EXPERIMENTAL AND EMPIRICAL STUDIES OF BELIEF FORMATION

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ABSTRACT OF THE DISSERTATION

Experimental and Empirical Studies of Belief Formation

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This dissertation concerns the formation and the consequences of people's beliefs and preferences. Empirically and experimentally, I focus on individuals' beliefs and ideologies through mechanisms such as group dynamics, education, and educational content.

In Chapter 2, I conduct a laboratory experiment in which subjects play a variant of a public goods game without free-riding incentives. With no private information, the prospect of investment in the experiment involves self-evaluation of uncertain personal and group types, which depend on performance on a pretest. I compare subjects' individual investment decisions with their decisions when in groups. Timing and structure of communication are the two dimensions of controlled treatments. The results suggest that individuals tend to invest more often and that they increase their subjective beliefs of being a "high" type when in groups, especially when group decisions are made prior to individual decisions.

Chapter 3 estimates the causal effect of education on religiosity in the United States using NLSY97. Fixed effects and instrumental variable method are used as identification strategies.

Although cross-sectional ordinary least squares estimation shows a positive correlation between religious outcomes and educational attainment, both fixed effect models and IV estimation show statistically significant negative effects of education, even when cognitive test score is controlled. This suggests that conventional OLS omits factors that push both education and religiosity.

In Chapter 4, I employ a regression discontinuity design and make use of a quasi-experimental approach by looking at the 1997 junior high school curriculum reform, "Knowing *Taiwan*," to examine the impacts of the increased focus on Taiwan in the textbooks on a variety of civic engagement and national identity in favor of Taiwan. Data is from the Taiwan Social Change Survey. I find a significant positive effect of the new curriculum on national identity. However, most measures of civic participation of the treatment group do not exhibit jumps because of the textbook reform.

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Dedication

To my wife, Chung-Chun "Duha" Li

Table of Contents

List of Figures

List of Tables

Chapter 1

Introduction

Belief is at the heart of social sciences. Examples abound. Generally, in economic decision-making processes, agents who face uncertainty form a system of beliefs about the probabilities of each contingent state. Policy assessments and any normative statements in economics require a set of evaluation criteria, a system of belief to conduct value judgments, to determine what is good and what is bad. More fundamentally, any assumptions and axioms in scientific models are adopted through a certain belief. Beyond economics, ideology, religion, and even mythology have their sociological functions in shaping norms of behaviors in human society.[1]

Beliefs, preferences, and values are treated as given in baseline economic models until recent decades. Since the establishments of experimental approaches and behavioral economic science, insights from psychology have been helpful for economists to better understand human behaviors in addition to computation of costs and benefits. One major insight from psychology that differs from classic economic models is that belief and preference is developed instead of given.

The research in my dissertation aims to provide empirical evidence of formation of beliefs and values. It addresses three different topics in belief formation: beliefs in group

[1] Joseph Campbell (1970).

decision-making, education and religiosity, and curriculum and national identity. All three topics are cohesive in a way that each of them represents an underlying belief system behind visible decisions.

Admittedly, the definitions of the word "belief" in different contexts differ. In Chapter 2, "belief" means prior. In Chapter 3 and 4, "beliefs" are ideologies on religious and political spectrums, respectively. I do not impose any theoretical or spiritual interpretation by the term "religious belief" in Chapter 3, and I am not measuring patriotism in Chapter 4. A shared feature of these definitions is that they are fundamental determinants of behaviors.

Chapter 2, "Together We Stand? Internal Dynamics of Group Decision Making," addresses the group settings as determinants of people's belief formation. Using a laboratory experiment, by observing the binary investment decisions subjects make, I induce individuals' beliefs of their own performances on a test when they are isolated, and their beliefs of the same performances when they are in groups. The experimental design allows for a comparison between the two, and thus allows for identifying the effect of being in a group on individuals' self-evaluation. Data from the experiment strongly suggests evidence that the group setting inflates individuals' beliefs about the probability of scoring in the top half on the test and consequently induces individuals to invest even when the costs are too high. Communication structures are found to be of minor influence. Instead, those who are involved in groups before making decisions by themselves have significantly stronger intention to overinvest.

Two distinctions from the existing research highlight the contribution of this paper. First, the novel design of group type makes it possible to exclude the incentive of strategic

interactions such as free-riding, and allows for a direct comparison between individual and group scenarios. Second, the exclusion of private information minimizes Bayesian updating and sharpens the interpretation of the results. The findings can be interpreted as groupthink and thus experimentally contributes to the recent literature on "motivated beliefs."

In Chapter 3, "Estimating the Relationships among Education, Cognitive Ability, and Religion: Evidence from NLSY," I examine the causal effects of education on people's religious behaviors and religious beliefs, using the 1997 National Longitudinal Survey of Youth (NLSY97). In order to deal with endogeneity, I account for individual fixed-effects and construct a set of newly proposed instrumental variables measuring college accessibility, which is derived by combining NLSY97 Geocode data and the Integrated Postsecondary Education Data System. Although cross-sectional ordinary least squares estimation shows a positive correlation between religious outcomes and educational attainment, both fixed effect models and IV estimation show statistically significant negative effects of education on measures of religiosity.

Chapter 4 provides evidence of the political influence of curriculum for junior high school students. Using the 1997 curriculum reform, which includes a lot more content about Taiwan in the "*Knowing Taiwan*" series, I employ a regression discontinuity design (RDD) to find students' later civic participation and national identity orientation. The reform is found to have an impact on national identity, with the treatment group's identity with exclusively Taiwan increasing by 16 percentage points, but it does not translate into higher civic participation.

Chapter 2

Together We Stand?

Internal Dynamics of Group Decision Making

2.1 Introduction

Many important decisions are made by groups. Despite considerable uncertainties of the returns, people invest time, effort, and money in starting-up businesses, political campaigns, academic co-authorships, religious groups,[2] sports teams, criminal gangs, and more. This study concerns the individual's decision to contribute to a group endeavor.

Theoretically, individual economic agents take everything into account and make their optimal decisions according to their beliefs about uncertainty. Generally, the expected returns to group projects depend on the following elements: 1) nature or random shocks, 2) the total effort made by group members (observable), 3) the quality of the group (not directly observable), and 4) competition.

In this study, in order to focus on individuals' beliefs about the quality of their own group, competition is not considered. For the same reason, without loss of generality, the experimental environment is constructed such that all the uncertainty comes from the

[2] Iannaccone (1992) provides a model applied to religious groups, in which participation generates positive externalities.

unknown quality of one's group. In other words, the only thing determined by nature is the unknown group type.[3]

In order to concretely define the type of a group and link it to the prospect of its investment, I let the subjects take a pretest and keep the score of each subject unrevealed until the end of the subsequent investment decisions. A subject is defined as a "high" type individual if he scores above the median on the test, a "low" type if otherwise. Note that an individual does not know his true type. After the pretest, each subject independently makes binary investment decisions in two parts: an individual part and a group part. In the individual part, the gross return entirely depends on the subject's own type and his decision. In the group part, the gross return depends on the average decisions made by all members and the types of all members in the group, i.e., the group type. The costs of investment in both parts vary over rounds and are not shared with group members. Therefore, a subject is expected to form expectations about his own type and the group types.

It remains standard in economic models to treat groups as single agents. Yet, a growing literature documents differences between individual and group decision making. Experimental research that compares the decisions made by individuals and the decisions made by a small group identifies differences in specific settings, including beauty-contest games (Kocher and Sutter, 2005), common value auctions (Cox and Hayne, 2006), simple decisions under risk (Charness et al., 2007; Rockenbach et al., 2007), information cascades (Fahr and Irlenbusch, 2011), and the prisoner's dilemma (Kagel and McGee, 2016). The results are mixed. In these experiments, groups do not seem to be more rational and to

[3] When applying to economic and social phenomena, this unknown shock can be easily interpreted as the quality of the group an agent belongs to, the prospect of a starting-up business, a political campaign's chance of winning an election, or the credibility of a religion or denomination.

make decisions that are more in line with rational predictions all the time.[4] Therefore, whether groups or individuals are better decision makers is inconclusive. Two heads are not necessarily better than one.

Some possible explanations for the ambiguous performance of groups compared to individuals come from psychology. The classic experiment of Asch (1956) shows that individuals may conform to the false opinion of the group even when the group is obviously wrong. At the same time, it underlines the importance of the existence of dissent opinion. Janis (1972) indicates that group members may suffer from "groupthink," which describes group members' distorted beliefs about the group's inerrancy or invulnerability.[5] The inner pressure from this false belief shuts down the unpleasant cautionary opinions a group member may express, and thus leads to bad decisions.

In recent decades, economic models have been developed that rationalize people's behavior of forgoing their own opinions and herding (Banerjee, 1992), and the phenomenon of information cascades (Bikhchandani et al., 1992). In these models, an agent with private information observes public information before making an individual decision. The weighted comparison of private and public messages is the crucial part of these models. Experimental work in this branch includes both private and public

[4] Kocher & Sutter (2005) find that groups are not better decision makers per se but they seem to learn faster in games. In Charness et al. (2006), groups are found to be more rational decision makers. Cox & Hayne (2006) find groups less rational compared to individuals when each member of a group has distinct information. The difference disappears when group members have common information. Fahr & Irlenbusch (2011) groups behave more rationally in the information cascade environment.

[5] Janis (1972) proposes eight symptoms of groupthink: (1) illusion of invulnerability, (2) belief in inherent morality, (3) collective rationalization, (4) stereotyped views of out-groups, (5) direct pressure on dissenters, (6) self-censorship, (7) illusion of unanimity, and (8) self-appointed mindguards.

information as well (McKelvey and Page, 1990; Anderson and Holt, 1997; Bougheas et al., 2015).

More recently, models of "motivated beliefs" describes a mechanism through which individuals cognitively choose beliefs to serve their own utilities. Overconfidence, which is well identified at the individual level (Camerer & Lovallo, 1999), is thought to be the most obvious type of motivated belief (Bénabou and Tirole, 2016). It is also found at the group level (Burks et al., 2013; Proeger and Meub, 2013; Brookins et al., 2014). In addition, asymmetric updating refers to the idea that agents update positive signals more than negative ones (Eli and Rao, 2011). Wishful thinking refers to people choosing to believe what they want to be true (Mayraz, 2011).

Bénabou (2013) constructs a model in which agents in a group face a tradeoff: they can either accept a public signal about a random shock, or hold their own beliefs by ignoring those signals while bearing the risk of making bad decisions out of over-optimism. In his model, the gross return of a group project is determined by a weighted average of an individual's effort and other group members' average effort, with all of this multiplied by a random shock that determines the sign of the gross return. Bénabou's main prediction is that groups are more likely to fall into groupthink: i) when the weight of other group members' effort, i.e., the level of interdependence, is higher; and ii) when the gross return of the random shock is very low in the bad state that rarely happens. One purpose of this paper is to experimentally examine Bénabou's model of groupthink.

How the group type is defined turns out to be one of the most important distinctions between the present work and previous research. Specifically, the impact of a low type individual on the gross return of a group is designed to offset the impact of a high type

individual. This provides two advantages. First, the group part of this game is a variant of public goods game without the incentive to free-ride. It is similar to a public goods game in the sense that each individual's payoff is determined not only by his own investment. The difference is that agents do not know the sign of the externalities generated by their investment and thus do not have a direct incentive to free-ride. In particular, the externality is positive only if the group type is positive. Therefore, without information of the group type, an agent does not have an incentive to free-ride or to misreport his prior belief about his type. This feature rules out a free-riding explanation to focus on the group dynamics alone.

Second, for the same reason, for an individual, the prior expectation of the group type equals the expectation of the individuals' own type since the other two members are most likely to offset each other. It allows me to explicitly identify a rational expectation of the group type. At the same time, by varying costs, I can create situations where it is rational to invest and not to invest both in the individual part and in the group part of the game.

Writing a collaborative grant proposal could be an example of this sort of situation this experiment is meant to inform. Suppose an individual is writing a grant proposal with two potential coauthors. The costs include time, effort, and forgone opportunities. The payoff is the grant itself. Suppose there exists an unofficial threshold by which the grant agency looks at each applicant's previous record. If applicants do not know how the funding agency sets the unofficial threshold, then what is known is that the funding will be granted to the project proposed by a research team that has more researchers above the threshold. Thus, an individual applicant has to estimate the number of individuals who pass the threshold, including himself. If, for example, the acceptance rate of funding is 50%,

then without any other information, the most reasonable expectation for an applicant to hold is to assume only one of the other two fellow applicants would pass the threshold, and take his evaluation about self as the evaluation about the whole proposal team. In this admittedly unrealistic scenario, co-authors are good ('high' types) or bad ('low' types), and the only thing that matters, as far as getting funding, is how many authors are "above the level." Individual co-authors may vary continuously in their ability, but it only matters if one is "good enough."

Another distinction between this paper and the existing research is that there is no private information in this experiment. This is true in two senses. First, individuals are not informed about their true types. Therefore, each subject has the same public information about the game – the payoff function and the costs – at all stages. Second, for the same reason, what subjects have is merely the private "belief" about the types. Therefore, the resulting beliefs about the group type are endogenously formed by "thinking about what others believe about the group type," not by "learning about the group type," since the exogenous type of each individual remains hidden. Bayesian updating conditional on new information is thus minimal.

In this paper, two dimensions of belief exchange are considered. First, I consider three different communication structures in the group part. In all treatments, subjects are aware of the existence of team members and how the payoff is determined by the joint effort and type of the group. In the benchmark "no-chat" treatment, no communication is allowed. The treatment of "full chat" is similar to open meetings, where subjects can freely talk to all other team members through an on-line chat box. The "one-way leadership"

treatment mimics a one-sided communication in which a leader can talk to the non-leaders, who can only receive but cannot send messages.

The other dimension of the belief exchange considers its timing. In particular, in half of the treatments, the individual part is played before the group part. The opposite order is played in the other half of the treatments. The purpose is to check whether the formation of individual beliefs is order-dependent. If being in groups does "bias" an individual's beliefs, I hypothesize the bias is greater when belief formation at the group level occurs before the individual level.

To summarize, the 2×3 experimental design allows me to seek answers to the following questions. Within subjects, I question whether being in a group changes how an individual estimates his performance and that of his own group. Specifically, I compare the beliefs of individuals when they are alone and when they are in groups. Between subjects, I examine how the communication structure and the order of belief formation alleviate or aggravate the effect of group dynamics on individuals' beliefs.

Given the design of the experiment, if group dynamics have no effect on individuals' beliefs, in the group part, subjects should invest as often as in individual part. However, if individuals' belief about the prospect of a group investment had been inflated by the group dynamics, more investments would be made in the group part, especially when they were, optimally, not supposed to make investments. I hypothesize that group dynamics makes an individual believe that the prospect of his own investment and thus the prospect of the group are systematically higher than rationally expected, with an incentive to free-ride and Bayesian updating of the information excluded.

The results of the experiment strongly suggest that the group setting makes individuals more optimistic than they are supposed to be. Subjects are more likely to invest in the group part than in the individual part by 12 to 17 percentage points. On average, an individual's belief of being a high type increases by 36 percentage points. The results are stronger when subjects are communicating with each other before making decisions alone. The results imply that individuals in groups exhibit patterns of decision making that can be interpreted as "groupthink."

The rest of paper proceeds as follows. Section 2 describes the model of the investment environment and its predictions. The experiment design is explained in section 3. Section 4 shows the results. Section 5 concludes.

2.2 The Model and Theoretical Predictions

There are a total of N subjects in an experiment session. Before being informed about the main investment environment, each subject takes an incentivized pretest consisting of ten true-false common sense questions.[6] The last question in the pretest is an additional incentivized question, which asks the subjects to estimate whether they are in the top half or the lower half of the distribution. After the pretest, the subjects are involved in an individual investment environment and a group investment environment.

The raw score of agent i in group j is denoted by $\theta_{ij}^{0}, i \in \{1,2,3\}$. In this experiment, the performance on the test is viewed as the type of the each subject. The median score of all N subjects, denoted by $\bar{\theta}$, separates the high type from the low type. θ_{ij} is defined as

[6] I revise the trivia questions from Biais et al. (2005) and Herz et al. (2014).

the performance of a subject relative to all subjects: $\theta_{ij} = \begin{cases} 1 & if\ \theta_{ij}^0 \geq \bar{\theta} \\ -1 & if\ \theta_{ij}^0 < \bar{\theta} \end{cases}$. In other words, an agent is of high type, $\theta_{ij} = 1$, if his score on the pretest is at the top half; an agent is of low type if otherwise. The setting is intuitive in the following sense. The prospect of the investment project depends on the types of team members in the project. A high type agent has a positive impact on the return on investment, and vice versa.[7] The type of an agent is hidden until the final return realizes. Each subject will involve in both individual decision making phase and group decision making phase. In half of the treatments, subjects start the experiments with the individual phase. The other half starts with the group phase.

2.2.1 Individual decision making

The group subscript j is dropped for the model of individual phase. Each round is independent and thus equally treated in terms of analysis. There is no subscript standing for round. In each round of the individual decision making phase, the profit of investment π_i is equal to an endowment ω plus the gross return R_i minus the cost. Only when the individual chooses to invest do the gross return and the cost occur. In particular,

$$\pi_i = \omega + R_i(\theta_i)e^i - ce^i, \text{ where } e^i = \begin{cases} 1 & if\ invest \\ 0 & if\ not\ invest \end{cases}. \quad (1)$$

The gross return $R_i(\theta_i)$ is a function of individual i's type θ_i:

$$R_i(\theta_i) = \begin{cases} R^+ & if\ \theta_i = 1 \\ R^- & if\ \theta_i = -1 \end{cases}. \quad (2)$$

[7] When applying to sports teams, a high type player increases the winning percentage of his team, while a low type decreases it. When applying to small business started by a small group of team members, a high type may help raise the profits by making right decisions, e.g., location, pricing strategies, etc., while the low type may hurt the profits because of wrong judgments.

Let $p_i \equiv p_i(\theta_i = 1)$ be agent i's belief of himself being a high type. Agent i's expected payoff from investing is equal to

$$E_i(\pi_i) = p_i\left[\omega + R^+e^i - ce^i\right] + (1 - p_i)\left[\omega + R^-e^i - ce^i\right]$$

$$= \omega + [p_iR^+ + (1 - p_i)R^-]e^i - ce^i = \omega + E_i[R_i(\theta_i)]e^i - ce^i.$$

Therefore, an agent would invest if

$$p_iR^+ + (1 - p_i)R^- = E_i[R_i(\theta_i)] \geq c. \quad (3)$$

2.2.2 Group decision making

In the phase of group decision making, subjects are randomly organized in groups of three. I use the sum of the relative performances of group j to represent group j's type: $\Theta_j = \sum_i \theta_{ij} = \theta_{1j} + \theta_{2j} + \theta_{3j}$, $\Theta_j \in \{3,1,-1,-3\}$ since $\theta_{ij} \in \{1,-1\}$. For example, if all three members of the group belong to the top half of the distribution, $\Theta_j = 3$; if all three members of the group are less talented than median, $\Theta_j = -3$. Θ_j is greater than zero if and only if at least two group members are high type.

After each round in the group part, I do not reveal the true group type to the subjects, but I reveal how many group members invested in the last round. This captures most situations of group decision making where efforts are observable, not real prospects of the group project. One might argue there exists learning underway, which makes individual i's expected group type in round t conditional on the decisions made by every group member in the last round $t - 1$. In other words, the solution concept of the group part would be a six-period perfect Bayesian Nash equilibrium.

For the following reasons, I treat each round in the group part independent as in the individual part. First, an individual's decision in the group part only partly reveals what he

"believes" about his own type. This belief is far from the true information of the group type and even his own. Second, in this game, individuals do not have an incentive to free-ride, to misreport their types, and to persuade other to make or not to make investments. Therefore, strategic interaction is minimal. Third, the payoff maximization rule for an individual in a group is independent of other teammates' decisions. Fourth, in each round, the payoff maximization problem remains the same, and thus the payoff maximization rule is the same. Except the cost, nothing varies by round. None of these have anything to do with how other individuals believe about their type. Table B1 in Appendix B shows the chat records of a sample round in a session of full-chat treatment. It appears that their primary concern is the cost in each round, not how they have learned about the group type based on previously observed actions. Finally, I control for the realized group type in the econometric models, which absorbs the rest of possibilities of learning the true group type. Nothing statistically significant is explained by the group type. In sum, in the model, no subscript t is needed. Implicitly, Bayesian Nash equilibrium is the solution concept.

Individual's payoff from the group project depends on the group type and each individual member's investment decision $e^{ij} \in \{0,1\}$. An individual subject's payoff in a given round of the group phase is determined by his endowment, ω, the cost of effort if he decides to make investment, ce^{ij}, and the gross return associated with the outcome resulted from the talent and the joint effort of his group, U^{ij}. In particular, the payoff function is given by

$$\pi_{ij} = \omega + U^{ij} - ce^{ij}. \tag{4}$$

The joint gross return function U^{ij} is given by

$$U^{ij} = R_i(\Theta_j)[\alpha e^{ij} + (1-\alpha)e^{-ij}], \tag{5}$$

where R_i is strictly increasing in group talent Θ_j. In particular, $R_i(3) > R_i(1) > 0 > R_i(-1) > R_i(-3)$. The gross return function is borrowed from Bénabou (2013) except the interpretation of the multiplier R_i. In Bénabou (2013), the multiplier θ is a random shock determined by nature. In this paper, without loss of generality, I let nature determine the talent of group j, Θ_j, which further determines the joint gross return of an individual's final payoff. If nature assigns good teammates to a group, they are a plus to the group's payoff.

The other terms in the gross return function follow Bénabou (2013)'s setting. e^{-ij} is the average investment level of the members in group j other than subject i. $1-\alpha$ represents the interdependence level of the group. A higher interdependence level means a larger part of the investment outcome is determined by other group members.

For instance, when $\alpha = 1$, the interdependence level $1-\alpha$ is 0. $U^{ij} = R_i(\Theta_j)e^{ij}$ if subject i's gross return only depends on his own type. The gross return does not depend on the investment decision of his group members, e^{-ij}. When $\alpha = 0$, the interdependence level $1-\alpha = 1$, subject i's gross return entirely depends on the investment decision of his group members, $U^{ij} = R_i(\Theta_j)e^{-ij}$.

In this experiment, the individual phase captures the setting of zero interdependence. The group phase considers the equally distributed interdependence level, $\alpha = \frac{1}{n} = \frac{1}{3}$. In order to make it directly comparable between individual and group settings, I let $R_i(\Theta_j)$ reduce to $R_i(\theta_i)$ in the individual phase. In other words, other team members' types do not affect the individual's gross return when the interdependence level is zero.

Similar to the individual game, agent i's expected payoff in group is

$$E_i(\pi_i) = p_i\big(\Theta_j = 3\big)\big[\omega + R_i(3)\big(\alpha e^{ij} + (1-\alpha)e^{-ij}\big) - ce^i\big]$$

$$+p_i(\Theta_j = 1)\left[\omega + R_i(1)(\alpha e^{ij} + (1-\alpha)e^{-ij}) - ce^i\right]$$

$$+p_i(\Theta_j = -1)\left[\omega + R_i(-1)\left(\alpha e^{ij} + (1-\alpha)e^{-ij}\right) - ce^i\right]$$

$$+p_i(\Theta_j = -3)\left[\omega + R_i(-3)(\alpha e^{ij} + (1-\alpha)e^{-ij}) - ce^i\right]$$

$$= \omega + E_i[R_i(\Theta_j)](\alpha e^{ij} + (1-\alpha)e^{-ij}) - ce^i.$$

Therefore, an agent i would choose to invest in group if $E_i[R_i(\Theta_j)] \geq \frac{c}{\alpha}$. Since $\alpha = \frac{1}{3}$, the condition for agent i to invest becomes

$$E_i[R_i(\Theta_j)] \geq 3c. \tag{6}$$

2.2.3 Identification assumptions

Throughout the paper, I assume that an agent would adopt the neutral prior belief about the other two team members. That is, an agent assumes the type combination of his teammates is one high and one low. Then the group investing condition (6) can be reduced to a form that is directly comparable to the individual part. An agent would invest if

$$p_i R_i(1) + (1-p_i)R_i(-1) \geq 3c. \tag{7}$$

Equations (3) and (7) are the conditions used to directly compare subjects' decision making in the individual and group environments. Because of this, the other identification assumption lies in how the expected value of group gross return, the left hand side of inequality (7) is calculated. I employ two identification assumptions to calculate the expected gross return in group.

As a benchmark, assume an agent has a prior belief $p_i = p_{-i} = 0.5$. Therefore, $p_i R_i(1) + (1-p_i)R_i(-1) = 0.5 \times R_i(1) + 0.5 \times R_i(-1) = 0$. Agent i would invest if

$$0.5 \times R_i(1) + 0.5 \times R_i(-1) \geq 3c. \tag{7a}$$

In this experiment, I let the impact of each agent in the team symmetric and thus $-R_i(3) = R_i(-3)$ and $-R_i(1) = R_i(-1)$. As a result, an agent with prior beliefs $p_i = p_{-i} = 0.5$ will invest in the group if $c \leq 0$, i.e., when they are subsidized to invest.

2.2.4 Gross return, costs, and the decision rules in actual parameters

In the individual part, R^+ is set to be 50 and R^- is 20. The costs in six rounds are {10, 25, 30, 35, 40, 60} shown in random orders.

In the group part, the gross returns of different group types are given by

$$R_i(\Theta_j) = \begin{cases} 30 & if\ \Theta_j = 3 \\ 10 & if\ \Theta_j = 1 \\ -10 & if\ \Theta_j = -1 \\ -30 & if\ \Theta_j = -3 \end{cases}. \quad (8)$$

In addition to the formula, Table 1 is provided to make sure subjects understand how the types of the whole group and each member's decision jointly determine the payoff. The costs in six rounds are {-13, -7, -2, 2, 7, 13} shown in random orders.

2.3 Experimental Design

2.3.1 Experimental design

The experiment is coded and conducted on the platform of z-Tree (Fischbacher, 2007). Here I briefly describe the design of the experiment. The full instruction, including rules, formula, and examples, are provided in the appendix.

The pretest appears at the beginning of the experiment. Subjects are informed that the score on the test is positively correlated with their final payment. After the pretest, they

are asked to predict whether their scores are on the top half among the subjects. They are informed that a correct prediction will be rewarded a bonus of 2.5 U.S. Dollars.

After the pretest comes the investment environment. In half of the experimental sessions, subjects faced the individual phase first, then the group phase. In the other half of sessions, subjects faced the group phase first. At the beginning of each phase, the formula of the payoffs is provided along with examples. They are informed that the currency in the experiment, Experimental Currency Unit (ECU), will be converted into U.S. Dollars at the rate of $1 per 8 ECUs. In each of the two parts, there are six rounds. Subjects have 50 ECUs as endowment in each round. They are informed that the final payment will be equal to their show-up fee of $5 plus the payoff of one random round in each part. Examples and detailed explanation of payoffs are provided. The group members do not change throughout the experiment.

2.3.2 Communication structures

Presumably, group dynamics could have a more significant effect when the group members are able to affect others' beliefs and thus their decisions, mentally or virtually. Therefore, I hypothesize that the communication structure could make a substantial difference to decisions in the group task.

Three structures of communication are considered. The treatment without communication is the benchmark, in which the group members are not allowed to communicate with anyone.

The other extreme communication structure is a fully connected network. In this treatment, I allow for communication among all three subjects in the same chat room. It captures all "open meeting" communication formats.

Between the two extremes, there are many possibilities to consider. In this paper, the communication structure of interest is "one-way leadership" structure. In this treatment, one randomly assigned subject in each group would be appointed as the leader. The leader can talk to the other two group members, but the group members can only listen. It approximates the scenario in which a strong leader possesses unchallengeable opinions. Figure 1 summarizes the communication structures considered in this paper.

In the treatments with communication, subjects have 90 seconds to communicate before each round of the investment decision. A fixed alias is given to each participant. They are not allowed to reveal their real identities during the communication. Table 2 summarizes the six treatments of the experiment. Within each cell, within-subject effects are observed. Between-subject effects are observed across the cells.

2.4 Results

The experiment sessions were conducted at Rutgers University – New Brunswick in the Gregory Wachtler Experimental Economics Laboratory in May, June, and September 2016. Subjects were recruited via the Rutgers Economics Experiment Recruiting Site[8] enabled by Sona Systems.[9] The total number of subjects over all sessions is 138. Table 3

[8] https://rutgers-econ.sona-systems.com/Default.aspx?ReturnUrl=%2f#.
[9] http://www.sona-systems.com/default.aspx.

shows the number of sessions, subjects, and groups in each treatment. Two separate sessions were conducted for each treatment.

2.4.1 Descriptive Statistics

Table 4 shows the summary statistics of the main variables. Gender is equally distributed. Three-fourths of the subjects have taken economics and statistics, respectively. Sixty-two percent have taken both. The average score on the pretest is 5.01 out of ten points. About 73% of the scores lies between four points and six points, as shown in Figure 2. Sixty percent of the subjects estimated themselves to score on the top half. It suggests the individuals exhibit the "better-than-median" effect.[10] Fifty-five percent of the subjects are high type. Forty-four percent of the subjects correctly estimated their type and thus earned the bonus. Figure 3 shows the distribution of the group type. Because of the randomness of group formation, sixty percent of the groups are positive types.

2.4.2 Primary results

First, let us look at the average contributions assuming subjects are risk-neutral and have prior beliefs $p_i = 1/2$. Table 5 shows the average contributions of each treatment. By design, an average risk neutral profit maximizer should invest in exactly half of the rounds. Therefore, the average contribution would be 0.5 – one being invest and zero being not invest.[11]

[10] The one-tail p-value is 0.008.
[11]For the individual part, I exclude the rounds in which the cost equals 25 in order to maintain the theoretical average as 0.5.

Overall, the average contribution is significantly higher when the subjects are involved in groups (0.61 vs. 0.44). This is true in all but one treatment. The tendency that subjects invest more when making group decision is stronger for those who start the experiment with the group phase. It may suggest that if there is any group effect, it is stronger especially when people do not have a chance to solely evaluate themselves. This will be econometrically examined in a later section.

Second, I look at rounds in which the expected payoff is negative, assuming $p_i = 1/2$. The theoretical prediction for each such round is zero, i.e., no one should invest. Table 6 shows the average contributions in such rounds. It is clear that subjects invested much more in groups than they invested alone when the expected payoff is negative. When it was unprofitable to make an investment, individuals made an investment 14% of the time when the investing alone, as opposed to 46% of the time when being in groups. Again, the tendency for subjects to invest more in groups is even stronger for those who encountered the group environment before the individual environment.

2.4.3 Probit model: probability of making an investment

The first strategy of the econometric analysis is to estimate the probability of making investment using a probit model. The model is given by

$$P\left(e^i = 1 \middle| X^i = x\right) = \Phi(\beta_0 + \beta_1 group_i + \beta_2 groupfirst_i + \beta_3 group_i \times groupfirst_i + \beta_4 chat_i + \beta_5 group_i \times chat_i + \beta_6 chat_i \times groupfirst_i + \beta_7 grouptype_i \times group_i + \gamma' X_i). \quad (8)$$

In equation (8), $P\left(e^i = 1 \middle| X^i = x\right)$ denotes individual i's probability of making an investment, $e^i = 1$, conditional on a set of independent variables X^i. β_1 captures the group effects as $group_i$ is the dummy representing the rounds in the group part. $groupfirst_i =$

1 if the individual starts the game with the group part. Therefore, β_2 is the between-subject effect of starting the experiment with groups, and β_3 would show the effects of being in the group for those who start the experiment with the group part. β_4 is the coefficient of the dummy for communication structure, $chat_i$, with the reference category being the No-chat treatments. So, β_5 shows the effect of being in the group for those who are in the One-way chat and Full-chat treatments. $grouptype_i$ is the realized group type. The interaction term between $grouptype_i$ and $group_i$ will be significant only if some information about the true group type is learned through the group dynamics over the rounds. The vector X_i includes self-evaluation dummy (equals 1 if the individual self-evaluated as a high type), gender, whether the individual has taken economics, and taken statistics courses. The standard errors are clustered at the individual level. The marginal effects are shown in Table 7.

In Table 7, as in Table 5, all models exclude the observations when $c = 25$ in the individual part to ensure equal probability of investment in both parts. In addition, since the theoretical prediction in the group part is that individuals should not invest when the cost is positive, one might worry that the results could be driven by the rounds in which the cost is only slightly larger than the cutoff. Therefore, in model (2) and model (4), I exclude the rounds in which the costs are equal to 2. Consequently, I also exclude the round of $c = -2$ in the group part to maintain the 50% theoretical probability of investing.

The results of the probit estimation show that the group setting increases an individual's probability of making investment by 12 to 17 percentage points. The exclusion of the rounds with costs near the cutoff does not wipe out all the effect. Therefore, the effect of being in the group part is not solely driven by those rounds.

As expected, in all four models, the true group type does not explain the tendency to invest. This may explain why the communication structures do not have a significant impact on the probability of making investments. Individuals communicate, but the learning from the communication is minimal. It justifies the independence assumption.

In model (3), those who self-evaluated themselves as "high" types are 5 percentage points more prone to make investments. However, this seems to be driven by the rounds in which costs near the cutoff are included, since the coefficient is not statistically significant in model (4).

In models (3) and (4), males tend to invest more often than females, which is consistent with previous work that addresses gender differences in overconfidence[12] and risk aversion.[13] Taking economics or statistics does not matter.

2.4.4 Alternative strategy: lower bound and upper bound of beliefs

Knowing that being in a group increases one's tendency invest, the second econometric strategy is to explicitly examine the belief of being a high type for each individual. An individual's true belief is latent. However, given the cost and the investment decision in a given round, an individual reveals his upper bound of belief of being a high type if he chooses not to invest, and vice versa.

2.4.4.1 Cutoff beliefs of an individual: individual part

In the individual part, suppose an agent invested in a given round where the cost is equal to c. His lower bound of belief, p_i, can be inferred by solving for the condition of a

[12] For example, Barber and Odean (2001).
[13] Borghans et al. (2009).

profitable investment. That is, $E_i[R_i(\theta_i)] - c \geq 0$, or $p_i R^+ + (1 - p_i)R^- \geq c$. Therefore, the lower bound of belief is given by $p_i = \frac{c - R^-}{R^+ - R^-}$ if the agent invested when the cost is c. Likewise, if an agent chooses not to invest given the cost c, $E_i[R_i(\theta_i)] - c < 0$, or $p_i R^+ + (1 - p_i)R^- < c$. Therefore, the cutoff point of belief is given by $p_i = \frac{c - R^-}{R^+ - R^-}$.

2.4.4.2 Cutoff beliefs of an individual: group part

In the group part, suppose an agent takes the types of other team members as given. An agent will only have to consider his own type when he compares the expected gross return and the cost.

Suppose an agent has a neutral prior belief about teammates' types (one high and one low.) In particular, his payoff maximization problem is

$$\max_{e^i \in \{0,1\}} E_i(\pi^i) = \omega + E_i[R_i(\Theta_i)]\left(\frac{1}{3}e^i + \frac{2}{3}e^{-i}\right) - ce^i$$

$$= \omega + [p_i R_i(1) + (1 - p_i)R_i(-1)]\left(\frac{1}{3}e^i + \frac{2}{3}e^{-i}\right) - ce^i$$

$$= \omega + e^i\left(\tfrac{1}{3}[p_i R_i(1) + (1 - p_i)R_i(-1)] - c\right) + \tfrac{2}{3}[p_i R_i(1) + (1 - p_i)R_i(-1)]e^{-i}.$$

Agent i will invest if $\frac{1}{3}[p_i R_i(1) + (1 - p_i)R_i(-1)] - c \geq 0$, or $p_i R_i(1) + (1 - p_i)R_i(-1) \geq 3c$. So, the lower bound of agent i's belief is given by $p_i = \frac{3c - R_i(-1)}{R_i(1) - R_i(-1)}$ if the agent invested when the cost is c. Likewise, if an agent chooses not to invest given the cost c, $\frac{1}{3}[p_i R_i(1) + (1 - p_i)R_i(-1)] - c < 0$, or $p_i R_i(1) + (1 - p_i)R_i(-1) < 3c$. Therefore, the cutoff point of agent i's belief is given by $p_i = \frac{3c - R_i(-1)}{R_i(1) - R_i(-1)}$.

2.4.4.3 Evidence from analysis of lower bound and upper bound beliefs

In the individual part, when a subject chooses not to invest, the upper bound belief is $p_i(\theta_i = 1|e^i = 0) = \frac{c-R^-}{R^+-R^-} = \frac{c-20}{50-20}$. Out of the six rounds, I choose the smallest $p_i(\theta_i = 1|e^i = 0)$ and define it to be the least upper bound belief in the individual part, $\overline{p_i}$. If the upper bound belief is greater than 1 or if an individual chooses to invest in all six rounds, the least upper bound belief is replaced by 1.

In the group part, when a subject chooses to invest, I can calculate the lower bound belief by $p_i^g(\theta_i = 1|e^i = 1) = \frac{3c-R_i(-1)}{R_i(1)-R_i(-1)}$. Out of the six rounds in the group part, I choose the greatest $p_i^g(\theta_i = 1|e^i = 1)$ and define it to be the greatest lower bound belief in the group part, $\underline{p_i^g}$. Again, negative numbers are recoded as zero, and numbers greater than one are recoded as one.

Table 8 shows the cutoff points given the individual part and group part given the costs in the experiment. Column (2) are the cutoffs in the individual part. Negative numbers are recoded as zero, and numbers greater than one are recoded as one.[14] The cutoffs in the group part are shown in Column (5).

If an individual's least upper bound belief in individual part is less than the greatest lower bound belief in the group part, it gives evidence in the most conservative way that being in a group enhances individual's belief. Therefore, I define the increase in belief because of group by $\Delta p_i = \underline{p_i^g} - \overline{p_i}$.[15] Figure 4 shows the distribution of Δp_i. The mean of Δp_i is 0.36 (significantly different from zero.) It suggests there is evidence of a within-subject effect of being in a group on individuals' belief of being a high type.

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[14] Fourteen observations are treated this way. Ten invested in all rounds in the individual part except then the cost was 60, the only cost that would even make a high type lose money. Four invested in all rounds.

[15] Of course, the realized Δp_i depends on the chosen gross returns and the costs.

Column (1) and (2) in Table 9 report the OLS estimations on Δp_i. Column (1) suggests that being in the group part before the individual part would increase an individual's belief of being a high type by 24 percentage points, but column (2) seems to suggest this effect is driven by the treatments when communication exists. Those who self-evaluated as high type or those who have taken economics increase beliefs less than others. Those who have taken both economics and statistics increase beliefs more than others. Note that none of the negative coefficients outweigh the statistically significant intercepts in the OLS models, which indicates overall the subjects increase their beliefs of being high type.

Column (3) in Table 9 reports the probit estimation on the probability of having a positive Δp_i. It seems the chat structures do not exhibit significant between-subject differences. The timing of being in a group, however, increases the probability of having a positive change in belief by 14 percentage points.

2.5 Concluding Remarks

A laboratory experiment of a 2×3 design controlling for group communication structure and its order was conducted to detect the effect of group dynamics on individuals' self-evaluation inferred by a series of investment decisions. In the absence of information leakage through strategic interaction, a subject's best policy is to evaluate the type of the group he belongs to as if he evaluates himself since the most neutral expectation of the other teammates' types is that they offset each other.

Data from the experiment sessions indicate strong evidence of overinvestment in the group settings. Compared to the individual part, subjects are more likely to make investments in the group part by 12 to 17 percentage points (Table 7). Assuming a neutral

prior belief about the teammates, a subject's belief of being a high type increases by 36 percentage points on average. Being in the group part first further pushes the increase in beliefs, especially when subjects are communicating with each other. Being in the group part before the individual part makes a subject more likely to have an increase in belief by 14 percentage points (Table 9).

An individual's belief about the group's ability as a whole is clearly inflated simply because of being in a group. The payoff of any individual in the investment project depends on all the group members' efforts and the state of nature, which is the group type in this paper. The group setting makes an individual impose an overoptimistic belief upon the state of nature: a belief he would not hold had he been isolated. Therefore, I interpret the results as experimental evidence of at least some variety of "groupthink." The paper contributes to the literature of motivated beliefs and can be easily replicated using different parameters and controlling for different factors.

The assumption of an individual's neutral estimation about his teammates is worth more discussion. Under this assumption, an individual's overly high estimated group talent comes from his own talent. If this assumption is alternatively released, the increased belief of high type group may be a result of the belief of a better self, a set of better teammates, or both. I would call this the "decomposition" of groupthink. In this paper, I choose one way to decompose it by holding team members' type neutral. One can choose another way to decompose. However, the observed results do not depend on which decomposition is chosen. Therefore, although by this experimental design one is not sure where the overconfidence comes from, it is clear that it is there, and it must be from the individual himself, the teammates, or both.

There are several paths to consider in future work. First is the decomposition just mentioned. It would be interesting to alter the design of the experiment and to hold one's estimation of self or the team members' type constant, thus to enable identification of the decomposition. It would be of interest for anyone who is concerned about the effect of group dynamics, e.g., in the areas of management, group learning in education, or other social groups. The second potential extension is to consider incorporating competition or a tournament. The best hypothesis of the effect of competition is that it would enlarge the effect of group dynamics. However, experimental evidence is still required. Lastly, an analysis of the chat records would be worth exploring. What kinds of language are potentially pushing the groups to overinvest? What sort of language is helpful for facing reality? Can we find a predictor of groupthink by observing how people interact? How do we prevent unwarranted illusions of groups?

The findings in this paper suggest that a significant portion of "investments," including financial investments, commitments to new projects, political movements, as well as time and energy devoted to social groups such as amateur sports teams, non-profit organizations, and religious institutions, may simply be the result of the nature of the a group setting. The group is one of the most common units of decision making. Each group is a living organism. A better understanding of the systematic patterns in group decision making is crucial to analysts and decision makers, and of course economists.

Figure 1. Communication structures.

(a) No chat. (b) One-way leadership. (c) Full chat.

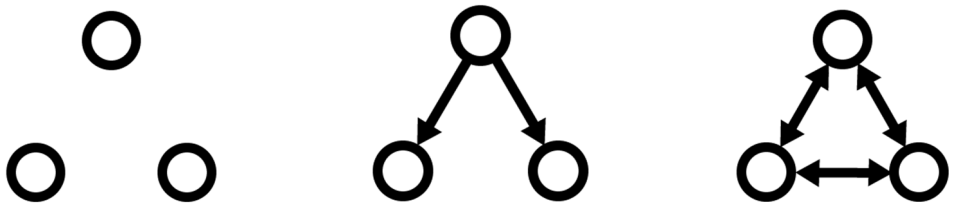


Figure 2. Distribution of the Scores

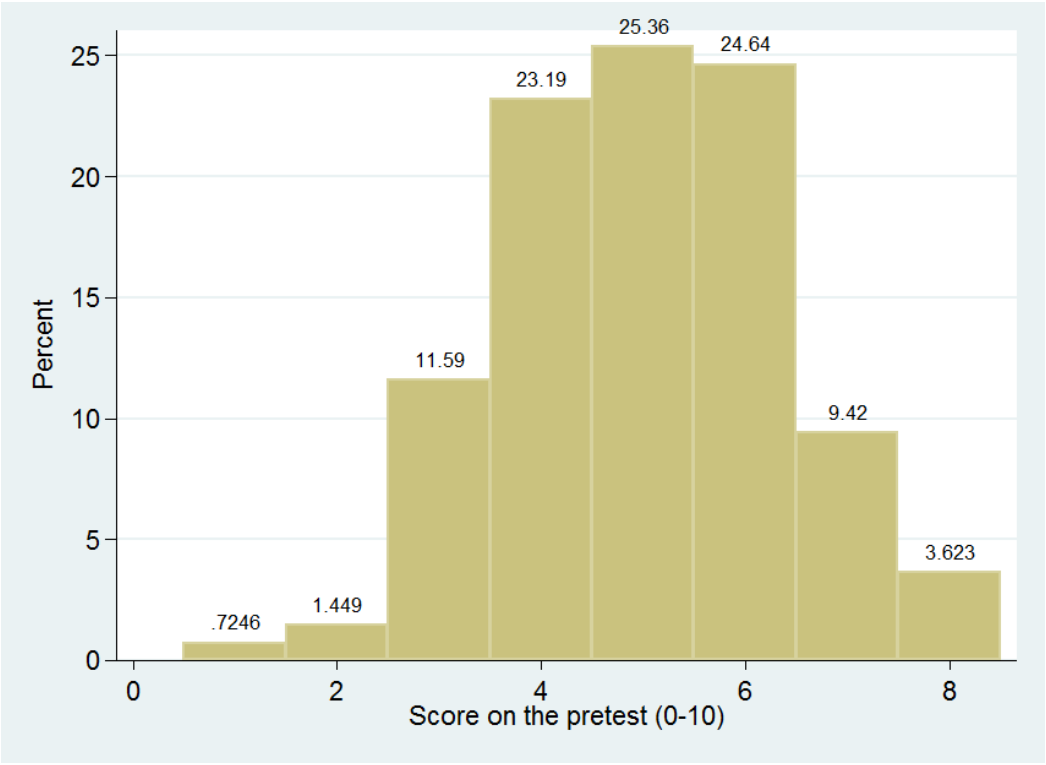
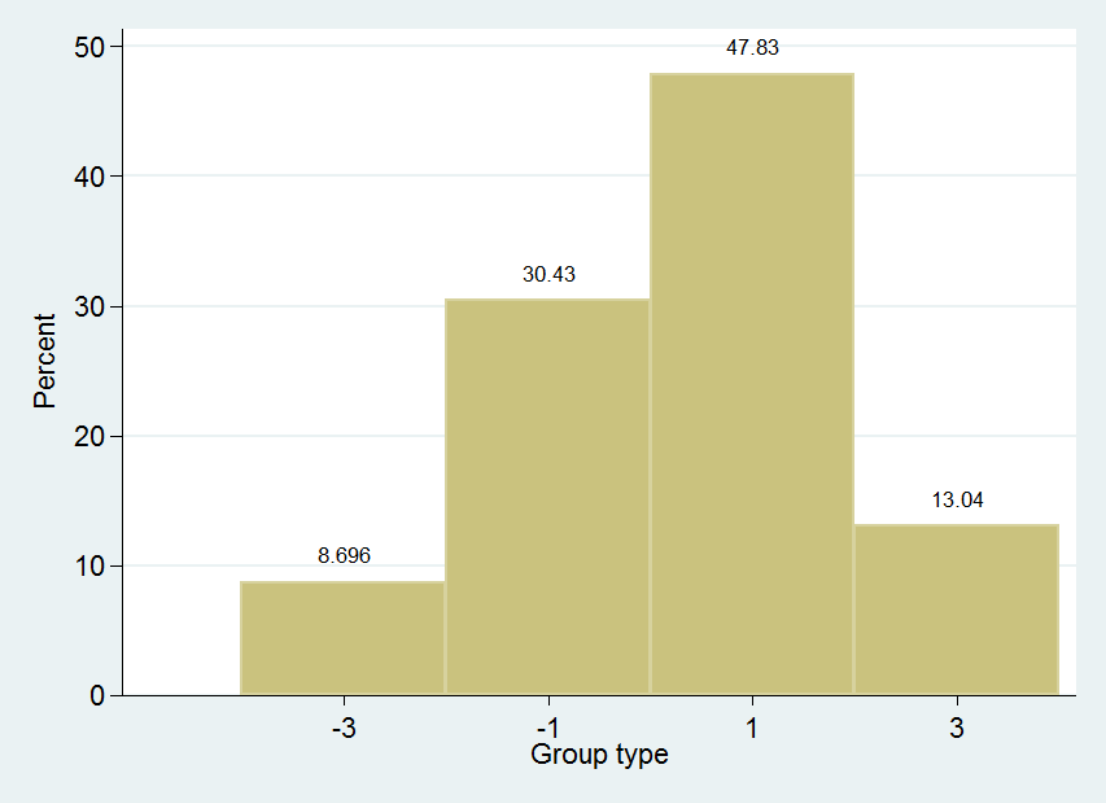


Figure 3. Distribution of the group types



Notes: 1. The total number of groups is 46.

Figure 4. Distribution of increase in belief because of group, Δp_i

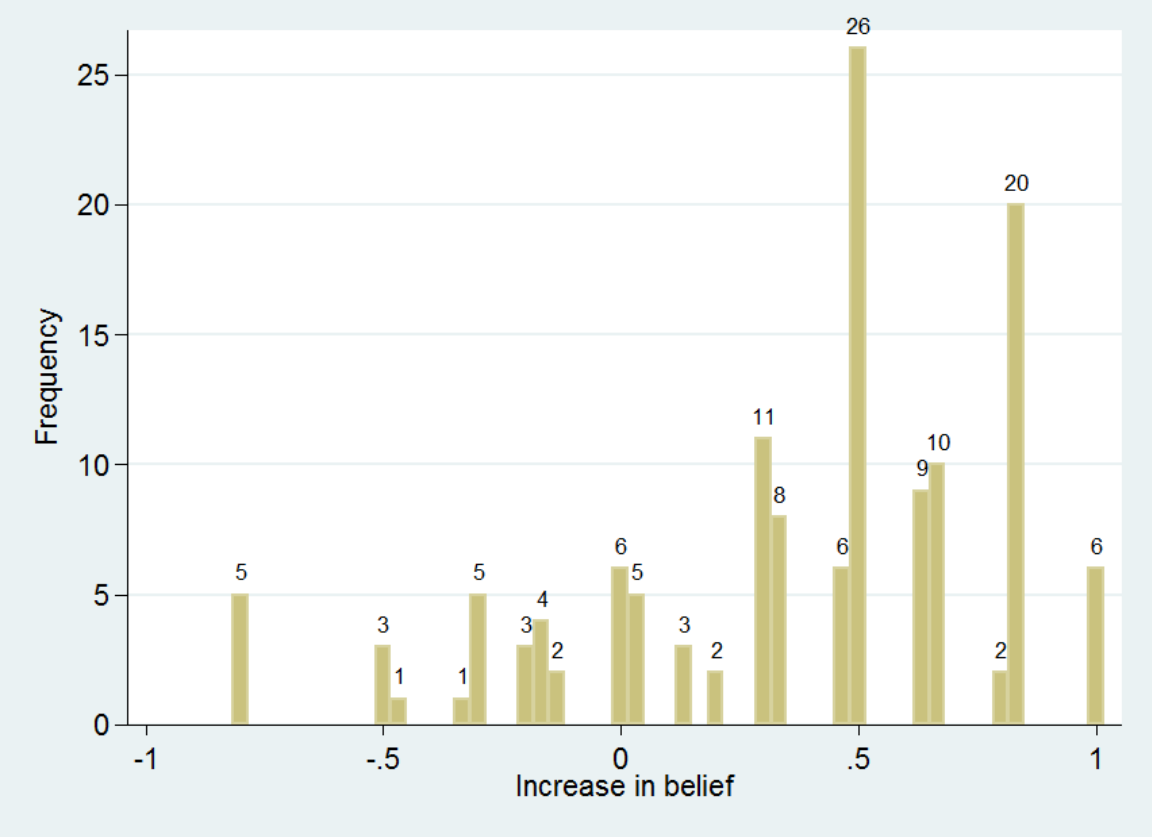


Table 1. Gross returns in the group part

		Number of team members score in the top half			
		3	2	1	0
	3	30	10	-10	-30
	2	20	6.67	-6.67	-20
	1	10	3.33	-3.33	-10
	0	0	0	0	0

Table 2. The 2×3 experimental design

Order / Communication	1: Individual → Group	2: Group → Individual
A: No chat	Treatment A1	Treatment A2
B: One way leadership	Treatment B1	Treatment B2
C: Full chat	Treatment C1	Treatment C2

Table 3. Summary of the treatments.

Treatment	Sessions	Subjects	Groups
A1	2	15 + 9 = 24	8
A2	2	15 + 6 = 21	7
B1	2	9 + 15 = 24	8
B2	2	9 + 12 = 21	7
C1	2	12 + 12 = 24	8
C2	2	12 + 12 = 24	8
Total	12	138	46

Notes: 1. Treatment A: no chats; treatment B: one-way chat; treatment C: full chats. Treatment 1: individual part first; treatment 2: group part first.
2. Subjects number: number of subjects in session 1 + number of subjects in session 2.
3. The first session of each treatment was conducted in May, June 2016. The second session of each treatment was conducted in September 2016.

Table 4. Descriptive statistics.

Variable	Mean	Standard deviation	Min	Max	Number of observations
Score on the pretest	5.01	1.38	1	8	138
Self-eval (to be high type =1)	0.60	0.49	0	1	138
High type (=1)	0.55	0.50	0	1	138
Group type	0.30	1.63	-3	3	138
Earned bonus (=1)	0.44	0.50	0	1	138
Average contribution in individual part (invest=1)	0.48	0.50	0	1	828
Average contribution in group part (invest=1)	0.61	0.49	0	1	828
Profit per round of individual (ECU)	5.70	14.31	-40	40	828
Profit per round in group (ECU)	3.47	13.15	-37	43	828
Male (=1)	0.54	0.50	0	1	138
Taken Econ course (=1)	0.76	0.43	0	1	138
Taken Stats course (=1)	0.72	0.45	0	1	138

Notes: 1. The profits are gross profits without the endowment.

Table 5. Average contributions (exclude $c = 25$ in individual part)

Order / Communication	Column (1) 1: Individual → Group		Column (2) 2: Group → Individual		Column (3)	
	A1		A2		A	
	Individual: 0.47 (n=120) In group: 0.60 (n=144)	Difference: 0.13**	Individual: 0.38 (n=105) In group: 0.65 (n=126)	Difference: 0.27***	Individual: 0.42 (n=225) In group: 0.57 (n=180)	Difference: 0.15***
	B1		B2		B	
	Individual: 0.43 (n=120) In group: 0.48 (n=144)	Difference: 0.05	Individual: 0.34 (n=105) In group: 0.64 (n=126)	Difference: 0.30***	Individual: 0.39 (n=225) In group: 0.56 (n=180)	Difference: 0.17***
	C1		C2		C	
	Individual: 0.52 (n=120) In group: 0.65 (n=144)	Difference: 0.13**	Individual: 0.47 (n=105) In group: 0.65 (n=126)	Difference: 0.18***	Individual: 0.49 (n=225) In group: 0.65 (n=180)	Difference: 0.16***
	Overall: Individual → Group		Overall: Group → Individual		Overall	
	Individual: 0.47 (n=360) In group: 0.58 (n=432)	Difference: 0.11***	Individual: 0.40 (n=330) In group: 0.65 (n=396)	Difference: 0.25***	Individual: 0.44 (n=690) In group: 0.61 (n=828)	Difference: 0.17***

Notes: 1. This table excludes the observations when $c = 25$ in the individual part. Therefore, the theoretical average in each cell is 0.5.

2. n is the number of observations. The number of subjects is $n/6$.

3. *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$. One-tail p-values.

Table 6. Average contributions with negative expected payoff

Order / Communication	Column (1) 1: Individual → Group		Column (2) 2: Group → Individual		Column (3)	
	A1		A2		A	
	Individual: 0.13 (n=48) In group: 0.35 (n=72)	Difference: 0.22**	Individual: 0.07 (n=42) In group: 0.51 (n=63)	Difference: 0.44***	Individual: 0.10 (n=90) In group: 0.42 (n=135)	Difference: 0.32***
	B1		B2		B	
	Individual: 0.08 (n=48) In group: 0.35 (n=72)	Difference: 0.27***	Individual: 0.10 (n=42) In group: 0.60 (n=63)	Difference: 0.50***	Individual: 0.09 (n=90) In group: 0.47 (n=135)	Difference: 0.38***
	C1		C2		C	
	Individual: 0.19 (n=48) In group: 0.57 (n=72)	Difference: 0.38***	Individual: 0.27 (n=48) In group: 0.43 (n=72)	Difference: 0.16**	Individual: 0.23 (n=96) In group: 0.50 (n=144)	Difference: 0.27**
	Overall: Individual → Group		Overall: Group → Individual		Overall	
	Individual: 0.13 (n=144) In group: 0.42 (n=216)	Difference: 0.29***	Individual: 0.15 (n=132) In group: 0.51 (n=198)	Difference: 0.36***	Individual: 0.14 (n=276) In group: 0.46 (n=414)	Difference: 0.28***

Notes: 1. Only include the observations with the theoretical expected payoff is negative. Therefore, the theoretical average in each cell is 0.

2. The number of observations is denoted by *n*. The number of subjects is *n*/6.

3. *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$.

Table 7. Marginal effects of probit models

Dependent variable: $P(e^i = 1 \mid X = x)$	Model (1)	Model (2)	Model (3)	Model (4)
Group	.175*** (.023)	.123*** (.026)	.175*** (.022)	.123*** (.025)
Group first	-.003 (.025)	-.015 (.028)	-.012 (.024)	-.022 (.026)
Chat (No chat = 0)				
One-way leadership	-.045 (.030)	-.048 (.034)	-.047 (.031)	-.054 (.035)
Full chat	.044 (.031)	.047 (.034)	.036 (.030)	.040 (.033)
Group type (ref. -3)				
-1 (2 low 1 high)	.054 (.064)	.039 (.064)	.040 (.061)	.025 (.060)
1 (1 low 2 high)	.019 (.060)	-.012 (.059)	.015 (.058)	-.018 (.058)
3 (3 high)	.084 (.070)	.055 (.071)	.072 (.066)	.040 (.065)
Exclude costs near zero (cost = 2 & -2)	No	Yes	No	Yes
Self-eval (to be high type =1)	No	No	.057* (.029)	.045 (.031)
Male	No	No	.054* (.030)	.070** (.031)
Taken econ course	No	No	-.032 (.038)	-.022 (.040)
Taken stats course	No	No	.013 (.036)	.013 (.036)
Pseudo R-squared	0.0336	0.0236	0.0398	0.0307
Number of observations	1,518	1,242	1,518	1,242

Notes: 1. Standard errors (in the parentheses) are clustered at the individual level.

2. The reference group of group type dummy is -3 (three low types.)

3. The number of observations in model (1) and (3) is equal to 138 subjects times 11 rounds, by the exclusion of $c = 25$ in the individual part.

4. The number of observations in model (2) and (4) is equal to 138 subjects times 9 rounds, by the exclusion of $c = 25$ in the individual part and the exclusion of $c = 2$ and -2 in the group part.

5. *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$.

Table 8. Cutoff beliefs in the individual part and in the group part

Column (1)	Column (2)	Column (3)	Column (4)	Column (5)	Column (6)
Costs in the individual part	Cutoffs $\overline{p_i}$: $\frac{c-20}{50-20}$	Number of observations	Costs in the group part	Cutoffs $\underline{p_i^g}$: $\frac{3c-R_i(-1)}{R_i(1)-R_i(-1)}$	Number of observations
10	-0.333→0	10	-13	-1.45→0	3
25	0.167	38	-7	-0.55→0	5
30	0.333	19	-2	0.2	20
35	0.500	45	2	0.8	34
40	0.667	12	7	1.55→1	26
60	1.333→1	14	13	2.45→1	49

Table 9. OLS and probit estimation of the increase in belief, Δp_i

Independent Variables	Column (1) OLS Dependent variable: Δp_i	Column (2) OLS Dependent variable: Δp_i	Column (3) Probit (Marginal effects) Dependent variable: $P(\Delta p_i > 0)$
Group first (=1)	.238*** (.071)	.167 (.133)	.142** (.063)
Chat (ref.: No chat)			
One-way	.091 (.090)	.007 (.137)	.047 (.079)
Full chat	.059 (.090)	.056 (.144)	-.024 (.084)
Group first × One way	No	.368*** (.116)	-
Group first × Full chat	No	.238* (.135)	-
Group type (ref.: -3)			
-1 (2 lows 1 high)	-.191 (.134)	-.205 (.150)	-.048 (.134)
1 (1 low 2 highs)	-.109 (140)	-.114 (.145)	.019 (.133)
3 (3 highs)	-.051 (.166)	-.038 (.170)	.014 (.161)
Score evaluation (high = 1)	-.162** (.080)	-.164** (.081)	-.081 (.074)
Individual type (high = 1)	-.083 (.090)	-.083 (.090)	-.066 (.082)
Male (=1)	-.068 (.074)	-.071 (.076)	-.065 (.073)
Taken econ (=1)	-.289* (.153)	-.283* (.153)	-.037 (.097)
Taken stats (=1)	-.124 (.121)	-.129 (.121)	.079 (.099)
Taken econ × stats	.362** (.183)	.345* (.183)	-
Constant	.580*** (.136)	.627*** (.180)	-
R-squared	0.1827	0.1922	0.1032 (pseudo R^2)
Number of observations	138	138	138

Notes: 1. Standard errors in the OLS model are clustered at the individual level.

2. *: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$.

Chapter 3

Estimating the Relationships among Education, Cognitive Ability, and Religion: Evidence from NLSY

3.1 Introduction

Education is the most central element of human capital in modern economy. Scholars have examined the private and social returns to education (e.g., Moretti, 2004). It has been shown to be related to better labor market outcomes (Card, 1999) and a number of nonproduction benefits, such as better health (Silles, 2009), less crime behavior (Lochner and Moretti, 2004), and additional positive outcomes (Lochner, 2011). Research establishing causal impacts of education has used multiple approaches to solve endogeneity concerns, including various instrument variable strategies, controlling family background or using family background as instrument, and estimates exploiting outcomes of twins (Card, 1999). Yet, there is little empirical research on the effect of education on religious outcomes.

Religion Recently, economists have linked religion to several economic outcomes.[16] For example, Gruber (2005) finds evidence that higher level of religious

[16] See Chen and Hungerman (2014) and Iyer (2016) for the most recent introductions of this fast-growing field of research in the discipline of economics.

participation is related to higher education and income, lower disability and welfare receipt, better health and marital outcomes. Studies have shown that religiosity has an effect on individual criminal behavior and crime rates,[17] subjective well-being and happiness (e.g., Soydemir et al., 2004), suicide rate (e.g., Huang, 1996), altruistic behavior (e.g., Hrung, 2004), several health outcomes (e.g., Deaton, 2011) and more.[18]

There are several possible mechanisms through which educational attainment could have causal effects on individual's religiosity, which is most commonly measured by religious attendance and self-reported religious beliefs. First, for attendance, education increases wages, and thus increases the opportunity cost of time spent on religious activities. Contrarily, Glaeser and Sacerdote (2008) suggest that the benefit from social connection, which includes church attendance, increases with education and thus provides educated people more incentive to participate in religious activities. Second, for religious beliefs, while the content of secular education may directly decrease religious belief, education may allow deeper understanding of the religious teachings and help people enhance their religious beliefs.

economic study of the interaction of these two topics is relatively new, but long before the recent interest of economists, rather than education, psychologists have focused on the relationship between religiosity and intelligence.[19] They find a consensus that religiosity is negatively related to intelligence (e.g., Lynn, Harvey, and Nyborg (2009) and

[17] Baier and Wright (2001) provide evidence of deterrent effect of religion (religious beliefs and behaviors) on criminal behavior. On the contrary, Heaton (2006) uses past religiosity as instrument and finds no effect of religion on crime rates at the community level.
[18] See Johnson, Tompkins, and Webb (2002) for a review of the relationships between religion and several health outcomes and well-being outcomes.
[19] For example, Howells (1928) and Sinclair (1928) proposed negative relationship between religiosity and intelligence.

Ganzach et al. (2013)). Recently, on the relationship between education and religiosity, psychologists Ganzach et al. (2013) show that when parent's religiosity (measured by parents' religious attendance) is high, children's educational attainment is associated with higher religiosity; when parent's religiosity is low, children's education is associated with lower religiosity. However, since they do not consider endogeneity of education in their models, the effects they suggest are not necessarily causal.

Evidence of causal effects of education on religion in the economic literature is growing but still limited.[20] Cross-sectional data in the United States reveals a positive relationship between schooling years and measures of religiosity. In his ground-breaking introduction to the economics of religion, Iannaccone (1998) uses the General Social Survey (GSS) to investigate numerous theories on economics of religion. He finds a positive correlation between schooling years and church attendance. He also finds the more educated contribute more money in churches, perhaps unsurprising given the high correlation between education and income. Glaeser and Sacerdote (2008) show that people with higher education go to church more frequently overall, but the denominations with higher attendance rates have lower educated members. Arias-Vazquez (2012) shows that attending college attendance decreases one's religiosity (measured by self-reported religious attendance and importance) by estimating the individual fixed effects from the longitudinal Monitoring the Future (MTF) data. He also uses compulsory schooling laws and child labor laws to instrument for education in GSS data and shows similar results. Hungerman (2014) uses Canadian compulsory schooling laws as instrument for education, and finds that an additional year of schooling raises the probability of an individual

[20] See Hungerman (2014) for a discussion of the economic literature on the educational effect on religion.

reporting non-affiliated to any religious group by about 4.5 percentage points. In addition, Gervais and Norenzayan (2012) indicate that analytical thinking, which presumably grows with education, encourages religious disbelief.

An important concern is that religion may as well influence educational attainment. Gruber (2005) uses GSS and the 1990 census data to analyze the effect of religious market density, which is defined by "the share of the population in an area that is of an individual's religion," on religious participation and economic outcomes. He uses area ancestral mix as an exogenous source of variation of religious preference. His findings suggest that higher density of religion market leads to higher religious attendance as well as higher education and other economic outcomes.

Empirically, the main challenge of this estimation is the endogeneity of education. It is possible that certain unobserved individual characteristics make people pursue more education and more (or less) religiosity at the same time. On the one hand, for example, Hungerman (2014) notes that tastes of civil participation enhance both attendance in school and religious activities. Moreover, people who prefer metaphysical thinking may also pursue more education and more religion. On the other hand, the findings of Gervais and Norenzayan (2012) imply people who tend to think analytically might pursue more education but less religion. In the former case, OLS is expected to overestimate the effect of education on religiosity. In the latter case, OLS underestimates its effect.

In this paper, I use two identification strategies, individual fixed effect models and instrumental variable method, to deal with the endogeneity of education as an explanatory variable of religiosity. First, I allow for individual fixed effects in the model to deal with endogeneity by removing the time-invariant heterogeneity. In addition to showing the

effect of education on measures of religiosity, I show that controlling for parents' religiosity in the fixed effects model matters to the results. One worry is thatT fixed effects model may ignore the confounders that are not constant over time. For this reason, I develop alternative instrumental variable strategies to compensate this caveat. I adopt various measures of college accessibility as an exogenous source of variation. This identification strategy relies on the assumption that the accessibility of college from an individual's residence is not related to his religiosity. More details are described in paper.

use the confidential geocode data of 1997 National Longitudinal Survey of Youth (NLSY97 hereafter). The NLSY97 has the great benefits of questions on both religious beliefs and attendance, allowing some insight into possible mechanisms. Also, lots of control variables allowing psychological model of intelligence are paramount. I consider a measure of cognitive ability as another channel through which education may affect religiosity, measured by the score of Armed Force Qualifying Test (AFQT hereafter). Finally, there are sufficient observations to allow adopting IV strategy using geographic information. To do this, I apply for the confidential NLSY97 Geocode Data in order to obtain individuals' residence information. This allows me to calculate the college accessibility at the county level.

The structure of the rest of the paper is as follows. In section 2, I describe the NLSY97 data and the variables I use to measure the factors of interest. Section 3 shows the models of fixed effects. Section 4 provides empirical results. I show the pooled OLS and the panel regression results, along with robustness checks. Section 5 discusses the alternative identification strategy of instrument variables. Section 6 concludes.

3.2 Data

3.2.1 Overview

To conduct this estimation, three measures of the key variables are required: measures of religiosity, measures of education, and the measure of cognitive ability. The NLSY97 includes rich set of questions regarding these aspects. Here in this paper, I include the most recent Round 15 of NLSY97, which has not been analyzed on this topic.[21]

It is necessary to define the term "religiosity" used throughout this paper before proceeding. While the sociological definition of religiosity consists of broad components,[22] most papers in this education-religion literature measure religiosity by religious attendance and religious beliefs. At the same time, most surveys employed in this literature provide questions about attendance and self-reported beliefs. Although they do not capture every aspects of religiosity in its sociological definition, they are concrete, observable, and measurable. By this definition, they are not measurements of one's spirituality and godliness, but measurements of how much an individual's life style and ideology are related to religion.

The NLSY97 is a nationally representative survey started in 1997. It consists of 8,984 youths who were 12 to 17 years old when they were first interviewed in 1997. The longitudinal survey takes a yearly basis. Round 15 was conducted in 2011 and was released in August 2013. The survey contains various topics about the youth, including economic,

[21] The most recent data release, round 16, was released in 2016. It does not contain the questions regarding religious activities and beliefs. Therefore, round 15 is the newest data available on this topic to date.

[22] For example, according to Cornwall et al. (1986), religiosity consists of cognitive component, affective component, and the behavioral component.

social, psychological questions, and provides information about the individuals' family background.

To measure educational attainment, I use the number of full-time schooling years completed as the main independent variable. College attendance is the other variable of interest. Arias-Vazquez (2012) uses MTF data to show that college attendance has a negative effect on religiosity. I check whether the same effect happens in NLSY97 sample by the models using highest degree as an alternative explanatory variable.

3.2.2 Measuring religiosity

Throughout this paper, the general level of religiosity includes two distinct elements: the frequency of religious attendance and an index of religious beliefs. Religious attendance captures how much time people spend on the most identifiable religious activity. The index of religious beliefs constructed from questions in the NLSY97 explicitly reveals the individuals' ideology about religion.

3.2.2.1 Religious attendance days

Religious participation is the most widely adopted form of religious practice. From 2000 to the most recent round, NLSY97 reports the respondents' religious attendance by asking "how often did you attend worship service in past 12 months."[23] The answer is categorical and ranges from 1 (Never) to 8 (Everyday). According to this categorical

[23] In the questionnaire, this variable describes the church attendance days in the last period, $t-1$. In this paper, I run the regression of period t on this last period church attendance variable. The reason is that the educational attainment, highest grade "completed" or highest degree "received," are also describing the accumulative levels that have occurred in the past. In fact, even if I use the right-hand-side variables in period t to explain the church attendance in period t, the results do not change.

response of religious attendance, I generate a continuous variable representing religious attendance days in the past year.[24] [25]

Table 10 shows the percentage of religious attendance over the years using the sample of this paper.[26] As shown in Table 10, compares to 2000, nearly half of the respondents decreased religious attendance in 2011; about only one fifth increased attendance since 2000. The trends of each frequency of attendance are shown in Figure 5. From Table 10 and Figure 5, the number of people attending worship services for zero times increased the most (from 23% in 2000 to 35% in 2011); the number of people attending services for about once a week decreased the most (from 21% in 2000 to 12% in 2011).

3.2.2.2 Index of religious beliefs

I generate an index of religious beliefs from the responses to questions about religion. In 2002, 2005, 2008, and 2011, the NLSY97 records the responses to questions regarding attitudes to religion. There are five true-false questions regarding religious beliefs: 1. "I do not need religion to have good values (reverse coded);" 2. "Religious teachings are to be obeyed exactly as written;" 3. "I often ask God to help me make decisions;" 4. "God has nothing to do with what happens to me personally (reverse coded);"

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[24] I use the following rule to construct the church attendance days. To avoid over-estimation, I choose the lower bound number when imputation is required. To be specific, those who report "never" are assigned zero days of church attendance. "Once or twice" is assigned one day per year. "Less than once a month (3-12 times)" is assigned three days. "About once a month" is assigned 12 days. "About twice a month" is assigned 24 days. "About once a week" is assigned 52 days. "Several times a week" is assigned 104 days. "Everyday" is assigned 365 days in a year.

[25] Since the dependent variable is categorical, an alternative is to conduct ordered probit estimation.

[26] The construction of the sample is described in section 2.4.

5. "I pray more than once a day."[27] In each question, answer 1 means true and answer 0 means false. I conduct principle component factor analysis and use the correlation matrix to construct a weighted index of religious beliefs with mean zero and standard deviation one.[28] Table 11 shows the percentage of religious answers over the years. In 2011, more than one-third of the sample reported decreasing religious belief since 2002, and only one-fourth of the sample reported increasing.

The religious practices and beliefs of youth could be highly affected by the family religious background. To control for family religious background, I use a variable that includes the responses to the same true-false questions from the parents of the youths reported in 1997.[29] These questions were answered only by parents of the youths who were 14 years old or younger as of December 31, 1996. Therefore, in order to control for family religious background of the sample individuals, the sample is restricted to cover the cohorts of those who were 14 years old or younger before the first day of 1997. This restriction does not cause selection problem. The parents' religiosity variable is standardized to mean of zero and standard deviation of one.

3.2.3 Measuring cognitive ability

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[27] One may consider prayer as a religious practice instead of religious belief. I include the response to this question for two reasons. First, it is consistent with the variable of parents' religiosity (described later). Second, factor analysis shows that "I pray more than once a day" accounts for a considerable variation of the index. In fact, when I run all the regressions excluding this question, the results do not change by this exclusion.

[28] Alternatively, I construct another unit weight index by simply adding up five answers (ranging from 0 to 5.) The results from both indices do not differ from each other.

[29] This variable includes another binary-response question: "In the past 12 months, how often have you attended a worship service (like church or synagogue service or mass)?" "1" represents more than once or more and "0" otherwise. Therefore, this variable reveals parental background about both religious beliefs and practices. In fact, when family religious background is divided into religious attendance and religious beliefs and used to control corresponding religious variables, the results are similar and do not change the conclusion of this paper.

Cognitive ability is measured by the AFQT score,[30] coming from the computer-adaptive form of Armed Services Vocational Aptitude Battery (CAT-ASVAB), which was conducted in the first round of the NLSY97. The AFQT scores are standardized to a variable with mean of zero and standard deviation of one.

Although the psychological debate is not the focus of this paper,[31] there is room for discussion about the essential role of such IQ-like score (e.g., Heckman, 1995). While psychologists' initial focus is on the general inherited and less immutable intelligence, or the so-called "*g*", the measured cognitive ability is believed to grow with education. Therefore, the AFQT score itself should be better viewed as a measure of educational achievement (e.g., Cawley et al., 1997; Winship and Korenman, 1997; Hansen et al., 2004).

However, the AFQT score is still frequently employed as a proxy in social-science literatures to represent individual's cognitive ability, aptitude, or intelligence.[32] Winship and Korenman (1997) states that "both education and early IQ have important effects on later intelligence as measured by the AFQT."[33] In addition, according to Cawley et al. (1997), the AFQT and "*g*" are highly correlated. In this paper, the correlation coefficient

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[30] AFQT score is a percentile score coming from a weighted average of mathematical knowledge (MK), arithmetic reasoning (AR), word knowledge (WK), and paragraph comprehension (PC) in the ASVAB. In addition to these four areas, ASVAB also includes general science (GS), electronics information (EI), automotive and shop information (AS), mechanical comprehension (MC), and assembling objects (AO). These additional fields are closer to skills of the test takers and thus not included in the AFQT.

[31] There was a debate on the correlation between intelligence, race, and achievement after the book "*The Bell Curve*" by Herrnstein and Murray was published in 1994.

[32] For example, treating the AFQT score as ability, Carneiro and Heckman (2002) control for ability to estimate the relationship between family income and college attendance. Altonji et al. (2012) compare cognitive skills, represented by the AFQT scores, across the cohorts in NLSY79 (1979 National Longitudinal Survey of Youth) and NLSY97.

[33] Page 231 in Winship and Korenman (1997).

between 2011 schooling years and the AFQT score is 0.59.[34] It preliminarily reveals that the AFQT score is not an inappropriate measure of cognitive ability.

In summary, as long as we do not overplay the interpretation of the AFQT, it provides a different angle from schooling years and highest degrees to take intellectual ability into account. We do see interesting difference from the results.

3.2.4 Sample description

This section describes how I construct the sample of this paper. What determines the sample size and the number of observation is the availability of the key variables. In order to account for the effect of the educational variables on the various religious variables, and to control for family religious background and other important demographic characteristics, missing values inevitably have effects on the sample size.

As described in section 2.1, the biggest drop of the sample size from the full NLSY97 sample is due to missing values of parents' religiosity, which results from the fact that only the parents of youth of 14 years old or younger have the responses to the questions about religion. It does not raise sample selection issue. The second largest drop is due to the missing values of the AFQT score. The relevant religious variables do not differ by inclusion or exclusion of AFQT scores.[35]

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[34] For the full sample in the NLSY97, the correlation coefficient is almost the same with the sample I construct in this paper.

[35] Considering the representativeness of the NLSY, Altonji et al. (2012) discuss the attrition and missing data in NLSY79 and NLSY97. They find small difference between the full sample and the stayers in certain observable characteristics according to attrition. They show that some of the observable variables for those who with AFQT scores differ from those who without AFQT scores. However, since the observable results of AFQT takers and the full sample have similar features, the differences are acceptable (see Altonji et al. (2012), Table B2 and Table B3 in Appendix B). Moreover, they provide evidence that the joint distributions of the AFQT score and some observable variables do not depend on the age at which the individuals took the test (see Altonji et

Table 12 summarizes the effects of the missing data on the sample size. It shows that I control for the religion-related variables, which include religious preference in 1997, parents' religiosity, as well as demographic characteristics including race, marital status, living in urban area or not, and having children or not. The final sample size in this paper is 4,480 and the sample accounts for 49.9 percent of the full NLSY97 sample. While the final sample represents just fewer than 50 percent of the overall sample of the NLSY97, it is 83 percent of those who were eligible, i.e., 14 years old and younger as of 12/31/1996, for the relevant bank of questions.[36]

As for the number of observations, when running religious attendance regression, there are observations of 12 years, which include 45,439 observations. The numbers of observations per individual is 10.1. For the index of religious beliefs, since the questions about religion were asked in 2002, 2005, 2008, and 2011, the observation years are restricted to these four years. The total number of observations is 13,371. The number of observations per individual is 3.0.

3.2.5 Descriptive Statistics

Table 13a shows the 2011 cross-sectional descriptive statistics of the key educational and religious variables. I report the means and standard deviations of religious attendance days, the index of religious beliefs, the parents' religiosity index (reported in

al. (2012), Table B4 in Appendix B). In this paper, since the cohorts included are smaller than the full NLSY97 sample, the problem of test-taking age would be even smaller.

[36] In fact, not all people in my sample were 14 or less in the first round. First, it is natural that some people who were 14 years old in 1996 became 15 in 1997. Second, in the constructed sample, there are a small part of people aged 16 or 17 in round 1 of the interview, which should have been taken in 1997. This was because some of the round 1 interview was taken in 1998 due to administrative reasons.

1997), highest grades completed, and the AFQT scores (reported in 1997). The average schooling years in 2011 is 13.8 years. About 36 percent of the sample have bachelor's degree or above. Religious attendance is on average 15.5 days per year or 1.3 days per month in the past year.

Table 13b shows the descriptive statistics of the control variables of the constructed sample. Age, gender, marital status, childbearing, living area (urban area or not), and religious preference in 1997 are reported. The mean age of the sample in 2011 is 28.3 years old. Forty-three percent of the sample are married, and 45 percent of the sample have children. Seventy-eight percent of the sample live in urban areas. Sixty-one percent of the sample are whites. As for their early religious preference, Roman Catholic and Protestants of all denominations account for 85 percent of the sample.

3.3 Econometric Models

3.3.1 Identification strategy: individual fixed effects

To estimate the effect of education on religiosity, the key challenge is endogeneity of education. In particular, there might be factors affecting both education and religiosity. Some of these factors are observable. For example, as Arias-Vazquez (2012) points out, children from religious families may be more likely to stay in high schools and thus are more likely to go to college, since religious families might be better at preventing high school children from dropping out. A solution to this problem is to control for parents' religiosity.

There might also be some unobserved individual characteristics affecting both education and religiosity. For instance, people with a preference for metaphysical thinking

might pursue more education and more religion. When a model ignores such a confounding factor, the effect of education on religion will be upward biased. On the contrary, Gervais and Norenzayan (2012) and Mocan and Pogorelova (2014) argue that people who tend to think analytically might pursue more education and less religion. If their argument is correct, the effect of education on religion will be downward biased. Therefore, the expected sign of the bias is ambiguous.

Assuming the unobserved variables are constant over time, the longitudinal property of the panel data allows me to estimate a fixed effects model. In particular, I assume the unobserved individual characteristics affecting education and religiosity are included in time-invariant individual heterogeneity term ϕ_i in the following model:

$$R_{i,t} = \alpha Edu_{i,t} + \beta X_{i,t} + \phi_i + \tau_t + \epsilon_{i,t}. \qquad (9)$$

For individual *i* in survey year *t*, the dependent variable $R_{i,t}$ is religious attendance days or the index of religious beliefs, depending on specification. Both religious attendance days and the index of religious beliefs are treated as continuous variables. $Edu_{i,t}$ is educational attainment, namely, highest grade completed or highest degree received. $X_{i,t}$ is a set of individual-level control variables including age, gender, race, marital status, childbearing, religious preference, rural or urban area dummy, and parents' religiosity. τ_t is year effect. ϕ_i is the time invariant heterogeneity. Because of the identification assumption that the unobserved characteristics ϕ_i is time-invariant for each person *i*, it has no subscript *t*. That

is, the confounder of the effect of education on religiosity is constant over the years. $\epsilon_{i,t}$ is the error term.[37][38]

For those control variables that are time variant, they are included in fixed effects models. For those time invariant ones, they are only included in pooled OLS model, which is shown in section 4.1 as a benchmark. In the fixed effects models, selected time-invariant control variables are used to divide the sample into different set of subsamples. Specifically, I divide the sample by parent's religiosity, cognitive ability, and gender, and run the fixed effects models separately. The results of subsample fixed effects model are shown in section 4.3.

3.3.2 Long change fixed effects estimation

In most cases, the highest grade completed and the individuals' age increase one-for-one simultaneously, at least up to some point when they finish education. One might worry that this mechanical manner in fixed effects model would reduce or limit the variation that can be identified. Therefore, I deal with this one-year-by-one-year relationship between highest grade completed and age in two ways.

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[37] In fact, a cohort effect C_c could be included in the model. However, since the sample used in this paper only consists of three birth-year cohort, the cohort effect is expected to be minor. Therefore, I only include age in $X_{i,t}$ as a control variable.

[38] Ideally, adding an individual-specific trend, $g_i * t$, to account for the source of heterogeneity would help to gain the results that are closer to causal effects. It is possible that there is a systematic difference of religious trend between people with different education level. However, since the highest grade completed increases one year per year of age for most people, the individual specific trend would only identify non-traditional students whose age and schooling year do not increase at the same time, which is a particular group and generates concerns about sample selection and external validity. Therefore, I choose not to include this $g_i * t$ term in this paper.

First, I include highest degree received as an alternative explanatory variable. Unlike highest grades, the time interval between one's degrees and age is less prone to have one-to-one relationship.

Second, to check whether this relationship cause any problems on the estimation, I run the same fixed effects model with only the observations in the first and the last years. We expect the results to be sharper than what is in the model with full observations, as the changes of the religious variables and the educational years are greater. The discussion of the results is shown in section 3.4.2.3.

3.4 Empirical Results

3.4.1 Benchmark results: pooled OLS

First of all, I examine the general correlations between the measures of religiosity and education without dealing with endogeneity. By running the conventional pooled OLS, Table 14 shows the relationship between religious attendance days and educational variables, including highest grade completed, highest degree received, and the AFQT score. Table 15 runs the same model with the dependent variable being the index of religious beliefs.

Table 14 confirms the positive relationship between religious participation and schooling at the cross-sectional level, as appears in Iannaccone (1998) and Gruber (2005). Religious attendance days per year is positively and significantly correlated with highest grade completed and highest degree received. The AFQT score also has positive and significant relationship with religious attendance. Not surprisingly, parents' religiosity has a strong and significant positive impact on children's church attendance. Controlling for

parents' religiosity (model (iv) to (vi)), the relationship between educational attainment and cognitive ability becomes a little smaller but still positive and significant. Moreover, it can also be seen that married people and female goes to church more than non-married and male, respectively. Living in urban area is generally negatively related to church attendance. Also, church attendance days decreases with age. The ones who have children appear to have less church attendance days.

In Table 15, the dependent variable is the index of religious beliefs. What is interesting is that, although the relationship between religious beliefs and highest degrees are mixed and somewhat less significant, higher AFQT score is significantly associated with lower index of religious beliefs, even when parent's religiosity is controlled. This relationship confirms the findings of most psychological research. Again, married people and female has higher degree of religious beliefs than non-married and male. People live in urban areas have lower level of religious beliefs than those who live in rural areas. The index lowers with age. The ones who have children appear to have slightly higher religious beliefs index.

In models (vii) and (viii) of Table 14 and Table 15, the AFQT score is included as well as educational attainment as an explanatory variable. In both cases, highest grade completed is still significant. The AFQT becomes less significant in Table 14 while educational attainment is still significant. Highest degree becomes less significant in Table 15 while AFQT and highest grade completed are still significant. It gives an idea that although without causal links, cognitive ability and educational attainment have separate relationships with religiosity.

3.4.2 Panel models

In model (1), running pooled OLS means that we ignore individual effect ϕ_i, or assume that the individual effect ϕ_i is uncorrelated with the explanatory variables $Edu_{i,t}$ and $X_{i,t}$. However, as discussed before, we worry that there would be some omitted factors affecting individual's religiosity. Fixed effects analysis allows us to consider individual time-invariant heterogeneity ϕ_i, and gives us a consistent estimation.

3.4.2.1 Fixed effects estimation

Table 16 shows the results of the fixed effects estimation. In column (i), the highest grade completed has a negative and significant effect on church attendance days. This is consistent with recent research which suggests negative causal effect of educational attainment on religious attendance days. Each schooling year makes an individual decrease church attendance per year by 1.15 days. With the average highest grade completed being 13.76, on average, it is equivalent to 15.8 days less of religious attendance per year after one completes education.

Column (ii) uses the highest degree received to explain the variation in religiosity. Similar to the effect of highest grade completed, the effect of highest degree received on church attendance is opposite to pooled OLS results, and shows that the higher degree received, the less often the youths attend worship services. Compares to the reference group, high school graduates, college attendance makes people decrease about two days less attending religious service. The difference between none degree and high school graduate is even larger. It reports more than seven days less per year after one gets high school degree.

The effects of educational attainment on the index of religious beliefs are shown in column (iii) and (iv). For highest grade completed, each schooling year causes an individual to decrease religious-belief index by 0.019. It is equivalent to an average decrease of religiosity index by a quarter of standard deviation after one completes education. There is also a negative effect on the individuals' index of religious beliefs for each higher degree received. College attendance decreases people's religious belief index by 0.06 standard deviation.

3.4.2.2 Discussion

The results of fixed effects estimation tell us that the OLS overestimates the effect of educational attainment on religious attendance and religious beliefs. In particular, assuming endogeneity of education is a result of omitted variable bias which is fully captured by the individual fixed effects, this implies that the OLS biases the estimate upward. The results of pooled OLS on the index of religious beliefs are already negative. Compare to OLS, the fixed effects estimates on the index are even smaller. Therefore, the OLS estimates on the index of religious beliefs are again biased upward.

The direction of the bias of the OLS estimation suggests that the effects of unobserved individual characteristics on education and on church attendance are of the same direction. That is, if the omitted variable increases education, it increases measures of religiosity as well. As for religious attendance, a possible explanation is the benefit from social connection increases with educational attainment and in the form of religious attendance (Glaeser and Sacerdote (2008)), or the tastes of civic participation (Hungerman

(2014)). As for religious beliefs, it could be the individual's taste of metaphysical thinking or certain subjects such as philosophy and theology.

3.4.2.3 Long period change model

Table 17 reports the results of the same models on the observations of only the beginning year and the last year available. The source of variation here is from the long-period changes in religious attendance and beliefs, and the long-period changes in educational attainment. In other words, the immediate responses in these behaviors are not required in this model to identify the effects. If the responses on religious variables for some reason take some time to reflect on the effect of education, then this model would capture the effects more precisely.

In Table 17, almost all the coefficients of the educational attainment variables are greater than the ones in the original fixed effects model shown in Table 16, which includes the observations in all available years. This alleviates the worry that the identification could only come from a peculiar group. Therefore, it further reassures the idea that in general the educational attainment reduces religious attendance and beliefs.

3.4.3 Time-invariant control variables

In the fixed effects models, some important time-invariant control variables are not applicable. Therefore, the sample is divided into subsamples in various ways to show how these constant control variables have impacts on the estimation. I run the fixed effects models on the sample with stronger family religious background versus weaker background, using the median of parents' religiosity as the threshold. Similarly, higher

cognitive ability and lower cognitive ability are defined by above and below the median of the AFQT score. I also include regressions on only male and female. Table 18a shows the effects by subsamples on church attendance. Table 18b shows the effects by subsamples on the index of religious beliefs. In these cases, the other demographic control variables do not account for the differences we see in the effects of educational attainment.[39]

3.4.3.1 Subsample by family religious background

When dividing the sample by the family religious background, it shows that the negative effects of educational attainment on religious participation and religious beliefs are much stronger for those who come from families that are more religious.

For religious attendance, according to column (ii) and column (iii) in Table 18a, the difference between the effects on people from stronger and weaker family religious background is about 1 day for each additional schooling year, or 13.8 days after they complete education. Similarly, the highest degree received also sees difference by religious background. The coefficients of highest degree received on people from weaker religious families are smaller and insignificant. For the index of religious beliefs, as shown in column (ii) and column (iii) of Table 18b, the difference of the effects on the index has a similar pattern.

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[39] In fact, as shown in Table 16, most control variables are not significant in fixed effect models. The reason might be that the periods are not long enough to account for enough variation to show their influences. The control variables tend to have higher significance when I run the subsamples of which the educational attainment has lower explanatory power. For instance, in model (iii) of Table 18a, the control variables have higher *p*-values, while in Table 16 they are insignificant as in the full sample. However, when they are significant, the signs are consistent with the full sample model. Thus we do not consider the differences come from the control variables.

In summary, educational attainment influences people's attitudes toward religion. The effect is stronger for people from more religious families. This result does not support the findings in Ganzach et al. (2013), which argues that when the religious background is strong, education increases individuals' religiosity, and vice versa.

3.4.3.2 Subsample by AFQT scores

In column (iv) and column (v) of Table 18a and 18b, the sample is divided into subsamples by their AFQT scores. In Table 18a, the effects of the highest grade completed on church attendance for higher and lower AFQT scorers are both significant, and the effect for people with lower AFQT scores is somewhat larger, but the difference is not as large as it is when divided by religious background. As for the highest degree, the effect of college attendance on church attendance is higher for people with higher AFQT scores, while the effect for people with lower AFQT scores is less precise. However, none of them are larger than the effect of college attendance on church attendance for the full sample.

In Table 18b, on the contrary, the highest grade has slightly larger and more significant negative effect on religious beliefs for people with higher AFQT scores, while the coefficients of college attendance along with other degrees are larger (in absolute values) for the low AFQT group.

In short, the subsample analysis by the AFQT scores is less consistent and less informative than by family religious background. It suggests that if cognitive ability has a causal effect on religiosity, what is shown in the fixed effect model is not enough to correct the omitted variable bias from OLS. Therefore, an obvious causal link between cognitive

ability and religiosity is not found. Again, it does not support the findings in Ganzach et al. (2013), which suggests a negative direct effect of intelligence on religiosity.

3.4.3.3 Subsample by gender

Subsample by gender shows that there is a clear distinction between the effects on male and female. The negative effects of educational attainment on religiosity are stronger for male. Each higher grade completed causes male participate about 0.3 days per year less than female, or 4.1 days per year less than female after graduation. Effects of education on religious beliefs are also greater for men. This is consistent with the fact shown in OLS estimation, in which the coefficients of male dummy are always negative.

3.5 Alternative Identification: Instrument Variables

If the omitted variable bias cannot be completely erased by the individual fixed effects due to unobserved variables that are not constant over time, the fixed effects method may not be able to fully capture the causal effect of education. Therefore, I propose using various measures of accessibility of college as instrument variables to estimate the effect of education on religious outcomes. To begin with, I will discuss some efforts done in recent years.

3.5.1 Literature review and candidate instruments

Hungerman (2014), Arias-Vazquez (2012), and Mocan and Pogorelova (2014) use compulsory schooling laws to instrument education. All of them find negative effect of education on religion and find OLS biased upward. However, LATE (local average

treatment effect) interpretation implies that the estimation can only apply to people with education under high school level. The limit of using compulsory schooling laws as instrument is that the effect of higher education on religiosity, which is the interest of many, is not captured.

Second, Kortt et al. (2012) use parents' education as IV of education. They find positive relationship between education and religious attendance under OLS estimation, but find no causal relationship once they treat education as endogenous and use parents' education as instrument. However, since parents' own education has an impact on their own religion, which further affects youths' religiosity, parents' religiosity might not be exogenous enough.

Another candidate instrument for education is college proximity. Addressing the estimation of returns to schooling, Card (1995) argues that the differences in the accessibility of college could be a source of variation that is exogenous to the labor market outcomes. The rationale behind this method is that the cost of transportation affects the opportunity cost of getting education, especially for people from low income families. The identification assumption is thus college proximity influences labor market earnings only through its impact on the individuals' education decision. He also uses the interaction term of college proximity and family educational background as an alternative instrument. Both instruments he employs are shown to be effective to correct the bias of OLS when estimating the effect of education on labor market outcomes.

3.5.2 Instrument variable: college accessibility

In order to capture the effect of higher education on religiosity, I adopt a similar instrument variable to Card's college proximity – college accessibility. Below I describe how I construct the two measures of college accessibility.

With the county-level NLSY97 Geocode data, I construct two measures of college accessibility. First, compare to counties with fewer total seats available (relative to total young population), counties with more seats available in colleges provide more accessibility of education to the potential students. Using the 2000 census data, I calculate the ratio of number of seats (enrollment) in colleges relative to the population from 18 to 29 years old in the individual's residential county.

People live in areas with more colleges are presumably more possible to get higher education than those who live in areas with fewer colleges around. Second, in order to consider the variation of geographical education premium, I consider the percentage of people with bachelor's degree or higher in a county. Because the percentage of people go to college could still be endogenous to educational attainment, I use this variable dated back to 1990 to alleviate the worry. The rationale is that a certain part of difference in educational attainment comes from geographical difference. Recognizing this cannot remove endogeneity entirely, I view this instrument as a last attempt.

Table 19 lists the potential instrument variables with their descriptive statistics. The last column displays the first-stage coefficients of the instruments in models for schooling years, controlling for other individual characteristics. Both candidate IV's seem to have direct impact on the educational attainment of the individuals in the sample.

The exclusion restriction requirement of college accessibility depends on whether the accessibility of college in the residential area is uncorrelated to its religious outcomes.

There are possibilities that this exclusion restriction may not hold. One is the possibility that people of certain religious level have selected to live in areas that have more colleges.[40] If families that care more about education select to live in counties that provide more seats in college, and if their emphasis on education cause them to have less religiosity, then children from these families might have higher education and lower religiosity at the same time. In this case, OLS biases the effect of education downward, and vice versa. Taking this caveat into account, Rouse (1995) argues that if the location decision is made by the parents, not the children, then the caveat is alleviated and the validity of this instrument cannot be ignored. The bottom line is the exogeneity needs to be recognized conditional on families' background and geographic effects.

A second caveat of this instrument is that religious institutes in counties with higher college accessibility may face more secular competition. Activities associated with college life, no matter on campus or off campus, may have an impact on the attendance rates of religious institutes. If this is true, when a county has more college students, the number of religious adherents might be lower.

If at least one of the caveats is true, then the number of college students should be correlated with the number of religious adherents. To check this, I use the 2000 county-level data of Religious Congregations and Membership Study from theARDA.com to match with the Census data. I check whether there is a significant correlation between number of college students and the number of adherents by county,[41] controlling for the

[40] I thank Douglas Norton for bringing up this issue. Another one is secular competition for religious attendance.

[41] I adapt the "adjusted total number of adherents" computed by Finke and Scheitle (2005).

population. There is no correlation between the two.[42] Therefore, it shows the accessibility of college has no direct effect on religious outcomes.

3.5.3 Model and results

Given the college accessibility has no direct effect on religious outcomes, it can therefore be used to instrument for education to explain religiosity. The IV estimation model would be as follows:

$$R_i = \delta Edu_i + \gamma X_i + \mu_i, \qquad (10)$$

$$Edu_i = \theta X_i + \pi C_i + \varepsilon_i. \qquad (11)$$

Again, R_i is individual i's religious outcome. Edu_i is i's educational attainment. X_i is a vector of characteristics, not including i's college accessibility, C_i. The principle of relevance holds when the coefficient π is not equal to zero, as shown in the last column of Table 19. Exclusion restriction holds when $E(C_i\varepsilon_i) = 0$. The satisfaction of exclusion restriction and relevance of college proximity guarantees that IV estimator generates unbiased estimate of δ.

I conduct the IV estimation using the most recent round (2011 data) of the NLSY97, since it has the most complete educational attainment. The results are shown in Table 20. Column (1) shows the coefficients of conventional OLS estimation. As in Table 14 and Table 15, conventional OLS shows positive relationship between schooling years and religious attendance, and the relationship between schooling years and the index of religious beliefs. The IV estimates, however, gives opposite estimation of the effects of

[42] The results of the regression are not shown in the paper and are available from the author upon request.

educational attainment. In panel A, the effect of an additional schooling year appears to decrease religious attendance days by 3.82 to 4.86 days per year, but the coefficients are imprecise. In panel B, the IV estimation reveals that an additional year causes a decrease in the index of religious beliefs by 0.5 to 0.72 standard deviation. The IV estimation reverses the sign of the coefficient which is positive in OLS. Both estimates are significant.

The IV estimations are consistent with the findings of the fixed effects model. Like the fixed effects estimation, and IV estimations show that OLS upward biases the effect of educational attainment on religious outcomes. Both strategies provide evidences that the hidden factors which cause endogeneity drive educational attainment and religious outcomes to the same direction, and that educational attainment causes religiosity to decrease.

3.6 Conclusion

Recent research has shown negative relationship between education and religion. This paper provides additional evidence to the literature of the relationship between education and religion with cognitive ability considered, using a nationally representative dataset that is less exploited on this topic. Consistent with the findings of the literature using cross-sectional estimation on GSS data, the pooled OLS results show positive relationships between educational attainment and religiosity. In order to deal with endogeneity, I adopt fixed effects models and use two measures of college accessibility to instrument for education. The results suggest that the educational attainment has negative impacts on both religious attendance and beliefs. Cognitive ability, which is positively

related to educational attainment, only has an imprecise negative link with religious beliefs, but not with religious attendance.

The fixed effects models suggest that each schooling year decreases 1.2 to 2.2 religious attendance days per year, or 15.8 to 29.7 days less after one completes education, and decreases the index of religious beliefs by 0.019 to 0.029, or 0.25 to 0.4 standard deviations after graduation. These results are especially stronger for those who come from families with stronger religious background. IV estimation gives larger estimates. Each schooling year is associated with 3.8 to 4.8 less religious attendance days per year, and a decrease in the index of religious beliefs by 0.5 to 0.7 standard deviations. The fixed effects results are robust to the length of periods between observations.

At the cross-sectional level, cognitive ability represented by the AFQT scores has no significant relationship with religious attendance days, but interestingly has a strong negative relationship with religious beliefs. However, the fixed effects estimates of the effects of educational attainment on religiosity show little difference between high AFQT scorers and low AFQT scorers. Cognitive ability does not show meaningful impacts on religiosity in IV estimations either.

Lastly, there are some thoughts for future research agenda. First, a branch of literature of economics of religion is based on structural modeling of time allocation. Azzi and Ehrenberg (1975) set a classic model of religious participation that is in the fashion of labor supply model and is followed by many theoretical and empirical works. When it comes to time allocation, taking educational background, income, occupation, and industry into consideration, the opportunity cost of church attendance can be modeled. Using the

individual time allocation in micro-data to conduct further research is an interesting way to investigate this question.

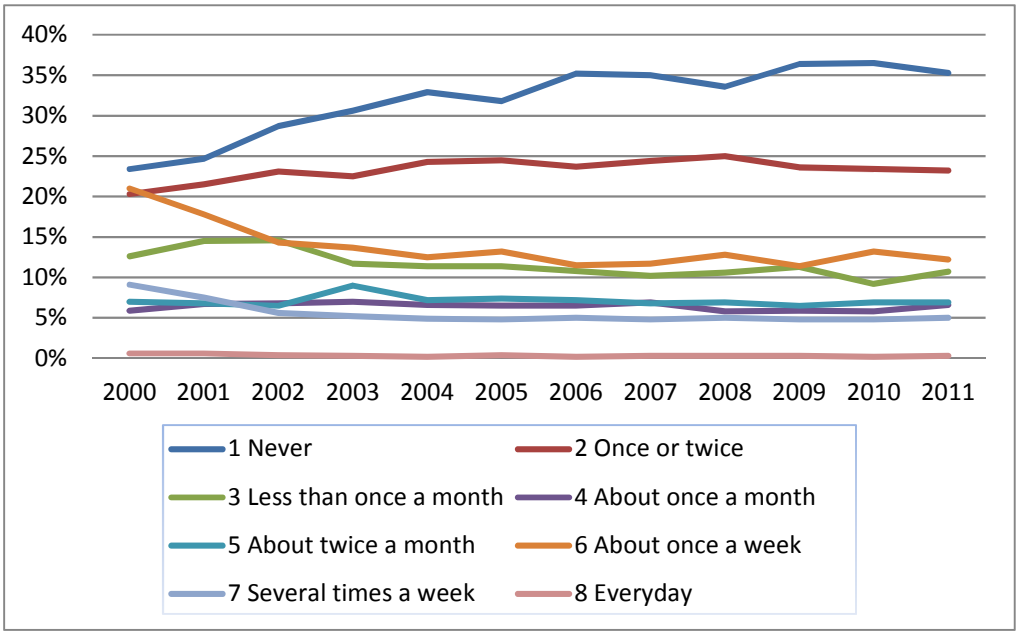
Second, about the choice of model, if one believes that previous religious status determines later religiosity, then a lagged responses model would be a proper choice. However, the choice between fixed effects model and lagged response model requires strong assumption about how religiosity is determined. Even though one alternatively assumes it is past religiosity that determines current religious activity instead of unobserved characteristics, we still need alternative identification strategy such as instrument variable to give an interval of the causal effect estimation.[43]

Third, most papers in this literature look at American data. However, different countries have different educational systems and cultural backgrounds that convey various views of beliefs. It requires an overall international investigation to call the findings of this literature conclusive.[44]

[43] Arellano and Bond (1991) provide a GMM estimator for models include both fixed effect and lagged dependent variable.

[44] A psychological paper Meisenberg et al. (2012) addresses this topic at the country level. They find negative relationships between education and religion, and conclude that the effects differ by country and by religion. The results they show are correlations, not causal effects.

Figure 5. Percentage of religious attendance over the years.



Note: This figure includes the constructed sample of this paper. The sample construction is in section 2.4.

Table 10. Percentage of religious attendance over the years.

Religious Attendance in past 12 months	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011
1. Never	23.4%	24.7%	28.7%	30.6%	32.9%	31.8%	35.2%	35.0%	33.6%	36.4%	36.5%	35.3%
2. Once or twice	20.3%	21.5%	23.1%	22.5%	24.3%	24.5%	23.7%	24.4%	25.0%	23.6%	23.4%	23.2%
3. Less than once a month / 3-12 times	12.6%	14.5%	14.6%	11.7%	11.4%	11.4%	10.8%	10.2%	10.6%	11.3%	9.2%	10.7%
4. About once a month / 12 times	5.9%	6.7%	6.8%	7.0%	6.6%	6.5%	6.5%	6.9%	5.8%	5.9%	5.8%	6.6%
5. About twice a month / 24 times	7.0%	6.8%	6.5%	9.0%	7.2%	7.4%	7.2%	6.8%	6.9%	6.5%	6.9%	6.9%
6. About once a week	21.0%	17.8%	14.3%	13.7%	12.5%	13.2%	11.5%	11.7%	12.8%	11.4%	13.2%	12.2%
7. Several times a week	9.1%	7.5%	5.6%	5.2%	4.9%	4.8%	5.0%	4.8%	5.0%	4.8%	4.8%	5%
8. Everyday	0.6%	0.6%	0.4%	0.3%	0.2%	0.4%	0.2%	0.3%	0.3%	0.3%	0.2%	0.3%
% remain unchanged since last year	-	45.3%	46.5%	47.9%	49.3%	52.2%	53.1%	53.5%	53.7%	55.2%	55.8%	57.2%
% increased since last year	-	22.0%	18.9%	23.2%	20.6%	22.7%	19.6%	22.3%	23.6%	20.1%	21.8%	20.9%
% decreased since last year	-	32.6%	34.5%	28.9%	30.2%	25.1%	27.3%	24.2%	22.6%	24.7%	22.4%	21.9%
In 2011, % remain unchanged since 2000						29.5%						
In 2011, % increased since 2000						21.3%						
In 2011, % decreased since 2000						47.3%						

Note: The percentage is based on valid observations from the constructed sample.

Table 11. Percentage of answers about religious beliefs and index over the years.

Religious beliefs questions (True=1, False=0)	2002	2005	2008	2011
1. "I don't need religion to have good values." (reverse coded)	50.6%	46.9%	43.7%	44.0%
2. "Religious teachings should be obeyed exactly as written in every situation."	37.9%	35.1%	34.9%	35.2%
3. "I often ask God to help me make decisions."	71.4%	71.5%	69.5%	68.6%
4. "God has nothing to do with what happens to me personally." (reverse coded)	77.9%	79.6%	77.4%	75.0%
5. "I pray more than once a day."	35.3%	35.7%	37.6%	40.8%
Trend of index of religious beliefs over the years	2002	2005	2008	2011
% remain unchanged since last time	-	46.1%	48.5%	53.7%
% increased since last time	-	28.7%	30.2%	24.5%
% decreased since last time	-	31.3%	32.3%	25.8%
In 2011, % remain unchanged since 2002		40.1%		
In 2011, % increased since 2002		24.2%		
In 2011, % decreased since 2002		35.7%		
Four-question index of religious beliefs (sum of the four answers)	2.38	2.36	2.28	2.27
(Standard deviation)	(1.26)	(1.29)	(1.31)	(1.36)

Note: 1. Percentages are based on valid observations from the constructed sample.
2. Question 5, "I pray more than once a day," is not included in the estimation of this paper.

Table 12. Effects of Missing Data on Sample Sizes

	Religious attendance analysis			Index of religious beliefs analysis	
	Sample sizes	Percentage		Sample sizes	Percentage
NLSY97 full sample	8,984	100.00%	NLSY97 full sample	8,984	100.00%
Exclude if race missing	8,904	99.11%	Exclude if race missing	8,904	99.11%
Exclude if 1997 religious preference missing	8,786	97.80%	Exclude if 1997 religious preference missing	8,786	97.80%
Exclude if (A) religious attendance missing	8,565	95.34%	Exclude if (B) religiosity index missing	8,120	90.38%
Exclude if (C) highest grade completed missing	8,559	95.27%	Exclude if (C) highest grade completed missing	8,120	90.38%
Exclude if (D) highest degree missing	8,559	95.27%	Exclude if (D) highest degree missing	8,098	90.14%
Exclude if (C) and (D)	8,551	95.18%	Exclude if (C) and (D)	8,067	89.79%
Exclude if parent's religiosity missing	5,719	63.66%	Exclude if parent's religiosity missing	5,458	60.75%
Exclude if AFQT missing	4,737	52.73%	Exclude if AFQT missing	4,546	50.60%
	4,716	52.49%		4,483	49.90%
Exclude if (B) Religiosity index missing	4,480	49.87%	Exclude if (A) church attendance missing	4,480	49.87%
Subsample of 14 years old and young as of 12/31/1996	5,419	100%	Subsample of 14 years old and young as of 12/31/1996	5,419	100%
Percentage of the 14 and younger subsample	4,480	82.67%	Percentage of the 14 and younger subsample	4,480	82.67%

Notes: 1. The large amount of missing data on parents' religiosity is that only the parents of youths younger than 14 years old responded to the questions about attitudes, which questions about religion belong to.

Table 13a. Sample descriptive statistics of the key educational/religious variables in Round 15 (2011)

	Mean	Standard deviation	Number of valid data
Religious attendance days in past 12 months	15.53	32.43	3,897
Index of religious beliefs	-0.02	1.01	3,184
Parents' religiosity index	0	1.01	4,480
Highest grade completed as of the survey date	13.76	2.87	3,957
AFQT score	46.33	29.41	4,480
Highest degree received	**Frequency**	**Percentage**	
None	341	8.54%	
High school	2,211	55.36%	
Bachelor	1,192	29.84%	
Master	203	5.08%	
PhD	47	1.18%	
Total	3,994	100%	

Note: 1. Some numbers of valid observation in 2011 are less than the sample size 4,480 due to missing values.
2. "High school" includes GED and High school diploma.
3. "Bachelor" includes Associate/Junior college (AA) and Bachelor's degree (BA, BS).
4. "Master" includes MA and MS.
5. "PhD" includes PhD and Professional degree (DDS, JD, MD).

Table 13b. Sample descriptive statistics of the control variables in Round 15 (2011)

	Mean	Standard deviation	Number of valid data
Age	28.34	1.31	4,480
Gender (male equals one, zero otherwise)	0.51	0.50	4,480
Marital status (married equals one, zero otherwise)	0.43	0.50	3,988
Having any children (equals one, zero otherwise)	0.45	0.50	3,995
Living in urban area (equals one, zero otherwise)	0.78	0.41	3,946
Race	Frequency	Percentage	
White	2,762	61.65%	
Black or African American	1,163	25.96%	
American Indian, Eskimo, or Aleut	33	0.74%	
Asian or Pacific Islander	56	1.25%	
Else	466	10.40%	
Total	4,480	100%	
Current religious preference in 1997	Frequency	Percentage	
Roman Catholic	1,275	28.46%	
Protestants	2,554	56.99%	
Jewish	37	0.83%	
Other Christian	77	1.72%	
Muslim	16	0.36%	
Hindu/Buddhist	4	0.09%	
Others	377	8.41%	
None	140	3.13%	
Total	4,480	100%	

Note: 1. Some numbers of valid observation in 2011 are less than the sample size 4,480 due to missing values.
2. “Protestants” include Baptist, Methodist, Lutheran, Presbyterian, Episcopal/Anglican, United Church of Christ, Disciples of Christ, Reformed Church in America, Holiness, Pentecostal, Nondemonimational Christian, and other Protestant.
3. “Other Christian” includes Mormon, Eastern Orthodox, and Unitarian.
4. “Others” include Native American Tribal Religion, Bah'ai, Greek, Roman, Norse, .etc. Mythology, Satanic, Wicca/Witchcraft/Magic/Pagan, Scientology, other Eastern, and others.
5. “None” include Agnostic, Atheist, and no religion/personal philosophy.

Table 14. Pooled OLS results on religious attendance

Dependent variable	Religious attendance days per year							
Model	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)
Highest Grade	.952***	-	-	.873***	-	-	.806***	-
	(.151)			(.147)			(.142)	
Highest Degree								
1. None	-	-.434	-	-	-.174	-	-	.308
		(.660)			(.652)			(.653)
2. Bachelor	-	4.636***	-	-	4.277***	-	-	3.762***
		(.808)			(.790)			(.775)
3. Master	-	7.939***	-	-	7.410***	-	-	6.611***
		(2.458)			(2.429)			(2.356)
4. PhD	-	-2.187	-	-	-2.623	-	-	-3.610
		(2.190)			(2.244)			(2.285)
AFQT	-	-	1.196***	-	-	1.167***	.354	.793*
			(.417)			(.407)	(.411)	(.412)
Male	-1.924**	-2.186***	-2.477***	-2.135***	-2.387***	-2.633***	-2.133***	-2.347***
	(.760)	(.751)	(.743)	(.741)	(.732)	(.725)	(.742)	(.735)
Age	-1.027***	-.988***	-.769***	-1.009***	-.966***	-.775***	-.996***	-.932***
	(.076)	(.078)	(.066)	(.075)	(.077)	(.065)	(.075)	(.076)
Married	7.622***	7.536***	7.627***	7.152***	7.074***	7.147***	7.135***	7.051***
	(.778)	(.783)	(.784)	(.768)	(.772)	(.773)	(.768)	(.773)
Parent's religiosity	-	-	-	5.526***	5.544***	5.584***	5.529***	5.550***
				(.361)	(.362)	(.364)	(.362)	(.363)
Any children	-1.890***	-2.372***	-2.786***	-1.845***	-2.309***	-2.630***	-1.781**	-2.106***
	(.711)	(.698)	(.705)	(.699)	(.688)	(.693)	(.711)	(.706)
Urban area	-3.250***	-3.190***	-3.141***	-2.816***	-2.759***	-2.720***	-2.835***	-2.816***
	(.786)	(.790)	(.790)	(.766)	(.770)	(.770)	(.765)	(.767)
Sample size	4,480	4,480	4,480	4,480	4,480	4,480	4,480	4,480
Observations	45,439	45,439	45,439	45,439	45,439	45,439	45,439	45,439
R-squared	0.0919	0.0911	0.0896	0.1136	0.1130	0.1118	0.1136	0.1134

Notes: 1. Individual clustered robust standard errors are in the parentheses. All models control for race and religious preferences.
2. The reference group of the highest degree received is high school graduate.
*: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$.

Table 15. Pooled OLS results on index of religious beliefs

Dependent variable	Index of religious beliefs							
Model	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)	(viii)
Highest Grade	-.010*	-	-	-.014***	-	-	.020***	-
	(.005)			(.005)			(.005)	
Highest Degree								
1. None	-	.014	-	-	.030	-	-	-.092***
		(.029)			(.028)			(.029)
2. Bachelor	-	-.035	-	-	-.054**	-	-	.053*
		(.029)			(.027)			(.027)
3. Master	-	-.010	-	-	-.116*	-	-	.055
		(.064)			(.062)			(.063)
4. PhD	-	-.335***	-	-	-.352***	-	-	-.134
		(.124)			(.130)			(.128)
AFQT	-	-	-.156***	-	-	-.158***	-.179***	-.172***
			(.014)			(.013)	(.014)	(.014)
Male	-.185***	-.184***	-.203***	-.194***	-.191***	-.208***	-.196***	-.200***
	(.025)	(.025)	(.025)	(.024)	(.024)	(.023)	(.024)	(.024)
Age	-.012***	-.011***	-.012***	-.012***	-.011***	-.012***	-.017***	-.017***
	(.003)	(.003)	(.002)	(.003)	(.003)	(.002)	(.003)	(.003)
Married	.199***	.200***	.208***	.180***	.182***	.188***	.185***	.184***
	(.027)	(.027)	(.027)	(.027)	(.027)	(.026)	(.026)	(.026)
Parent's religiosity	-	-	-	.255***	.255***	.255***	.253***	.253***
				(.013)	(.013)	(.013)	(.013)	(.013)
Any children	.107***	.109***	.052**	.108***	.112***	.059**	.079***	.072***
	(.026)	(.026)	(.025)	(.025)	(.025)	(.024)	(.024)	(.024)
Urban area	-.106***	-.106***	-.094***	-.088***	-.089***	-.077***	-.081***	-.079***
	(.025)	(.025)	(.024)	(.023)	(.023)	(.023)	(.023)	(.023)
Sample size	4,480	4,480	4,480	4,480	4,480	4,480	4,480	4,480
Observations	13,371	13,371	13,371	13,371	13,371	13,371	13,371	13,371
R-squared	0.2334	0.2336	0.2515	0.2854	0.2855	0.3034	0.3048	0.3047

Notes: 1. Individual clustered robust standard errors are in the parentheses. All models control for race and religious preferences.

2. The reference group of the highest degree received is high school graduate.

*: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$.

Table 16. Fixed effects results on religious attendance and index of religious beliefs

Dependent variable	Religious attendance days per year		Index of religious beliefs	
Model	(i)	(ii)	(iii)	(iv)
Highest Grade	-1.150***	-	-.019***	-
	(.119)		(.005)	
Highest Degree				
1. None	-	7.304***	-	-.051
		(.698)		(.032)
2. Bachelor	-	-1.991***	-	-.062***
		(.560)		(.021)
3. Master	-	-1.696	-	-.132***
		(1.417)		(.046)
4. PhD	-	-5.857***	-	-.222**
		(1.960)		(.107)
Age	.255	.135	-.012	-.009
	(.511)	(.511)	(.023)	(.023)
Married	.699	.436	.031	0.036
	(.652)	(.656)	(.022)	(.022)
Any children	-.407	-.112	.029	.028
	(.570)	(.570)	(.021)	(.021)
Urban area	-1.100**	-1.046**	-.034*	-.035*
	(.503)	(.504)	(.020)	(.020)
Year	-.602	-.404	-.005	-.000
	(.504)	(.507)	(.022)	(.023)
Observations	45,439	45,439	13,371	13,371
Sample size	4,480	4,480	4,480	4,480
R-squared: within	0.0116	0.0140	0.0070	0.0073
between	0.0084	0.0042	0.0116	0.0082
overall	0.0001	0.0010	0.0131	0.0096

Notes: 1. Individual clustered robust standard errors are in the parentheses.
2. The reference group of the highest degree received is high school graduate.
*: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$.

Table 17. Fixed effects results on religious attendance per year and religiosity index: robustness check

Dependent variable	Religious attendance days per year (Only 2000 and 2011)		Index of religious beliefs (Only 2002 and 2011)	
Model	(i)	(ii)	(iii)	(iv)
Highest Grade	-2.157***	-	-.029***	-
	(.309)		(.007)	
Highest Degree				
1. None	-	8.151***	-	-.023
		(1.643)		(.046)
2. Bachelor	-	-4.886***	-	-.115***
		(1.778)		(.034)
3. Master	-	-7.845**	-	-.156**
		(3.066)		(.065)
4. PhD	-	-11.621**	-	-.357***
		(5.630)		(.138)
Age	.829	.287	-.003	.008
	(1.893)	(1.913)	(.038)	(.038)
Married	1.237	.588	.031	.038
	(1.766)	(1.765)	(.035)	(.036)
Any children	-.106	.712	.039	.040
	(1.783)	(1.756)	(.035)	(.034)
Urban area	-1.742	-1.848	-.072**	-.073**
	(1.647)	(1.647)	(.033)	(.033)
Year	-.968	-.393	-.003	-.015
	(1.862)	(1.890)	(.037)	(.038)
Observations	6,821	6,821	7,235	7,235
Sample size	4,233	4,233	4,357	4,357
R-squared: within	0.0462	0.0429	0.0177	0.0188
between	0.0004	0.0004	0.0083	0.0079
overall	0.0021	0.0060	0.0116	0.0117

Notes: 1. Individual clustered robust standard errors are in the parentheses.
2. The reference group of the highest degree received is high school graduate.
3. The sample was 14 years old and had parents' religiosity report in 1997.
4. The observation of church attendance days only includes 2000 and 2011. The observation of index of religious beliefs only includes 2002 and 2011.
*: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$.

Table 18a. Subsamples for the fixed effects estimation: Religious attendance days per year

Subsample	Full sample	Stronger religious background	Weaker religious background	Higher AFQT scores	Lower AFQT scores	Male	Female
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)
Highest Grade	-.1.150***	-1.581***	-.545***	-1.049***	-1.122***	-1.309***	-1.032***
	(.119)	(.169)	(.162)	(.134)	(.255)	(.166)	(.169)
Observations	45,439	26,637	18,802	23,825	21,614	22,774	22,665
Sample size	4,480	2,613	1,867	2,312	2,168	2,288	2,192
R^2: within	0.0116	0.0189	0.0037	0.0246	0.0050	0.0133	0.0102
between	0.0084	0.0202	0.0013	0.0065	0.0179	0.0098	0.0053
overall	0.0001	0.0000	0.0022	0.0015	0.0009	0.0000	0.0004
Highest Degree							
1. None	7.304***	9.869***	3.863***	8.673***	5.982***	6.412***	8.219***
	(.698)	(.934)	(1.040)	(.755)	(1.152)	(.954)	(1.022)
2. Bachelor	-1.991***	-3.357***	.024	-1.328**	-1.045	-2.617***	-1.520**
	(.560)	(.820)	(.666)	(.625)	(1.258)	(.826)	(.769)
3. Master	-1.696	-3.653*	1.267	-1.040	6.205	-5.551**	.368
	(1.417)	(2.114)	(1.551)	(1.483)	(5.487)	(2.752)	(1.579)
4. PhD	-5.857***	-8.242***	-1.886	-4.042**	-12.053*	-7.820**	-4.653*
	(1.960)	(2.820)	(2.007)	(2.037)	(7.290)	(3.252)	(2.435)
Observations	45,439	26,637	18,802	23,825	21,614	22,774	22,665
Sample size	4,480	2,613	1,867	2,312	2,168	2,288	2,192
R^2: within	0.0140	0.0226	0.0050	0.0305	0.0065	0.0141	0.0147
between	0.0042	0.0108	0.0021	0.0027	0.0085	0.0032	0.0052
overall	0.0010	0.0005	0.0030	0.0049	0.0000	0.0012	0.0008

Notes: 1. Strong family religious background subsample includes people of whose parents' religiosity indices are higher than median, and vice versa.

2. Higher cognitive ability subsample includes people of whose AFQT scores are higher than median score, and vice versa.

*: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$.

Table 18b. Subsamples for the fixed effects estimation: Index of religious beliefs

Subsample	Full sample	Stronger religious background	Weaker religious background	Higher AFQT scores	Lower AFQT scores	Male	Female
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)
Highest Grade	-.019***	-.025***	-.011	-.020***	-.005	-.023***	-.017***
	(.005)	(.006)	(.007)	(.006)	(.008)	(.007)	(.006)
Observations	13,371	8,025	5,346	7,133	6,238	6,694	6,677
Sample size	4,480	2,613	1,867	2,312	2,168	2,288	2,192
R^2: within	0.0070	0.0109	0.0038	0.0180	0.0006	0.0102	0.0069
between	0.0116	0.0016	0.0415	0.0029	0.0003	0.0025	0.0177
overall	0.0131	0.0051	0.0312	0.0072	0.0001	0.0037	0.0160
Highest Degree							
1. None	-.051	-.078*	-.013	-.007	-.064	-.056	-.049
	(.032)	(.042)	(.048)	(.050)	(.041)	(.046)	(.043)
2. Bachelor	-.062***	-.075***	-.040	-.013	-.121***	-.074**	-.058**
	(.021)	(.027)	(.033)	(.024)	(.047)	(.031)	(.028)
3. Master	-.132***	-.194***	-.024	-.063	-.253	-.144**	-.136**
	(.046)	(.053)	(.084)	(.051)	(.190)	(.071)	(.061)
4. PhD	-.222**	-.289*	-.125	-.137	-.752***	-.170	-.267**
	(.107)	(.160)	(.118)	(.113)	(.050)	(.175)	(.136)
Observations	13,371	8,025	5,346	7,133	6,238	6,694	6,677
Sample size	4,480	2,613	1,867	2,312	2,168	2,288	2,192
R^2: within	0.0073	0.0119	0.0036	0.0159	0.0041	0.0099	0.0080
between	0.0082	0.0026	0.0219	0.0164	0.0007	0.0000	0.0127
overall	0.0096	0.0068	0.0172	0.0157	0.0012	0.0005	0.0119

Notes: 1. Strong religious background subsample includes people of whose parents' religiosity indices are higher than median, and vice versa.

2. Higher cognitive ability subsample includes people of whose AFQT scores are higher than median score, and vice versa.

*: $p < 0.1$; **: $p < 0.05$; ***: $p < 0.01$.

Table 19. Instrument variables for education using college accessibility

IV's	Description	Mean	Standard Deviation	Number of Observations	First-stage coefficient on schooling years
(1) Seats-young population ratio in 2000	Number of seats (enrollment) in colleges in the individual's residential county[1, 2], divided by the number of population from 18 to 29 years old.	35.31%	11.02%	2,970	0.872** (0.354)
(2) BA percent in 1990	Percentage with bachelor's degree or higher, in the population of 25-years old and older in 1990[3].	19.31%	7.97%	2,970	0.013*** (0.005)

Notes: 1. Generated from 2000 Census data.
2. A robustness check about the choice of year is conducted.
3. From NLSY97 Geocode data.
4. Standard errors are in the parentheses in the last column.

Table 20. Instrumental variable estimates: college accessibility

	(1)	(2)	(3)
Estimator	OLS	2SLS IV: studentPCT young2000	2SLS IV: BApercent1990
	Panel A: Church Attendance Days as dependent variable		
Coefficients	0.969*** (0.249)	-3.820 (5.443)	-4.857 (5.577)
Number of observations	3,647	3,647	3,647
	Panel B: Index of Religiosity as dependent variable		
Coefficients	0.026*** (0.007)	-0.723** (0.320)	-0.509** (0.210)
Number of observations	2,970	2,970	2,970

Note: 1. Robust standard errors are in the parentheses.
2. This is conducted cross-sectionally using the 2011 data.

Chapter 4

Curriculum, Civic Participation, and National Identity: Evidence from Taiwan

4.1 Introduction

In this paper, I estimate the causal effect of a textbook reform in Taiwan on the youths' later civic participation and attitudes towards national identity. I test the hypothesis that this relatively more Taiwan-centered curriculum accounts for part of the increase in Taiwanese citizens' national identity with Taiwan and the recent increase in civic participation and student movements. Broadly speaking, this work examines whether 1) the citizens' national identity with the local country, and 2) the citizens' political engagement to the local society are dependent on the educational content people receive in the younger ages.

In September 1997, Taiwan adopted a new set of middle school textbook series, "Knowing Taiwan," also translated as "Understanding Taiwan," to teach students more about Taiwan's own history, geography, and society, compares to the old China-centered curriculum. The new curriculum was adopted for students who entered middle school after September 1997, who were born after September 1984 according to Taiwan's enrollment system. It provides an opportunity to exploit the exogenous variation by individuals' birth month near the cutoff enrollment month. I use data from Taiwan Social Change Survey to

compare the national identity, political attitudes, and civic engagement of individuals who were born before and after September 1984.

National identity has its unique economic policy implication in Taiwan. It has been the most divisive issue in Taiwan's political spectrum when it comes to voting behavior and for-against attitudes towards policy debates. It is also the best predictor of Taiwanese voters' political and economic orientations.

The positive externalities of education have been considered a justification for governmental intervention in educational policies (Friedman, 1955). For example, empirical and theoretical studies show that general education may increase civic engagement (or civic participation, e.g., Milligan et al., 2004; Dee, 2004) and the quality of democracy (Glaeser et al., 2007). At the same time, however, Acemoglu et al. (2005) do not find evidence supporting the causal effect of education on democracy. I argue that a closer look at the education content can contribute to this literature and help explain the mixed results. More discussion about the general effects of education is provided in Section 2.

The challenge in this causal estimation, like other estimation of the effect of education on social outcomes, is that civic engagement and national identity may change over time. There might be social trends of civic engagement and national identity, and they may have different effects for individuals of different ages. At the same time, one has to rule out the possibility that observed changes are not due to other events. Again, different cohorts may respond to other events differently.

With a repeated cross-sectional data set at the individual level, in order to deal with the age effect and the social trend, I control for individuals' age and survey year fixed

effects to account for the potential trends. Most importantly, the regression discontinuity (RD) experimental design can measure the causal effect of the curriculum since the students near the cutoffs were exposed to the same phase of the social trend, if there is any.

The 1997 Knowing Taiwan curriculum reform provides a special opportunity to find the causal impacts of educational content. Students who entered junior high school before September 1997 were not exposed to the new textbooks. They can be viewed as the comparison group of this natural experiment. The assignment of the treatment is clearly according to the birth year and month. Conditional on individual characteristics, the treatment group, those who entered junior high school after September 1997 and the comparison group should have similar traits near the cutoff. Therefore, I exploit this clear starting date to conduct a regression discontinuity design to find the causal effects.

In this study, the target effects to identify are national identity and civic participation. I use data from a repeated cross-sectional, nationally representative survey, Taiwan Social Change Survey (TSCS), which contains a rich set of questions on the target measurements – ideologies, national identities, and individuals' participation in social and political activities.

This study provides a special example of effects of education on social outcomes and contributes to the intersection between the literature of the social effects of education and the formation of ideology. First, for the social effects of education, it relates to the literature of the causal effect of education on political ideology, national identity, and civic participation. Second, among other factors including family background, peers, personal experiences (e.g., Giuliano and Spilimbergo, 2014), and media (Yanagizawa-Drott, 2014; DellaVigna and La Ferrara, 2015; Strömberg, 2015), education is one of the most important

preference and behavior shapers. This paper contributes to this branch of literature by providing evidence and attempting to identify the channel through which education forms people's ideology.

The outline of the paper is as follows. Section 4.2 reviews the empirical study of the collective outcomes of education, including democracy, civic participation, and national identity, along with its general empirical strategies. In Section 4.3, I introduce the special opportunity in the context of the 1997 Knowing Taiwan curriculum. Section 4.4 describes the TSCS data and the variables of interest. The results are presented in Section 4.5. Section 4.6 concludes.

4.2 Empirical Study of Education on Collective Outcomes

The positive externality of education is one important justification for government intervention in education. Beyond the effects of education on individual labor market outcomes (for example, Card, 1999) and other outcomes at the individual level (for example, Lochner, 2011), this short review focuses on the economic studies of collective outcomes of education at the society level or above.

4.2.1 Democracy and civic participations

Acemoglu *et al*. (2005) use the country-level Freedom House Political Rights Index and account for country fixed effects in arguing there is no causal relationship between education and democracy within a country. They point out that many evidence in the existing literature that show a positive relationship between education and democracy reflect the growing trends of the two. Their results, however, are not meant to nullify the

relationship between education and democracy in a long period of time. A companion paper by Acemoglu *et al*. (2008) looks at the relationship between income and democracy and again shows no causal link.

Gleaser *et al*. (2007) propose a theoretical model to explain why education might promote democracy. In their model, individuals choose a more democratic or a more dictating regime. One hypothesis – socialization hypothesis – is that education enhances the benefits of political participation and facilitates better communication, which they argue is crucial for democracy to be more easily functioned. They also argue that the other hypothesis, indoctrination, is not supported by evidence since education increases not only the kind of civic participation that is encouraged in curricula.

Milligan *et al*. (2004) suggest two channels through which education affects political behaviors: quality and quantity. Through the first channel, education qualitatively changes how citizens behave in political decisions. Through the second channel, citizens who receive education become more involved in political behaviors due to higher political interest and better political knowledge. They focus on the second channel and use compulsory schooling laws as instrumental variables in the United States and the United Kingdom to find a strong relationship between education and one's probability of voting in the US, but not in the UK. In both the US and the UK, individuals who attended high schools are more likely to involve in civic and political activities than those who did not.

Dee (2004) uses datasets in High School and Beyond and the General Social Survey and exploits exogenous variation created by college availability and child labor laws. He finds significant effects of education on voter participation, support for free speech, and the quality of overall knowledge about civilization.

Friedman *et al*. (2016) use the randomization of Kenyan school girls' merit scholarship to examine the effect of education on a variety of the recipients' social and political awareness and actions. Their results suggest that education raises young women's sense of autonomy, tendency to read political news, and doubts in authority. However, the increased education does not promote political participations.

4.2.2 National identity

Clots-Figueras and Masella (2013) look at a Catalan survey data to investigate whether adding the Catalan language in the education system in 1983 accounts for an increase in students' identity with Catalonia. They exploit variation in different cohorts' compulsory schooling years exposed to the Catalan education system and find the effect of language *and* educational content on an individual's national identity to be positive.

Fuchs-Schündeln and Masella (2016) study the human capital and labor market outcomes of East German students who were exposed to socialist educational regime after the reunification. Students in the same cohort can have different length of exposure due to the enrollment policy. They conclude that one of the channels through which socialist education lowers labor market participation is that individual effort is not encouraged in such educational regime.

Cantoni *et al*. (2017a) conduct a survey at Peking University to study the effect of a textbook reform in China on high school students' attitudes and ideologies. They take advantage of the different adoption timings by different provinces to identify the causal effect. They find students exposed to the new curriculum that targets stronger ideological ties to the government's stance do exhibit deeper belief in the government's role and more

doubt in free markets and capitalism. Since the participants of the survey are college students, their results do not inform whether the reform has a longer effect on students' later life. The survey asks solely about attitude and ideologies. Therefore, their study does not inform whether the change in attitudes causes any political consequences.

On the determinants of political ideology, Cantoni *et al*. (2017b) survey university students in Hong Kong by checking a broad range of factors, including demographic information, individual characteristics, attitudes, and experimentally elicited economic preferences, to identify the predictors of the tendency toward participation in activities against authorities. They find that fundamental preferences such as risk tolerance and pro-social preferences to be most predictive. They also find demographic factors correlated to anti-authoritarianism. The findings in this work are correlations instead of causal effects.

Chen *et al*. (2016) exploit the enrollment cutoff and the curriculum reform in Taiwan and find students exposed to *Knowing Taiwan* series textbook are more likely to identify with Taiwan than China or both Taiwan and China. The effect is stronger among students on the academic education track than vocational education track, suggesting a channel of memorizing the textbook materials. The effect is found to last only in the short run.

4.2.3 Empirical strategies

The social outcome of interest is denoted by Y_i, and education is denoted by Edu_i. X_i includes controlled variables. The baseline model is given by:

$$Y_i = \beta_0 + \beta_0 Edu_i + \gamma X_i + \varepsilon_i. \qquad (12)$$

There are some challenges in estimating the causal effect of education on social outcomes. First, there might be unobservable characteristics that affect both social outcomes and educational attainment. For example, in attempting to explain the causal link between education and civic participation, if the core reason for pursuing more education is part of the reason why people choose to participate in more political activities, the positive correlation is overestimated when interpreted as causal. Existing research has used instrumental variables (Milligan *et al*., 2004), fixed effects (Acemoglu *et al*., 2005), and regression discontinuity design (Chen *et al*., 2016) to help eliminate the endogeneity problem.

Second, the estimation has to consider the long term trend of the social outcome. A common technique is to account for year fixed effects. Alternatively, including year dummies in the estimation can isolate possible existing specific effects in the given years. To state the obvious, however, both treatments are subject to the length of history the dataset can provide.

4.2.4 How long?

How long does it take for education to have influences on social outcomes? Perhaps because of the limitation of availability of the dataset, the literature has not provided a clear answer to this question. On a related note, Glaeser *et al*. (2007) argue that the reason why Acemoglu *et al*. (2005) do not find the causal relationship when accounting for country fixed effects is that their datasets cover a relatively short time span. Later, Acemoglu *et al*. (2008) analyze a panel that is five hundred years long. Chen *et al*. (2016) find the effect of *Knowing Taiwan* curriculum on national identity in Taiwan diminishes in the long run –

about fifteen years after the first adopters graduated from middle schools, where the new curriculum took place. Considering there is no definite answer to what time span is long enough, whether the effect they found diminishing will return in the future is an open question.

4.3 Empirical Evidence from Taiwan

4.3.1 1997 *Knowing Taiwan* Curriculum

Since 1949, the Chinese Nationalist Party, more often called KMT (Kuomintang), started to govern Taiwan as a Chinese regime. The education system in Taiwan, like many other countries, was used as a tool of propaganda from a Chinese point of historical view. Until 1997's textbook reform, Taiwan's textbook gave little attention to Taiwan's history, geography, and society. Take history subject as an example, as noted in Chen *et al*. (2016), in a three-year curriculum design, the old curriculum spends one year and a half to teach Chinese history and the other one year and a half to teach world history. In contrast, the new curriculum that features *Knowing Taiwan* spends one year on each of Chinese history, world history, and Taiwanese history. In terms of hours spent, Chen *et al*. (2016) calculate that students under the new curriculum would spend 12 times more than the hours spent in class under the old curriculum on Taiwan-related materials. Sixteen pages about Taiwan are in the old curriculum whereas 116 pages about Taiwan are in the new curriculum.

The new curriculum was implemented in September 1997. According to Taiwan's enrollment system, students who are born after August 31st will enroll in elementary schools in the next September after they turn seven years old, and thus will enroll in middle schools in the next September after they turn thirteen. Therefore, those who were born

between September 1984 and August 1985 were the first cohort who studied the new curriculum. This sharp implementation of education policy provides a good opportunity to exploit its exogenous variation to estimate the effect of curriculum change on social outcomes.

4.3.2 Econometric model

In this experimental design, the treatment is assigned to the individuals who entered middle school in September 1997 or later. They are the ones who were born after September 1984. Therefore, the running variable in the regression discontinuity design is the birth month, with September 1984 being the cutoff.

$$Y_{it} = \beta_0 + \beta_1 KnowingTW_i + \gamma X_{it} + \delta_t + \varepsilon_{it} \quad (13)$$

In equation (13), Y_{it} is national identity variable or civic participation variable of an individual i who takes the survey in year t. $KnowingTW_i$ takes value one if individual i belongs to the treatment group, that is, born after September, 1984. X_{it} is individual level control variables including gender, age, birth place, parents' education, and parent's birth place. δ_t is the year fixed effects.

The most important requirement of an RD design is that individuals cannot have the power to select into the treatment. In this study, the assignment of the treatment guarantees that parents did not have any control over the choice of curriculum in 1997 when they made decisions to give birth to children in 1984. The identification assumption of this RD design is independent variables other than $KnowingTW_i$ move smoothly with respect to the birth cohort at the month level.

4.4 Data

4.4.1 Taiwan Social Change Survey (TSCS)

Taiwan Social Change Survey (TSCS)[45] is an ongoing, face-to-face, nationally representative survey. It is a repeated cross-sectional data set. The survey started in 1984 and has continued annually on multiple social topics since 1989. The population includes residents above 18 years old. Subtopics under the main topic vary by waves.[46] TSCS targets individuals above the age of 18 who live in Taiwan. The first wave that includes the treatment group is the 2003 wave. Therefore, I use the data from the 2003 wave to the most recent available wave of 2015.

In all the estimations, I control for variables including gender, age, survey year, birth place (Taiwan or not), parent's birth place (Taiwan or not), hometown (the city individual spent most time living in), and educational attainment (highest degree).

Table 21 shows the availabilities of the variables needed in the estimation of interests and the construction of sample in this paper due to missing values. The construction of the sample is subject to the availability of the variables in each wave and the missing values. Since education and the influences from family's ethnic roots cannot be ignored in explaining national identity and civic participation, waves and observations without parents' birth place and education are excluded.

In models explaining voting behavior, the biggest drop is the exclusion of those who were not old enough to vote. On average, around 20% of the sample is dropped. In each wave, this rule drops the youngest cohort, who will be included in the next wave. In

[45] http://www.ios.sinica.edu.tw/sc/en/home2.php.
[46] There are five main topics rotating in a five-year cycle. The five main topics are Family, Social Stratification, Political Culture, Mass Communication, and Religion.

other words, since the voting age is 20 years old by law in Taiwan, when the dependent variable is voting behavior, the youngest age in the sample is 21 years old.

4.4.2 National identity

For national identity, I rely on survey years 2003, 2004, 2005, 2009, 2010, 2012, 2014, and 2015. I create a set of dummy variables to capture individuals exhibiting any feeling with Taiwan at all, any feeling with China at all. The question about national identity in the survey asks "which of the following names describes you the best?" The options are 1) Taiwanese, 2) Chinese, 3) both, and 4) other. I construct the dummy variable, *Taiwanonly,* which takes value one if 1) Taiwanese is chosen, and another dummy *Taiwanatall*, which equals one if 1) Taiwanese or 3) both is chosen.

Figure 11 shows the trends of both national identity variables. The percentage of people reporting identity with at least Taiwan remains over 90% and grows steadily, whereas the percentage of reporting identity only with Taiwan had a dramatic increase in this period. The question this study asks is whether *Knowing Taiwan* textbook series accounts for a meaningful difference in such differences.

4.4.3 Civic engagement

The measures of civic engagement are from the responses to the questions regarding the political participation reported by the individuals. I take the actions that fall into the three categories of civic engagement defined in Keeter *et al*. (2002) – civic movements, electoral actions, and political voice. For each action, I construct dummy variables to represent one's civic participation. A variable equals one if the individual has

participated in the given action, zero if otherwise. The availability of the measures in each year is shown in Table 21. Questions in the survey are in Appendix C.

In the category of civic movements, the variable $party$ is coded one if the individual is a current member of a political party, zero if otherwise. The variable $participated$ is coded one if the individual self-reports having participated in political movements, zero if otherwise. In the category of electoral actions, the variable $voted$ is coded one if the individual voted in the last major election, zero if otherwise. Observations who did not have the right to vote by the age of 20 in the last election are excluded.

In the category of political voice, the variable $petition$ is coded one if the individual filed a petition in the past year, zero if otherwise. The variable $protest$ is coded one if the individual participated in any protest in the past year, zero if otherwise. The variable $lobby$ is coded one if the individual sought to influence politicians' decisions in any way in the past year, zero if otherwise. The variable $media$ is coded one if the individual tried to voice on political issues through the public media, zero if otherwise.

Figure 7 shows the trends of civic movements and electoral actions in the period with available data. Generally, only a small proportion of people in Taiwan are official members of political parties, while the percentage of people participating in political movements experienced a growth in recent years. The share of people who voted in the previous elections remains high.

Figure 8 shows the trends of political voices in the period with available data. The share of people who voiced on political issues is low, but there is a slowly growing trend. The volume of political voice is subject to current events in the society. So, in all

econometric models, I account for year effects to control the time-dependent "event" effects.

4.4.4 Sample description

Table 22 provides the descriptive statistics of the variable used in this study. The main treatment variable is $Knowing\ Taiwan$. Thirteen percent of the sample in TSCS was young enough to be treated, first appearing in the 2003 wave. Gender is equally distributed. The average age of the respondents of the survey is 45 years old. The vast majority of the sample was born in Taiwan, with a lower percentage for older generations. For the same reason, a higher percentage of the parents of the respondents were born outside of Taiwan. When the birth place is unavailable, I use the information from whether the respondent reports a city in Taiwan as his or her "hometown."

4.5 Results

To present the results, I first report the observational analysis of *Knowing Taiwan* on national identity and civic participation. Then I conduct a regression discontinuity design using different bandwidths conditional on control variables to refine the results.

4.5.1 Observational analysis

I start the analysis with graphical observations. To begin with, I utilize a survey question in TSCS that is exclusively asked in the 2013 "National Identity" wave, which provides more information than binary responses in typical waves. This question asks the respondents to provide a score on a scale from zero to ten to describe how strong the

individual identifies with Taiwan and China, zero being not at all. Although this question appeared only in the 2013 wave, it still provides a good comparison with the estimation found in the primary models.

Figure 9 shows the quadratic fits of the identity with Taiwan and China on a 0-10 scale by birth month-year. The running variable on the *x*-axis is birth month-year. In other words, Figure 9 places all respondents on the horizontal axis by age and displays their identities in 2013. As shown in panel (a), identity with Taiwan remains high across the birth cohorts. However, a clear jump at the oldest birth cohort suggests there is likely a positive effect of the textbook on national identity with Taiwan. Panel (b) in Figure 9 shows a downward trend of identity with China among people in Taiwan.

Table 23(a) and Table 23(b) show the marginal effects out of probit estimations on national identity and measures of civic participation, respectively. All the waves available are pooled in the Tables 23. In Table 23(a), from column (1) and (2), it shows that individuals who were exposed to the new curriculum exhibit a big increase in the probability of reporting Taiwan to be the only country they identify with, with or without controlling for year effects. The effects of the new curriculum on the probability of reporting at least Taiwan are in columns (3) and (4). The effects are much smaller and statistically insignificant. In Table 23(b), the relationships between the new curriculum and civic participations are somewhat significantly negative.

Tables 23(a) and 23(b) include a widest range possible of cohorts in the TSCS dataset and therefore contains the most information the dataset can provide. However, it is reasonable to believe that merely control for year effects is not enough to capture the effect of different year on different cohorts at different ages. National identity and the tendency

of civic participation can still be systematically different for different cohorts in the same year.

4.5.2 Regression discontinuity analysis

Controlling for year fixed effect only accounts for a part of the yearly differences between the base year and the current years. Here I turn to a quasi-experimental regression discontinuity design to deal with the social trend. The adoption of the new curriculum is a sharp policy change. Therefore, a sharp regression discontinuity design is a natural tool for estimating the effect of the change in textbook content.

Figure 10(a) and 10(b) depict the mean residuals of $Taiwanonly$ and $Taiwanatall$ by birth month-year, conditional on individual characteristics including gender, age, birth country, parents' birth country, education, and parents' education. The vertical axis is the mean residual centered at zero. Instead of including all birth cohorts in Figure 9, I restrict the data used in this graph to three years above and below the cutoff. By visual observation, the graphs show a jump and change in slopes, suggesting there might be a systematic change after the cutoff.

The running variable is birth month in this RD design. The treatment is implemented sharply in September 1997. However, there are at least two reasons this presumably sharp RD design might be alternatively estimated in a fuzzy way. First, the data does not provide information about when the individual entered junior high school. In rare cases, there might be students who enter schools earlier or later than traditional students. Second, it may take a while for some teachers to fully adjust their teaching style and mindset to fully reflect the learning objectives of the new curriculum. The third

potential reason is peer effect. Non-treated (older) cohorts might be affected later in life because they are surrounded by more treated cohorts. These potential compliance issues lead to the alternative specifications that exclude the birth cohorts one year on one or both sides around the cutoff.

The same graphs of measures of the 10-point Taiwan and China identities asked in 2013 along with measures of civic participation are placed in Figures 6 and 7. Figures 6 (a) and (c) show that $Taiwan10$ and $China10$ exhibit jumps up and down at the cutoff, respectively. Figure 7 shows civic participations exhibit no jumps upward, and many of them even jump downward. It reflects the message revealed in Table 23(b) that shows no effects on civic participation.

In Figure 11 (b) and (d), I draw the counter parts of Figure 11 (a) and (c) with the observations one year below and above the cutoff being deleted. Visually, in Figure 11 (a), for example, the quadratic fit line is pulled down by the observation to the right of the cutoff, whereas the fit in Figure 11 (b) are free from this value. The jump at the cutoff is therefore more dramatic with the observations close to the cutoff being deleted. Of course, the downside of this specification is that observations below and above the cutoffs are less similar with the existence of the time trend.

Table 24 reports the coefficients by estimating equation (13) under both sharp and fuzzy RD design. It assures the evidence found in the observational analyses. Among all the dependent variables, the fuzzy coefficient in row (1) shows that students who were exposed to the new curriculum were 10 percentage points more likely to report Taiwan as the only country to identify. The small size and insignificance of the coefficients of $Taiwan\ at\ all$ in row (2) suggest that the main source of the effect on identity is not the

stronger feeling with Taiwan, but a weaker feeling with China. This is supported by the fuzzy coefficient in row (4) which shows a significant negative effect on $China10$. As shown in Figure 11 (b) and (d), the effects of *Knowing Taiwan* series on individuals' response in 2013 *Taiwan10* and *China10* questions become much more significant and the magnitudes of the coefficients become much larger in the expected signs – more Taiwan identity and less China identity. Excluding the potential non-complying cohorts, the treated group reports about 2 points higher Taiwan identity and 3 points lower China identity on a scale of 0 to 10.

From row (5) to row (9), it seems there is no evidence supporting the hypothesis that the treatment of the new textbook increases students' civic participation. Although $participated$ and $petition$ see larger magnitudes in coefficients when excluding potentially non-complying cohorts, most of other coefficients remain statistically insignificant.

4.5.3 Robustness checks

4.5.3.1 Fuzzy of different windows

In the last section, the fuzzy style RD analysis drops one year above and one year below the cutoff cohort. Here I conduct more possible fuzzy RD analysis by dropping various sizes of cohorts.

Table 25 shows the results of the robustness check of different sizes of dropped cohorts. Column (1) does not drop any year as in Table 24 column (1). Columns (2) drops one year above the cutoff, that is, the last cohort who learns the old curriculum. Columns (3) drops one year below the cutoff, that is, the last cohort who learns the old curriculum.

Column (4) is from Table 24 column (2). Columns (5) and (6) drop two years: one year above and below and cutoff, two years above the cutoff, and two years below the cutoff.

Generally, as expected, the more years dropped, the more significant the coefficients. Dropping younger cohorts has a slightly better effect on increasing the visibility of the coefficients of interest, which suggests a potential delaying effect due to teachers' inertia. However, it does not change the main result.

4.5.3.2 Short-run vs. long-run

As mentioned in Section 4.2.4, whether we can observe an effect of education on social outcome very likely depends on the duration of the available data. The analysis in Table 24(a) and 24(b) attempts to examine the potential difference. In Table 24(a), I conduct a subsample analysis and divide the sample by survey year. The cutoff year is 2010 in order to balance the size of the sample.[47]

As shown in Table 24(a) column (1), individuals are 16 percentage points more likely to report Taiwan as the only nation to identify with ($Taiwan\ only$), in the years before 2010. This effect will become insignificant in years after 2010 in column (2). However, while only the subsample before 2010 exhibits a positive effect of the textbook reform, the subsample after 2010 shows a significant and positive effect when excluding individuals near the cutoff. Unlike the sharp specification, the full sample specification also shows statistical significance: the complying treated group is 12.1 percentage points more likely to report being a Taiwanese only. For the variable that shows identity with at least

[47] Chen *et al*. (2016) find *Knowing Taiwan* only has a positive effect on Taiwan's national identity before 2010, and conclude that the effect lasts only in the short run.

Taiwan ($Taiwan\ at\ all$), like the previous specifications, no significant effect is shown from the data.

Similar reverse happens to the variable $participated$. Only the subsample before 2010 exhibits a positive effect of the textbook reform. Treated individuals are seven percentage points more likely than the untreated to participate in political movements. However, the complying treated group is 9.5 percentage points more likely to participate in political movements after 2010.

$Voted$ is both significantly negative before and after 2010. Before 2010, waves 2005, 2007, and 2009 are included. In these waves, the average ages of the sample around the cutoff are 21, 23, and 25. As mentioned, the voting age is 20 and the effective youngest age for voting behavior is 21 years. This may explain their lower voting turnout relative to their older counter part on the other side of the cutoff. Again, the small number of observations can dilute the accuracy. $Lobby$ becomes significant after 2010 but still negative.

4.6 Conclusion

Economists have started looking at education contents in addition to quantitative measurement of education.[48] This paper studies whether the 1997 $Knowing\ Taiwan$ curriculum reform, which puts more attention to Taiwan's own history and societal traits than before, can explain the national identity of Taiwan and individuals civic participation in their home country.

[48] For example, Cantoni and Yuchtman (2013).

The results suggest that the new curriculum has a significant effect on increased national identity with Taiwan, but has limited explaining power for measures of civic participation. For national identity, on the one hand, the findings is consistent with existing research that find causal positive effects of educational content curriculum in explaining national identity orientation (Clots-Figueras and Masella, 2013; Cantoni *et al*., 2017a; Chen *et al*., 2016). On the other hand, it finds that the main component that is changing is individuals' decreased identity with China. The regression discontinuity design addresses the issue that different cohort may be systematically different in terms of national identity and civic participation, and therefore plausibly assure the causal interpretation of the results.

For civic participation, this study attempts to provide a mechanism and test whether education content may be the channel through which education affects factors that are important to democracy. Although the result of this study stands closer to the argument by Acemoglu *et al*. (2005) who do not find education a necessary stimulant of civic participations that help democracy, the lack of evidence does not allow such interpretation.

The sampling nature of TSCS leaves a note and an interesting research question. Since the survey does not include immigrants, it is uncertain whether this sample selection, although a small portion of the population, would underestimate or overestimate the effects. To determine this, one has to find whether immigrants' national identity and (potential) civic participation are systematically different from non-immigrants, by destination country and by time.

This paper contributes to the literature in the following ways. First, politically motivated education reforms are believed to have political consequences and therefore

arouse debates.[49] However, these claims lack sufficient evidence to support. This paper confirms the causal effect of a politically motivated education reform of educational content on a broad range of measurements. Second, it adds to the literature by finding insignificant evidence of education contents on civic participation. Third, it broadly contributes to the literature of ideology formation and persuasion.

Lastly, because of the particular context of this reform, which turns the focus of the textbook content from a neighbor country to a local one, this paper asks whether studying knowledge about local area makes citizens or residents more committed to the area. For the studies of immigration in regional economics, education, and immigration politics, it sheds light on the potential determinant of immigrants' identity and its consequent social outcomes (e.g., Akerlof and Kranton, 2000).

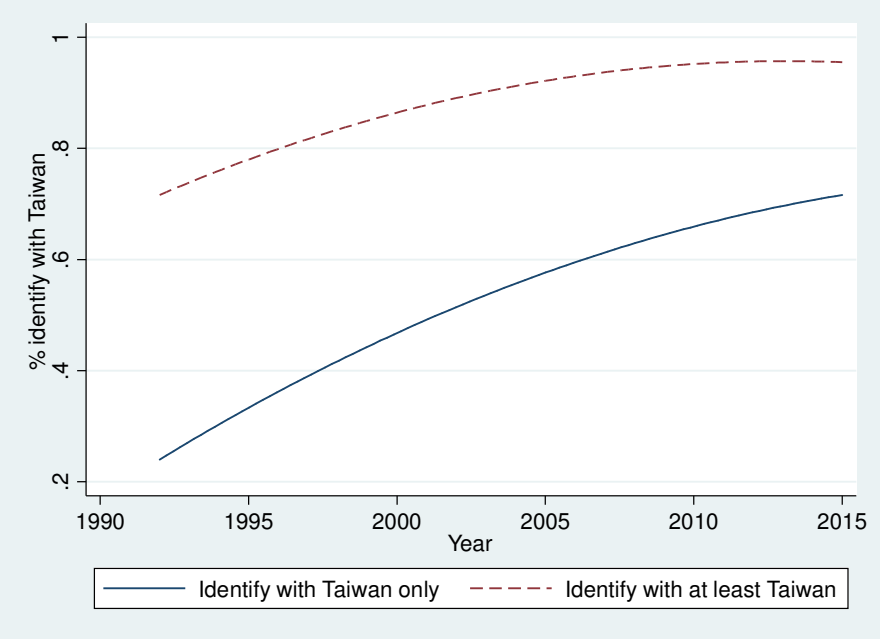
[49] Recent examples include the protest against the "Moral and national education" in Hong Kong in 2012 and the "Anti-Black Box Curriculum Movement" in Taiwan in 2015.

Table 21. Sample construction and availabilities of variables in TSCS by survey year.

Survey year/wave (# of observations)	National identity	Civic engagement variables (# of missing values)			Missing values in controls	Sample size	% missing
		Civic	Electoral	Political voice			
2003 (2,016)	$Taiwan\ only$ $Taiwan\ at\ all$	-	-	-	21 (birth month)	1,995	1.04%
2004 (1,781)	$Taiwan\ only$ $Taiwan\ at\ all$	$party$ $participated$	-	$petition, protest,$ $lobby, media$	Yes	1,781	0%
2005 (2,146)	$Taiwan\ only$ $Taiwan\ at\ all$	-	$voted$ (21)	$lobby$ $protest$ $media$	Yes	2,146	0.97%
2007(1) (2,040)	-	$party$	$voted$	$protest$	21 (birth month)	2,019	1.02%
2007(2) (2,147)	-	$participated$	-	-	Yes*	2,147	0%
2009 (2,026)	-	-	$voted$ (268+121=389)	-	12 (birth month)* + 45 (edu)	1,580	22.01%
2010 (1,895)	$Taiwanonly$ $Taiwanatall$	-	$voted$ (33+)	$protest$ $lobby$ $media$	Yes	1,590	16.09%
2012(1) (2,134)	-	$participated$ $activity$	$voted$ (44)	-	66 (birth month) + 6 (birth place)	1,794	15.93%
2012(2) (2,072)	$Taiwan\ only$ $Taiwan\ at\ all$	$party$ $participated$	$voted$ (195)	$lobby$ $media$	41 (birth month) + 1 (birth place)**	1,638	20.94%
2013(1) (2,005)	-	$participated$	$voted$ (190)	-	Yes*	1,561	22.14%
2013(2) (1,952)	$Taiwan\ only$ $Taiwan\ at\ all$	-	$voted$ (25)	-	4 (birth place) + 12 (parents birth place)*	1,666	14.65%
2014(1) (1,875)	$Taiwan\ only$ $Taiwan\ at\ all$	$party$ $participated$	$voted$ (59)	$petition, protest,$ $lobby, media$	6 (birth month) + 3(birth place) + 19 (parents birth place)	1,527	17.23%
2015 (2,034)	$Taiwan\ only$ $Taiwan\ at\ all$	$party$ (9)	$voted$ (75)	-	27 (birth month) + 9 (birth place) + 219 (parents birth place)	1,788	12.09%
Total observations	15,514	party: 9,449 participation: 11,983	16,348	petition: 3,632 protest: 9,691 lobby: 9,692 media: 9,606	-	36,196	-

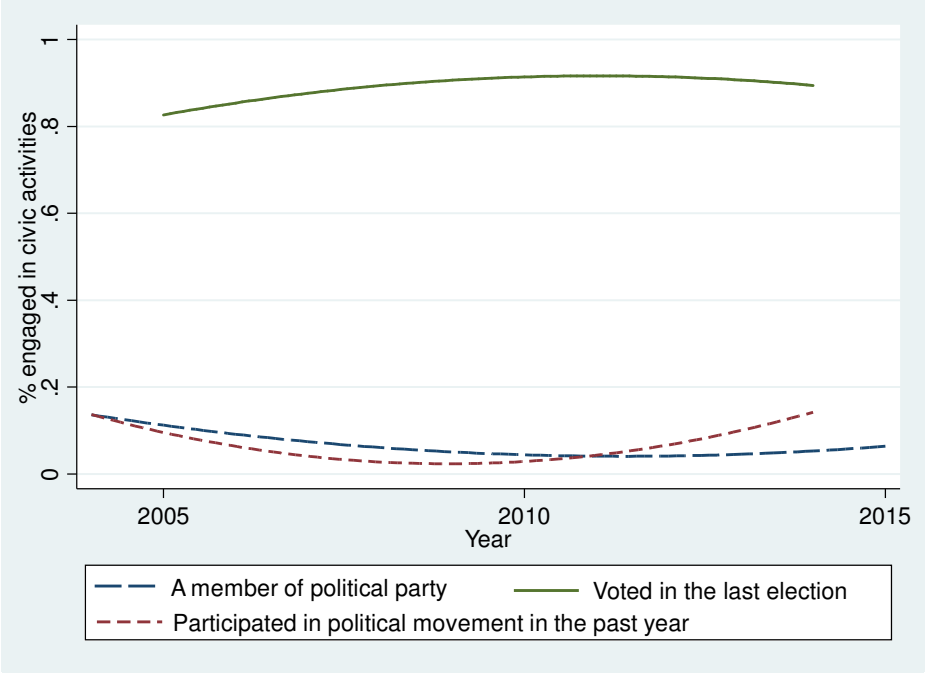
Notes: 1. $Taiwanonly = 1$ if Taiwan only is chosen, 0 if otherwise; $Taiwanatall = 1$ if Taiwan or both is chosen, 0 if otherwise.
2. $party = 1$ if the individual is a member of a political party, 0 if otherwise.
3. $participated = 1$ if the individual participated in political movements in the past year, 0 if otherwise.
4. $voted = 1$ if the individual voted in the last election, 0 if otherwise. It excludes individuals who did not have right to vote in the corresponding survey year.
5. $petition = 1$ if the individual submitted petition in the past year, 0 if otherwise.
6. Full set of controls include gender, age, parents' birth place, education, parents' education, and a dummy of "born in Taiwan."
7. *: The information of "born in Taiwan" is missing and replaced by another dummy "live in Taiwan."
8. **: The information of "born in Taiwan" is missing and replaced by another dummy "hometown is Taiwan."
9. Waves with insufficient variables for this study are not listed.

Figure 6. Individuals' national identity with Taiwan over the years



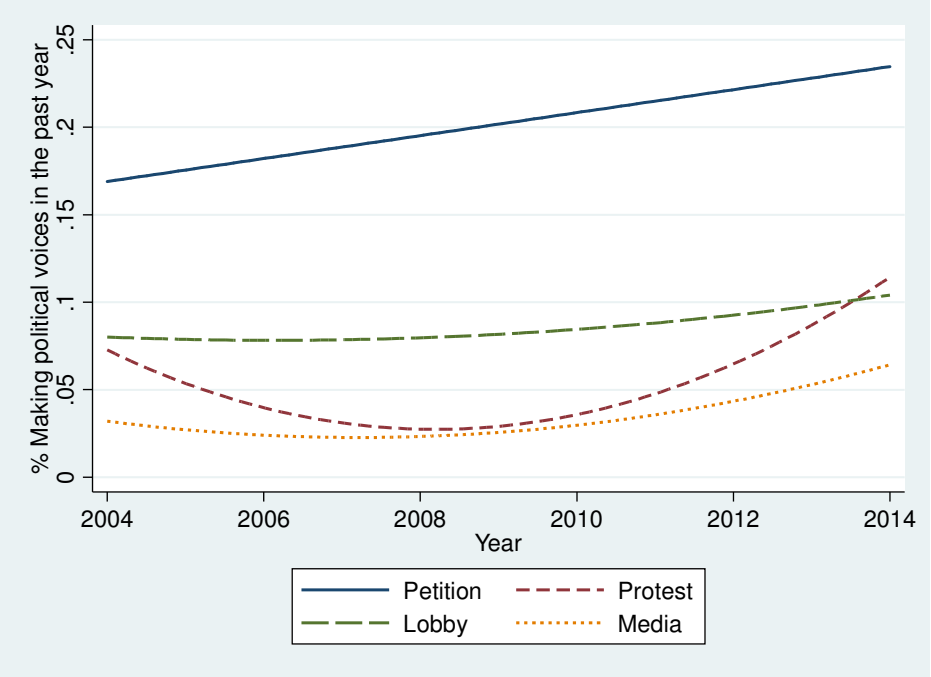
Notes:
1. Quadratic fit of the constructed sample from the Taiwan Social Change Survey.
2. $Taiwanonly = 1$ if Taiwan is the only chosen nation in reporting national identity, 0 if otherwise.
3. $Taiwanatall = 1$ if at least Taiwan is chosen in reporting national identity, 0 if otherwise.
4. Data includes waves 1992, 1995, 2000, 2003, 2004, 2005, 2010, 2012, 2013, 2014, and 2015.

Figure 7. Individuals' civic movements and electoral actions over the years



Notes: 1. Quadratic fit of the constructed sample from the Taiwan Social Change Survey.

Figure 8. Individuals' political voice over the years



Notes: 1. Quadratic fit of the constructed sample from the Taiwan Social Change Survey.

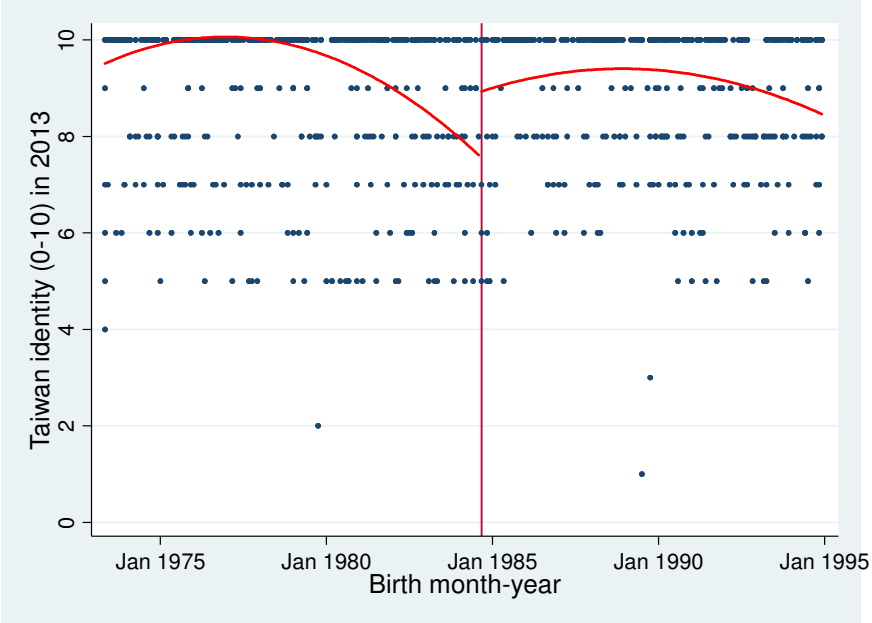
Table 22. Descriptive statistics – full sample

Variable	Mean	Standard Deviation	Min	Max	Number of observations
Male (=1)	.50	.49	0	1	36,196
Age	45.7	17.1	18	101	36,172
Born in Taiwan (=1)	.96	.17	0	1	21,710
Father born in Taiwan (=1)	.89	.30	0	1	35,947
Mother born in Taiwan (=1)	.93	.23	0	1	36,163
Live in Taiwan (=1)	.99	.05	0	1	26,316
Hometown being Taiwan	.97	.16	0	1	11,943
Knowing Taiwan (=1)	.13	.34	0	1	36,196
Taiwan at all	.94	.23	0	1	19,632
Taiwan only	.64	.47	0	1	19,632
*Taiwan*10 (2013 only)	9.12	1.58	0	10	1,900
*China*10 (2013 only)	3.66	3.60	0	10	1,900
Petition	.20	.40	0	1	3,656
Protest	.06	.23	0	1	9,737
Lobby	.08	.28	0	1	9,769
Media	.03	.19	0	1	9,769
Party	.07	.25	0	1	9,793
Participated	.09	.28	0	1	12,014
Voted	.82	.37	0	1	16,807
Activity (2012 only)	.04	.19	0	1	2,134

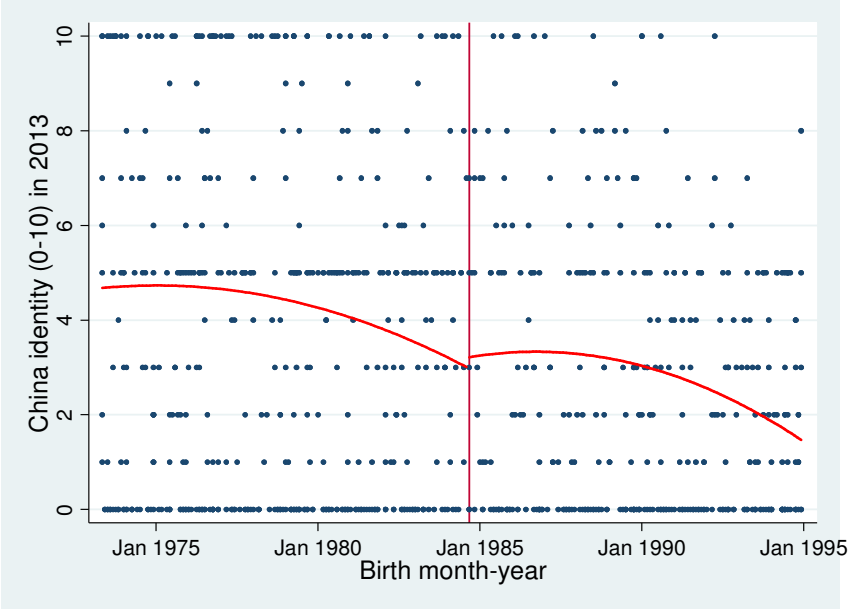
Notes: 1. *Voted* does not include individuals who did not have right to vote in the corresponding survey year.
2. *Hometown* is used to replace *birthplace* when *birthplace* is unavailable in a wave.

Figure 9. On a scale of 0 to 10, individuals' national identity with Taiwan and China in 2013

(a) Identity with Taiwan in 2013



(b) Identity with China in 2013



Notes: 1. Taiwan Social Change Survey, Wave 2013: *National Identity*.
2. The cutoff is September, 1984.
3. Unconditional on controls.

Table 23(a). Marginal effects in pooled sample: National identity

Dep. var. / Indep. var.	National identity			
	(1) Taiwan only	(2) Taiwan only	(3) Taiwan at all	(4) Taiwan at all
Knowing Taiwan	.190*** (.036)	.140*** (.018)	.020 (.015)	.011 (.012)
Gender	-.051*** (.012)	-.049*** (.014)	-.013*** (.004)	-.012** (.005)
Age	-.000 (000)	-.002*** (.000)	-.000 (000)	-.000*** (000)
Born in Taiwan	.275*** (.071)	.242*** (.056)	.168*** (.046)	.146*** (.019)
Father born in Taiwan	.194*** (.036)	.201*** (.027)	.049** (.022)	.056*** (.014)
Mother born in Taiwan	.052* (.027)	.049** (.024)	.006 (.006)	.009 (.008)
Education	Yes	Yes	Yes	Yes
Parent's education	Yes	Yes	Yes	Yes
Survey year dummies	No	Yes	No	Yes
Number of observations	15,514	15,514	15,467	15,467
Pseudo R-squared	0.0792	0.1002	0.0841	0.1142

Notes: 1. $Taiwanonly = 1$ if Taiwan only is chosen, 0 if otherwise; $Taiwanatall = 1$ if Taiwan or both is chosen, 0 if otherwise.
2. $party = 1$ if the individual is a member of a political party, 0 if otherwise.
3. $participated = 1$ if the individual participated in political movements in the past year, 0 if otherwise.
4. $voted = 1$ if the individual voted in the last election, 0 if otherwise. It excludes individuals who did not have right to vote in the corresponding survey year.
5. $petition = 1$ if the individual submitted petition in the past year, 0 if otherwise.
6. Full set of controls include gender, age, parents' birth place, education, parents' education, and a dummy of "born in Taiwan."
7. Standard error clustered at the year level.
8. ***: $p < 0.01$; **: $p < 0.05$; *: $p < 0.1$.

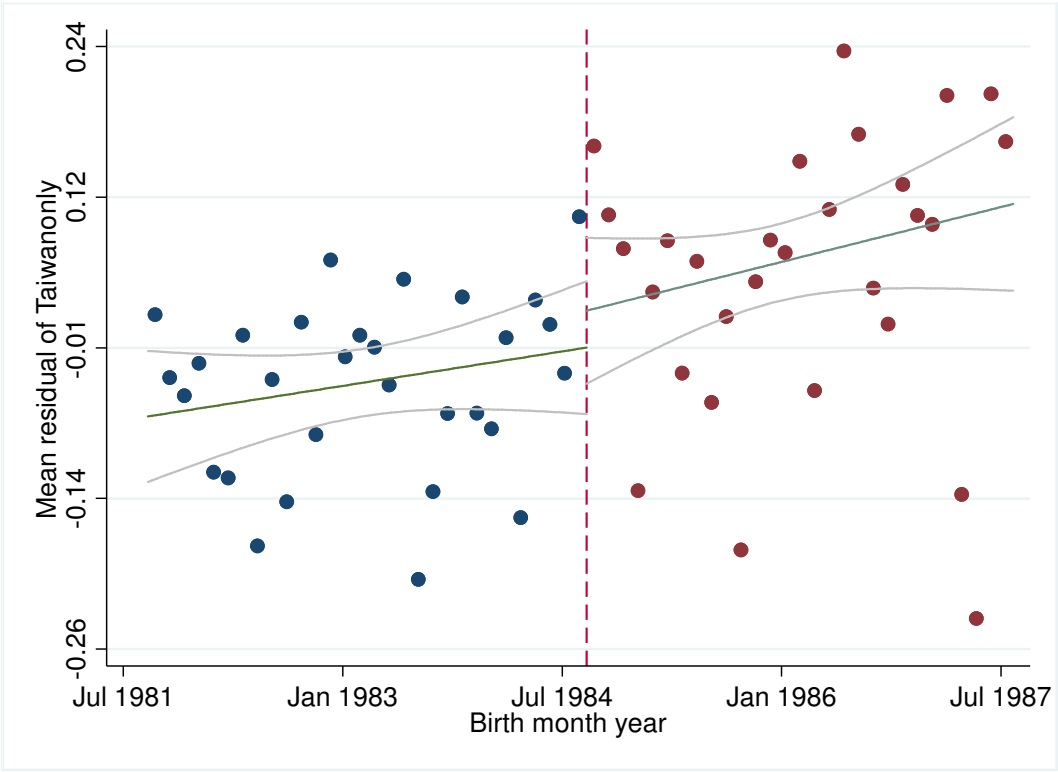
Table 23(b). Unconditional marginal effects in pooled sample: Civic participation

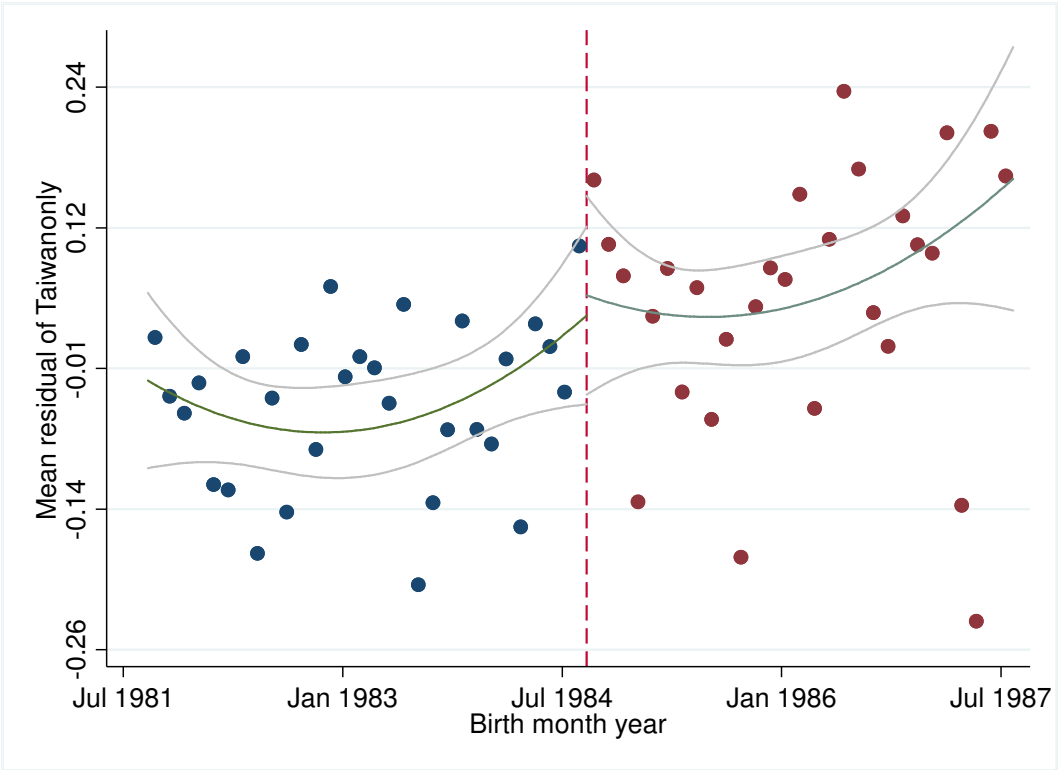
Dep. var. / Indep. var.	Civic participation						
	Civic		Electoral	Political voice			
	(1) party	(2) participa-ted	(3) voted	(4) petition	(5) protest	(6) lobby	(7) media
Knowing Taiwan	-.035*** (.007)	-.015* (.009)	-.069* (.038)	-.057*** (.002)	-.001 (.003)	-.034*** (.010)	-.012*** (.004)
Gender	.036*** (.002)	.026*** (.006)	-.025*** (.006)	.013 (.014)	.014** (.006)	.040*** (.006)	.006 (.004)
Age	.002*** (.000)	.002*** (.000)	.005*** (.000)	.001*** (.000)	.001*** (.000)	.002*** (.000)	.000*** (.000)
Born in Taiwan	-.011 (.007)	-.020 (.013)	.069* (.039)	.112*** (.029)	.017** (.007)	-.004 (.008)	.017 (.010)
Father born in Taiwan	-.050*** (.011)	-.053*** (.009)	-.019* (.011)	-.023 (.015)	-.017 (.014)	-.021 (.016)	-.013* (.007)
Mother born in Taiwan	-.009 (.011)	-.008 (.008)	-.029*** (.010)	-.117*** (.004)	-.043** (.017)	-.001 (.019)	-.000 (.009)
Education	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Parent's education	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Survey year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	9,449	11,983	16,348	3,632	9,691	9,692	9,606
Pseudo R-squared	0.1508	0.0891	0.0805	0.0998	0.0896	0.0597	0.0979

Notes: 1. $Taiwanonly = 1$ if Taiwan only is chosen, 0 if otherwise; $Taiwanatall = 1$ if Taiwan or both is chosen, 0 if otherwise.
2. $party = 1$ if the individual is a member of a political party, 0 if otherwise.
3. $participated = 1$ if the individual participated in political movements in the past year, 0 if otherwise.
4. $voted = 1$ if the individual voted in the last election, 0 if otherwise. It excludes individuals who did not have right to vote in the corresponding survey year.
5. $petition = 1$ if the individual submitted petition in the past year, 0 if otherwise.
6. Full set of controls include gender, age, parents' birth place, education, parents' education, and a dummy of "born in Taiwan."
7. Standard error clustered at the year level.
8. ***: $p < 0.01$; **: $p < 0.05$; *: $p < 0.1$.

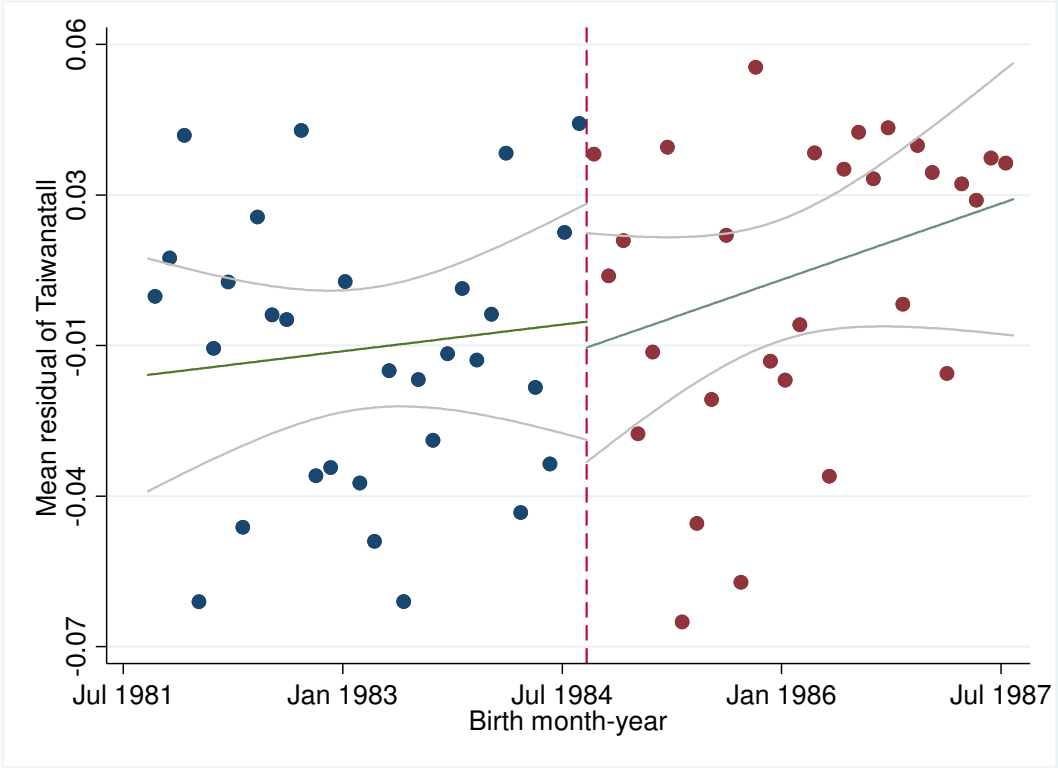
Figure 10. Regression discontinuity: National identity with Taiwan

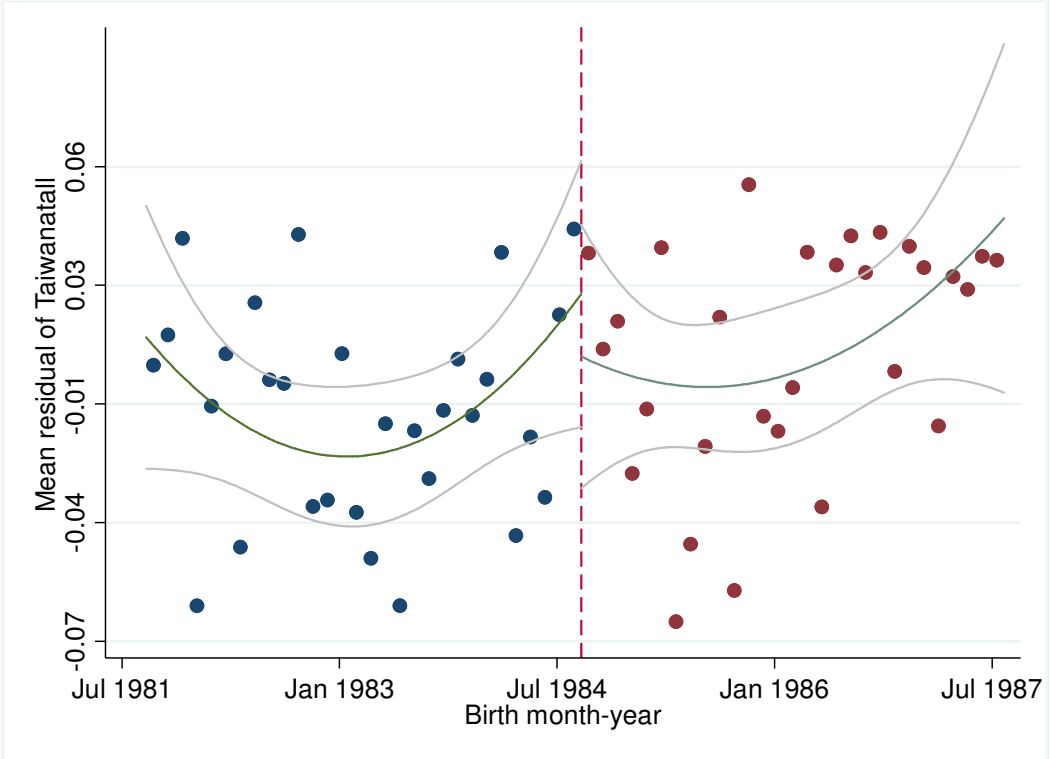
(a) Mean residual: People reporting only Taiwan to identify





(b) Mean residual: People reporting at least Taiwan to identify

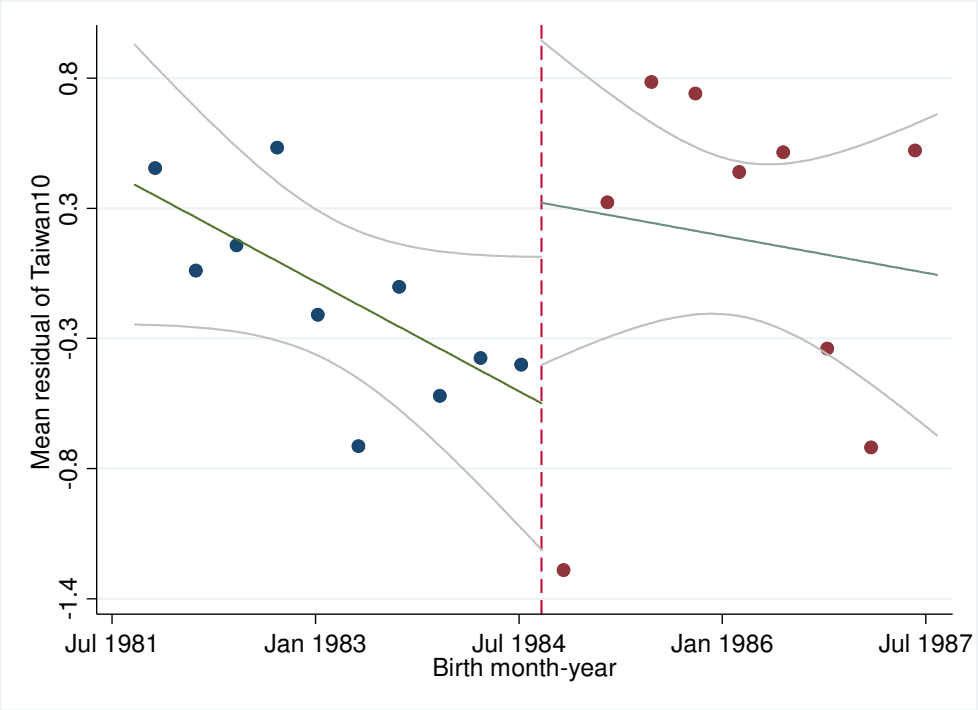


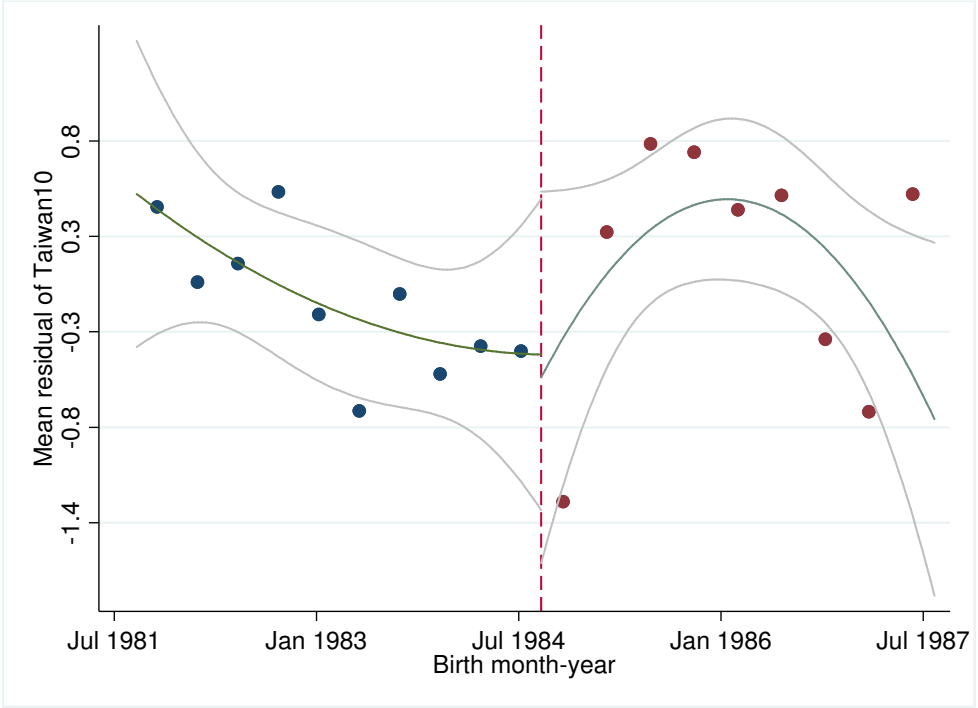


Notes: 1. Bandwidth is three years around the cutoff.
2. 2057 observations are included.
3. Each of (a) and (b) includes linear fit and quadratic fits.

Figure 11. Mean residuals of measure of Taiwan10 and China10, linear fits and quadratic fits

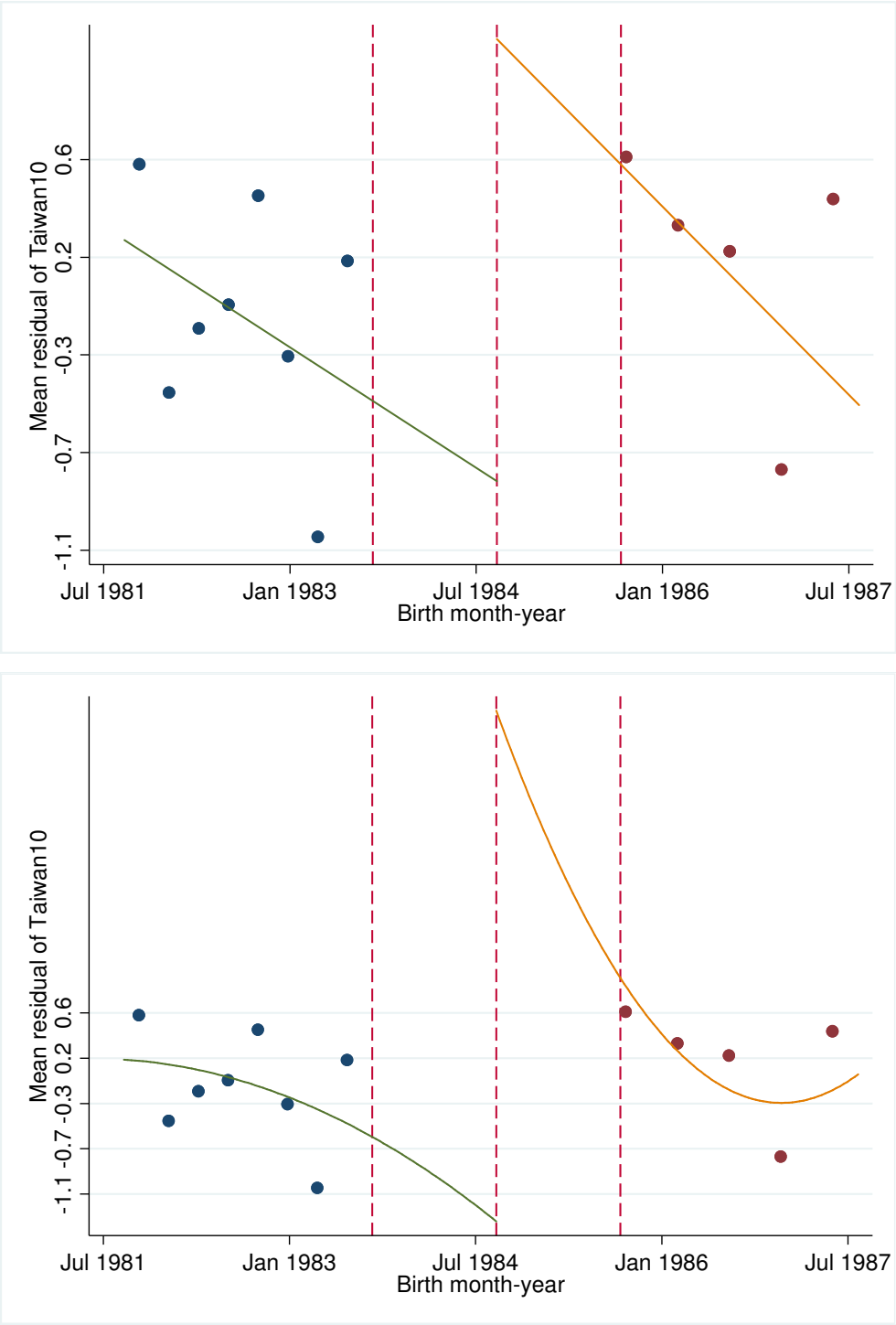
(a) $Taiwan10$





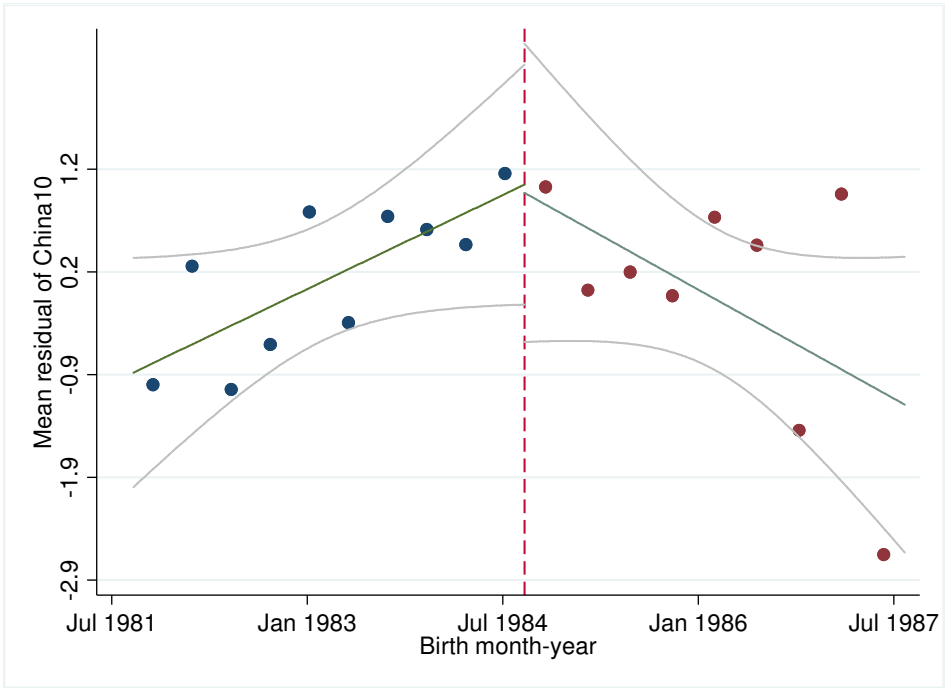
Notes: 1. 198 observations are included.

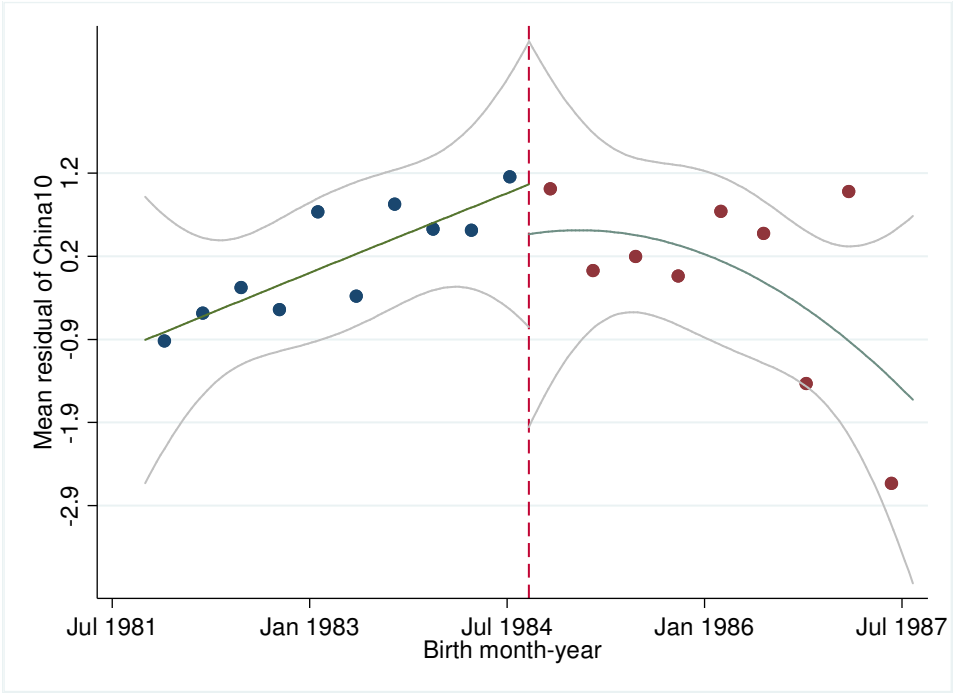
(b) *Taiwan10*, excluding one year above and below the cutoff



Notes: 1. 132 observations are included.

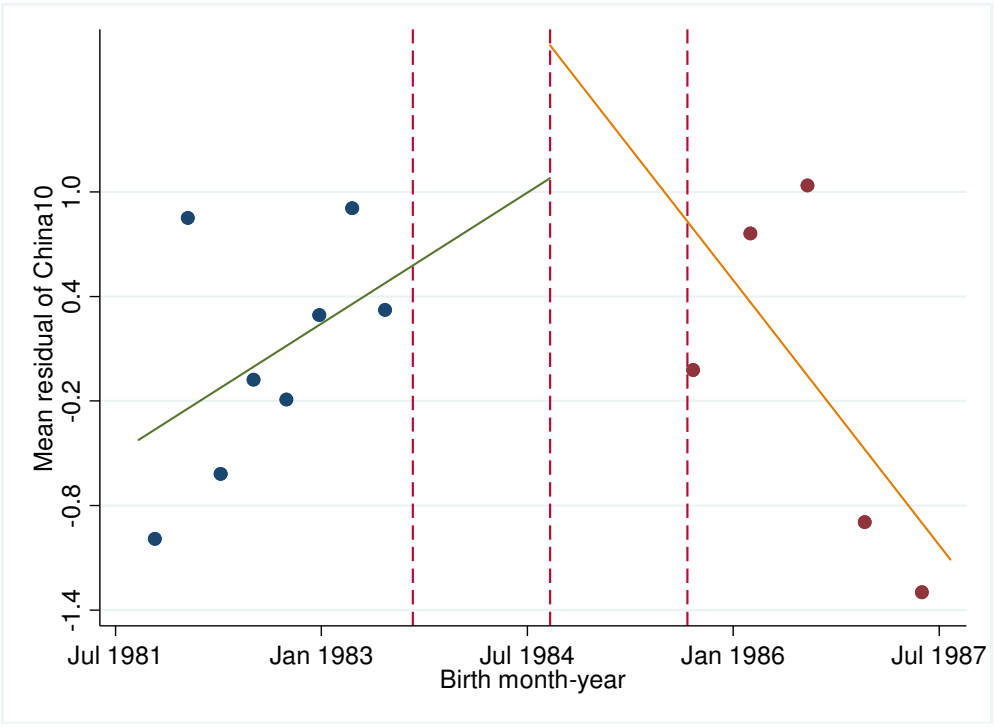
(c) $China10$

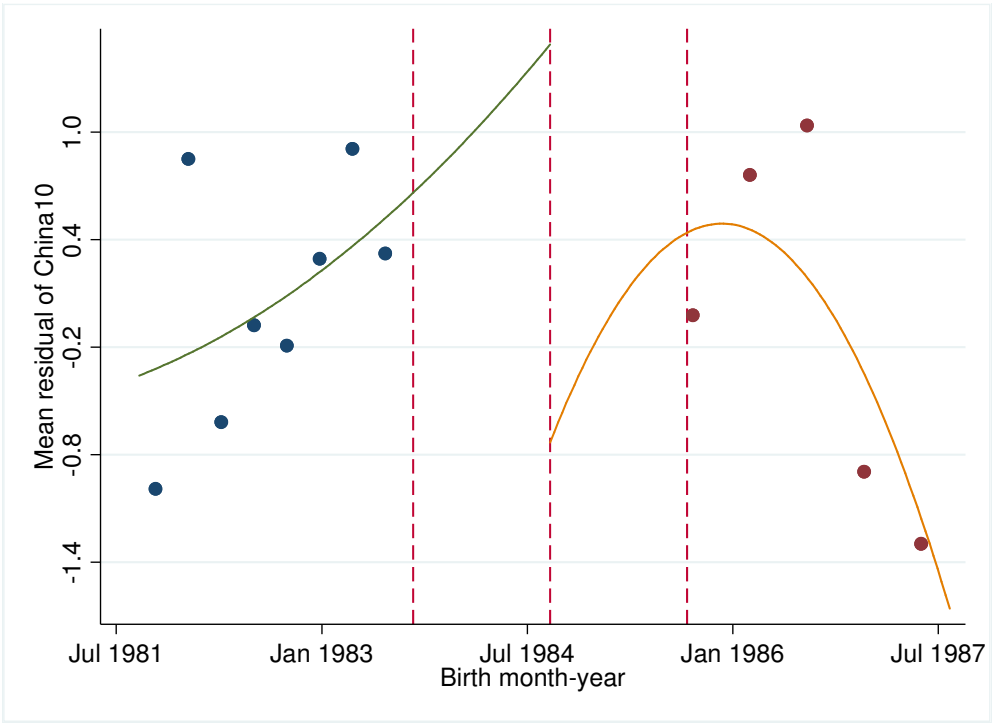




Notes: 1. 198 observations are included.

(d) *China10*, excluding one year above and below the cutoff





Notes: 1. 132 observations are included.

Table 24. RDD coefficients: National identity and civic participation

Indep. var. / Dep. var.	*Knowing Taiwan*		Effective # of observations	
	(1) Full sample	(2) Full fuzzy	Left	Right
(1) *Taiwan only*	.043 (.035)	.100** (.045)	1118	704
(2) *Taiwan at all*	.000 (.015)	.023 (.019)	1169	762
(3) *Taiwan*10	.448 (.492)	1.974*** (.515)		
(4) *China*10	-.078 (.999)	-3.109*** (.981)		
(5) *party*	.017 (.017)	.004 (.022)	795	539
(6) *participated*	.019 (.021)	.033 (.020)	1005	812
(7) *voted*	-.032 (.046)	.000 (.055)	743	685
(8) *petition*	.002 (.062)	.106 (.071)	348	182
(9) *protest*	.011 (.019)	-.008 (.024)	700	424
(10) *lobby*	-.009 (.015)	.001 (.022)	706	579
(11) *media*	.014 (.025)	-.001 (.019)	704	508

Notes: 1. Standard error clustered at the birth month-year level.
2. ***: $p < 0.01$; **: $p < 0.05$; *: $p < 0.1$.
3. All specifications are conditional on gender, age, birth country, parents' birth country, education, and parents' education.
4. Bandwidth: MSE optimal.
5. Kernel: triangular.
6. Fuzzy: one year below and above the cutoff deleted.

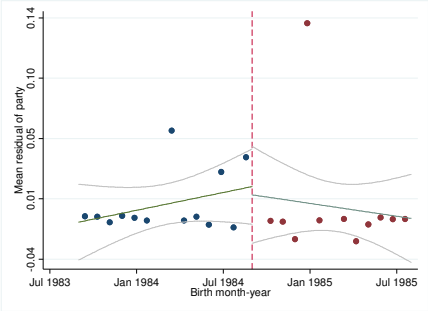
Table 25. RDD robustness checks on the size of dropped cohorts

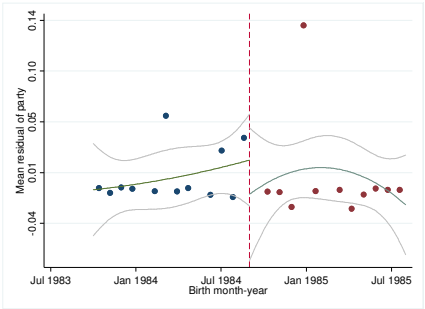
	Independent variable	*Knowing Taiwan*					
Dependent variables	Sample	(1) Full sample	(2) Drop 1 yr above the cutoff	(3) Drop 1 yr below the cutoff	(4) Drop 1 yr below and above the cutoff	(5) Drop 2 yrs above the cutoff	(6) Drop 2 yrs below the cutoff
(1) Taiwan only	Coefficient	.043 (.035)	.055 (.044)	.068* (.037)	.100** (.045)	.084** (.034)	.105*** (.037)
	Eff. # of obs (left/right)	1118/704	1003/595	1490/933	1118/704	1079/571	1356/884
(2) Taiwan at all	Coefficient	.000 (.015)	-.014 (.023)	.025** (.013)	.023 (.019)	.020 (.015)	.013 (.014)
	Eff. # of obs (left/right)	1169/762	678/412	1287/834	1169/762	791/418	1273/834
(3) Taiwan10	Coefficient	.448 (.492)	1.378*** (.516)	.219 (.492)	1.974*** (.515)	.941** (.426)	-1.126*** (.415)
	Eff. # of obs (left/right)	214/200	63/57	117/98	214/200	90/87	96/82
(4) China10	Coefficient	-.078 (.999)	-1.995** (1.017)	.123 (.612)	-3.109*** (.981)	-5.463*** (.974)	.108 (.578)
	Eff. # of obs (left/right)	214/200	83/77	117/98	214/200	87/80	99/85
(5) participated	Coefficient	.019 (.021)	.005 (.023)	.055*** (.017)	.033 (.020)	-.042 (.028)	.026 (.021)
	Eff. # of obs (left/right)	1005/812	947/686	779/624	1005/812	484/350	903/725
(6) voted	Coefficient	-.032 (.046)	-.060* (.033)	.026 (.069)	.000 (.055)	.029 (.029)	-.053 (.067)
	Eff. # of obs (left/right)	743/685	602/536	457/458	743/685	602/431	548/500

Notes: 1. Standard error clustered at the birth month level.
2. ***: $p < 0.01$; **: $p < 0.05$; *: $p < 0.1$.
3. All specifications are conditional on gender, age, birth country, parents' birth country, education, and parents' education.
4. Bandwidth: MSE optimal.
5. Kernel: triangular.

Figure 12. Mean residuals of measures of civic participation (Linear and quadratic fits)

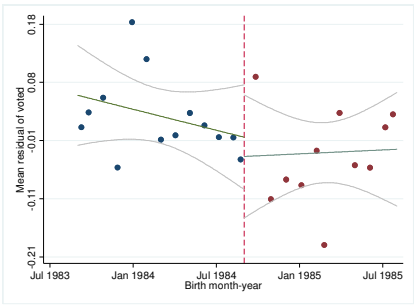
(a) Party

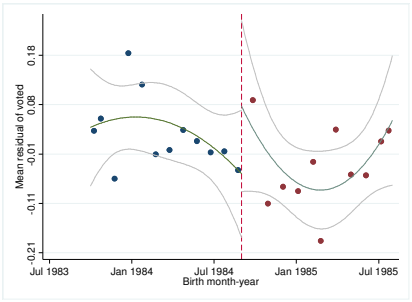




Note: 1. 364 observations are included.

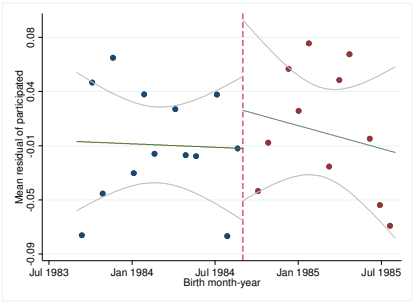
(c) voted

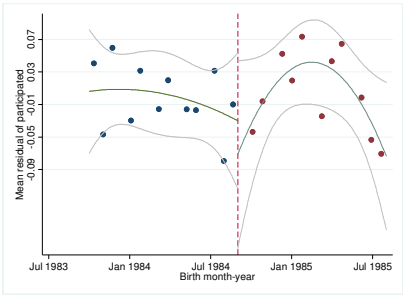




Notes: 1. 889 observations are included.
2. Excludes individuals below voting ages.

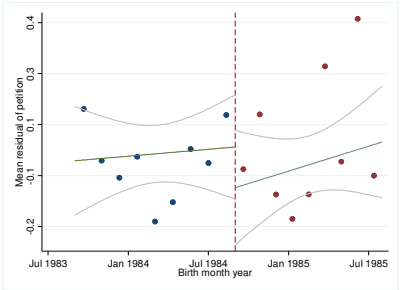
(b) Participated

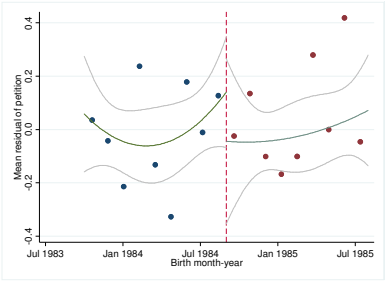




Note: 1. 469 observations are included.

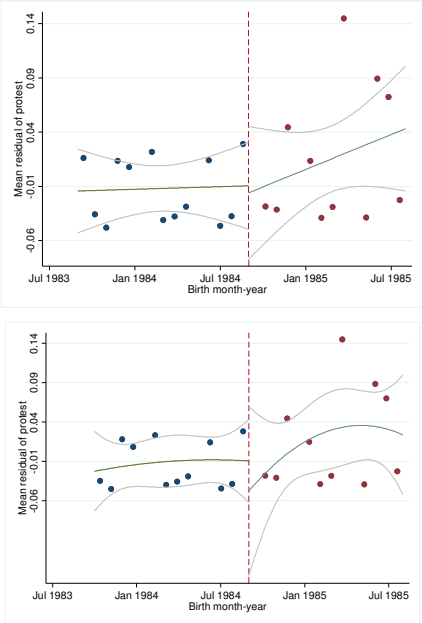
(d) petition





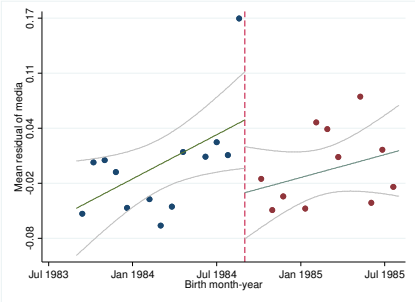
Note: 1. 158 observations are included.

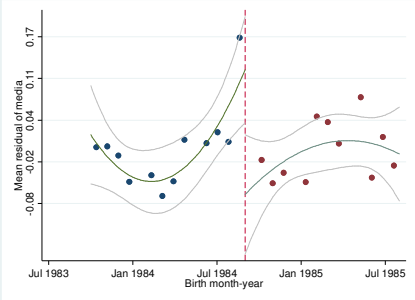
(e) protest



Note: 1. 400 observations are included.

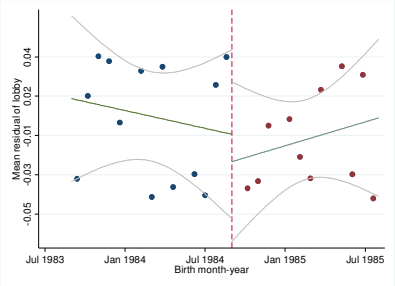
(g) media

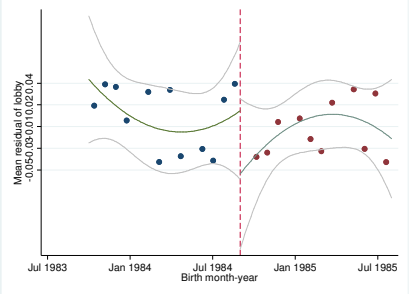




Note: 1. 399 observations are included.

(f) lobby





Note: 1. 399 observations are included.

Appendix A. Instructions for the group experiment

A. Introduction

This is an experiment about group decision making. In this experiment, you will make a number of decisions as part of a group. Funding for this experiment has been provided by the Center for Economic Behavior, Institutions and Design (CEBID) and the Department of Economics at Rutgers University. You will be paid $5 for your participation plus an additional amount which depends upon the decisions that you and your group members make and upon random luck. Please read these instructions carefully so that you understand how your decisions help to determine your earnings.

This experiment consists of two parts. There are six rounds in each part. At the end of the experiment, your payoff in one randomly chosen round will determine your monetary payment for that round. In other words,

Your total earnings = $5 + payoff in one round in part 1 + payoff in one round in part 2.

The currency in this experiment is the Experimental Currency Unit, or ECU. At the end of the experiment, your payoffs in ECUs will be converted into U.S. Dollars at the rate of $1 per 8 ECUs, and you will be paid this amount in cash before you leave the experiment.

B. Individual introduction

In this part of the experiment, you will be making a series of decisions about whether to invest at a cost in a project. In each of the six decision-making rounds, you will independently decide whether to invest or not. The payoffs that you earn in each round will only depend upon your own cost, your own investment decision in that round,

and your own score in the previous test. The specifics of how your payoff varies with the effort levels you choose will be explained below.

Your payoff of one randomly chosen round will determine your total monetary payoff in this part of the experiment.

Here we explain how your payoff is determined. Remember, payoff does not accumulate. The payoff in each round is independent of your payoff in another. At the end of this stage, one payoff of the six rounds will be randomly drawn to determine your monetary payment of this stage.

1. ENDOWMENT

In each of the following six rounds, you will decide whether or not to invest at your own cost. In each round, you will be given 50 ECU to begin with.

2. GROSS RETURN

The gross return of your investment is determined by your score in the previous test. If your score is in the top 50% amongst all the participants in this room right now in this experiment, the gross return of your investment will be 50 ECU. If your score is in the bottom 50% amongst all the participants in this room, the gross return of your investment will be 20 ECU. The gross return is always zero when you choose not to invest.

3. COST

In addition to the gross return, a cost will occur each time you invest. The cost is always zero when you choose not to invest. Unlike the gross return, the cost may vary in each

round. Before you make the investment decision in each round, you will learn the value of the cost in that round.

4. PAYOFF and the formula

The net payoff in each round is the endowment (50) plus the gross return of your investment (which depends on your score for the pretest) minus the cost in that round. In short, it is described by this formula:

Net payoff = Endowment** + Gross return of investment – Cost of investment in that round.

**Where endowment is equal to 50.

5. EXAMPLE

Suppose the cost in a certain round is 33 ECU.

If your score is amongst the top half, your gross return of investment is 50 ECU. Therefore, your payoff from investing in this round is 50 + 50 – 33 = 67 ECU.

If your score is amongst the bottom half, your gross return of investment is 20 ECU. Therefore, your payoff from investing in this round is 50 + 20 – 33 = 37 ECU.

If you choose not to invest, your payoff is 50 + 0 – 0 = 50 ECU, since there is neither a cost nor return for not investing.

C. Group introduction

In this part of the experiment, you will be randomly organized into a group of three. You will be making a series of decisions about whether or not to contribute at a cost to this group endeavor. In each of six decision-making rounds, each member of your group will independently decide whether to invest or not. The payoffs that you earn in

each round will depend upon the investment decisions made by all members of your group in that round, your cost, and the test scores of all your team members in the previous test. The specifics of how your payoff varies with you and your group's investment decisions and test scores will be explained below.

One payoff will randomly be selected to determine your total monetary payoff in this stage of the experiment.

Here we explain how your payoff is determined. Remember, payoff does not accumulate. The payoff in each round is independent of your payoff in another. At the end of this stage, one payoff of the six rounds will be randomly drawn to determine your monetary payment of this stage.

1. ENDOWMENT

   In each of the following six rounds, you will decide whether or not to invest at your own cost. In each round, you will be given 50 ECU to begin with.

2. GROSS RETURN

   The gross return to your investment is determined by the scores of all your teammates in the previous test AND the total investment of your team.

   If all three team members' scores are in the top 50% amongst all the participants in this room right now in this experiment AND if all three team members choose to invest, the gross return from your investment will be 30 ECU. If two of three team members' scores are in the top 50% AND if all three team members choose to invest, no matter what your own score is, the gross return from your investment will be 10 ECU. If one of three team members' scores is in the top 50% AND if all three team members choose to invest, no matter what your own

score is, the gross return from your investment will be -10 ECU (a loss.) If none of your team members' scores are in the top 50% AND if all three team members choose to invest (including you,) the gross return from your investment will be -30 ECU (a loss.)

If two out of three team members choose to invest, no matter what your own score is and regardless of whether you are one of the investors or not, the above mentioned gross returns will be multiplied by a factor of 2/3.

If one out of three team members choose to invest, no matter what your own score is and regardless of whether you are the investor or not, the above mentioned gross returns will be multiplied by a factor of 1/3.

The gross return is always zero when no one chooses to invest.

Summary Table of Gross Return

This following table shows how the test scores and decisions of you and your teammates jointly determine the gross return to investing. This table will be provided later each time when you make your investment decision.

		Number of team members score in the top half			
		3	2	1	0
	3	30	10	-10	-30
	2	20	6.67	-6.67	-20
	1	10	3.33	-3.33	-10
	0	0	0	0	0

Table A1. Gross returns in group investment

3. COST

However, a cost will occur each time you invest, which will be subtracted from your gross return. Unlike the gross return though, the cost may vary in each round. Before you make the investment decision in each round, you will learn the value of the cost in that round.

When the cost is a negative number, it is a subsidy. In other words, instead of spending money to invest, you are receiving money to make investment.

The cost or the subsidy is always zero when you choose not to invest.

4. PAYOFF and the formula

The net payoff in each round is the endowment (50) plus the gross return of your investment (which depends on your team's scores for the pretest) minus the cost in that round. In short, it is described by this formula:

Net payoff = Endowment** + Gross return of investment – Cost of investment in that round. **Where endowment is equal to 50.

5. EXAMPLES

Example 1.

Suppose your cost in a certain round is 10 ECU.

Suppose all of your team members choose to invest. Your payoff from investing is 50 + 30 – 10 = 70 if all of your scores are in the top half. Your payoff is 50 + (-30) – 10 = 10 if all of your scores are in the bottom half. Your payoff is 50 if ALL of you choose not to invest.

If all of your team members' scores are in the top half, and if you alone choose not to invest, your payoff is 50 + (30)*(2/3) – 0 = 70 ECU, since there is no cost or subsidy for not investing.

If two out of three in your team scored in the top half, and one person on your team chooses to invest, your gross return to investment is 10*(2/3) = 6.66 ECU if you also invest and 10*(1/3) = 3.33 ECU if you do not invest. Therefore, your payoff from investing in this round is now 50 + 6.66 – 10 = 46.66 ECU. If you choose not to invest, your payoff is 50 + 3.33 – 0 = 53.33 ECU, since there is no cost for not investing.

Example 2.

Suppose your cost in a certain round is -10 ECU. That is, you will be receiving 10 ECU if you choose to invest.

Suppose all of your team members choose to invest. Your payoff from investing is 50 + 30 – (-10) = 90 if all of your scores are in the top half. However, if all of your scores are in the bottom half, your payoff is 50 + (-30) – (-10) = 30. If ALL of you choose not to invest, your payoff is 50.

If all of your team members' scores are in the top half, and if only one another team member chooses to invest, your payoff is 50 + (30)*(1/3) – 0 = 60 ECU, since there is no cost or subsidy for not investing.

If one out of three in your team scored in the top half, and one person on your team chooses to invest, your gross return to investment is (-10)*(2/3) = -6.66 ECU if you also invest and (-10)*(1/3) = -3.33 ECU if you do not invest. Therefore, your payoff from investing in this round is now 50 + (-6.66) – (-10) = 53.34 ECU. If you choose

not to invest, your payoff is 50 + (-3.33) – 0 = 46.67 ECU, since there is no cost (subsidy here) for not investing.

Example 3.

Suppose your cost in a certain round is 5 ECU.

If you and one of your teammates scored in the top half, and one person on your team chooses to invest, your gross return to investment is 10*(2/3) = 6.66 ECU if you invest and 10*(1/3) = 3.33 ECU if you do not invest. Therefore, your payoff from investing in this round is now 50 + 6.66 – 5 = 51.66 ECU. If you choose not to invest, your payoff is 50 + 3.33 – 5 = 48.33 ECU, since there is no cost for not investing.

D. Communication (in treatments with communication)

In each round, after you learn the cost and before you make your investment decision, you will have the opportunity to communicate with all members of your group. You are free to communicate about what investment decision you intend to choose, or anything else that you think would be useful to communicate prior to the start of play. The only restriction is that YOU MAY NOT REVEAL YOUR IDENTITY in your messages. We will record the messages to verify whether this restriction is violated.

E. Decision making screen

1. Individual decision making screen

In this round, you cost is ___.

If your score in the pretest is in the top 50%, your payoff will be 50 + 50 - ___ = ___.

If your score in the pretest is in the top 50%, your payoff will be 50 + 20 - ___ = ___.

If you choose not to invest, your payoff will be 50.

Your choice is: ___ Invest.

___ Not invest.

2. Group decision making screen

In this round, your cost is _____.

Your payoff will be the Endowment (50) + Gross Return (see table below) - _____.

This following table shows how the test scores and decisions of you and your teammates jointly determine the gross return to investing. This table will be provided later each time when you make your investment decision. Your cost is zero when you choose not to invest.

(Insert Table A1. Gross returns in group investment.)

Your choice is: ___ Invest.

___ Not invest.

Appendix B. Sample chat records

Table B1. Sample chat records

Treatment C1; Round 3; Session 2			
Group 1, cost = 2	Group 2, cost = -2	Group 3, cost = -7	Group 4, cost = 2
9: let's invest 4: 2? 12: this round is cheap 9: the cost is only 2 12: lets all invest 4: okay 12: BJ lol 4: LMAOOOO 9: childish lmfao 12: haha sorry 4: I feel like this is a trap 9: i wonder what they get from making us do this 4: I sorta dont wanna invest now 4: it sounds too good to be true 12: nahh werre all gonna make mad money 9: INVEST 9: i think we got it 12: they are only gaining data from us doing this 4: my heart says no 12: so lets invest	11: we should all invest right 3: so what's a low cost? anything less than 25? 11: yeah less than 20 3: and yes, I think so! 7: okay so if its above 20 noone invest 3: it's -2 for next round 3: so all of us 7: oh wow didnt see that haha okay so all invest 3: invest 7: yeah 11: yeah 3: lol I didn't either at first	1: so everyone invest? 1: lol its hard to tell bc if we all invest and we all got it wrong then we have the biggest loss lol 10: lol true 8: so if we don't it gets bigger or smaller? 1: what? 10: it depends 10: but i dont think everyone should invest 1: it depends on how many people invest and how many people got it right 1: alright so shoudl we do 1 or 2 people invest 8: I won't then I guess 1: ok so i will 10: i will too	2: i think we should invest in this round 6: I am planning to invest in this round 6: yeaa 5: me too

Appendix C. Survey questions in Taiwan Social Change Survey[50]

(1) National identity: $Taiwanonly$ and $Taiwanatall$.

"Here are several names to call oneself. Which one do you think is the most suitable for you?" (2005, 2010, 2014, 2015.)

1. I am a Taiwanese.
2. I am a Chinese.
3. I am both Taiwanese and Chinese.
4. I am both Chinese and Taiwanese.
5. Other.
6. I cannot choose.
7. I do not understand the question.
8. I do not know.
9. I refuse to answer.

"In our society, some say they are Taiwanese. Some say they are Chinese. Do you think you are Taiwanese, Chinese, or both?" (2003, 2004, 2012, 2013, 2014,)

1. Taiwanese.
2. Chinese.
3. Both.
4. Neither.
5. Cannot decide.
6. I do not understand the question.
7. I do not know.
8. I refuse to answer.

$Taiwan10$:

"On a scale from 0 to 10, ten being 'totally Taiwanese,' zero being 'totally not Taiwanese,' what number would you choose?" (2013)

$China10$:

"On a scale from 0 to 10, ten being 'totally Chinese,' zero being 'totally not Chinese,' what number would you choose?" (2013)

(2) Civic participation

a. $party$: "Are you a member of a political party or a political group?"
b. $participated$: "Did you participate in any movements organized by political parties or groups?"

[50] The translation of the survey questions from Chinese to English is my own.

c. $voted$: “Did you vote in the last presidential/congress/mayor election?”
d. $petition$: “Have you ever done the following things: taking part in petitions.”
e. $protest$: “Have you ever done the following things: attending protests.”
f. $lobby$: “Have you ever done the following things: expressing your opinions to politicians or officials.”
g. $media$: “Have you ever done the following things: expressing your opinions through public media.”

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