Pre-Analysis Plan: Not My Money to Touch: Experimental Evidence on Redistributive Preferences under Market Transition in China*

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1 Introduction

1.1 Abstract

China's economic rise is arguably one of the most significant economic events of the past four decades. However, its opening-up and liberalization have also created new inequalities. As China's growth slows and inequality becomes more pronounced, redistributive policies have entered public discussion.

A crucial question arises: how does the average Chinese person view those who have become wealthy over the past 40 years? Do they consider their wealth fair, or do they call for redistributive measures? This project builds on a pilot survey experiment we conducted in 2021, in which we found that priming–via vignettes–getting rich via relatively less meritocratic, yet representative ways under market transition in post-reform China - such as inheritance, housing arbitrage, or house demolition compensation - significantly reduces redistributive support, and in particular support for policies aimed at taking from the rich, while one may have a priori expected the opposite to happen.

To verify the results of the pilot, we replicate the previous survey on a larger representative sample of 2,000 Chinese respondents, replicating the priming treatment on a randomly drawn subsample of 1,000 respondents. To better understand the results, we asked treated and controlled respondents what factors they think are the main sources of wealth in today's society, as well as the sources of wealth of the individuals in the examples.

1.2 Motivation

As rapid economic development led emerging economies like China to reach high inequality levels, understanding how, in those contexts, citizens perceive the current economic inequalities and policies to alleviate them is crucial.

Our pilot showed, that while one may have expected the opposite, in the Chinese context, priming luck (rather than effort or talent) based paths to wealth *decreases* support for redistributive policies, and in particular for policies aimed at taking from the wealthy. A complementary survey we ran showed that Chinese respondents do not perceive these lucky winners in market transition to have acquired their wealth in a particularly unfair way —corruption, structural inequalities, and personal connections are perceived as much more unfair.

Thus, while one may have a priori thought that priming luck-based paths to wealth should *increase* support for redistribution, the opposite may happen if it decreases respondents' perception that contemporary inequalities come from corruption, structural inequalities, or personal connections. If the results of our pilot are confirmed, this will imply that campaigns stressing the luck-based origins of wealth in an attempt to increase popular support for redistribution may backfire in countries where pure luck is perceived as redistributive against a backdrop of high corruption and structural inequalities.

1.3 Research Questions

Does priming the luck-based origins of contemporary economic inequalities in China decrease support for redistribution among the Chinese public? Can this be explained by a decreased perception that contemporary inequalities are the result of entrenched corruption, structural inequalities and personal connections?

2 Research Strategy

2.1 Sampling

2.1.1 Sampling Frame

The sample will be a representative sample of Chinese citizens between 18 and 60 years old. It will include 2000 respondents.

2.1.2 Statistical Power

With the control group and treatment group each at 1000 respondents, 5% alpha, and 80% power, the minimum detectable effect size is 0.125 standard deviations. This is similar to the size of the effect we have previously obtained with the pilot study (between 0.1 and 0.15 standard deviations) and the literature.

2.1.3 Assignment to Treatment

The randomization will be performed on a computer by a survey company (Chinese filiale of Ipsos). Respondents will be assigned to either the treatment group (vignettes) or the control group, and the randomization will be done at the individual level.

2.1.4 Attrition from the Sample

Although to respect informed consent, respondents will be given the possibility to exit the survey at any time, we only expect a minimal attrition. Indeed, respondents will be drawn from the sample of respondents of the survey company with which we are working, so they should be used answering online surveys with relatively large willingness. Additionally, the experiment is a relatively short survey experiment with no follow-up survey and questions that do not require a very large cognitive effort. Furthermore, the survey company with which we are working will ensure that the final sample of respondents meets the sample size requested (2000 respondents, equally divided between the treatment and control groups) and is representative of China's adult population, so they will correct for any attrition by contacting new respondents. They achieve this through a quota system: the research team provides the survey company with a quota based on demographic features such as region, educational level, gender, and ethnicity of the Chinese adult population based on the 7th population census carried out in 2020. The survey company will then recruit a sample that satisfies this quota by inviting their panelists who meet these criteria to participate and decline the participation of people whose quota is already filled. Randomizing the sample into treatment and control groups is done the moment a respondent gains access to the questionnaire, but in our pilot study, the control and treatment groups were well-balanced.

Although the vignettes make the survey slightly longer for treated individuals, the difference in length and required cognitive effort should be minimal so we do not expect differential attrition.

2.2 Fieldwork

2.2.1 Instruments

We will use a single questionnaire which we already used in our pilot study. We changed the original questionnaire by removing certain treatment arms to mitigate multiple testing concerns. We also added, at the end of the questionnaire, questions about the perceived sources of contemporary inequalities and sources of wealth in the vignettes, to gauge whether our treatment affects belief about the sources of inequalities, and how.

2.2.2 Data Collection

The data collection will take two weeks starting from the beginning of questionnaire distribution, which will take place immediately after obtaining IRB approval. It will be performed by the survey company with which we are working. During the data collection process, the data will be stored on the company's servers. Once collection is complete, the company will transmit the data in a de-identified .csv file.

2.2.3 Data Processing

The data comes in a cleaned .csv file with each respondent's answer taking one line and completely anonymized, thus it does not require excessive processing. The data will be stored on secured servers in PSE and Warwick for safekeeping and will be used only for scientific research.

The researcher team's institutions have ownership of the processed data and each member of the research team may use the data for their other research projects. However, the collected data will not be divulged to a person or entity outside of the research team unless every member of the team explicitly consents. Upon publication, the data will not be published for public use but remain available in case of replication demands by the editors and other researches with the same condition of divulgation (unanimous consent in the research team).

3 Empirical Analysis

3.1 Variables

We will be interested in the effect of the treatment (= a dummy taking value 1 if the respondent was presented the vignettes, 0 otherwise) on the extent to which respondents agree with redistributive policies (that could be implemented or have been implemented in history) and on their perception of government duty (primary outcomes) and on the extent to which respondents think that being rich today is attributable to a series of factors (individual ability and effort / pure luck/opportunities present in the specific time period/connection, privilege and inequality of opportunity). In the survey, we will measure responses to those questions with Likert scales. Given the large number of redistributive policies considered, to mitigate multiple testing, we will calculate respondents' average z-score for tax-rich policies, help-poor policies and all policies and use those z-scores as dependent variables. For government duty, we will similarly ask 4 questions on government duty (generally reducing

inequality, uniform standard of admission, providing jobs and whether it is just for the government to regulate wealth and income) and will compile a z-score which we will use as dependent variable. For secondary outcomes, we will use, for transparency, the answer (between 1 to 10) to each question separately.

3.2 Balancing Checks

We will check (through a test of equality of means) if the treated and the control group differ significantly along each of the following dimensions we measure: gender, age, province fixed effects, city size fixed effects, hukou fixed effects, employment status fixed effects, occupation fixed effects, type of work unit fixed effects, education fixed effects, income range fixed effects, family size, membership of the Chinese Communist Party fixed effect, answers to pre-treatment subjective well-being and financial security questions, answers to pre-treatment questions on inequality views (from 1-10).

3.3 Treatment Effects

3.3.1 Intent to Treat

We will regress the outcome variables (specified in the Variables section) on a dummy equal to 1 for assignment to the vignette treatments. For accuracy, we will control for demographic and other variables in a similar way as the pilot study: we test three sets of controls, a "smaller control" that includes only the demographics, "full controls" that includes all the demographics, life satisfaction and opinion variables and using LASSO to select the most relevant set of controls, which is the version currently used in the paper. The three sets of controls should present results of similar magnitudes with only a difference in statistical significance. We will report the LASSO control results in the main body of the paper to keep consistency with the pilot study and test all three sets of controls.

Since the treatment assignment is not clustered, we will not cluster the standard errors but will use robust standard errors.

Hypothesis on Primary Outcomes: Given the previous survey, we expect that the overall preference for redistribution will decrease in the treatment group that has seen the vignette. Particularly, we expect the tax-rich index to decline more than

the help-poor index. In the previous study, we conjectured that the representative vignettes were perceived as fair in the transitional context, and thus seeing them evoked a sense of deservingness among the respondents that led them to be more hesitant in selecting redistributive policies in the following redistributive questions, particularly tax-rich policies.

That being said, given the very abrupt economic slowdown and high youth unemployment rates in China in the past four years, it has been purported that there is an increasing sense of pervasive pessimism across the entire Chinese population, where people increasingly view inequality of opportunities and unfair economic systems (as opposed to effort and ability) as the main causes of being stuck in poverty or getting wealthy in China, especially in the post-2022 period (Rozelle & Whyte, 2024). A priori, if lucky opportunities during the transition are still considered fair, and seeing these examples diminish the perceived importance of structural inequalities of opportunities and corruption in getting rich, they will still exhibit a decrease in support for redistribution. Particularly, the treatment might even yield a larger effect if they attribute economic success principally to unfair inequalities of opportunities now and the treatments represent fair opportunities.

However, against the backdrop of bleak economic prospects and people increasingly interpreting current inequality as the fruit of an unfair system, our respondents might retroactively reinterpret the economic transition as unfair and benefiting those who already have a structural advantage in the past. In this case, they might consider the examples in the vignettes as undeserving now and increase their support for redistribution after seeing them. Such an alternative would form an interesting contrast with the pilot study. (heterogeneity analyses based on different proxies of pessimism are discussed in section 3.4).

Hypothesis on Secondary Outcomes: We hypothesize that the Chinese public regards all the opportunities present in the transitional period, and maybe also pure luck, as fair, despite significant randomness and a lack of extraordinary effort or ability. The only unacceptable forms of inequalities are privilege and structural inequality of opportunities, which dominated the pre-reform period and still play a large role in producing wealth today.

This fairness view is slightly deviant from the standard meritocratic view, where only merit (item (1) effort and ability) is considered just and not the rest (including

item (2) (3) (4)), but a mentality of 'anything but political privilege' is rationalizable given China's historical trajectories.

If representative vignettes are indeed considered examples of period-specific chances, then seeing these examples temporarily should prime respondents to believe that wealth today is due to period-specific opportunities rather than the (unfair) connection and privilege. We hope to see the following result: Compared to the control group, the treatment group attributes the sources of wealth in today's society more to opportunities in the transition period and less to the connection, privilege, and inequality of opportunities.

3.3.2 Treatment on the Treated

Our survey instrument will not allow for verifying that the respondents actually read the vignettes. Therefore, we will only estimate the ITT. However, we expect the ITT to be very close to the TT since treatment take-up (reading rather than skipping the vignettes) is very low effort as the text in the vignettes is very short and that reading it does not demand any large cognitive effort. Since our survey instrument includes post-treatment questions on the sources of wealth in the vignettes, we could however check what share of respondents give highly incoherent answers to those questions and check robustness of the results to dropping those respondents.

3.4 Heterogeneous Effects

The main heterogeneous treatment effect that we are interested in is with respect to subjective economic pressure. In the pilot study, we found a larger treatment effect for respondents with low subjective financial pressure (measured before treatment), that is, a greater decrease in their demand for redistribution. This result is consistent with the fact that such respondents may feel less risk of needing redistribution (hence lower demand for help-poor policies) and better identify with the examples in the vignettes (hence lower support for tax-rich policies). If the result is successfully replicated, we will also attempt to pin down the main components of subjective economic pressure and test whether the heterogeneous effect is driven by one part of economic pressure (e.g. insurance, mobility, or the residual which characterizes some sort of optimistic personality) or the ensemble of these drivers by attempting the same regression with each major determinant of financial pressure.

Ultimately, questions such as subjective financial pressure, security of life, and satisfaction with life also reflect the general level of optimism and pessimism, which is an interesting heterogeneity to study given what has been discussed in section 3.3.1 with regards to the general pessimism of the Chinese public recently. We will try to use two different ways to form a proxy of pessimism: one simple aggregation of the three variables, such as a z-score average or the first principle component, and one "residual" pessimism, which is the residual of the subjective variables after regression on socio-economic characteristics such as gender, property ownership, job, and income. The first would represent the raw pessimism felt by the individual within his or her socio-economic situation, while the second represents an individual's pessimistic personality by comparing them with people of their social class. If the main treatment effect is a decreased demand for redistribution, we expect pessimism to attenuate this effect as in the pilot: more pessimistic respondents perceive themselves as more likely to need redistribution. If the main treatment effect is an increased demand for redistribution (respondents reinterpret the vignettes as unfair so treated respondents demand more redistribution) however, the direction of the heterogeneous effect might be ambiguous: on the one hand, more pessimistic individuals might demand more redistribution, deeming themselves more wronged by the system; on the other hand, pessimism might discourage them from making any active revendication, thinking that all is lost, leading to an attenuation of the treatment effect among pessimistic individuals.

Additionally, if persuasion drives our results, we might obtain larger effects for respondents who, pre-treatment, had more negative views on the state of inequalities. However, this might not be the case, as these respondents may also be more opinionated and therefore have beliefs that are harder to move.

In addition to the heterogeneities that we discovered during the pilot, we will also test the heterogeneous treatment effect of major demographic indicators such as gender, age, region, income and occupation. Given the significantly larger sample size compared to the pilot study, we can employ more nonparametric approaches (such as CATE) to heterogeneity and outline the features of the group that is most affected by the treatment.

3.4.1 Intent to Treat

For our main heterogeneous effect of interest (financial pressure), we will use a classic OLS regression with an interaction term to estimate the size of the heterogeneous treatment effect. More specifically, we will code a dummy that represents below/above median subjective financial pressure and use it as an interaction term in the regression. The controls used in this regression will be the same as in the main regression.

For the more explorative part, we will use a generic machine-learning approach to predict whether there is a significant treatment effect heterogeneity within the data, and if yes, which are the groups most affected by the treatment, as per the method mentioned in Chernozhukov et al. (2018). This method consists of first predicting the size of the treatment effect using any general machine-learning method (random forest, boosting, neural network, etc.), then calculating the aggregated characteristics for samples that are least and most affected by the treatment. For example, we would be able to see whether the respondents in the 1st quantile of treatment effect (the least treated) are overwhelmingly female, young, or have expressed fewer opinions on redistributive preferences in the baselines.

3.4.2 Treatment on the Treated

As before, our survey instrument will only allow us to estimate an Intent to Treat effect.

3.5 Standard Error Adjustments

Since neither the sampling nor the treatment assignment is clustered, we will not cluster the standard errors. However, we will use robust standard errors.

For our primary outcomes, since the question we asked is a series of real-stake redistributive policies, we will use an index to indicate their combined support to avoid multiple hypothesis testing: we will calculate the average Z-scores for all the primary outcomes, as detailed in Section 3.1.

For secondary outcomes (meant to bring evidence on the mechanism), since the number of secondary outcomes is more limited and they capture different things, we will not aggregate those for transparency.

4 Other Tests

In addition to estimating the effect of exposure to the vignettes on the primary and secondary outcomes, we would like to test the following to investigate the mechanism driving the results:

- The respondents (both treated and control) overwhelmingly attribute the examples in the vignettes to period-specific opportunities, instead of ability and effort or privilege.
- A higher attribution of contemporary wealth to fair sources (individual ability and effort, period-specific opportunities, maybe pure luck) is negatively correlated to demand for redistribution, holding other factors (such as demographics) constant.

5 Research Team

The PI is Zhexun Mo. Other investigators include Yuchen Huang, Nora Chen and Margot Belguise. The 4 researchers will equally take part in the analysis of the data and writing.

6 Deliverables

The main product will be an updated manuscript to resubmit to the European Journal of Political Economy. In addition, Margot Belguise will write a short report and present the results in a Warwick Dr@w talk, as part of the requirements of Warwick Behaviour Spotlight funding.

7 Calendar

The sample collection will start upon receiving adequate IRB approval and sending out the initial payment to the survey company, which is estimated to be between the beginning and mid-July of 2024.

The sample collection will take about 2 weeks and the research team expect to receive the raw data document by the beginning of August.

The research team will work on the primary data processing, secure storage, and compile a ready-to-use data set in August and proceed to carry out the main analysis in September 2024. They will proceed to the writings and aim for a resubmission before the end of the year 2024.

8 Budget

All the budget will go to the payment of the survey company, which quoted us CNY 72,000 for a sample of 2,000 respondents. We will cover the costs using funding from PSE's Economic History group (2,000 euros), PSE's Behaviour group (2,000 euros), Warwick Economics Department's discretionary funding (2,000 pounds), a mini-grant from Warwick Applied Microeconomics Research Group (2,000 pounds), funding from Warwick's Behaviour Spotlight (463.67 pounds).

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