

Deiningner, Klaus; Jin, Songqing; Yadav, Vandana

Working Paper

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WIDER Working Paper, No. 2011/82

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Suggested Citation: Deiningner, Klaus; Jin, Songqing; Yadav, Vandana (2011) : Long-term effects of land reform on human capital accumulation: Evidence from West Bengal, WIDER Working Paper, No. 2011/82, The United Nations University World Institute for Development Economics Research (UNU-WIDER), Helsinki

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Economics Research

Working Paper No. 2011/82

Long-term Effects of Land Reform on Human Capital Accumulation

Evidence from West Bengal

Klaus Deininger,¹ Songqing Jin,² and
Vandana Yadav³

November 2011

Abstract

We use data on inter-generational gains in educational attainment by some 500,000 individuals in 200 West Bengal villages to explore gender-differentiated impacts of land reform on human capital accumulation at the individual level. While there are significant gains (of about 0.3 years for males) in the immediate post-reform generation, their magnitude pales in comparison to second-generation effects of between 0.85 and 1.2 years that appear irrespectively of the land reform modality. Moreover, there are possibly significant spillover benefits on villagers who did not directly benefit from reform. Placebo tests and alternative specifications support robustness of the results. By contrast, levels of beneficiary productivity and welfare remain far below average, something that could likely be avoided if land reform beneficiaries would receive full ownership rights—rather than being recognized as permanent share tenants and if restrictions on transferability of land were abandoned.

Keywords: India, land reform, long-term effects, human capital

JEL classification: O12, O15, Q15

¹ Lead Economist, World Bank, email: kdeininger@worldbank.org, ² Assistant professor, Michigan State University, email: jins@msu.edu, ³ PhD student, Michigan State University, email: yadavvan@msu.edu

This study has been prepared within the UNU-WIDER project on Land Inequality and Decentralized Governance in LDCs.

UNU-WIDER acknowledges the financial contributions to the research programme by the governments of Denmark (Ministry of Foreign Affairs), Finland (Ministry for Foreign Affairs), Sweden (Swedish International Development Cooperation Agency—Sida) and the United Kingdom (Department for International Development).

Acknowledgements

The data for this study have been collected by EIT, Calcutta under the FAO-World Bank Cooperative Program. Our most sincere thanks are due to A. K. Roy and D. Mazumdar for their outstanding efforts in data collection and processing. The views expressed in this paper are those of the authors and do not necessarily reflect those of the World Bank, FAO, or MSU.

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UNU World Institute for Development Economics Research (UNU-WIDER)
Katajanokanlaituri 6 B, 00160 Helsinki, Finland

Typescript prepared by Anne Ruohonen at UNU-WIDER

The views expressed in this publication are those of the author(s). Publication does not imply endorsement by the Institute or the United Nations University, nor by the programme/project sponsors, of any of the views expressed.

1. Introduction

Given the importance of land as a productive asset, especially for the poor, many countries with highly unequal land ownership distributions have historically taken up programmes to redistribute land to the poor. In addition to moral arguments for equity of opportunity, economic justifications for such efforts have been derived from two sets of arguments. One was the reference to low productivity of land use in large farms that could be considerably enhanced by transferring control over land to small cultivators. A second, more recent argument, refers to ‘poverty traps’, i.e. situations where credit market imperfections prevent poor households who lack assets from making indivisible investments, e.g. in education, that would enable them to fully exploit their productive capacity or actively participate in political processes. If such imperfections are indeed present, a one-time redistribution of assets could in principle break the vicious cycle of poverty that the poor would otherwise find themselves in. Under either of these scenarios, land reform can be used as a way to create the pre-conditions for faster and broad-based growth by augmenting productivity of land use and by enabling the poor to accumulate physical as well as human capital.

Although a large body of micro-level evidence supports the relative inefficiency of wage-labour operated large farms and points towards a negative relationship between inequality and subsequent growth, such relationships are only a necessary condition for land reform to have positive effects. In fact, one-time asset redistribution is likely to have a positive impact on household’s patterns of asset accumulation and the growth trajectory of the wider economy only if the manifold challenges of implementation and post-transfer support can be appropriately addressed. Even then, detailed evidence on effectiveness, incidence, magnitude, and time path of land reform impacts will need to be looked at to assess the potential for land redistribution in comparison to, and possibly combination with, other policy initiatives. The fact that few of the large number of reforms undertaken historically have been subjected to rigorous evaluation makes it difficult to provide policy advice to countries who either may consider initiation of such reforms or aim to make inherited policies more effective.

To contribute to this debate, this paper analyzes land reform in the Indian state of West Bengal, one of the world’s largest peacetime land redistributions. We go beyond existing approaches in three ways. First, we focus on individual data to discern between village-wide and beneficiary-specific effects and to explore differences in impact between *pattadars* and *bargadars*¹. This is of interest as studies suggest that land reform implementation and provision of support services at village level are likely to be endogenous. Second, as agricultural productivity will be affected by other factors such as local governments’ provision of public goods and the rigor with which legal restrictions on transferability of reform land are enforced, we focus instead on land reform impacts on human capital accumulation. Such effects will be particularly important if, as hypothesized by many studies, poverty traps induced by imperfect credit markets prevent poor individuals from making optimum levels of investment in indivisible assets so that asset redistribution could eliminate credit constraints and bring

¹ *Pattadars* and *bargadars* refer to beneficiaries of land ceiling reform, and beneficiaries of tenancy reform, respectively.

investment levels closer to the optimum. Third, information on human capital accumulation over three generations—30 years after the reform took place—allow us to make inferences on human capital accumulation impacts beyond the first generation that can complement evidence on short-term productivity effects and provide evidence on the time lags for full realization of land reform effects.

Our outcome variable is individuals' educational gain, defined as difference in the number of years of schooling obtained by an individual whose education decisions were finalized after land reform was implemented (i.e. who was below 14 or 6 years of age in 1978) to that of the average of all dynasty individuals who completed their education before land reform. Identification is achieved by comparing the magnitude of such gains between individuals in households who did to those who did not benefit from land reform. A placebo test simulating the impacts of a hypothetical land reform in 1958 rather than 1978 does not allow us to reject the underlying hypothesis of parallel trends.

We find that, consistent across methodologies, those who benefited directly from land reform experienced significant educational gains that closed what had been a widening gap in educational attainment. These effects persist and increase markedly in the second post-reform generation where total educational gains add up to more than one year of extra school attendance. As those who benefited from land reform had lower rather than higher growth rates of educational attainment before 1978, this effect cannot be attributed to pre-existing differences. Descriptive data also suggest that land reform coincided with a shift from gender—and caste-divergence in educational attainment towards convergence and that it replaced a stagnant with a much more dynamic pattern of educational attainment. Although we lack data from states other than West Bengal that would allow us to test for causality effect, this does point towards the existence of positive land reform effects beyond the individual level that are not captured by our estimates.

The paper is structured as follows. Section two reviews the literature on land reform with a view towards productivity—and asset accumulation benefits and provides general background on land reform in India, with a particular focus on West Bengal. Section three presents data and descriptive statistics and discusses our estimation strategy which relies on changes in human capital accumulation between old and young generations in the same dynasty and discusses underlying assumptions. Section four provides estimation results from two alternative approaches, a test for the underlying parallel trend assumption, and a placebo test to check for robustness. Section five concludes by highlighting some of the paper's innovative aspects and drawing out key implications for policy and research.

2. Background and motivation

Redistributive land reform can be justified as a means to either increase productivity of land use or asset accumulation by the poor. However, although an increasing body of evidence points towards negative effects of unequal asset distribution on subsequent growth through various channels, most of the empirical evidence regarding land reforms has focused on productivity effects. West Bengal's 1978 land reform programme provides a unique opportunity to test the impacts of land reform on asset accumulation

and at the same time draw conclusions on productivity that could be of great policy relevance.

2.1 The importance of the land ownership distribution

A large body of empirical literature on agricultural production has shown that, due to the transaction costs involved in supervising hired labour (Carter 1984; Feder 1985; Eswaran and Kotwal 1985; Benjamin 1995), a farm structure based on owner-operated units is more efficient than one based on wage labour (Berry and Cline 1979; Binswanger et al. 1995). Although market mechanisms can, in principle, help to equalize the operational land distribution and thus maximize aggregate production, challenges remain. For example, many studies demonstrate the potential of increased output that can materialize through a variety of channels if, under appropriate conditions, land or its ownership is transferred from large or absentee landlords to cultivators. First, transaction costs and borrowing constraints may reduce the number of sales (and in some cases also rental) market transactions well below the optimum. Second, to the extent that land transfers in rental and sales markets will be associated with transfers of resources among the parties involved even productivity-enhancing transactions may have undesirable distributional implications. At low levels of development, and especially with high inequality in the land ownership distribution, landlords may be able to reduce the benefits to tenants by exerting market power. At higher levels of development, speculative elements may lead to a discrepancy between the market value of land and its underlying fundamental value that is based on profits from agricultural production, thereby preventing movement of land to the most productive producers. Third, with imperfections in other markets, e.g. those for labour or credit, market transactions may not achieve first best outcomes in terms of production. As a result, government interventions that aim to provide the most productive producers with land access can have significant social and economic benefits (Chau 1998; Carter and Zimmerman 2000).

In addition to its potential to increase productivity, the redistribution of assets implied in land reform can alter the ability of poor households to access credit markets. Insofar as imperfections in such markets had prevented poor households from making indivisible investments, e.g. in education, that will enable them to fully exploit their productive capacity or actively participate in political processes, land reform could break a vicious cycle of poverty and create the pre-conditions for faster and broad-based growth by enabling the poor to accumulate physical as well as human capital at a rate that corresponds to their innate ability (Galor and Zeira 1993; Gersbach and Siemers 2005). In such situations, one-time asset redistribution could bring the level of investment closer to the social optimum and also be beneficial to the individuals concerned. This effect can be enhanced if limited access to economic resources translates into lack of political influence or segregation (Acemoglu et al. 2004), social unrest, and strife that may translate into to a downwards spiral of high inequality, bad institutions, and low economic growth.²

² For a theoretical underpinning of the relationship between distribution and provision of public goods, including social cohesion, see (Bardhan and Ghatak 1999). Especially in rural environments where other markets are imperfect, greater access to assets can help improve households' nutritional status, risk-bearing capacity, and investment incentives, in addition to enhancing their ability to access credit markets (Burgess 2001).

Evidence from cross-sectional regressions pointing towards a negative relationship between initial levels of inequality and subsequent growth in developing countries (Barro 2000; Vollrath 2007) is increasingly substantiated at the micro-level with differences in land holding patterns affecting productivity, growth, and social articulation (Nugent and Robinson 2002; Banerjee and Iyer 2005). The political implications of unequal land ownership and associated patron-client relationships are illustrated by the case of Chile where, before introduction of a secret ballot in 1958, localities with more attached labourers (*inquilinos*) as well as land concentration exhibited much stronger support for right-wing parties traditionally associated with landed oligarchy, an effect that disappeared with the reform (Baland and Robinson 2008).

Recent contributions provide strong evidence not only for the growth-retarding effects of unequal land ownership distributions but also the potentially far-reaching political impacts of land reform. For example, empirical data on some 3,000 US counties show that land inequality, contemporaneous and historical (1890), had a strong negative impact on level of educational expenses and tax revenue per capita in 1930, at a time when local government paid for 82 per cent of the entire educational system. These effects are not only robust but quantitatively large; a one standard deviation increase in inequality is associated with a 18 per cent decline in education expenditures and a drop in tax revenues of between 9 and 23 per cent (Ramcharan 2010). Consistent with a theoretical framework where land concentration slows down the emergence of human-capital promoting institutions and the associated pace and nature of the transition from an agrarian to an industrial economy, lagged decadal changes in land concentration are found to be associated with significantly reduced growth of education expenditure per student at the state level over the 1900-1940 period (Galor et al. 2009). The negative impact of land concentration is not limited to education; data also point towards a negative impact of land inequality on financial sector development that could, for example, arise from the fact that greater availability of credit would increase wages and competition for labour while at the same time depriving large landlords of their ability to use exclusive credit access as a means for extracting rents. In fact, state- and county-level evidence from the US in the 1920s and 1930s is suggestive of such a link which, according to estimates, persisted until 1970s and 1980s when bank liberalization equalized credit access (Rajan and Ramcharan 2009). All this suggests that, land reforms could profoundly affect economic development through a number of channels.

Historically, land reform was undertaken in many countries at large scale, in terms of area affected and number of beneficiary households. Land reforms in Japan, Korea, and Taiwan at the end of World War II redistributed between 30 per cent and 40 per cent of the cultivated area, affecting about two-thirds of rural households. Although they were drawn out over longer time periods, reforms in Bolivia, Nicaragua, Peru, and Mexico, affected sizeable portions of their countries' arable land endowment and benefited up to a third of the rural population. However, compared to the magnitude of efforts, evidence on their effect is scant and often plagued by lack of a credible counterfactual. In Japan, Korea, and Taiwan, land reforms helped improve productivity and set the stage for an impressive increase in non-agricultural development (Jeon and Kim 2000). In the Philippines, early land reforms that benefited more than 0.5 million households and green revolution technology, improved household welfare (Otsuka 1991; Balisacan and Fuwa 2004) and increased investment and human capital accumulation (Deininger and

Olinto 2001). Although quite effective, land reforms undertaken immediately after independence in some African countries, e.g. Kenya and Zimbabwe (Scott 1976; Gunning et al. 2000; Deininger et al. 2004) were often abandoned for political reasons (Kinsey and Binswanger 1993). In Latin America, reforms distributed comparatively large amounts of land (Barraclough 1970; Eckstein and Horton 1978; Jarvis, 1989) but often failed to improve productivity and were insufficient to help overcome deep-rooted structural inequalities (de Janvry and Sadoulet 1989). Following a relative decline of interest in the topic during the late 1970s, it has received renewed attention recently, partly because growth in countries with high land inequality often failed to narrow gaps between the rich and the poor, implying that the task of reform remains unfinished in many respects (Lipton 2009). As land reform is only one instrument that can be used to bridge such asset-gaps, the empirical evidence of its potential and limitations is important.

2.2 Land reform in India: evidence on level of implementation and impact

Given the long-standing inequality in the distribution of productive assets, especially land, which India inherited at independence, land reform was a key issue in the policy debate at independence and remained so ever thereafter. Three main types of land reforms are (i) abolition of intermediaries (*zamindars*) shortly after independence; (ii) tenancy laws aiming to improve welfare of sitting tenants through registration to enhance tenure security and limits on the amount of rent paid;³ (iii) and ceiling laws that limit the amount of land that can be owned by any individual or household and allow expropriation of any ‘above ceiling’ land beyond this limit to transfer it to poor farmers or landless workers. With the exception of the first, progress in implementing reforms was slow. Also, as the Indian constitution makes the implementation of land reforms a state responsibility, implementation varies widely across states.

Given states’ responsibility for land reform, a popular way to identify its impact is to rely on differences in levels of reform effort or implementation across states. Indeed, a large part of the literature on land reform impacts in India relies on administrative data at state or district level. An influential study uses the cumulative number of land reform laws in a state by category (abolition of intermediaries, tenancy or ceiling reform, land consolidation) to infer impacts. The number of land reform laws is found to have had a positive effect on poverty reduction but not on productivity, something that could be explained as possibly arising from general equilibrium effects on wage rates (Besley and Burgess 2000). Subsequent studies draw on such data to point towards pronounced equity-efficiency trade-offs. Specifically, use of an output measure that excludes livestock yields a positive though insignificant impact of tenancy reform and a negative and significant impact of land ceiling and consolidation laws. This is interpreted to imply that, largely because of tenant evictions by landlords who aimed to pre-empt land reform implementation (Appu 1997), reform legislation by itself led to a decrease in productivity that could have been averted by effective implementation (Ghatak and Roy 2007).⁴ Household data, together with state-level information on actual implementation

3 Many states combined rent ceiling legislation tenants with either a complete prohibition of land leasing or provisions to provide tenants who had been on the land for some time with very strong occupancy rights, thus limiting supply of new land to the rental market (Deininger et al. 2008).

4 The increased Gini coefficient of operated land in all states, if West Bengal is excluded, is interpreted as supporting this (Ghatak and Roy 2007). Of course, as productivity will be affected by an array of factors, other interpretations are possible.

by type of reform provides one option of addressing this issue. It suggests significant positive reform effects on income, consumption, and accumulation of human and physical capital. Such effects are larger for the poor and can be attributed to implementation, in terms of the area or number of households affected by land reforms, rather than passage of laws (Deininger et al. 2009).⁵

Together with Kerala, West Bengal is one of the states that made serious efforts to implement land reform. Most of the activity is associated with a drive to register tenants and identify ceiling surplus land known as ‘Operation Barga’ (Bandyopadhyay 1986; Lieten 1992) that was initiated shortly after the ascent to power of the Left Front in 1977. A first element of the reform was the vesting of any ‘above-ceiling’ land (i.e. land owned in excess of the ceiling of 12.5 ac) with government and its subsequent distribution to landless or small land owners, commonly referred to as *pattadars*. A second element was rapid registration of sharecroppers, village by village, to prevent them from eviction and provide them with permanent and heritable tenancy rights. The amount of (share) rent to be paid to landlords was limited to 25 per cent (50 per cent if the landlord provides all non-labour inputs) and land leasing through mechanisms other than share tenancy, e.g. fixed rent, was prohibited by law. The law also prohibited transfer of land received via land reform through sale, gift, exchange or (sub)lease. To prevent elite capture, eligibility for reform benefits is tightly regulated.⁶

Determinants of implementation and impact of reforms have long been of interest to researchers. A recent study finds that eligibility rules were generally adhered to, i.e. there was little if any elite capture, and that variation in terms of land reform implementation across villages or gram panchayats can be explained by the level of political competition and village characteristics such as land inequality and caste composition (Bardhan and Mookherjee 2010). District data comparing West Bengal with neighboring Bangladesh—where no such reform had been implemented—suggest that 28 per cent of post-reform agricultural productivity growth can be attributed to tenancy reform. A pipeline comparison based on inter-district variation in programme implementation within the state yields similar results (Banerjee et al. 2002). Similarly, a village-level study estimates the effect of tenancy registration on rice yields to be of similar magnitude (Bardhan and Mookherjee 2007). To be able to interpret these estimates as reform effects, a number of rather stringent assumptions regarding identification and absence of aggregation- and reporting-bias, need to be satisfied. Individual data can avoid some of these and allow distinguishing effects by reform type. A recent study finds that tenancy reform (*barga*), but not of vesting of land (*patta*) had positive productivity impacts, a result that is attributed to uneconomically small holdings sizes and a focus on low quality land under *patta* (Bardhan and Mookherjee 2010). It also finds general equilibrium effects from tenancy reform to be surprisingly large, a finding interpreted as resulting from the fact that reform-induced demand for water by tenants led to higher levels of investment in tubewells emergence of water markets. Still, even aggregate impacts of land reform on yields are modest compared to

5 The positive impact of reform implementation decreases in the time since promulgation of laws, possibly because enforcement becomes more difficult with the age of a law (Deininger et al. 2009).

6 To be eligible for a *patta*, households had to be residents of the village where the land was located and own less than one acre of agricultural land (reduced from 2.47 acres in 1980). In descending order of priority, preference for the allocation of land rights was given to (i) tenants who had cultivated the land before it was vested; (ii) the landless in the village; and (iii) scheduled castes and tribes (SCs and STs).

those ascribed to programmes supporting rural development through credit (Integrated Rural Development Project, IRDP) and input distribution through so-called minikits.⁷

Our study adds to the literature in three respects. First, a focus on individual data enables us to clearly separate effects at the individual from those at the village level and explore differences in impact between *pattadars* and *bargadars*. This is of relevance as, according to existing studies, both the extent of land reform implementation and provision of support services at village level are endogenous. Second, rather than focusing on agricultural productivity, an outcome that will be affected by other factors such as local governments' provision of public goods and the extent to which legal restrictions on transferability of reform land were enforced, we focus on long-term impact of land reform on human capital accumulation. Such effects will be particularly important if, as hypothesized by many studies, poverty traps induced by imperfect credit markets prevent poor individuals from making optimum levels of investment in indivisible assets so that asset redistribution could eliminate credit constraints and bring investment levels closer to the optimum. Finally, as our survey was undertaken 30 years after operation *barga*, we can separate out long-term impacts on beneficiaries in the first and second post-reform generation. This is of interest as the full investment-effects from land reform may fully materialize only with some time lag.

3. Data and approach

Data on levels of education for all individuals in at least two generations of a large household sample in West Bengal allow us to assess the impact of benefiting from land reform on inter-generational changes in educational attainment. Identification is through a difference-in-difference (DID) approach that compares educational gains relative to the parent dynasty by individuals in the first and second post-reform generations between beneficiaries and non-beneficiaries. This section describes data and descriptive statistics before discussing estimation strategy, the underlying assumptions and ways to test them, as well as options for robustness checks.

3.1 Data and descriptive statistics

Our data come from a listing that comprises the entire population of some 94,000 households in a 200 village sample from 10 districts of West Bengal. Villages were selected randomly with probability of selection proportional to the number of beneficiaries in 1978 land reforms, based on official lists. Although significant time had passed since land reform implementation, recall was relatively easy as the event was a watershed in West Bengal's political history. As out-migration at household level was limited, in line with the literature (Munshi and Rosenzweig 2007), attrition remained low and in more than 98 per cent of cases households listed on official beneficiary lists at least one member could be identified.

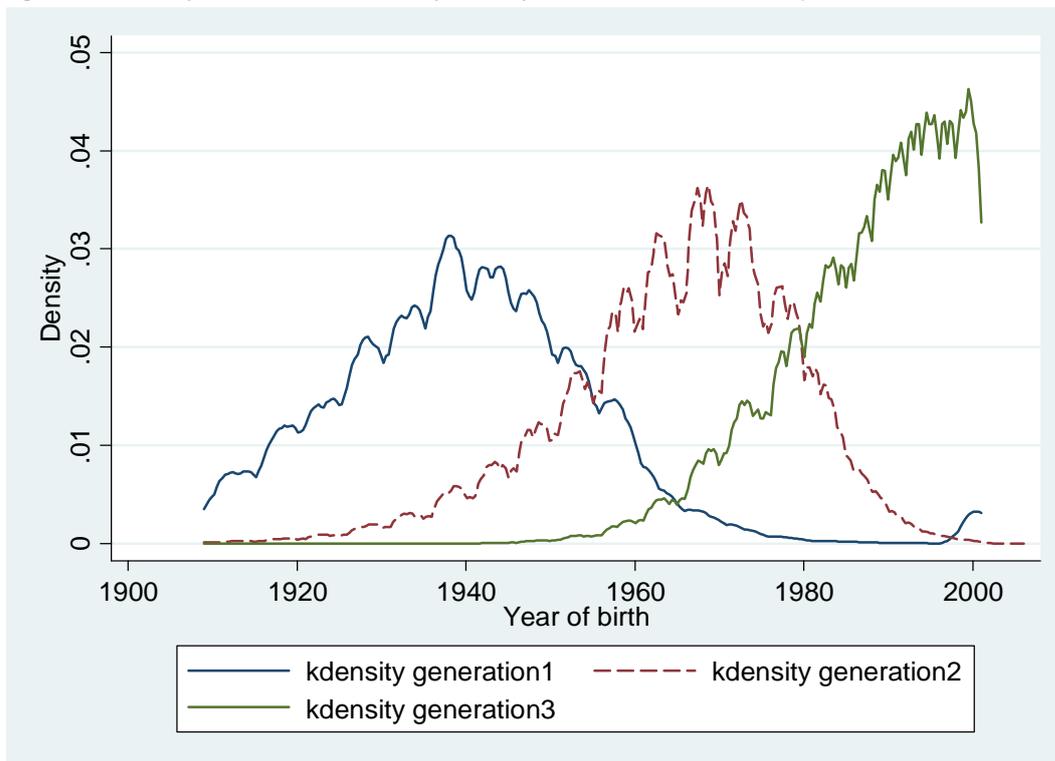
Household level data includes information on how a household was affected by the 1978 land reform and recall-based characteristics (size, assets including land, sector of

⁷ Yields are estimated to have increased by 8-20 percentage points and some 5 percentage points for reform beneficiaries and for non-reform beneficiaries, respectively. These effects are dwarfed, however, by estimated gains from minikits (500 percentage points) and IDRP (100 percentage points).

occupation) in 1978. All these variables, plus information on agricultural production and income, were also asked for 2008. To make inferences on changes in educational attainment by dynasty, information on years of schooling completed was collected for all individuals in three generations: generation I comprising the 1978 head and all his or her siblings (163,534 individuals); generation II including direct descendants (217,855 individuals); and grandchildren (5,567) in generation III. Densities by year of birth as plotted in Figure 1 illustrate that birth years for those individuals in generations 1, 2, and 3 peaked in 1938, 1970, and 1995, respectively.

As land reform was implemented throughout the state in 1978, the set of potential beneficiaries includes all individuals whose educational decisions were made post-1978. To make this operational, we assume that ‘completion of education’ occurs at the age of 14 and consider two polar cases regarding the underlying decision-making process. The first one is that educational decisions will be made right up to the age of 14, while under the second one, decisions will be made once and for all at the age of 6 when the formal education process gets underway. In practical terms, the difference between these assumptions affects who will be included in the regression; under the first assumption we include all individuals 14 or less in 1978 (i.e. between 44 and 14 in 2008) while under the second one only those aged less than 6 in 1978 (i.e. 36 to 14 in 2008) are considered.⁸

Figure 1: Density distribution of birth years by individuals in our sample



Source: Own computation based on 2008/9 West Bengal listing survey.

⁸ This assumption that can be justified by the rather low mean level of education (3.5 and 6.2 years in generations 2 and 3), and the fact that only 5 per cent of observations in generation II and III, respectively have more than 9 years of schooling.

Table 1 displays descriptive statistics at the household level for the two time periods with information for the initial (1978) period in the top panel, the 2008 period in the middle, and differences between the two periods in the bottom. Figures in different columns are for the entire sample of almost 96,000 households (col. 1), the 12,759 and 83,167 households who did or did not benefit from land reform, respectively (cols. 2 and 3), and the 6,838 and 5,921 beneficiaries from *barga* or *patta* programmes (cols. 4 and 5).⁹ With few exceptions, differences between beneficiaries and non-beneficiaries or beneficiary types are highly significant. This suggests that use of estimates controlling for household-level fixed effects will be desirable and we will need to ensure that there are no pre-existing differences.

Virtually all of the sample households depend on agriculture which, either through own account farming (40%) or wage labour (36%), provided the main source of livelihood for more than three quarters (76%) of them in 1978. It continued to do so for almost two-thirds (a total of 59%, 29% of farmers and 30% of wage workers) in 2008, implying that increases in non-agricultural wages and self-employment as main source of employment remained, with 8 per cent and 10 per cent, respectively, limited over the period and the main change for land reform beneficiaries was their move out of agricultural labour into self-employed farming. Also, levels of educational achievement improved only slowly over time and from a low base. For example, only a quarter (25%) of heads were literate in 1978, a share that remained below one half (43%) in 2008. With levels of initial landlessness at 57 per cent, socio-economic differentiation was pronounced and per capita income, even in 2008, remained very low. While a decrease in the incidence of deficient roofing and walls by 40 and 15 percentage points, respectively, points towards improving housing conditions, population growth resulted in a significant reduction of area available and increased fragmentation as area owned even for land owners almost halved from 2.64 to 1.35 ac. per household.

In line with what is reported by other studies, land reform was well targeted to segments of the population with low initial endowments. Beneficiary households were more likely to belong to scheduled castes or tribes (56% for *barga* and 73% for *patta* beneficiaries compared to 43% by the population) and had lower levels of initial endowments with regard to head's education (0.3 years below), housing conditions (86% vs. 70% with a bad roof) a higher incidence of landlessness (65% vs. 54%). However, reform did not eliminate beneficiaries' poverty; even 30 years after having received an asset transfer, their level of welfare remained, with per capita income of Rs. 4,528 vs. Rs. 5,615 for the entire sample, well below average.

Focusing on changes over time in the bottom panel, we note that in terms of the measures that can be obtained based on recall, improvements in land reform beneficiaries' housing conditions were at par with that of the rest of the population and beneficiaries were about equally likely to move out of agricultural wage work and less likely to get out of farming. A total of 65 per cent of reform beneficiaries, compared to 54 per cent in the general population, were landless in 1978, a share that declined to 57 per cent in 2008.

⁹ As information for 1978 is based on recall, descriptive statistics are illustrative only. Econometric analysis relies only on variables (education, land ownership, and caste) that are either time invariant or that can be obtained reliably in this manner.

Table 1: Household characteristics in 1978 and 2008

	Total	Beneficiary		Sig.	Type of beneficiary		Sig.
		No	Yes		Barga	Patta	
<i>1978</i>							
Household size (no.)	6.21	6.21	6.19	***	6.49	5.84	***
Members 14-60	3.49	3.48	3.57	***	3.76	3.36	***
Dependents	2.72	2.73	2.61	***	2.73	2.48	***
SC/ST	0.45	0.43	0.64	***	0.56	0.73	***
Head literate	0.25	0.26	0.18	***	0.22	0.14	***
Head's education (years)	1.24	1.29	0.95	***	1.042	0.85	***
Landless	0.57	0.54	0.65	***	0.55	0.75	***
If land access, size of land (acres)	2.64	2.74	1.88	***	2.00	1.65	***
Bad roof (thatch/plastic/mud)	0.72	0.70	0.86	***	0.83	0.90	***
Bad wall (mud/bamboo)	0.70	0.69	0.82	***	0.85	0.78	***
<i>Head's occupation</i>							
Farming	0.40	0.39	0.48	***	0.61	0.33	***
Agric. wage labourer	0.36	0.35	0.41	***	0.30	0.54	***
Non-agric. wage worker	0.12	0.13	0.06	***	0.05	0.07	***
Self-employed	0.12	0.13	0.05	***	0.04	0.06	***
No. of obs.	95,926	83,167	12,759		6,838	5,921	
<i>2008</i>							
Household size (no.)	4.75	4.70	5.09	***	5.22	4.93	***
Members 14-60	3.04	3.00	3.34	***	3.42	3.24	***
Dependents	1.71	1.70	1.75	***	1.80	1.69	***
SC/ST	0.45	0.43	0.64	***	0.56	0.73	***
Head literate	0.43	0.44	0.35	***	0.40	0.30	***
Head's education (years)	3.11	3.23	2.30	***	2.75	1.79	***
Landless	0.58	0.58	0.57		0.45	0.70	***
If land access, size of land (acres)	1.35	1.37	1.22	*	1.17	1.33	
Bad roof (thatch/plastic/mud)	0.32	0.30	0.48	***	0.45	0.52	**
Bad wall (mud/bamboo)	0.55	0.52	0.72	***	0.72	0.71	
Income per capita (Rs.)	5467.94	5615.97	4528.77	***	4640.02	4400.40	**
<i>Head's occupation</i>							
Farming	0.29	0.26	0.47	***	0.57	0.34	***
Agric. wage labourer	0.30	0.29	0.32	***	0.23	0.42	***
Non-agric. wage worker	0.20	0.21	0.09	***	0.08	0.11	***
Self-employed	0.22	0.23	0.12	***	0.12	0.13	
No. of obs.	95,926	83,167	12,759		6,838	5,921	
<i>Changes between 1978 and 2008</i>							
Household size (no.)	-1.45	-1.50	-1.10	***	-1.27	-0.91	***
Head literate	0.18	0.18	0.17	***	0.19	0.15	***
Landless	0.01	0.02	-0.08	***	-0.10	-0.05	***
If land access, size of land (acres)	-1.43	-1.52	-0.70	***	-0.72	-0.66	
Bad roof (thatch/plastic/mud)	-0.40	-0.40	-0.39	***	-0.39	-0.38	***
Bad wall (mud/bamboo)	-0.15	-0.16	-0.10	***	-0.13	-0.07	***
<i>Head's occupation</i>							
Farming	-0.11	-0.12	-0.01	***	-0.04	0.01	***
Agric. wage labourer	-0.07	-0.06	-0.09	***	-0.07	-0.12	***
Non-agric. wage worker	0.08	0.08	0.03	***	0.03	0.04	*
Self-employed	0.10	0.10	0.07	***	0.07	0.07	

Source: Own computation based on 2008/9 West Bengal listing survey.

Assessment of levels and changes in levels of human capital across generations in Table 2 supports earlier conclusions. First, land reform targeted most backward parts of the population; in the parent generation beneficiaries had significantly lower levels of human capital than non-beneficiaries with differences were particularly pronounced for females in this generation who, with an average of 0.5, had less than half the level of schooling than non-beneficiaries (1.08). Second, male beneficiaries caught up quickly with non-beneficiaries; in fact the increases observed in generations II and III compared to generation I were larger than those for non-beneficiaries. Finally, while females' rate

of catch up in the second generation was slower for beneficiaries than non-beneficiaries, it was indistinguishable for the third generation. As these do not control for potential cohort- or village-specific effects and may confound aggregate effects that are not due to a household having benefited from land reform, econometric analysis will be appropriate.

Table 2: Levels and changes in human capital accumulation

	Total	Beneficiary		Sig.	Type of beneficiary		Sig.	No. of Obs.
		No	Yes		Barga	Patta		
<i>Generation I</i>								
All	2.05	2.16	1.33	***	1.56	1.04	***	163,534
Male	2.59	2.75	1.72	***	2.01	1.34	***	109,434
Female	0.95	1.08	0.50	***	0.60	0.37	***	54,100
<i>Generation II</i>								
All	3.55	3.62	3.07	***	3.32	2.76	***	217,855
Male	3.83	3.88	3.49	***	3.77	3.15	***	189,812
Female	3.04	3.15	2.42	***	2.66	2.13	***	79,157
<i>Generation III</i>								
All	6.21	6.31	5.74	***	6.11	5.17	***	5,567
Male	6.43	6.47	6.20	***	6.52	5.69	**	2,854
Female	5.99	6.14	5.24	***	5.65	4.62	***	2,713

Source: Own computation based on 2008/9 West Bengal listing survey.

3.2 Estimation strategy

Our identification strategy uses individuals' educational gains in the first and second post-reform generation—relative to the parent generation in the same household to eliminate time invariant household characteristics—as the relevant outcome variable. This measure is then compared between beneficiary and non-beneficiary households. If there are no structural and pre-existing differences in trends of growth rates of educational attainment between the two groups, the gain can be attributed to land reform.

Let village be indexed by v , households (dynasties) by h , gender by j and let \bar{E}_{vhj}^{old} be the mean education level by the parent generation, defined as the 1978 head and all of his or her siblings who were 14 years or older in 1978. For each individual i younger than 14 in 1978 (and 14 or older in 2008), the (gender specific) 'educational gain' relative to the old generation in household h , ΔE_{vhi}^j can be constructed as the difference between i 's education in 2008 and \bar{E}_{vhj}^{old} . The regression to be estimated is then

$$\Delta E_{vhi}^j = \alpha + \beta R_{vh} + \phi X_{vh} + \rho Z_{vhi}^j + \delta D_v + \varepsilon_{vhi}^j \quad (1)$$

where R is a dummy indicating whether household h benefited from land reform, either as a *bargadar* or a *pattadar*, X_{vh} and Z_{vhi} are vectors of household and individual characteristics that include gender and generation as well as a set of dummies to control for age, D is a set of birth-year and village dummies to control for time-variant and location-specific effects, respectively, and ε_{vhi}^j is an *iid* error term. The hypothesis of a positive impact of land reform on educational attainment, i.e. a larger 'educational gain' for those who benefited from reform than those who did not, translates into $\beta > 0$. Interactions between R and elements of Z or X , e.g. gender, caste, generation, or initial land holding, allow to test whether reform impacts differ depending on these pre-existing attributes or by generation.

To be able to interpret β , the difference-in-difference estimate in (1), as reform impact, the ‘parallel trend assumption’ that reform participation is uncorrelated with the unobserved time-varying group specific characteristics needs to hold. Availability of a large number of observations from long before the reform allows us to formally test whether, as implied by this assumption, educational levels without reform for the two groups would have grown at equal rates. To do so, we implement a placebo test for a hypothetical earlier reform date in 1958 to allow equal intervals of 20 years before and after this date. Noting that educational decisions for anybody younger than 14 (or 6) in 1978 would be affected by the actual reform, this would imply an old cohort aged above 14 or 6 in 1958 (i.e. above 34 or 26 in 1978) and a young cohort from 14 to 34 (or 26) years in 1978. If trends of our outcome variable (i.e. educational attainment) were indeed different between the two groups even without the reform, coefficients on a reform dummy in such a regression will differ from zero. By contrast, finding an ‘impact’ of the pseudo-reform would imply that growth rates between the two groups differed independently from land reform, presumably because of unobserved time-varying characteristics between the two groups that would have implications for the validity of the parallel trend assumption.

4. Econometric results

We use our DID strategy on the entire sample to assess the impact of land reform on increases in educational attainment, to test for different impacts between the first and second post-reform generation, and to explore whether effects vary by reform type. Results point towards a significant and positive impact that increases markedly in the second generation with surprising consistency across methods.

Table 3 presents estimates for equation (1) regarding impacts of land reform on increments in members’ educational attainment controlling for clustering at village level. Top and bottom panels use cutoffs of 14 and 6 years, for samples of 128,374 and 72,132 individuals, respectively. As discussed earlier, the dependent variable is the intra-dynasty difference in educational gain, dummies for year of birth and village are included throughout, and clustering at village level is adjusted for. We present specifications estimating aggregate effects across generations first (cols. 1 and 2), followed by models that allow these effects to vary between generations (cols. 3 and 4). Note that coefficients can be interpreted in terms of changes rather than levels, i.e. positive or negative coefficients imply that a group would catch-up or fall behind at a rate that differs from the average.

The base regression in cols. 1 and 2 suggests a positive and highly significant effect of land reform in the aggregate; irrespectively of the cut-off, educational gains by beneficiary individuals are estimated to amount to 0.09 years without and to 0.33 or 0.45 years with caste or gender interactions. Females’ levels of educational attainment increased at rates that were much higher than the average, suggesting that they caught up with males independently of land reform. Tests reported in the bottom panel imply that reform made females and SCs/STs better off in absolute but not in relative terms. In other words, even though they benefited from land reform, the magnitude of the benefits accruing to them was smaller than that attained by males and non-SCs/STs. Still, comparing the magnitudes of the interactions in col. 2, estimated land reform effects on

Table 3: Impact of land reform on human capital formation

	<i>Cutoff at 14 years</i>			
Beneficiary (α_1)	0.090*** (2.83)	0.334*** (6.53)	0.299*** (5.79)	0.312*** (5.99)
Female (α_2)	0.376*** (16.38)	0.461*** (18.59)	0.463*** (18.64)	0.463*** (18.64)
SC/ST (α_3)	-0.190*** (7.39)	-0.173*** (6.36)	-0.173*** (6.39)	-0.173*** (6.39)
Landless (α_3)	-0.449*** (20.29)	-0.447*** (20.20)	-0.446*** (20.16)	-0.447*** (20.16)
Beneficiary*female (β_1)		-0.581*** (9.04)	-0.588*** (9.13)	-0.610*** (9.33)
Beneficiary*SC/ST (β_2)		-0.122** (1.97)	-0.111* (1.79)	-0.122* (1.93)
2 nd Gen. beneficiary (γ_1)			0.842*** (5.77)	0.494** (2.09)
2 nd Gen. benef. *female (δ_1)				0.582* (1.91)
2 nd Gen. beneficiary*SC/ST (δ_2)				0.288 (0.99)
<i>Tests:</i>				
$\alpha_1 + \beta_1 = 0$		-0.247*** (3.82)	-0.289*** (4.44)	-0.298*** (4.52)
$\alpha_1 + \alpha_2 + \beta_1 = 0$		0.214*** (3.41)	0.174*** (2.75)	0.163*** (2.56)
$\gamma_1 - \alpha_1 = 0$			0.543*** (3.39)	0.182 (0.72)
$\beta_1 + \delta_1 = 0$				0.028 (0.33)
$\beta_2 + \delta_2 = 0$				0.166 (0.58)
$\alpha_3 + \beta_2 + \delta_2 = 0$				-0.007 (0.01)
No. of observations	128,374	128,374	128,374	128,374
R-squared	0.12	0.12	0.12	0.12
	<i>Cutoff at 6 years</i>			
Beneficiary (α_1)	0.089** (2.02)	0.453*** (6.38)	0.418*** (5.82)	0.444*** (6.09)
Female (α_2)	0.670*** (21.16)	0.779*** (22.72)	0.780*** (22.74)	0.780*** (22.74)
SC/ST (α_3)	-0.085** (2.35)	-0.050 (1.30)	-0.050 (1.32)	-0.050 (1.31)
Landless (α_3)	-0.330*** (10.67)	-0.325*** (10.51)	-0.324*** (10.48)	-0.324*** (10.48)
Beneficiary*female (β_1)		-0.729*** (8.28)	-0.732*** (8.31)	-0.769*** (8.52)
Beneficiary*SC/ST (β_2)		-0.247*** (2.88)	-0.237*** (2.76)	-0.261*** (2.97)
2 nd Gen. beneficiary (γ_1)			0.558*** (3.33)	0.093 (0.34)
2 nd Gen. benef. *female (δ_1)				0.698** (1.96)
2 nd Gen. beneficiary*SC/ST (δ_2)				0.457 (1.37)
Test for $\alpha_1 + \beta_1 = 0$		-0.276*** (3.02)	-0.314*** (3.51)	-0.325*** (3.58)
Test for $\alpha_1 + \beta_2 = 0$		0.206*** (3.23)	0.181*** (2.82)	0.183*** (2.82)
No. of observations	72,132	72,132	72,132	72,132
R-squared	0.09	0.09	0.09	0.09

Absolute value of t statistics in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%. Year of birth and village dummies included throughout. Cluster effect at village level controlled for. Young cohort is 14-44 (14-36) in 2008 and old cohort 14-44 (14-36) in 1978 for top (bottom) panel. Source: Own computation based on 2008/9 West Bengal listing survey.

educational attainment were large enough to compensate for pre-existing disadvantages in access to education by SCs/STs as well as females.

Allowing effects to differ across generations suggests that land reform-induced impacts on gains in human capital increased significantly in the 2nd post-reform generation. Depending on the cut-off, 2nd generation descendants of land reform beneficiaries are estimated to enjoy an educational gain of 1.24 to 0.98 years (col. 3), between two to three times the estimated first-generation effect of this intervention. This suggests that, in our setting, much of the gains from land reform-induced asset transfers accrue to the grandchildren of reform beneficiaries rather than their direct descendants. This is in line with studies suggesting that the full impact from positive asset shocks takes time to materialize and may eventually provide a basis for beneficiaries' off-spring to move out of agriculture (Quisumbing and Maluccio 2003). Tests in the bottom panel indicate that the hypothesis of second generation benefits exceeding those enjoyed by the first generation cannot be rejected statistically. Point estimates in the bottom panel are similar in magnitude to those in the top, suggesting that general results are not sensitive to assumption as to when educational decisions are completed. At an anecdotal level, the large estimated results are consistent with the fact that the 1978 land reforms continue to enjoy strong popularity and are generally viewed as one of the main reasons for the communist party to have governed the state without interruption since then.

Before trying to interpret these results, we examine their robustness. Table 4 reports results from the placebo test where, as discussed, the only difference to Table 3 is that, instead of 1978, the reform date is set to 1958 and only individuals who were older than 14 in 1978, and thus had their educational decisions made at the time when land reform was actually implemented, are included. This reduces sample size to 32,633 or 29,310 for the case of age cut-offs at 14 or 6 years, respectively. We note that over this period members of households who eventually benefited from land reform in 1978 fell back compared to the average by some 0.3 years of education, a coefficient that is statistically highly significant. While the assumption of parallel trends between treatment and control group in the pre-intervention period does not hold, land reform seems to have contributed to a reversal of trends, in line with evidence of the programme having targeted the poor. The estimated coefficient in Table 3 is thus a lower bound on the true effect of land reform on participating individuals. It is also worth noting that, although few of the other interactions are significant, dummies for female, SC/ST, and landless individuals are consistently negative, suggesting that before reform was implemented, education by these groups strongly diverged from the mean. In line with the literature that has long emphasized the importance of asset redistribution in bringing the economy towards a different equilibrium, this raises the possibility of broader effect of land reform on social interactions and access to economic opportunities over and above that impacts on direct beneficiaries estimated here.

Although the above estimates did not distinguish different types of land reform, we can do so by splitting the land reform dummy into two, depending on type of land reform implementation (*barga* or *patta*). Results from doing so are reported in Appendix Table 1. Interestingly, in view of the differences in terms of economic performance, we cannot reject the hypothesis of land reform impacts having been equal for the two types of

Table 4: Results from placebo-tests

<i>Cutoff at 14 years</i>				
Beneficiary (α_1)	-0.255*** (4.27)	-0.347*** (3.78)	-0.349*** (3.80)	-0.349*** (3.80)
Female (α_2)	-0.831*** (17.84)	-0.855*** (17.05)	-0.855*** (17.05)	-0.855*** (17.05)
SC/ST (α_3)	-0.693*** (13.97)	-0.707*** (13.47)	-0.707*** (13.46)	-0.707*** (13.47)
Landless (α_3)	-1.021*** (23.93)	-1.022*** (23.94)	-1.023*** (23.95)	-1.023*** (23.95)
Beneficiary*female (β_1)		0.168 (1.29)	0.166 (1.28)	0.161 (1.24)
Beneficiary*SC/ST (β_2)		0.099 (0.85)	0.101 (0.86)	0.103 (0.88)
2 nd Gen. beneficiary (γ_1)			1.777 (0.99)	-0.434 (0.12)
2 nd Gen. benef. *female (δ_1)				4.534 (1.03)
2 nd Gen. beneficiary*SC/ST (δ_2)				-0.178 (0.04)
Test for $\alpha_1 + \beta_1 = 0$		-0.197 (1.37)	-0.183 (1.40)	-0.183 (1.44)
Test for $\alpha_1 + \beta_2 = 0$		-0.756*** (2.87)	-0.754*** (2.88)	-0.752*** (2.85)
Observations	32633	32633	32633	32633
R-squared	0.10	0.10	0.10	0.10
<i>Cutoff at 6 years</i>				
Beneficiary (α_1)	-0.163** (2.57)	-0.315*** (3.23)	-0.314*** (3.22)	-0.314*** (3.22)
Female (α_2)	-0.400*** (8.24)	-0.422*** (8.10)	-0.422*** (8.10)	-0.422*** (8.10)
SC/ST (α_3)	-0.473*** (9.09)	-0.504*** (9.16)	-0.504*** (9.16)	-0.504*** (9.16)
Landless (α_3)	-0.697*** (15.51)	-0.700*** (15.56)	-0.700*** (15.55)	-0.700*** (15.55)
Beneficiary*female (β_1)		0.161 (1.17)	0.161 (1.16)	0.161 (1.16)
Beneficiary*SC/ST (β_2)		0.213* (1.73)	0.212* (1.73)	0.213* (1.73)
2 nd Gen. beneficiary (γ_1)			-0.560 (0.22)	-0.479 (0.13)
2 nd Gen. benef. *female (δ_1)				0.000 (0.33)
2 nd Gen. beneficiary*SC/ST (δ_2)				-0.161 (0.03)
Test for $\alpha_1 + \beta_1 = 0$		-0.154 (1.21)	-0.153 (1.21)	-0.153 (1.21)
Test for $\alpha_1 + \beta_2 = 0$		-0.209 (1.26)	-0.210 (1.25)	-0.209 (1.27)
Observations	29310	29310	29310	29310
R-squared	0.05	0.05	0.05	0.05

Absolute value of t statistics in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%

Year of birth and village dummies included throughout. Cluster effect at village level controlled for.

Young cohort is 14-34 (14-26) in 1978 and old cohort 14-34 (14-26) in 1958 for top (bottom) panel.

Source: Own computation based on 2008/9 West Bengal listing survey.

beneficiaries in most regressions.¹⁰ To put the magnitude of the estimates into perspective, we compare them to those obtained in literature evaluating impacts of interventions aimed specifically at increasing educational attainment. Doing so suggests that the size of the effects obtained here is non-negligible. Mexico's *oportunidades* (Progresa) programme providing cash grants conditional on children's school attendance is estimated to have induced a gain in long-term educational attainment of 0.5-0.6 years (Todd and Wolpin 2006), somewhat higher than the estimated first generation effect but smaller than the second generation effect obtained here. In Vietnam, a 50 per cent increase in household per capita consumption led to an increase in schooling of 0.28 years, from 7.53 to 7.81 (Glewwe and Jacoby 2004), suggesting that the size of the impact obtained here is non-negligible. Of course, more detailed analysis will require adjustments for programme cost and outreach that are beyond the scope of this paper.

5. Conclusion

Our paper was motivated by a growing body of literature pointing to potentially adverse economic and political effect of high asset inequality and that high levels of such inequality continue to persist in a number of developing countries. In poor countries, land reform is one of the most viable ways to transfer wealth and also create a potential for improved productivity. However, rigorous empirical study of the impact of this type of intervention has been limited. By exploring impact of West Bengal's land reform on beneficiaries' human capital accumulation, this paper contributes to the literature in two ways.

Substantively, we find that, while it failed to raise productivity of land use, land reform—of either form—increased levels of education by up to 0.3 years in the first and more than one year in the second post-reform generation. The estimated magnitude of effects compares favourably to those from other programmes such as conditional cash transfers. The robustness of the result to different estimation methods, together with the fact that a placebo test points to significantly *lower* rather than higher growth in educational attainment by beneficiary dynasties before 1978, increase our confidence in the result. At the same time, the increase of estimated benefits for the second generation suggests that land reform benefits may take some time to fully materialize. Attention to long-term impacts of land reform may usefully complement the literature's emphasis on short-term productivity effects of this intervention in other settings.

At the same time, the large long-term benefits from land reform ascertained here are in contrast to the inconsistent and relatively low levels of productive performance identified by the literature (Ghatak and Roy 2007). In a setting that is still dominated by agriculture, low productivity of land use seriously undermines beneficiary welfare and, the long-term benefits from land reform notwithstanding, finding ways to achieve higher productivity could have a far-reaching effect on the performance of the broader rural economy. Such measures are likely to differ between the two types of reform

10 More disaggregated time varying data will be needed to explore reasons that might underlie this rather surprising phenomenon and the inter-temporal trade-offs made by the affected households. Such study could provide interesting insights into the dynamics of asset accumulation by the poor and the channels through which land reform affects productivity and welfare.

beneficiaries: For *bargadars*, the continued obligation to pay (share) rent to land owners may lead to inefficiently low effort supply in any given production period while at the same time undermining investment incentives by both the tenant and the landlord. While *pattadars* often received very marginal lands, restrictions on the transferability of land—even by leasing—received via land reform may further preclude efficient land use, by keeping households with low agricultural skills in the sector and neglecting the fact that land reform may also be a springboard for beneficiaries' offspring to move out of agriculture. Follow-up research to quantify these effects and explore the scope for policy changes to overcome them could help contribute to realization of productivity gains to match the long-term educational impacts of land reform more closely and, in doing so, improve both beneficiary welfare and broader growth of West Bengal's rural economy.

Appendix Table 1: Impact of land reform on human capital formation (separating *barga* and *patta* effects)

	(2)	(3)	(4)	(6)
<i>Barga</i> beneficiary (α_1)	0.170*** (2.72)	0.279*** (3.42)	0.246*** (3.01)	0.265*** (3.12)
<i>Patta</i> beneficiary (β_1)	-0.021 (0.27)	0.404*** (3.31)	0.368*** (3.01)	0.364*** (2.93)
Female	0.376*** (6.75)	0.461*** (7.88)	0.463*** (7.90)	0.463*** (7.90)
Landless	-0.444*** (9.22)	-0.443*** (9.20)	-0.442*** (9.17)	-0.442*** (9.17)
SC/ST	-0.188** (2.13)	-0.174* (1.96)	-0.175* (1.97)	-0.175* (1.97)
<i>Barga</i> beneficiary*female (α_2)		-0.399*** (3.85)	-0.406*** (3.92)	-0.431*** (4.05)
<i>Patta</i> beneficiary*female (β_2)		-0.835*** (6.43)	-0.840*** (6.48)	-0.858*** (6.70)
<i>Barga</i> beneficiary*SC/ST (α_3)		0.024 (0.21)	0.035 (0.30)	0.013 (0.10)
<i>Patta</i> beneficiary*SC/ST (β_3)		-0.265* (1.80)	-0.258* (1.75)	-0.244* (1.65)
2 nd Gene. <i>barga</i> beneficiary (α_4)			0.727*** (2.85)	0.277 (0.65)
2 nd Gene. <i>patta</i> beneficiary (β_4)			1.011*** (3.94)	1.098*** (2.78)
2 nd Gen. <i>barga</i> ben.*female (α_5)				0.569 (1.36)
2 nd Gene. <i>patta</i> ben. *female (β_5)				0.520 (1.03)
2 nd Gene. <i>barga</i> ben. *SC/ST (α_6)				0.572 (1.08)
2 nd Gene. <i>patta</i> ben. *SC/ST (β_6)				-0.390 (0.86)
<i>Tests</i>				
$\alpha_1 - \beta_1 = 0$ ST effect equal between <i>barga</i> and <i>patta</i>	0.149* (1.86)	-0.125 (0.91)	-0.122 (0.60)	-0.099 (0.69)
$\alpha_2 - \beta_2 = 0$		0.436*** (3.03)	0.434 (3.01)	0.423 (2.98)
$\alpha_3 - \beta_3 = 0$		0.298* (1.78)	0.293 (1.80)	0.25 (1.55)
$\alpha_4 - \beta_4 = 0$			-0.280 (0.83)	-0.821 (1.40)
$\alpha_5 - \beta_5 = 0$				0.049 (0.01)
$\alpha_6 - \beta_6 = 0$				0.962 (1.41)
Observations	128,374	128,374	128,374	128,374
R-squared	0.12	0.12	0.12	0.12

Year of birth and village dummies included throughout. Cluster effect at village level controlled for. Young cohort is 14-44 in 2008 and old cohort 14-44 in 1978 for top (bottom) panel (results for cut-off at 6 years available from the authors upon request). Absolute value of t statistics in brackets.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Source: Own computation based on 2008/9 West Bengal listing survey.

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