



DID HIGHER INEQUALITY IMPEDE GROWTH IN RURAL CHINA?*

Dwayne Benjamin, Loren Brandt and John Giles

We estimate the relationship between village inequality and subsequent income growth for households in rural China. Using a longitudinal household-level survey spanning 1987–2002, we find that households from higher inequality villages experienced lower income growth. However, the effect of local inequality fades by 2002. Our evidence points to unobserved village institutions at the time of economic reforms, associated with household access to higher income activities, as the source of the link between inequality and growth. We address several econometric issues including measurement error and attrition, but underscore others that are probably intractable for all investigations of the inequality–growth relationship.

For researchers estimating the effect of inequality on growth, China would seem a promising ‘laboratory’. Since the start of economic reform in the early 1980s, it experienced plenty of both: per capita income has grown nearly 8% annually, while the Gini coefficient rose from 0.28 to 0.39 (Ravallion and Chen, 2007). There was also significant within-China variation in growth and inequality at the local level (Benjamin *et al.*, 2005). Experience with cross-country aggregate data, however, demonstrates that estimating a robust correlation, let alone a causal relationship between inequality and growth, faces major empirical challenges, some of which stem from less than ideal data.¹ Better data alone, however, cannot solve the causality problem. Kuznets (1955) suggests the opposite chain of causality, from growth to changes in the income distribution; and almost certainly, unobserved heterogeneity is a potential factor, with inequality reflecting other factors that drive growth. Finally, even if we can estimate a ‘reduced form’ effect of inequality on growth, it may still be impossible to identify the channels through which it matters.

In this article, we use the post-reform experience of rural China to determine whether local inequality impeded the growth of household incomes. We are able to address some of the methodological problems that plague cross-country data. By using a repeated, consistently applied household survey we avoid some of the measurement problems endemic in the cross-country setting. At least compared to international variation, the relative similarity of local institutions across villages in China also permits a cleaner isolation of the impact of inequality from other unobserved factors. At the

* Corresponding author: John Giles, Development Research Group, The World Bank, Mail-Stop MC 3-3111, 1818 H Street NW, Washington, DC 20433, USA. Email: jgiles@worldbank.org.

This is a completely rewritten version of the previous working papers, ‘Inequality and Growth in Rural China: Does Higher Inequality Impede Growth’ (University of Toronto Working Paper #237, June 2006; and Institute for the Study of Labor (IZA) Discussion Paper 2344, September 2006). We thank Gustavo Bobonis for helpful comments, and the editor and referees for constructive suggestions for this revision. Benjamin and Brandt gratefully acknowledge the Social Sciences and Humanities Research Council of Canada for financial support, and Giles acknowledges support from the National Science Foundation (SES-0214702).

¹ The potential problems are numerous: cross-country heterogeneity of data and measurement standards, problems of aggregation, measurement error of key variables, short time-series of inequality and growth, and less-than-comparable estimates of inequality.

1 same time, there are sufficient spatial differences in institutions and inequality that
2 exploring the experiences of rural households scattered across villages can inform us
3 about the potential channels by which inequality affects income growth. In particular,
4 we can distinguish between two broad classes of explanations that have been offered as
5 to why inequality affects growth: imperfect factor markets, including credit, or growth-
6 inhibiting institutions.

7 The foundation of this article is a rich panel that tracks household incomes from
8t early in the reform period (1987) to nearly the present (2002). The data allow us to link
9 the detailed trajectories of household income to initial village and household condi-
10 tions. Our question is simple: controlling for a rich set of covariates, did higher village
11 inequality dampen household income growth? There are several advantages to using
12 household-level data.² Most importantly, we can control for a host of household-level
13 variables – notably flexible functions of initial household income – that may be con-
14 founded with local inequality. This helps rule out some potential explanations for the
15 inequality–growth relationship, in particular those that rely on the aggregation of non-
16 linear effects of own-income. With household-level data we are also able to explore
17 ‘within-village’ heterogeneity of the impact of inequality on growth: Are the poor hurt
18 more than the rich? Are households with higher educated members immune to the
19 impact? We are also able to broaden the set of outcomes from ‘income growth’ to other
20 economic variables, like the composition of income, e.g. concentration in agriculture,
21 or participation in off-farm employment, that inform the question of how inequality
22 may affect growth.

23 Repeated observations at the village-level allow us to evaluate the stability and con-
24 sistency of the ‘treatment’ of higher inequality: we examine whether all variation in
25 inequality is the same, and whether it has a constant effect on growth. To do so, we
26 construct a village-level panel of inequality and growth rates, and explore the impact of
27 cross-sectional differences of initial inequality across villages, varying the time periods.
28 These multiple sources of ‘treatment’ provide identifying information on the nature of
29 any causal relationship between inequality and growth. This exercise also underscores
30 the limits to which conventional panel data methods can be employed to address
31 whether unobserved heterogeneity confounds the impact of initial inequality.

32 While we are able to provide robust estimates of the correlation between village
33 inequality at the outset of reforms and subsequent growth, there remain disheartening
34 limits as to what we can learn from even these data. Paramount among these limits, the
35 endogeneity problem is intractable: solving it requires finding instruments that predict
36 initial income inequality, but are otherwise excludable from a growth equation, and in
37 particular are uncorrelated with *any* institutions that may be related to subsequent
38 growth. The next best thing is to trace the correlation of income inequality through
39 various *observable* institutional channels that affect growth. While we previously
40 attempted this (Benjamin *et al.*, 2006), we were unable to find any robust linkages
41 between inequality and measured institutions at the outset of reforms. Again, this
42 problem is thorny to solve, as initial inequality in a village in 1987 will reflect not just
43

44
45 ² Ferreira (2010), among others, has highlighted the importance of using micro-level data to shed light on
46 those mechanisms driving the growth–inequality relationship that cannot be addressed by the cross-country
macroeconomics literature.

1 the immediate institutional structure of the reform period, but the entire history of the
2 village throughout the occasionally tumultuous post-1949 period, e.g. land reform,
3 collectivisation, the Great Leap Forward, the Cultural Revolution, as well as its pre-1949
4 socio-economic structures. Moreover, 'village' inequality will reflect 'local' conditions
5 beyond the village, at the township and even county-level, making it difficult to pin
6 down the precise institutional mechanism.

7 Despite these limitations, we are able to establish robust patterns in the data that are
8t suggestive of the channels by which inequality operates – and which it does not. First,
9 we find that initial inequality has a robust negative effect on household income growth
10 that is impossible to dismiss, confirming a result noted by Ravallion (1998) using data
11 from 1986 to 1991 for southwest China. Second, the effect of inequality fades over time:
12 As villages became more integrated with the wider economy, the influences of initial
13 conditions on trajectories were 'swamped' by rapidly expanding external opportunities,
14 and possibly local institutional change. Third, we find that only inequality from the very
15 beginning (1987) matters, at least for the 15-year period that we observe. There is no
16 evidence that generally rising inequality has an impact on household income growth.
17 Fourth, we find that education and access to off-farm opportunities play a critical role
18 in this relationship: better-educated individuals are less affected by the adverse impact
19 of inequality, and households in more equal villages are better able to move into
20 off-farm wage employment. Fifth, setting aside its link with education, we find that high
21 inequality is an 'equal opportunity' growth inhibitor: rich and poor alike in high-
22 inequality villages suffer a growth penalty.

23 Overall, we interpret our results as suggesting that the effect of inequality reflects
24 something fundamental about the economic and institutional characteristics of villages
25 at the outset of reforms that shaped economic opportunity for all households. Credit-
26 market stories that we expect to be reflected in differences across household income
27 strata, or in the effect of changes in inequality on growth over time, seem much less
28 likely.³ At least in the Chinese context, institution-based hypotheses linking inequality
29 to growth are most relevant.

30 We begin with a brief review of the reasons why inequality is believed to affect growth,
31 and why these factors are relevant in rural China. Next, we provide a formal overview of
32 our empirical framework, highlighting two main issues. First, we discuss the identifi-
33 cation problem, underscoring the difficulty of finding naturally occurring variation
34 that could ever substitute for a proper experiment. There are other empirical prob-
35 lems, however, that we *can* address, including panel-attrition, aggregation and
36 measurement error. Ravallion (1998) suggests that aggregation in particular, and the
37 use of aggregate rather than household-level data, might explain the weakness of the
38 estimated effect of inequality on growth in the previous literature. We provide a
39 detailed explanation of the links between our household-level specification and the
40 aggregate-level regression commonly employed in the literature. After describing the
41 data, we present our core empirical results: estimates of the effect of village inequality
42 on household income growth, and how this evolves over time. As part of this exercise,
43 we line up the household- and village-level regressions, and show that aggregation *per se*
44

45 ³ These results are also inconsistent with 'poverty traps' insofar as incomes are correlated with household
46 wealth and access to credit.

1 is not an issue. We then explore dimensions of the heterogeneity in the response of
2 household growth to inequality, focusing on education, household age and initial
3 household income. The third set of results concerns the impact of inequality on the
4 evolution of village economic structure, and household participation in agriculture,
5 wage labour and family businesses. Our last set of results addresses issues of dynamics,
6 where we exploit the evolution of inequality and growth at the village-level to estimate a
7 series of cross-section specifications that underscore the difficulty of drawing strong
8t conclusions about the general relationship between inequality and growth, from these,
9 and almost certainly other data sets. The final section draws together our conclusions.
10

11 **1. Why Might Village Inequality Affect Growth?**

13 There are three conventional classes of explanation: imperfect credit markets, imper-
14 fect factor markets and political economy.⁴ In the first class of explanations, credit-
15 market constraints tie the ability of households to exploit opportunities for growth to
16 their own resources. As the poorest of households have the fewest resources, holding
17 average village incomes fixed, unequal villages have more resource-constrained
18 households. If credit markets are fully developed, the relationship breaks down, as the
19 distribution of own resources no longer determines the distribution of household
20 growth rates. A second possible channel is through factor markets. Higher income
21 inequality (or inequality of land, human, or physical capital) may be associated with
22 imperfect competition or other impediments to factor-market development that limit
23 opportunities for trade, especially for the poor.⁵

24 The most common channel in the cross-country literature, however, implies the
25 mechanism is through local political economy: unequal communities make different
26 collective choices that affect the growth potential of households. For example, high-
27 inequality communities may adopt more progressive tax structures, as low-income
28 households pressure for redistribution in ways that inhibit growth. This is the
29 conventional taxation-based story offered by Alesina and Rodrik (1994), Persson and
30 Tabellini (1994), and Benabou (1996). Alternatively, the greater homogeneity (equality)
31 of households may facilitate consensus for more efficient tax systems, and higher
32 investment in public goods and services.⁶ Localities also play an important role in
33 targeting assistance to the poor, and international evidence suggests that there may be
34 greater leakage, and thus poorer targeting in more unequal communities.⁷ There may
35

36 ⁴ See Perotti (1996), Aghion *et al.* (1999), and Lloyd-Ellis (2003) for excellent summaries of the cross-**II**
37 country inequality and growth literature, with detailed discussions of the theoretical linkages between
38 inequality and growth.

39 ⁵ Notable examples include Galor and Zeira (1993), Besley and Burgess (2000), Banerjee *et al.* (2001,
40 2002), Galor and Moav (2004), Banerjee and Iyer (2005), and Besley *et al.* (2010).

41 ⁶ Several papers suggest that collective action and provision of public goods may be complicated by high
42 levels of inequality within communities. See for example, Alesina and La Ferrara (2000), Dayton-Johnson
43 (2000), Bardhan *et al.* (2007), Araujo *et al.* (2008), and Khwaja (2009). With respect to public finance,
44 Sokoloff and Zolt (2005) find that high inequality is correlated with more regressive taxes, and less funding of
45 local public investments and services. Glaeser (2006) reviews evidence suggesting that unequal societies are
46 less likely to have governments that respect property rights. Acemoglu *et al.* (2008) show that in the case of
Columbia, it was political, *not* economic (land) inequality that adversely affected long run outcomes, further
reinforcing the importance of this class of explanations.

⁷ See, for example, Galasso and Ravallion (2005), Bardhan *et al.* (2008), Baird *et al.* (2009), and Shankar
et al. (2010).

1 also be strong links between the distribution of income, levels of education and the
2 provision of public education.⁸

3 To what extent can we expect any of these factors to be important at the village, or
4 local, level in rural China? In principle, all three could have been important. At the
5 outset of reforms, formal sources of credit were limited in Chinese villages. Factor
6 markets were also poorly developed. Land for farming, for example, was allocated
7 administratively, with limited opportunities for either land rental or the hiring of
8 labour among households (Benjamin and Brandt, 2002). Migration to the cities, and
9 even other villages, was restricted by the household registration, or *hukou*, system. Last,
10 from the perspective of political economy explanations, the village was administratively
11 important.⁹ Over the period we examine, village governments controlled policy levers
12 that could affect household incomes: They oversaw the allocation of land use rights for
13 cultivated land to households as part of the Household Responsibility System, and
14 exercised control over the allocation and management of other collective assets such as
15 forest land and village-run enterprises.¹⁰ They also had taxing authority, and until the
16 2002 Tax-for-Fee Reform, were the most important provider of local public goods,
17 including primary education, agricultural infrastructure and health care.¹¹ Finally,
18 village leaders played an important role in targeting poor households for assistance.¹²
19 With individual and household geographic mobility severely limited, household
20 income opportunities were heavily shaped by local policy. And in this context,
21 inequality at the village-level may have had an effect on the evolution of household
22 incomes through village policy. Inequality at the village-level was also likely correlated
23 with governance structures at the township and county-level that mattered more
24 broadly for economic policy.¹³

25 Thus, a case can be made that differences in local economic conditions and village
26 institutions paralleled some of those across countries, including along dimensions
27 believed to link inequality to growth. Over time, however, Chinese villages have become
28 less isolated, and access to new markets and opportunities, e.g. through migration, has
29 expanded. Factors that were key determinants of income and institutions in the 1980s
30 may be less important now. Governance reforms such as those associated with the
31

32 ⁸ See, for example, Benabou (1996) or Lloyd-Ellis (2000). This channel may be especially important if there
33 are externalities associated with the distribution of education in the economy (e.g. Acemoglu 1996). Note
34 that, as in Galor *et al.* (2009), imperfect credit markets and political economy channels may interact to give
35 rise to underinvestment in human capital.

36 ⁹ Villages are at the lowest rung of the rural administrative hierarchy. Above villages, township and county
37 governments have authority over some fees and taxes, and above the county lies the provincial government.
38 The Village Organic Law of 1988 formally recognised village 'self-government'.

39 ¹⁰ Early in the reform process, it was likely that inequality was heavily influenced by how collectivisation
40 proceeded. The allocation of use-rights of land to households was complete by 1983. By all indications, the
41 distribution of cultivated land was fairly egalitarian. Reports suggest, however, that this was much less the case
42 with respect to the allocation and sale of other collective assets.

43 ¹¹ See Fan *et al.* (2004) and Zhang *et al.* (2005). Jalan and Ravallion (2002) show that local levels of income,
44 and associated public investments (e.g. roads and health care) are positively related to household con-
45 sumption growth, and help explain the existence of 'geographic poverty traps', whereby households in poorer
46 areas of rural China experience lower growth than those in richer areas.

¹² Over the period covered by this panel, this has included assistance through *wu baohu* programs for those
unable to work and employment in food for work programs (Park *et al.*, 2002).

¹³ Those localities in which local cadre were able to capture the rents associated with de-collectivisation, for
example, were often those in which off-farm activity was effectively discouraged through excessive taxation by
village, township and county governments (Oi, 1989).

implementation of the Village Organic Law may have also narrowed some institutional differences across villages (Martinez-Bravo *et al.*, 2010). These dynamics themselves may be informative about the processes linking inequality and growth.

2. Empirical Framework

2.1. *Inequality and Growth at the Household-level*

Our core analysis is based on the household-level specification:

$$g_{i,vT} \equiv \ln y_{i,vT} - \ln y_{i,vt-1} = \alpha_0 + \alpha_1 \ln y_{i,vt-1} + \alpha_2 \overline{\ln y}_{(-i)vt-1} + \alpha_v \text{IQ}_{(-i)vt-1} + \gamma' X_{i,vt-1} + \beta' X_{vt-1} + u_{i,vT} \quad (1)$$

where $g_{i,vT}$ is the (average) growth rate of per capita income for household i in village v between the initial period, $t - 1$, and the terminal period, T . This is a structural model relating household growth to own-household initial resources, $\ln y_{i,vt-1}$, and the distribution of resources across other households in the village. We summarise two dimensions of the distribution by the mean log incomes of *other households* besides household i , $\overline{\ln y}_{(-i)vt-1}$, and the level of inequality (i.e. the Gini coefficient) among *other households*, $\text{IQ}_{(-i)vt-1}$ (denoting the village statistic excluding household i by the subscript $(-i)$). We also include controls for household and village-level covariates from the initial period, $X_{i,vt-1}$ and X_{vt-1} .

Equation (1) has several inherent advantages over previous specifications. Most importantly, compared to typical specifications based on aggregate data, we are better able to distinguish among the competing explanations for the growth–inequality relationship. The inclusion of household-level covariates, notably flexible controls for initial household income, i.e. polynomials of $\ln y_{i,vt-1}$, helps to minimise the influence of potentially omitted non-linearities of own-income, and reduces the possibility that inequality reflects the spurious effects of aggregation. Use of the ‘leave-one-out’, or jackknifed inequality index also better highlights the nature of the ‘treatment’ we wish to isolate: the purely external effect of inequality.

Imagine a Chinese ‘Robin Hood’ stealing from a rich family, and giving to a poor one, while leaving household i untouched.¹⁴ This reduces overall inequality, without changing average village income, or the income of household i . Why might this redistribution affect the growth trajectory of household i ? If imperfect credit markets are the only source of the inequality–growth relationship, then in the household-level specification $\text{IQ}_{(-i)vt-1}$ *should not be significant* because we are controlling for household own resources. Under the credit-market explanation, the distribution of household income among one’s neighbours has no independent effect on a household’s own-growth. Indeed, the credit-market explanation is only relevant for the aggregate (village-level) specification. As noted by Deaton (2003) in the context of inequality and health, and Ravallion (1998) for inequality and growth at the county-level in China, some of the key reasons why inequality might be correlated with average outcomes are

¹⁴ One candidate for ‘Chinese Robin Hood’ is Song Jiang (宋江) and the 108 bandits from Mount Liang (梁山) who feature in the Chinese literary classic *The Water Margin* (水浒传) (Shi and Luo, 1365c). *The Water Margin* is also known under the following alternative English titles as *All Men are Brothers* (Buck, 1933), *Outlaws of the Marsh* (Shapiro, 1981) and *The 108 Heroes*.

pure artifacts of aggregation: inequality is a proxy for heterogeneity of household resources, or returns to resources, that are captured at the aggregate-level. Estimation at the household-level therefore allows us to determine directly whether village inequality has an external effect on household growth. If it does, this provides strong evidence that factors besides imperfect credit markets are the source of the relationship. If we also control for own-household endowments of land, human capital and labour, we reduce the chance that imperfect factor markets drive the inequality-growth relationship. A significant effect of $IQ_{(-i)vt-1}$ will therefore point towards the political economy, or institution-based class of explanations.

Another benefit of the household-level specification is that it enables us to explore heterogeneity in the response of growth to income inequality. This exercise may inform us as to the mechanism by which inequality affects household income growth. For example, if inequality hurts the poor more than the rich, one interpretation is that own resources mitigate the adverse impact of inequality, consistent with a model of imperfect credit markets. Of course, political economy models can also generate the prediction that the rich are less harmed by institutional failure than the poor, especially if the institutional failure is self-serving. On the other hand, a general failure to provide growth-enhancing public goods, or the imposition of taxes that discourages a shift to non-agricultural pursuits, might affect all village residents similarly. If inequality affects everyone similarly, the public-good oriented explanations seem more plausible than the credit-market models.

2.2. *Econometric Issues: Endogeneity*

Before discussing problems of identification, to make equation (1) operational, we need to finalise details of functional form. Note first one seemingly minor departure in equation (1) from the usual aggregate-level specification: the village mean income is the average of log household per capita incomes, *not* the log of average household per capita incomes. This distinction becomes important in aggregation, and linking the household parameter α_v to the village-level coefficient on inequality. We also need to specify an index of income inequality, IQ_{vt-1} . Typically, researchers employ the Gini coefficient, and for comparability we also report results with the Gini. However, our main index of inequality is the ‘Mean Log Deviation’, which is defined as $MLD_{vt-1} = \ln \bar{y}_{vt-1} - \overline{\ln y}_{vt-1}$, the difference between the log of mean income, and the mean of log income. This is the same measure used by Ravallion (1998) in his exploration of the consequences of aggregation in estimating the growth-inequality relationship.¹⁵ We use this measure of inequality because it is highly convenient for aggregating from household to village-level results, and for addressing other statistical issues. We note, however, that the empirical results are not sensitive to the choice of inequality measure.

Turning to the assumptions required for causal inference about the impact of $IQ_{(-i)vt-1}$ on growth, there are at least three classes of econometric issues that need to

¹⁵ Our specification departs from Ravallion (1998) in two key ways. First, we use the ‘leave-one-out’ version of the inequality index, $MLD_{(-i)vt-1} = \ln \bar{y}_{(-i)vt-1} - \overline{\ln y}_{(-i)vt-1}$. Second, we use the mean of log incomes, not the log of mean incomes, as our control for the ‘level’ of village income (our measure of this is also jack-knifed).

1 be addressed. The first two are a consequence of less than perfect data, and the third of
 2 the non-randomisation of inequality.

3 4 2.2.1. *Measurement error*

5 There are several ways for measurement error, especially of household income, to
 6 generate spurious links between initial inequality and subsequent growth. For example,
 7 we might poorly control for own-initial income. Village summary statistics, like
 8t inequality, which are correlated with own-household income, may then pick-up some of
 9 the effect of own-income on growth. To some extent, this is mitigated by our use of the
 10 jackknifed $IQ_{(-i)vt-1}$ that excludes own-household income. Indeed, use of the jack-
 11 knifed $IQ_{(-i)vt-1}$ breaks potential mechanical and small-sample bias links between the
 12 village-level statistics and household i outcomes, and helps address some of the mea-
 13 surement error that arises from using group means as regressors. Indeed, in Technical
 14 Appendix S1 we show that equation (1) is part of the reduced form for Deaton's
 15 suggested estimator for correcting measurement error in grouped means' specifica-
 16 tions. But as Ammermueller and Pischke (2009) show in the context of peer effects, use
 17 of a leave-one-out mean does not eliminate all measurement error problems of this
 18 type: At the very least, some of the true effect of own-income may still be picked up by
 19 one's neighbours' incomes, and thus $IQ_{(-i)vt-1}$. The ideal solution would be to employ
 20 instruments for both own-income and $IQ_{(-i)vt-1}$, for example, measurement-
 21 error-independent (e.g. second measures) estimates of household income (and
 22 $IQ_{(-i)vt-1}$). In the absence of such instruments, we can mitigate the potential conse-
 23 quences of measurement error by including a rich set of household-level covariates,
 24 such as household endowments of land, labour and human capital, that should reduce
 25 the extent to which the error term contains mis-measured household-level 'growth
 26 potential' correlated with $IQ_{(-i)vt-1}$.

27 More conventionally, however, mis-measurement of household income may simply
 28 contaminate our estimate of initial inequality, $IQ_{(-i)vt-1}$. For example, 'outlier'
 29 households may generate a high-level of apparent inequality combined with high initial
 30 income. With mean-reversion of household incomes, such households in high-
 31 inequality villages will appear to have lower growth rates, even if there is no link
 32 between inequality and growth. Alternatively, noise in the measurement of $IQ_{(-i)vt-1}$
 33 may result in conventional attenuation bias, a weakening of the observed relationship
 34 between inequality and growth. A related problem derives from the fact that $IQ_{(-i)vt-1}$
 35 is estimated from a sample of village households, not the population. As noted earlier,
 36 in its simplest form this type of measurement error is addressed by our implementation
 37 of the Deaton (1985) estimator. This strategy has limitations, however, if household
 38 inclusion in the village sample is non-random (see Ammermueller and Pischke, 2009).
 39 The direction of the resulting bias is also indeterminate. The ideal solution, as always,
 40 would be to find instruments for $IQ_{(-i)vt-1}$ that are robust to this source of measure-
 41 ment error (along with the other sources). We do not have such instruments. We can,
 42 however, at least find an instrument that is robust to outliers as the source of mea-
 43 surement error: We use the 90–10 ratio for this purpose, jackknifed in the same way as
 44 other village-level variables, $RAT_{(-i)vt-1} = \left[y_{(-i)vt-1}^{90} \right] / \left[y_{(-i)vt-1}^{10} \right]$.

2.2.2. *Attrition*

Our best estimates of village-level initial conditions are based on the largest sample of households surveyed in 1987, which because of attrition is significantly larger than the balanced panel of households observed continuously between 1987 and 2002. The attrition rate is about 30% over the 15 years. While this hurts the sample size, our greater concern is that household attrition may be correlated with both initial inequality and subsequent growth. For example, there may be selective out-migration, with households more likely to leave slow-growing villages. The key question is whether such migration is correlated with initial inequality. If out-migration was more common in the high-inequality, low-growth potential villages, then depending on which households leave, we might observe a spurious link between initial inequality and the growth rates calculated on the basis of the initial sample collected in 1987, and the revised (attrition-affected) sample in 2002. In our empirical work, we explore the issue of attrition in detail, and ultimately present our main results adopting the ‘Inverse Probability Weighting’ procedure recommended by Wooldridge (2002).

2.2.3. *Omitted variables bias*

Our greatest concern is that even measured perfectly, $IQ_{(-i)vt-1}$ may be correlated with a village-level unobservable component of $u_{i,vT}$, which we denote as θ_{vt-1} . No matter how many covariates we include to control for initial conditions, there would always be the suspicion that inclusion of a better proxy for θ_{vt-1} could eliminate the apparent impact of inequality (e.g. see Kanbur (2005) for a summary). Perhaps, the initial income distribution is related to policies in place at $t - 1$ that affect future growth? What characteristic of θ_{vt-1} might be of particular concern? Suppose that θ_{vt-1} is a long-standing village taste, or predisposition, for low inequality. This is of concern only if θ_{vt-1} is also related to growth, for example, if egalitarian villages are more likely to invest in growth-enhancing public goods like schools, or to keep growth-distorting taxation to a minimum. From an interpretation standpoint, if θ_{vt-1} drives the inequality–growth relationship, then our conclusions will only be accurate from an ‘historical’ descriptive perspective: unequal villages in our sample grew more slowly. If it is the underlying taste for low inequality (θ_{vt-1}) that drives growth, then a Robin Hood ‘intervention’ would not affect the growth trajectory: controlling for θ_{vt-1} , the ‘treatment’ of lower inequality would have no impact on growth. Rising inequality, similarly, would be of no consequence to growth. So while we could confidently conclude that low inequality villages historically grew faster, if unobserved ‘egalitarianism’ was the driving force, we could not draw conclusions concerning the present-day increase in rural inequality. Ideally, we need to disentangle the impact of actual inequality from unobserved ‘tastes’ for low inequality.

One way to accomplish this is to find instruments that help predict initial inequality, but are independent of θ_{vt-1} . In Benjamin *et al.* (2006), we attempted such a strategy, using as instruments the asset distribution in period $t - 1$, especially the initial distribution of land. However, it is very difficult to plausibly claim that the land distribution is independent of θ_{vt-1} , as one expects that a taste for egalitarianism, for example, also spills over to the land distribution, which was under significant control of the village government, and thus likely a function of θ_{vt-1} . Indeed, it is difficult to imagine any set

of instruments succeeding.¹⁶ Moreover, even if we could find correlates of initial inequality that are independent of θ_{vt-1} , it is hard to rule out just about any variable as potentially contributing to household growth (i.e. being related to household own resources), and thus violating the exclusion restriction.

In the end, we do not attempt to find such instruments, but choose instead to live with the important qualification that initial income inequality may be correlated with other factors at the village-level (or possibly the township or county-level). Indeed, the interpretation of inequality as causally determining institutional development, and thus growth, is predicated on such a correlation. While there are assumptions under which inequality alone can be interpreted purely causally, at the very least, we can determine whether high inequality is a ‘marker’ or predictor of low-growth potential. Besides including as many covariates as possible, another way to address the unobserved heterogeneity problem would be to treat θ_{vt-1} as a fixed effect, using repeated observations of village inequality and growth to implement a village-panel-based estimation procedure. In Section 3.5, we discuss the merits of this exercise, but note at the outset that it is subject to important, and well-recognised limitations. In our application, we show that such a strategy is not informative.

2.3. A Note on Aggregation

For comparability with the previous literature, we also show results for the village analogue of (1). Setting aside the jackknife dimension:

$$g_{vT} = \overline{\ln y_{vT}} - \overline{\ln y_{vt-1}} = \beta_0 + \beta_1 \overline{\ln y_{vt-1}} + \beta_v (\ln \bar{y}_{vt-1} - \overline{\ln y_{vt-1}}) + \phi' X_{vt-1} + \varepsilon_{vT}. \quad (2)$$

In the Technical Appendix S1, we show in detail how this equation can be aggregated from the household-level (e.g. equation 1), and thus how the coefficients can be compared across household and village-level specifications. A few key points are worth highlighting. First, given our chosen functional form, it is easy to aggregate from the household-level regression by simple averaging, which is equivalent to using either village means, or village dummies, as instruments for the appropriate household specification. The estimated effect of inequality will be numerically identical, whether estimated at the household or village-level. Second, our specification allows us to better highlight the distinction between the village and household-level analysis: important differences in key parameter estimates emerge in the details of empirical implementation, especially the loss of household-level controls, and the use of an internally consistent sample that properly aggregates (i.e. only the non-attributed, balanced panel). Third, following the insights of Devereux (2007), we show that our jackknifed specification (1) can be derived as the reduced form growth equation for Deaton’s (1985) measurement-error-correcting estimator of the village-level specification (2). This provides ‘bonus’ justification for using

¹⁶ Several papers have tried to follow-up on the insights of Engermann and Sokoloff (1997) that the distribution of factor endowments, especially land, may drive subsequent institutional development, inequality and growth. See, for example, Easterly (2007) and Galor *et al.* (2009). Lundberg and Squire (2003) also use the distribution of land as an instrument for inequality in a growth–inequality specification. As noted by several authors, however (e.g. Chong and Gradstein 2007a), inequality (of income and productive assets) and institutions probably co-evolve, mutually affecting each other in various ways. It is almost impossible to imagine how one could be taken as exogenous relative to the other.

equation (1) as our base specification. We are thus able to also implement the Deaton (1985) estimator for the village-level equation (2).

3. Empirical Results

3.1. Data

The data come from annual household surveys conducted by the Survey Department of the Research Center on the Rural Economy (RCRE). The survey collected detailed household-level information on income and other household characteristics. The starting point is a sample from 1987 of 4,847 households drawn from 82 continuously observed villages in nine provinces.¹⁷ Originally planned as an annual longitudinal survey, by the end of our sample (2002) there was significant attrition of households, on the order of 30%.¹⁸ We are able to follow a ‘pure’ balanced panel of 3,424 households every year between 1987 and 2002, excluding 1992 and 1994 when there was no survey.

A variety of definitions are useful.¹⁹ First, household membership and the corresponding sampling frame are defined on the basis of residency and registration. Second, income is calculated as the sum of net income (gross revenue less current expenditures) from agriculture, farming sidelines (e.g. animal husbandry and live-stock), family-run businesses, plus wage income (earned inside and outside the village) and transfers. We calculate the value of farm output that is not sold, and thus largely consumed by the household, at market prices. We also use household income before taxes, and deflate all nominal values into 1986 prices using the NBS rural consumer price index for each province.

The key outcome in our analysis is ‘growth’ of household per capita income. To mitigate the effect of transitory shocks, or measurement error, on income, we construct two-year averages of household income for each two-year time period. Our initial period is 1987–8, and all household-level variables, and the village statistics calculated from them, are calculated using the average of 1987 and 1988 outcomes for each household. Subsequent two-year endpoints, ‘*T*’, include: 1990–1, three years after the initial period (1987–8); 1995–6, the next period for which we have adjacent time periods (eight years out); 1997–8; 1999–2000; and finally 2001–2, 14 years after the initial period.

In Table 1, we report the mean and quantiles of key variables for the panel households. Several points are noteworthy, starting with the income variables. First, over the entire period per capita income growth averaged 3.0% per annum, but there were significant differences in the rates of growth from 1987 to 1988 to our five ending dates.

¹⁷ The complete RCRE survey covers over 22,000 households in 300 villages in 31 provinces and administrative regions. RCRE’s complete national survey is 31% of the annual size of the NBS Rural Household Survey. By agreement, we have obtained access to data from nine provinces (Anhui, Gansu, Guangdong, Henan, Hunan, Jiangsu, Jilin, Shanxi and Sichuan), or roughly one-third of the RCRE survey.

¹⁸ The RCRE survey continued past 2002; however, there were significant changes in survey design from 2003 that introduced serious comparability problems for income. The post-2002 data cannot be pooled with previous surveys.

¹⁹ The RCRE fixed point survey team has followed the definitions and protocols established as standards by the NBS Rural Household Survey unit since its inception in 1986. Further details and comparisons of the RCRE data source with other data from rural China are found in the main text and extensive appendices of Benjamin *et al.* (2005)

Table 1
Sample Summary Statistics

| | | Percentiles | | |
|--|--------|-------------|--------|-------|
| | Mean | 10th | 50th | 90th |
| I. Household-level data (panel households; N = 3,424) | | | | |
| Per capita income (constant 1986 yuan) | | | | |
| 1987-8 | 526 | 229 | 448 | 896 |
| 1990-1 | 518 | 234 | 425 | 870 |
| 1995-6 | 709 | 316 | 593 | 1,192 |
| 1997-8 | 742 | 330 | 630 | 1,223 |
| 1999-2000 | 749 | 310 | 614 | 1,312 |
| 2001-2 | 826 | 334 | 678 | 1,443 |
| Annualised growth rates from 1987-8 to: | | | | |
| 1990-1 | -0.009 | -0.178 | -0.009 | 0.157 |
| 1995-6 | 0.036 | -0.045 | 0.039 | 0.116 |
| 1997-8 | 0.034 | -0.034 | 0.034 | 0.102 |
| 1999-2000 | 0.027 | -0.035 | 0.029 | 0.089 |
| 2001-2 | 0.030 | -0.026 | 0.031 | 0.085 |
| Composition of income (shares) | | | | |
| Agriculture, 1987-8 | 0.55 | 0.17 | 0.56 | 0.89 |
| Wages, 1987-8 | 0.22 | 0.00 | 0.13 | 0.61 |
| Business, 1987-8 | 0.14 | 0.00 | 0.04 | 0.45 |
| Agriculture, 2001-2 | 0.36 | 0.03 | 0.31 | 0.79 |
| Wages, 2001-2 | 0.34 | 0.00 | 0.31 | 0.79 |
| Business, 2001-2 | 0.16 | 0.00 | 0.00 | 0.59 |
| Other household characteristics in 1987-8 | | | | |
| Household education | 5.56 | 2.88 | 6.00 | 9.00 |
| Household size | 4.77 | 3.00 | 5.00 | 7.00 |
| Dependency ratio | 0.43 | 0.20 | 0.50 | 0.60 |
| Cultivated land (mu) | 1.46 | 0.50 | 1.17 | 2.81 |
| Head age \leq 30 | 0.09 | | | |
| Head age between 31 and 40 | 0.33 | | | |
| Head age between 41 and 50 | 0.35 | | | |
| Head age between 51 and 60 | 0.16 | | | |
| Head age 61 and over | 0.06 | | | |
| II. Village-level characteristics in 1987-8 (N = 82) | | | | |
| Inequality of per capita household income | | | | |
| Gini coefficient | 0.20 | 0.14 | 0.19 | 0.28 |
| Mean Log Deviation | 0.07 | 0.03 | 0.06 | 0.12 |
| 90-10 Ratio | 2.56 | 1.91 | 2.46 | 3.39 |
| Other characteristics | | | | |
| Household education | 5.53 | 4.00 | 5.66 | 7.13 |
| Share of income from agriculture | 0.54 | 0.25 | 0.59 | 0.74 |
| Share of income from wages | 0.20 | 0.07 | 0.16 | 0.37 |
| Share of income from family enterprises | 0.15 | 0.05 | 0.13 | 0.30 |
| Village total tax revenue per capita | 0.46 | 0.06 | 0.31 | 0.92 |
| Village total public expenditure per capita | 0.46 | 0.06 | 0.30 | 0.90 |
| Mountainous terrain | 0.26 | | | |
| Hilly terrain | 0.39 | | | |
| Near a city | 0.06 | | | |

Notes. Household-level statistics are calculated over the 3,424 balanced panel households, while the village-level statistics are calculated over all available households in the 82 villages in 1987-8 (4,847 households).

1 This largely reflects cyclical factors: Marked declines in economic activity accompanied
2 the post-Tiananmen economic retrenchment and the Asian Financial Crisis. Incomes
3 actually fell between 1987–8 and 1990–1, but recovered significantly by 1995–6 so that
4 over the period between 1987–8 and 1995–6, growth averaged 3.6%. Growth in the
5 period ending in 1999–2000 was also lower than that ending in 1997–8. Second, there
6 was clear heterogeneity in the success of households, but the dispersion in growth rates
7 narrowed steadily over time. At the top end, for example, the 90th percentile of growth
8 rates fell from 11.6% for the period ending in 1995–6 to ‘only’ 8.5% by 2001–2, while
9 fewer households experienced negative growth rates. Third, data in Table 1 also reveal
10 a pronounced shift in the structure of incomes. In 1987–8, agriculture accounted for
11 half of household income (55%), but by 2001–2, this had fallen by more than a third to
12 36%. Offsetting this decline was the growing importance of wage income earned locally
13 and from outside the village, which grew from only 22% of income to 34%, and a slight
14 increase in the role of income from family businesses.

15 We turn now to the household-level covariates summarised in Table 1. Of particular
16 interest is household education, in this case the years of education for working-age
17 household members. The average in 1987–8 was only 5.56 years of schooling, varying
18 from 2.88 to 9.00 years at the 10th and 90th percentiles. There was also variation across
19 households in factor endowments of labour and land, as summarised by the depen-
20 dency ratio and cultivated land. Finally, note that most household heads were between
21 31 and 50 years of age in 1987–8. This does not represent the complete age distribution
22 in 1987, but reflects the higher attrition of older households by 2002. Those too young
23 to head households in 1987 do not appear in our panel either.

24 In the bottom part of Table 1, we report the key village characteristics. Our preferred
25 estimates of these variables are based on all available households in the 1987–8 sample,
26 i.e. not just the balanced panel of households.²⁰ Our objective is to consistently capture
27 local conditions at that time, and statistics based only on the panel households will be
28 less reliable and sample sizes smaller.²¹ Our key regressor is village inequality, and we
29 report three measures in Table 1. First, the Gini coefficient for an average village was
30 0.20 in 1987–8. The mean, however, hides the variation of inequality across villages;
31 inequality is as low as 0.14 at the 10th percentile, and more than double at 0.28 in the
32 90th percentile. The Mean Log Deviation shows the same pattern, though is lower in
33 magnitude. The 90–10 ratio, which we use as a more robust indicator of inequality, is
34 2.56 on average. In the low inequality villages, the ‘rich’ (90th percentile) earn less than
35 double of the ‘poor’ (10th percentile), while in the high-inequality villages, the rich
36 earn more than triple the incomes of the poor. Additional village controls include
37 village averages of household education, the share of village income derived from the
38 main sources (agriculture, wages and family businesses) and two measures of local
39 public finance: Village tax revenue and public expenditures per capita. Our final
40 controls are for topography and geography, including a full set of province dummies.

41
42
43 ²⁰ In some specifications, however, when we wish to illustrate issues of aggregation, we will use village means
and variables that are based entirely on an ‘internally consistent’ set of balanced panel households only.

44 ²¹ Sample sizes in the full sample range from 15 to 137 households per village, with an average sample size
45 of 60 households per village, with most villages having between 30 and 90 households. In terms of underlying
46 population, villages range in size from approximately 700 people (10th percentile) to 3,000 people (90th
percentile), with an average population of 1,500.

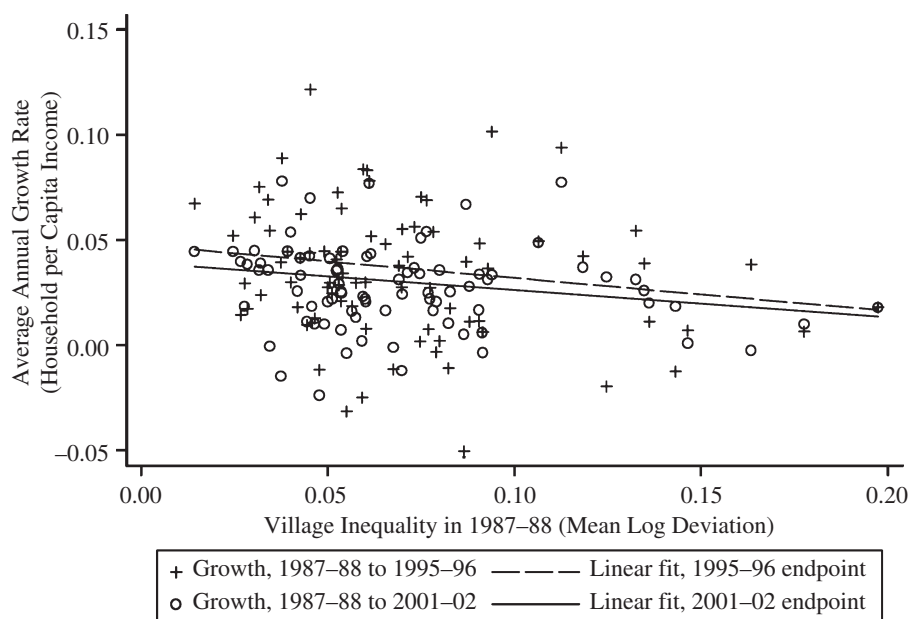


Fig. 1. *The Growth–Inequality Relationship: Different Endpoints*

Notes. The fitted values are based on simple regressions of growth from period 1987–8 through period T, on initial inequality in 1987–8. The regressions are weighted by the number of households in the village sample. The number of villages is 82. The regression coefficients (and t-values) are for 1995–6: -0.157 (1.87) and for 2001–2: -0.130 (2.30).

3.2. Regression Results: Main Household Findings

In Figure 1, we show a village-level scatter plot of the relationship between initial inequality and growth for two of the endpoints, 1995–6 and 2001–2. While there is more dispersion of growth rates in 1995–6, the pictures for the two years are similar: a negative estimated relationship between inequality and growth. Furthermore, the relationship does not appear to be driven by outliers: no single village or cluster of villages exerts undue influence on the slope of the regression line. The raw correlations therefore suggest that inequality is negatively related to growth.

In Table 2, we present estimates of our core household-level regression (equation 1) for each possible endpoint. We divide the analysis into two parts. In columns (1) through (6), we explore a few important issues of specification, culminating in our ‘bottom-line’ results in column (5). In the second part, columns (7) through (10), we demonstrate the link between our household-level results, and the conventional village-level results. Column (1) shows the impact of inequality on growth when we use the Gini coefficient as our measure of inequality. The estimated effect of inequality is significant and negative for all endpoints, declining from -0.257 in 1990–1, to -0.105 in 2001–2. The decline of the impact of inequality is consistent across all specifications, and a key result of the paper. In column (2), we move to the Mean Log Deviation as our inequality index. The pattern of coefficients (relative magnitude and statistical significance across endpoints) is identical to that for the Gini. To benchmark the magnitude of the effect, a coefficient of -0.20 means that moving from a ‘low’ to ‘high’

Table 2
Estimated Impact of Inequality on Household Income Growth, Various Specifications and Endpoints

| Endpoint | Core household-level results (varying specifications) | | | | | Exploring aggregation: from the household to village-level | | | | |
|---------------------------|---|--------------------|--------------------|--------------------|--------------------|--|--------------------|--------------------|--------------------|--------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| 1990-1 | -0.257* (0.122) | -0.346* (0.172) | -0.368* (0.171) | -0.607* (0.216) | -0.354* (0.173) | -0.029* (0.010) | -0.247 (0.202) | -0.072 (0.175) | -0.107 (0.216) | -0.175 (0.223) |
| 1995-6 | -0.197* (0.061) | -0.265* (0.088) | -0.248* (0.086) | -0.322* (0.096) | -0.240* (0.084) | -0.016* (0.005) | -0.189* (0.085) | -0.165* (0.078) | -0.207* (0.093) | -0.228* (0.104) |
| 1997-8 | -0.179* (0.058) | -0.242* (0.085) | -0.208* (0.087) | -0.320* (0.095) | -0.197* (0.085) | -0.016* (0.004) | -0.198* (0.091) | -0.167 (0.088) | -0.212* (0.102) | -0.240* (0.109) |
| 1999-2000 | -0.150* (0.051) | -0.203* (0.074) | -0.180* (0.072) | -0.256* (0.079) | -0.173* (0.071) | -0.012* (0.004) | -0.150* (0.072) | -0.089 (0.070) | -0.119 (0.085) | -0.138 (0.089) |
| 2001-02 | -0.105* (0.045) | -0.137* (0.067) | -0.117 (0.070) | -0.201* (0.082) | -0.111 (0.068) | -0.010* (0.004) | -0.133 (0.086) | -0.091 (0.068) | -0.115 (0.087) | -0.110 (0.098) |
| <i>Specification</i> | HH | HH | HH | HH | HH | HH | HH | HH | Vill | Vill |
| Unit of observation | Gini | MlnD | MlnD | MlnD | MlnD | 90-10 | MlnD | MlnD | MlnD | MlnD |
| Inequality index | Full | Full | Full | Full | Full | Full | Lean | Lean | Lean | Lean |
| Covariates (lean or full) | No | No | Yes | Yes | Yes | Yes | No | No | No | No |
| Attrition weights? | Everyone | Everyone | Everyone | Everyone | Everyone | Everyone | No | No | No | No |
| Sample for inequality | Everyone | Everyone | Everyone | Everyone | Everyone | Everyone | Everyone | Pure | Pure | Pure |
| OLS or IV? | OLS | OLS | OLS | IV-1 | OLS | OLS | OLS | panel | panel | panel |
| Jackknifed inequality | No | No | No | No | Yes | Yes | No | IV-2 | OLS | OLS |
| | | | | | | | Yes | Yes | No | No |

Notes. Each reported coefficient is the coefficient on the inequality variable (the Gini, the Mean Log Deviation (MlnD), or the 90-10 ratio) from the specified regression with a given endpoint. Robust, cluster-corrected (at the village-level) standard errors in parentheses, where * indicates statistically significant at the 5% level. The unit of observation is usually the household ($N = 3,424$), except for specifications (9) and (10) where it is the village ($N = 82$). Covariates are either the 'Full' set of household and village variables from 1987 to 1988 (cubic in own-initial income and village mean log income, household age/cohort indicators, education at the household and village-level, household size and dependency ratio, village dependency ratio, household land endowment, village land endowment, village shares of income in agriculture, wages, and family business, village public finance variables, village geographic controls and province dummies), or the 'Lean' set which includes no household-level controls, and only village controls for initial income, education, geography, and province dummies. Attrition weights are included where noted. Village statistics (like inequality) are calculated either over the entire sample available in 1987-8 (Everyone), or only the households observed throughout the sample (balanced panel). Some specifications are estimated by instrumental variables (IV): IV-1 uses the 90-10 ratio as an instrument for the MlnD; while IV-2 uses Jackknifed village means as instruments for household-level variables. The main specifications (where indicated) use Jackknifed village means for inequality, log mean village income and village education. Specifications (9) and (10) are both estimated at the village-level. Specification (9) uses 'mean of log village income' as elsewhere, while Specification (10) is the 'macro' specification (functional form), with 'log of mean village income' used to measure both growth (the dependent variable), and initial village log income.

1 inequality village (e.g. an increase in the Mean Log Deviation of 0.10) is associated with
2 a 0.02 decrease in average annual growth rates. This compares to average annual
3 growth rates of 0.035. In column (3), we employ the weights for attrition in order
4 to evaluate the impact of this correction.²² The results change modestly: the esti-
5 mated effect of inequality is generally a bit smaller, to the extent that by 2001–2, the
6 estimated effect is no longer significant at the 5% level.²³ Given that the correction for
7 attrition does matter – even if only a bit – we retain this throughout the remaining
8t specifications.

9 In column (4), we address the possibility of outlier-driven measurement error by
10 using the more robust 90–10 ratio as an instrument for the Mean Log Deviation. The
11 first stage results are highly significant: the t-value on the 90–10 ratio is 19.51, so there is
12 no ‘weak instrument’ problem. The strength of the instrument is also illustrated in
13 Figure 2, where we plot the Gini and the Mean Log Deviation against the 90–10 ratio.
14 This figure also illustrates a more important point: the three inequality measures are
15 highly correlated. The second-stage results in column (4) show notably higher esti-
16 mated effects of inequality on growth, with a similar temporal pattern as before: steadily
17 declining influence of initial inequality, though statistically significant through 2001–2.
18 These results suggest that the Mean Log Deviation and the Gini may be noisy measures
19 of income dispersion in the village, and that the more robust 90–10 ratio should be
20 preferred (on its own, not merely as an instrument). There is otherwise no reason to
21 prefer one inequality measure over another, and so for our main results, we show both
22 the Mean Log Deviation and the 90–10 ratio.

23 Column (5) shows results comparable to those in column (3), but using the
24 jackknifed Mean Log Deviation as the inequality index, i.e. estimates of equa-
25 tion (1).²⁴ Comparing the coefficients in column (5) to those in column (3), we see
26 that the estimated effect of inequality is slightly smaller. The inclusion of household
27 ‘ \bar{z} ’ in the village inequality, at least in this instance, does not seem to lead to signif-
28 icant bias in the coefficient. We still retain this procedure, however, as *ex ante* there
29 are good reasons to employ the jackknifed statistics. Our estimated effect of
30 inequality is statistically significant in all time periods except the most recent, and as
31 in previous columns, the effect declines monotonically over time. In column (6), we
32 show estimates comparable to column (5), but with the jackknifed 90–10 ratio as our
33 inequality measure. The results are similar to those using the Mean Log Deviation,
34
35

36 ²² Table S1 in the Technical Appendix shows the effects from the Probit Model used to calculate inverse
37 probability weights based on observable characteristics as suggested in Wooldridge (2002). Note in particular
38 that the probability of attrition is not correlated with the initial level of village inequality, which would
39 otherwise be of potential concern.

40 ²³ We formally test whether the inequality coefficients are significantly different from each other across
41 years. Consider the results from Specification (3). We reject the equality of the five coefficients, with a p-value
42 of 0.02. Comparing the 1995–6 and 2001–2 coefficients alone, we marginally reject their equality with a
43 p-value of 0.048.

44 ²⁴ In Table S2, we report the coefficients on the other covariates for the specification in column (5). At the
45 household-level, we find the most important predictors of household income growth to be age, with younger
46 households doing better, and to some extent, education (though only through 1995–6). For 1997–8 and
1999–2000, we find that village mean log income is positively related to growth consistent with Ravallion
(1998), but otherwise its effect also fades over time. Very few other village-level variables matter in terms of
statistical significance. In that sense, village-level inequality is remarkable as being the most significant village
characteristic that matters throughout this time period.

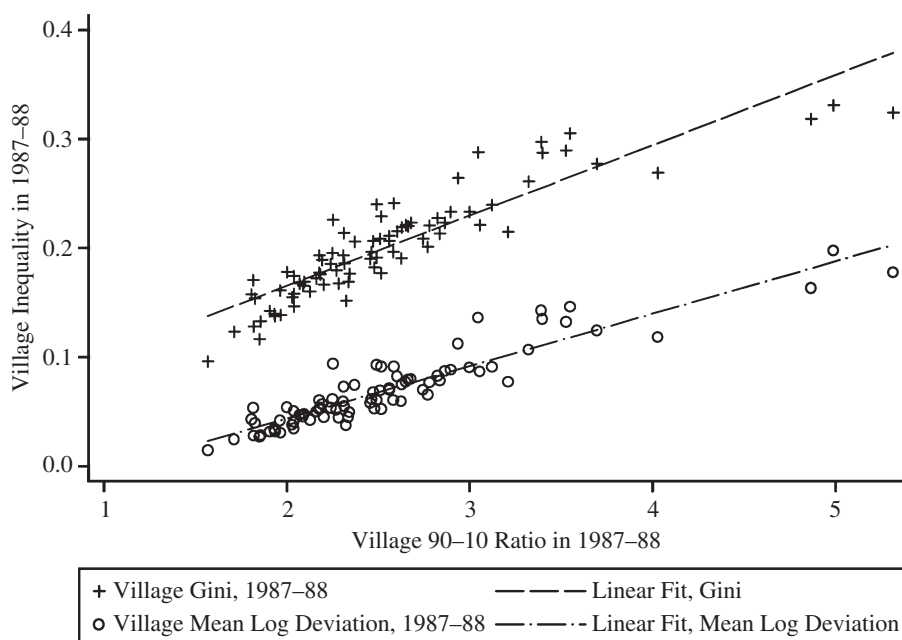


Fig. 2. *The Relationship Between Inequality and the 90-10 Ratio, 1987-8*

Notes. Each scatter plot is a graph of village inequality (for the Mean Log Deviation, or the Gini Coefficient) versus the village 90-10 ratio in 1987-8. The relationship represented in these plots underlies the 'reduced form' or 'first stage' for the correction of potential outlier-based measurement error of inequality, using the 90-10 ratio as an instrument. For the Mean Log Deviation, the t -value of the coefficient on the 90-10 ratio is 24.68 ($F = 609.26$), while the similar t -value for the Gini coefficient regression on the 90-10 ratio is 20.38 ($F = 415.43$). As an aside, the t -value of a regression of the Mean Log Deviation on the Gini Coefficient is 46.70 ($F = 2180.66$).

though with sharper statistical significance, including the most recent time period, 2001-2.

Controlling for a rich array of household and village regressors, the effect of inequality is difficult to dismiss: it is robust to controls for sample attrition, small-**1** sample bias, and accounting for some types of measurement error. While it fades over time, local inequality appears to exert a purely external effect on household growth over and above any household-level proxies for growth potential.

In the remaining columns, we show how the household-level results can be directly compared to the village-level, following the discussion in Section 3.3 and in Technical Appendix S1. Column (9) is the final destination of the aggregation exercise, estimated at the (appropriately weighted) village-level: the effects are generally weaker than at the household-level, with significant coefficients only for the 1995-6 and 1997-8 endpoints. There are a few key steps, however, moving between the household-level specification in column (2) to the village-level in column (9). As it turns out, using village-level instead of household-level measures of growth and inequality has nothing to do with the change in results. The first big step is from column (2) to column (7), which is estimated at the household-level, but where we drop many of the covariates, notably the household-level controls. Compared to column (2), the estimated coeffi-

1 coefficients in column (7) are smaller and less significant.²⁵ The next step in aggregation is
 2 switching to village-level statistics calculated using only the balanced panel households.
 3 Using either village dummies, or village means, as instruments for the household-level
 4 variables in a household-level specification would then yield identical coefficients to
 5 those in the village-level specification (column 9). In the process of aggregation, it is
 6 the combination of these two changes in specification – excluding the household-level
 7 covariates, and using only the balanced panel sample to calculate initial village condi-
 8t tions – that drives the drop in the estimated effect of inequality from the household
 9 to the village-level regressions: It has nothing to do with aggregation from one level to
 10 the other *per se*.

11 We next step back to column (8), which while estimated at the household-level,
 12 yields the Deaton (and Devereux) corrections for small-sample bias (for the village-level
 13 specification). This entails only a small variation on the specification in underlying
 14 column (9): we use the jackknifed village means instead of the straight village means
 15 implicitly used as instruments for the household variables in equation (A.4) and col-
 16 umn (9). For the specification in column (8), the estimated effects of inequality are
 17 slightly smaller, and significant only for the ‘peak’ year of 1995–6. Because it accounts
 18 for the small-sample measurement-error bias, this is the preferred ‘bottom-line’ village-
 19 level result: inequality has a negative but generally insignificant effect that fades quickly
 20 after 1995–6. However, our overall preferred specification is the household-level col-
 21 umn (5), which uses the same jackknifed inequality measure, but with the inclusion of
 22 the full suite of household covariates, and with village statistics calculated using all
 23 available observations from 1987 to 1988. As explained in the Technical Appendix S1,
 24 column (5) is a richer ‘reduced form’ for the Deaton and Devereux specification
 25 presented in column (9).

26 As a final exercise, and to facilitate comparisons with other studies, in column (10)
 27 we report the village-level results based on a ‘macro’ construction of key income vari-
 28 ables. The specification is identical to column (9), except that we use ‘log means’
 29 instead of ‘mean logs’. This affects the construction of the dependent variable, which
 30 becomes $g_{vT} = \ln \bar{y}_{vT} - \ln \bar{y}_{vT-1}$, and the control for initial village income, which
 31 becomes $\ln \bar{y}_{vT-1}$. Everything else is the same. The results in column (10) are very
 32 similar to those in column (9). Clearly, attention to details of aggregation yields a more
 33 elegant integration of household and village-level results. However, aggregation itself
 34 has very little impact on our conclusions. Using household-level data, in particular
 35 household-level covariates, however, *does* matter.

37 3.3. Regression Results: Heterogeneous Responses

38
 39 Who is hurt most by high inequality? In Table 3, we present estimates of two variants of
 40 equation (1), adding interaction effects between inequality and key dimensions of
 41 household covariates: education and cohort in one variant, and own-income in the
 42 other. For each time period, in column (1), we estimate interaction effects for
 43

44 ²⁵ Indeed, the differential inclusion of the household-level covariates may also be part of the reason that
 45 Ravallion (1998) finds more significant results at the household than county-level: controlling for household
 46 characteristics affects the precision of estimation, and also addresses possible omitted variables (non-linear-
 ities of the effect of own-income, etc.).

Table 3
Exploring Interaction Effects: the Effect of Inequality on Growth by Education, Cohort and Initial Income

| | 1995–6 | | 1997–8 | | 1999–2000 | | 2001–2 | |
|------------------------------------|----------|----------|----------|----------|-----------|----------|----------|----------|
| | (1) | (2) | (1) | (2) | (1) | (2) | (1) | (2) |
| <i>Base effects (1987–8)</i> | | | | | | | | |
| Inequality | -0.310* | -0.214* | -0.344* | -0.188 | -0.268* | -0.131 | -0.142 | -0.074 |
| (Jackknifed MInD) | (0.092) | (0.107) | (0.092) | (0.104) | (0.078) | (0.086) | (0.082) | (0.078) |
| HH education | 0.000 | 0.002 | -0.002 | 0.000 | -0.001 | 0.001 | 0.000 | 0.000 |
| | (0.001) | (0.000) | (0.001) | (0.000) | (0.001) | (0.000) | (0.001) | (0.000) |
| Age ≤ 30 | -0.006 | -0.011* | -0.001 | -0.008* | 0.004 | -0.006 | -0.001 | -0.005 |
| | (0.006) | (0.004) | (0.005) | (0.003) | (0.004) | (0.003) | (0.004) | (0.003) |
| 31 ≤ Age ≤ 40 | -0.006 | -0.011* | 0.000 | -0.007* | 0.005 | -0.005* | 0.000 | -0.004* |
| | (0.005) | (0.003) | (0.004) | (0.002) | (0.003) | (0.002) | (0.004) | (0.002) |
| 51 ≤ Age ≤ 60 | -0.006 | -0.006 | -0.009* | -0.009* | -0.007* | -0.007* | -0.010* | -0.010* |
| | (0.005) | (0.003) | (0.004) | (0.003) | (0.003) | (0.002) | (0.003) | (0.002) |
| Age > 60 | -0.027* | -0.027* | -0.018* | -0.018* | -0.017* | -0.017* | -0.019* | -0.019* |
| | (0.005) | (0.005) | (0.004) | (0.004) | (0.003) | (0.003) | (0.003) | (0.003) |
| Income quartile (Q1) | | 0.002 | | 0.000 | | 0.005 | | 0.005 |
| | | (0.008) | | (0.006) | | (0.005) | | (0.006) |
| Income quartile (Q2) | | 0.001 | | 0.003 | | 0.009 | | 0.009 |
| | | (0.007) | | (0.005) | | (0.005) | | (0.006) |
| Income quartile (Q3) | | 0.004 | | -0.001 | | 0.002 | | 0.004 |
| | | (0.006) | | (0.005) | | (0.005) | | (0.005) |
| <i>Interactions</i> | | | | | | | | |
| Inequality × Education | 0.016 | | 0.031* | | 0.024* | | 0.008 | |
| | (0.011) | | (0.010) | | (0.009) | | (0.009) | |
| Inequality × Under 40 in 1987–8 | -0.072 | | -0.108 | | -0.134* | | -0.052 | |
| | (0.067) | | (0.056) | | (0.039) | | (0.055) | |
| Inequality × Q1 | | -0.067 | | -0.054 | | -0.075 | | -0.079 |
| | | (0.099) | | (0.084) | | (0.070) | | (0.069) |
| Inequality × Q2 | | -0.015 | | -0.034 | | -0.103 | | -0.084 |
| | | (0.099) | | (0.073) | | (0.069) | | (0.087) |
| Inequality × Q3 | | -0.032 | | 0.028 | | -0.003 | | -0.006 |
| | | (0.085) | | (0.069) | | (0.067) | | (0.067) |
| <i>Combined effect by quartile</i> | | | | | | | | |
| Combined Q1 | | -0.281* | | -0.249* | | -0.206* | | -0.154* |
| | | (0.091) | | (0.094) | | (0.078) | | (0.074) |
| Combined Q2 | | -0.230* | | -0.221* | | -0.234* | | -0.158* |
| | | (0.102) | | (0.090) | | (0.077) | | (0.080) |
| Combined Q3 | | -0.246* | | -0.160 | | -0.134 | | -0.081 |
| | | (0.088) | | (0.086) | | (0.075) | | (0.077) |
| F-interactions | 1.23 | 0.33 | 6.48* | 0.77 | 8.21* | 2.06 | 1.51 | 1.59 |
| | (0.2980) | (0.8005) | (0.0024) | (0.5162) | (0.0006) | (0.1121) | (0.2264) | (0.1982) |

Notes. Each specification is based on household-level specification with full household and village covariates and Jackknifed Inequality (column 5 of Table 2). Robust, cluster-corrected (at the village-level) standard errors in parentheses, with * indicating statistical significance at the 5% level. For each endpoint, we estimate the household specification with separate sets of interactions for education and cohort, and income quartile. The cohort interaction is based on an indicator of whether the household head was 40 years or younger in 1987–8. 'F-interactions' is the F-statistic for the null hypothesis of whether the interaction effects are jointly zero. 'Combined Q1' (etc.) are the total effects of inequality on income growth for households in a specified 1987–8 income quartile.

1 education and cohort. For the cohort interaction, while we maintain the more flexible
2 base effects for household age, we define the interaction in terms of 'young' and 'old',
3 with age 40 as the cut-off. In column (2), we report interactions between inequality and a
4 household's position in the income distribution. We assign households to their quartile
5 within their village, where the quartiles (like the inequality index) are calculated by a
6 jackknife estimator (i.e. quartiles are calculated excluding household i). The base
7 regression specification for the exploration of interactions is column (5) from Table 2.

8t Turning first to the education and cohort interactions, they are jointly and individ-
9 ually insignificant for 1995–6, and 2001–2, when the overall effect of inequality is also
10 insignificant. However, in the middle two time periods of our 1990's sample (1997–8
11 and 1999–2000), we see that education significantly offsets the adverse effect of
12 inequality: the impact of inequality hurts less educated households most. The timing
13 coincides with the Asian financial crisis, and suggests that inequality hurt the least
14 educated most during those periods, possibly due to reduced migration opportunities.
15 The cohort interaction is also significant for 1999–2000, and borderline in 1997–8. This
16 interaction coefficient suggests that the effect of inequality was worse for the young,
17 which seems somewhat surprising. However, it is the young that normally have the
18 highest income growth potential and are the most mobile, and the adverse impact of
19 inequality seems to have hurt these households most. In other words, income growth is
20 potentially higher for younger households, but those in more unequal villages expe-
21 rience stunted growth.

22 In column (2) for each time period, we report the interaction effects for income.
23 The most striking finding is that the interaction effects for income quartile are indi-
24 vidualy and jointly insignificant for all time periods. The adverse effects of inequality
25 hurt rich and poor equally. In terms of implications about the mechanism, we view this
26 as speaking most clearly to credit-market explanations: the effect of inequality exists
27 even when controlling for a rich set of household characteristics, and furthermore,
28 affects rich as much as poor households within a village. This is not what we would
29 expect to observe if imperfect credit markets drove the relationship. Instead, the effect
30 of inequality spills over households across the income distribution, though with less
31 impact on households with higher education. The only nuance of the income inter-
32 actions worth noting is that in 2001–2, where we estimated an insignificant impact
33 overall, we find significant negative effects of inequality on the bottom two quartiles.
34 While not significantly different than the effect on their richer neighbours, to the
35 extent that there is an impact of inequality in the most recent time period, it does seem
36 to fall on the poorest households. The effect of inequality does not completely go away
37 for those quartiles.

39 3.4. *Regression Results: Composition of Income*

41 We may be able to learn more about the nature of the effect of inequality by investi-
42 gating links between inequality and other measures of economic development. One
43 potentially informative dimension is the composition of household income, which will
44 reflect changes in the economic structure of villages, and households' ability to par-
45 ticipate in more lucrative economic opportunities, especially in non-agricultural
46 employment. To conduct this exercise, we make a slight modification to the base

specification (equation 1), replacing the dependent variable by the share of income earned by a household in a given activity, A : $S_{Ai,vT}$. The activities are ‘Agriculture’, ‘Wage Income’, and ‘Family Businesses’.²⁶ The omitted category is ‘Other’ income. We also add as controls the household’s share of income from each activity in the initial time period. In this way, we are estimating the effect of inequality on the *change* in share of income in each activity, controlling for initial household participation in the portfolio of activities. Did high inequality distort household evolution or movement into various income generating activities? The regression specification is therefore:

$$S_{Ai,vT} = \delta_0 + \beta' X_{vt-1} + \gamma' X_{i,vt-1} + \sum_{A=1}^3 \phi_A S_{Ai,vt-1} + \delta_1 \ln y_{i,vt-1} + \delta_2 \overline{\ln y}_{(-i)vt-1} + \delta_v IQ_{(-i)vt-1} + u_{Ai,vT}. \quad (3)$$

Again, our base specification is the same as column (5) of Table 2. Core results are shown in columns (1) to (3) for the most recent time period, 2001–2 to keep the dimension of discussion manageable (results are similar across time periods). We also introduce interactions between initial inequality and education and cohort following the discussion of Table 3, presented in columns (4) through (6).

We do not find any effect of inequality on the composition of income. The signs are consistent with inequality reducing participation in wage-earning activities, and increasing the share of income in agriculture, but the estimates are imprecise. The interaction results reported in columns (4) to (6) are more interesting. There we see that inequality significantly affects the ability of lower education households to move into wage-earning activities inside or outside the village, tilting them instead towards the less lucrative agricultural sector. This does not ‘explain’ the growth results, but it does help with some of the accounting: the adverse impact of inequality appears to operate by limiting access of households to higher income off-farm employment opportunities. Stated differently, in more equal villages, households are better able to participate in off-farm employment. In the unequal villages, those households with low education are ‘trapped’ in farming, and experienced lower income growth, especially through the 1990s when crop prices were low.²⁷

3.5. *The Potential Role of Village Heterogeneity*

We find robust evidence that initial village inequality has a long-lasting, but fading, effect on household growth rates, but as we noted in Section 3.2, this is insufficient evidence to establish a causal link between inequality and growth. While we can rule out a large set of mechanical and other endogeneity explanations, our most serious reservation is that inequality reflects other initial conditions that are correlated with

²⁶ Using a share-based specification allows us to estimate this equation at the household-level, even when households have ‘zero’ income from a particular source. The overall response will capture the combined movement from zero to positive (i.e. participation), as well as the level of income from a given activity.

²⁷ This is also consistent with the prediction of Chong and Gradstein (2007b) that households in high-inequality settings may operate disproportionately in the ‘informal sector’ to avoid predation by the higher income households through local institutions. We doubt that this specific mechanism operated in Chinese villages, and that the education interactions instead point to better educational opportunities, or higher returns to education, in lower inequality villages.

1 both inequality and growth, which we denoted θ_{it-1} . The conventional approach for
2 dealing with that sort of unobserved heterogeneity is to control for village fixed effects
3 through a first-differenced implementation of the growth equation.²⁸ If we can track
4 repeated episodes of growth, i.e. first-difference growth rates, then this exercise is
5 potentially informative. As is well known, however, there are significant limitations to
6 examining growth rates over short periods in first-differenced models because the
7 co-variation of high-frequency measures of growth and inequality after controlling for
8t village-level unobservables may not be sufficient to identify the relationship between
9 inequality and growth. Inequality is a slowly evolving variable, and even with national-
10 level samples, period-to-period changes may be driven largely by measurement error. In
11 the context of rural China over this time period, measurement error is an obvious
12 concern. We are also concerned about simultaneity bias, as growth itself may change
13 the income distribution (a ‘Kuznets Process’).²⁹ Moreover, some of the core implicit
14 assumptions about the error term necessary for the panel-based approach to work may
15 be violated in our data.

16 In earlier versions of our paper, we implemented the panel-based strategy. In doing
17 so, we discovered at least two important issues that underscore the difficulty of using
18 repeated observations on the villages to control for initial village heterogeneity. A
19 discussion of these issues is worthwhile, as it helps to reveal important aspects of the
20 inequality–growth relationship. We first constructed a village-level panel data set using
21 the balanced panel of households to construct the growth rate and mean log income
22 variables for each period (symmetric with the household regressions), and the larger
23 non-attributed samples to construct key village covariates like village inequality. There are
24 seven available two-year periods: 1987–8, 1989–90, 1991–3, 1995–6, 1997–8, 1999–2000
25 and 2001–2.³⁰

26 Estimation of a ‘fixed effect’ or related specification that is informative about the
27 village-level equation (2) is predicated on the different sources of variation in
28 inequality (e.g. within villages, across years) affecting household income growth
29 through a constant set of parameters. Stated differently, we need to assume that the
30 panel-structure yields different experimental variation of inequality that informs us
31 about the impact of a well-defined ‘treatment’ on household growth. Instead of naively
32 pooling the data into a common regression, we break apart the village-panel into a
33 series of pair-wise cross-sections, and estimate the village-level cross-section specifica-
34 tion with varying beginning and endpoints.³¹ The results are quite striking and
35 reported in Table 5. We find a significant negative relationship between growth and
36 inequality in 1987–8 and every endpoint, except 2001–2 (as in Table 2), though even
37 for 2001–2 the result is significant for the 90–10 ratio. However, when using pairs of
38

39
40 ²⁸ See, for example, Forbes (2000) and Banerjee and Duflo (2003) for a discussion of the merits and pitfalls
associated with the panel data (fixed effects) approach in cross-country data.

41 ²⁹ See Kuznets (1955). Ravallion and Chen (2007) explore precisely this question as they investigate
42 linkages between growth rates and various measures of poverty and inequality using nationally representative
43 NBS data. Panel analyses using state-level data from the US also finds evidence of a Kuznets process leading to
a positive association between inequality and growth (Frank, 2009).

44 ³⁰ Note that there was no survey in 1992 or 1994, so the period 1991–3 is constructed slightly differently
45 from all of the other periods with adjacent years (1991 and 1993 being non-adjacent).

46 ³¹ In all specifications, we include the core set of village covariates: controls for initial mean log income,
education, land, dependency ratio, composition of village income, geography and province dummies.

Table 4
The Effect of Inequality on the Composition of Income

12

| | | Endpoint: 2001–2 | | | | | |
|------------------------------|------------------------|--------------------|--------------------|-------------------|--------------------|--------------------|--------------------|
| | | (1) | (2) | (3) | (4) | (5) | (6) |
| | | Agric | Wages | Business | Agric | Wages | Business |
| <i>Base effects (1987–8)</i> | | | | | | | |
| | Inequality (MlnD) | 0.233 (0.442) | -0.396 (0.623) | -0.030 (0.441) | 0.955 (0.492) | -1.657* (0.560) | 0.659 (0.559) |
| 13 | HH education 8788 | -0.004* (0.002) | -0.004 (0.002) | 0.005* (0.002) | 0.003 (0.004) | -0.016* (0.005) | 0.011* (0.004) |
| | Age ≤ 30 | -0.015 (0.017) | 0.000 (0.018) | 0.019 (0.016) | 0.008 (0.020) | -0.043 (0.028) | 0.039 (0.021) |
| | 31 ≤ Age ≤ 40 | 0.006 (0.010) | 0.001 (0.015) | 0.007 (0.013) | 0.028 (0.017) | -0.041 (0.023) | 0.027 (0.023) |
| | 51 ≤ Age ≤ 60 | 0.009 (0.012) | -0.035* (0.013) | -0.016 (0.014) | 0.009 (0.013) | -0.034* (0.013) | -0.016 (0.014) |
| | Age > 60 | 0.008 (0.020) | -0.095* (0.027) | -0.029 (0.018) | 0.006 (0.020) | -0.091* (0.027) | -0.031 (0.018) |
| <i>Interactions</i> | | | | | | | |
| | Inequality × Education | | | | -0.100* (0.047) | 0.171* (0.060) | -0.096* (0.049) |
| | Inequality × Under 40 | | | | -0.317 (0.193) | 0.595 (0.314) | -0.281 (0.263) |
| <i>F-Interactions</i> | | | | | 3.91* (0.0240) | 7.81* (0.0008) | 2.60 (0.0803) |

Notes. These specifications are based on the one in Table 2, column (5): full household and village covariates, Jackknifed inequality (MlnD). Those specifications with interactions are the same as those in Table 3. The key difference in covariates is that we also include *household-level* controls for the initial share of income in each of agriculture, wages and family business. The dependent variables are the share of income in the endpoint period from a given source of income (agriculture, wages, or family business). Robust, cluster-corrected (at the village-level) standard errors in parentheses, with * indicating statistical significance at the 5% level. The 'F-Interactions' is a test of the joint significance of the interaction effects.

endpoints with the starting period *after* 1987–8, we do not find a relationship between inequality and subsequent growth. Specifically, by 1989–90, village inequality loses its predictive power for subsequent growth. This means that to the extent that village inequality matters, it was inequality, or factors correlated with inequality, at the outset of reforms in the mid-1980s that affected subsequent growth. While robust to a battery of specifications, the 1987–8 results do not hold for subsequent sets of initial conditions. Because we do not have data prior to 1987–8, we cannot evaluate whether there is something specific to this period alone, or whether it represents the end of the early or pre-reform period. What we can conclude, however, is that whatever predictive content is contained in 1987–8 village inequality, it is gone for subsequent periods.

To some extent, this should not be surprising: there is no reason to believe that the determinants of inequality in 1987–8 were the same as in subsequent periods. The sources of income variation across households evolved with economic and market development, as well as being driven by economic shocks. Indeed, we can see this in the evolution of village-level inequality over the time period. In Figure 3, we illustrate this by plotting the final-period level of village inequality as a function of initial inequality, with the 45-degree line (no change in inequality) as reference. A few points are worth

Table 5
How Does Initial Inequality Relate to Various Subsequent Growth Rates? Varying the Beginning and End Periods, Village-level 'Cross-section' Specification

| | Endpoint period | | | | | |
|--|--------------------|-------------------|--------------------|--------------------|--------------------|--------------------|
| | (2) | (3) | (4) | (5) | (6) | (7) |
| | 1989–90 | 1991–3 | 1995–6 | 1997–8 | 1999–2000 | 2001–02 |
| <i>Measure of inequality: Mean Log Deviation</i> | | | | | | |
| Beginning period | | | | | | |
| (1) 1987–8 | –0.642* (0.254) | –0.176 (0.136) | –0.225* (0.088) | –0.213* (0.092) | –0.174* (0.080) | –0.116 (0.082) |
| (2) 1989–90 | | –0.147 (0.215) | –0.086 (0.131) | –0.111 (0.094) | 0.020 (0.077) | 0.027 (0.064) |
| (3) 1991–3 | | | 0.166 (0.286) | 0.119 (0.198) | 0.119 (0.120) | 0.139 (0.093) |
| (4) 1995–6 | | | | 0.188 (0.238) | –0.002 (0.192) | 0.102 (0.128) |
| (5) 1997–8 | | | | | –0.088 (0.323) | 0.079 (0.188) |
| (6) 1999–2000 | | | | | | 0.294 (0.180) |
| <i>Measure of inequality: 90–10 ratio</i> | | | | | | |
| Beginning period | | | | | | |
| (1) 1987–8 | –0.043* (0.014) | –0.013 (0.007) | –0.013* (0.004) | –0.014* (0.004) | –0.011* (0.004) | –0.008* (0.004) |
| (2) 1989–90 | | –0.004 (0.008) | 0.000 (0.006) | –0.005 (0.004) | 0.001 (0.004) | 0.000 (0.003) |
| (3) 1991–3 | | | 0.005 (0.012) | 0.002 (0.008) | 0.005 (0.006) | 0.006 (0.005) |
| (4) 1995–6 | | | | 0.009 (0.011) | 0.000 (0.008) | 0.006 (0.006) |
| (5) 1997–8 | | | | | –0.009 (0.018) | 0.001 (0.010) |
| (6) 1999–2000 | | | | | | 0.008 (0.008) |

Notes. All specifications use the village-panel data set, constructed by averaging household incomes across adjacent years (as indicated), then aggregated into village-level means. Average growth rates between the beginning period ($t - 1$) and the endpoint (t) are calculated using the balanced panel households only. The reported numbers are the coefficients (and robust standard errors) of the effect of inequality (MnD or 90–10 ratio) on growth from a regression of growth between period ' $t - 1$ ' and ' t ' as a function of village characteristics in period ' $t - 1$ '. Village-level regressors are calculated for each time period using the largest possible sample, conditional on households being in the 1987–8 sample. Village-level covariates include: mean log per capita income, average education, village land endowment, village dependency ratio, village shares of income in agriculture, wages, and family business, village geographic controls and province dummies. *Statistically significant at the 5% level.

noting. First, village inequality generally, though not universally, increased, as most villages have inequality above the 45-degree line. Second, there is general convergence of the levels of inequality across villages: increases of inequality were systematically highest for those villages with the lowest levels of inequality. The 'experiment' that generated differences of inequality across villages in 1987–8 is not likely the same as that which generated changes of inequality between 1987–8 and 2001–2. A panel data exercise is thus likely doomed from the outset: it is predicated on variation in the explanatory variables being comparable between and within villages. Combined with

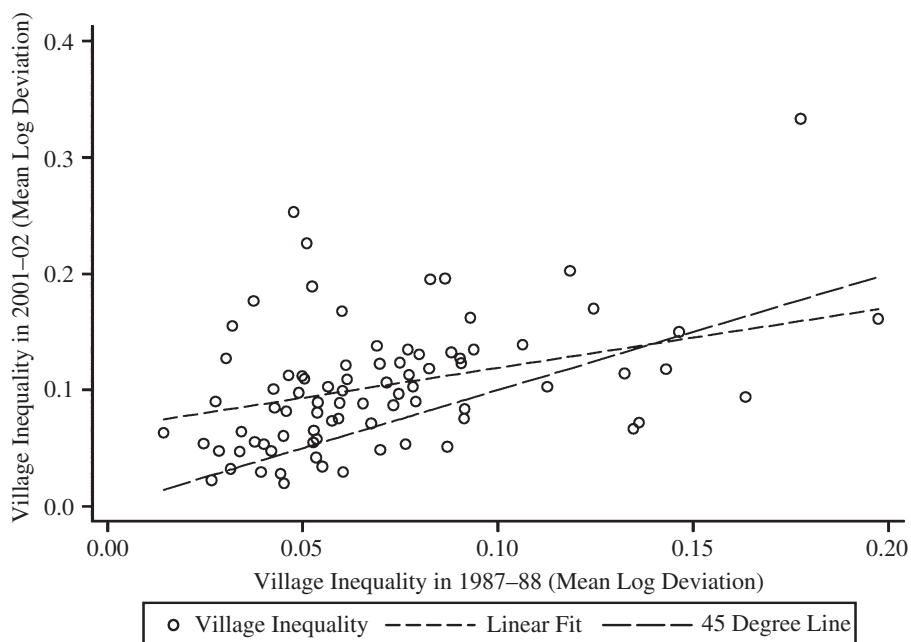


Fig. 3. *The Relationship Between Inequality in 1987-8 and Inequality in 2001-2*

Notes. This graph plots village-level inequality in 2001-2 versus initial inequality in 1987-8. We show the 45-degree line along which villages would be if inequality were unchanged over time. We also show the fitted values of a regression of inequality in the final period (2001-2) as a function of inequality in 1987-8.

results from Table 2 that showed that the effect of inequality on growth was not constant in the first place, Table 5 and Figure 3 suggest that a village-panel cannot be used to address the question of whether the growth-inequality relationship is driven by fixed heterogeneity.³² However, the attempt reveals the limited nature by which increases in inequality potentially mattered over this time period: after its initial impact from 1987 to 1988, there is no evidence of a link between subsequent inequality and growth.

5. Conclusions and Interpretation

If in 1987 a compulsive gambler wagered that between two otherwise identical Chinese villages, the low inequality village would be richer in 1997 than the high-inequality village, he would likely win. Our estimates suggest that if the difference in the Mean Log Deviation was 0.09, i.e. the difference between the 10th and 90th percentiles of inequality in 1987, the average annual growth rate for households in the low inequality village would be 1.8 percentage points higher relative to a median household annual growth rate of 3.4%. By this standard, high inequality was a robust and economically significant predictor of slower growth. If he made the same bet for growth 15 years out

³² In results not reported here, we find that accounting for fixed effects, irrespective of the panel estimator we use (e.g. accounting for dynamic panel data problems), yields statistically insignificant estimates of the impact of inequality on growth. As we explain above, we believe this is driven by the absence of any relationship between inequality and growth after 1987-8, as opposed to informing us about the nature of the 1987-8 based relationship.

1 (to 2002), it would be a closer call, though a better than fair bet. Beyond that, however,
2 if he was greedy enough to ride the inequality horse in other dimensions, placing
3 money on rising inequality as a marker of lower growth, it would be no better than a
4 crap shoot. The preponderance of evidence we find suggests that inequality did not
5 have a reliable causal impact on growth. We do not believe that higher inequality
6 impeded growth in rural China.

7 This conclusion rests on several strands of evidence. First, by using household-level
8t data we are able to rule out imperfect credit markets as the source of the causal
9 relationship between observed inequality and growth. Instead, the results using
10 household-level data point to institutional features of villages at the outset of reforms as
11 underlying the correlation between inequality and growth. Important though those
12 institutions may have been, whatever they were, there is little to suggest that they were a
13 *causal* consequence of inequality. Using village inequality from the 1990s as the basis of
14 predicting growth, we find that inequality ceases to matter: inequality in the 1990s is
15 different from inequality earlier in the reform period. Even focusing on 1987
16 inequality, the source of a safe bet for growth to 1997, we find that the effect of
17 inequality fades by 2002.

18 We still learn something about the inequality-related determinants of growth in rural
19 China. For example, our evidence points to the likelihood of growth-impeding policies
20 that were associated with higher inequality. The policies seemed to trap their victims,
21 especially the lower-educated in agriculture (a relative dead end during much of this
22 period), and impeded their movement into more lucrative labour markets. We cannot
23 be sure exactly what these institutions might be, but likely candidates include those
24 affecting household returns to running small businesses, the flexibility accorded to
25 households in meeting local grain quotas (i.e. the degree to which these commitments
26 could be made with cash instead of grain) and the costs of getting local government
27 permission to migrate to take advantage of newly emerging job opportunities (deBrauw
28 and Giles, 2008). Whatever the policies or institutions were, their effects eroded very
29 quickly in the reform period. Our data do not allow us to determine whether high
30 inequality caused such policies, or reflected a more general level of dysfunction at the
31 local level as villages embarked on the reform process.

32 More generally, and setting aside the profound problem of causality, our results show
33 the value of using household-level data to address the question of whether inequality
34 has a purely external impact on growth. Most obviously, we can rule out aggregation as
35 either hiding or exaggerating the link between inequality and growth. Furthermore,
36 the household-level data can be used to explore the heterogeneity of responses that
37 may be informative about the underlying economic mechanisms. Such evidence is
38 especially useful in ruling out factor-market explanations, and pointing by implication
39 to an institutional class of explanations. Our excellent cross-village data, with all the
40 benefits of clean and comparable measurement of inequality and growth over time,
41 however, also show the limits of how much can be ultimately learned in a growth-
42 inequality regression. Unless a researcher is willing to believe that both the underlying
43 relationship between growth and inequality is stable, and all variation in inequality is
44 driven by the same 'treatment', then little that is definitive can be learned from this
45 exercise. While it is difficult to believe that either condition holds in the Chinese
46 context, it is even harder to believe that such conditions hold in a cross-country setting.

1 *University of Toronto*
 2 *The World Bank*

3 *Submitted: xx Xxxx 20xx* **2**

4 *Accepted: xx Xxxx 20xx*

5
 6 Additional Supporting Information may be found in the online version of this article:

7 **Appendix S1.** Relating the Household and Village-level Specifications

8 **Appendix S2.** Data and Programs **3**

9 **Table S1.** Attrition Equations.

10 **Table S2.** Additional Covariates (Corresponding to Table 2, Column 5).

11
 12 Please note: The RES and Wiley-Blackwell are not responsible for the content or functionality of
 13 any supporting materials supplied by the authors. Any queries (other than missing material)
 14 should be directed to the authors of the article.

15 **References**

- 16
 17 Acemoglu, D. (1996). 'A microfoundation for social increasing returns in human capital accumulation',
 18 *Quarterly Journal of Economics*, vol. 111(3), pp. 779–804.
 19 Acemoglu, D., Bautista, M.A., Querubin, P. and Robinson, J. (2008). 'Economic and political inequality in
 20 development: the case of Cundinamarca, Colombia', in (E. Helpman, ed.), *Institutions and Economic*
 21 *Performance*, Cambridge, MA: Harvard University Press. **5**
 22 Aghion, P., Caroli, E. and Garcia-Penalosaand, C. (1999). 'Inequality and economic growth: the perspective of
 23 the new growth theories', *Journal of Economic Literature*, vol. 37(4), pp. 1615–60.
 24 Alesina, A. and La Ferrara, E. (2000). 'Participation in heterogeneous communities', *Quarterly Journal of*
 25 *Economics*, vol. 115(3), pp. 847–904.
 26 Alesina, A. and Rodrik, D. (1994). 'Distributive politics and economic growth', *Quarterly Journal of Economics*,
 27 vol. 109(2), pp. 465–90.
 28 Ammermueller, A. and Pischke, J.-S. (2009). 'Peer effects in European primary schools: evidence from the
 29 Progress in International Reading Literacy Study', *Journal of Labor Economics*, vol. 27(3), pp. 315–48.
 30 Angrist, J. and Pischke, J.-S. (2008). *Mostly Harmless Econometrics: An Empiricists Companion*, Princeton,
 31 NJ: Princeton University Press.
 32 Araujo, M.C., Ferreira, F.H.G., Lanjouw, P. and Özler, B. (2008). 'Local inequality and project choice: theory
 33 and evidence from Ecuador', *Journal of Public Economics*, vol. 92(5–6), pp. 1022–46.
 34 Baird, S., McIntosh, C. and Özler, B. (2009). 'The squeaky wheels get the grease: applications and targeting in
 35 Tanzania's social action fund', Working Paper, IRPS UC San Diego.
 36 Banerjee, A. and Dufo, E. (2003). 'Inequality and growth: what can the data say?', *Journal of Economic Growth*,
 37 vol. 8, pp. 267–99.
 38 Banerjee, A. and Iyer, L. (2005). 'History, institutions, and economic performance: the legacy of colonial land
 39 tenure systems in India', *American Economic Review*, vol. 95(4), pp. 1190–213.
 40 Banerjee, A., Mookherjee, D., Munshi, K. and Ray, D. (2001). 'Inequality, control rights, and rent seeking:
 41 sugar cooperatives in Maharashtra', *Journal of Political Economy*, vol. 109(1), pp. 138–90.
 42 Banerjee, A., Gertler, P. and Ghatak, M. (2002). 'Empowerment and efficiency: tenancy reform in West
 43 Bengal', *Journal of Political Economy*, vol. 110(2), pp. 239–80.
 44 Bardhan, P., Ghatak, M. and Karaivanov, A. (2007). 'Wealth inequality and collective action', *Journal of Public*
 45 *Economics*, vol. 91(9), pp. 1843–74.
 46 Bardhan, P., Mitra, S., Mookherjee, D. and Sarkar, A. (2008). 'Political participation, clientelism and targeting
 of local government programs: analysis of survey results from rural West Bengal, India', Working Paper,
 Boston: Department of Economics, Boston University.
 Benabou, R. (1996). 'Heterogeneity, stratification and growth: macroeconomic implications of community
 structure and school finance', *American Economic Review*, vol. 86(3), pp. 584–609.
 Benjamin, D. and Brandt, L. (2002). 'Property rights, labour markets, and efficiency in a transition economy:
 the case of rural China', *Canadian Journal of Economics*, vol. 35. **6**
 Benjamin, D., Brandt, L. and Giles, J. (2005). 'The evolution of income inequality in rural China', *Economic*
Development and Cultural Change, vol. 53(4), pp. 769–824.
 Benjamin, D., Brandt, L. and Giles, J. (2006). 'Inequality and growth in rural China: does higher inequality
 impede growth', Working Paper No. 237, Toronto: University of Toronto; and Discussion Paper No.
 2344, Bonn: Institute for the Study of Labor (IZA).

- 1 Besley, T. and Burgess, R. (2000). 'Land reform, poverty reduction, and growth: evidence from India',
2 *Quarterly Journal of Economics*, vol. 115(2), pp. 389–430.
- 3 Besley, T., Leight, J., Pande, R. and Rao, V. (2010). 'Land reform and inequality in southern India: a natural
4 experiment', Paper Presented at NEUDC 2010, November 6, 2010, Cambridge, MA.
- 5 deBrauw, A. and Giles, J. (2008). 'Migrant opportunity and the educational attainment of youth in rural
6 China', Policy Research Working Paper No. 4526, Washington, DC: The World Bank.
- 7 Buck, P.S. (1933). *All Men are Brothers* (translation of Shi and Luo, Shui Hu Zhuan [The Water Margin]),
8 New York: J. Day Co.
- 9 Chong, A. and Gradstein, M. (2007a). 'Inequality and institutions', *Review of Economics and Statistics*, vol. 89(3),
10 pp. 454–65.
- 11 Chong, A. and Gradstein, M. (2007b). 'Inequality and informality', *Journal of Public Economics*, vol. 91(1–2),
12 pp. 159–79.
- 13 Dayton-Johnson, J. (2000). 'Determinants of collective action on the local commons: a model with evidence
14 from Mexico', *Journal of Development Economics*, vol. 62(1), pp. 181–208.
- 15 Deaton, A. (1985). 'Panel data from time series of cross-sections', *Journal of Econometrics*, vol. 30(1–2), pp. 109–26.
- 16 Deaton, A. (2003). 'Health, inequality, and economic development', *Journal of Economic Literature*, vol. 41(1),
17 pp. 113–58.
- 18 Devereux, P. (2007). 'Improved errors-in-variables estimators for grouped data', *Journal of Business and
19 Economic Statistics*, vol. 25(3), pp. 278–87.
- 20 Easterly, W. (2007). 'Inequality does cause underdevelopment: insights from a new instrument', *Journal of
21 Development Economics*, vol. 84(2), pp. 755–76.
- 22 Engermann, S. and Sokoloff, K. (1997). 'Factor endowments, institutions, and differential paths of growth
23 among new world economies: a view from economic historians of the United States', in (S. Haber, ed.),
24 *How Latin America Fell Behind*, Stanford, CA: Stanford University Press. **7**
- 25 Fan, S., Zhang, L. and Zhang, X. (2004). 'Reforms, investment, and poverty in rural China', *Economic Devel-
26 opment and Cultural Change*, vol. 52(2), pp. 395–421.
- 27 Ferreira, F.H.G. (2010). 'Distributions in motion: economic growth, inequality and poverty dynamics', Policy
28 Research Working Paper 5424, Washington, DC: The World Bank.
- 29 Forbes, K. (2000). 'A reassessment of the relationship between inequality and growth', *American Economic
30 Review*, vol. 90(4), pp. 869–87.
- 31 Frank, M.W. (2009). 'Inequality and growth in the United States: evidence from a new state-level panel of
32 income inequality measures', *Economic Inquiry*, vol. 47(1), pp. 55–68.
- 33 Galasso, E. and Ravallion, M. (2005). 'Decentralizing targeting of an antipoverty program', *Journal of Public
34 Economics*, vol. 89(4), pp. 705–27.
- 35 Galor, O. and Moav, O. (2004). 'From physical to human capital accumulation: inequality and the process of
36 development', *Review of Economic Studies*, vol. 71(4), pp. 1001–26.
- 37 Galor, O. and Zeira, J. (1993). 'Income distribution and macroeconomics', *Review of Economic Studies*,
38 vol. 60(1), pp. 35–52.
- 39 Galor, O., Moav, O. and Vollrath, D. (2009). 'Inequality in landownership, the emergence of human-capital
40 promoting institutions, and the great divergence', *Review of Economic Studies*, vol. 76(1), pp. 143–79.
- 41 Glaeser, E. (2006). 'Inequality', in (D. Fullerton and B. Weingast, eds.), *The Oxford Handbook of Political
42 Economy*, Oxford University Press. **8**
- 43 Jalan, J. and Ravallion, M. (2002). 'Geographic poverty traps? A micro model of consumption growth in rural
44 China', *Journal of Applied Econometrics*, vol. 17(4), pp. 329–46.
- 45 Kanbur, R. (2005). 'Growth, inequality and poverty: some hard questions', *Journal of International Affairs*,
46 vol. 58(2), pp. 223–32.
- Khwaja, A. (2009). 'Can good projects succeed in bad communities?', *Journal of Public Economics*, vol. 93(7–8),
pp. 899–916.
- Kuznets, S. (1955). 'Economic growth and income inequality', *American Economic Review*, vol. 45(1), pp. 1–28.
- Lloyd-Ellis, H. (2000). 'Public education, occupational choice and the growth-inequality relationship', *Inter-
national Economic Review*, vol. 41(1), pp. 171–201.
- Lloyd-Ellis, H. (2003). 'On the impact of inequality on productivity growth in the short and long term:
a synthesis', *Canadian Public Policy*, vol. 29(Supplement), pp. S65–86.
- Lundberg, M. and Squire, L. (2003). 'The simultaneous evolution of growth and inequality', *ECONOMIC
JOURNAL*, vol. 113, pp. 326–44.
- Martinez-Bravo, M., Padro-i-Miquel, G., Qian, N. and Yao, Y. (2010). 'Accountability and growth: the costs of
village democracy in China', Paper presented at the China Economics Summer Institute at Qinghua
University, July 2010, New Haven, CT: Yale University.
- Oi, J. (1989). *State and Peasant in Contemporary China: The Political Economy of Village Government*, Berkeley, CA:
University of California Press.
- Park, A., Wang, S. and Wu, G. (2002). 'Regional poverty targeting in China', *Journal of Public Economics*, vol.
86(1), pp. 123–53.

- 1 Perotti, R. (1996). 'Growth, income distribution, and democracy: what the data say', *Journal of Economic Growth*,
2 vol. 1(2), pp. 149–87.
- 3 Persson, T. and Tabellini, G. (1994). 'Is inequality harmful for growth?', *American Economic Review*, vol. 84(3),
4 pp. 600–21.
- 5 Ravallion, M. (1998). 'Does aggregation hide the harmful effects of inequality on growth?', *Economics Letters*,
6 vol. 61(1), pp. 73–7.
- 7 Ravallion, M. and Chen, S. (2007). 'China's (uneven) progress against poverty', *Journal of Development*
8 *Economics*, vol. 82(1), pp. 1–42.
- 9 Shankar, S., Gaiha, R. and Jha, R. (2010). 'Information and corruption: the national rural employment
10 guarantee scheme in India', ASARC Working Paper Series, Canberra: Australian National University.
- 11 Shapiro, S. (1981). *Outlaws of the Marsh* (translation of Shi and Luo, *Shui Hu Zhuan* [The Water Margin]),
12 Beijing: Foreign Languages Press and Bloomington: Indiana University Press.
- 13 Shi, N. and Luo, G. (1365c). *Shui Hu Zhuan* [The Water Margin] (see Buck, 1933, and Shapiro, 1981, among
14 other translations). **9**
- 15 Sokoloff, K.L. and Zolt, E.M. (2005). 'Inequality and taxation: evidence from the Americas on how inequality
16 may influence tax institutions', *Tax Law Review*, vol. 59(2), pp. 167–241.
- 17 Wooldridge, J. (2002). *Econometric Analysis of Cross Section and Panel Data*. Cambridge/London: MIT Press,
18 pp. xxi, 752. **10**
- 19 Zhang, L., Yan, Y., Brandt, L. and Rozelle, S. (2005). 'China's rural public finance: the village perspective',
20 Annex 7: Report to the World Bank— *Village Finance: Tax-for-fee Reform, Village Operating Budgets, and Public*
21 *Goods Investment*, Washington, DC: World Bank.
- 22
- 23
- 24
- 25
- 26
- 27
- 28
- 29
- 30
- 31
- 32
- 33
- 34
- 35
- 36
- 37
- 38
- 39
- 40
- 41
- 42
- 43
- 44
- 45
- 46

Author Query Form

Journal: ECOJ

Article: 2452

Dear Author,

During the preparation of your manuscript for publication, the questions listed below have arisen. The numbers pertain to the numbers in the margin of the proof. Please attend to these matters and return the form with this proof.

Many thanks for your assistance.

| Query reference | Query | Remarks |
|-----------------|--|---------|
| Q1 | AUTHOR: Do you mean 'it is robust to control...'? Please check. | |
| Q2 | WILEY-BLACKWELL: Please supply date of submission and acceptance. | |
| Q3 | AUTHOR: Please forward the necessary files to the Production Editor for Appendix S2 so that they can appear online with your paper, and also cite the same. | |
| Q4 | AUTHOR: Angrist and Pischke (2009) has been changed to Angrist and Pischke (2008) so that this citation matches the Reference List. Please confirm that this is correct. (see Technical Appendix) | |
| Q5 | AUTHOR: Please provide the page range for reference Acemoglu et al. (2008). | |
| Q6 | AUTHOR: Please provide the page range for reference Benjamin and Brandt (2002). | |
| Q7 | AUTHOR: Please provide the page range for reference Engermann and Sokoloff (1997). | |
| Q8 | AUTHOR: Please provide the city location of publisher, page range for reference Glaeser (2006). | |

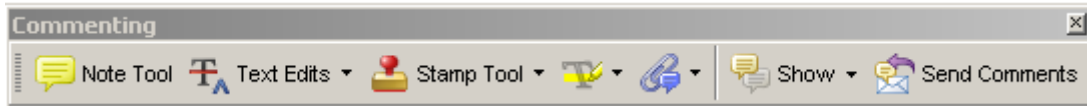
| | | |
|------------|---|--|
| Q9 | AUTHOR: Please provide the name of the publisher, city location of publisher, and also check the publication year for reference Shi and Luo (1365c). | |
| Q10 | AUTHOR: Please check the page numbers for the reference Wooldridge (2002). | |
| Q11 | AUTHOR: Lloyd-Ellis (2003) has been changed to Lloyd-Ellis (2003) so that this citation matches the Reference List. Please confirm that this is correct. | |
| Q12 | AUTHOR: Table 4 has not been mentioned in the text. Please cite the table in the relevant place in the text. | |
| Q13 | AUTHOR: Is it 'HH education 1987-8'? Please check 'HH education 8788' in Table 4. | |

USING E-ANNOTATION TOOLS FOR ELECTRONIC PROOF CORRECTION

Required Software

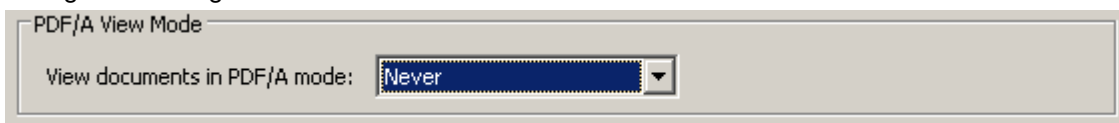
Adobe Acrobat Professional or Acrobat Reader (version 7.0 or above) is required to e-annotate PDFs. Acrobat 8 Reader is a free download: <http://www.adobe.com/products/acrobat/readstep2.html>

Once you have Acrobat Reader 8 on your PC and open the proof, you will see the Commenting Toolbar (if it does not appear automatically go to Tools>Commenting>Commenting Toolbar). The Commenting Toolbar looks like this:



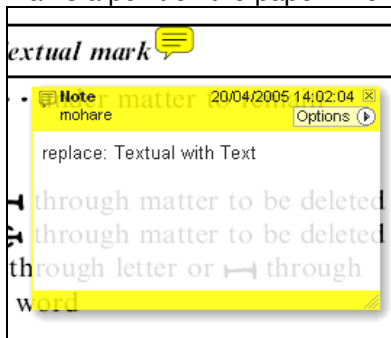
If you experience problems annotating files in Adobe Acrobat Reader 9 then you may need to change a preference setting in order to edit.

In the “Documents” category under “Edit – Preferences”, please select the category ‘Documents’ and change the setting “PDF/A mode:” to “Never”.



Note Tool — For making notes at specific points in the text

Marks a point on the paper where a note or question needs to be addressed.

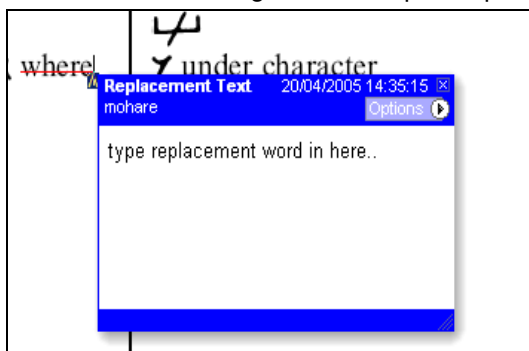


How to use it:

1. Right click into area of either inserted text or relevance to note
2. Select Add Note and a yellow speech bubble symbol and text box will appear
3. Type comment into the text box
4. Click the X in the top right hand corner of the note box to close.

Replacement text tool — For deleting one word/section of text and replacing it

Strikes red line through text and opens up a replacement text box.

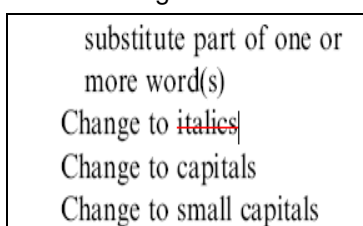


How to use it:

1. Select cursor from toolbar
2. Highlight word or sentence
3. Right click
4. Select Replace Text (Comment) option
5. Type replacement text in blue box
6. Click outside of the blue box to close

Cross out text tool — For deleting text when there is nothing to replace selection

Strikes through text in a red line.



How to use it:

1. Select cursor from toolbar
2. Highlight word or sentence
3. Right click
4. Select Cross Out Text

Approved tool — For approving a proof and that no corrections at all are required.

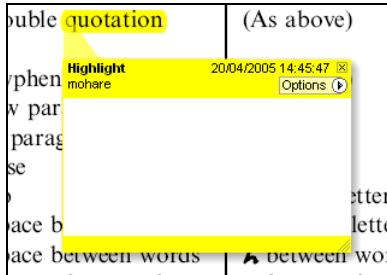


How to use it:

1. Click on the Stamp Tool in the toolbar
2. Select the Approved rubber stamp from the 'standard business' selection
3. Click on the text where you want to rubber stamp to appear (usually first page)

Highlight tool — For highlighting selection that should be changed to bold or italic.

Highlights text in yellow and opens up a text box.

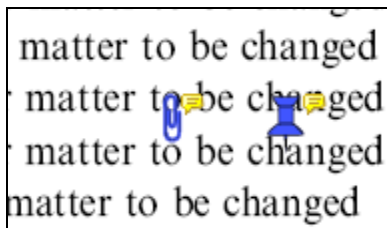


How to use it:

1. Select Highlighter Tool from the commenting toolbar
2. Highlight the desired text
3. Add a note detailing the required change

Attach File Tool — For inserting large amounts of text or replacement figures as a files.

Inserts symbol and speech bubble where a file has been inserted.

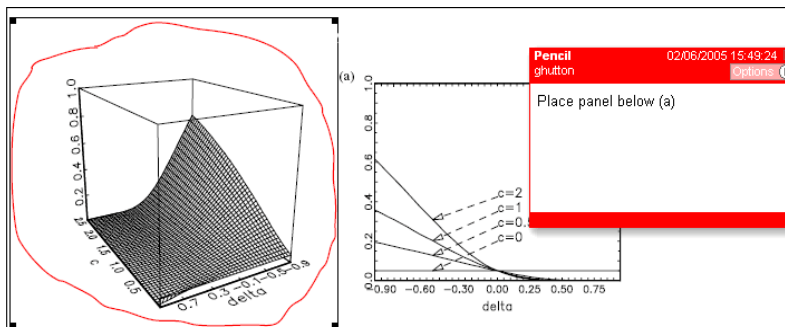


How to use it:

1. Click on paperclip icon in the commenting toolbar
2. Click where you want to insert the attachment
3. Select the saved file from your PC/network
4. Select appearance of icon (paperclip, graph, attachment or tag) and close

Pencil tool — For circling parts of figures or making freeform marks

Creates freeform shapes with a pencil tool. Particularly with graphics within the proof it may be useful to use the Drawing Markups toolbar. These tools allow you to draw circles, lines and comment on these marks.



How to use it:

1. Select Tools > Drawing Markups > Pencil Tool
2. Draw with the cursor
3. Multiple pieces of pencil annotation can be grouped together
4. Once finished, move the cursor over the shape until an arrowhead appears and right click
5. Select Open Pop-Up Note and type in a details of required change
6. Click the X in the top right hand corner of the note box to close.

Help

For further information on how to annotate proofs click on the Help button to activate a list of instructions:

