DOES SOCIAL ASSISTANCE INCREASE SMOKING AND DRINKING AMONG THE POOR? EVIDENCE FROM THE MINIMUM LIVING SECURITY SYSTEM IN CHINA

A Thesis

Presented to the Faculty of the Graduate School

of Cornell University

in Partial Fulfillment of the Requirements for the Degree of

Master of Science

by

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August 2021

ABSTRACT

This paper examines the effect of one of the world's largest social assistance programs, the Minimum Living Security System (MLSS) in China, on its recipients' smoking and drinking behaviors. In order to address the endogeneity of risky health behaviors and income as well as the issue of selection bias, this paper uses a propensity score matching (PSM) method on the 2012 wave of the China Family Panel Studies Survey (CFPS) to explore the effect. The results indicate that the MLSS receipt decreases the probability of smoking and smokers' consumption on cigarettes while it does not affect smoking intensity. Meanwhile, there is no evidence that the MLSS has an effect on recipients' drinking behaviors.

BIOGRAPHICAL SKETCH

Xiaoyuan Zhang was born and raised in Jiangxi, China. She attended Beijing Institute of Technology in China in 2015 and graduated with a Bachelor's Degree of Management in Accounting and a Bachelor's Degree of Economics in 2019. She is now a Master student in the Dyson School of Applied Economics and Management at Cornell University. Her research interests are health economics, health policy and development economics.

This document is dedicated to all Cornell graduate students.

ACKNOWLEDGEMENTS

First of all, I would like to express my sincere gratitude to my advisor, Prof. Calum Turvey, for his continuous support for my research and Ph.D. application. Prof. Turvey had provided me with the valuable opportunity to design and run a field experiment in China. Although the study was called off due to the COVID-19 pandemic in the last minute, I have learned a lot about the methodology and the process of running experimental studies. My sincere thanks also go to my minor advisor, Prof. Nicolas Ziebarth, for his support and encouragement. I attended Prof. Ziebarth's course during the first year of my Master's study, which further inspired me to conduct research in the field of health economics. I'm also grateful for Prof. John Cawley and Prof. Hongmei Yi, whose perspectives and thoughtful advice were vital in guiding me revise and improve this study. Last but not least, I would also like to thank my parents from the bottom of my heart. Without their unconditional support, I would not have been able to make it through the intense two years of study and life.

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CHAPTER 1

INTRODUCTION

Under the objective of poverty reduction, social assistance is widely employed as a key component of the social security system in both developed and developing countries. Researchers have found that social assistance leads to a rise in household consumption among recipients, especially in food consumption (Barrientos, 2013). The Minimum Living Security System (MLSS, colloquially, *Dibao* in Chinese)[1] is the largest social assistance program in China as well as one of the largest social assistance programs across the world (Ivaschenko *et al.*, 2018). In 2019, the MLSS reached 24 million households and 43 million individuals at a total cost of 165 billion RMB yuan (25 billion US dollars)[2]. Following the market-oriented economic reform in the 1980s, the traditional work unit-based social security system in China exposed its inadequacy and inefficiency to manage the increasing unemployment and impoverishment (Leung, 2003), urging the Chinese government to construct a more effective social security system to ensure social stability. A new multilevel framework was introduced, comprising social insurance, social assistance, social welfare and charity. As the core of social assistance, the MLSS was first established in Shanghai in 1993 as a pilot policy, then largely expanded to the urban area nationwide starting from 1997 and to the rural area in the 2000s. Aiming to provide non-conditional cash transfer to help impoverished populations purchase necessities, such as food, clothing and shelter, it is regarded as the final social safety net to ensure basic livelihood in China. The MLSS receipt has been found to be associated with

[1]The Minimum Living Security System is also called the Minimum Living Allowance (MLA), the Minimum Living Standard Scheme (MLSS), the Minimum Living Standard Guarantee Scheme (MLSGS) or the Minimum Standard of Living Scheme (MSLS) in other papers.

[2]The figures come from the 2019 Statistical Report by the Ministry of Civil Affairs.

increased health, education and food consumption and decreased clothing and leisure consumption possibly because the recipients try to avoid public attention (Gao *et al.*, 2014; Yi *et al.*, 2019). Over the years, the wide coverage of the MLSS contributes to the steady decline of poverty in China (OECD, 2017).

In terms of implementation, the MLSS is administered at the county government level. While the central government issues guidelines for the program, funding of the MLSS comes from preferential-level and county-level governments. County governments are responsible for setting up the minimum living standard threshold, selecting recipients, and delivering allowances to the recipient household's bank account by month. The amount of benefits received by a household equals the difference between the total benefits eligible (the local MLSS threshold × number of household members) and the total household income. Determination of the threshold is based on local cost of living and financial capability of local governments. Therefore, the local administration leads the standard threshold and benefit value to vary considerably across regions. According to the statistics reported by the Ministry of Civil Affairs, in the year of 2019, the average per-capita annual income threshold is 7,404 RMB yuan (about 1,150 US dollars) for urban residents and 5,842 RMB yuan (about 900 US dollars) for rural residents. Figure 1.1 illustrates the provincial average threshold by urbanicity in the second season of 2012 (the year when the data used in this paper were collected). As is shown by the figure, provinces in the eastern area, such as Shanghai, Beijing, and Tianjin have much higher thresholds than some western provinces, such as Xinjiang, Qinghai, and Ningxia. The significant variation implies unbalanced economic development across different

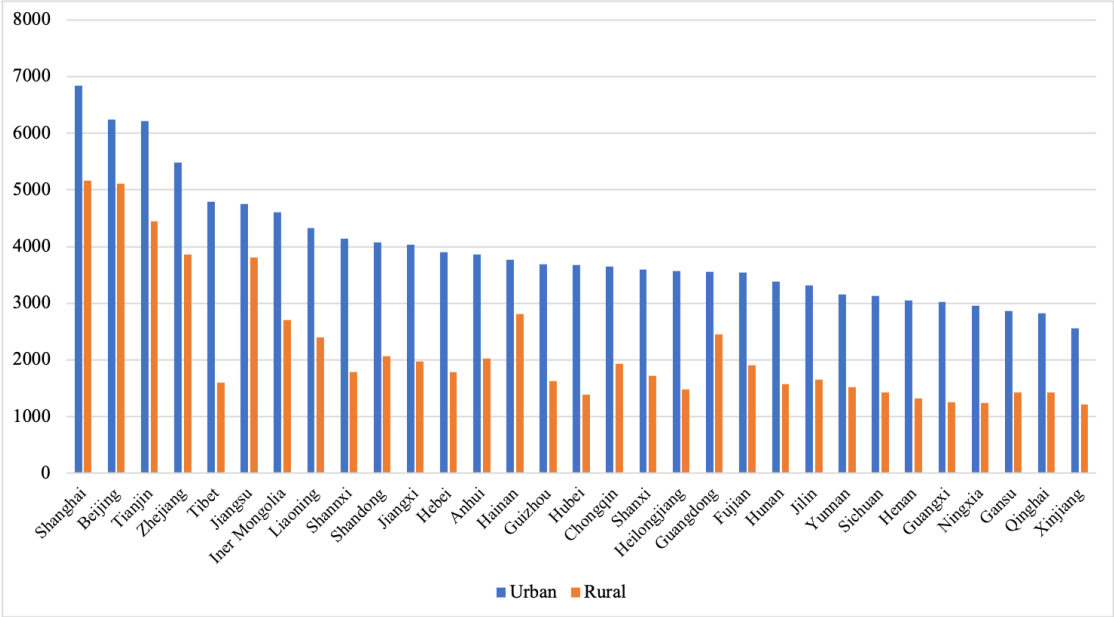

Figure 1.1: Provincial Average MLSS Standard Threshold of Annual Per-capita Income, by Urbanicity

regions in China.

Following the establishment and expansion of the MLSS and its central place in China's social security system, an emerging literature has examined the effects of the MLSS on poverty (Wu and Ramesh, 2014; Golan *et al.*, 2015; Zhao *et al.*, 2017; Golan *et al.*, 2017; Westmore, 2018). Despite the growing body of literature on the impact evaluation of the MLSS, scarce literature has explored its impact on health outcomes (Qi and Wu, 2018). A knowledge gap falls on the impact of the program on health behaviors. Government provides the recipients cash transfers to keep living essentials, however, it is important to ask whether they inappropriately spend the extra money on unhealthy products due to the lack of monitoring or stipulated conditions on the cash transfer. Therefore, I seek to illustrate and quantify the potential effects of the MLSS receipt on smoking and drinking behaviors.

Health behaviors could change by the increase in income. Consumption of unhealthy products, such as cigarettes and alcohol, could rise if they are a normal good. However, considering that good health might also be a normal good, the demand for unhealthy products might decline as a result of more investment in the production of good health (Cawley and Ruhm, 2011). This is closely related to the theory of health capital and the demand for good health (Grossman, 1972). Substantial literature has calculated the income elasticity of smoking and drinking. A meta-analysis (Gallet and List, 2003) based on 375 estimates of the income elasticity of cigarettes yields that the mean is 0.42 with a standard deviation of 0.49 and ranges from (-0.80, 3.03). Kenkel *et al.* (2014) suggest that the income elasticity of cigarettes systematically varies across different settings, including time periods, countries and demographic groups. In the developed world such as the United States, cigarettes used to be identified as a normal good but it appears to switch to an inferior good over time due to the influence of anti-smoking campaigns (Cheng and Kenkel, 2010). Among developing countries, previous studies have provided evidence that cigarettes have positive income elasticity and thus is a normal good (Saloojee, 1995; Bobak *et al.*, 2000; Peck, 2011; Chalopka, 2011). A laboratory-based study (Koffarnus *et al.*, 2015) reveals that the demand elasticity of cigarettes is highly related to income level: as in-study income increases, participants' demand elasticity decreases. In terms of alcohol use, evidence from meta-analyses on published estimates also implies that it typically has a positive income elasticity and is a normal good at large (Gallet, 2007; Fogarty, 2010; Wagenaar, 2009; Nelson, 2013). In the setting of the MLSS in China, Yi *et al.* (2019) and Gao *et al.* (2014) used the China Household Income Project (CHIP) data to find no significant effect of the receipt on the tobacco and alcohol consumption.

There is mixing evidence on how exogenous changes in income generated by social assistance influence recipients' demand for smoking and drinking. Kenkel *et al.* (2014) find that the expansion of benefits from the Earned Income Tax Credit (EITC), a program providing a refundable transfer to low income working families through the tax system, increases smoking and decreases smoking cessation. Their results suggest cigarettes are a normal good among the low-income population. In contrast, some studies find the expansion of the EITC reduces the probability of maternal smoking (Strully *et al.*, 2010; Cowan and Teftt, 2012; Averett and Wang, 2013). Such positive protection on health through changes of health behaviors could partly relate to increased work participation. In contrast, Hamad and Rehkopf (2015) and Collin *et al.* (2020) find no impacts of the EITC on smoking. Pega *et al.* (2017) find the New Zealand's Family Tax Credit (FTC) program has no impact on smoking in the long run as well. In the setting of developing countries, a recent study in Indonesia finds that social assistance increases the intensity of tobacco consumption (Dartanto *et al.*, 2021). For alcohol use, estimations on the impact of the EITC show no evidence of the increase in alcohol consumption in the short-term (Rhekopf *et al.*, 2014; Collin *et al.*, 2020). Psychological studies indicate that low-income individuals who experience persistent hardship of basic living necessities tend to use alcohol for anxiety relief (Pearlin and Radabaugh, 1976; Moos *et al.*, 1988). As a type of social support, tangible assistance or material aid is found to provide a buffering effect on the relationship between financial stress and drink to cope (Peirce *et al.*, 1996).

The possible endogeneity of consumption of cigarettes and alcohol to low income has been well documented in the literature (for example: Lantz *et al.*, 1998; Binkley, 2011). Low income individuals are more likely to consume temptation

products when facing a trade-off between pleasure and health. Additionally, observational studies often face the challenge of selection bias given the systematic differences between participants and non-participants (Gao *et al.*, 2010). To address the issue of endogeneity and selection bias in welfare participation, I utilize a *propensity score matching* (PSM) method following some existing studies in the context of the MLSS (Gao *et al.*, 2010, 2014; Yi *et al.*, 2019) to identify comparable non-recipients to the recipients. This paper builds on a growing body of literature that uses the matching strategy on policy analysis (for example: Rosenbaum and Rubin, 1983; Dehejia and Wahba, 2002; Caliendo, 2008; Austin, 2009; Imbens, 2015). The results show that the MLSS receipt decreases the probability of smoking and smokers' consumption on cigarettes while the smokers' smoking intensity is not affected. Meanwhile, there is no evidence that the MLSS has an effect on recipients' drinking behaviors.

This paper adds to the literature on the impact evaluation of the MLSS and provides evidence to inform complete policy decisions on social assistance worldwide. In addition, the paper contributes to the current limited but growing literature on the potential health outcomes of non-health anti-poverty programs. The remainder of this paper is structured as follows. Chapter 2 describes and summarizes the data. Chapter 3 describes the empirical method. Chapter 4 presents and discusses the results. Chapter 5 performs sensitivity analysis to the main results. Finally, Chapter 6 concludes and discusses the policy implications.

CHAPTER 2

DATA

This paper uses the China Family Panel Studies (CFPS) survey data, administered by the Institute of Social Science Survey (ISSS) at Peking University. It is a general purpose survey that collects national representative, longitudinal data at the individual-, household-, and community-levels (Xie and Hu, 2014). Six waves (2010, 2012, 2014, 2016, 2018, 2020) of the survey have been carried out so far. The 2012 wave data is used in this paper, which is the only wave that contains full information required for analysis on the research question, including household enrollment status in the MLSS, individual smoking and drinking behaviors as well as demographic and socioeconomic characteristics. It has a good national representativeness, covering 25 provinces[1] and containing 13,315 households and 35,719 adults. After excluding the observations without household survey data or individual survey data , the sample used in this study consists of 35,458 adults from 13,108 households.

Table 2.1 presents the descriptive statistics of the characteristics for the sample. The observations are weighted to provide nationally representative estimates. *Ex ante* per-capita annual household income and the MLSS receipt are collected from the household survey. The other characteristics, including sex, age, marital status, education years, urbanicity and employment status, are collected from the individual survey. Out of the 35,458 observation units, 3,922 received the MLSS allowance, accounting for around 11% of the full sample. Compared with the full sample, the average *ex ante* per-capita annual household income of the MLSS recipients is almost as half as that of the full sample.

[1]The 2012 CFPS survey excludes Hong Kong, Macao, Taiwan, Xinjiang, Tibet, Inner Mongolia, Ningxia and Hainan.

However, the average *ex ante* per-capita annual household income of the MLSS recipients (7779.034 RMB yuan) is higher than the highest provincial average MLSS threshold in 2012 (6,485 RMB yuan) and the maximum income (513,500 RMB yuan) is far above the thresholds. There are two possible reasons for the discrepancies. First, the household income information is self-reported in the survey instead of being collected from the records of the MLSS application claims that were used by the government staff to determine eligibility. Second, previous evaluations on the MLSS have found targeting errors in the selection of recipients as a result of the highly decentralized nature of the scheme and community consultation in assessing eligibility (OECD, 2017; Leung and Xiao, 2015). Apart from household income, household assets are also taken into account to determine eligibility for the program in practice. Household income and assets holdings are often based on self-reporting and the difficulty to verify that information gives rise to the targeting errors. In addition, corruption and unfair practices have been existing in the administration of social security programs. Therefore, not all households that met the criteria received the allowance and some recipients had income above the threshold. In order to ensure a more representative treatment group of the MLSS recipients, I dropped the observations whose *ex ante* per-capita annual household income are above 10,000 RMB yuan, leaving me with 2,740 recipients in the restricted treatment group. After restricting the subsample, the average *ex ante* per-capita annual household income is 4,388.625 RMB yuan. Compared to the control units, we can see that the restricted MLSS recipients tend to be older. Unsurprisingly, they are also more likely to be a male, unmarried and unemployed, have less education, and live in the rural area.

The smoking and drinking outcomes are investigated in the individual-level

Table 2.1: Descriptive Statistics of the Characteristics

	Mean	Std. Dev.	Min	Max
Full Sample (N=35,458)				
The MLSS recipient (=1)	0.110	0.313	0	1
Ex ante per-capita annual household income (RMB yuan)				
Full sample (N=35,458)	13126.541	20348.406	0.2	1518023
The MLSS recipients (N=3,797)	7779.034	12756.530	60	513500
Sex (male=1)	0.497	0.500	0	1
Age	44.714	16.759	16	99
Marital status (married=1)	0.809	0.393	0	1
Education years	6.901	4.928	0	22
Urbanicity (urban=1)	0.492	0.500	0	1
Employment status (employed=1)	0.979	0.143	0	1
The Restricted Recipients Sample (N=2,740)				
Ex ante per-capita annual household income (RMB yuan)	4388.625	2845.130	60	9875
Sex (male=1)	0.503	0.500	0	1
Age	48.570	18.501	16	93
Marital status (married=1)	0.737	0.441	0	1
Education years	4.685	4.670	0	16
Urbanicity (urban=1)	0.377	0.485	0	1
Employment status (employed=1)	0.964	0.186	0	1
Non-MLSS Recipients Sample (N=31,536)				
Ex ante per-capita annual household income (RMB yuan)	13779.978	20996.512	0.2	1518023
Sex (male=1)	0.495	0.500	0	1
Age	44.388	16.532	16	99
Marital status (married=1)	0.817	0.387	0	1
Education years	7.116	4.906	0	22
Urbanicity (urban=1)	0.504	0.500	0	1
Employment status (employed=1)	0.980	0.138	0	1

survey. Previous research has shown that self-reported smoking and alcohol use are basically reliable to determine these behavior patterns although there are many factors that may have an effect on the validity of self-reports, such as interactions with respondents and context of interview (Midanik, 1982, 1988; Soulakova *et al.*, 2012). In the CFPS survey, for smoking behaviors, interviewees were asked whether they smoked in the past month, the amount of cigarettes they smoked per day, and the cost of cigarettes they smoked the day before the interview. For drinking behaviors, interviewees were asked whether they drank alcohol at least three times a week in the past month, the types of alcohol they drank in the past week, which consist of three categories: hard liquor (*Baijiu*/whiskey/vodka), wine/rice wine and beer, and the amount of each type of alcohol they drank in the past week. With the information on the types and amount of alcohol consumed, it can be determined whether an individual participated in heavy drinking, following the definition that heaving drinking is consuming 15 or more drinks per week for males and 8 or more drinks per week for females[2].

Table 2.2 presents the summary statistics on the outcome variables for the restricted full sample, the restricted MLSS recipients, and the non-recipients respectively as well as the differences between the two subsample groups. Overall, 29% of the population were smokers. The smokers smoked an average of 16.7 cigarettes and spent 6.5 RMB yuan on cigarettes per day. 17% of the population drank more than 3 times per week, among which 33.5% had heavy drinking behaviors. The differences between the restricted MLSS recipients and non-

[2]Defined by the U.S. Department of Health and Human Services, Centers for Disease Control and Prevention: `https://www.cdc.gov/alcohol/faqs.htm`. Definition of a standard drink: 14.0g (0.6 oz) of pure alcohol, equivalent to: 12 oz of beer (5% alcohol content), 8 oz of malt liquor (7% alcohol content), 5 oz of wine (12% alcohol content), 1.5 oz of spirits (40% alcohol content or 80 proof).

recipients show that the recipients are less likely to smoke and drink more than three times a week, however, the differences are small and insignificant. The smokers smoked an average of 0.7 less cigarettes and spent 2.7 RMB yuan less per day than the smokers who didn't receive the MLSS allowance. In terms of drinking, the frequent drinkers in the restricted MLSS recipients group were less likely to be heavy drinkers than their counterparts. The difference might be attributed to the unique drinking culture in China. Drinking is pervasive in social occasions for Chinese. Compared to the drinking style in the Western countries, it is more ritualized and purposeful as it is regarded as a significant part of successful business in China. Furthermore, individuals who earn more are more likely to participate in social gatherings and business meetings and thus are more prone to heavy drinking.

Table 2.2: Summary Statistics of Outcome Variables, by Treatment Status

	Full sample	Restricted recipients	Non-recipients	Differences
	(1)	(2)	(3)	(2)-(3)
Smoking				
Smoker(=1)	0.289	0.285	0.287	-0.002
	(0.453)	(0.452)	(0.453)	(0.009)
N	32,044	2,493	28,591	
Daily consumption of cigarettes	16.739	16.098	16.807	-0.710*
	(9.822)	(10.043)	(9.836)	(0.387)
N	9,056	693	8,046	
Cost of cigarettes per day	6.513	4.042	6.759	-2.717***
(RMB yuan)	(6.591)	(3.155)	(6.833)	(0.255)
N	9,275	721	8,233	
Drinking				
Drinking more than 3 times	0.171	0.167	0.171	-0.005
per week (=1)	(0.377)	(0.373)	(0.377)	(0.008)
N	32,040	2,493	28,587	
Heavy drinking(=1)	0.335	0.228	0.346	-0.119***
	(0.472)	(0.420)	(0.476)	(0.025)
N	4,808	341	4,327	

Notes: Standard deviations for the mean of each variable in parentheses. Standard errors for the differences of each variable between the treatment and control groups in parentheses. * p<10%, ** p<5%, ***p<1%.

CHAPTER 3

EMPIRICAL METHOD

The possible endogeneity of consumption of cigarettes and alcohol to low income has been well documented in the literature. For example, Binkley (2011) found that low income individuals are more likely to smoke cigarettes when facing trade-offs between pleasure and health because they are more reluctant to forgo the pleasure of smoking than the future utility. Furthermore, observational studies often face the challenge of selection bias given the systematic differences between participants and non-participants. To address the issue of endogeneity and selection bias, I implement a propensity score matching identification strategy. Next, I will first introduce the setup for the strategy.

3.1 The Potential Outcome Framework

To evaluate the impact of a treatment on the outcome of a treated unit, we need to speculate what the performance of the treated unit would be if he had not been treated. This is called causal inference. For each observation unit i out of a sample of N units, enrollment in the MLSS is a binary decision denoted by W_i. $W_i = 1$ for those who enrolled in the MLSS, i.e. the treated units, while $W_i = 0$ for the control units who did not enroll. The potential outcomes are:

$$Y_i(W_i) = \begin{cases} Y_i(0), \text{ if } W_i = 0 \\ Y_i(1), \text{ if } W_i = 1 \end{cases}$$

That is, $Y_i(1)$ and $Y_i(0)$ are a treated and a control unit i's smoking and drink-

ing outcomes, respectively. For those who were actually treated, the difference between their actual outcome values and their expected outcome values if they had not been treated is defined as the *average treatment effect on the treated* (ATT). If we directly compare the outcomes of those recipients and non-recipients, observed difference comprises both ATT and *selection bias*:

$$\underbrace{E[Y_i(1)|W_i = 1] - E[Y_i(0)|W_i = 0]}_{\text{observed difference}} = \underbrace{E[Y_i(1) - Y_i(0)|W_i = 1]}_{\text{average treatment effect on the treated}}$$
$$+ \underbrace{E[Y_i(0)|W_i = 1] - E[Y_i(0)|W_i = 0]}_{\text{selection bias}}$$

In this case, if cigarettes and alcohol are normal goods and rich people are more likely to smoke and drink anyway, then the selection bias would be negative and thus exaggerate the effect of the MLSS receipt.

In observational studies, there are some assumptions key to solve the problem of selection bias. The first assumption is *unconfoundedness*. Under this assumption, conditional on a selected set of observed covariates, the outcome of interest is independent of the treatment assignment. It is noted as:

$$Y_i(1), Y_i(0) \perp W_i|X_i$$

The second assumption is *overlap*. This assumption implies that the units with the same values of covariates have the same probability of being treated:

$$0 < Pr(W_i = 1|X_i) < 1$$

The probability of being treated is called the *propensity score*, denoted by $e(x)$. The treatment assignment conditional on the given covariates is *strongly ignorable* when both of the assumptions hold (Rosenbaum and Rubin, 1983). Under strong ignorability, we can estimate the average effects by adjusting for differences in covariates between the treated and control units.

3.2 The Estimation Strategy: Propensity Score Matching

In this paper, I will follow the steps and algorithms proposed by Caliendo (2008) and Imbens (2015) to estimate the average effect of the treatment on the treated units.

Step I: Estimating the Propensity Score

As the treatment assignment in this study, i.e. the MLSS receipt, is a binary variable, I choose the logit model by maximum likelihood proposed by Imbens and Rubin (2015) to estimate the propensity score for each unit in the restricted subsample:

$$e(X_i) = Pr[W_i = 1|X_i] = \frac{exp(h(x)'\gamma)}{1 + exp(h(x)'\gamma)}$$

where $h(x)$ is the vector of functions of covariates with linear and higher order terms and γ is the vector of unknown parameters.

The next is to decide what covariates should be included in the specification of the propensity score. Here, I follow the algorithm outlined by Imbens (2015) for selecting the covariates. First, I estimate the logit model by maximum likelihood with *ex ante per-capita annual household income*, *local MLSS standard threshold* and *employment status* as the basic covariates. *Ex ante* income and local threshold together imply a unit's eligibility for the MLSS, while the employment status is significant to one's income. Since the MLSS is administered at the county level and the threshold is set by local county governments, it is more valid to use the county-level MLSS standard threshold for estimating the probability of treatment assignment. However, for security reasons, the CFPS data does not provide county identifying information publicly and thus I will instead use the

provincial average MLSS threshold based on one's urbanicity. That is, for a unit living in the urban or rural area, the local MLSS threshold used for estimation is the urban or rural provincial average threshold, respectively, in the year of survey. Next, I estimate one additional logit model by maximum likelihood for each of the remaining covariates, *sex*, *age*, *education years*, *marital status* and *urbanicity*. For each of the estimations, I include the basic three covariates and the additional covariate, and then calculate the likelihood ratio (LLR) test statistics for the null hypothesis that the coefficient of the additional covariate is equal to zero. If the largest of the LLR test statistic is larger or equal to a chosen C_{lin}, the corresponding covariate is added to the linear part of the model. Then I estimate the logit model by maximum likelihood with the already selected X_b and the additional covariate, plus one of the remaining covariates at one time. Again, the LLR results are calculated and compared to C_{lin} to decide which additional covariate to be selected for inclusion. This step is repeated until the maximum of the LLR results is smaller than C_{lin} (i.e. no additional covariates would improve the model) or the covariates are run out. By now, the linear terms have been selected and the next step is to choose a subset of second-order terms. Only the covariates of which the linear terms have been selected for inclusion are used to create interactions and quadratic terms to be the candidate second-order terms. The process of selection is similar to that for the linear terms, except that the LLR test statistics are now compared to C_{qua}. The process is repeated until the maximum of the LLR results is smaller than C_{qua} (i.e. no additional second-order term would improve the model) or the second-order terms have run out. After selecting all the linear and second-order terms for inclusion, the propensity score is estimated by using this specification.

Step II: Ensuring Overlap: Trimming

As mentioned above, overlap or the common support is crucial to estimating the average effects and failure of common support leads to evaluation bias (Heckman *et al.*, 1997). To improve the extent of overlap, trimming constructs a subsample that is more balanced between the treated and control units. The goal of trimming is to discard the units with extreme values of propensity score that are not comparable to the units in the opposite group. Imbens and Rubin (2015) describes the procedure of trimming, including how to choose a threshold to determine whether to discard a unit or not. The threshold for assessing whether a unit should be included or discarded is determined by the covariates and treatment indicator, denoted by α. The units with the propensity score falling outside the interval $[\alpha, 1-\alpha]$ would be discarded. First, we need to check the following inequality:

$$\sup_{x \in \mathbb{X}} \frac{1}{e(x) \cdot (1 - e(x))} \leq 2 \cdot E_{sample}\left[\frac{1}{e(X_i) \cdot (1 - e(X_i))}\right]$$

where $\mathbb{X}$ is the entire covariate space where there are N units. If the inequality holds, then the asymptotic sampling variance of $\mathbb{X}$ is smaller than its subsets and thus no units need to be discarded. If the inequality does not hold, then the threshold is calculated by:

$$\alpha = \frac{1}{2} - \sqrt{\frac{1}{4} - \frac{1}{\delta}}$$

where δ is the solution to:

$$\delta = \frac{1}{2} \cdot E_{sample}\left[\frac{1}{e(X_i) \cdot (1 - e(X_i))} \middle| \frac{1}{e(X_i) \cdot (1 - e(X_i))} \leq \delta\right]$$

In implementation, the estimated propensity score is denoted by $\hat{e}(X_i)$. The inequality becomes:

$$\max_{i=1,\ldots,N} \frac{1}{\hat{e}(X_i)(1 - \hat{e}(X_i))} \leq 2 \cdot \frac{1}{N} \sum_{i=1}^{N} \frac{1}{\hat{e}(X_i)(1 - \hat{e}(X_i))}$$

Solving the value for δ in the following equation:

$$\frac{\delta}{N}\sum_{i=1}^{N} 1_{\frac{1}{\hat{e}(X_i)(1-\hat{e}(X_i))}\leq\delta} = 2\cdot\frac{1}{N}\sum_{i=1}^{N}\frac{1}{\hat{e}(X_i)(1-\hat{e}(X_i))}\cdot 1_{\frac{1}{\hat{e}(X_i)(1-\hat{e}(X_i))}\leq\delta}$$

gives $\hat{\alpha} = \frac{1}{2} - \sqrt{\frac{1}{4} - \frac{1}{\hat{\gamma}}}$. Only the units that satisfy $\hat{\alpha} \leq \hat{e}(X_i) \leq 1 - \hat{\alpha}$ are included in the trimmed subsample (Y^T, W^T, X^T).

Step III: Choosing a Matching Algorithm

Matching algorithms, such as *nearest neighbor (NN) matching*, *caliper matching*, *kernel matching*, etc., differ in how the neighborhood region for the treated units is defined and how the weights for the neighbor control units are constructed. There are trade-offs between bias and efficiency for each matching algorithm (Caliendo, 2008). In this paper, I will use a traditional and the most straightforward matching method, *one-to-one nearest neighbor matching* without replacement, to construct a balanced sample. The estimated propensity score is first transformed into the log odds ratio (Imbens, 2015):

$$\hat{\ell}(x) = ln\left(\frac{\hat{e}(x)}{1-\hat{e}(x)}\right)$$

Then, the control unit with the closest log odds ratio is matched to a treated unit i where each control unit can be used only once. The treated units get their match in an order by their estimated log odds ratio from the highest to the lowest. However, this algorithm has the shortcomings that bad matching arises when the treated and control units have very different propensity scores and that the estimates depend on the order in which the units get matched. Therefore, I will perform a sensitivity test to the matching algorithm as a robustness check for the analysis results.

Step IV: Assessing Unconfoundedness

The next step is to assess the plausibility of unconfoundedness. Since the unconfoundedness assumption itself, $Y_i(1), Y_i(0) \perp W_i|X_i$, is not testable, Imbens (2015) proposes to estimate the effect of treatment on some pseudo-outcomes. Such pseudo-outcomes are usually pretreatment characteristics that are not affected by the treatment. To implement, the covariates are divided into the two groups, the pseudo-outcomes X_p and the remaining of the covariates X_r. Here, I select the local MLSS standard cutoff as the pseudo-outcome. Then the trimming procedure outlined in Step III is employed to construct the trimmed pseudo sample (X_p^T, W^T, X_r^T). The pseudo effect for the pseudo-outcomes is:

$$\hat{\tau}_X = \tau(X_p^T, W^T, X_r^T)$$

If the pseudo effect differs from zero, we have more confidence in the plausibility of unconfoundedness and therefore the credibility of the estimated average effect of the treatment on the outcomes of interest increases.

Step V: Calculation of Treatment Effects

Finally, it comes to the step to calculate the effects. Under strong ignorability, the PSM estimator of ATT is the mean difference of outcome values between the matched treated and control units over their common support region:

$$\tau_{ATT} = E_{\hat{\ell}(x)|W_i=1}[E[Y_i(1)|W_i = 1, \hat{\ell}(x)] - E[Y_i(0)|W_i = 0, \hat{\ell}(x)]]$$

CHAPTER 4

RESULTS

Before reporting the results of the PSM estimation, I will first estimate an *ordinary least square* (OLS) specification as a baseline. The estimation results are presented in Table 4.1. The model controls for the covariates including *sex*, *age*, *marital status*, *urbanicity*, *education years*, *ex ante per capita annual household income*, *local MLSS standard threshold*, and *employment status* and includes province fixed effects. The standard errors are clustered at the household level. The reason for including the fixed effects is that smoking bans differ across provinces in terms of specific terms and the time when the bans were issued. For example, Shanghai was the first province to stipulate a smoking ban in 2010 and Beijing came with its no smoking policy in public places two years later. The coefficient estimates show that enrollment in the MLSS is negatively associated with the probability of smoking, the number of cigarettes smoked per day and daily cost of cigarettes for smokers, suggesting that smoking is an inferior good for the individuals with a low income. Specifically, enrollment in the MLSS decreases the probability of smoking by 2.6%, statistically significant at the 10% level. The smokers smoked an average of 0.5 less cigarettes per day with the treatment but the estimate is not significantly different from zero. In addition, the recipient smokers spent an average of 1.1 RMB yuan less than the non-recipient smokers, statistically significant at the 1% level. In terms of drinking, the treatment of the MLSS is positively associated with the probability of drinking more than three times a week, however the estimate is close and not significantly different from zero. Nevertheless, the MLSS decreases the probability of heavy drinking by 6.4%, statistically significant at the 1% level.

Table 4.1: OLS Estimation

	Enrollment in the MLSS (Recipient=1)
Smoking	
Smoker	-0.026*
	(0.015)
Daily consumption of cigarettes	-0.513
	(0.767)
Cost of cigarettes per day	-1.098***
	(0.239)
Drinking	
Drinking more than 3 times per week	0.004
	(0.016)
Heavy drinking	-0.064*
	(0.035)

Notes: Standard errors in parentheses and clustered at the household level. All models control for sex, age, marital status, urbanicity, education years, *ex ante* per capita annual household income, local MLSS standard threshold, and employment status. * p<10%, ** p<5%, ***p<1%.

4.1 Estimating the Propensity Score

To select the covariates for inclusion in the propensity score, I follow the algorithm described in Chapter 3 with $C_{lin} = 1$ and $C_{qua} = 2.71$. Table 4.2 presents the parameter estimates for the final specification for the propensity score. The covariates are listed in the order they are selected for inclusion. Specifically, all the linear terms of the characteristic variables are selected as well as eight more second-order terms.

Table 4.2: Estimated Parameters of the Propensity Score

Variable	Estimated	(Standard Error)
Intercept	-0.443	(0.722)
Preselected Linear Terms		
Ex ante per-capita annual household income	0.000***	(0.000)
Local MLSS standard threshold	-0.001***	(0.000)
Employment status	-0.152	(0.657)
Additional Linear Terms		
Marital status	-0.601***	(0.090)
Education years	-0.113***	(0.027)
Urbanicity	0.532*	(0.294)
Age	0.034**	(0.015)
Sex	0.172***	(0.063)
Second-order Terms		
Ex ante per-capita annual household income × *Ex ante* per-capita annual household income	-0.000***	(0.000)
Local MLSS standard threshold × Local MLSS standard threshold	0.000***	(0.000)
Ex ante per-capita annual household income × Local MLSS standard threshold	-0.000***	(0.000)
Education years × Urbanicity	0.037**	(0.015)
Education years × Age	0.001**	(0.001)
Employment status × Age	-0.031**	(0.015)
Ex ante per-capita annual household income × Urbanicity	0.000*	(0.000)
Ex ante per-capita annual household income × Employment status	0.000*	(0.000)
R-squared	0.19	
N	17,334	

Notes: The local MLSS standard threshold is the provincial average MLSS threshold (RMB yuan) of annual per-capita income based on urbanicity. * p<10%, ** p<5%, ***p<1%.

4.2 Trimming

In order to improve the overlap between the treatment and control group, the restricted sample is further trimmed to drop the units with an extreme propensity score. First, checking the inequality:

$$\max_{i=1,\ldots,N} \frac{1}{\hat{e}(X_i)(1-\hat{e}(X_i))} \leq 2 \cdot \frac{1}{N} \sum_{i=1}^{N} \frac{1}{\hat{e}(X_i)(1-\hat{e}(X_i))}$$

and it does not hold for the restricted sample. The threshold α for the propensity score is calculated to be 0.1299. Table 4.3 reports the trimming results. Under this threshold, 12,932 units with an estimated propensity score outside the threshold interval $[0.1299, 0.8701]$ are discarded, leaving a trimmed sample of 804 treated units and 3,598 control units.

Table 4.3: Subsample Sizes with the Propensity Score between α and $1-\alpha$

	Low	Middle	High	All
	$e(X) < \alpha$	$\alpha(X) \leq 1-\alpha$	$1-\alpha < e(X)$	
the MLSS Recipients	489	804	0	1,293
Non-recipients	12,443	3,598	0	16,041
All	12,932	4,402	0	17,334

Then the propensity score is reestimated on the trimmed sample. Following the same algorithm starting with *ex ante per-capita annual household income*, *local MLSS standard threshold* and *employment status* as the basic covariates, three additional linear terms and four second-order terms are selected for inclusion for the specification of propensity score. Table 4.4 presents the parameter estimates and the covariates are listed in the order they are selected for inclusion.

Figure 4.1 illustrates the density distribution of the estimated propensity score for the trimmed treated and control units, respectively. It provides a visual

Table 4.4: Reestimated Parameters of the Propensity Score on the Trimmed Sample

Variable	Estimated	(Standard Error)
Intercept	-2.329***	(0.734)
Preselected Linear Terms		
Ex ante per-capita annual household income	0.000*	(0.000)
Local MLSS standard threshold	-0.000**	(0.000)
Employment status	0.898	(0.709)
Additional Linear Terms		
Marital status	-0.441***	(0.101)
Education years	-0.007	(0.037)
Age	0.048***	(0.017)
Second-order Terms		
Ex ante per-capita annual household income × Education years	-0.000***	(0.000)
Employment status × Age	-0.037**	(0.017)
Education years × Education years	0.006*	(0.003)
Ex ante per-capita annual household income × Age	-0.000*	(0.000)
R-squared	0.013	
N	4,402	

Notes: The local MLSS standard threshold is the provincial average MLSS threshold (RMB yuan) of annual per-capita income based on urbanicity. * p<10%, ** p<5%, ***p<1%.

analysis on the extent of common support between the two groups. With a good overlap as shown by the figure, it is plausible to proceed with the estimation of treatment effects.

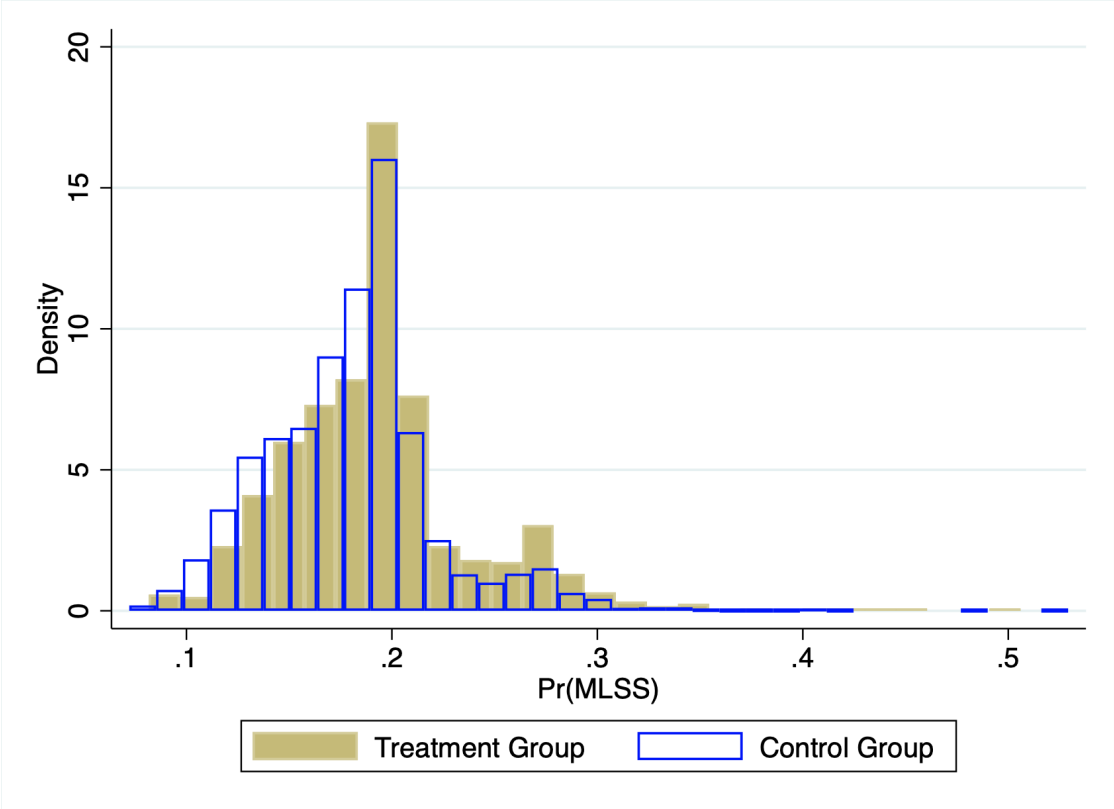

Figure 4.1: Common Support of the Propensity Score

4.3 Matching

After performing NN matching without replacement on the trimmed sample, 804 recipients are matched with 804 non-recipients. Figure 4.2 demonstrates the process of sample selection.

Table 4.5 summarizes the covariates of the matched two groups and reports the t-statistics and standardized differences. Standardized difference is increasingly used to assess the balance in the covariates for propensity-score matched

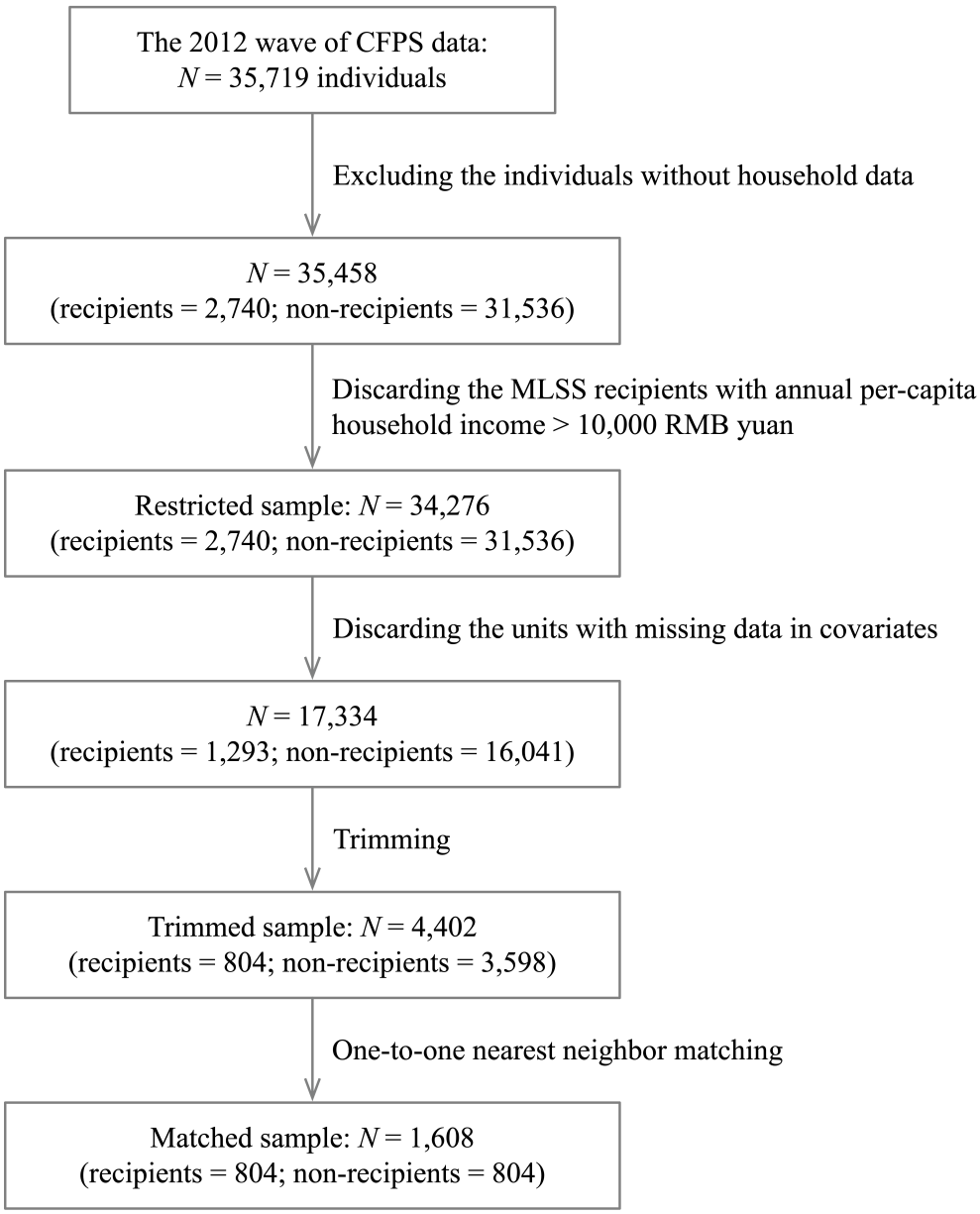

Figure 4.2: The Process of Sample Selection

samples (Austin, 2009), calculated by:

$$d = \frac{\bar{x}_t - \bar{x}_c}{\sqrt{(s_t^2 + s_c^2)/2}}$$

where $\bar{x}_t$ and $\bar{x}_c$ are the mean value of a covariate for the treatment and control groups, respectively, while s_t and s_c are the corresponding standard devia-

tions. The results show that the t-statistics and the standardized differences of the characteristics between the two groups are modest, all of which are lower than 0.1[1]. Therefore, the matched sample is considered as well balanced, increasing the credibility of the estimates of treatment effects.

Table 4.5: Summary Statistics of the Matched Sample

	Recipients (N=804)	Non-recipients (N=804)	t-stat	Standardized Difference
Ex ante per-capita annual	3909.408	3927.781	-0.181	-0.009
household income	(2030.844)	(2046.357)		
Local MLSS standard threshold	1977.128	1933.664	0.957	0.048
	(941.205)	(879.194)		
Sex	0.561	0.566	-0.201	-0.010
	(0.497)	(0.496)		
Age	49.239	49.200	0.052	0.003
	(14.808)	(14.696)		
Marital status	0.781	0.765	0.774	0.039
	(0.414)	(0.424)		
Education years	2.943	2.998	-0.274	-0.014
	(4.036)	(3.964)		
Urbanicity	0.226	0.204	1.092	0.054
	(0.419)	(0.403)		
Employment status	0.964	0.968	-0.411	-0.021
	(0.187)	(0.177)		

Notes: Standard deviations in parentheses. The local MLSS standard threshold is the provincial average MLSS threshold (RMB yuan) of annual per-capita income based on urbanicity.

[1]Standardized difference is increasingly used to assess the balance in the covariates for propensity-score matched samples (Austin, 2009). There is yet consensus on the threshold for imbalance. Normand *et al.* (2001) proposes that a standardized difference with the absolute value < 0.1 indicates a good balance.

4.4 Assessing Unconfoundedness

To assess the plausibility of unconfoundedness assumption, I will estimate the effect of treatment on some pseudo-outcomes. *Local MLSS standard threshold* is selected as the pseudo-outcome as it is not affected by the treatment. The remaining covariates are *ex ante per-capita annual household income*, *sex*, *age*, *employment status*, *marital status*, *urbanicity*, and *education years*. Starting with the untrimmed restricted sample, I redo the estimation of the propensity score, trimming, reestimation of the propensity score and matching. Table 4.6 shows the estimated effect of the MLSS on the pseudo-outcome. The estimate is not statistically significant from zero, supporting the unconfoundedness assumption in this setting.

Table 4.6: Estimates of Causal Effects on Pseudo Outcomes

Dependent Variable: Enrollment in the MLSS (Recipient=1)	Estimates	(Standard Error)	t-stat
Local MLSS standard threshold	1.383	(0.911)	75.250
N	2,546		

Notes: The local MLSS standard threshold is the provincial average MLSS threshold (RMB yuan) of annual per-capita income based on urbanicity. * p<10%, ** p<5%, ***p<1%.

4.5 Estimation of the Effects of the MLSS Receipt

Table 4.7 presents the estimated average treatment effects of the MLSS on the treated units. Similar to the OLS estimation, the province fixed effects are included in the specification to control for the policy variations across provinces

and the standard errors are clustered at the household level. Compared to the baseline results in Table 4.1, the magnitude of coefficient estimates as well as the statistical significance change considerably upon matching. The probability of smoking decreases by 4.2% with the MLSS receipt (2.6% by OLS estimation), statistically significant at the 10% level. The change of daily consumption of cigarettes becomes positive but still insignificant. The recipient smokers spent 0.73 RMB yuan less than the non-recipient smokers (1.1 RMB yuan less by OLS estimation), statistically significant at the 1% level. In alignment with the OLS estimates, the impact of the MLSS on the probability of drinking more than three times per week is trivial and insignificant. The MLSS decreases the probability of heavy drinking among the recipients, however the impact is no longer significant. The differences between OLS and PSM results suggest that the OLS estimates are biased as expected.

Table 4.7: Causal Effects of the MLSS on Drinking and Smoking

	Enrollment in the MLSS (Recipient=1)
Smoking	
Smoker	-0.042*
	(0.023)
Daily consumption of cigarettes	0.079
	(0.823)
Cost of cigarettes per day	-0.725***
	(0.224)
Drinking	
Drinking more than 3 times per week	0.003
	(0.019)
Heavy drinking	-0.090
	(0.055)

Notes: Standard errors in parentheses and clustered at the household level. * p<10%, ** p<5%, ***p<1%.

CHAPTER 5

SENSITIVITY ANALYSIS

5.1 Sensitivity to the Specification of Propensity Score

In order to check the robustness of the estimates, I will first perform a sensitivity analysis to the specification of propensity score. The ATT estimates under two alternative specifications of propensity score, one with only linear terms and another with all the linear and second-order terms, are compared to the estimates under the preferred specification. The results are presented in Table 5.1. The effects of the MLSS on recipients' smoking and drinking outcomes under the linear specification of propensity score are close to the main results in direction, magnitude and significance. Under the linear and second-order specification, the estimated effect on daily consumption of cigarettes becomes negative but still insignificant. The estimated effect on the probability of drinking more than three times per week has changed from negative to positive and is statistically significant at the 10% level. The differences might be attributed to the heterogeneity across the matched samples compared to that under the preferred specification (see Appendix Table A.1).

5.2 Sensitivity to the Matching Algorithm

In the main analysis, the one-to-one NN matching without replacement was employed as it is the most straightforward and commonly used algorithm in the literature. However, the matching quality is defected when the closest con-

Table 5.1: Sensitivity to the Specification of Propensity Score

Dependent Variable: Enrollment in the MLSS (Recipient=1)	Linear Specification (Matched Sample = 2,462)	Linear and Second-order Specification (Matched Sample = 2,444)
Smoking		
Smoker	-0.043**	-0.039**
	(0.019)	(0.019)
Daily consumption of cigarettes	0.084	-0.172
	(0.706)	(0.711)
Cost of cigarettes per day	-0.788***	-0.730***
	(0.207)	(0.206)
Drinking		
Drinking more than 3 times per week	0.001	-0.028*
	(0.016)	(0.016)
Heavy drinking	-0.058	-0.012
	(0.046)	(0.043)

Notes: Standard errors in parentheses and clustered at the household level. * p<10%, ** p<5%, ***p<1%.

trol unit is far away and the treated units are forced to be matched with a control unit with a quite different estimated propensity score. Although matching without replacement increases the precision of estimates, it leads to larger bias (Dehejia and Wahba, 2002). In addition, the matching results are dependent on the order in which treated units are matched. Therefore, I will test the sensitivity of the estimates to the matching algorithm. Different matching estimators are essentially different in assignment of weights to the control units when computing the average effects. If there is a substantial overlap of the propensity score, the estimates of effects should be similar under different algorithms. Four alternative matching algorithms, one-to-one NN matching with replace-

ment, K-nearest neighbor matching, caliper matching, and kernel matching, are performed for comparison.

Matching with replacement overcomes the problem of bad matches when there are few close control units to the treated units. The order in which treated units are matched does not affect the matching results as well. The treated units are still matched to a control unit with the closest $\hat{\ell}(x)$ but each control unit can be used more than once. However, matching with replacement increases variance because less information is used to construct the counterfactual. K-nearest neighbor matching, also known as oversampling (Caliendo, 2008), matches each treated unit with k control units with the closest $\hat{\ell}(x)$ with replacement. It reduces variance of the estimates but increases bias as a result of possible bad matches. Here, I choose $k = 5$ and assign uniform weights to the matched control units. Caliper matching is another way to match each treated unit with a set of control units where only the control units with the $\hat{\ell}(x)$ within a chosen caliper are matched. Similar to K-nearest matching, it also reduces variance but increases bias. The key issue of using caliper matching is to decide on an appropriate caliper. Following the recommendation by Austin (2011), I choose a width of caliper equaling to 0.2 of the standard deviation of $\hat{\ell}(x)$. Finally, kernel matching is a form of nonparametric estimator which assigns all the control units a kernel weight based on the chosen kernel function and uses the weighted averages of the control units as a counterfactual outcome (Caliendo, 2008). This estimator is featured with reduced variance as more information is used but increased bias. In the implementation, I use the Epanechnikov kernel with the rule of thumb bandwidth as the kernel function. Appendix Table A.2 provides the details of the matched samples under different matching algorithms.

The results of the sensitivity test are reported in Table 5.2. The estimates on smoking outcomes are comparable to the main results. The one exception occurs with the daily consumption of cigarettes outcome, where the effect becomes negative but still insignificant across the alternative matching algorithms. In terms of the drinking outcomes, the matching estimators yield comparable results to the estimates of effects in Table 4.7. The consistency of four sets of results further supports that the sample has a good overlap between the recipients and non-recipients. Therefore, the estimates are credible and robust.

Table 5.2: Sensitivity to the Matching Algorithm

Dependent Variable: Enrollment in the MLSS (Recipient=1)	One-to-one NN Matching with Replacement	K-NN Matching (K=5)	Caliper Matching	Kernel Matching
Smoking				
Smoker	-0.034	-0.026	-0.035	-0.026
	(0.026)	(0.020)	(0.026)	(0.018)
Daily consumption of	-0.108	-0.314	-0.128	-0.718
cigarettes	(0.866)	(0.683)	(0.869)	(0.644)
Cost of cigarettes per day	-0.775***	-0.993***	-0.785***	-0.975***
	(0.250)	(0.220)	(0.251)	(0.196)
Drinking				
Drinking more than 3 times	0.010	0.001	0.009	0.007
per week	(0.021)	(0.017)	(0.021)	(0.015)
Heavy drinking	-0.084	-0.050	-0.084	-0.056
	(0.059)	(0.043)	(0.059)	(0.040)

Notes: Standard errors in parentheses and clustered at the household level. * p<10%, ** p<5%, ***p<1%.

CHAPTER 6

CONCLUSION AND DISCUSSION

In this paper, I examine the impact of the Minimum Living Security Scheme (MLSS) on recipients' smoking and drinking behaviors by using the propensity score matching method and the 2012 wave of the China Family Panel Studies Survey (CFPS). The results indicate that the MLSS receipt decreases the probability of smoking by 2.6 to 4.2 per cent and decreases the recipients' spending on cigarettes by 0.73 to 0.99 RMB yuan per day, suggesting that cigarettes are an inferior good for the low-income population in China. The findings are in contrast to Kenkel *et al.* (2014) and Dartanto *et al.* (2021) that find social assistance programs increase smoking among the recipients. The results partly also echo the findings of existing studies that the MLSS recipients prioritize their spending in health (Gao *et al.*, 2010; Gao *et al.*, 2014; Yi *et al.*, 2019), further supporting that good health is a normal good as the demand for cigarettes decline as a result of more investment in the production of good health. Despite the evidence of reduced probability of smoking and spending on cigarettes, I find that the MLSS receipt has limited effect on the intensity of smoking (less than 0.1 cigarettes per day) and the effect is not statistically significant. Meanwhile, I do not find any significant effect of the MLSS receipt on the probability of frequent drinking and heavy drinking, which is consistent with the findings of Rehkopf *et al.* (2014) and Collin *et al.* (2020) that there is no evidence of short-term increases in alcohol consumption caused by the EITC, a governmental transfer program in the U.S. targeting at low income working families.

One limitation of this study is that the eligibility threshold used for matching purposes is the provincial average threshold while the county-level income

threshold is actually used by local governments to select recipients. For some provinces like Beijing and Shanghai, there are not substantial variations in the eligibility thresholds across counties. However, for some provinces where there exists uneven economic development, the average of income varies a lot across regions and thus the eligibility threshold of the MLSS also varies substantially. Therefore, using the provincial average thresholds as a matching criterion may not be accurate and could harm the reliability of the results. With the limitation in mind, this study has several contributions to the literature. First of all, it adds new evidence to the growing literature on the potential health outcomes of non-health anti-poverty programs. Another contribution is that, unlike Gao *et al.* (2014) and Yi *et al.* (2019) which only examined the effect of the MLSS on tobacco and alcohol consumption, this paper seeks to explore the effects on multiple dimensions of smoking and drinking given that the CFPS data includes the variables that measure the consumption as well as the intensity.

The findings of this study provide some implications for future policy reforms in China and beyond. The public and policy makers may concern the unconditional cash transfers provided to the recipients are inappropriately used in unhealthy products such as cigarettes and alcohol rather than basic living necessities, and may consider change the delivery of benefits in a way that recipients can only spend the allowance on necessities such as foods in authorized grocery stores, as how the Supplemental Nutrition Assistance Program (SNAP) in the U.S. operates. However, investing in an electronic benefit transfer system for a large scale social assistance program can be very costly. It is good news that the MLSS is not found to encourage smoking or drinking despite the lack of monitoring or stipulated conditions. Therefore, there is no incentive to change the current benefit deliveries of the program. Even if there are unintended neg-

ative health consequences of social assistance programs found in future studies, policy makers should carefully consider the cost-benefit relationship and inform policy development. Furthermore, the effect of the exogenous change in income caused by social assistance programs on smoking and drinking may vary from country to country. This is probably due to the differences in contexts and culture. Therefore, the discrepancy calls for considerations on the contingency of social costs that could result from unintended consequences of social assistance.

APPENDIX A

APPENDIX TABLES

Table A.1: Summary Statistics of the Matched Sample, by Specification of Propensity Score

	Recipients (N=1,222)	Non-recipients (N=1,222)	t-stat	Std. Diff.
	Linear Specification			
Ex ante per-capita annual	4381.385	4329.261	0.458	0.018
household income	(2580.438)	(3050.530)		
Local MLSS standard	2090.163	2061.938	0.794	0.032
threshold	(904.519)	(859.280)		
Sex	0.546	0.553	-0.364	-0.015
	(0.498)	(0.497)		
Age	48.125	48.989	-1.509	-0.061
	(14.045)	(14.376)		
Marital status	0.836	0.846	-0.717	-0.029
	(0.371)	(0.361)		
Education years	4.012	4.012	0.000	0.000
	(4.419)	(4.263)		
Urbanicity	0.260	0.238	1.258	0.051
	(0.439)	(0.426)		
Employment status	0.971	0.970	0.119	0.005
	(0.169)	(0.171)		
	Linear and Second-order Specification			
Ex ante per-capita annual	4311.588	4373.45	-0.607	-0.025
household income	(2524.591)	(2516.546)		
Local MLSS standard	2128.771	2095.891	0.834	0.034
threshold	(997.601)	(950.058)		
Sex	0.544	0.547	-0.122	-0.005
	(0.498)	(0.498)		
Age	48.116	48.391	-0.486	-0.020
	(14.016)	(13.955)		
Marital status	0.836	0.085	-1.000	-0.040
	(0.371)	(0.357)		
Education years	3.968	3.845	0.693	0.028
	(4.429)	(4.324)		
Urbanicity	0.267	0.258	0.506	0.020
	(0.442)	(0.438)		
Employment status	0.971	0.974	-0.371	-0.015
	(0.167)	(0.160)		

Notes: Standard deviations in parentheses. The local MLSS standard threshold is the provincial average MLSS threshold (RMB yuan) of annual per-capita income based on urbanicity.

Table A.2: Summary Statistics of the Matched Sample, by Matching Algorithm

	One-to-one NN Matching with Replacement			K-NN Matching (K=5)		
	Non-recipients (N=676)	t-stat	Std. Diff.	Non-recipients (N=2,245)	t-stat	Std. Diff.
Ex ante per-capita annual household income	3918.914	-0.094	-0.005	3897.058	0.121	0.006
	(2030.146)			(2050.669)		
Local MLSS standard threshold	1909.222	1.507	0.075	1979.268	-0.046	-0.002
	(864.068)			(922.553)		
Sex	0.570	-0.352	-0.018	0.558	0.121	0.006
	(0.495)			(0.497)		
Age	49.266	-0.037	-0.002	49.010	0.188	0.009
	(14.643)			(14.885)		
Marital status	0.762	0.949	0.047	0.768	0.621	0.031
	(0.427)			(0.422)		
Education years	2.919	0.119	0.006	3.094	-0.749	-0.037
	(3.905)			(4.053)		
Urbanicity	0.189	1.845	0.092	0.222	0.203	0.010
	(0.392)			(0.416)		
Employment status	0.973	-0.996	-0.050	0.965	0.108	-0.005
	(0.163)			(0.184)		
	Caliper Matching			Kernel Matching		
	Non-recipients (N=676)	t-stat	Std. Diff.	Non-recipients (N=3,594)	t-stat	Std. Diff.
Ex ante per-capita annual household income	3921.600	-0.029	-0.001	3924.073	-0.053	-0.003
	(2032.650)			(2029.388)		
Local MLSS standard threshold	1910.422	1.415	0.071	1975.930	-0.035	-0.002
	(865.490)			(923.989)		
Sex	0.568	-0.302	-0.015	0.546	0.579	0.029
	(0.496)			(0.498)		
Age	49.236	-0.020	-0.001	49.377	-0.212	-0.011
	(14.663)			(14.655)		
Marital status	0.760	1.010	0.050	0.778	0.176	0.009
	(0.427)			(0.416)		
Education years	2.885	0.234	0.012	2.954	-0.110	-0.006
	(3.872)			(4.023)		
Urbanicity	0.190	1.726	0.086	0.221	0.163	0.008
	(0.392)			(0.415)		
Employment status	0.976	-1.058	-0.053	0.969	-0.161	-0.008
	(0.152)			(0.173)		

Notes: Standard deviations in parentheses. The local MLSS standard threshold is the provincial average MLSS threshold (RMB yuan) of annual per-capita income based on urbanicity.

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