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The Food (In)Security Impact of Land Redistribution in South Africa: Microeconometric Evidence from National Data

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Summary. — The South African land-reform program has been widely criticized for its slow pace as well as its apparent lack of contribution to poverty reduction. No econometric evidence of the impact of land transfers has been provided to date and this paper attempts to fill this gap by considering the impact of receiving a land grant on households' food insecurity. Propensity score matching and univariate probit estimates using two national household surveys indicate that, on average, land grant recipients are more food insecure than comparable non-participants. Recursive bivariate probit estimates suggest that selection bias is not driving this result. © 2009 Elsevier Ltd. All rights reserved.

1. INTRODUCTION

Long before *apartheid* began, the 1913 Land Act initiated the process of confinement of the black population of South Africa into specific areas that represented less than 10% of the country's land, away from the cities and farms of the White. As a consequence of this policy and the many that followed, the majority of the population were concentrated in overcrowded reserves. Post-*apartheid* South Africa was therefore confronted with the glaring need for the previously disadvantaged to be provided with land access for housing purposes, agricultural, and non-agricultural activities. A three-component land-reform policy was devised, with the aim to redistribute 30% of the country's agricultural land from white landowners to black people. The restitution component was to tackle the legal claims of people who were dispossessed of their land after 1913. Land tenure reform was aimed at securing people's land rights to the land they already occupied on an informal basis. Finally, the redistribution component was intended as the main instrument of this ambitious land reform, and consisted of distributing land grants allowing black people to buy land from white willing sellers. The scope of the latter component was wide-ranging, as it aimed to "include the urban and rural very poor, labor tenants, farm workers as well as new entrants to agriculture" "for residential and productive uses, to improve their livelihoods and quality of life" (Department of Land Affairs, 1997).

Land reform was not only seen as the "central and driving force of a program of rural development" (African National Congress, 1994), but also seen by many observers as a crucial measure for the development of the country as a whole. Binswanger and Deininger (1993), for instance, suggest that "substantive and rapid market-assisted land reform and resettlement is the greatest if not the only hope of peaceful development in South Africa". But 15 years after the emergence of the "new South Africa" the actual rate of redistribution has fallen short of expectations and there are doubts regarding the achievement of the program's expected welfare outcomes.

Although much has been written about the impact of land redistribution in South Africa, no econometric evidence has been provided so far, probably due to data scarcity. Using data from two national household surveys carried out by the national statistics agency (Statistics South Africa), this paper aims to fill this lacuna by estimating the impact of having benefited from a land grant on the household's self-reported food insecurity.

In South Africa, where 43% of the population suffer from food poverty (Rose & Charlton, 2002), food security was identified as the "primary determinant of the well-being of people directly affected by land reform" in a Quality of Life Survey commissioned by the Department of Land Affairs to evaluate the impact of land reform (Ahmed *et al.,* 2003). Therefore, an important dimension of the livelihoods improvement expected from land reform is food security, or the ability of all the household members to "at all times have physical, social, and economic access to sufficient, safe, and nutritious food that meets their dietary needs and food preferences for an active and healthy life" (FAO, 2001). Over and above its immediate impact on well-being, malnutrition has substantial long term effects on health, which in turn affects productivity and income at both the micro- and macroeconomic levels (see Weil, 2007). This explains the special attention given to hunger in the first UN Millennium Goal of halving the proportion of people below the one dollar a day poverty line *and* halving the proportion of people who suffer from hunger. In the face of the alarming trend in world food prices, the latter goal has recently received renewed attention.

This paper investigates the effect of land redistribution on household food insecurity through the use of two household surveys. The Labour Force Survey (LFS) allows us to consider the impact on the probability for households to have experienced "difficulties in satisfying their food needs" during the 12 months preceding the survey, whilst data from the General Household Survey (GHS) permits considering the impact on

the probability for children and adults to have gone "hungry because there was not enough food" in the 12 months preceding the survey.

Section 2 presents the theoretical background and relevant existing evidence, Section 3 describes the data, Section 4 details the econometric approach, Section 5 contains the estimation results, which are discussed in Section 6. Section 7 concludes.

2. THEORY AND EXISTING EVIDENCE

(a) *The poverty-reduction impact of land redistribution at the household level: theory and international evidence*

It is widely accepted that improved access to land is good for the poor, in particular in terms of food security. There are many arguments supporting the idea that food insecurity may be reduced through broadening land access, especially if increased land *ownership* rather than just land use is achieved. Firstly, income should increase with land access by the direct income value of additional production or renting out of land. In the presence of labor market constraints, increased land access should also increase returns to family labor. Furthermore, improved land ownership may relax credit constraints and hence allow households to undertake profitable but lumpy investments, therefore preventing them from remaining stuck in a "poverty trap" or, from the perspective of endogenous class formation models, in a lower income class. [1] Increased land ownership should also help to reduce vulnerability to shocks, due to the larger savings and enhanced insurance access enabled by higher income and, if land is a liquid asset, through the ability to sell this asset in the face of a shock. Finally, a change in the property rights over land on which a household is already producing may improve the household's returns to the land by making them the residual claimants. Ultimately, food security is expected to be enhanced by land redistribution indirectly, through higher and/or more secure income, but also directly when markets are imperfect.

The idea that broader access to land, and, a fortiori, that broader land ownership is good for the poor is therefore largely supported by economic theory. However, the literature on land reform has been dominated by debate about the existence of an inverse relationship between farm size and productivity (as remarked by Carter, 2003; Besley & Burgess, 2000) and hence the possibility of achieving agricultural efficiency as well as equity through land redistribution, rather than by the question of the actual impact of land redistribution on its beneficiaries. Evidence is especially rare at the household level due to data scarcity. Using panel data for Indian states during 1958–92, Besley and Burgess (2000) find that land reform has decreased poverty at the state level, but this appears to be due to a change in production relations rather than to land *redistribution*. However, a recent study of land reform in India by Deininger, Jin, and Nagarajan (2007) finds that actual redistribution measured at the state level had no effect on education but significantly increased income, consumption and non-land assets at the household level. The existence of a panel dataset of land-reform beneficiaries for Zimbabwe has allowed several evaluation studies to be carried out. They constitute the only published instances of econometric analyses of the effect of land reform at the household level. The latest estimation exercise based on these data is Deininger, Hoogeveen, and Kinsey (2004). Matching Zimbabwean households who resettled in the early 1980s with a control group of rejected applicant households, these authors find that

participation in land reform increased *per capita* expenditure by US$17 [2] per year in 1997–99.

With the recent reappearance of land redistribution as a priority in the development policy agenda, as illustrated by the publication in 2003 of a "Land Policies for Growth and Poverty Reduction" report by the World Bank (Deininger, 2003) and by a wave of vast land reforms in Colombia, Brazil, and South Africa, it seems essential to monitor closely the impact of recent or ongoing land redistribution programs on their beneficiaries. In addition, given the current concerns with international food scarcity, it is important to investigate the impact that policies such as land reform can have on food security.

(b) *Land redistribution in South Africa*

The theory and international evidence on the impact of land redistribution on its direct beneficiaries is a useful yardstick for the South African case, but the specificity of the South African context is worth emphasizing for at least four reasons. First, the main tool for the transfer of land ownership, namely the land *redistribution* component, encompasses a disparate set of needs, since it "aims to provide the disadvantaged and the poor with access to land for residential and productive purposes. Its scope includes the urban and rural very poor, labor tenants, farm workers as well as new entrants to agriculture" (Department of Land Affairs, 1997). [3] In addition to this diversity of needs, the South African context also differs from the typical land-reform environment because of the lack of farming experience among the targeted group (Bradstock, 2005; Cross, Mngadi, Sibanda, & Jama, 1996), which comes as a consequence of the impossibility of practicing agriculture on a substantial scale in the former reserves. [4] Third, often long distances separate the beneficiaries' current place of residence and the land of which they acquire ownership. Fourth, beneficiaries were only given about R15,000 per household until mid-2001, whilst commercial farms have evolved to be generally quite large due to past agricultural policies, so that beneficiaries have had to pool their grants and acquire farms as an entity of anything up to several hundreds of households. Indeed, subdivision of farmland is still restricted in South Africa (see van den Brink, Thomas, Binswangor, Bruce, & Byamugisha, 2006). Despite a policy change in 2001, since which the amount of the individual grant can go up to R100,000 if a beneficiary contributes R400,000, pooling is still the rule as most applicants bring none or very little financial contribution and legal barriers to subdivision imply that most farms on sale are too large for acquisition by single individuals. Fifth, business plans drawn by consultants as part of the land grant application process have promoted the use of redistributed farms as a single, capital-intensive collective enterprise, in line with the organization of production under the previous owner (van den Brink, 2006).

In the light of these specificities, it is especially important to investigate empirically the impact of land redistribution on its beneficiaries, since the multiplicity of policy objectives assigned to a single instrument (land grants), the lack of farming experience and familiarity with the land obtained, the distance problem, and the collective nature of ownership and production, are likely to offset at least some of the expected benefits from land transfers.

To date, about 5% of the country's land has been redistributed under the land-reform program (Lahiff, 2008), as opposed to the initial target of redistributing 30% of the country's agricultural land by 1999. As well as the lack of actual redistribution, doubts have been raised with respect to the impact of land redistribution on the livelihoods of its

beneficiaries. Indeed, benefiting from land redistribution does not seem to be contributing to the livelihoods of a substantial share of the households involved: in 2001, no more than 34% of projects surveyed by the Department of Land Affairs paid any salary to land grant holders working on the project, even when they worked full-time (Ahmed et al., 2003). In addition, only 8.1% of the beneficiary households surveyed by Ahmed et al. (2003) report achieving a higher income, and only 11.1% achieving a more secure income as a consequence of participation in land redistribution. In his case studies in the Northern Cape, Bradstock (2005) finds that while household incomes have increased during the period of observation (2001–03), agricultural income from land redistribution is not the cause of this increase. In his study of communal land redistribution projects (Communal Property Associations) carried out during 1999–2001 in Limpopo, McCusker (2002) finds that "change in livelihoods as a result of land reform [is] minimal largely due to general disorganization, farm size problems, lack of capital, lack of skills and labor, gender bias, and skewed age distribution" (p. 113). More specifically, he reports that only 21.1% feel that their income has increased, whereas 55.8% of respondents find that their income has stayed the same, and, more worryingly, 23.1% find that it has dropped (McCusker 2002, p. 117). Citing an unpublished report elaborated for the Department of Land Affairs (May, Roberts, Govender, & Gayadeen, 2000), Andrew, Ainslie, and Shackleton (2003) state that "in many projects, no production is happening and some beneficiaries are worse-off". Indeed, many beneficiaries do not use their redistributed land for productive purposes. Several factors can account for this phenomenon, including coordination problems between co-beneficiaries, lack of know-how and other complementary resources, discouragement, and/or because they were simply used by the project leaders to make-up the needed numbers to obtain enough grants to cover the price of the farm. The impression that benefits are generally small or non-existent is confirmed by Lodge (2003), Aliber (2003), and van den Brink (2006). In the only academic study providing numerical evidence on the actual revenue for participants in land-reform projects, Deininger and May (2000) find that the median gross annual revenue per beneficiary (incomes minus expenditures on variable inputs, divided by beneficiary group size) is equal to R10,552 in the 16% of land-reform projects classified as "high-revenue" – this corresponds roughly to twice the annual per capita poverty line in 2000 prices. But for the remaining 84% of projects surveyed, the median gross annual revenue per beneficiary is in fact slightly negative (-R9). Given the average size of "high-" and "low-revenue" projects (9.14 and 27.56, respectively) in Deininger and May (2000), this suggests that, for a large majority of beneficiaries in their sample, there is no positive profit to be distributed between land, labor, and management, thus indicating that only a small minority of beneficiaries could be expected to obtain an income from their project at that early stage.

In addition, there are more specific concerns regarding the ability of the poor to benefit from land reform. These concerns have been strongly reinforced since 2001 with the shift away from SLAG (Settlement and Land Acquisition Grant) toward LRAD (Land Redistribution for Agricultural Development) grants. SLAG was a uniform R15,000 (and later R16,000) grant subject to households earning no more than R1500 per month, [5] whereas LRAD is a scheme allowing any black individual to apply for a land-purchase grant that increases (in absolute terms) with their own contribution. [6] Even before LRAD was introduced, concerns had been raised about the cost for the poor of relocating to the land acquired.

Zimmerman (2000) argues that these costs are likely to have a deterrent effect on participation, but they can also be thought of as preventing the poor from generating income from any newly acquired land. For instance, Bradstock (2005) and Wegerif (2004) emphasize the cost to households on traveling to their agricultural land. Furthermore, some suspect that the benefits from land redistribution may have disproportionately profited an "élite" group. Bradstock (2005) finds inequitable access to land, with the project's richest tercile having a mean holding five times larger than that of the other terciles (p. 1985). In addition, Hall, Jacobs, and Lahiff (2003) report that in the Western Cape, LRAD beneficiaries at the bottom of the grant scale have accessed 3 ha of land on average, as opposed to 88 ha for "well-resourced" beneficiaries. Cross et al. (1996) mention instances of power manipulation by the communities' élite, who used the opportunity of being turned into land administrators to appropriate land (p. 153). And Wegerif (2004) suggests that, despite many of Limpopo LRAD beneficiaries being mainly poor, "they were not without useful connections" which were crucial for their participation in land redistribution (p. 37). However, Deininger and May (2000) draw different lessons about the program's targeting. Using data from a national survey conducted in 1999 on 1168 randomly selected beneficiaries in 87 land-reform projects and comparing these with the black respondents of the 1993 PSLDS survey carried out by the Southern African Labour and Development Research Unit, they argue that the land-reform program was well targeted at the poorest and most vulnerable, although this view is not consensual (Sender & Johnston, 2004). With respect to the impact of participation rather than the program's targeting, and contrary to most commentators, it is interesting to note that Deininger and May (2000) find that the share of expenditure-poor beneficiaries is significantly higher in high-revenue projects (81.4%) than in unsuccessful projects (73.7%), suggesting that poorer participants may be more likely to derive benefits from participation when they are involved.

3. DATA

(a) Description of datasets

The Labour Force Survey (LFS) has been conducted by Statistics South Africa every six months since February 2000. Four waves, September 2001, 2002, 2003, and 2004, allow for the estimation of the impact of receiving a land grant on the household's probability to satisfy their food needs. Despite being advertized as a panel dataset and 80% of the sample of each wave being interviewed again in the following wave, a given household cannot be identified between rounds, which precludes the use of panel data techniques. On the other hand, results obtained when pooling the four available waves together suffer from the caveat that many households are sampled twice or even three times in these waves (although no household sampled in the 2001 wave could still be interviewed in the 2004 wave). Standard errors are artificially underestimated in the pooled data estimation if the correlation between the error terms for the same household in two different survey rounds is ignored. Therefore, I present results obtained with each wave separately.

As a consequence of the cross-sectional nature of the data, it is not possible to estimate the impact of receiving a land grant by comparing beneficiary welfare before and after participation, which warrants attention to the usual estimation biases. In addition, since the issue of land redistribution is only

peripheral in the LFS, one is confronted with several difficulties when trying to estimate the impact on welfare of benefiting from the land redistribution policy. Firstly, the sampling of observations is based on the population census and income strata and not on the number of beneficiaries per geographical unit or/and type of land acquisition scheme, so that the sample of beneficiaries in the LFS is not necessarily representative of the land grant beneficiary population. For instance, beneficiaries of potentially more successful compartments of land transfers, such as shared-equity schemes, could be under-sampled. However, there is no reason why it should be regarded as non-randomly biased toward a certain type of beneficiaries. Moreover, there is no land-reform specific module, so that we do not have information regarding land use and characteristics, access to complementary factors and extension services, participation-related costs and benefits, or the date at which the land grant has been received. Therefore, we cannot distinguish between heterogeneous beneficiaries, for example, according to whether or not they are using the land transferred. As a consequence, the data do not allow a close investigation of the different channels through which participation in land-reform impacts on household welfare, nor do they allow scrutiny of the potentially heterogeneous impact of the reform on different types of beneficiaries, so I focus instead on the global effect on food security status.

Despite these limitations, and given the scarcity of data on the impact of land redistribution, the LFS has enviable features: (i) it provides a control group of non-beneficiary households, which represents a significant advance on the previous literature; (ii) it is a large dataset, therefore providing substantial degrees of freedom and a large pool of observations for matching purposes; (iii) it covers a comprehensive range of questions, thus offering a wealth of controls; (iv) there are four LFS rounds that can be used to estimate the effect of receiving a land grant on household food security, which allows checking the robustness of the results obtained.

The General Household Survey has been conducted every July since 2002, and focuses on living standards and access to public services and infrastructure. The data limitations of the LFS also apply here. In addition, there are only two survey rounds in which respondents were asked about whether they had received a land grant (2002 and 2003), and detailed ethnicity, which we will see is an important determinant of both participation in land reform and food insecurity, cannot be derived from this survey. However, a new sample, distinct from that of the LFS, is drawn for each round, and the questions relating to food insecurity are more explicit than the LFS's (see next section). Therefore, the GHS 2002 and 2003 are used to check the robustness of results obtained with the LFS to a change of sample and food insecurity variable.

(b) Variables

(i) Dependent variables

The importance of evaluating the impact of land redistribution on food insecurity was motivated in the introduction. It appears all the more important to consider this impact here as in all LFS waves, there is a much higher proportion of households reporting difficulties in satisfying their food needs among those who report having received a land grant (see Table 1).

It may be worth noting why I do not also estimate the impact of receiving a land grant on usual welfare indicators such as income or expenditure. The main reason for not doing so has to do with data limitations. In the LFS, we only have information on income from the individuals' *main* activity,

Table 1. *Food insecurity amongst beneficiaries and non-beneficiaries*

	Food secure	Food insecure	Total
LFS2001			
Non-beneficiaries	55%	45%	100% (23,833)
Beneficiaries	43%	57%	100% (652)
Total	55%	45%	100% (24,485)
LFS2002			
Non-beneficiaries	61%	39%	100% (22,849)
Beneficiaries	46%	54%	100% (543)
Total	60%	40%	100% (23,392)
LFS2003			
Non-beneficiaries	69%	31%	100% (23,328)
Beneficiaries	56%	44%	100% (337)
Total	69%	31%	100% (23,665)
LFS2004			
Non-beneficiaries	65%	35%	100% (24,841)
Beneficiaries	57%	43%	100% (783)
Total	65%	35%	100% (25,624)

Source: The author's calculations based on the sample of households headed by a black individual in Statistics South Africa's LFS September 2001–2004. Sample size in parentheses.

which is likely to exclude most income generated by land transfers and is therefore not relevant for the purpose of this study. Furthermore, there is one question about household expenditure but this (i) does not include explicitly own produce and payments in kind, and (ii) provides very limited information about the respondents' expenditure level since they are only asked to report a R400-wide expenditure interval for the month preceding the survey. This interval is very wide compared to the poverty line, which lies between R322 and R593 *per capita* per month in 2000 prices according to Hoogeveen and Ozler (2006). For most beneficiaries, the activities carried out on newly acquired land are only one aspect of their livelihood strategies, and one essential benefit to be expected for participants is the consumption of their own produce and natural resources collection such as firewood and plants. In other words, it would be difficult to judge whether a decrease in expenditure, which could be due to the household producing themselves a larger share of the goods consumed, would be a good outcome or a bad outcome.

The food insecurity measure used in the analysis of LFS data is derived from the self-reported inability of the household to satisfy the food needs of its members in the 12 months preceding the survey. The dependent variable is a binary variable that takes the value one when the household reports difficulties in satisfying their food needs at least "sometimes", and zero otherwise ($F1$). A positive coefficient on a regressor therefore means that it *increases* food insecurity. Qualitatively similar findings are obtained when the dependent variable is an ordinal variable equal to 1 for households reporting "never" having experienced difficulties in satisfying their food needs in the past 12 months, 2 if "seldom", 3 if "sometimes", 4 if "often", and 5 if "always". Although less information-rich, the binary variable was preferred because the marginal effect of participation is more conveniently interpreted, as well as due to methods for the estimation of a bivariate model being more readily available for a binary variable rather than an ordered variable. The questions related to food insecurity are different in the GHS compared to those in the LFS. In the former, the questions about food insecurity are: "In the past 12 months, did any adult in this household go hungry because there wasn't enough food?" and "In the past 12 months, did

any child (17 or younger) in this household go hungry because there wasn't enough food?", with answers ranging from "never" to "always". The dependent variable used in the GHS regressions is a binary variable equal to one if either children or adults are reported to have gone hungry at least "sometimes".

The reliability of this type of self-reported food insecurity measure is well documented both in developed and developing countries. It has been shown that answers to food insecurity questions are rather well correlated with measures of adults' actual nutritional outcomes [7] such as a healthy eating index, Body Mass Index, and low energy and nutrient intake when controlling for household socio-economic characteristics. Melgar-Quiñonez et al. (2006) also find strong evidence of the correlation between self-reported food insecurity measures and (i) highly nutritive food expenditure in all their study areas, and (ii) total food expenditure in all study areas except rural Burkina-Faso. In addition, authors who have compared self-reported food insecurity with the food insecurity status of households based on in-depth interviews find a strong correspondence between the two classifications (e.g., Frongillo, 1999; Frongillo & Nanama, 2006; Wolfe, Olson, Kendall, & Frongillo, 1998). Most of these studies use a composite measure of self-reported food insecurity based on answers to several food insecurity questions (Bhattacharya et al., 2004, is an exception), but there is evidence of a strong correlation between answers to different food insecurity questions. [8]

However, given the self-reported nature of the dependent variables, there may be a degree of misreporting. For instance, some respondents might be tempted to over-report food insecurity in the hope of obtaining government assistance. But potential misreporting only constitutes a problem for the present analysis if the direction or extent of misreporting is correlated with participation, or, in other words, if there is an unobserved tendency to over- or under-report food insecurity systematically associated with participation. If this is the case, then it can be thought of as one particular instance of unobserved variable bias which, as we shall see, is dealt with using recursive bivariate probit.

(ii) *Regressors*

The main regressor of interest is a binary variable equal to one if the household answers "yes" to the question "Did the household receive a land grant to obtain a plot of land for residence or for farming?" and zero otherwise (LG). While the term "grant" is usually used with reference to the redistribution component of the land-reform program, land restitution beneficiaries have also generally received a small "Restitution Discretionary Grant" of R3,000 *along with* the original land restored or compensatory land (Hall, 2004).

The other regressors have been selected following the empirical literature on nutritional status and food insecurity. The regressors used in the literature (e.g., in Bhattacharya *et al.*, 2004; Rose, 1999; Smith, Ruel, & Ndiaye, 2005) to explain food insecurity can generally be grouped under three categories: socio-economic status, household composition, and cultural attitudes. All these variables can be expected to have an impact on both participation and food insecurity, and should therefore be included in the regressions in order to isolate the effect of participation.

However, the inclusion of socio-economic characteristics that can be caused by participation (e.g., current expenditure) should be avoided for two reasons. First, this inclusion could bias the estimates since these characteristics may be correlated with the residual term. Second, it would make the interpretation of the coefficient on the participation dummy ambiguous.

For instance, if we think that benefiting from a land grant has increased the beneficiary households' income, and that this increase in income has in turn reduced food insecurity, it could be argued that the coefficient on the participation dummy is only picking up the effect of having received a land grant over and above its effect on other income-related regressors. I therefore focus on variables that are unlikely to be affected by participation. Namely, socio-economic status is proxied by a set of dummies indicating the educational attainment of the household head, the gender of the household head, and whether the household receives any welfare grants. [9]

Demographic characteristics included in the regressions are: household composition dummies (whether couple with/without child, single parent, or else); the number of children aged less than 5; the number of children aged less than 15; the number of total household members and its square; age and square of the age of the household head. I also include a set of binary variables capturing ethnicity (as defined by the main language spoken at home) aimed at controlling for cultural attitudes, which could be correlated with the tendency to *report* existing food insecurity (for instance due to the potentially different degree of stigmatization of food insecurity, or differences in the exact meaning given to the wording of the question in different languages) as well as with the tendency of households to experience such problems (due, e.g., to diversity in informal insurance mechanisms). Finally, I include province fixed effects in order to control for province-specific factors, either relevant to food insecurity (e.g., food prices) or to participation in land reform (quality of land, infrastructure, and post-settlement support). This is particularly important insofar as land-reform implementation is largely decentralized at the provincial level.

4. ECONOMETRIC APPROACH

As mentioned earlier, the main difficulty with estimating the impact of receiving a land grant on household food insecurity with the present cross-sectional data has to do with the difficulty of comparing like with like when the counterfactuals are not the same households before participation, but non-participants observed at the same point in time. Three types of biases can arise: (i) a bias due to a difference in supports, that is, to differences in the set of values of characteristics at which participants and controls are observed, (ii) a bias due to a mis-weighting of observations due to diverging distributions of characteristics between participants and controls, (iii) a bias due to differences in unobserved characteristics or "selection bias" (Heckman, Ichimura, Smith, & Todd, 1998). For brevity, I refer to biases (i) and (ii) as the "observed variable bias" and to bias (iii) as the "unobserved variable bias". I first focus on my strategy to deal with the observed variable bias (propensity score matching), before turning to my approach to tackle unobserved variable bias (recursive bivariate probit).

(a) *Dealing with observed variable bias: propensity score matching*

Section 5a presents propensity score matching estimates of the average treatment effect on the treated of having received a land grant on household food insecurity. The main principle behind Propensity Score Matching (PSM) estimation is to evaluate the impact of a binary treatment (having received a land grant LG) on an outcome variable (food insecurity status FI) by comparing the observed value

for this outcome variable between households who have benefited from the treatment and households who have not, but who are found to be sufficiently similar, according to a set of observed variables X, to act as controls. By construction, this procedure does away with bias (ii) and, when the sample is restricted to the region of common support as it is the case here,[10] also removes bias (i) (Heckman *et al.,* 1998).

Define the Average Treatment Effect on the Treated (ATT) as:

$$ATT = E[FI_1|X, LG = 1] - E[FI_0|X, LG = 1],$$

where the subscript on *FI* indicates the treated (FI_1) and untreated (FI_0) outcomes.

Also define the propensity score $p(X) = Prob[LG = 1|X]$. Rosenbaum and Rubin (1983) show that, if outcomes without the intervention are independent of exposure to treatment within cells defined by X, then these outcomes are also independent of exposure to treatment within cells defined by values of $p(X)$, that is, $FI_0 \perp LG|p(X)$. The latter condition is often referred to as the unconfoundness assumption, and implies that there is no unobserved variable bias after conditioning on $p(X)$.

The propensity score $p(X)$ can be computed using a binary model (here, probit), so as to evaluate the probability of household j to receive a land grant conditional on the set of pre-treatment covariates X. In this paper, participants are then matched with the non-participant household with closest propensity score (nearest-neighbor matching), and the ATT is non-parametrically estimated as $ATT = \frac{1}{B}\sum_{j=1}^{B}(FI_{1j} - FI_{0ij})$, where FI_{0ij} refers to non-beneficiary household i matched with beneficiary household j, and B is the total number of beneficiaries.

Heckman *et al.* (1998) find that observed variable bias, which is removed by matching, is by far the largest source of bias in their data. However, the respective sign and size of each bias is bound to vary between datasets, so that it is important to check whether the PSM estimates could be led by unobserved variable bias. In the following section, I describe how I investigate the effect of neglecting the issue of selection on *unobserved* characteristics.

(b) *Dealing with unobserved variable bias: recursive bivariate probit*

Section 5b first presents probit estimates of the following equation:

$$FI^* = X'\beta + \alpha LG + \varepsilon, \quad FI = 1 \text{ if } FI^* > 0, 0 \quad \text{otherwise} \quad (1)$$

where ε is a residual term and X is the full set of controls introduced in Section 3. I then turn to investigate the sensitivity of these results to potential unobserved variable bias using a recursive bivariate probit model in which participation and food insecurity are determined simultaneously. The following system is estimated simultaneously:

$$\begin{cases} FI^* = X'\beta_1 + \gamma LG + \varepsilon_1, & FI = 1 \text{ if } FI^* > 0, 0 \quad \text{otherwise} \\ LG^* = X'\beta_2 + \varepsilon_2, & LG = 1 \text{ if } LG^* > 0, 0 \quad \text{otherwise} \end{cases} \quad (2)$$

allowing for the residuals of the two equations to be correlated. More specifically, this bivariate probit model can be written as:

$$Prob[FI = 1, LG = 1|X] = \Phi_2(X'\beta_1 + \gamma LG, X'\beta_2, \rho)$$

where: Φ_2 is the cumulative bivariate normal distribution function; X is the full set of controls described in Section 3; $\rho = cov(\varepsilon_1, \varepsilon_2)$.

The potential existence of a correlation between the error terms ε_1 and ε_2, that is, the presence of some unobserved variable bias, is thus testable (by testing whether $\rho = 0$), and accounted for in the estimation procedure. If $\rho \neq 0$, the bivariate estimates should be preferred to single equation estimates, whereas the model is correctly estimated by two separate probit models when $\rho = 0$. If ε_1 is positively correlated with ε_2, for example, due to low income or food insecurity before participation increasing both the likelihood of participation and that of being currently food insecure, then $\rho > 0$ and the coefficient on the participation variable is overestimated. However, if the correlation goes in opposite directions, $\rho < 0$ and the participation coefficient is underestimated. For instance, this could be the case because better informed, better connected, more able or simply more opportunistic individuals may be both more likely to apply for a land grant and less likely to experience lack of food.

Wilde (2000) demonstrates that as long as one regressor X_1 in X offers enough variation, which is guaranteed in most economic applications including the present one,[11] the full rank of the matrix of regressors is a sufficient condition for the model's identification, so that, in theory, identification does *not* require that one or more regressors entering the participation equation are excluded from the food insecurity equation. It would certainly be preferable to include such an instrument in the model, especially since in the absence of such variable the reliability of the exogeneity test (i.e., the test that $\rho = 0$) depends on the assumption that the residuals are bivariate normal distributed (Monfardini & Radice, 2007). No suitable instrument could be found in the present dataset. Results should therefore be taken with caution. Despite this caveat, it is interesting to use recursive bivariate probit estimates as an indicator of the likely direction of the unobserved variable bias, that is, of the sign of the correlation between ε_1 and ε_2.

5. RESULTS

(a) *Propensity score matching estimates*

In Table 2, propensity score matching estimates are presented for each of the four LFS waves and two alternative sets of regressors entering the propensity score. The average difference in propensity scores between treated households and their matched control is virtually zero (see Table 2), indicating that nearest-neighbor matching does not produce matches between distant neighbors. Furthermore, nearly all beneficiaries can be matched to a suitable control thanks to the large pool of potential controls, with most of the difference between matched beneficiaries and their total number being due to the trimming of 2.5% of participants to further ensure common support.[10]

In addition to the unconfoundness assumption, the reliability of the ATT estimates obtained by propensity score matching relies on the condition that the distribution of the variables included in X should be the same for beneficiaries and non-beneficiaries with the same propensity score, that is, $LG \perp X|p(X)$ (so that matching on $p(X)$ leads to comparing observations with similar values of X, on average). This balancing property is here considered satisfied if a *t*-test does not reject equality of means in each covariate included in the propensity score between treated and matched households.

Table 2. *Propensity score matching estimates of the effect of receiving a land grant on food insecurity, LFS*

	2001		2002		2003		2004	
ATT	0.084***	0.058**	0.088***	0.021	0.102***	0.129***	0.087***	0.050*
	[0.027]	[0.027]	[0.030]	[0.031]	[0.037]	[0.030]	[0.027]	[0.027]
Balancing property[a]	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regressors included in propensity score								
Education								
Female head								
Receiving welfare	√		√		√		√	
Single parent								
Household size								
Age of head								
Full set of regressors		√		√		√		√
Number of matched beneficiaries	620	620	523	523	325	325	756	756
Mean difference in propensity scores between beneficiary and matched control	2.43e−07	.0000173	2.37e−07	.0000305	2.01e−07	.0000722	2.26e−07	.000033

Source: Author's calculations based on the sample of households headed by a black individual in Statistics South Africa's LFS September 2001–04. ATT, Average Treatment Effect on the Treated. Matching algorithm is nearest-neighbor matching without replacement using psmatch2 (Leuven & Sianesi, 2003). To further ensure common support, the 2.5% participants at which the propensity score density of the controls is lowest were trimmed off.
[a] The smallest *p*-value for a *t*-test of difference in means in a single variable is 0.028 (2004, full set of regressors), with the second smallest as high as 0.095. Bootstrapped standard errors in brackets.
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

Comparing treated and non-treated households with similar distributions for education, gender, age of the household head, household size, single parenthood, and benefits receiving, I find that beneficiaries are still significantly *more* likely to report difficulties in satisfying their food needs than non-participants by between 8.4% and 10.2%-points, and this difference is significant at the 1% significance level. However, when the propensity scores are estimated using the full set of regressors, the difference in food insecurity prevalence between beneficiaries and non-beneficiaries increases for the 2003 wave, but decreases for the three other waves, even becoming insignificant for LFS 2002. Results obtained for 2001 and 2004 are very similar. This is of interest because the LFS survey design implies that no household is sampled in both the 2001 and the 2004 waves, suggesting that the results obtained are not specific to one cohort of beneficiaries.

These results confirm the negative picture conveyed by the literature. It is important to remark that, given the nature of the counterfactuals, namely, households with similar characteristics observed at the same point in time, these findings do not necessarily imply that participants have not, on average, personally enjoyed more food security as a consequence of their participation, but that their endowments would have been better rewarded in terms of food security if they had not taken part in land reform.

What more can we learn from these PSM estimates? First, it is interesting to note that the ATT estimates obtained do not differ much from those derived from single probit estimates except for 2002, suggesting only limited observed variable bias. Indeed, the probit ATT estimate using the full set of regressors is 6.9%, 4.6%, 10.8%, and 5.0%-points for 2001, 2002, 2003, and 2004, respectively, compared to the corresponding PSM estimates of 5.8%, 2.1%, 12.9%, and 5%-points.[12] Second, the propensity score equations indicate that the usual socio-economic variables affect participation only weakly. As can be seen in columns (3), (6), (9), and (12) of Table 3, only ethnicity and province systematically affect the probability of receiving a land grant. Socio-economic and demographic vari-

ables, on the other hand, are not robust predictors of participation, since none of these variables have a significant impact on participation in more than two out of four waves.[13] The poor individual predictive power of socio-economic variables in the participation equation should not surprise given the "one-size fits all" approach to land redistribution adopted in the country and the mixed evidence regarding the program's pro-poor targeting. On the contrary, the significant role of ethnic affiliation and province of residence suggests that there might be unobservables correlated with participation. Such unobservable characteristics may also affect food insecurity in an *a priori* undetermined direction, thus creating a bias. In the following section, I shed light on the likely direction of this potential unobserved variable bias.

(b) *Recursive bivariate probit estimates*

Ideally, one would like to carry out bivariate probit regressions on the sample of matched beneficiaries and non-beneficiaries, in which the *observed* variable bias is minimized. This is, however, more than the data can support, insofar as the recursive bivariate probit model including the full set of regressors does not converge for two out of four rounds when the sample is restricted to matched households. For the two rounds for which the model converges, the estimate of ρ and its standard error are, respectively, −0.91 (0.230) for 2001 and 0.21 (0.636) for 2004, although for both rounds, we cannot reject that $\rho = 0$ on the basis of the likelihood-ratio test (*p*-values are, respectively, 0.41 and 0.75). In addition to the convergence problem encountered with the 2002 and 2003 rounds, there are three reasons why I focus here on recursive bivariate probit models estimated using the whole sample: (i) as witnessed by the very large standard error estimate of ρ for 2004 using the matched sample only, the estimates based on the small matched samples (the largest of these being 1512, that is, 756 beneficiaries matched to 756 controls) are very imprecisely estimated; (ii) except for 2002, the estimates of the average treatment effect on the treated obtained with

Table 3. *Probit and bivariate probit estimates of the effect of receiving a land grant on food insecurity, LFS*

	2001			2002			2003			2004		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Probit	Bivariate Probit		Probit	Bivariate Probit		Probit	Bivariate Probit		Probit	Bivariate Probit	
Dependent variable	*FI*	*FI*	*LG*	*FI*	*FI*	*LG*	*FI*	*FI*	*LG*	*FI*	*FI*	*LG*
=1 if land grant	0.192***	1.000***		0.126**	0.668*		0.303***	0.367		0.141***	0.440	
	[0.053]	[0.289]		[0.057]	[0.342]		[0.075]	[0.304]		[0.050]	[0.269]	
Socio-economic characteristics												
Education of household head (omitted: no education)												
=1 if primary	−0.108***	−0.112***	0.106**	−0.133***	−0.131***	−0.034	−0.135***	−0.135***	−0.003	−0.074***	−0.075***	0.087*
	[0.026]	[0.026]	[0.054]	[0.027]	[0.027]	[0.061]	[0.028]	[0.028]	[0.077]	[0.025]	[0.025]	[0.052]
=1 if lower secondary	−0.296***	−0.297***	0.067	−0.241***	−0.241***	0.099	−0.256***	−0.256***	0.048	−0.237***	−0.238***	0.063
	[0.027]	[0.027]	[0.058]	[0.028]	[0.028]	[0.062]	[0.029]	[0.029]	[0.081]	[0.027]	[0.027]	[0.056]
=1 if senior secondary	−0.527***	−0.526***	0.068	−0.493***	−0.492***	0.022	−0.575***	−0.575***	0.029	−0.498***	−0.499***	0.067
	[0.030]	[0.030]	[0.065]	[0.031]	[0.031]	[0.069]	[0.032]	[0.032]	[0.087]	[0.030]	[0.030]	[0.059]
=1 if higher	−1.087***	−1.082***	0.080	−1.143***	−1.132***	−0.318***	−1.267***	−1.267***	−0.107	−1.114***	−1.115***	0.062
	[0.046]	[0.046]	[0.088]	[0.050]	[0.051]	[0.123]	[0.057]	[0.057]	[0.142]	[0.052]	[0.052]	[0.088]
=1 if female head	0.099***	0.091***	0.172***	0.035	0.034	0.063	0.051**	0.050**	0.051	0.029	0.029	0.021
	[0.024]	[0.024]	[0.054]	[0.025]	[0.025]	[0.061]	[0.025]	[0.025]	[0.078]	[0.024]	[0.024]	[0.048]
=1 if receives welfare	0.127***	0.129***	−0.085	0.101***	0.099***	0.052	0.127***	0.127***	−0.067	0.166***	0.164***	0.102**
	[0.025]	[0.025]	[0.053]	[0.025]	[0.025]	[0.053]	[0.024]	[0.024]	[0.063]	[0.022]	[0.022]	[0.043]
Demographic composition												
No. children ⩽ 5	0.012	0.013	−0.013	0.004	0.004	−0.020	−0.038**	−0.038**	−0.015	−0.027*	−0.028*	0.059**
	[0.015]	[0.015]	[0.033]	[0.016]	[0.016]	[0.037]	[0.017]	[0.017]	[0.047]	[0.015]	[0.015]	[0.029]
No. children ⩽ 15	0.039***	0.041***	−0.055**	0.025**	0.026**	−0.044	0.044**	0.045**	−0.078**	0.057***	0.057***	−0.014
	[0.012]	[0.012]	[0.023]	[0.012]	[0.012]	[0.028]	[0.013]	[0.013]	[0.036]	[0.011]	[0.011]	[0.023]
Household composition (omitted: "other")												
=1 if couple, no child	0.031	0.022	0.209***	0.015	0.013	0.085	0.019	0.019	−0.027	−0.055	−0.057	0.118*
	[0.035]	[0.035]	[0.075]	[0.036]	[0.036]	[0.085]	[0.037]	[0.037]	[0.101]	[0.035]	[0.035]	[0.068]
=1 if couple with kid	−0.029	−0.047	0.370***	−0.043	−0.050	0.266***	−0.074**	−0.075**	0.079	−0.153***	−0.155***	0.083
	[0.036]	[0.036]	[0.074]	[0.034]	[0.034]	[0.079]	[0.035]	[0.036]	[0.101]	[0.029]	[0.029]	[0.058]
=1 if single parent	0.143***	0.133***	0.158***	0.164***	0.158***	0.191***	0.124***	0.124***	−0.038	0.116***	0.115***	0.050
	[0.030]	[0.029]	[0.061]	[0.029]	[0.029]	[0.065]	[0.029]	[0.029]	[0.088]	[0.022]	[0.022]	[0.044]
Household members	0.073***	0.070***	0.067**	0.066***	0.066***	0.011	0.055***	0.055***	0.113***	0.061***	0.061***	0.047
	[0.016]	[0.016]	[0.030]	[0.012]	[0.012]	[0.033]	[0.013]	[0.013]	[0.039]	[0.012]	[0.012]	[0.029]
Household members^2	−0.004***	−0.004***	−0.004*	−0.003***	−0.003***	−0.000	−0.002**	−0.002**	−0.005	−0.004***	−0.004***	−0.006**
	[0.001]	[0.001]	[0.002]	[0.001]	[0.001]	[0.002]	[0.001]	[0.001]	[0.003]	[0.001]	[0.001]	[0.002]
Age of of head^2	−0.000*	−0.000*	−0.000	−0.000	−0.000	−0.000***	−0.000*	−0.000**	−0.000	−0.000**	−0.000**	−0.000
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Language dummies (omitted: "other")												
Debele	0.180**	0.147*	0.649***	0.300***	0.299***	0.246*	0.622***	0.623***	0.288*	0.458***	0.469***	−0.422***
	[0.075]	[0.075]	[0.139]	[0.083]	[0.083]	[0.146]	[0.083]	[0.083]	[0.149]	[0.080]	[0.080]	[0.132]
Xhosa	0.492***	0.469***	0.443***	0.663***	0.647***	0.453***	0.727***	0.725***	0.457***	0.601***	0.589*	0.497***
	[0.038]	[0.039]	[0.088]	[0.040]	[0.041]	[0.087]	[0.042]	[0.043]	[0.102]	[0.038]	[0.039]	[0.069]
Zulu	0.415***	0.391***	0.570***	0.538***	0.516***	0.599***	0.726***	0.723***	0.721***	0.525***	0.527***	−0.231**
	[0.044]	[0.045]	[0.107]	[0.048]	[0.050]	[0.105]	[0.051]	[0.053]	[0.111]	[0.048]	[0.048]	[0.091]

Table 3—Continued

	2001			2002			2003			2004		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Probit	Bivariate Probit		Probit	Bivariate Probit		Probit	Bivariate Probit		Probit	Bivariate Probit	
Sotho	0.407***	0.389***	0.398***	0.567***	0.559***	0.280**	0.559***	0.559***	0.004	0.538***	0.533***	0.206**
	[0.046]	[0.047]	[0.112]	[0.049]	[0.050]	[0.114]	[0.053]	[0.053]	[0.164]	[0.048]	[0.048]	[0.103]
Sepedi	0.361***	0.333***	0.602***	0.434***	0.435***	−0.042	0.469***	0.469***	−0.080	0.541***	0.552***	−0.594***
	[0.045]	[0.046]	[0.107]	[0.049]	[0.049]	[0.121]	[0.054]	[0.054]	[0.135]	[0.051]	[0.052]	[0.110]
Tswana	0.327***	0.319***	0.246**	0.426***	0.417***	0.341***	0.494***	0.494***	0.308**	0.488***	0.485***	0.141
	[0.045]	[0.044]	[0.096]	[0.048]	[0.048]	[0.105]	[0.051]	[0.051]	[0.120]	[0.048]	[0.048]	[0.088]
Siswati	0.426***	0.421***	0.202	0.661***	0.685***	−0.287**	0.441***	0.446***	−0.548***	0.451***	0.480***	−0.807***
	[0.064]	[0.063]	[0.148]	[0.066]	[0.067]	[0.144]	[0.073]	[0.076]	[0.175]	[0.073]	[0.077]	[0.125]
Tsonga	0.306***	0.263***	0.836***	0.360***	0.355***	0.351***	0.382***	0.382***	−0.126	0.356***	0.363***	−0.265**
	[0.052]	[0.054]	[0.111]	[0.056]	[0.056]	[0.128]	[0.061]	[0.061]	[0.193]	[0.063]	[0.063]	[0.123]
English	−0.273***	−0.281***	−0.035	−0.349***	−0.355***	−0.171	−0.174**	−0.174*	−5.128***	−0.251***	−0.247***	−0.306**
	[0.057]	[0.057]	[0.160]	[0.066]	[0.067]	[0.199]	[0.073]	[0.073]	[0.120]	[0.076]	[0.076]	[0.131]
Province dummies (omitted: Eastern Cape)												
Western Cape	−0.454***	−0.439***	−0.214**	−0.510***	−0.490***	−0.417***	−0.414***	−0.415***	0.786***	−0.343***	−0.354***	0.524***
	[0.043]	[0.044]	[0.094]	[0.044]	[0.046]	[0.090]	[0.046]	[0.047]	[0.136]	[0.043]	[0.044]	[0.085]
Northern Cape	−0.200***	−0.218***	0.405***	−0.373***	−0.366***	−0.024	−0.152***	−0.154***	1.040***	0.069	0.049	0.864***
	[0.053]	[0.053]	[0.091]	[0.054]	[0.055]	[0.102]	[0.056]	[0.057]	[0.149]	[0.048]	[0.051]	[0.089]
Free State	−0.324***	−0.296***	−0.782***	−0.413***	−0.388***	−0.799***	−0.408***	−0.408***	0.183	−0.209***	−0.210***	−0.390**
	[0.049]	[0.049]	[0.128]	[0.050]	[0.053]	[0.119]	[0.051]	[0.051]	[0.204]	[0.048]	[0.048]	[0.158]
Kwazulu−Natal	−0.563***	−0.527***	−1.221***	−0.508***	−0.469***	−1.520***	−0.480***	−0.478***	−0.279	−0.108**	−0.122**	0.762***
	[0.048]	[0.049]	[0.140]	[0.050]	[0.056]	[0.169]	[0.050]	[0.051]	[0.174]	[0.047]	[0.049]	[0.101]
North West	−0.299***	−0.324***	0.352***	−0.419***	−0.397***	−0.607***	−0.189***	−0.189***	0.274*	0.049	0.042	0.186*
	[0.049]	[0.049]	[0.081]	[0.050]	[0.053]	[0.111]	[0.051]	[0.051]	[0.162]	[0.052]	[0.052]	[0.110]
Gauteng	−0.577***	−0.557***	−0.317***	−0.650***	−0.625***	−0.718***	−0.668***	−0.669***	0.612***	−0.476***	−0.488***	0.531***
	[0.042]	[0.043]	[0.086]	[0.045]	[0.048]	[0.106]	[0.047]	[0.047]	[0.145]	[0.045]	[0.046]	[0.093]
Mpumalanga	−0.338***	−0.337***	−0.017	−0.436***	−0.458***	0.306***	−0.414***	−0.421***	1.523***	−0.016	−0.069	1.760***
	[0.052]	[0.052]	[0.099]	[0.053]	[0.054]	[0.095]	[0.055]	[0.064]	[0.146]	[0.054]	[0.070]	[0.098]
Limpopo	−0.194***	−0.177***	−0.336***	−0.385***	−0.369***	−0.496***	−0.411***	−0.413***	0.535***	−0.366***	−0.384***	0.738***
	[0.049]	[0.049]	[0.099]	[0.052]	[0.053]	[0.112]	[0.055]	[0.055]	[0.173]	[0.053]	[0.055]	[0.108]
Constant	−0.051	−0.040	−2.830***	−0.185**	−0.185**	−2.860***	−0.433***	−0.431***	−4.052***	−0.532***	−0.519***	−3.070***
	[0.090]	[0.090]	[0.198]	[0.092]	[0.092]	[0.224]	[0.093]	[0.094]	[0.301]	[0.089]	[0.089]	[0.185]
Observations	24074	24074	24074	23066	23066	23066	23438	23438	23438	25341	25341	25341
Pseudo R^2	0.0974			0.1045			0.1098			0.0939		
ρ[a]		−0.367** [.130]			−0.249 [.154]			−0.0295 [.132]			−0.137 [.119]	
p-value of likelihood-ratio test of $\rho = 0$		0.0195			0.1262			0.8275			0.2771	

Source: Author's calculations based on the sample of households headed by a black individual in Statistics South Africa's LFS September 2001–2004. *FI*, food insecurity indicator (=1 if problems satisfying food needs "sometimes", "often" or "always"), *LG*, participation indicator (=1 if received a land grant). Robust Standard errors in brackets.
[a] Significance level based on likelihood-ratio test.
* $p < 0.1$.
** $p < 0.05$.
*** $p < 0.01$.

propensity score matching are reasonably close to those obtained with probit models using the full samples; and (iii) given the reliance of the likelihood-ratio test of $\rho = 0$ on the normality of the joint distribution of ε_1 and ε_2, it would seem preferable to use larger samples.

Table 3 shows the results obtained with each LFS wave, using the full (Black) sample. Unsurprisingly, results indicate that households with lower-educated heads, receiving welfare grants, whose head is a single parent, and larger households are more food insecure. Although one could expect these households to be poorer, female-headed households and households whose head is elderly are not found to be systematically more food insecure. This could be due to the way money is spent in these households: for instance, women are known to favor food expenditure over tobacco (e.g., Hoddinott & Haddad, 1995). In the case of elderly household heads, the effect may have to do with old age pensioners receiving comparatively high pensions (R940 as of April 2008). I also find that food insecurity significantly decreases with the number of children under 5 years old in 2003 and 2004, but systematically increases with the number of children age 15 or under. This may be due to the fact that the ubiquitous child support grant (R210 as of April 2008) does not vary with child age, whilst the cost of child care tends to increase. Households of different ethnic origins, as captured here by the language spoken at home, also have significantly different probabilities of reporting food insecurity. Finally, the province of residence affects the propensity to report difficulties satisfying food needs, with households in the Eastern Cape (the omitted category) appearing the most often food insecure and households in Gauteng the least.

Although the LR exogeneity test only leads to the rejection of the hypothesis that $\rho = 0$ for the 2001 round, the estimate of ρ is negative in all rounds, suggesting that unobserved variable bias would tend to bias the naïve estimate of the land grant coefficient downwards. In other words, the result that land grantees are more food insecure than non-grantees with similar observed characteristics does not appear to be due to unobserved variable bias. Indeed, the land grant coefficient is larger in the bivariate probit specification than in the univariate probit, although it is insignificant in the 2003 and 2004 rounds.

(c) Further results

The General Household Survey provides the opportunity for testing the robustness of the results obtained with the LFS, since samples, month of interview, and the food insecurity questions asked to respondents differ. Contrary to the LFS, a new sample is drawn for each round of the GHS, so that the two relevant waves (2002 and 2003) can be stacked together. [14] As with the LFS, land grant beneficiaries in the GHS report experiencing hunger more often than non-beneficiaries (see Table 4).

Based on probit results, beneficiary households are, on average, 4.01%-points more likely to report either children or adults in the household having gone hungry at least sometimes in the 12 months preceding the survey, controlling for all the socio-economic and geographical factors listed in Section 3, except for the set of household language dummies, that cannot be created from the GHS data. The corresponding average treatment effect on the treated based on propensity score matching using the same controls is 4.13%-points, suggesting little observed variable bias. Both the probit coefficient on the land grant indicator and the ATT estimated with PSM are statistically significant at the 5% significance level. Recursive bivariate probit estimates suggest that the probit estimate

Table 4. Prevalence of hunger in GHS 2002/2003

	"Never" or "seldom" hungry	Hungry at least "sometimes"	Total
Child food insecurity (17 or younger)			
Non-beneficiaries	74%	26%	100% (30,456)[a]
Beneficiaries	68%	32%	100% (668)[a]
Adult food insecurity			
Non-beneficiaries	74%	26%	100% (45,472)[a]
Beneficiaries	65%	35%	100% (883)[a]

Source: Author's calculations based on the sample of households headed by a black individual in Statistics South Africa's GHS 2002 and 2003. Sample size in parentheses.
[a] Sample size varies as some households have no children 17 or younger or no member over 17 years of age.

Table 5. Estimation results for GHS 2002/2003

	Propensity score matching[a]	Probit	Bivariate probit
Dependent variable		Hunger	Hunger
ATT	0.0413** [0.0438]		
No. of matched beneficiaries	847		
Coefficient on *LG*		0.115** [0.046]	0.429* [0.255]
Observations		45712	45712
Pseudo R^2		0.076	
ρ			−0.138 [0.113]

Source: Author's calculations based on the sample of households headed by a black individual in Statistics South Africa's GHS 2002 and 2003. ATT, Average Treatment Effect on the Treated. Regressors included: a dummy variable equal to one if the observation comes from the GHS 2003, and zero if it comes from the GHS 2002, and all regressors in Table 3 except the set of household language indicators. Robust standard errors in brackets. Matching algorithm is nearest-neighbor matching without replacement using psmatch2 (Leuven & Sianesi, 2003). To further ensure common support, the 2.5% participants at which the propensity score density of the controls is lowest were trimmed off.
[a] Balancing property satisfied (the smallest *p*-value for an individual *t*-test of equality of means in a regressor between treated and controls is 0.07).
* $p < 0.1$.
** $p < 0.05$.

is not overestimating the adverse effect of participation on the household propensity to report hunger episodes, since the estimate of ρ, the coefficient of correlation, is negative (and statistically insignificant) (see Table 5).

6. DISCUSSION

Confirming the insights provided by non-quantitative studies, data from the South African Labour Force Survey (LFS) and General Household Survey (GHS) suggest that, on average, land-reform beneficiaries do not appear to have experienced lower food insecurity as a consequence of land redistribution. On the contrary, land grant recipients tend to have more difficulties in satisfying their food needs than non-beneficiaries with a similar profile. It is important to reiterate that, given the nature of the counterfactuals, namely, households with similar characteristics observed at the same

point in time, these findings do not necessarily imply that participants have not, on average, personally enjoyed more food security as a consequence of their participation, but that their endowments would have been better rewarded in terms of food security should they not have taken part in land reform.

The seemingly adverse effect of participation warrants discussion. Higher food insecurity as defined here is mainly the result of (i) a lower average or "typical" level of resources that can be transformed into food (increased "poverty") and/or (ii) of an increased probability of falling below a critical threshold in the availability of these resources (increased "vulnerability"). The term "poverty" is only opposed to "vulnerability" for the clarity of the argument. It certainly is not meant to imply that vulnerability is not a component of a broader definition of poverty.

Surely, households take part in land redistribution because they expect to gain from participation. However, households may end up with an unexpectedly low level of resources after land redistribution because they have miscalculated the costs and benefits of participation. In particular, they may have misjudged the extent of relocation costs, land and non-land production factors needs or availability, project disorganization, or the previously suggested unequal land appropriation by a project élite. The costs contemplated here are manifold. There are displacement costs of course: 43.4% of households surveyed for the Department of Land Affairs (DLA) in 2001 (Ahmed et al., 2003, p. 43) had to move to live on redistributed land. These displacement costs are material costs, including costs of transportation of family members and goods, potential loss of income-generating activities for some of the households' members, and transaction costs such as search costs to find all the needed services and goods in a new living area. These costs are likely to be particularly important in South Africa due to the historical confinement of the Black into areas well apart from the white-owned land that is now being redistributed (Zimmerman, 2000). Transport costs to travel to redistributed land when beneficiaries have not relocated are also often cited as a major barrier for land-reform beneficiaries to take advantage of their newly acquired land (e.g., in Bradstock, 2005) especially for rural women (Wegerif, 2004). The costs of starting a new economic activity may be very high

for these beneficiaries since the land grants do not usually cover much more than the cost of the sole land. Some farmers in Wegerif (2004) even report "that they are currently subsidizing the farms with their own money from other sources" (p. 38). In addition, beneficiary households and implementers of the program report various forms of costly disorganization at the project level (Ahmed et al., 2003; McCusker, 2002; Wegerif, 2004), including a high prevalence of unpaid work (see Ahmed et al., 2003, p. 118).

There is little discussion, in the literature, of the role of collective production in the poor performance of land redistribution in South Africa, and the proportion of redistributed farms following a collective production model is unknown. However, anecdotal evidence suggests that the proportion is high. This may contribute to the disorganization problems mentioned above, and suggests low productivity, due to the poor incentive structure in collective farms, as an additional explanatory factor for the disappointing impact of land redistribution.[15]

Even in the context of very successful participation in land reform, one would expect costs to dominate benefits at the onset. It could be the case that the beneficiaries in the present sample have simply not started reaping the benefits of participation yet. In the absence of information on the date of the land transfer, it is not possible to test this hypothesis. However, it is unclear whether the food insecurity differential between land grantees and non-land grantees has decreased over the period covered by the data: in 2001, the PSM average treatment effect on the treated was 0.058 (i.e., 5.8%-points), and in 2004 it was 0.050. Between these two dates, it decreased (from 2001 and 2002) before reaching its maximum in 2003. Having received land earlier in time does not necessarily follow from being observed to be a land grantee at an earlier date, but if the time factor was overwhelming, one would expect to find at least some indication of a decreasing trend in the beneficiary/non-beneficiary food insecurity differential.

Looking at factors that may have specifically increased the *variability* of households' resources, it is useful to consider separately *ex ante* and *ex post* insurance mechanisms. *Ex ante* risk coping strategies include essentially the choice of low risk
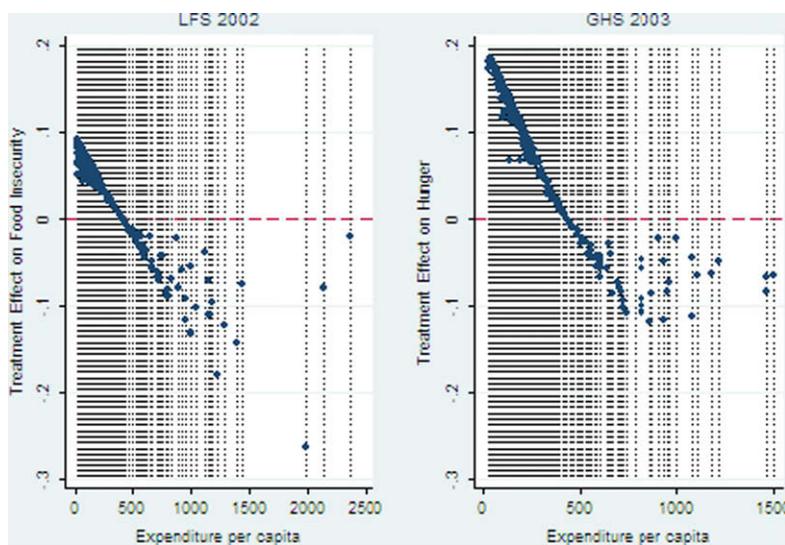

Figure 1. *Treatment effect on the treated according to expenditure. Source: Author's calculations based on the sample of households headed by a black individual in Statistics South Africa's LFS September 2002 and GHS 2003. Contrary to the other datasets used in this paper, the negative interaction coefficient between participation and expenditure per capita is statistically significant (at 5% and 1% for LFS 2002 and GHS 2003, respectively). The expenditure variable is adult-equivalent expenditure per capita in Rand per month. Top expenditure percentile omitted.*

activities and diversification of activities. *Ex post* risk coping strategies are, for example, the use of savings and participation in informal insurance mechanisms. It might be argued that agriculture being a risky activity – and especially so in mostly semi-arid South Africa – beneficiaries become more vulnerable as they choose to employ their time and effort in agriculture rather than in less risky activities. Furthermore, broken social links and solidarity networks due to resettlement, [16] along with the scooping out of their savings to incur resettlement costs, may be making beneficiary households more vulnerable to shocks affecting them.

In the context of the South African land reform, whereby much of the land has been transferred to often large groups of beneficiaries without subsequent subdivision between beneficiaries, it is important to remark that credit constraints at the household level may not be reduced by land transfers when the household does not have individual property rights to a specific plot of land.

In addition, if complementary production factors are not readily available, then land redistribution beneficiaries may not be able to use land productively. Indeed, lack of appropriate human capital and poor access to ancillary markets have been shown to prevent an efficient use of land in the former homelands (see Aliber, 2003; Carter & May, 1999) and are likely to affect productivity on newly acquired land in "white" areas as well. Data limitations prevent much analysis of the role played by access to complementary factors, but it is possible to use the limited information on household expenditure provided in the LFS/GHS to shed some light on the role of credit constraints. If credit constraints were driving the poor welfare outcomes of land grants found in the present analysis, then one would expect wealthier households to have better participation outcomes. In order to shed some light on this point, I used the expenditure intervals provided in the surveys, dividing their mid-points by the number of adult-equivalent household members to obtain a proxy for wealth, and included this proxy and its interaction with the participation indicator in the probit equations reported in Table 3. The same was done for the two GHS datasets (separately for GHS 2002 and GHS 2003 to ensure comparability of expenditure levels across observations). There is limited evidence of wealth-dependent participation outcomes in the data used here, as the participation effect appears to vary with expenditure level in two out of six datasets. The estimated treatment effects on the treated are plotted against the wealth proxy in Figure 1 for the individual datasets in which the land grant-expenditure interaction term is significant, namely LFS 2002 and GHS 2003. [17]

7. CONCLUSION

The present study confirms econometrically the doubts previously expressed by most commentators on the impact of land redistribution in South Africa. Comparing beneficiary and non-beneficiary households with similar distributions for a rich set of covariates, propensity score matching estimates indicate that households who say they have received a land grant are more likely to report difficulties in satisfying their food needs than non-land grantees by between 2.1% and 12.9%-points, depending on which of the four relevant LFS waves is considered, with only the lowest estimate insignificant at the 10% significance level. In addition, propensity score matching estimates show that beneficiary households surveyed in the General Household Surveys 2002 and 2003 are, on average, 4.13%-points more likely to report either children or adults in the household having gone hungry at least sometimes

in the 12 months preceding the survey compared to households with similar socio-economic and demographic characteristics.

The main limitation of these estimates is that they may be biased if there are unobserved confounding factors correlated both with participation in the program and food insecurity. Recursive bivariate probit models allow for the testing of the presence and direction of such bias. In all datasets but one (LFS 2001), these suggest that there is no statistically significant omitted variable bias, since we cannot reject the null that the coefficient of correlation between the residuals of the participation equation and the food insecurity equation ρ) is equal to zero. The reliance of the test of $\rho = 0$ on the assumption that the residuals are bivariate normal casts some doubt on its reliability. However, in all five datasets used in this paper, $\rho < 0$, which would suggest that unobserved variable bias, if it exists, tends to lead to the *underestimation* of the food insecurity effect of participation in land reform (in other words, the adverse effect of participation may be larger than indicated by naïve probit estimates).

With the limitations of the data currently at hand, caution should prevail, especially with respect to the exact magnitude of the effect of participation in the program. Further research is needed to shed light on (i) the impact of participation on other welfare indicators, (ii) whether the difficulties of land-reform beneficiaries are only transitory, (iii) the causes of the difficulties experienced – and in particular, whether poorer participants face specific challenges, (iv) conditions for land transfers to truly benefit their recipients. Complementary policies may be needed to make land redistribution an efficient tool to reduce poverty (Finan, Sadoulet, & de Janvry, 2005). Potential complementary policies to be considered are, among others, an increased provision of agricultural support services, [18] facilitation of access to credit for poorer beneficiaries, infrastructure improvement or simply effective transitional food support for poorer participants if their difficulties are found to be of a temporary nature.

Several policy changes introduced recently may change the outlook of land reform in the country. In 2004, a Comprehensive Agricultural Support Programme (CASP) was introduced to improve the provision of skills and (on- and off-farm) infrastructure support. Since 2006, other more radical policy changes have occurred, as a result of discussions originated at the July 2005 Land Summit between representatives of the government, farmer unions, and civil society. In 2006, the government launched a public small-credit scheme (not exclusively aimed at land-reform beneficiaries), the Micro Agricultural Financial Institute of South Africa (MAFISA), as well as the Proactive Land Acquisition Strategy (PLAS) whereby the Department of Land Affairs initiates land purchases and then redistributes it to beneficiaries. The most significant recent policy initiative is the introduction of the Land and Agrarian Reform Project (LARP) in 2008. The main principles behind this program are to identify "focus areas" for land redistribution efforts, to provide comprehensive and integrated support to beneficiaries from the different government departments and non-governmental partners involved, to further decentralize administrative decision-making, and to enhance the monitoring and evaluation of the performance of redistributed farms (Department of Agriculture, 2008). Furthermore, there has been a move toward the extension of redistribution targets along the whole agricultural chain rather than simply at the land ownership level through the adoption of a specific Black Economic Empowerment (AgriBEE) charter in February 2008.

The government itself has expressed disappointment with the performance of CASP (Department of Agriculture, 2008). But lack of data and the early stage of development of some of these policy innovations limit the scope for evaluation. LARP documentation promises that a "scientifi-cally rigorous impact evaluation will be undertaken" as part of the program (Department of Agriculture, 2008, p. 42). Given the high profile of land reform in the country and the concerns confirmed here regarding its impact on participants, a data collection effort indeed appears most urgently needed.

NOTES

1. A highly influential example is Eswaran and Kotwal (1986); the type of activity carried out by the household depends on the availability of working capital, which is in turn an increasing function of wealth as proxied by land ownership.

2. This corresponds to nearly 20% of the mean yearly expenditure for land reform beneficiaries as reported in Deininger et al. (2004).

3. This particularity is not unique to the South African land reform programme. Targeting on equally broad grounds occurred within the Zimbabwean land reform (see Owens, Hoddinott, & Kinsey, 2003).

4. As a consequence, 58% of the beneficiary population aged 15 years and older surveyed in Ahmed et al. (2003) did not have any farming experience before accessing the land reform project (p. 34).

5. It is important to note, however, that when groups applied for land grants jointly, which was invariably the case before 2001, this means testing was applicable to the group average, not to each individual. This allowed for a substantial number of people with a much higher income to participate in the scheme.

6. LRAD grants range from R20,000 for a personal contribution of R5000 (possibly under the form of labour, the so-called "sweat equity") up to R100,000 for a personal contribution of R400,000.

7. When children are studied separately, their nutritional outcomes do not seem to be well correlated with subjective food insecurity measures (Bhattacharya, Currie, & Haider, 2004; Rose 1999). Results in Frongillo and Nanama (2006) suggests that part of the explanation may be that one important determinant of children's nutritional outcomes is illness.

8. For instance, Bhattacharya et al. (2004) find a minimum agreement ratio of 90.4% between a composite food insecurity scale based on the 1999 Current Population Survey (of the United State Department of Agriculture) and answers to individual food insecurity questions, including one question very similar to the one asked in the LFS ("Do you have enough food to eat, sometimes not enough to eat, or often not enough to eat?").

9. Eligibility for welfare grants is not linked to receiving a land grant or not.

10. The matching algorithm used here applies nearest neighbour matching, whereby only the control with closest propensity score is used for comparison. In the present application, this secures common support insofar as the matches are made between treated and control observations with very close propensity scores. To further strengthen common support, the 2.5% participants at which the propensity score density of the controls is lowest were trimmed off. Results including all observations (available upon request) are very close in terms of magnitude, and as much as or more statistically significant.

11. In the recursive bivariate probit models presented in this paper, there are 68 unknown parameters to be estimated, so that, as shown in Wilde (2000), theoretical identification requires that at least 68 independent probabilities enter the likelihood function. This condition is easily satisfied in the data used here. Further details are available upon request.

12. The average treatment effect on the treated using probit estimates is obtained as follows: $ATT = (1/B)\sum_{j=1}^{B}[\Phi(X'_j\hat{\beta} + \hat{\alpha}LG_j) - \Phi(X'_j\hat{\beta})]$.

13. For brevity's sake, the estimates of the probit models used to calculate the propensity scores are not reported here, as they are qualitatively similar to the participation equations of the bivariate models shown in Table 3.

14. When the analysis is carried out separately on GHS 2002 and GHS 2003, the sign of the participation coefficient and of ρ are the same for both datasets, but the participation effect is not significant in GHS 2002.

15. See Deininger (1995) for a review of the international experience with collective production.

16. See Cross et al. (1996), for South Africa and Dekker (2004) for a study of Zimbabwean land reform beneficiaries.

17. Regression results are available upon request.

18. The provision of extension services has been found to improve the value of resettled farmers' crop production elsewhere, notably in nearby Zimbabwe (see Owens et al., 2003).

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