091)	−0.951 (0.068)	−0.145 (0.068)
control CRs	9,999 (520)	−1.491 (0.142)	−1.319 (0.123)	−0.451 (0.109)
p−value of difference	0.001	0.174	0.009	0.018
$t = 4$: Households/children in:				
treated CRs	15,613 (655)	−0.977 (0.076)	−0.712 (0.065)	−0.101 (0.067)
control CRs	13,617 (775)	−1.354 (0.108)	−0.923 (0.105)	−0.087 (0.104)
p−value of difference	0.050	0.005	0.090	0.907

Table 4: Mean (standard error) of household expenditures per capita and child anthropometrics, by period, for treated and control CRs: p−value of difference.

Dep. var.	Log expenditures per capita	Child anthropometrics z−scores: Weight-for-age	Height-for-age	Weight-for-height
Estimator	Household FE	Child FE	Child FE	Child FE
	(1)	(2)	(3)	(4)
Full sample				
Average effect	0.042 (0.08)	0.261 (0.17)	0.408 (0.15)	0.051 (0.17)
males		0.318 (0.18)	0.325 (0.18)	0.207 (0.20)
females		0.207 (0.20)	0.484 (0.21)	−0.093 (0.21)
0-12 months		−0.384 (0.26)	−0.433 (0.25)	0.043 (0.23)
12-24 months		0.049 (0.18)	0.239 (0.19)	−0.121 (0.18)
24-36 months		0.413 (0.17)	0.516 (0.15)	0.133 (0.19)
Observations (treated)	3,458 (1,956)	2,069 (1,116)	2,069 (1,116)	2,069 (1,116)
Time periods	5	5	5	5
CRs	36	36	36	36
Villages	71	71	71	71
Households	756	498	498	498
Children		1,000	1,000	1,000
Balanced sample				
Average effect	0.025 (0.08)	0.224 (0.17)	0.381 (0.16)	0.017 (0.19)
By initial expenditure class:				
poor	0.012 (0.14)	0.941 (0.36)	0.770 (0.34)	0.501 (0.43)
middle class	0.009 (0.05)	0.148 (0.19)	0.275 (0.24)	0.053 (0.17)
rich	0.073 (0.08)	−0.456 (0.64)	0.532 (0.41)	−1.144 (0.62)
Observations (treated)	2,860 (1,597)	1,786 (966)	1,786 (966)	1,786 (966)
Time periods	5	5	5	5
CRs	36	36	36	36
Villages	71	71	71	71
Households	572	391	391	391
Children		817	817	817

Table 5: The impact of PNIR eligibility on log household expenditures per capita and child anthropometrics (z−scores). Initial expenditure classes defined on the basis of our baseline survey (standard errors clustered at the CR level in parentheses).

Dep. var.	Access to basic infrastructure			
	Water	Health	School	Road
	(1)	(2)	(3)	(4)
Average effect	0.224 (0.06)	0.241 (0.09)	0.187 (0.10)	0.032 (0.02)

Table 6: The impact of PNIR eligibility on the access to basic services (1 if access in village, 0 otherwise). 5 time periods, 38 CRs, 71 villages and 341 observations, of which 193 are treated (standard errors clustered at the CR level in parentheses).

Dep. var.	Log expenditures per capita	Child anthropometrics z−scores		
		Weight-for-age	Height-for-age	Weight-for-height
Estimator	Household FE	Household FE	Household FE	Household FE
	(1)	(2)	(3)	(4)
Average effect	0.101 (0.19)	−0.158 (0.55)	−0.333 (0.42)	−0.058 (0.53)
males		−0.246 (0.57)	−0.219 (0.46)	−0.320 (0.54)
females		−0.046 (0.55)	−0.481 (0.45)	0.271 (0.57)
0-12 months		0.267 (0.59)	−0.047 (0.46)	0.202 (0.57)
12-24 months		−0.681 (0.45)	−0.391 (0.42)	−0.659 (0.50)
24-36 months		−0.142 (0.62)	−0.621 (0.62)	0.259 (0.61)
By initial expenditure class:				
poor	−0.149 (0.19)	0.842 (1.24)	0.013 (0.98)	1.086 (1.03)
middle class	0.081 (0.14)	−0.601 (0.44)	−0.271 (0.39)	−0.604 (0.52)
rich	0.221 (0.20)	0.701 (0.74)	−0.678 (0.80)	0.761 (0.98)
Observations (treated)	1,400 (474)	837 (232)	837 (232)	837 (232)
Time periods	2	2	2	2
CRs	36	36	36	36
Villages	71	71	71	71
Households	700	450	450	450

Table 7: Testing the parallel trends assumption. Simple DD estimates of impact of PNIR eligibility on log household expenditures per capita and child anthropometrics (z−scores). Initial period is ESAM2 (2001), final period is our initial survey (June 2003). Initial expenditure classes defined on the basis of ESAM2 (standard errors clustered at the CR level in parentheses).

Dep. var.	Log expenditures per capita	Child anthropometrics $z-$scores		
		Weight-for-age	Height-for-age	Weight-for-height
Estimator	Household FE	Child FE	Child FE	Child FE
	(1)	(2)	(3)	(4)
$t = 1$: 19 treated rural communities				
Average effect	−0.014 (0.09)	0.064 (0.21)	0.189 (0.16)	−0.0003 (0.22)
males		0.104 (0.23)	0.289 (0.21)	−0.071 (0.23)
females		0.031 (0.25)	0.107 (0.21)	0.058 (0.26)
By initial expenditure class:				
poor	−0.272 (0.18)	0.065 (0.21)	0.739 (0.48)	0.271 (0.47)
middle class	−0.003 (0.08)	−0.077 (0.24)	−0.031 (0.22)	0.082 (0.22)
rich	0.075 (0.15)	−0.291 (0.58)	0.654 (0.48)	−0.826 (0.54)
Observations (treated)	1144 (370)	677 (213)	667 (209)	667 (209)
Households	572	300	294	294
Children		457	450	450
$t = 2$: 22 treated rural communities				
Average effect	−0.046 (0.15)	0.436 (0.26)	0.425 (0.22)	0.138 (0.32)
males		0.459 (0.27)	0.475 (0.21)	0.176 (0.36)
females		0.410 (0.32)	0.372 (0.34)	0.097 (0.36)
By initial expenditure class:				
poor	−0.172 (0.27)	1.194 (0.46)	0.773 (0.39)	0.939 (0.51)
middle class	−0.064 (0.09)	0.433 (0.29)	0.359 (0.39)	0.211 (0.35)
rich	0.145 (0.20)	−1.558 (0.89)	0.174 (0.51)	−2.518 (0.75)
Observations (treated)	1,144 (409)	653 (234)	653 (234)	653 (234)
Time periods	2	2	2	2
CRs	36	36	36	36
Villages	71	70	70	70
Households	572	308	308	308
Children		483	483	483

Table 8: Discarding the time-series dimension. The impact of PNIR eligibility on log household expenditures per capita and child anthropometrics ($z-$scores). Initial expenditure classes defined on the basis of our baseline survey; simple 2-period DD estimates, various final periods (standard errors clustered at the CR level in parentheses).

Dep. var.	Log expenditures per capita	Child anthropometrics $z-$scores		
		Weight-for-age	Height-for-age	Weight-for-height
Estimator	Household FE	Child FE	Child FE	Child FE
	(1)	(2)	(3)	(4)
$t=3$: 22 treated rural communities				
Average effect	0.084 (0.12)	0.253 (0.25)	0.210 (0.36)	0.181 (0.29)
males		0.327 (0.27)	−0.116 (0.34)	0.556 (0.32)
females		0.197 (0.27)	0.454 (0.42)	−0.099 (0.32)
By initial expenditure class:				
poor	0.107 (0.19)	1.544 (0.32)	1.157 (0.79)	0.926 (0.59)
middle class	0.064 (0.12)	0.253 (0.27)	0.087 (0.37)	0.388 (0.29)
rich	0.113 (0.15)	−1.644 (1.40)	−1.038 (0.93)	−1.855 (1.19)
Observations (treated)	1,144 (409)	681 (250)	681 (250)	681 (250)
Households	572	330	330	330
Children		559	559	559
$t=4$: 22 treated rural communities				
Average effect	0.084 (0.14)	0.443 (0.37)	0.574 (0.44)	0.141 (0.28)
males		0.537 (0.39)	0.306 (0.41)	0.480 (0.41)
females		0.383 (0.44)	0.745 (0.53)	−0.074 (0.27)
By initial expenditure class:				
poor	0.355 (0.26)	1.720 (0.60)	1.528 (0.65)	0.728 (0.85)
middle class	0.032 (0.12)	−0.010 (0.44)	0.159 (0.69)	0.036 (0.35)
rich	−0.055 (0.17)	0.765 (0.87)	1.239 (0.75)	−0.283 (0.64)
Observations (treated)	1,144 (409)	735 (269)	735 (269)	735 (269)
Time periods	2	2	2	2
CRs	36	36	36	36
Villages	71	69	69	69
Households	572	351	351	351
Children		646	646	646

Table 9: Discarding the time-series dimension. The impact of PNIR eligibility on log household expenditures per capita and child anthropometrics ($z-$scores). Initial expenditure classes defined on the basis of our baseline survey; simple 2-period DD estimates, various final periods (standard errors clustered at the CR level in parentheses).

Matching Procedure	Log expenditures per capita		Child anthropometrics z−score: Weight-for-age		Height-for-age		Weight-for-height	
	ATE (s.e.)	treated / control	ATE (s.e.)	treated / control	ATE (s.e.)	treated / control	ATE (s.e.)	treated / control
Nearest neighbor	0.006 (0.047)	1,844 / 742	0.157 (0.108)	1,039 / 454	0.117 (0.122)	1,039 / 454	0.139 (0.100)	1,039 / 454
Radius: $r = 0.001$	0.031 (0.036)	1,684 / 1,305	0.288 (0.076)	867 / 762	0.180 (0.086)	867 / 762	0.255 (0.074)	867 / 762
Radius: $r = 0.0001$	0.084 (0.055)	541 / 486	0.472 (0.131)	205 / 196	0.272 (0.153)	205 / 196	0.380 (0.129)	205 / 196
Stratification	0.031 (0.039)	1,844 / 1,448	0.212 (0.076)	1,039 / 911	0.086 (0.084)	1,039 / 911	0.234 (0.071)	1,039 / 911
Kernel	0.044 (0.036)	1,844 / 1,448	0.212 (0.072)	1,039 / 912	0.074 (0.081)	1,039 / 912	0.242 (0.072)	1,039 / 912
Number of observations in region of common support	3,292		1,950		1,950		1,950	
Original number of observations (Table 5)	3,458		2,069		2,069		2,069	

Table 10: Propensity score matching estimates of the impact of PNIR eligibility on log household expenditures per capita and child anthropometrics.

	Full sample	Control CRs	22 CRs that are treated by $t = 4$
	Mean (std.)	Mean (std.)	Mean (std.)
Village is PNIR-eligible	0.565		0.775
	(0.49)		(0.41)
Village chief:			
Identifies a village priority	0.739	0.782	0.722
that is PNIR eligible	(0.43)	(0.41)	(0.44)
Expects villagers will be	0.516	0.586	0.489
willing to contribute labor	(0.50)	(0.49)	(0.50)
Expects economic situation	0.041	0.010	0.052
to get worse over next 5 years	(0.19)	(0.10)	(0.22)
Village political capital:			
Villager is a member of the biggest			0.502
party on the Conseil rural			(0.50)
Villager is a member of the majority			0.429
party on the Conseil rural			(0.49)
Number of female villagers			0.457
on the Conseil rural			(0.82)
Observations	341	92	249
Time periods	5	5	5
Villages	71	19	52

Table 11: Summary statistics on village chief and village political capital IVs.

	Village chief identifies the village priority as being: water	health	school	Village chief believes economic situation has improved in the past	Village chief believes villagers will contribute only labor	Political mobilization: a functional CCG exists in the CR	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
No access in village to:							
Water	0.065 (0.06)						
Health center		−0.013 (0.05)					
School			−0.008 (0.02)				
Proportion of villagers who believe that economic situation has improved in the past				0.183 (0.05)			
Village chief believes village is very poor					0.038 (0.08)		
Village chief excluded IVs							
Village chief:							
Expects economic situation to get worse over next 5 years						0.014 (0.12)	0.005 (0.11)
Identifies a village priority that is PNIR eligible						−0.029 (0.05)	−0.058 (0.05)
Expects villagers will be willing to contribute only labor						−0.014 (0.05)	−0.021 (0.04)
Village political capital excluded IVs							
Village's stock of political capital on Conseil rural (in months)							0.001 (0.0006)
Villager is a member of the biggest party on the Conseil rural							0.203 (0.25)
Villager is a member of the majority party on the Conseil rural							−0.001 (0.36)
Observations	341	341	341	341	341	341	249
Time periods	5	5	5	5	5	5	5
Villages	71	71	71	71	71	71	52
CRs	36	36	36	36	36	36	22

Table 12: Heuristic tests of the validity of the village chief IVs, and of the link between village chief opinions and political capital variables and political mobilization. Period dummies, village covariates and village-specific fixed effects in all specifications (standard errors clustered at the village level in parentheses).

Dep. var.	Log expenditures per capita	Child anthropometrics z−scores Weight-for-age	Height-for-age	Weight-for-height
Estimator	Household FE	Child FE	Child FE	Child FE
	(1)	(2)	(3)	(4)
Full sample				
Least squares estimate	0.189	0.187	0.198	0.118
	(0.07)	(0.12)	(0.13)	(0.13)
IV estimate	0.102	0.762	1.063	0.218
	(0.19)	(0.38)	(0.48)	(0.38)
Test of the OID restrict.	0.011	0.199	0.434	1.044
[p−value]	[0.913]	[0.905]	[0.804]	[0.593]
Hahn-Hausman				
m_2 test statistic	−0.012	0.167	−0.246	−0.785
[p−value]	[0.990]	[0.867]	[0.805]	[0.432]
Observations	3,314	1,972	1,972	1,972
(treated)	(1,844)	(1,039)	(1,039)	(1,039)
Time periods	5	5	5	5
CRs	36	36	36	36
Villages	71	71	71	71
Households	754	495	495	495
Children		981	981	981
Balanced sample				
Least squares estimate	0.159	0.166	0.203	0.082
	(0.07)	(0.13)	(0.14)	(0.15)
IV estimate	0.036	0.745	1.030	0.198
	(0.22)	(0.43)	(0.53)	(0.42)
Test of the OID restrict.	0.533	0.270	0.318	1.060
[p−value]	[0.465]	[0.873]	[0.852]	[0.588]
Hahn-Hausman				
m_2 test statistic	0.446	0.339	−0.090	−0.291
[p−value]	[0.655]	[0.734]	[0.928]	[0.770]
By initial expenditure class				
poor	−0.137	2.396	1.794	1.498
	(0.38)	(0.85)	(0.92)	(0.83)
middle class	0.042	0.468	0.685	0.235
	(0.24)	(0.42)	(0.53)	(0.43)
rich	0.236	−0.511	1.380	−2.322
	(0.65)	(1.23)	(1.33)	(1.45)
Observations	2,746	1,705	1,705	1,705
(treated)	(1,509)	(900)	(900)	(900)
Time periods	5	5	5	5
CRs	36	36	36	36
Villages	71	70	70	70
Households	572	390	390	390
Children		804	804	804

Table 13: Instrumental variables (Fuller) estimates of the impact of completed PNIR projects on log household expenditures per capita and child anthropometrics (z−scores). Initial expenditure classes defined on the basis of our baseline survey (standard errors clustered at the village level in parentheses). Village-, household- and child-specific covariates included, as applicable.

Dependent variable: PNIR project completed in village

Observation	Household		Child	
Estimator	Household FE		Child FE	
Sample	Full	Balanced	Full	Balanced
	(1)	(2)	(3)	(4)
Village chief excluded IVs				
Village chief:				
Expects economic situation to get worse over next 5 years			−0.257 (0.12)	−0.234 (0.10)
Identifies a village priority that is PNIR eligible	−0.031 (0.01)	−0.027 (0.01)		
Expects villagers will be willing to contribute only labor			0.080 (0.05)	0.092 (0.05)
Village is in a PNIR-eligible CR	0.334 (0.02)	0.310 (0.02)	0.363 (0.08)	0.341 (0.08)
Weak IV diagnostics:				
Partial F	103.45	79.25	8.74	7.92
Partial R^2	0.075	0.068	0.120	0.115
Observations (treated)	3,314 (1,844)	2,746 (1,509)	1,972 (1,039)	1,705 (900)
Time periods	5	5	5	5
CRs	36	36	36	36
Villages	71	71	71	70
Households	754	572	495	390
Children			981	804

Table 14: The determinants of completed PNIR projects: reduced form equations (standard errors clustered at the village level in parentheses). Village-, household- and child-specific covariates included, as applicable.

Dep. var.	Log expenditures per capita	Child anthropometrics $z-$scores		
		Weight-for-age	Height-for-age	Weight-for-height
Estimator	Household FE	Child FE	Child FE	Child FE
	(1)	(2)	(3)	(4)
Full sample				
Least squares estimate	0.190 (0.08)	0.089 (0.13)	0.020 (0.16)	0.127 (0.14)
IV estimate	0.651 (0.16)	1.086 (0.61)	0.113 (0.71)	1.242 (0.65)
Test of the OID restrict. [$p-$value]	2.350 [0.308]	0.677 [0.712]	1.129 [0.568]	0.025 [0.987]
Hahn-Hausman				
m_2 test statistic [$p-$value]	−1.052 [0.292]	−0.143 [0.885]	−0.761 [0.446]	0.432 [0.665]
Observations (treated)	2,403 (1,844)	1,315 (1,039)	1,315 (1,039)	1,315 (1,039)
Time periods	5	5	5	5
CRs	22	22	22	22
Villages	52	52	52	52
Households	550	343	343	343
Children		663	663	663
Balanced sample				
Least squares estimate	0.155 (0.08)	0.080 (0.15)	0.062 (0.16)	0.072 (0.16)
IV estimate	0.603 (0.18)	0.750 (0.63)	0.314 (0.74)	0.650 (0.65)
Test of the OID restrict. [$p-$value]	3.540 [0.17]	1.366 [0.504]	1.946 [0.377]	1.202 [0.548]
Hahn-Hausman				
m_2 test statistic [$p-$value]	−2.402 [0.016]	−0.799 [0.424]	−1.374 [0.169]	−0.640 [0.522]
By initial expenditure class				
poor	0.385 (0.37)	1.916 (1.09)	0.672 (1.10)	1.760 (0.92)
middle class	0.245 (0.17)	0.263 (0.79)	−0.032 (0.95)	0.237 (0.86)
rich	−0.251 (0.28)	0.805 (3.88)	1.911 (4.82)	−0.908 (4.26)
Observations (treated)	1,957 (1,509)	1,127 (900)	1,127 (900)	1,127 (900)
Time periods	5	5	5	5
CRs	22	22	22	22
Villages	52	51	51	51
Households	409	266	266	266
Children		532	532	532

Table 15: The 22 CRs that become PNIR-eligible by $t = 4$. Instrumental variables (Fuller) estimates of the impact of completed PNIR projects on log household expenditures per capita and child anthropometrics ($z-$scores). Initial expenditure classes defined on the basis of our baseline survey (standard errors clustered at the village level in parentheses). Village-, household- and child-specific covariates included, as applicable.

Dependent variable: PNIR project completed in village				
Observation	Household		Child	
Estimator	Household FE		Child FE	
Sample	Full	Balanced	Full	Balanced
	(1)	(2)	(3)	(4)
Village political capital excluded IVs				
Villager is a member of the biggest party on the Conseil rural	−0.309 (0.03)	−0.309 (0.04)	−0.351 (0.07)	−0.304 (0.17)
Villager is a member of the majority party on the Conseil rural	0.782 (0.07)	0.788 (0.08)	0.633 (0.17)	0.490 (0.29)
Number of female villagers on the Conseil rural	0.232 (0.01)	0.235 (0.01)	0.145 (0.03)	0.165 (0.03)
Weak IV diagnostics:				
Partial F	222.15	193.95	19.78	9.71
Partial R^2	0.153	0.154	0.044	0.047
Observations (treated)	2,403 (1,844)	1,957 (1,509)	1,315 (1,039)	1,127 (900)
Time periods	5	5	5	5
CRs	22	22	22	22
Villages	52	52	52	51
Households	550	409	343	266
Children			663	532

Table 16: The 22 CRs that become PNIR-eligible by $t = 4$. Determinants of completed PNIR projects: reduced form equations (standard errors clustered at the village level in parentheses). Village-, household- and child-specific covariates included, as applicable.

Dep. var.	Log expenditures per capita	Child anthropometrics z−scores		
		Weight-for-age	Height-for-age	Weight-for-height
Estimator	Household FE	Child FE	Child FE	Child FE
	(1)	(2)	(3)	(4)
Full sample				
Least squares estimate				
Agriculture	0.162 (0.214)	0.314 (0.213)	0.231 (0.320)	0.298 (0.226)
Health	0.116 (0.127)	−0.137 (0.157)	−0.177 (0.248)	0.028 (0.189)
Potable water	0.113 (0.212)	0.031 (0.295)	−0.024 (0.180)	−0.034 (0.417)
Education	0.289 (0.103)	0.301 (0.227)	0.197 (0.173)	0.238 (0.206)
IV estimate				
Agriculture	1.057 (0.485)	1.692 (0.543)	0.942 (0.897)	1.179 (0.381)
Health	−1.282 (1.115)	0.923 (1.110)	−0.023 (1.266)	1.353 (1.158)
Potable water	0.962 (1.873)	0.695 (1.283)	−0.861 (1.697)	1.247 (1.568)
Education	1.201 (0.464)	0.388 (1.081)	−1.143 (1.789)	1.168 (0.882)
Test of the OID restrict. [p−value]	0.762 [0.382]	0.693 [0.404]	1.511 [0.218]	0.059 [0.807]
Hahn-Hausman m_3 test statistic [p−value]	0.753 [0.451]	0.397 [0.691]	−0.298 [0.765]	0.953 [0.340]
Observations (treated)	2,394 (1,844)	1,002 (792)	1,002 (792)	1,002 (792)
Time periods	5	5	5	5
CRs	22	22	22	22
Villages	52	52	52	52
Households	550	343	343	343
Children			663	663

Table 17: The 22 CRs that become PNIR-eligible by $t = 4$. Instrumental variables (Fuller) estimates of the impact of completed PNIR projects on log household expenditures per capita and child anthropometrics (z−scores): Disaggregating by project type (standard errors clustered at the village level in parentheses). Village-, household- and child-specific covariates included, as applicable.

Estimator	Agriculture: Hh. FE (1)	Agriculture: Child FE (2)	Health: Hh. FE (3)	Health: Child FE (4)	Education: Hh. FE (5)	Education: Child FE (6)	Water: Hh. FE (7)	Water: Child FE (8)
	Dependent variable: PNIR project completed in village involving							
Village chief excluded IVs								
Village chief:								
Identifies a village priority that is PNIR-eligible	−0.022	−0.011	−0.012	0.047	0.032	−0.016	−0.018	0.030
	(0.010)	(0.015)	(0.015)	(0.024)	(0.014)	(0.015)	(0.008)	(0.023)
Expects villagers will be willing to contribute only labor	0.038	0.026	−0.082	−0.047	0.063	0.078	0.060	0.011
	(0.010)	(0.015)	(0.015)	(0.025)	(0.014)	(0.015)	(0.008)	(0.023)
Village political capital excluded IVs								
Villager is a member of the biggest party on the Conseil rural	−0.060	−0.064	−0.058	0.025	−0.148	−0.027	−0.045	−0.288
	(0.050)	(0.071)	(0.073)	(0.114)	(0.071)	(0.072)	(0.041)	(0.107)
Villager is a member of the majority party on the Conseil rural	0.720	0.617	0.060	0.022	−0.025	0.015	0.0006	−0.026
	(0.073)	(0.111)	(0.108)	(0.179)	(0.104)	(0.113)	(0.061)	(0.169)
Number of female villagers on the Conseil rural	0.013	−0.008	0.036	0.111	0.167	0.054	0.017	−0.010
	(0.007)	(0.013)	(0.010)	(0.022)	(0.010)	(0.014)	(0.005)	(0.020)
Weak IV diagnostics:								
Partial F	34.55	9.11	8.89	6.84	62.08	7.97	13.26	2.81
Partial R^2	0.086	0.066	0.023	0.051	0.144	0.059	0.034	0.021
Observations	2,403	1,315	2,403	1,315	2,403	1,315	2,403	1,315
(treated)	(1,844)	(1,039)	(1,844)	(1,039)	(1,844)	(1,039)	(1,844)	(1,039)
Time periods	5	5	5	5	5	5	5	5
CRs	22	22	22	22	22	22	22	22
Villages	52	52	52	52	52	52	52	52
Households	550	343	550	343	550	343	550	343
Children		663		663		663		663

Table 18: The 22 CRs that become PNIR-eligible by $t = 4$. Determinants of completed PNIR projects: reduced form equations (standard errors in parentheses). Village-, household- and child-specific covariates included, as applicable.