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Aggregate and distributional impacts of China's household responsibility system*

John Gibson †

It is 40 years since China started to abandon collective farming, with initial rural reforms in 1978 that culminated in adoption of the household responsibility system (HRS). Existing studies of impacts of these reforms do not consider nonrandom spread of the HRS, spillovers from early adopters, or distributional effects. In this paper, the synthetic control method and spatial autoregressive panel models with autoregressive errors are used to estimate impacts of the HRS that account for these features. The HRS had a significant positive effect on grain output and food supply in China, while also helping to reduce regional inequality.

Key words: China, household responsibility system, regional inequality, spatial spillovers, synthetic controls.

1. Introduction

In 1978, China began to abandon collective farming, as households started to regain responsibility for decisions about inputs and outputs. What became known as the household responsibility system (HRS) is considered to have started as an illegal experiment in Anhui Province and then spread throughout China. In this 40-year retrospective study, I quantify some aggregate and distributional effects of these reforms, accounting for features not previously considered, by using modern methods of quasi-experimental analysis.

Applied economic studies of these reforms developed soon after they occurred. Lin (1987) showed that the switch to the HRS happened sooner in provinces where crops were more important than livestock, and draft animals relied on more than machinery. This nonrandom spread, and the rapid rate of HRS adoption saw it become a national-level reform within approximately years. This limited the number of years with spatial variation in uptake rates – which complicates attempts to measure impacts of the HRS on agricultural growth rates (Lin 1992). Other studies set the reform era in the context of longer-term changes; for example, during the HRS adoption years, productivity rose about 8 - 12 per cent per year for rice, maize, and wheat (Jin *et al.* 2002). In contrast, productivity had stagnated under communal farming,

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† John Gibson (email: jkgibson@waikato.ac.nz), is Professor of Economics at School of Accounting, Finance, and Economics, University of Waikato, Hamilton, New Zealand.

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being approximately about 20 per cent lower on the eve of the HRS than in 1957, when an earlier experiment with rural cooperatives ended (Wen 1993).

New analyses of the impacts of these reforms may be warranted, because the existing literature does not account for some features, such as spillovers from early adopters, and largely ignores the distributional impacts. This re-examination is also timely, given the 40 years that have elapsed since the reforms began. Moreover, a renewed focus on effects of these reforms may help to correct an imbalance in the literature, which now pays more attention to the impacts of policy mistakes, such as the Great Leap Forward and the Cultural Revolution, in part because these mistakes provide more exogenous variation over time and space that helps to aid the identification of impacts (e.g. on subsequent height, or schooling).[1] This emphasis in the recent literature may be misplaced, because it is the rural reforms that began in 1978 that triggered much of China's later spectacular growth, and the escape from mass poverty (Ravallion and Chen 2007). A comprehensive study of the impacts of these reforms is valuable for understanding China's recent economic history and for considering lessons for other countries.

The rest of the paper is structured as follows. Section 2 covers development of the HRS, noting the variety of responsibility systems that initially evolved and their nonrandom spread over space. Section 3 provides aggregate-level evidence on the effects of the reforms on China's food supply, in terms of daily per capita calories. The synthetic control method is used, as a way to deal with the lack of a control group (and with any time-varying omitted variables that may reflect the nonrandom spread of the reforms). Section 4 considers spillovers from provinces who adopted the HRS earlier than their neighbours, using a spatial econometric panel model of per capita grain output. Some of the distributional impacts are described in Section 5; the reforms reduced some of the income gaps between poorer, rural, provinces and richer, more urban, ones, and triggered a long-run decline in interprovincial inequality. The conclusions are in Section 6.

2. The household responsibility system

An illegal experiment by 18 peasants in Xiaogang village, in Fengyang County of Anhui Province, in late 1978, is typically credited as the origin of the HRS. Under the commune system at the time, resource allocation decisions were made by commune authorities and their agents, the leaders of production brigades and production teams, leaving farm households with no control over capital, land and output (Wen 1993). For the key input of labour, commune authorities could allocate workers to tasks, with a work points system to reflect the quantity and quality of the effort each team member put in (Lin 1987). Instead of this collective approach, these 18

[1] A *Google Scholar* search shows more than four times as many articles on the Cultural Revolution as on the HRS.

peasants secretly agreed to divide village land into private family plots, with each responsible for their own plot, handing over some output to the Government and to the commune, but with the surplus kept for themselves. They were apparently spurred into this illegal activity by the need to pre-empt the risk of famine (Anhui had extremely high mortality in the Great Leap Famine, 20 years earlier).[2]

After this secret reform, village harvests grew much larger, which attracted interest from surrounding areas, and was soon noticed by the Government in Beijing. At about the same time, reformist Deng Xiaoping began to take over as paramount leader from Hua Guofeng (Mao Zedong's anointed successor), even as Hua continued as nominal chairman of the Communist Party of China (CPC) until 1981. While China continued to officially adhere to collectivised agriculture in 1979 and 1980, Deng Xiaoping sent reformers to oversee development of family farming in Anhui and Sichuan; two poor and remote provinces, where any failure of the reforms could be contained without causing major political problems (Bai and Kung 2014). The impetus for the reform is thus often attributed to peasant grassroots activism:

> ...the household responsibility system was worked out among farmers, initially without the knowledge and approval of the central government. It was generated through the efforts of peasants themselves and spread to the other areas because of its merits. (Lin 1988, p. 201)

Subsequent research suggests a more complicated reality, with divided attitudes amongst farmers and a less central role for Deng Xiaoping. One beginning to the opening up and reform era in China can be dated to the 3rd Plenary Session of the 11th Central Committee of the CPC, in December 1978. In terms of the rural sector, a key statement from the Plenary was as follows:

> 'Commune members' private plots, family side lines and trade in rural collective markets are necessary supplements to the socialist economy. In the people's communes, the system of three-level ownership – commune, brigade, and team –with the production team as the basic accounting unit, must be resolutely enforced' (*People's Daily*, December 24, 1978).

While affirming the central role of the collective, private plots were allowed and the autonomy of production teams within the commune was increased. This Plenary also lifted procurement prices, which effectively improved farmers' terms of trade. Under the new flexibility, at least six broad forms of production responsibility systems evolved (Teiwes and Sun 2016). These

[2] Yixin (2010) reports 6.3 million famine deaths in Anhui from 1958 to 1961, which was 18.4% of the population.

included fixing output quota for each household, with bonuses and fines to ensure compliance, and with production teams responsible for distributing what was produced under the contract (*baochan daohu*), or having work groups as the contractors (*baochan daozu*), or fixing farm output quotas to individual labourers under production team ownership (*baochan daolao*).

The form that eventually became the universal practice was assigning full responsibility to the household (*baogan daohu* or *dabaogan*), summarised as 'ensuring state requirements, retaining sufficient for the collective, and keeping all the rest for oneself' (Johnson 1982, p. 438). This was not universally demanded by peasants in the initial period; in many areas, peasants were ambivalent about decollectivisation; for example, Zweig (1983, p. 887) notes production teams who turned down the chance to establish household quotas in 1981, and others who, likewise, did not 'want to move off Chairman Mao's road'. Responsibility systems could be crop-specific (Johnson 1982), and as production teams rarely mono-cropped, they faced several types of responsibility systems in the transition from collectivised to family farming. It is also notable that the policy embodiment of decollectivisation was *baochan daohu* (contracting production to households), and even then much of the CPC leadership saw this as a temporary expedient for desperate situations and the return to socialist agriculture remained a basic objective (Teiwes and Sun 2016). However, by the time that the HRS became national policy, with Document No. 1 in early 1982, *baochan daohu* had been largely superseded by the far-reaching *baogan daohu* system of households taking over the unified operations and distribution functions of the production teams in the commune structure.

These details matter because the spread of the HRS reflected not only if areas were poor and remote, the type of places where responsibility systems were meant to gain official support, but also, at least up until 1981, the role of provincial leaders who varied in their backing of the reforms. While provincial leaders in Anhui (Wan Li), Guangdong (Ren Zhongyi), Guizhou (Chi Biqing), and Sichuan (Zhao Ziyang) supported reforms, others, such as in Jiangsu (Xu Jiadun), were less supportive of what was interpreted as a capitalist restoration. Location also mattered as reforms spread to border areas of provinces adjacent to Anhui (Chung 2000) but were adopted more slowly in farther-away provinces, like Heilongjiang and Jilin. The northeast also had more rural industry and more mechanised agriculture; so, with higher output and incomes they had less to gain from switching to responsibility systems, and villagers in mechanised areas also feared that household quotas would complicate their use of commune machinery (Zweig 1983). Finally, weather shocks also seem to have affected HRS adoption rates (Bai and Kung 2014).

This nonrandom spread is illustrated in Figure 1, showing the situation at the end of 1980 and the end of 1981. I use the adoption rates reported for each province in each year from 1979 to 1984 by Bai and Kung (2014), where the 1979 to 1982 data were originally collected by Chung (2000). The adoption rates varied within provinces, and also by crop and type of responsibility system, as noted above, but such fine level data have not been tabulated on a consistent basis across time and space. The values that are mapped are also used in the empirical analysis in Section 4 and are for the *baogan daohu* type of responsibility system, of contracting everything to the household. At the end of 1980, two provinces: Anhui; and Guizhou, had higher adoption rates than anywhere else, and most of their neighbours are in the next highest class. Meanwhile, a belt through central China, from Hunan and Hubei, extending up through Shaanxi and Shanxi, and into the northeast, had the lowest adoption rates. By the end of 1981, a belt from Gansu down to Guizhou, plus two neighbours (Anhui and Jiangxi), had the highest adoption rates, while adjacent provinces other than Hunan and Shaanxi were in the second or third highest classes. In contrast, Heilongjiang (<1 per cent) and Jilin (4 per cent) had adoption rates that were far below other provinces.

The situation shown in Figure 1 entails considerable spatial autocorrelation; provinces with high adoption rates cluster together and likewise for those with low adoption rates.[3] This pattern did not persist for long; by the time of Document No. 1 in early 1982, when the HRS was officially proclaimed as the production responsibility system of the socialist economy, two-thirds of production teams had switched to household-based farming. By early 1983, 79 per cent of production teams were using the HRS. After that, the central government put pressure on recalcitrant provinces that were slow adopters, with the remaining 20 per cent of teams switching to the HRS within a year (Bai and Kung 2014). Therefore, the HRS quickly became a national-level reform.

3. Synthetic control evidence on aggregate impacts of the HRS

The nonrandom spread of the HRS illustrated in Figure 1, and the rapid rate of adoption that saw it become essentially a national-level reform within approximately three years, limits variation that could help to estimate impacts. In some situations, outcomes for slow adopters can be used to estimate counterfactual outcomes for fast adopters, if they had not adopted, but the nonrandom set of provinces that were fast to adopt the HRS, and the limited period when outcomes for slow adopters and fast adopters can be observed, weakens such approaches. While this issue has been noted in past attempts to measure the impact of the HRS on China's agricultural growth (Lin 1992), newly available empirical methods suit such situations. In particular, the synthetic control method has been developed to study one-off

[3] Moran's local spatial autocorrelation measure for the data in Figure 1 is statistically significant for three provinces in 1980 and four in 1981 but for only two in 1982 and one in 1983. The convergence in HRS adoption rates is also seen in the falling coefficient of variation, for the HRS adoption rates across provinces, which had values of 0.99, 0.56, 0.25, and 0.04 from 1980 to 1983.

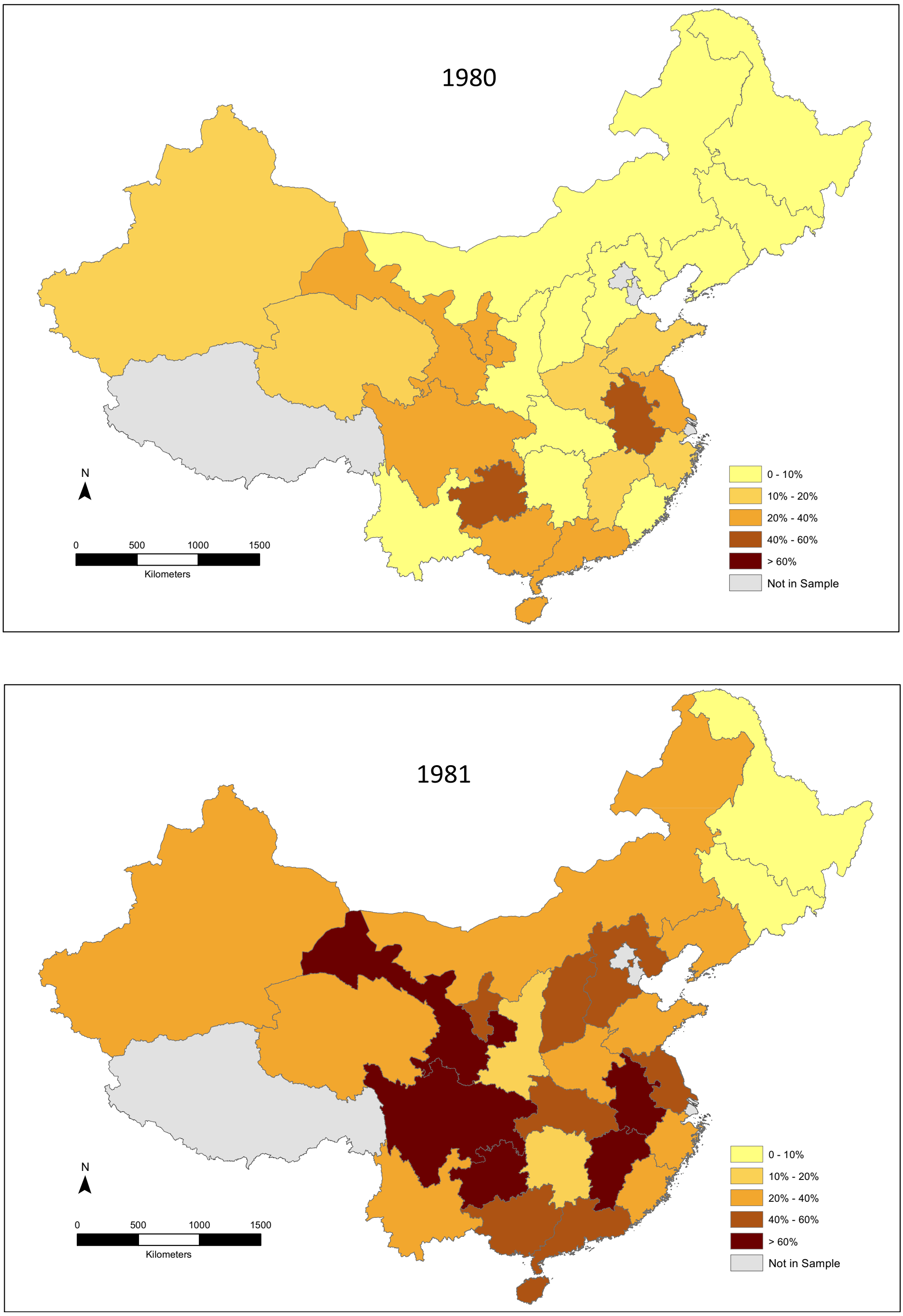

Figure 1 The nonrandom spread of the household responsibility system (HRS) (percentage of production teams in each province that had switched to HRS). [Colour figure can be viewed at wileyonlinelibrary.com]

events that lack a natural control group (Abadie *et al*. 2010). Previous use of the synthetic control method in related situations of rapid reform includes study of the impacts of economic liberalisation episodes (Billmeier and Nannicini 2013) and impacts of German reunification (Abadie *et al*. 2015).

With the synthetic control method, a weighted average of nontreated units that evolves most closely to how the treated unit evolved over time, prior to the treatment, is formed. The impact of the treatment is then estimated as a post-treatment difference between the treated unit and the synthetic control. This method overcomes the difficulty of finding a comparison country to show what China would have looked like if rural responsibility systems had not been introduced from 1978 onwards, with this task delegated to the matching algorithm. Note this method picks up effects of reforms coincident with the HRS, such as the higher state procurement prices introduced at the third Plenary Session of the CPC in December 1978. These are estimated by Lin (1992) to have an impact on crop output growth equivalent to approximately one-third of the impact of the HRS, although the two reforms interact because higher prices better motivate individuals to work harder if they are residual claimants. The synthetic control method provides an overall estimate of the impact of the whole package of pro-agricultural reforms introduced from the third Plenary onwards.

An advantage of synthetic control over traditional regression methods is any omitted unobservable confounders do not have to be either time-invariant (as in the fixed-effects method) or have parallel trends (as in the difference-in-differences method). Instead, synthetic control can handle endogeneity biases from time-varying omitted variables (Abadie *et al*. 2015). At least one-third of the temporal and spatial variation in province-level adoption rates for the HRS remains unexplained in studies by Lin (1987, 1988) and Bai and Kung (2014). Thus, even if one adopted the traditional approach of introducing a comprehensive set of control variables to deal with confounders, it is always possible that apparent impacts of the HRS could be due to the effects of omitted variables. Another advantage over traditional regression approaches, especially the typical empirical methods used in cross-country studies, is the synthetic control method makes explicit the contribution of each comparison unit to the counterfactual and avoids extrapolation beyond the support of the comparison units (Abadie *et al*. 2015).

I focus on the effect of the HRS on China's per capita food supply, in terms of calories. I use annual FAO data from 1970, when GDP variables for many low-income countries first entered FAO statistics, until 2000. From 2001, after joining the WTO, China starts to have a larger effect on world food trade and so may affect food supply in the comparison countries, which undermines the identification of impacts because the counterfactual groups are potentially affected by the treatment. A linear combination of untreated countries is algorithmically selected based on their similarity to China in the pretreatment period. This matching is in terms of past realisations of the outcome variable (total food supply, in terms of daily per capita calories) and relevant covariates (agricultural land per capita, growth rates of population and GDP, and mean per capita GDP).

The synthetic control is made up of 28.4 per cent Thailand, 23.1 per cent Myanmar, 19.3 per cent Indonesia, 12 per cent India, 11.1 per cent Guinea-Bissau, 6 per cent Cape Verde, and slight amounts from 18 other countries (the donor list of 24 countries was based on criteria of data availability and similarity of economic structure to China in the 1970s). The evolution of average per capita daily calories in China from 1970 until 2000 is shown in Figure 2. In the 8 years prior to the third Plenary in late 1978, the average per capita food supply provided 1,915 calories per day. After 1978, average calories were 2,495 per person per day. Thus, a naïve estimate of the effect of the HRS (and coincident rural reforms) is that it produced a gain in food supply equivalent to 580 calories per person per day.

This naïve estimate ignores improvement in food supply that may occur anyway. This is seen in how calories in 'synthetic China' would have evolved, where this synthetic control tracks the evolution of China's per capita food supply perfectly from 1970 to 1977 (with a 1970 to 1978 average of 1,912 calories versus 1,915 for actual China). A gap between synthetic China and actual China opens up from 1978 and is over 300 calories per person per day by 1982, by which time over three-quarters of China's rural production teams had switched to the HRS. The food supply improvement predicted to have occurred even without the rural reforms in China is seen from the post-1978

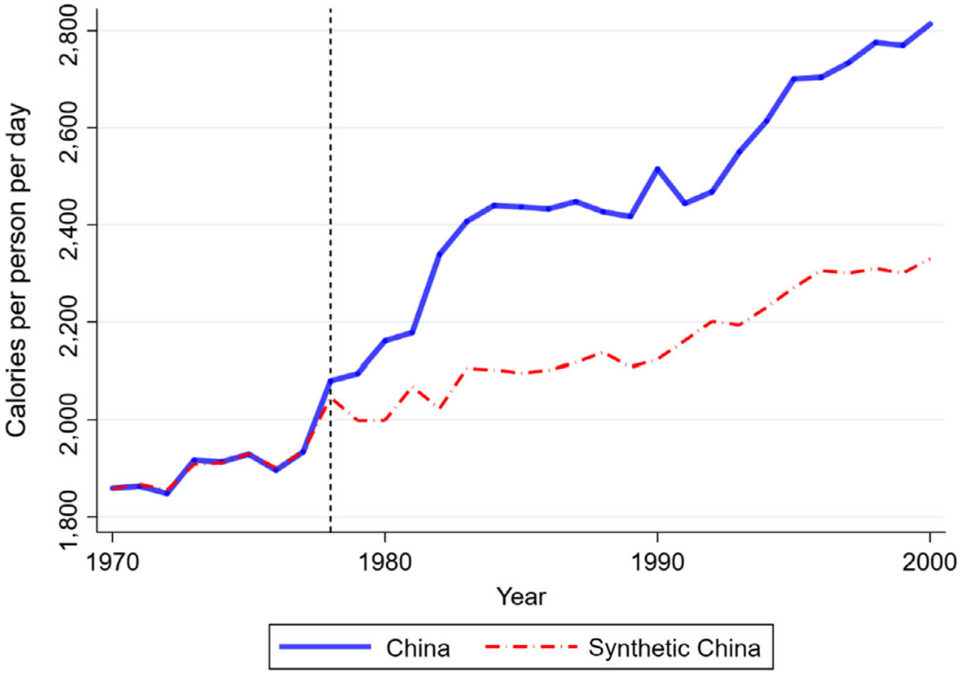

Figure 2 The evolution of calorie availability in China and in synthetic China. [Colour figure can be viewed at wileyonlinelibrary.com]

average daily calories of 2,162 for synthetic China, which is an increase of 250 calories per day compared to the prior period. Given that the actual increase in China was 580 calories per person per day, the gap of 330 calories per person per day between the two differences is the synthetic control estimate of the treatment effect of the 1978 rural reforms. These 330 calories are equivalent to 15 per cent of average daily calories for synthetic China. Also, over this period, China's population grew rapidly, from just below 1 billion in 1978 to 1.3 billion by 2000. Thus, the rural reforms helped improve per capita food supply even with a rapidly growing population.

To establish statistical significance of the estimated impact, in-space placebos are used. Each country in the donor list was, successively, set as the country subject to a (placebo) treatment from 1978 onwards, with the difference between actual and synthetic calories for each country plotted in Figure 3 (with the values for China shown with a bold line). In just four per cent of the cases (years × countries) where the HRS did not take place is there an estimated impact on food supply (in the post-treatment period) that is greater than what is actually observed for China. Thus, this placebo test gives an empirical *P*-value of 0.04 for the statistical significance of the aggregate impact on food supply of the HRS.

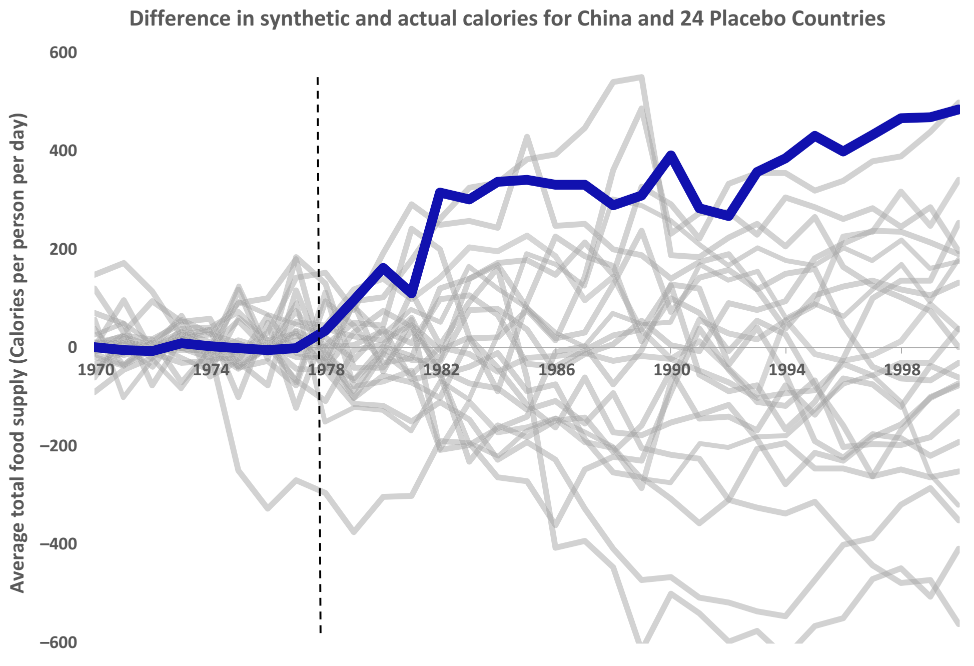

Figure 3 In-space placebo test to establish statistical significance of household responsibility system (HRS) impact. [Colour figure can be viewed at wileyonlinelibrary.com]

4. Spatial econometric panel analysis

The synthetic control method deals with threats to identifying impacts, such as lack of a natural control group, but does not show the various pathways through which impacts occur. A particular pathway of interest, given the nonrandom HRS spread shown in Figure 1, is possible spillovers from one province to another. Case studies note examples of diffusion from early adopters in Anhui to nearby areas like Heze prefecture in Shandong (Chung 2000), while Chen and Huffman (2006) also note that the HRS is an example of an innovation that was adopted in neighbouring regions soon after it emerged. However, existing regression studies that use spatial and temporal variation in HRS adoption rates, such as Lin (1992), have not allowed for these effects.

Instead, a typical approach uses a panel model, with fixed effects for provinces and years, and observations treated as independent of each other (Xu 2016). The results of such a model are reported in column 1 of Table 1, for (the log of) per capita grain output of each province over 1979 - 1984, GR_{it}. The adoption rate of the HRS in province i in year t is HRS_{it}, the μ_i are time-invariant fixed effects for each province, the ϑ_t are year fixed effects, and e_{it} is a random error[4]:

$$GR_{it} = \beta_1 HRS_{it} + \mu_i + \vartheta_t + e_{it}. \tag{1}$$

The interpretation of the coefficient $\widehat{\beta}_1$ in column 1 of Table 1 is that moving from an HRS uptake rate of zero to 100 per cent lifted per capita grain output by 26 per cent.[5]

Spillovers can be allowed for with a spatial weights matrix, W, that has a value of one for neighbours (provinces sharing any point on the border with province i) and zero otherwise. The diagonals of W are zero because a province cannot be a neighbour to itself. The spatial weights matrix allows spatial lags to be introduced; these are averages of the dependent variable and the independent variable(s) in the neighbouring provinces:

$$GR_{it} = \lambda WGR_{it} + \beta_1 HRS_{it} + \beta_2 WHRS_{it} + \mu_i + \vartheta_t + e_{it}. \tag{2}$$

This spatial Durbin model allows for changes in an outcome variable in a particular time and space to affect contemporaneous outcomes in the surrounding areas (via the autoregressive spatial lag of the dependent variable, if $\lambda \neq 0$). It also lets changes in the independent variables affect not

[4] Adoption rates from Bai and Kung (2014) exclude Tibet, Beijing, Tianjin, and Shanghai. The models require a strongly balanced panel, so missing adoption rates (2 years for Qinghai and Ningxia, 1 year for Xinjiang) are interpolated.

[5] With a log-dependent variable, the Table 1 coefficient of 0.233 gives a percentage change of $(e^{0.233}\text{-}1) \times 100$.

Table 1 Impacts of the Household Responsibility System on per capita grain output

	Standard panel model analysis	Spatial lag of the covariate and outcome	Spatial lag of errors, covariate and outcome
Adoption rate of HRS	0.233*** (0.082)	0.225*** (0.076)	0.270*** (0.071)
Average impacts:			
Direct	0.233*** (0.082)	0.218*** (0.078)	0.277*** (0.072)
Indirect	n.a.	0.074 (0.052)	0.145* (0.078)
Total	n.a.	0.292*** (0.066)	0.423*** (0.112)
Province fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Spatial lag of HRS	No	Yes	Yes
Spatial lag of output	No	Yes	Yes
Spatial lag of errors	No	No	Yes
All covariates = 0	χ^2 = 65.16***	χ^2 = 90.99***	χ^2 = 312.79***
Spatial effects = 0	n.a.	χ^2 = 13.16***	χ^2 = 23.98***

Note: Sample period is 1979 - 1984, for 25 provinces (omitting Beijing, Tianjin, Shanghai and Tibet). Data on HRS uptake by province and year are from Bai and Kung (2014). Grain output is from Table 26 of Colby *et al*. (1992). Standard errors in ( ), with statistical significance at the 1%, 5%, and 10% level denoted by
***, **, *. HRS, household responsibility system.

only own-province grain output but also the grain output in neighbouring provinces (if $\beta_2 \neq 0$).

The most general model is a spatial autoregressive model with spatial autoregressive errors (SARAR), where everything in this model is spatially lagged.[6]

$$GR_{it} = \lambda WGR_{it} + \beta_1 HRS_{it} + \beta_2 WHRS_{it} + \mu_i + \vartheta_t + \rho Wu_{it} + e_{it}. \quad (3)$$

The additional term ρWu_{it} allows for spatial autocorrelation, where the errors for a province are correlated (ρ) with a weighted average of the errors in surrounding provinces within that year.

Equation (3) nests the spatial Durbin model if $\rho = 0$, the spatial autoregressive model (*aka* the spatial lag model) if $\beta_2 = \rho = 0$, where only the dependent variable is spatially lagged, the spatial error model, where only the errors are spatially lagged (if $\lambda = \beta_2 = 0$), and aspatial models, such as the fixed-effects panel estimator without any spatial lags (if $\lambda = \beta_2 = \rho = 0$). The SARAR model should give unbiased coefficient estimates even if the true data-generation process is one of the nested models, but the reverse is not true given that it involves omitting relevant variables. Models with spatial lags of either dependent variables or independent variables have the property that the total effect of changes in an independent variable such as an increase in the HRS adoption rate may be quite different to what is shown by the regression coefficient. For example, $\widehat{\beta}_1$ may not capture the total effect of a change in HRS adoption if a higher rate of HRS adoption in one province

[6] This is estimated by quasi-maximum likelihood (Lee and Yu 2010), using the Drukker *et al*. (2013) estimator.

affects grain output of nearby provinces or if grain output of one province affects grain output of adjacent provinces.

These spillover and feedback effects let us decompose effects of HRS adoption on grain output into direct and indirect components. To see how, note first that Equation (3) in matrix notation (for simplicity, the subscripts are dropped and the fixed effects and error terms are combined in v because the errors do not affect this decomposition) is as follows:

$$GR = (I - \lambda W)^{-1}(HRS\beta_1 + WHRS\beta_2) + (I - \lambda W)^{-1}v. \tag{4}$$

Following Elhorst (2012), the partial derivatives w.r.t. the k'th element of the explanatory variable vector can then be written as (noting that the diagonal elements of W are zero):

$$\frac{\partial GR}{\partial HRS_k} = (I - \lambda W)^{-1}(\beta_{1k}I_N + \beta_{2k}W), \tag{5}$$

where HRS_k is the HRS adoption rate in province k and β_{1k} is the k$^{\text{th}}$ element of the vector β_1 and similarly for β_{2k}. The total marginal effect of HRS_k on grain output has both direct and indirect effects, which vary over space as spillovers get weaker the further away one looks. The spatial panel estimator that we use follows LeSage and Pace (2009) in reporting a single direct effect that averages the diagonal elements of the matrix in Equation (5) and a single indirect effect that averages the row sums of the nondiagonal elements of that matrix. Indirect effects arise not just from a region's neighbours, when $\beta_{2k} \neq 0$, but also from (potentially) all areas through the spatial autoregressive effect when $\lambda \neq 0$. Thus, there can be both local and global spillovers and when these are accounted for the derivative, $\partial GR/\partial HRS_k$ may be quite different to the direct impact effect, $\widehat{\beta}_1$.

The results of the spatial Durbin model (Equation (2)) are reported in the second column of Table 1. The two spatial lags are both statistically significant ($P < 0.01$), with $\widehat{\beta}_2 = 0.04$ and $\widehat{\lambda} = -0.08$. Thus, grain output was slightly higher for a province surrounded by provinces with higher HRS adoption rates and slightly lower if grain output of neighbours was higher, all else the same. The first effect may reflect learning about the reforms from adjacent areas, while the second may reflect two aspects of an economy that was still largely centrally planned; possible competition for inputs coming from unreformed sectors, such as chemical fertiliser, and if neighbours produce more grain and the state has an overall output target then less may need to be produced by the i^{th} province. The indirect effects due to these spillovers (estimated using Equation (5) and averaging over all cross-province combinations) are approximately one-third as large as the direct effects, but are not precisely estimated. The total effect, which combines direct and indirect effects, is precisely estimated and is 31 per cent higher than result in column 1 of Table 1, which ignores spillovers. Specifically, moving from zero to 100 per cent HRS would raise overall grain output 34 per cent once account is also taken of the effects beyond the own province.

Even larger effects, of a 53 per cent increase in grain output if moving from zero to 100 per cent HRS, show up when spatial lags in the regression errors are also allowed for, using the SARAR model reported in column 3 of Table 1. The error lags are statistically significant ($P < 0.01$) and once accounted for, make the average direct, indirect, and total impacts slightly larger and more precisely estimated than in the spatial Durbin model in the second column. Notably, almost one-third of the impact of the HRS is the indirect impact, which is due to spillovers between provinces, and the literature to date has ignored these effects. The 53 per cent increase in grain output in column 3, compared to the 26 per cent increase in column 1, fits with the synthetic control results in Section 3. With per capita calories rising 15 per cent in the synthetic control results, while population rose 30 per cent, aggregate calories in China, which are mainly from grain, should have risen by at least 45 per cent.

5. Distributional impacts

As well as aggregate effects of the HRS in increasing food supply, there are important and ongoing distributional consequences of this reform. In the initial reform period, the adoption of rural responsibility systems was officially supported in poorer, and more remote, areas. This targeting enabled relatively poor early adopters, like Anhui, to close more of the gap with what was (at the time) the income frontier for China, which was the level of per capita income in the province-level municipalities (Beijing, Tianjin, and Shanghai), than was closed by later adopters like Heilongjiang. This effect is shown in Figure 4, where the position of both of these provinces relative to the frontier is set at an index value of 1 in 1978 (noting that Heilongjiang had higher per capita income than Anhui). Over the next 6 years, Anhui increased its position relative to that frontier by 43 per cent, while Heilongjiang increased its position by only 13 per cent (Figure 4). Thus, the spatial targeting of the HRS helped to close some income gaps in the initial years.

The larger, and still ongoing, effect of the rural reforms is that overall interprovincial inequality (in terms of GDP per capita) declined sharply and is still lower, 40 years after the reforms began, than it was in 1978. This is shown in Figure 5, which gives a time series for the Theil index of inequality, using province-level data on GDP per resident.[7] The rural reforms triggered a

[7] Many studies in the literature on China use GDP per registered population, which was commonly reported in statistical yearbooks in the past. This is a misleading measure, as shown in Li and Gibson (2013) because it would mechanically increase with each additional migrant from the interior to the coastal provinces because the migrant contributed to GDP in the destination but was still included in the population denominator of the origin. Indeed, the freeing up of rural labour to seek better opportunities in cities is one of the lasting contributions of the HRS.

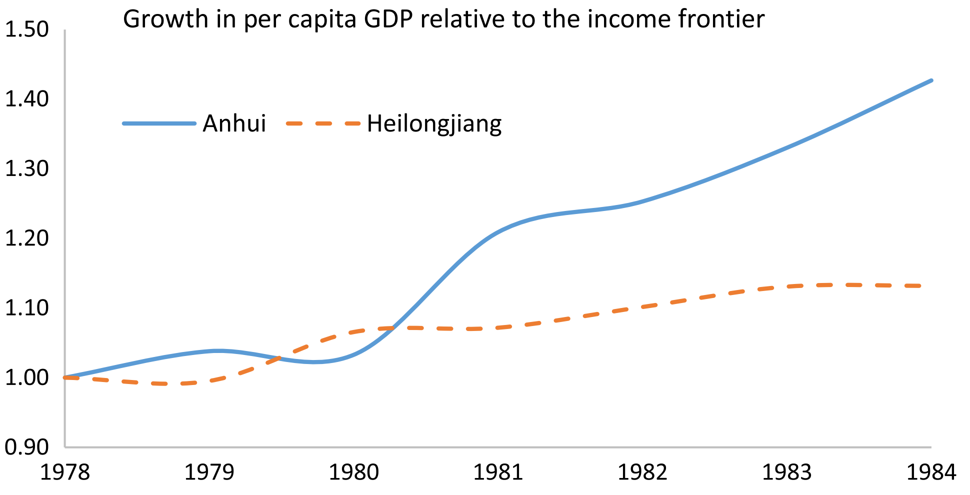

Figure 4 Faster catch-up by poor provinces that were earlier adopters of the household responsibility system. [Colour figure can be viewed at wileyonlinelibrary.com]

12-year decline in interprovincial inequality. For example, the Theil index fell from 0.16 in 1978 to just over 0.08 in 1990. One-third of the initial fall in the Theil index was reversed over the 1990 -1993 period; for the next 11 years, the trend in interprovincial inequality was fairly flat, followed by a 1-year rise in 2005 and then a sharp fall until 2014. Over the full 38-year period shown in Figure 5, the Theil index fell from 0.16 to 0.07, with a statistically significant trend annual rate of decline of 1.1 per cent. Thus, the HRS not only had a

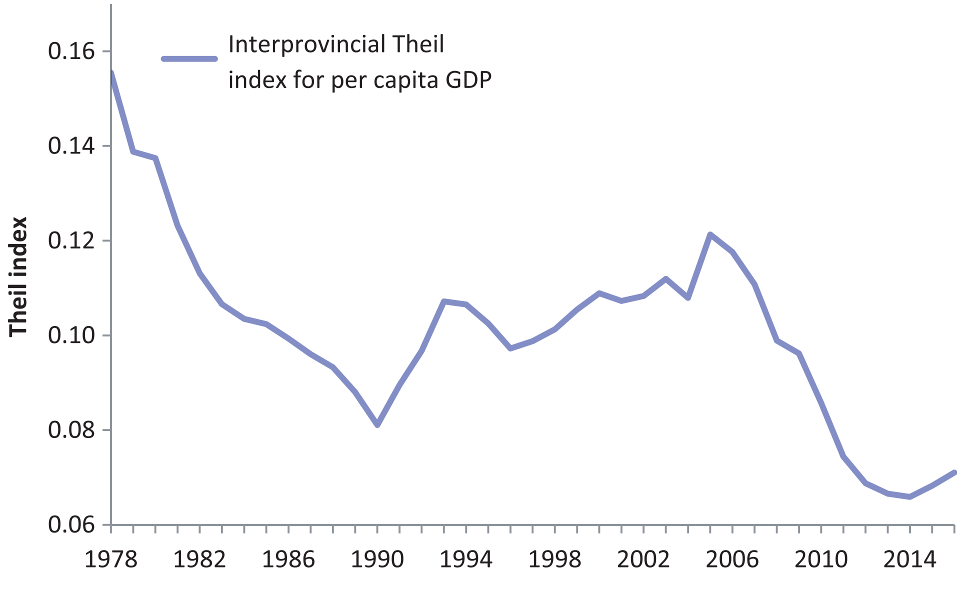

Figure 5 Interprovincial inequality fell after household responsibility system was adopted. [Colour figure can be viewed at wileyonlinelibrary.com]

major impact in boosting China's food supply, but it also helped reduce regional inequality.

6. Conclusions

The rural reforms that began in 1978 and culminated in the universal adoption of the HRS had a significant positive effect on grain output and food availability in China, which can be shown using modern quasi-experimental research methods. The reforms were also pro-poor, in allowing poorer rural areas to close some of the income gap with urban areas. In other words, this reform had positive effects on both efficiency and equity.

In terms of lessons for designing and analysing reforms, allowing pioneers that either by intent, or through commitment of key local leaders, or even by serendipity, progress more rapidly with reforms than do others can be helpful. In addition to spillover effects, such as demonstrating how to reform, there may also be complementarities where rising output and incomes due to the reforms increase the payoff to further reforms, in the initial areas and elsewhere. At the analysis stage, taking account of the possibility of such spillovers, rather than assuming that each observation is independent of others, may help to provide a more complete understanding of the long-term effects of reforms.

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