The Impact of Hukou Reform on the Rural and Urban Income Gap

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Abstract:

In 1999 and 2001, the Chinese Central Government implemented a reform of the residency restriction system, or *hukou*, in small cities and towns that removed the limitation on labor migration and deregulated the labor market. Although this policy aims to bridge the income gap between rural and urban citizens, there is no systematic and quantitative analysis of the real effect of this policy. Based on an empirical study of small cities and towns in 377 counties of five provinces for seven years and using a Two-Way Fixed Effects estimation model, I found that the labor market deregulation actually caused the income gap to widen, instead of decreasing the gap. The rural-urban income ratio gap actually increased by 0.015 after the implementation of this reform. The reason for this counterintuitive result might be that government takes advantage of *hukou* reform as a chance to give more power and rents to officials. Finally, this suggests that attempts to deregulate the labor market and promote the migration of rural peoples may end by hurting the rural sector.

KEY WORDS: Hukou Reform, Fixed Effect Estimator, Inequality between Urban-Rural Areas

I. Introduction:

Since 1958, the Chinese central government issued a population control policy using a household registration (*hukou*) system. Unlike population registration systems in many other countries, the Chinese system was designed not merely to provide population statistics and identify personal status, but also directly to regulate population distribution and serve many other important objectives desired by the state (Chan & Zhang, 1999). In fact, the *hukou* system functions as a powerful tool of public administration and social control. Under this system, around 800 million rural residents are treated as second-class citizens deprived of the right to settle in cities and to most of the basic welfare programs and government-provided services enjoyed by urban residents. These benefits range from small perks like being able to buy a city bus pass, to much more important matters such as enrolling their children in public schools in cities where their parents work (Solinger, 1999). The *hukou* system created a system of "cities with invisible walls"[1], making it a major source of injustice and inequality (Yu, 2002, pp.56-57) and perhaps the most crucial foundation of China's social and spatial stratification (Li, 2005).

II. Reform of Hukou System

From the end of 1980s, some local governments began to adopt a more flexible *hukou* system due to the increase of population movement between the countryside and cities. In 1988, local governments of Laian County and Quanjiao County of Anhui Province initiated the practice of selling urban *hukou* by charging peasants a fee to change their household registration from agricultural to nonagricultural status. By 1992, almost all provinces had launched similar schemes, with the price varying from a few thousand to several tens of thousand yuan[2] (Shi, 1994:75). Unlike the regular *hukou*, these are not administered by the central

[1] Peter Alexander and Anita Chan, ''Does China have an apartheid pass system?'' *Journal of Ethnic and Migration Studies*, Vol. 30, No. 4 (2004), pp. 609–29; Tim Luard, ''China rethinks peasant apartheid','' *BBC News*, 10 November 2005, http://news.bbc.co.uk/2/hi/asia-pacific/4424944.stm, accessed 3 April 2006; Kam Wing Chan, *Cities with Invisible Walls: Reinterpreting Urbanization in Post-1949 China* (Hong Kong: Oxford University Press 1994).

[2] Between 1990 and 1994, local government sold about 3 million urban hukou at an average price of 8300 yuan a piece (Chan and Zhang, 1999).

government; instead, the design and implementation are up to local governments as an important tool for them to accumulate fiscal revenue.

2.1 Hukou reform in large and medium size cities: Since 2000, a few large cities began to release their *hukou* control system, but the extent and specifics of *hukou* reform vary greatly. In general, the larger the city, the more difficult it is to obtain a local urban *hukou.* A number of large and medium size cities such as Zhuhai, Nanjing and Xi'an have indeed relaxed their criteria for granting *hukou* (Cai 2002: 227). Shijiazhuang in Hebei province is the first provincial level city to remove *hukou* restrictions and grant 450,000 new *hukou* between August 2001 and June 2003 (Wang 2003). Yet, in most large cities, *hukou* reform is minimal; only an extremely small minority of rural migrants who satisfy stringent criteria such as educational attainment (with at least bachelor degree) and financial ability (occupying housing of at least 100 square meters housing in these cities) are awarded local *hukou* and given access to urban benefits (Cai 2003: 210-211)[3]. In short, *hukou* reform has not been widespread or completely liberalized.

2.2 Hukou Reform in Small Cities and Towns: The wider and real *hukou* reform was carried out at in over 20,000 county level cities and towns and it has been characterized as "requiring minimum conditions and complete opening-up" (Cai & Wang, 2009: 246). In 1997, the State Council approved a pilot scheme to grant urban *hukou* to rural migrants who had a stable urban job and who had resided in selected 382 towns and small cities for more than two years (Yu 2002: 379). Unlike earlier practices, qualified migrants were not required to pay a hefty sum (Yu 2002: 382)[4]. After years of experimentation in some regions, in 2001 the Ministry of Public Security (MPS) expanded further reform in small towns. In most cases, the minimum requirements for obtaining a local *hukou* were a stable source of income and a fixed place of residence in such small cities. This was considered the most significant step in the *hukou* reform since the system was put into place in 1958 (Cai & Wang 2009: 246). In 2003, the State Council issued a directive affirming the rights of rural migrants to work in cities (Cai 2003: 212). Even if there still are

[3]Even in Shijiazhuang, the reform was suspended in 2003 due to the underdeveloped social service system in the cities, which could not satisfy the huge flow of rural migrants.

[4]Since then, the principal criteria for obtaining *hukou* in small cities and towns have been a fixed and legal residence and a stable source of income (Cai 2003: 210).

some requirements for immigrants, the criteria are rather low. For example, there is no requirement on the size of housing in square meters or level of education. Another very important feature involving in this *hukou* reform is that county level cities only encourage proximal migration, that is, the farmers can only get urban *hukou* in their original county[5]. The policy is called "Jiudiqianyi," which indicates migration within the same administrative unit (Huang & Wang: 17). This policy is also carried out in tandem with the state encouragement of the development of Town and Village Enterprises (TVEs). Farmers are encouraged to work in nearby small towns where emerging TVEs were seeking labor (Cai&Wang, 2009: 246). Another reason for the catchment effect is that the attraction of such small cities and towns is rather low and limited to farmers from other areas.

III. Empirical Analysis.

Literature Review: Most of the research about *hukou* reform is qualitative analysis and case studies that argue the merits and problems involved in this reform. Only several papers provide us with quantitative analysis of how the reform will influence the inequality of rural and urban areas in China. Most papers predict favorable effects of rural-urban migration on income inequality. For example, Zhong Xiaohan uses the Gini-coefficient to analyze this problem, arguing that migration often, but not always, reduces income inequality, and even tends to increase it at early stages of migration. Thomas Hertela and Fan Zhai (2005) used a household-disaggregated, recursive dynamic computable general equilibrium (CGE) model to analyze *hukou* reforms on rural–urban inequality and income distribution of China. The simulation results show that the reforms in *hukou* system would reduce the urban–rural income ratio dramatically. But rigorous statistical analysis is even rarer in investigating the *hukou* reform in small cities and towns. Based on an empirical study of *hukou* reform in Zhejing Province, Huang Yi and Wang Gewei (2003) attempt to discuss the labor market deregulation and the migration consequences on the rural population. Using a general and an adjusted difference-in-difference (DID) estimator and probit

[5]In general, the administrative divisions of China have consisted of six levels (Central—Province—Prefecture ---Counties—Township---Village). One county in China always includes several towns and several dozen of villages. The county level *hukou* reform as a result means the farmers of nearby villages (under the administrative control of the county) can get the urban *hukou* of county level cities or towns.

models, they show that peasants did not swarm into cities and the labor market deregulation reforms have no significant effects on rural income and macro-economy of reform town. But this paper is only based on 50 towns and villages' reform in the middle of 1990s. It uses a rather small sample and all of the outcomes are statistically insignificant.

Data: The data I collected includes five provinces: Henan, Jiangsu, Zhejiang, Fujian and Shangdong for seven years from 1999 to 2005[6]. The essential dependent variables, urban and rural income, were collected manually from each province's statistical yearbooks. All of the other variables are all sourced from China Data Online, including GDP per capita, fiscal revenue and expenditures, value added of primary and secondary industries, export value and investment of foreign capital.[7] Overall the sample covers 377 counties in five provinces. 113 counties began implementing *hukou* reform in 2000, primarily in Henan province; 121 started in 2001, concentrating in Fujian and Zhejiang provinces; 56 counties in Jiangsu began implementation in 2003 and the remaining 88 counties in Shandong Province began from 2005[8]. The dependent variable, the disparity between growth rate of farmers and urban residents' net income, was calculated from annual farmers' net income per capita and annual disposable income of urban residents from 1999 to 2005. The key independent variable, *hukou* reform, was treated as a dummy variable, with counties that had adopted the reform coded as a one.

Econometric Models:

Two-way fixed effects were applied as my baseline model. Under a strict edogeneity assumption on the explanatory variables, the fixed effects estimator is unbiased: roughly, the idiosyncratic error u_{it} should be uncorrelated with each explanatory variable across all time periods. The fixed effects estimator allows for arbitrary correlation between a_i and the explanatory variables in any time period. By taking first

[6] Fujian and Zhejiang province began at the beginning at 2001; Henan started at the beginning of 2000; Jiangsu started from 2003 and finally Shandong got the treatment since October 2004, which I will code it beginning from 2005.

[7] See Appendix A for a detailed description of major variables.

[8] The urban income of Shandong province is rather incomplete, among the 88 counties, only around 8 counties have this income data.

differences, any explanatory variable that is constant over time for all units get swept away by the fixed effects transformation (Wooldridge 2005). I have also utilized a random effects model, of which the outcome is more efficient, but it should only be applied under the assumption that there is no autocorrelation between explanatory variables and unobservable county or province variation. To decide whether I should take advantage of a Fixed Effects or a Random Effects model, I conducted Hausman tests on all of my models at county level[9]. The p value indicates a systematic difference in coefficients, which suggests I should use Fixed Effects.[10]

First, I used Fixed Effects on county level to estimate the impact of reform. The model is:

$$\boldsymbol{Ratio_{it}= \beta_0+\beta_1 Reform_{it}+\sum_{1999}^{t} \delta year_t + \alpha_i + u_{it}}$$

Here *i* stands for each county and *t* stands for the variation of time. I dummy out each year as $\sum_{1999}^{t} \delta year_t$ shows.

The problem involved in this model is that policy intervention occurs at the province level, but the data I have collected is county level. To solve the problem of hierarchical data structure, I need to correct the model with Fixed Effects at the province level, which can cluster all the residuals of the same province. This method also can solve the problem that each province has its own unique features and characteristics that are unobservable. Without controlling for these unobservables, the coefficient on the income gap between urban and rural areas may be biased. By clustering the residuals at the province level, I can also remove theses differences.

In addition, China is a country within comparative advantage in the clothing industry but comparative disadvantage in agriculture. In an ideal international trade model, only if the wages of these two industries are equalized, then market equilibrium exists. However, this theory is based on the assumption that labor can migrate freely, which is not satisfied under China's old *hukou* system. If China began to remove the *hukou*, my hypothesis is that cities with higher degree of trade and export will attract the farmers at a

[9] For province level data, cluster is not compatible with Hausman test.
[10] See Appendix B

greater degree and have stronger influence to drive more low skilled labor to flow into the urban areas. If this is the case, then rural and urban wages should be more equalized. In this case, I will add the export value as my control variable. Finally, most of the provinces also underwent agricultural tax reform during this time period, so agricultural reform should also drive the increase in rural income, which may have positive bias to my model and so I will also utilize a dummy for tax reform.

My model with these two control variables should be:

MODELII: Ratio $_{igt} = \beta_0 + \beta_1 Reform_{igt} + \sum_{1999}^{t} \delta year + \beta_2 Lgexpvalt_{ig} + \beta_3 taxreform + + \alpha_i + u_{it}$ [11]

If we use Random Effects or Pooled OLS methods, we must assume that there is no correlation between the income gap and unobservable provincial variation. Appendix B presents the results of Hausman Tests for my entire county levels model. As the test of the model with just year dummies and the model with year dummies and control variables indicates that we should reject the null hypothesis, we can safely claim that fixed effects—rather than random effects model—is the preferred model.

IV. Regression Results

County Level: The coefficient of rural-urban income ratio in my baseline model (Fixed Effects model on reform dummy without any control) is -0.068. This result indicates that the reform increased income gap and that the ratio will go down by 0.068 units. The coefficient in my model with year dummies decreases to -0.013, which implies that there is a trend wherein the income gap grows, but the reform still accounts for part of the widening of the gap. My final model with control variables *lgexpval* (log of export value) and *taxreform* shows that the income gap will still become wider at -0.015 units. All results are significant at 99% level. In addition, with all else equal, the coefficient of *lgexpvalue* is 0.011 and highly significant, signifying that areas with higher export value will narrow down the income gap by 0.011. However, the *taxreform* variable is not significant, although it is positive.

[11] In this equation, *g* stands for province level variation. *g* is province level Fixed Effects and u_{igt} stands for errors that vary across time. Taxreform is a dummy variable that changed during this *hukou* reform and exportvalue is a continuous variable.

Province Level: First of all, after clustering standard errors at province level, the baseline model does not change and the coefficient is still -0.068. Secondly, the result shows that all else equal, counties that have high levels of export will drive the income gap down. That is the percentage of farmers' income of urban people's will be 0.012 units higher if the export value increase 1%. However, no evidence demonstrates that farmers that benefit from tax reform will experience faster income growth than urban areas. From 1999 to 2005, all of the provinces under study undertook the tax reduction policy to increase farmers' income. Although the coefficient is positive at 0.005, the result is not statistically significant. In fact, the change of ratio is opposite to my original hypothesis. It is negative which means that the *hukou* reform actually widens the income gap because the percentage of the rural income by urban income goes down. This result begs the question of whether this is due to biases in the regression model or if this is an actuality. To further confirm the result, I need to do a series of robustness checks before I can provide an explanation for this unexpected result.

V. Robustness Checks

1) Endogeneity: The regression above is based on the assumption that *hukou* reform is a "natural experiment," which requires that the selection of the time to treat for every county is random. Although the central government announced that it would carry out this policy from the end of 1997, different provincial governments could choose their own entry times. There may be bias due to the targeting effect. Then the question comes out whether or not the government purposefully chose particular counties to reform first. Does this kind of decisive factor have endogenous factor to the above results? If hukou reform is a "controllable experiment," then the time to entry is decided by some index instead of subjective desire, we can still treat hukou reform as a quasi-experiment (Huang &Wang, 13). To see whether this is the case, I tested whether the time of treatment is related to certain baseline factors, or to the change in income gap before the reform.

Firstly, I ran a cross sectional regression based on the data of year 2000 without Henan province[12] to test what kind of factors decide the time to be treated. The regression result as Appendix E shows that the variable *second_ind, fisrev, n_stu_primary, urpop* and *ratio* are statistically significant at 99% level, while *percgdp, lgexpval andn_industry* are not significant. For example, if the development of the county's secondary industry is more advanced, and then the county will get treated earlier because the incentive behind the *hukou* reform is to attract rural surplus labor to solve the problem of short of labor in secondary industry.[13] The result also shows that counties with a narrower income gap (if the ratio is bigger) will adopt reform earlier. Although it appears that there is targeting effect in this reform, all differences presented in this cross-sectional regression will be eliminated by the two-way Fixed Effects model.

2) Endogenous Timing in a Staged Entry. The next step is to test the identifying assumption of Fixed Effects, which is that the treatment and control groups would change at the same rate in absence of the reform. To further confirm whether the sequence of entry is correlated with counterfactual trend of growth rate, I will generate a new variable: "the change of ratio" before treatment, as I am concerned about the change rate of the ratio instead of the change levels of ratio may bias my conclusion.

Firstly, I will test it using standard regression models. I split up my sample into counties that began enforcing reform in 2003 and those that began reform in 2005. I used data from 2001 and 2002 to test the counties that were treated in 2003. Again, I took advantage of the data from 2003 and 2004 to test the counties that were treated in 2005. The regression results[14] show that coefficient on change rate of ratio is insignificant; this means that entry into the treatment is not correlated with the pre-treatment change in income ratio. The graph in Appendix F once again confirms the regression results that both the disparity of growth rate and the income ratio are rather stable that there are no systematic differences in the rate of

[12] I have not used the data of 1999 because there is no observation for the export value. Henan province began the reform from 2000, so I excluded its data.
[13] Especially after the central government had launched the policy to develop secondary industries in small cities and towns since 2000.
[14] See Appendix F.

the change before the treatment. Consequentially, I do not need to worry about the endogenous timing in the staged entry and can treat the *hukou* reform as a quasi-experiment.

3) Autocorrelation: Another problem that must be considered is autocorrelation in the error term for the models. Autocorrelation, if it exists, would not result in the "wrong" coefficient estimates, but would increase greatly the standard error and generate an incorrect confidence interval. Upon running residuals on their lags, there is evidence for one lag in the error terms[15]. To correct this problem, Newey-West standard errors were applied to account for the autocorrelation[16] and the result is still significant for both *hukou* reform and *lgexpval*. To further deal with this problem, I have run the model on bi-annual data (2001 and 2003), the reform dummy in this model became significant at the 99% level and the marginal effect even became larger (change from -0.015 to -0.034)[17]. The other bi-annual data sample (2000, 2002 and 2004) has the same effect: both expand the marginal effect of *hukou* reform and make the result more significant[18]. This indicates that the real effect of the reform on income gap could have been higher.

4) Tau: The problem that counties are not exogenously assigned to the reform leads me to worry about Ashenfelter's dip[19]. I have created a *tau* variable that collapsed the staggered entry and reconciled counties' first reform year by using leads and lags. I plotted both income ratio and growth rate disparity against this variable. The graphs[20] show that there is no Ashenfelter's dip before the counties' entry into reform. Even more, the two graphs present vivid information about the change of the ratio and disparity pre- and post- reform, which further confirm the regression results above. There is an obvious decline trend of the ratio after the reform in at least five years' period, which implies that the *hukou* reform does drive the income gap wider. The trend in disparity graph over the short run is not clear, but after three

[15] See Appendix H.
[16] See Model 4 in Appendix D.
[17] See Model 1 in Appendix I.
[18] See Model 2 in Appendix I.
[19] Ashenfelter (1978) noted a potentially serious limitation in evaluating government raining programs when he observed that the mean earnings of participants in government training programmesdeclinn in the period prior to program entry. It means that fact that the selection for treatment is influenced by individual-transitory shocks on past outcomes.
[20] See Appendix G.

years' reform, the disparity gives the same result. The regression result[21] provides another perspective to prove that my conclusion from the *tau* graph, which is that the effect of the reform is significant starting from the second year after the county adopted the *hukou* reform.

The regression result of my model and the robustness checks confirm that the removal of *hukou* and the introduction of free labor migration does not narrow the income gap as scholars and government officials originally expected, which is also against my hypothesis. Does this mean that groups of people who have transformed their *hukou* status are not low skilled labor, and instead, they are comparative high skilled with higher education? The best way to answer this question is to examine the individual features of the migrants. However, this data is not yet available. Urbanization (the growth rate of urban population) may provide some explanation for changes in the income gap; however, the growth of *urpop* is not statistically significant[22]. This result indicates that the government may still have in place some barriers for farmers' mobility, such as requiring them to have a fixed job and housing in cities. These barriers are thought to protect the welfare of registered urban residents. Another important reason is that if the farmers want to get an urban *hukou*, they have to sell their lands to the government and submit themselves to the One-Child Policy[23]. Using new household survey data for 1995 and 2002, Sicular and Yue confirmed the hypothesis that it is more educated and high-income farmers that move into the cities.

VI. Conclusion:

Based on an empirical study of *hukou* reform in small cities and towns in 377 counties of five provinces for seven years and using Two-Way Fixed Effects estimator, I found that the labor market deregulation actually caused the income gap between urban and rural citizens to become wider, instead of decreasing the gap. The rural-urban income ratio decreased 0.015 after the implementation of this reform. This result

[21] See Appendix K.
[22] See Appendix J.
[23] Only Fujian Province issued official documents, claiming that farmers can keep their land for three years. In parts of the rural areas, farmers can have two children if the first one is daughter.

was supported by a series of robustness checks and empirical tests. The results imply that the rural and urban income gap actually became bigger after the hukou reform, which is opposite to much qualitative and theoretical analysis. The reasons behind this problem may come from the obstacles set by urban governments due to the fare of burden on social welfare constructions, security and social security. In addition, the government may be taking advantage of *hukou* reform as a chance to give more power and rents to officials to extract fiscal revenue. If government wants to solve the problem of social inequality in the future, they should draw down the threshold of migration under the *hukou* policy.

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Appendix A:
Description of the Main Variables

Variables	Description
Year	Year (1999-2005)
Pid	Province ID (Start from 1 to 5)
Cid	County ID (Start from 1 to 379)
Ratio	Rural Income[1] divided by Urban Income in the same county
Disparity	The disparity of income growth rate between rural and urban areas in the same county
Reform	Binary Dummy, county adopted Hukou reform=1
Fisrev	Local government fiscal revenue (100 million yuan)
Percgdp	Per capital GDP of each county
Primary_ind	Value-added of Primary Industry (100 million yuan)
Second_ind	Value-added of Secondary Industry (100 million yuan)
N_industry	Number of Industrial Enterprises above Designated Size (unit)
Taxreform	Dummy, county adopted Tax reform=1
Lgexpval	Log of the exports value (1000 US dollars)
Invest	Completed Investment in Capital Construction (100 million yuan)
N_stu_primary	Students enrollment in primary schools (10000 persons)
Time	Dummy, the year that county is selected to carry out the hukou reform. (2001=1, 2003=2, 2005=3)

Appendix B: Hausman Test of Model at County Level
1. Hausman Test of Baseline Model

```
                 ---- Coefficients ----
                |      (b)          (B)            (b-B)     sqrt(diag(V_b-V_B))
                |       fe           re         Difference          S.E.
----------------+----------------------------------------------------------------
        reform  |  -.0675206    -.0676202        .0000996        .0001791
----------------------------------------------------------------------------------
                          b = consistent under Ho and Ha; obtained from xtreg
           B = inconsistent under Ha, efficient under Ho; obtained from xtreg

    Test:  Ho:  difference in coefficients not systematic

                  chi2(1) = (b-B)'[(V_b-V_B)^(-1)](b-B)
                          =          0.31
                Prob>chi2 =        0.5782
```

2. Hausman Test of Baseline Model with year dummy

	Coefficients (b) fe	(B) re	(b-B) Difference	sqrt(diag(V_b-V_B)) S.E.
reform	-.012515	-.013972	.0014569	.
y2000	-.0239442	-.02343	-.0005141	.
y2001	-.0456472	-.0447928	-.0008544	.
y2002	-.0630279	-.0615211	-.0015068	.
y2003	-.0860415	-.0842051	-.0018364	.
y2004	-.0810566	-.0790634	-.0019932	.
y2005	-.0846585	-.0826795	-.001979	.

b = consistent under Ho and Ha; obtained from xtreg
B = inconsistent under Ha, efficient under Ho; obtained from xtreg

Test: Ho: difference in coefficients not systematic

chi2(7) = (b-B)'[(V_b-V_B)^(-1)](b-B)
= -22.85 chi2<0 ==> model fitted on these data fails to meet the asymptotic assumptions of the Hausman test; see suest for a generalized test

3. Hausman Test of Model with control variable

	Coefficients (b) fe	(B) re	(b-B) Difference	sqrt(diag(V_b-V_B)) S.E.
reform	-.0164716	-.0153481	-.0011235	.
taxreform	.0018717	.0094598	-.0075881	.
expval	-.006884	-.0046148	-.0022693	.
y2000	.0607359	.0686719	-.007936	.
y2001	.0403456	.0453225	-.0049769	.
y2002	.022594	.0289946	-.0064006	.
y2003	.0045306	.007954	-.0034235	.
y2004	.0058706	.0111497	-.0052791	.

b = consistent under Ho and Ha; obtained from xtreg
B = inconsistent under Ha, efficient under Ho; obtained from xtreg

Test: Ho: difference in coefficients not systematic

chi2(8) = (b-B)'[(V_b-V_B)^(-1)](b-B)
= 504.97
Prob>chi2 = 0.0000
(V_b-V_B is not positive definite)

Appendix C:

County Level Regression Results (Time dummies not reported):

Column1	Model1	Model2	Model3	Model4	Model5
VARIABLES	Ratio	Ratio	Ratio	Ratio	Ratio
reform	-0.068***	-0.013***	-0.014***	-0.016***	-0.015***
	-0.002	-0.003	-0.003	-0.004	(0.004)
taxreform				0.002	0.008
				-0.005	(0.005)
lgexpval				-0.007***	0.011***
				-0.002	(0.001)
Constant	0.416***	0.440***	0.442***	0.368***	0.229***
	-0.001	-0.002	-0.005	-0.008	(0.016)
R-squared	0.476	0.593		0.572	
Number of cid	287	287	287	264	264
Rmse	0.0359	0.0317	0.032	0.0296	0.0308
*** p<0.01, ** p<0.05, * p<0.1					

Note:
Model1: Only the reform as an independent variable by Fixed Effect Model.
Model2: Add year dummy as controls using Fixed Effect model.
Model3: Add year dummy as controls using Random Effect model.
Model4: Add year dummy, taxreform and lgexpvalas controls using Fixed Effect model.
Model5: Add year dummy,taxreform and lgexpvalas controls using Random Effect model.

Appendix D: Province Level (Time dummies not reported)

	Model1	Model2	Model3	Model4
VARIABLES	**Ratio**	**Ratio**	**Ratio**	**Ratio**
reform	-0.068***	-0.010	-0.015*	-0.015*
	(0.006)	(0.008)	(0.006)	(0.012)
taxreform			0.005	0.005
			(0.010)	(0.007)
lgexpval			0.012**	0.012**
			(0.004)	(0.006)
Observations	1768	1768	936	936
R-squared	0.179	0.226	0.246	
Number of pid	5	5	5	
Rmse	0.0687	0.0668	0.0617	.
*** p<0.01, ** p<0.05, * p<0.1				

Note:
Model1: Only the reform as an independent variable
Model2: Add year dummy as controls using Fixed Effect model.
Model3: Add year dummy, taxreform and lgexpvalas controls using Fixed Effect model.
Model4: Model6: Newey-West standard error and does not report province id dummy

Appendix E:

What kind of factors decide the time of reform

VARIABLES	Time to Treat
percgdp	-0.425
	(0.280)
second_ind	0.024***
	(0.008)
n_industry	0.002
	(0.001)
fisrev	0.294***
	(0.098)
urpop	4.667***
	(1.219)
n_stu_primary	0.078***
	(0.021)
ratio	5.982***
	(1.116)
lgexpval	-0.029
	(0.051)
Constant	-1.400**
	(0.591)
Observations	152
R-squared	0.459
rmse	0.820
*** p<0.01, ** p<0.05, * p<0.1	

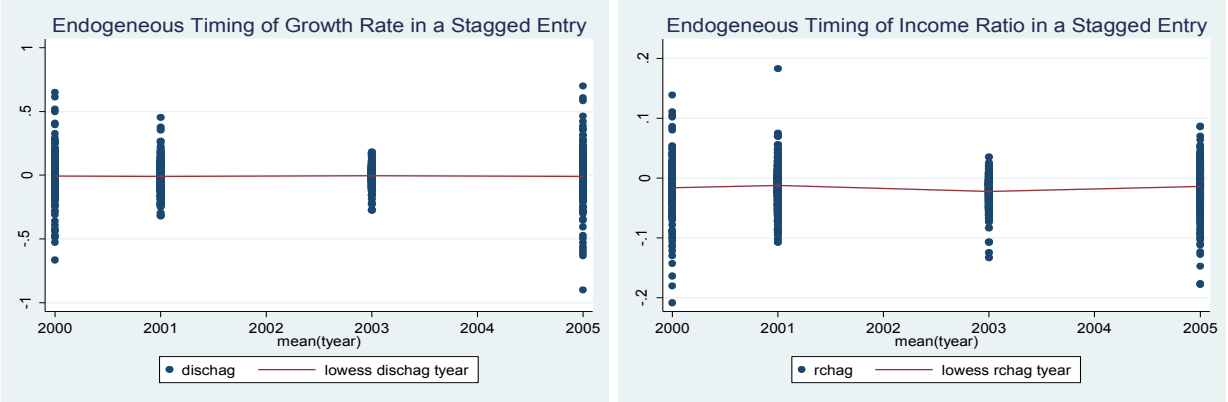
Appendix F:

1. Test for Change in Pre-treatment Period

VARIABLES	Rchag (Model 1)	Rchag (Model 2)
Adopted reform in 2003	-0.002	
	(0.004)	
Adopted reform in 2005		0.001
		(0.003)
lgexpval	0.002***	0.002***
	(0.001)	(0.001)
Constant	-0.047***	-0.039***
	(0.007)	(0.006)
Observations	526	390
R-squared	0.021	0.035
rmse	0.0357	0.0292
*** p<0.01, ** p<0.05, * p<0.1		

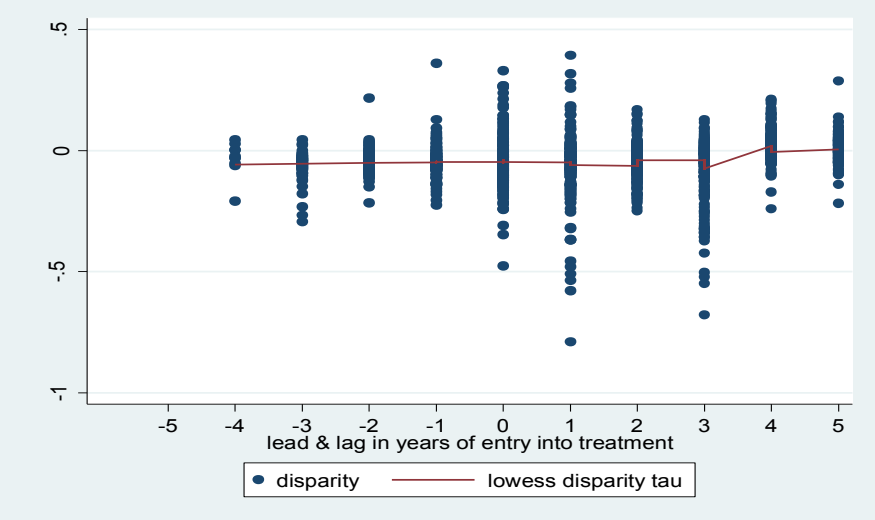
Notes:
Model 1: Using data in year 2001 and 2002 to test whether there are differences in income growth between counties treated/not treated in 2003
Model 2: Using data in year 2003 and 2004 to test whether there are differences in income growth between counties treated/not treated in 2005.

2. Graphs to show change in pre-treatment period

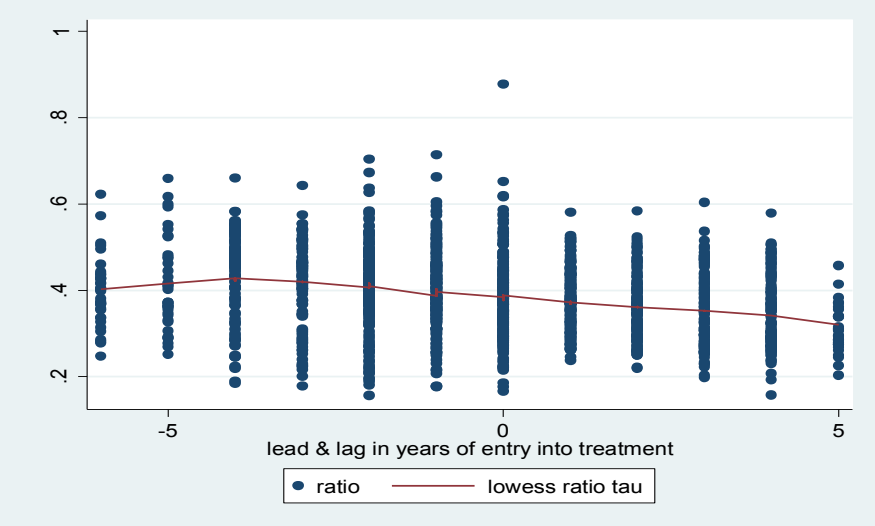


Note: The graph at the left side is the change of income growth disparity; the graph at the right side is the change of income ratio.

Appendix G:Tau variable



Note: This graph shows the disparity between urban rural growth rates



Note: This graph shows the Rural and Urban income ratio

Appendix H: Autocorrelation Tests

Independent Variable	Dependent Variable residual of Income Ratio
L.ehat	1.051***
	(0.101)
L2.ehat	0.076
	(0.131)
L3.ehat	-0.134
	(0.095)
L4.ehat	-0.068
	(0.103)
L5.ehat	0.044
	(0.066)
Constant	0.001
	(0.003)
Observations	76
R-squared	0.955
rmse	0.0179
Standard errors in parentheses	
*** p<0.01, ** p<0.05, * p<0.1	

Appendix I:

Correction of the Autocorrelation with two year interval data

	Model1	Model2
VARIABLES	**Ratio**	**Ratio**
reform	-0.034***	-0.045**
	(0.007)	(0.016)
taxreform	0.010	-0.040**
	(0.005)	(0.012)
lgexpval	0.018	0.010
	(0.012)	(0.007)
Constant	0.371***	0.439***
	(0.007)	(0.013)
Observations	488	387
R-squared	0.148	0.122
Number of pid	5	5
rmse	0.0636	0.0661
Robust standard errors in parentheses		
*** p<0.01, ** p<0.05, * p<0.1		

Note:

Model 1: Using data of 2001 and 2003

Model 2: Using data of 2000, 2002 and 2004

Appendix J: Impact of reform on urban population

VARIABLES	urpop	urpop2
reform	0.001	-0.000
	(0.003)	(0.004)
Constant	0.143***	0.143***
	(0.002)	(0.003)
Observations	2254	2254
R-squared	0.008	0.002
Number of cid	377	
rmse	0.0404	0.0772
Number of pid		5
Standard errors in parentheses		
*** p<0.01, ** p<0.05, * p<0.1		

Appendix K: Long Term Effects of the Reform

VARIABLES	ratio
Reform year	-0.009
	(0.027)
First year after reform	-0.017
	(0.032)
Second year after reform	-0.054**
	(0.019)
Thrid year after reform	-0.048**
	(0.016)
Fourth year after reform	-0.015
	(0.018)
Fifth year after reform	-0.022**
	(0.020)
taxreform	-0.048***
	(0.007)
lgexpval	0.011*
	(0.005)
Constant	0.314***
	(0.047)
Observations	833
R-squared	0.158
Number of pid	5
rmse	0.0639
*** p<0.01, ** p<0.05, * p<0.1	