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DID HIGHER INEQUALITY IMPEDE GROWTH IN RURAL CHINA?*

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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

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[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.

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

The foundation of this article is a rich panel that tracks household incomes from early in the reform period (1987) to nearly the present (2002). The data allow us to link the detailed trajectories of household income to initial village and household conditions. Our question is simple: controlling for a rich set of covariates, did higher village inequality dampen household income growth? There are several advantages to using household-level data.[2] Most importantly, we can control for a host of household-level variables – notably flexible functions of initial household income – that may be confounded with local inequality. This helps rule out some potential explanations for the inequality–growth relationship, in particular those that rely on the aggregation of non-linear effects of own-income. With household-level data we are also able to explore 'within-village' heterogeneity of the impact of inequality on growth: Are the poor hurt more than the rich? Are households with higher educated members immune to the impact? We are also able to broaden the set of outcomes from 'income growth' to other economic variables, like the composition of income, e.g. concentration in agriculture, or participation in off-farm employment, that inform the question of how inequality may affect growth.

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

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

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

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

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

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

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

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

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

1. Why Might Village Inequality Affect Growth?

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

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

[4] See Perotti (1996), Aghion *et al.* (1999), and Lloyd-Ellis (2003) for excellent summaries of the cross-country inequality and growth literature, with detailed discussions of the theoretical linkages between inequality and growth. 11

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

[6] Several papers suggest that collective action and provision of public goods may be complicated by high levels of inequality within communities. See for example, Alesina and La Ferrara (2000), Dayton-Johnson (2000), Bardhan *et al.* (2007), Araujo *et al.* (2008), and Khwaja (2009). With respect to public finance, Sokoloff and Zolt (2005) find that high inequality is correlated with more regressive taxes, and less funding of local public investments and services. Glaeser (2006) reviews evidence suggesting that unequal societies are 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).

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

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

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

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

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

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

[11] See Fan *et al.* (2004) and Zhang *et al.* (2005). Jalan and Ravallion (2002) show that local levels of income, and associated public investments (e.g. roads and health care) are positively related to household consumption growth, and help explain the existence of 'geographic poverty traps', whereby households in poorer 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:

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

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*, $\mathrm{IQ}_{(-i)vt-1}$ (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 $\mathrm{IQ}_{(-i)vt-1}$ *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, 1365*c*). *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).

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

2.2.1. *Measurement error*

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

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

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, $\mathrm{IQ}_{(-i)vt-1}$ 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-attritted, 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 2007*a*), 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 livestock), 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

	Mean	Percentiles 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).

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

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

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

[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.

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

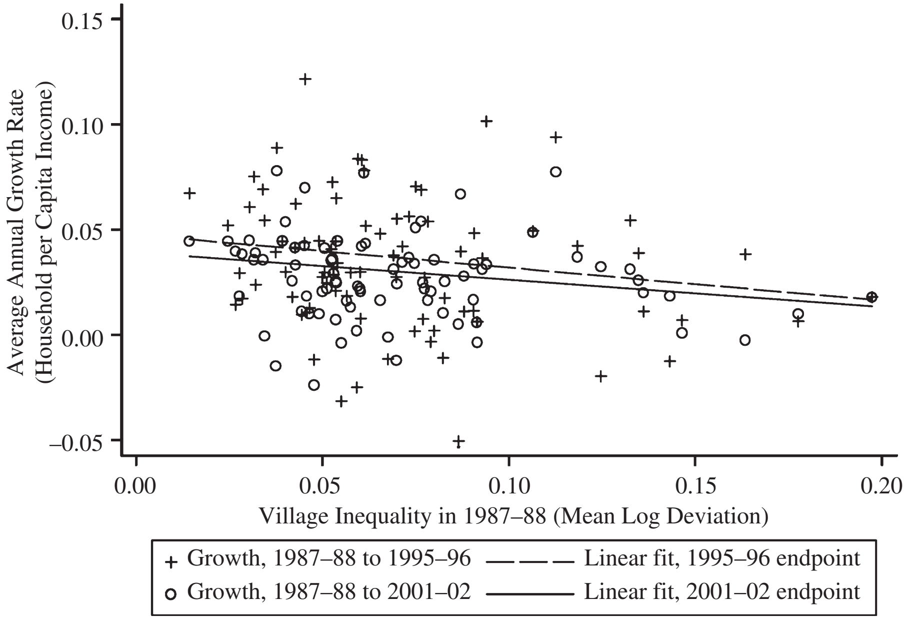

Fig. 1. *The Growth–Inequality Relationship: Different Endpoints*
Notes. The fitted values are based on simple regressions of growth from period 1987–8 through period T, on initial inequality in 1987–8. The regressions are weighted by the number of households in the village sample. The number of villages 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

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

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

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

Column (5) shows results comparable to those in column (3), but using the jackknifed Mean Log Deviation as the inequality index, i.e. estimates of equation (1).[24] Comparing the coefficients in column (5) to those in column (3), we see that the estimated effect of inequality is slightly smaller. The inclusion of household ‘*i*’ in the village inequality, at least in this instance, does not seem to lead to significant bias in the coefficient. We still retain this procedure, however, as *ex ante* there are good reasons to employ the jackknifed statistics. Our estimated effect of inequality is statistically significant in all time periods except the most recent, and as in previous columns, the effect declines monotonically over time. In column (6), we show estimates comparable to column (5), but with the jackknifed 90–10 ratio as our inequality measure. The results are similar to those using the Mean Log Deviation,

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

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

[24] In Table S2, we report the coefficients on the other covariates for the specification in column (5). At the household-level, we find the most important predictors of household income growth to be age, with younger 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.

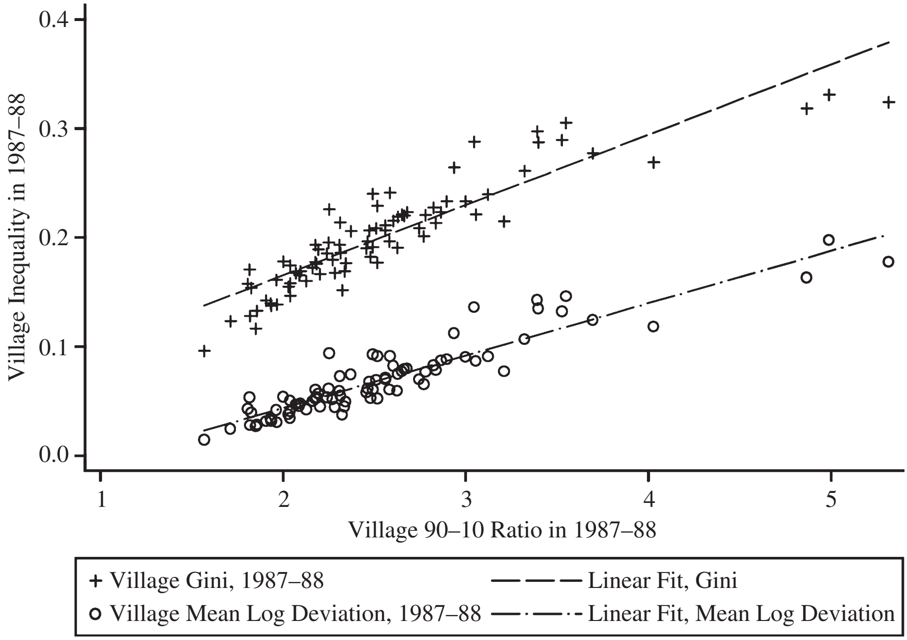

Fig. 2. *The Relationship Between Inequality and the* 90–10 *Ratio,* 1987–8

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

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

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

3.3. *Regression Results: Heterogeneous Responses*

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

[25] Indeed, the differential inclusion of the household-level covariates may also be part of the reason that Ravallion (1998) finds more significant results at the household than county-level: controlling for household characteristics affects the precision of estimation, and also addresses possible omitted variables (non-linearities 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)
Base effects (1987–8)								
Inequality	−0.310*	−0.214*	−0.344*	−0.188	−0.268*	−0.131	−0.142	−0.074
(Jackknifed MlnD)	(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 $\leq$ 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 $\leq$ Age $\leq$ 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 $\leq$ Age $\leq$ 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)
Interactions								
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)
Combined effect by quartile								
Combined Q1		−0.281*		−0.242*		−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.

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

Turning first to the education and cohort interactions, they are jointly and individually insignificant for 1995–6, and 2001–2, when the overall effect of inequality is also insignificant. However, in the middle two time periods of our 1990's sample (1997–8 and 1999–2000), we see that education significantly offsets the adverse effect of inequality: the impact of inequality hurts less educated households most. The timing coincides with the Asian financial crisis, and suggests that inequality hurt the least educated most during those periods, possibly due to reduced migration opportunities. The cohort interaction is also significant for 1999–2000, and borderline in 1997–8. This interaction coefficient suggests that the effect of inequality was worse for the young, which seems somewhat surprising. However, it is the young that normally have the highest income growth potential and are the most mobile, and the adverse impact of inequality seems to have hurt these households most. In other words, income growth is potentially higher for younger households, but those in more unequal villages experience stunted growth.

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

3.4. *Regression Results: Composition of Income*

We may be able to learn more about the nature of the effect of inequality by investigating links between inequality and other measures of economic development. One potentially informative dimension is the composition of household income, which will reflect changes in the economic structure of villages, and households' ability to participate in more lucrative economic opportunities, especially in non-agricultural 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:

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

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 (2007*b*) 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.

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

In earlier versions of our paper, we implemented the panel-based strategy. In doing so, we discovered at least two important issues that underscore the difficulty of using repeated observations on the villages to control for initial village heterogeneity. A discussion of these issues is worthwhile, as it helps to reveal important aspects of the inequality–growth relationship. We first constructed a village-level panel data set using the balanced panel of households to construct the growth rate and mean log income variables for each period (symmetric with the household regressions), and the larger non-attritted samples to construct key village covariates like village inequality. There are seven available two-year periods: 1987–8, 1989–90, 1991–3, 1995–6, 1997–8, 1999–2000 and 2001–2.[30]

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

[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.

[29] See Kuznets (1955). Ravallion and Chen (2007) explore precisely this question as they investigate linkages between growth rates and various measures of poverty and inequality using nationally representative 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).

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

[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
Base effects (1987–8)						
Inequality (MlnD)	0.233	−0.396	−0.030	0.955	−1.657*	0.659
	(0.442)	(0.623)	(0.441)	(0.492)	(0.560)	(0.559)
13 HH education 8788	−0.004*	−0.004	0.005*	0.003	−0.016*	0.011*
	(0.002)	(0.002)	(0.002)	(0.004)	(0.005)	(0.004)
Age $\leq$ 30	−0.015	0.000	0.019	0.008	−0.043	0.039
	(0.017)	(0.018)	(0.016)	(0.020)	(0.028)	(0.021)
31 $\leq$ Age $\leq$ 40	0.006	0.001	0.007	0.028	−0.041	0.027
	(0.010)	(0.015)	(0.013)	(0.017)	(0.023)	(0.023)
51 $\leq$ Age $\leq$ 60	0.009	−0.035*	−0.016	0.009	−0.034*	−0.016
	(0.012)	(0.013)	(0.014)	(0.013)	(0.013)	(0.014)
Age > 60	0.008	−0.095*	−0.029	0.006	−0.091*	−0.031
	(0.020)	(0.027)	(0.018)	(0.020)	(0.027)	(0.018)
Interactions						
Inequality × Education				−0.100*	0.171*	−0.096*
				(0.047)	(0.060)	(0.049)
Inequality × Under 40				−0.317	0.595	−0.281
				(0.193)	(0.314)	(0.263)
F-Interactions				3.91*	7.81*	2.60
				(0.0240)	(0.0008)	(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
Measure of inequality: Mean Log Deviation						
Beginning period						
(1) 1987–8	−0.642*	−0.176	−0.225*	−0.213*	−0.174*	−0.116
	(0.254)	(0.136)	(0.088)	(0.092)	(0.080)	(0.082)
(2) 1989–90		−0.147	−0.086	−0.111	0.020	0.027
		(0.215)	(0.131)	(0.094)	(0.077)	(0.064)
(3) 1991–3			0.166	0.119	0.119	0.139
			(0.286)	(0.198)	(0.120)	(0.093)
(4) 1995–6				0.188	−0.002	0.102
				(0.238)	(0.192)	(0.128)
(5) 1997–8					−0.088	0.079
					(0.323)	(0.188)
(6) 1999–2000						0.294
						(0.180)
Measure of inequality: 90–10 *ratio*						
Beginning period						
(1) 1987–8	−0.043*	−0.013	−0.013*	−0.014*	−0.011*	−0.008*
	(0.014)	(0.007)	(0.004)	(0.004)	(0.004)	(0.004)
(2) 1989–90		−0.004	0.000	−0.005	0.001	0.000
		(0.008)	(0.006)	(0.004)	(0.004)	(0.003)
(3) 1991–3			0.005	0.002	0.005	0.006
			(0.012)	(0.008)	(0.006)	(0.005)
(4) 1995–6				0.009	0.000	0.006
				(0.011)	(0.008)	(0.006)
(5) 1997–8					−0.009	0.001
					(0.018)	(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 (MlnD 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

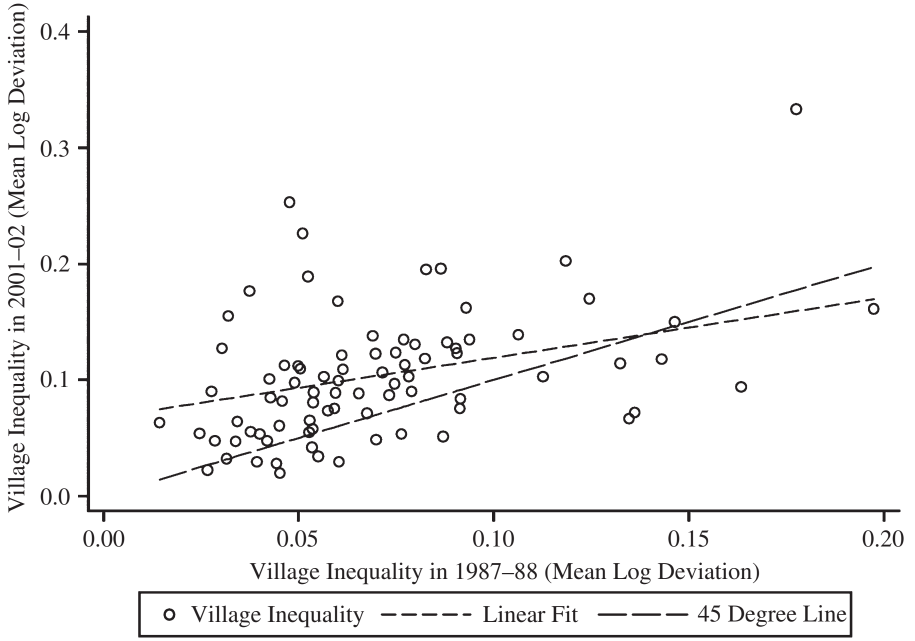

Fig. 3. *The Relationship Between Inequality in 1987–8 and Inequality in 2001–2*

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.

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

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

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

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

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

Appendix S1. Relating the Household and Village-level Specifications
Appendix S2. Data and Programs 3
Table S1. Attrition Equations.
Table S2. Additional Covariates (Corresponding to Table 2, Column 5).

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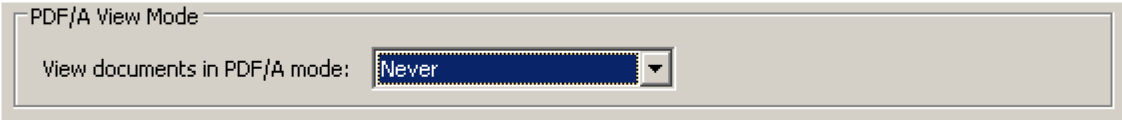

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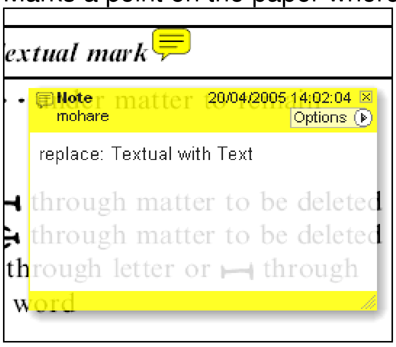

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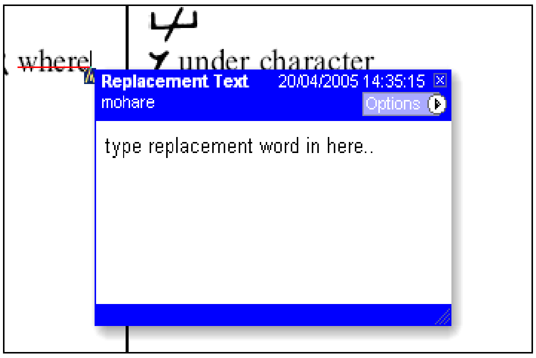

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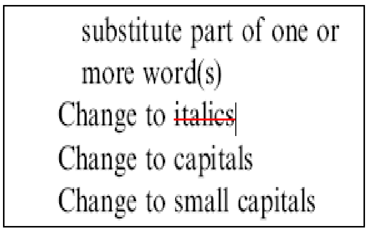

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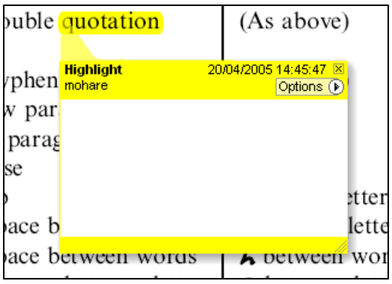

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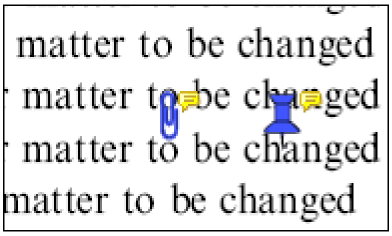

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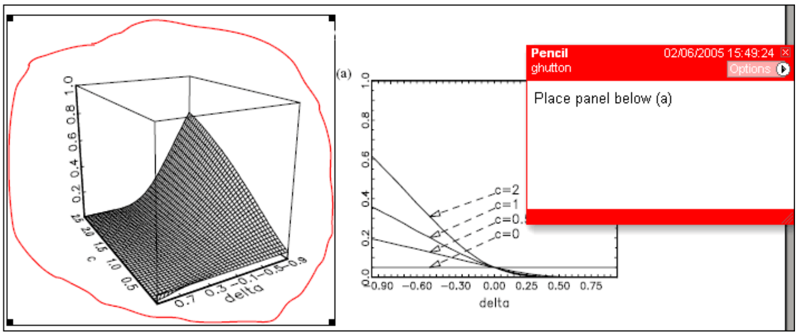

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