Essays on the Political Economy of Welfare and Redistribution

by

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Submitted to the Department of Political Science
in partial fulfillment of the requirements for the degree of

Doctor of Philosophy in Political Science

at the

MASSACHUSETTS INSTITUTE OF TECHNOLOGY

February 2011

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Abstract

This dissertation explores two main puzzles. First, why do some countries have more generous welfare policies than others? Second, why do some people support welfare policies more than others? This collection of essays aims to answer these two questions, focusing on the political and economic determinants of welfare policy and attitudes. Chapter 2 deals with methodological issues that will be addressed in the later substantive chapters. While this chapter discusses measurement error in general, it focuses on the problem that some respondents are likely to choose around the middle for reasons other than their true moderate attitudes in many survey items. The chapter formally analyzes the effects of this "concentrated measurement error" on the bias in regression coefficient estimates. It then proposes two estimation strategies for the handling of this problem. Turning to substantive research questions, Chapter 3 addresses the determinants of government welfare spending around the world. With the use of a unique dataset that has been constructed from six different cross-country social surveys and government finance statistics, this chapter demonstrates that public ideological preferences influence government decisions regarding the size of welfare expenditure. The chapter further presents a meaningful difference between fully and less democratic countries in welfare policy responsiveness; among less democratic countries, welfare spending policies have been little affected by public preferences. The empirical findings presented in this chapter serve as better evidence to support the mechanisms that traditional representation theories offer. In Chapter 4, I turn my attention to individual-level determinants. Recognizing the unique situation of the US, where the immigrant population is large and the natives have a distinctively individualistic taste for redistribution, this chapter assesses the role of socialization and assimilation by examining the political preferences of first-, second-, and third-generation immigrants with regard to welfare spending. It provides empirical evidence that first-generation immigrants show greater support for welfare than US-born natives; however, it also shows that the political views of immigrants more closely resemble those of US-born natives the longer that the immigrants stay in the US, thereby suggesting their assimilation into US society. Furthermore, this chapter documents that the more liberal views of first-generation immigrants do not persist into the next generation due to the effects of assimilation and socialization.

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Acknowledgements

I would like to express my sincere gratitude to my main advisor, Jim Snyder. None of the projects in this dissertation would have been possible without his support, advice, encouragement, and piercingly thoughtful comments. Conversations with Jim have been valuable to every part of this dissertation, including research design, quantitative methods, and computer programming. Jim is not only the best advisor that a student could possibly work with, but he is also a wonderful mentor who is always ready with advice about life beyond the subject of research.

Jonathan Rodden also provided insight and guidance at every stage of this dissertation. All of the research topics were inspired and encouraged by Jonathan, both directly and indirectly as a result of the course that he taught. He has been always supportive and always available even though he is not in Cambridge. Gabe Lenz effectively served as my third committee member. Even before joining the committee, Gabe provided thoughtful comments on multiple drafts of this project. I am especially indebted to him for his sage advice regarding how to frame research questions and arguments for publication in competitive journals.

I would like to thank Orit Kedar for her helpful comments and support, most notably for Chapter 3 and Chapter 4 in the dissertation. Her comments were essential for the balancing of projects that could have become unduly math-oriented. I also wish to thank Jens Hainmueller for his enthusiastic explanations of various pieces of causal inference. I am undoubtedly influenced by his research methods, and this influence will be reflected in my future work as well. I would also like to thank Professor Suzanne Berger for her countless encouragements. Without her help, I could never have overcome serious adjustment issues that occurred during my first year at MIT.

I am very grateful to Professor Youngjae Jin of Yonsei University for his mentorship and friendship. He has never lost faith in me, despite seeing me go through some hard times. He also provided every possible support for my occasional field research in South Korea. I would like to thank Professor Chung-in Moon for helping me to become a better scholar, for offering me various forms of advice, and, of course, for officiating at my wedding. I also thank Professor Yongho Kim for helping me to work through moments that were frustrating and downright miserable.

Very special thanks to my dear friends at MIT. I have enjoyed working and commiserating with Erik Freeman (who became Brother Benedict Joseph upon graduation), Timea Pal, Jon

Rose, Jiyoon Kim, Carmen Taveras, Chris Wendt, Erica Dobbs, Gustavo Setrini, and William Leblanc. Rachel Wellhausen deserves to be singled out for her sincere friendship and excellent editing of some parts of the dissertation. Other fellow political scientists and economists in the Political Economy Breakfast working group have provided crucial inspiration for my research. I also have enjoyed happy distractions with BK, Jay, and David.

I offer my most heartfelt thanks to my family. The unlimited and unconditional support from my parents was key to coping with many difficulties during my years of graduate studies. Without the support of my parents-in-law, I would have spent most of my time raising my son, which would have even further delayed the finishing of my doctorate. Finally, I owe the most to my wife, Min A, who endlessly supports me and who has sacrificed so much for me to finish this dissertation. I can't imagine my life without her love.

Chapter 1

Introduction

This dissertation explores two main puzzles. First, why do some countries have more generous welfare policies than others? Second, why do some people support welfare policies more than others? During the past 20 years, considerable scholarly efforts have contributed to answering these two questions in political science and economics. My dissertation speaks to this body of literature.

Chapter 2 deals with methodological issues that need to be addressed for the substantive analyses in later chapters. One characteristic of the substantive chapters is the heavy use of survey data that is not limited to the individual data analysis. I extract some representative values from survey data, even when I rely on a cross-country dataset. For example, in Chapter 3, I obtained levels of public ideological preferences by computing the fractions of people who placed themselves as leftists (or rightists) on 10-point scaled ideological disposition items in several cross-country social surveys. While survey items provide direct measures of political behaviors, it is a well-known fact that the responses to survey items are plagued with large amounts of measurement errors that may badly bias empirical results.

Although political scientists have long been interested in understanding the measurement error problem in survey research, the existing literature has exclusively relied on the classical errors-in-variables assumption that measurement error is completely random and orthogonal to the true predictors. However, this convenient assumption is violated in some survey items, where a mid-point choice is provided in response categories. Many items in political surveys involve odd-numbered scales with a mid-point category. With these types of questions, the problem is that some respondents are likely to choose the options around the middle for reasons other than their true moderate attitudes. Despite a vast literature on this issue, few researchers have recognized that this middling tendency constitutes a non-classical measurement error that is negatively correlated with true variables. This error type is also distinguishable from a typical non-classical measurement error as it is concentrated around the middle.

In Chapter 2, I formally analyze the effects of this unique error in variables on the bias in regression coefficient estimates. I then examine several candidates for the correction of this bias. Because the measurement error is concentrated around the middle, researchers may be tempted to exclude the middle and estimate regression coefficients. However, this study formally shows that this is not an ideal solution. It also revisits the Wald and Bartlett estimators to analyze whether they can improve consistency in the presence of this "concentrated measurement error." Although this study does not provide consistent point estimation, it suggests that bounds on parameters of interest can be constructed on the basis of information obtained from biased estimators. Researchers may also obtain tighter bounds by exploiting some auxiliary information from follow-up survey questions that measure the levels of uncertainty, intensity, involvement, and importance. These measurements allow us to find a subset of individuals with a lower propensity toward the middle (e.g., certain respondents as opposed to uncertain ones). I also consider trichotomization strategy, which involves grouping the response categories into sets of three. Assuming that the observed variable has a Pearson Type-VII distribution, I show that trichotomization reduces measurement error bias. An empirical illustration follows, applying the proposed strategies to the study of ideological voting.

Turning to substantive research questions, Chapter 3 discusses why some countries have more generous welfare policies than others. In particular, I ask whether the generosity of welfare policies is associated with public ideological preferences. This association is crucial to the logical bases of a large number of previous studies on the determinants of the size of welfare programs. For example, Alesina and Angeletos (2005) argued that a country is likely to spend more on welfare if more citizens in the country believe that poor people are poor not because they are lazy but because they are unlucky. This argument strongly depends on the assumption that mass preferences influence social welfare policies. Without this assumption, association between the beliefs about the fairness of social competition and the size of welfare spending is less convincing.

With the use of a unique dataset constructed from six different cross-country social surveys and government finance statistics, I empirically investigate whether and to what extent public ideological preferences influence the size of welfare expenditure. The use of survey data to measure public preferences is not new; most applications of opinion–policy linkage employ the same approach. However, as will be discussed in Chapter 2, responses to survey items are plagued with large amounts of measurement error. To address this problem, I use several estimators such as Fuller and Lewbel to mitigate—if not eliminate—the bias from measurement error.

Chapter 3 further examines whether welfare policy responsiveness is conditioned by the level of democracy. Estimating this conditional effect may enhance our understanding of policy responsiveness. According to democratic representation theories, responsiveness is a product of a high-quality democratic system. With a high level of democracy, politicians are held accountable for public policy, and they have incentives to offer policies that are preferred by the general public. By contrast, with a low level of democracy, politicians only need a small size of winning coalition to keep their political positions due to many reasons including less institutionalized constraints on executives' power and less competitive elections. These politicians need to please the smaller group only and thus may be less responsive to what the general public wants. Therefore, a meaningful difference between fully and less democratic countries with regard to welfare policy responsiveness should serve as better evidence to support the mechanism that traditional representation theories offer.

In Chapter 4, I turn my attention to individual-level determinants, asking why some people support welfare policies more than others. Chapter 4 pays particular attention to the role of socialization and assimilation. The research exploits the unique situation of the US, where the immigrant population is large and the natives have a distinctively individualistic taste for redistribution. The flood of immigration highlights the importance of understanding the political behavior of immigrants, but it also offers a unique opportunity to study socialization more generally. Immigrants are born and grow up in their home countries, and they are then exposed to new political cultures after they emigrate. Therefore, analyzing the political behavior of immigrants and comparing it with that of natives allows us to better understand the influences of socialization and assimilation. However, immigration and socialization have been marginalized in previous research on the determinants of attitudes toward welfare.

With the use of three nationwide surveys and a scaling method to address the issue of measurement error, this study examines the political preferences of first-, second-, and third-generation immigrants with regard to welfare spending in the US. I first investigate whether first-generation immigrants differ from US-born natives. This is an empirical question, but the "American exceptionalism" literature, which states that Americans are distinctively individualistic, provides a prior belief that first-generation immigrants are more liberal than US-born natives. Because they have been raised and socialized in foreign countries, immigrants are expected to be more liberal than US-born natives, who have had lifelong exposure to deeply rooted individualism.

After establishing that first-generation immigrants are indeed more supportive of welfare spending than US-born natives, I examine the effects of assimilation and socialization by focusing on immigrants' length of residence and their generational status. Immigrants may maintain their more liberal political views, or they may become more like US-born natives the longer they live in the US. Therefore, tracking the development of the political preferences of first-generation immigrants allows us to analyze the effects of assimilation in American society. Generational status is also useful when considering the effects of socialization. Second-generation immigrants grow up in the US just like natives, but, at the same time, they are influenced by their non-native parents at home. If both socialization and family shape one's political preferences, then the welfare views of second-generation immigrants are expected to be somewhere in between those of first-generation immigrants and those of US-born natives on the conservative–liberal spectrum. Therefore, comparing the political views of first-, second-, and third-generation immigrants with those of US-born natives contributes to our understanding of the political socialization of immigrants.

Chapter 2

Concentrated Measurement Error in Political Opinion Surveys: Problems and Cures

2.1 Introduction

The measurement error problem has received considerable attention in political science. In particular, political scientists have long been interested in understanding and correcting for a large amount of measurement error in survey research (e.g. Achen, 1975; Ansolabehere, Rodden, and Snyder, 2008; Bartels, 1993). Measurement error in political surveys mostly occurs for reasons like ambiguity in the wording of questions and response categories, and inattentiveness on the part of respondents (Achen, 1975; Mosteller, 1968). Researchers have therefore aimed to recover individuals' true attitudes from the erroneous survey answers. For example, a recent study by Ansolabehere, Rodden, and Snyder (2008) uses a scaling method to aggregate multiple survey items in order to refine responses and thus obtain true latent issue preferences. Several studies employing the harshly criticized linear structural relations (LISREL) approach also strive to identify true latent variables by integrating measurement error in the structural models (e.g. Judd and Milburn, 1980; for critics of this approach, see Ansolabehere, Rodden, and Snyder (2008) and Converse (1980)).

While various consequences of and solutions for measurement error have been extensively studied, the existing literature has exclusively relied on the classical errors-in-variables (CEV) assumption that measurement error is completely random and orthogonal to the true predictors. This assumption is, however, violated in some survey items, where a middle choice is provided in response categories. Many items in political surveys involve odd-numbered scales with a mid-point category. For example, respondents are asked to choose one among seven different options that lie on the continuum of a liberal-moderate-conservative ideological scale. Specific policy attitudes such as preferences on government health insurance and abortion are also measured with five-, seven-, or nine-point scaled items with a mid-point provided. In this set of odd-number scaled questions, the problem is that some respondents are likely to choose around the middle for reasons other than their true moderate attitudes (O'Muircheartaigh et al., 2000; Payne, 1951; Presser and Schuman, 1980).

While these middle choices are qualitatively different from the actual moderate attitudes, they are also not substitutes for nonattitudes, as the inclusion of "Don't Know" or "No Opinion" does not eliminate this "middling tendency" (Kalton et al., 1978). By using several survey experiments, researchers have found that the difference is very small between the proportion of people choosing the "Don't Know" category for questions offering a mid-point and of those not offering one (O'Muircheartaigh et al., 2000; Presser and Schuman, 1980). Rather, many respondents who choose the middle have other true attitudes at the non-middle categories (e.g. Aldrich et. al., 1982; Presser and Schuman, 1980).

Previous literature has addressed this issue in the context of measurement error and considered it a threat to survey quality (e.g. Converse and Presser, 1986; Saris and Galhofer, 2007). However, the researchers have not recognized that this middling tendency constitutes a non-classical measurement error. This type of measurement error is necessarily negatively correlated with true attitude; for example, in the (typically seven-point scaled) response categories of liberal-moderate-conservative ideological disposition, liberal respondents choosing "moderate" introduce positive measurement error and conservative respondents choosing "moderate" introduce negative measurement error. This error type is also distinguishable from a typical non-classical measurement error as it is concentrated around the middle.

In this chapter, I formally analyze the effects of this unique error in variables on the bias in regression coefficient estimates. I then examine several candidates to cure the bias. Since the measurement error is concentrated around the middle, researchers may be tempted to exclude the middle and estimate regression coefficients. This paper formally shows that it is not an ideal solution. It also revisits the Wald and Bartlett estimators to see if they can improve the consistency under this "concentrated measurement error" assumption (Bartlett, 1949; Madansky, 1959; Pakes, 1982; Wald, 1940). While this study does not provide consistent point estimation, it suggests that informative bounds on parameters of interest can be constructed and that trichotomization, i.e. grouping the response categories into sets of three, reduces measurement error bias, which may be counter-intuitive at the first glance. I apply the proposed solutions to the study of ideological voting.

Before modeling the concentrated measurement error, the next section briefly discusses why some people choose the responses near the middle.

2.2 The Sources of Concentrated Measurement Error

While errors-in-variables theories in statistics and econometrics date back to Frisch (1934), the standard model of measurement error with CEV assumption in political survey research arose as a challenge to the disturbing possibility raised by Converse (1964). In the frequently cited paper, "The Nature of Belief Systems in Mass Publics," Converse argued that nearly 80% of American voters are not politically sophisticated and do not have meaningful attitudes. They simply choose almost at random among the response options. This argument has been criticized by a number of scholars who counter-argued that citizens do have underlying true attitudes that are masked by measurement error inherent in the ambiguity of question wording and categories (e.g. Achen, 1975; Erikson, 1979; Feldman, 1989).

It is now widely accepted by most scholars that survey items are plagued with measurement error originating from vague or confusing language in the survey instruments. However, it is naive just to blame surveys for their randomness. Rather, it is more realistic to incorporate the level of political sophistication into the model of erroneous survey responses. This is the approach taken by Ansolabehere, Rodden, and Snyder (2008), where they stated "...(while measurement error is large in general) measurement error is especially pronounced among the least educated respondents." Powell (1989) also argued that survey responses contain two types of errors, one attributable to the political sophistication of respondents and the other to the ambiguity of survey instruments.

A large body of works has been devoted to identifying why some respondents choose the middle for reasons other than true moderate attitudes. Two important points need to be mentioned. First, many factors affecting the middling tendency coincide with those identified by the standard measurement error models mentioned above. Thus, the middling tendency is attributed to vague questions, confusing categories, ability of respondents to understand the questions, etc. Second, there is little debate over the two competing views of randomness—respondents to blame vs. surveys to blame. Rather, we can easily find an integrated approach, or at least an approach that does not exclude the respondent side of randomness.

Psychologists use this integrated approach most explicitly by employing the idea of cognitive difficulties (Krosnick, 1991; Kulas and Stochowski, 2009). They posit that cognitive demands are substantially entailed in the survey process, from reading a questionnaire to choosing a response (Tourangeau and Rasinski, 1988). Unclear items or response categories increase cognitive demands on the respondents to provide an optimal true answer, so they tend to cope with the difficult task by simply choosing the middle when it is offered (e.g. Kulas and Stochowski, 2009). A respondent's ability level also contributes to the difficulty of the cognitive process; put simply, vague questions are harder to interpret for respondents who have less information. It is therefore argued that respondents who have a well-formed knowledge of, who are more involved with, or who pay more attention to a specific question have a relatively easier cognitive process and thus deliver their true attitudes (e.g. Bassili and Krosnick, 2000; Bishop, 1990). In contrast, respondents with low levels of ability, motivation, or attention have to go through a harder cognitive process, resulting in more choices gravitating to the middle. More generally, the level of uncertainty has also been suggested as a factor affecting the middling tendency (Alvarez and Franklin, 1996; 1994; Bassili and Krosnick, 2000).[1]

The increase of affinity for choosing near the middle is also attributed to a respondent's reluctance to reveal a true answer (Tourangeau et al., 1997). Respondents may not want to reveal their true answers when interviewers ask personal questions (about illness, for example)

[1]Uncertainty seems to be more general and integrative since it arises from both vague questions and individual lack of information.

or socially sensitive questions, for instance about racial issues. Web survey mode yields a higher probability of choosing the middle than face-to-face mode, as it is harder to monitor the respondent's attention (Heerwegh and Loosveldt, 2008).

2.3 The Concentrated Measurement Error Problem in Theory

In this section, I formally model the concentrated measurement error caused by the middling tendency. I then further consider the impacts of such error on regression coefficient estimates.

2.3.1 Modeling Concentrated Measurement Error

Suppose x_i^* represents the true attitude of individual i. This attitude is not observed but a noisy x_i measured with error u_i is available. Following the standard errors-in-variables (EIV) model,

$$x_i = x_i^* + u_i \tag{2.1}$$

The measurement error term u_i is not orthogonal to x_i^* if some respondents choose the middle for reasons other than true moderate attitudes. Moreover, it has been suggested that those respondents have other true non-middle attitudes (e.g. Aldrich et al., 1982; Presser and Schuman, 1980). u_i can then be modeled as follows:

$$u_i = -(x_i^* - x_c)v_i + r_i \tag{2.2}$$

and

$$v_i = \begin{cases} 1 & \text{if } w_i \geqq \alpha_{1-p} \\ \frac{w_i - \alpha_q}{\alpha_{1-p} - \alpha_q} & \text{if } \alpha_q < w_i < \alpha_{1-p} \\ 0 & \text{if } w_i \leqq \alpha_q \end{cases} \tag{2.3}$$

where, x_c denotes a concentration point, i.e. the middle response category in this case, and r_i is a random error. Determining how errors are concentrated, v_i has a censored distribution with underlying error w_i representing the middling propensity. A higher w_i is assumed to have a higher propensity of middling. w_i can be any combination of several factors mentioned in the previous section, such as the level of uncertainty, political sophistication, item clarity, and social desirability. α_{1-p} and α_q denote the inverse cumulative distribution function of w_i evaluated at $1-p$ and q, respectively. Therefore, equation (3) indicates that p proportion of people choose the middle and q proportion of people do not have this tendency. Alternatively, p can be interpreted as a respondent's probability of choosing the middle, and q as a respondent's probability of not choosing the middle.

If w_i is greater than α_{1-p}, the individual i strongly tends to choose the middle as $x_i = x_c + r_i$. On the other hand, if w_i is smaller than α_q then $x_i = x_i^* + r_i$, indicating that the individual i does not gravitate towards choosing the middle at all. Along the distribution of w_i, the model also contains the possibility of choosing "responses near the middle" with probability of $1-p-q$. This model set-up is consistent with the observations of previous literature (e.g. Alvarez and Franklin, 1996; 1994; Payne, 1951), and it keeps the continuum of the middling propensity. Nevertheless, it is entirely possible to exclude the "near the middle" possibility if $p+q=1$. Equations (2) and (3) indicate that the middling tendency occurs even when x_i^* is at the very extremes. One may think that only people whose true attitudes are located near the middle tend to choose the exact middle when the level of clarity is low. It is possible to model in this way by setting the support of v_i in terms of x_i^*. However, previous literature consistently provides evidence that people who are attracted by the middle category seem to give responses not only around the middle but also in more extreme categories when the mid-point is intentionally not offered in experiments (e.g. O'Muircheartaigh et al., 2000; Presser and Schuman, 1980).

Without loss of generality, let us assume that x_i^* has been standardized so that it has mean 0 and variance 1. Let us further assume that r_i and w_i also have mean 0 with $Var(r) \equiv \sigma_r^2 < \infty$ and $Var(w) \equiv \sigma_w^2 < \infty$. The variances of r_i and w_i can be interpreted as signal-to-noise ratios. I will set $x_c = 0$ in order to reflect the middling tendency.[2] Conventional independence is also assumed to hold among x_i^*, r_i, and w_i. Finally, note that equations (1), (2), and (3) yield the non-classical measurement error: $Cov(x^*, u) = -E(v) \equiv -\mu_v \neq 0$. Since $\mu_v \in (0,1)$, $Cov(x^*, u) < 0$.

[2]It is possible to model different types of concentration by changing the value of x_c. For example, in a society where extreme leftist orientation is desirable, x_c can be set equal to x_L where the support of x_i^* is $[x_L, x_R]$.

2.3.2 The Effects of Concentrated Measurement Error on Regression Coefficients

Consider

$$y_i = \alpha + \beta x_i^* + \varepsilon_i \tag{2.4}$$

where $\varepsilon_i \sim iid(0, \sigma_\varepsilon^2)$. Estimating β by regressing y_i on x_i that is measured with concentrated error gives

$$plim\hat{\beta}_L = \frac{1-\mu_v}{(1-\mu_v)^2 + \sigma_v^2 + \sigma_r^2}\beta \tag{2.5}$$

where $\mu_v \equiv E(v)$ and $\sigma_v^2 \equiv Var(v) < \infty$. Thus, the regression coefficient is biased but in an unknown direction. Downward bias should be more common, however. Note that, if the following assumption holds in equation (5), $\hat{\beta}_L$ is biased toward 0.

$$\mu_v^2 - \mu_v + \sigma_v^2 + \sigma_r^2 > 0 \tag{2.6}$$

Assumption (6) is equivalent to $Var(x) > Cov(x, x^*)$, which has been assumed in a number of previous studies on the non-classical measurement error (e.g. Black, et al., 2000; Bollinger, 1996; Imai and Yamamoto, 2008). As an illustrative example, consider a simplified model where v_i is a 0-1 random variable, ignoring the possibility of "choosing near the middle." v_i equals 1 with probability p and equals 0 with probability $1-p$. Then, $Var(x) = (1-\mu_v)^2 + \sigma_v^2 + \sigma_r^2 = 1-p+\sigma_r^2$ and $Cov(x^*, x) = 1-\mu_v = 1-p$, indicating that $Var(x) > Cov(x, x^*)$. More generally, assumption (6) holds if $\sigma_r^2 \geq \frac{1}{4}$. Since it is widely accepted that survey responses are plagued with a large amount of measurement error, assumption (6) almost surely holds and thus $\hat{\beta}_L$ is usually attenuated toward 0.[3]

[3] While this study focuses on the regression coefficients, it is easy to see that this concentrated measurement error biases correlations among true attitudes toward 0. If there are two survey items dealing with two different issues, and if we assume that the observed responses on the two items are plagued with concentrated measurement error, then the absolute value of correlation estimate between x_1 and x_2 in lieu of x_1^* and x_2^* is $|\rho| = \left| \frac{(1-\mu_{v_1})(1-\mu_{v_2})+\sigma_{v_1 v_2}}{\sqrt{(1-\mu_{v_1})^2+\sigma_{v_1}^2+\sigma_{r_2}^2}\sqrt{(1-\mu_{v_2})^2+\sigma_{v_2}^2+\sigma_{r_2}^2}}\rho^* \right| < |\rho^*|$, where $\sigma_{v_1 v_2} \equiv Cov(v_1, v_2)$ and ρ^* is the correlation between the two true attitudes. The Cauchy–Schwarz inequality is sufficient to prove the inequality of the equation above. Therefore, the familiar attenuation bias occurs in the correlation coefficient under the concentrated measurement error assumption.

Equation (5) clearly indicates that the degree of attenuation depends on the behavior of v_i. This is not a surprise since the middling propensity w_i underlies v_i, and thus μ_v depicts how much the observed response is plagued with concentrated measurement error. Then, how does the bias in $\hat{\beta}_L$ vary by the parameters in v_i, namely p and q?

For the sake of simplicity, let us assume asymptotic normality of x_i^*, r_i, and w_i.[4] Then, by the law of iterated expectation,

$$\begin{aligned}\mu_v &\equiv E(v) = E[E(v|v = v_i)]\\ &= p - \frac{\alpha_q^s(1-p-q) + [\phi(\alpha_{1-p}^s) - \phi(\alpha_q^s)]}{\alpha_{1-p}^s - \alpha_q^s}\end{aligned} \tag{2.7}$$

where $\phi(\cdot)$ denotes the standard normal density function and α_{1-p}^s and α_q^s denote the inverse cumulative standard normal distribution of w_i evaluated at $1-p$ and q, respectively. The variance of v_i can be computed as follows. By the law of total variance,

$$\begin{aligned}\sigma_v^2 &\equiv Var(v) = E[Var(v|v = v_i)] + Var[E(v|v = v_i)]\\ &= q\mu_v^2 + p(1-\mu_v)^2 + (1-p-q)[(\Pi - \mu_v)^2 + \Xi]\end{aligned} \tag{2.8}$$

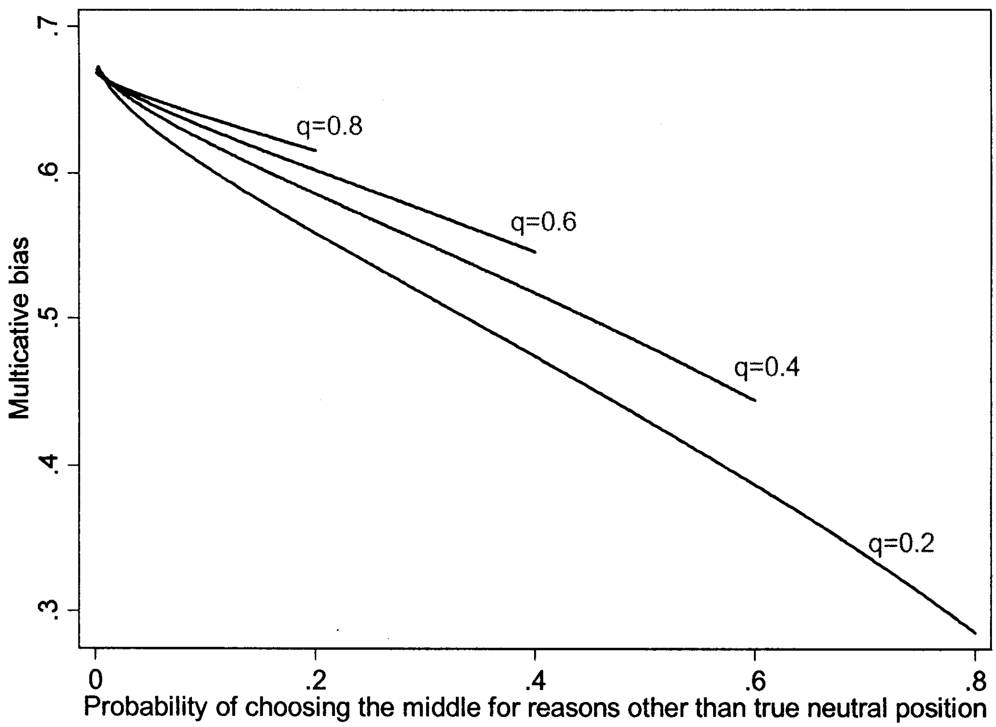
where,

$$\begin{aligned}\Pi &\equiv E\left(\frac{w - \alpha_q}{\alpha_{1-p} - \alpha_q} | \alpha_q < w_i < \alpha_{1-p}\right)\\ &= -\frac{1}{\alpha_{1-p}^s - \alpha_q^s}\left[\alpha_q^s + \frac{\phi(\alpha_{1-p}^s) - \phi(\alpha_q^s)}{1-p-q}\right]\end{aligned}$$

$$\begin{aligned}\text{and,}\quad \Xi &\equiv Var\left(\frac{w - \alpha_q}{\alpha_{1-p} - \alpha_q} | \alpha_q < w_i < \alpha_{1-p}\right)\\ &= \frac{1}{(\alpha_{1-p}^s - \alpha_q^s)^2}\left[1 - \frac{\alpha_{1-p}^s\phi(\alpha_{1-p}^s) - \alpha_q^s\phi(\alpha_q^s)}{1-p-q} - \left\{\frac{\phi(\alpha_{1-p}^s) - \phi(\alpha_q^s)}{1-p-q}\right\}^2\right]\end{aligned}$$

[4] Likert-scaled variables are not continuous, but it is a common practice to assume that such variables behave like normal variables.

Figure 1. Bias from Concentrated Measurement Error as a function of p



Note: Graphical representation of equation (5) as a function of p and q. σ_r^2 is set to 0.5.

Finally, equation (5) can be expressed in terms of p and q using equations (7) and (8). Figure 1 plots the multiplicative bias term, $plim\frac{\hat{\beta}_L}{\beta}$, as a function of p, a respondent's probability of (or proportion of respondents) choosing the middle for reasons other than true neutral position. Four different values of q are considered and plotted in the panel. σ_r^2 is set to 0.5 so that assumption (6) holds. Figure 1 shows that the bias from concentrated measurement error gets worse as p increases and/or q decreases. This is consistent with our intuition, as a larger p and smaller q means a higher level of concentrated measurement error.

2.4 Candidates for Cure

2.4.1 End-Group Estimator

Since the measurement error is concentrated around the middle, researchers may be tempted to estimate β by first excluding the middle and then regressing y_i on x_i.[5] This strategy is akin to the recommendation of some survey experts to provide even-number categories in order to suffer less from the middling tendency (e.g. Converse and Presser, 1986; Moser and Kalton, 1972; Saris and Galhofer, 2007).

A similar idea can be found in Wald-type estimators. Under the classical errors-in-variables assumption, Wald (1940) proposed a method of estimating β by first ordering the observed pairs (x_i, y_i), dividing them into two groups by the median-split, and then fitting a line through the means of the two groups. Later, Bartlett (1949) argued that the estimation is improved by dividing the data into three groups of same size, eliminating the middle group, and then fitting a line through the group means of the remaining data. While Wald-type estimators have been criticized under the classical errors-in-variables assumption in terms of large sample properties (Pakes, 1982), one may conjecture that Bartlett estimators could reduce the bias from the concentrated measurement error.[6] At first glance, these strategies look straightforward and intuitive, but they are not ideal solutions, even in the presence of measurement error concentrated around the middle, as shown below.

Let S_M be the set of x_i such that $S_M = \{x_i | \kappa_L < x_i < \kappa_H\}$, where κ_L and κ_H are finite real values along the distribution of x_i. If $\kappa_L = -\kappa_H$, then S_M contains the responses around the middle with equidistance to the left and to the right. Similarly, define S_H and S_L be the sets of x_i such that $S_H = \{x_i | x_i > \kappa_H\}$ and $S_L = \{x_i | x_i < \kappa_L\}$, respectively. For any given variable g_i, let $\tilde{g}_i = g_i - \bar{g}$, i.e. $\tilde{g}_i$ denotes a demeaned variable. The regression coefficient can then be estimated after excluding the middle by

[5] Without the notion of measurement error problem, a number of psychological studies have employed this strategy of excluding the middle in the framework of correlation and ANOVA. The practices and consequences have been reviewed by Preacher et al. (2005).

[6] Note that Bartlett's argument is mostly about the improvement of efficiency. However, it is still worthwhile to reconsider his idea of excluding the middle group and see whether it reduces the bias from concentrated measurement error.

$$\hat{\beta}_E = \frac{\sum \tilde{x}_i \tilde{y}_i \mathbb{1}\{x_i \notin S_M\}}{\sum \tilde{x}_i^2 \mathbb{1}\{x_i \notin S_M\}} \tag{2.9}$$

where $\mathbb{1}\{\cdot\}$ denotes the indicator function that equals 1 if the condition in the brace is met and 0 if it is not.

A Wald-type estimator using different thresholds can be generally expressed as:

$$\hat{\beta}_W = \frac{[prob(i \in S_H)]^{-1} \sum y_i \mathbb{1}\{x_i \in S_H\} - [prob(i \in S_L)]^{-1} \sum y_i \mathbb{1}\{x_i \in S_L\}}{[prob(i \in S_H)]^{-1} \sum x_i \mathbb{1}\{x_i \in S_H\} - [prob(i \in S_L)]^{-1} \sum x_i \mathbb{1}\{x_i \in S_L\}} \tag{2.10}$$

or equivalently,

$$\hat{\beta}_W = \frac{\sum \tilde{z}_i \tilde{y}_i}{\sum \tilde{z}_i \tilde{x}_i}, \text{ where } z_i = \begin{cases} 1 \text{ if } x_i > \kappa_H \\ -1 \text{ if } x_i < \kappa_L \end{cases} \tag{2.11}$$

Unfortunately, the asymptotic biases in $\hat{\beta}_E$ and $\hat{\beta}_W$ are exactly the same as the asymptotic bias of $\hat{\beta}_L$. Assuming that p and q are given,

$$plim \frac{\hat{\beta}_E}{\beta} = plim \frac{\hat{\beta}_W}{\beta} = \frac{1 - \mu_v}{(1 - \mu_v)^2 + \sigma_v^2 + \sigma_r^2} = plim \frac{\hat{\beta}_L}{\beta} \tag{2.12}$$

Technical proof is given in Appendix A. Therefore, even in the presence of measurement error being concentrated around the middle, the idea of excluding the middle does not provide a better solution in a regression framework. This result holds regardless of the values of κ_L and κ_H chosen.[7]

Note that equation (12) relies on the limiting distribution of the estimators. However, Madansky (1959) speculates that $\hat{\beta}_w$ may be an "adequate" estimator for small samples. For simplicity, this study focuses only on large samples but is open to the possibility that the "end-group strategy" could improve the finite sample properties.

[7]Note that excluding the middle increases the correlation estimates, but it is not clear whether it is due to the reduction in measurement error. Excluding the middle mechanically increases the correlation estimates even when there is no measurement error. Formally, if there are two perfectly measured variables x_1^* and x_2^* with mean 0 and standard deviation 1, the correlation after excluding the middle of x_1^* is $\hat{\rho}_E = \sqrt{\frac{\rho^{*2} V_1}{\rho^{*2} V_1 + 1 - \rho^{*2}}} \geq \rho^*$, where V_1 denotes the variance of x_1^* after selection. $\hat{\rho}_E$ is always greater than ρ^* because $V_1 \geq 1$.

2.4.2 Bounds on the Parameter

In many situations where it is difficult, if not impossible, to obtain a consistent point estimation, researchers have relied on some information from biased estimates. These studies often present the results in the form of bounds around the true parameter (e.g. Black et al., 2000; Bollinger, 1996; Imai and Yamamoto, 2008; Klepper and Leamer, 1984). Because of the absence of the conveniently assumed orthogonal random measurement error, this paper also finds difficulties in obtaining a consistent point estimation. However, informative bounds on the parameters of interest may be obtained if some additional information is available.

Equation (5), given assumption (6), indicate that the least square estimator $\hat{\beta}_L$ is biased toward 0. From this, we also get the useful information that $\hat{\beta}_L$ becomes a lower bound on β.

Now, suppose that we have a panel survey in which items plagued with concentrated measurement error are repeatedly asked. Rewriting equations (1)-(4) with an additional subscript t representing each panel wave, we get

$$y_{it} = \alpha + \beta x_{it}^* + \varepsilon_{it} \tag{1'}$$

$$x_{it} = x_{it}^* + u_{it} \tag{2'}$$

$$u_{it} = -x_{it}^* v_{it} + r_{it} \tag{3'}$$

$$v_{it} = \begin{cases} 1 & \text{if } w_{it} \geqq \alpha_{1-p_t} \\ \frac{w_{it}-\alpha_{q_t}}{\alpha_{1-p_t}-\alpha_{q_t}} & \text{if } \alpha_{q_t} < w_{it} < \alpha_{1-p_t} \\ 0 & \text{if } w_{it} \leqq \alpha_{q_t} \end{cases} \tag{4'}$$

Even if identical questions are repeatedly asked, there must be some change between x_{i1}^* and x_{i2}^*. I simply model this by

$$x_{i,t+a}^* = \delta x_{it}^* + s_{i,t+a} \tag{2.13}$$

for $t = 1, 2, \cdots T$, where s_i denotes a random shock that satisfies

$$E(s_{t+a}|x_t^*) = 0, \quad \forall a \in \mathbb{Z}^+ \tag{2.14}$$

Under the assumptions of (13) and (14), we can use x_{i2} as an instrument for x_{i1}. However, the IV estimator is not consistent under the concentrated measurement error assumption. A straightforward calculation of the IV estimator yields

$$plim\hat{\beta}_{IV} = \frac{1 - \mu_{v_2}}{1 - \mu_{v_1} - \mu_{v_2} + \mu_{v_1}\mu_{v_2} + \sigma_{v_1 v_2}}\beta \tag{2.15}$$

where $\mu_{v_t} \equiv E(v_t)$, $\sigma^2_{v_t} \equiv Var(v_t)$, and $\sigma_{v_1 v_2} = Cov(v_1, v_2)$. Note further that $\hat{\beta}_{IV}$ is upwardly biased if the following assumption holds

$$-\mu_{v_1} + \mu_{v_1}\mu_{v_2} + \sigma_{v_1 v_2} < 0 \tag{2.16}$$

Assumption (16) is equivalent to $Cov(x_1^*, x_2) > Cov(x_1, x_2)$, stating that the observed response at $t + 1$ is correlated more with the true attitude than with the observed response at t. This assumption makes sense because intertemporal stability between true attitudes at two different time points tends to lose its strength as more noises are added. Therefore, the IV estimator can constitute an upper bound of β.

Using the IV estimator as an upper bound is not a unique idea when measurement errors are correlated with a true predictor (e.g. Black et al., 2000; Frazis and Loewenstein, 2003; Loewenstein and Spletzer, 1997). What is unique in this paper is the utilization of the panel surveys and employment of the response of identical item at one wave as an instrument for the observed response at the other wave. Moreover, the IV estimator is strictly tighter than the old Frisch bounds under the concentrated measurement error assumption. Frisch's upper bound is the reverse regression estimator, which is $\frac{\beta}{1-\mu_v} + \frac{\sigma_\varepsilon^2}{\beta(1-\mu_v)}$. This is strictly bounded further away from 0 than $\hat{\beta}_{IV}$ as $\hat{\beta}_{IV} < \frac{\beta}{1-\mu_{v_1}}$.

Finally, we can use the relationship between the parameters of w_i and the least square estimator. Figure 1 shows that the lower bound from $\hat{\beta}_L$ gets closer to 0 as p increases and/or q decreases. Although these two parameters are not observed, we may obtain some information about w_i by adding some follow-up questions. Recall that w_i represents the middling propensity that may include factors such as the level of uncertainty, item clarity, political sophistication, and social desirability. Several researchers have advocated incorporating questions to directly measure the level of uncertainty, intensity, involvements, and importance (e.g. Alvarez and

Franklin, 1996; 1994; Converse and Presser, 1986).[8] These measurements enable us to find a subset of individuals with lower propensity to middle, e.g. certain respondents as opposed to uncertain ones. This subset can be interpreted as a group with lower p and higher q.

Running a regression with a subset of individuals with lower middling propensity yields a tighter lower bound on β if the following condition is satisfied. Letting G_j denote a subset of individuals differentiated by the follow-up survey instruments, we can write

$$[\beta_{G_j} = \beta_{G_k} = \beta] \wedge [\hat{\beta}_{L,G_j} \neq \hat{\beta}_{L,G_k} \neq \hat{\beta}_L] \quad \text{for } j \neq k \tag{2.17}$$

Condition (17) specifies the homogeneity of the effects and the heterogeneity of the estimates. In other words, the true parameter β is constant across the groups, but the estimates differ due to the (heterogeneity of) measurement error. Whether or not this condition is satisfied depends on (a) how precisely the follow-up measures reveal the middling pattern of survey responses without differentiating between true attitudes and (b) how comprehensively such measurements represent w_i.

2.4.3 Trichotomization

While it is fairly common to create a binary variable from a continuous or categorical predictor, political scientists have not been analyzing the consequences of this practice. On the other hand, methodologists in biology and medical studies, other disciplines where researchers frequently dichotomize continuous variables, have elucidated the effects of this approach in various aspects (e.g. Farewell et al., 2004; Flegal et al, 1991; Gustafson and Le, 2002).

Gustafson and Le (2002) is particularly relevant to this study. They show that dichotomization of a continuous predictor reduces the EIV bias under the CEV assumption. In this section, I show that this comforting result works in the presence of concentrated measurement error but not in a substantively meaningful way. I further show that trichotomization can be an alternative strategy.

[8]The question wordings are, for instance, "How certain are you of your answer in the previous question? Very certain, pretty certain, or not very certain?", "How important is this issue to you? Extremely important, very important, somewhat important, not too important, or not important at all?", "How strongly do you feel about the issue? Extremely strongly, very strongly, somewhat strongly, or not at all strongly?" etc. See Alvarez and Franklin (1994), Bassili and Krosnick (2000), and Converse and Presser (1986).

Suppose that y_i and x_i^* are related as in the equation (4). Researchers can dichotomize x_i^* and estimate

$$y_i = \zeta + \gamma d_i^* + e_i \tag{4''}$$

where, $d_i^* = \begin{cases} 1 \text{ if } x_i^* \geqq \kappa \\ 0 \text{ if } x_i^* < \kappa \end{cases}$. However, the true attitude x_i^* is not available. Instead, researchers can only observe x_i. By dichotomizing x_i as if it were x_i^*, researchers obtain $d_i = \begin{cases} 1 \text{ if } x_i \geqq \kappa \\ 0 \text{ if } x_i < \kappa \end{cases}$. γ in equation (4″) is the difference-in-means (DIM), i.e. $\gamma = E(y|d^* = 1) - E(y|d^* = 0)$. Running a regression of y_i on d_i in place of d_i^* yields $plim\hat{\gamma}_d = E(y|d = 1) - E(y|d = 0)$. A straightforward calculation of γ and $\hat{\gamma}_d$ gives

$$plim\hat{\gamma}_d = \frac{E(x^*|x \geqq \kappa) - E(x^*|x < \kappa)}{E(x^*|x^* \geqq \kappa) - E(x^*|x^* < \kappa)}\gamma \tag{2.18}$$

Under the CEV assumption where $x_i = x_i^* + u_i$, $x_i^* \sim N(0,1)$, $u_i \sim N(0, \sigma_u^2)$, and $E(u|x^*) = 0$, Gustafson and Le show that

$$plim\frac{\hat{\gamma}_d}{\gamma} = \xi\frac{\Lambda^*(\kappa)}{\Lambda^*(\xi\kappa)} < \xi^2 = plim\frac{\hat{\beta}_L}{\beta} \tag{2.19}$$

where $\xi = \left(\sqrt{1+\sigma_u^2}\right)^{-1}$ and

$$\Lambda^*(c) = \frac{\Phi(c)[1 - \Phi(c)]}{\phi(c)} \tag{2.20}$$

with $\phi(\cdot)$ and $\Phi(\cdot)$ being the standard normal density function and cumulative distribution function, respectively. $\Lambda^*(c)$ is symmetric and decreasing in $|c|$ as $\Lambda^*(c) = \Phi(c)MR(c)$, where $MR(c)$ is the Mill's ratio. The multiplicative bias of $\hat{\gamma}_d$ is also symmetric and decreasing in $|\kappa|$ with the range of $[\xi, \xi^2)$ as $\Lambda^*(0) = 1$ and $\lim_{|\kappa|\to\infty} \xi\frac{\Lambda^*(\kappa)}{\Lambda^*(\xi\kappa)} = \xi^2$. Therefore, dichotomization reduces the multiplicative bias under the CEV assumption even if an infinitely large κ is chosen.

This result is not always applicable to the concentrated measurement error problem. There are two key differences between CEV and the concentrated measurement error assumptions. First, CEV's convenient independence assumption between x_i^* and u_i gives $Var(x) > Var(x^*)$. However, $Var(x) \gtreqless Var(x^*)$ under the concentrated measurement error assumption. Due to the concentration around the middle, the variance of the observed variable can be smaller than that of the true variable. Second, x_i follows the distribution of x_i^* and u_i under the CEV assumption, and thus $x_i^* \sim N(0,1)$ and $u_i \sim N(0,\sigma_u^2)$ give $x_i \sim N(0,1+\sigma_u^2)$. Under the concentrated measurement error assumption, x_i may not be normally distributed even if the normalities of x_i^*, w_i, and r_i are assumed. Rather, it may have a leptokurtic distribution because of the concentration of the measurement error around the middle. The effect of concentration on the variance and kurtosis of x_i is reported in Table 1, where the simulated values of x_i are obtained by the equations (1)-(3), $n = 10000$, $x_i^* \sim N(0,1)$, $w_i \sim N(0,1)$, and $r_i \sim N(0,\sigma_r^2)$. Various levels of p, q, and σ_r^2 are considered. As one can see, $Var(x) \gtreqless Var(x^*)$ and the excess kurtosis of x_i increases in p and decreases in q.

The second point mentioned above makes it complicated to derive a functional form of $\hat{\gamma}_d$ from equation (18). In this paper, I take a general approach by assuming that x_i follows a Pearson type-VII distribution. This is a family of distributions whose densities are symmetric in the Pearson system. It subsumes various commonly used bell-shaped distributions such as normal, Cauchy, and t-distribution. Moreover, it is possible to set an arbitrary variance and kurtosis by changing the scale and shape parameters.

If x_i has a Pearson type-VII distribution, its density function is

$$p_{VII}(x) = \frac{\Gamma(Q-1/2)}{\sqrt{m\pi}\Gamma(Q-1)}\left[1+\frac{x^2}{m}\right]^{-(Q-1/2)} \tag{2.21}$$

where $\Gamma(\cdot)$ denotes a gamma function. m and Q are the scale and shape parameters, respectively, being related to the variance and kurtosis of x_i by

$$\begin{aligned}\sqrt{m} &= \sigma_x\sqrt{2(Q-2)} \\ Q &= 3+\frac{3}{\theta}\end{aligned} \tag{2.22}$$

where, σ_x is the standard deviation of x_i and θ denotes the excess kurtosis. Note that if $\theta \to 0$, then $Q \to \infty$ and $p_{VII}(x) \to \phi(x)$.

The associated cumulative distribution function is

$$P_{VII}(x) = \begin{cases} \frac{1}{2} I\left(\frac{m}{m+x^2}; Q-1, \frac{1}{2}\right) & \text{if } x < 0 \\ 1 - \frac{1}{2} I\left(\frac{m}{m+x^2}; Q-1, \frac{1}{2}\right) & \text{if } x \geqq 0 \end{cases} \tag{2.23}$$

where $I(x; a, b)$ denotes the regularized incomplete beta function defined by

$$I(x; a, b) = \frac{1}{B(a,b)} \int_0^x w^{a-1}(1-w)^{b-1} dw \tag{2.24}$$

Finally, under the concentrated measurement assumptions and $x_i^* \sim N(0,1)$, the multiplicative bias of $\hat{\gamma}_d$ becomes

$$plim \frac{\hat{\gamma}_d}{\gamma} = \frac{1-\mu_v}{(1-\mu_v)^2 + \sigma_v^2 + \sigma_\tau^2} \frac{\Lambda^*(\kappa)}{\Lambda(\kappa; Q, m)} \tag{2.25}$$

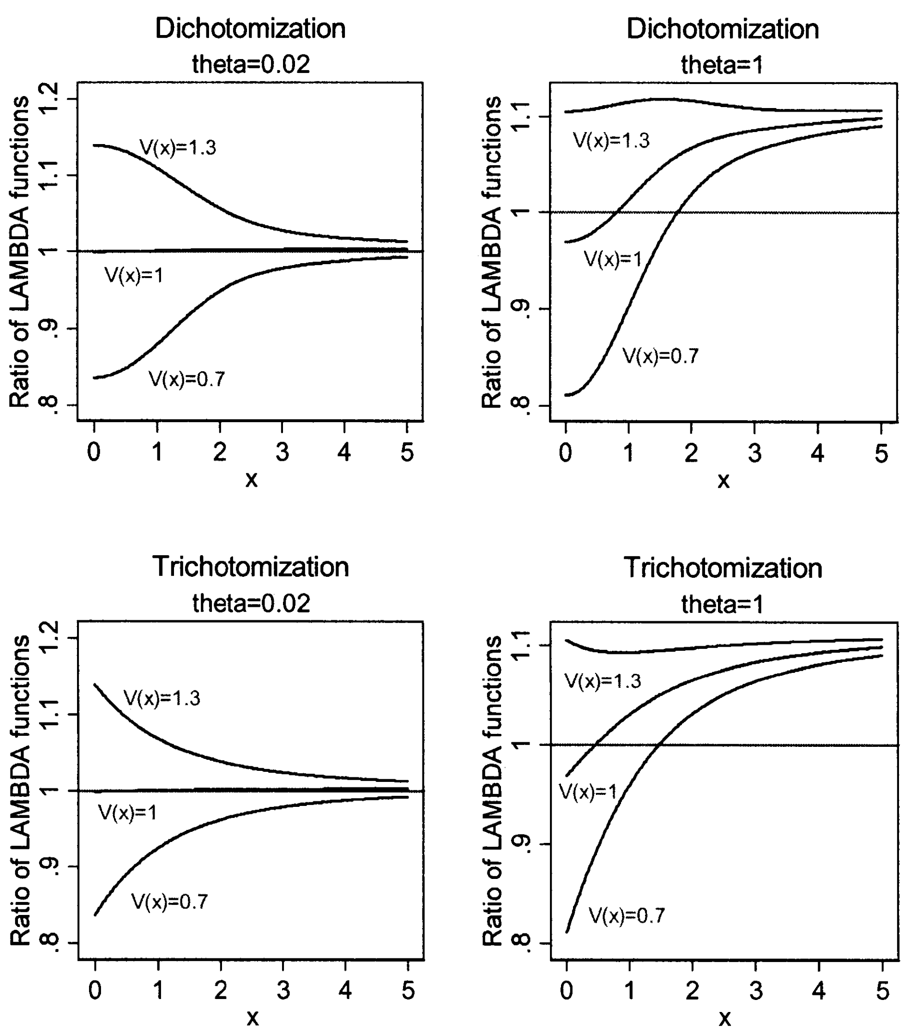
where

$$\Lambda(\kappa; Q, m) = \frac{\frac{1}{2} I\left(\frac{m}{m+\kappa^2}; Q-1, \frac{1}{2}\right) \left[1 - \frac{1}{2} I\left(\frac{m}{m+\kappa^2}; Q-1, \frac{1}{2}\right)\right]}{\left(\frac{m+\kappa^2}{2Q-3}\right) p_{VII}(\kappa; Q, m)} \tag{2.26}$$

and $\Lambda^*(\kappa)$ is defined above in equation (20). Proof of equations (25) and (26) is given in Appendix B. Equation (25) indicates that the level of attenuation depends on the ratio of two defined Λ functions of true and observed response, i.e. $\frac{\Lambda^*(\kappa)}{\Lambda(\kappa; Q, m)}$. If the ratio is greater than 1, the downward bias of $\hat{\gamma}_d$ is smaller than that of $\hat{\beta}_L$. Conversely, $\hat{\gamma}_d$ becomes more attenuated than $\hat{\beta}_L$ if the ratio is smaller than 1.

The two upper panels of Figure 2 plot the ratios of two Λ functions as a function x. Six scenarios are considered: $\{Var(x) > Var(x^*), Var(x) = Var(x^*), Var(x) < Var(x^*)\} \times \{$Low Excess Kurtosis ($\theta = 0.02$), High Excess Kurtosis ($\theta = 1$)$\}$. The ratio of Λ functions with $Var(x) = 1.3$ in the upper-left panel may represent the CEV case since $Var(x) > Var(x^*)$ and kurtosis stays the same. In this scenario, the ratio of Λ functions is always higher than 1, regardless of the cut-off point, which is the finding of Gustafson and Le (2002). However, concentrated measurement error leads to higher kurtoses and various variance inequalities as reported in Table 1. The upper-right panel shows that if x_i has a leptokurtic distribution and $Var(x)$ is not larger than $Var(x^*)$, then *choosing a cut-off point around the concentration point* may yield a worse attenuation bias than $\hat{\beta}_L$. $\hat{\gamma}_d$ may still be less biased than $\hat{\beta}_L$ if a researcher chooses a threshold that is far from the middle. However, it may not be substantively meaningful in political opinion researches. When it comes to dichotomization, we are mostly interested in the difference in means between two groups that are divided by median, e.g. liberals vs. conservatives.

Figure 2. Ratio of Lambda Functions for Dichotomization and Trichotomization



As an alternative strategy, I suggest a trichotomization, i.e. creating two binary variables from the observed response x_i. In political opinion researches, it is sometimes meaningful to compare the differences in means among three groups, with one group being the base. When, for instance, a seven-point scaled ideological preference item is available, one can set three groups of liberal, moderate, and conservative people with liberal respondents being the reference group. Of course, this strategy corresponds to some loss of variation just like dichotomization strategy, but it "dilutes" the effect of concentrated measurement error and therefore improves the estimator performance as will be shown below. In other words, there is a trade-off between losing variation and improving estimation.

As before, y_i and x_i^* are related as in equation (4). Without loss of generality, let us assume that trichotomization involves two cut-offs that have equidistance from the concentration point. The regression model is then,

$$y_i = \eta + \gamma_1 d_{1i}^* + \gamma_2 d_{2i}^* + \epsilon_i \qquad (4''')$$

where, $d_{1i}^* = \begin{cases} 1 \text{ if } -\kappa \leqq x_i^* \leqq \kappa \\ 0 \text{ otherwise} \end{cases}$ and $d_{2i}^* = \begin{cases} 1 \text{ if } x_i^* > \kappa \\ 0 \text{ otherwise} \end{cases}$ for $\kappa > 0$. The respondents with $x_i^* < -\kappa$ become the reference category. As x_i is observed instead of x_i^*, researchers create $d_{1i} = \begin{cases} 1 \text{ if } -\kappa \leqq x_i \leqq \kappa \\ 0 \text{ otherwise} \end{cases}$ and $d_{2i} = \begin{cases} 1 \text{ if } x_i > \kappa \\ 0 \text{ otherwise} \end{cases}$. Define

$$\gamma_1 = E(y| -\kappa \leqq x^* \leqq \kappa) - E(y|x^* < -\kappa)$$
$$\gamma_2 = E(y|x^* > \kappa) - E(y|x^* < -\kappa)$$

Since estimating γ_1 and γ_2 with d instead of d^* gives

$$plim\hat{\gamma}_1 = E(y| -\kappa \leqq x \leqq \kappa) - E(y|x < -\kappa)$$
$$plim\hat{\gamma}_2 = E(y|x > \kappa) - E(y|x < -\kappa)$$

it is straightforward to show that

$$plim\hat{\gamma}_{T1} = \frac{E(x^*| -\kappa \leqq x \leqq \kappa) - E(x^*|x < -\kappa)}{E(x^*| -\kappa \leqq x^* \leqq \kappa) - E(x^*|x^* < -\kappa)}\gamma_1 \tag{2.27}$$

$$plim\hat{\gamma}_{T2} = \frac{E(x^*|x > \kappa) - E(x^*|x < -\kappa)}{E(x^*|x^* > \kappa) - E(x^*|x^* < -\kappa)}\gamma_2 \tag{2.28}$$

Assuming that x_i has a Pearson type-VII distribution described in the equations (21)-(24) and $x_i^* \sim N(0,1)$, the multiplicative bias of $\hat{\gamma}_{T1}$ and $\hat{\gamma}_{T2}$ can be obtained by

$$plim\frac{\hat{\gamma}_{T1}}{\gamma_1} = plim\frac{\hat{\gamma}_{T2}}{\gamma_2} = \frac{1-\mu_v}{(1-\mu_v)^2 + \sigma_v^2 + \sigma_r^2}\frac{\Lambda_T^*(\kappa)}{\Lambda_T(\kappa; Q, m)} \tag{2.29}$$

where

$$\Lambda_T^*(\kappa) = \frac{1-\Phi(\kappa)}{\phi(\kappa)} \tag{2.30}$$

$$\Lambda_T(\kappa; Q, m) = \frac{\frac{1}{2}I\left(\frac{m}{m+\kappa^2}; Q-1, \frac{1}{2}\right)}{\left(\frac{m+\kappa^2}{2Q-3}\right)p_{VII}(\kappa; Q, m)} \tag{2.31}$$

in the presence of concentrated measurement error. Proof of the equations (29)-(31) is given in Appendix B. Therefore, the multiplicative biases of $\hat{\gamma}_{T1}$ and $\hat{\gamma}_{T2}$ are theoretically identical. They also depend on the ratio of two Λ functions which are defined differently from the case of dichotomization.

The two lower panels of Figure 2 display the ratios of two Λ functions, i.e. $\frac{\Lambda_T^*(\kappa)}{\Lambda_T(\kappa;Q,m)}$. When the excess kurtosis is low and $Var(x)$ is larger than $Var(x^*)$, $\hat{\gamma}_{T1}$ and $\hat{\gamma}_{T2}$ are better than $\hat{\beta}_L$ in terms of bias. Thus, trichotomization does not lose the improvement that dichotomization provides. A more important finding comes from the lower-right panel of Figure 2. Even if the excess kurtosis is high and $Var(x) < Var(x^*)$, trichotomization may reduce the bias from concentrated measurement error if a researcher chooses κ slightly offset from the middle.

To compare the dichotomization and trichotomization strategies, recall that dichotomization may induce a worse attenuation bias if a researcher chooses κ around the concentration point. It may reduce the bias if κ is chosen far from the middle, but it may cost him the substantive meaningfulness needed in political opinion research. In contrast, trichotomization leads to smaller attenuation biases if κ is placed away from the middle, and it is still politically meaningful to estimate the differences in means among three groups with one being the reference category. Although respondents with $-\kappa \leqq x \leqq \kappa$ are affected the most by the concentration, the level of attenuation in $\hat{\gamma}_{T1}$, which compares $E(y|-\kappa \leqq x \leqq \kappa)$ and $E(y|x < -\kappa)$, is still lower than $\hat{\beta}_L$ as it is identical to the level of attenuation in $\hat{\gamma}_{T2}$.

One may raise a concern that the discussion so far depends on the scenarios without directly mapping from the parameters of concentration (p and q) to the set of estimators. However, such direct mapping requires perfectly specified probability density and distribution functions of x_i. While it seems nearly impossible to meet this requirement, the six scenarios in Figure 2, following from the assumption that x_i has a Pearson type-VII distribution, provide intuitions about the differences among least squares, dichotomization, and trichotomization.

These intuitions can be supported by a brief Monte-Carlo simulation without knowing the exact distribution of x_i. The model for the simulation is as specified in the equations (1)-(4), (4″), and (4‴). I assume $x_i^* \sim N(0,1)$, $\varepsilon_i \sim N(0,1)$, $w_i \sim N(0,1)$, and $r_i \sim N(0,\sigma_r^2)$. To pursue some practical implications in political opinion surveys, values of x_i^* are rounded to create equally spaced seven-point scales ranging from -3 to 3. An example would be "(-3) strongly liberal" – "(-2) not so strongly liberal" – "(-1) lean to liberal" – "(0) moderate" – "(1) lean to conservative" – "(2) not so strongly conservative" – "(3) strongly conservative." To assess the performance of each estimator, the exact values of multiplicative bias and mean-squared error (MSE) are simulated using 100 replicates with 10000 observations. Note that the formulas for the bias and MSE are slightly different from widely used formulas in simulation studies as the bias here is not additive. Specifically, for any given parameter b, the values of

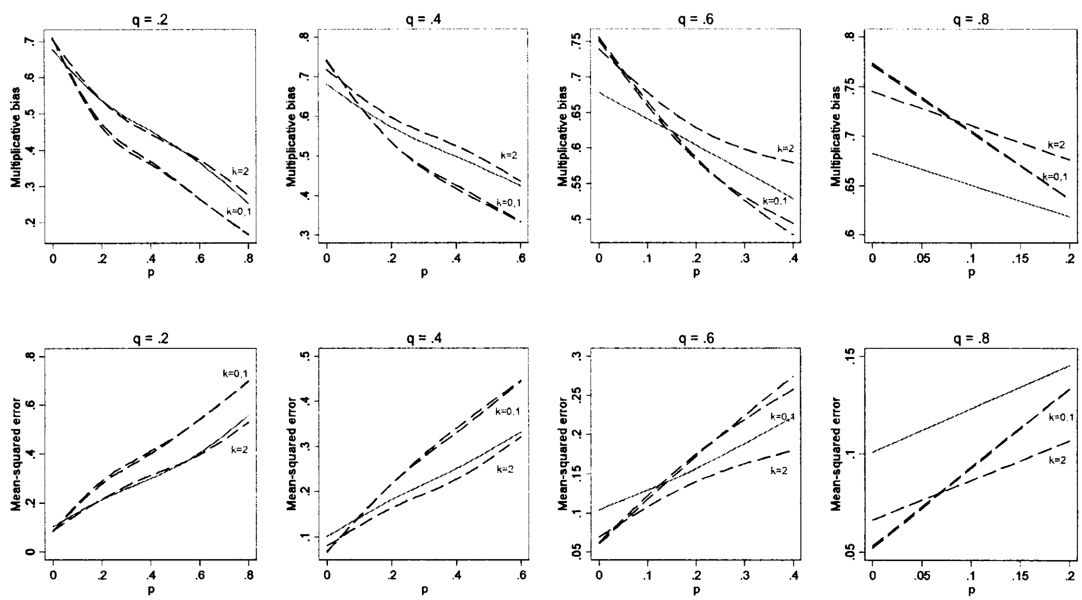
$$MB = \frac{E(\hat{b})}{b}$$
$$MSE = \left[\frac{E(\hat{b})}{b}\right]^2 + Var(\hat{b})$$

are simulated.

Figure 3 plots the bias and MSE of $\hat{\beta}_L$ and $\hat{\gamma}_d$ as a function of p for four different values of q. σ_r^2 is set to 0.5 so that $Var(x) \gtreqless Var(x^*)$. For dichotomization, three values for κ of 0, 1, and 2 are used. Recall that p is the parameter representing a respondent's probability of (or proportion of respondents) choosing the middle for reasons other than true neutral position. q

is the parameter of proportion of people who do not have this middling tendency.

Figure 3. Multiplicative Bias and Mean-Squared Error of $\hat{\beta}_L$ and $\hat{\gamma}_d$

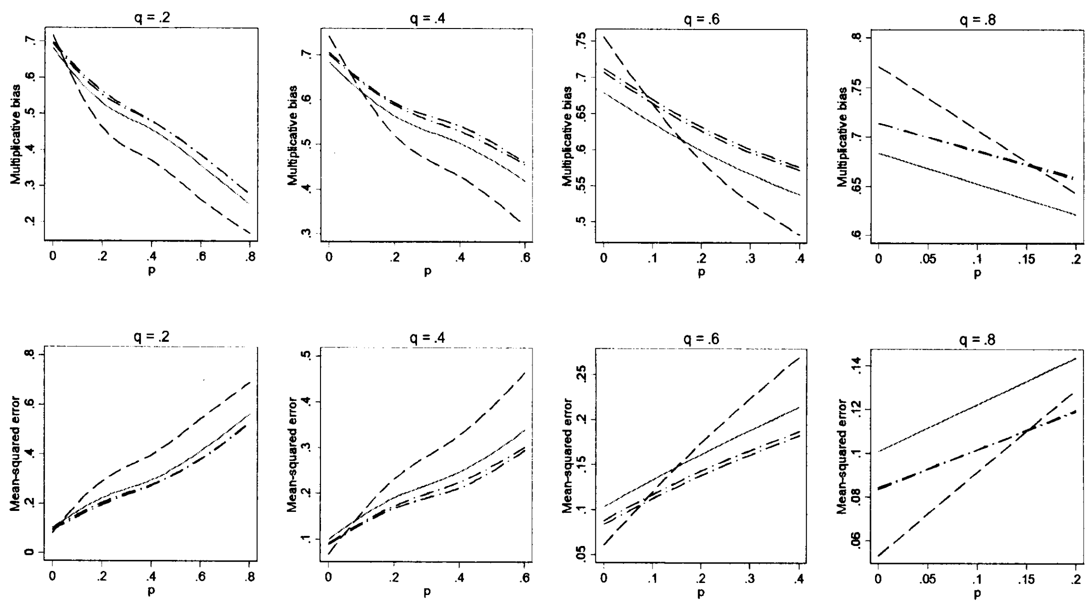


Note: Bias and MSE of $\hat{\beta}_L$ (solid curve) and $\hat{\gamma}_d$ (dashed curves). σ_r^2 is set to 0.5, $N = 10000$, and 100 replicates.

The plots in Figure 3 support the theoretical predictions based on the assumption of the Pearson type-VII distribution. When the cut-off point is chosen near the middle, the attenuation bias in $\hat{\gamma}_d$ is more severe than the bias in $\hat{\beta}_L$ unless p is sufficiently small and/or q is sufficiently large. When a threshold is away from 0, dichotomization tends to yield lesser attenuation. However, it may not be substantively meaningful in most political opinion researches. The lower panels indicate that dichotomization leads to higher MSE if $\kappa = 0, 1$ and the level of concentration is high. When $\kappa = 2$, MSE of $\hat{\gamma}_d$ tends to be smaller than that of $\hat{\beta}_L$.

I now conducted another simulation to compare three practical strategies: (S1) $\hat{\beta}_L$, (S2) $\hat{\gamma}_d$ with median split, and (S3) $\hat{\gamma}_{T1}$ and $\hat{\gamma}_{T2}$ with $\kappa = 1$. For example, in a seven-point scaled ideological preference item ranging from -3 (strongly liberal) to 3 (strongly conservative), the first strategy treats this variable as linear and continuous and estimates dy/dx. In the second strategy, a researcher splits the respondents into two groups of liberals and conservatives and estimate $E(y|x \geqq 0) - E(y|x < 0)$. The third strategy corresponds to grouping people into liberal-moderate-conservative and estimate $E(y|x > 1) - E(y|x < -1)$ and $E(y| -1 \leqq x \leqq 1) - E(y|x < -1)$.

Figure 4. Multiplicative Bias and Mean-Squared Error of $\hat{\beta}_L$, $\hat{\gamma}_d$ with $\kappa = 0$, and $\hat{\gamma}_{T1}$ and $\hat{\gamma}_{T2}$ with $\kappa = 1$.



Note: Bias and MSE of $\hat{\beta}_L$ (solid curve), $\hat{\gamma}_d$ with $\kappa = 0$ (dashed curves), and $\hat{\gamma_{T1}}$ and $\hat{\gamma_{T2}}$ with $\kappa = 1$ (dash-dotted curves). σ_r^2 is set to 0.5, $N = 10000$, and 100 replicates.

Figure 4 compares these three practical strategies. Again, the results support the theoretical predictions based on the scenarios in Figure 2. While dichotomization often results in a worse attenuation than least squares, trichotomization always yields a lesser attenuation bias. Moreover, the biases in $\hat{\gamma}_{T1}$ and $\hat{\gamma}_{T2}$ are (almost) identical, as equation (29) predicts. Even if the group of moderate respondents is most affected by the concentrated measurement error, the bias in $\hat{\gamma}_{T1}$ is still less than the bias in $\hat{\beta}_L$. The mean-squared errors in $\hat{\gamma}_{T1}$ and $\hat{\gamma}_{T2}$ are also always smaller than those in $\hat{\beta}_L$. Comparing the differences-in-means among liberal-moderate-conservative groups therefore seems to be a safe strategy for researchers worrying about the bias from concentrated measurement error. Of course, this suggestion accompanies the need to change the interpretation of the coefficients, which must be substantively meaningful.

2.5 An Empirical Illustration: Ideological Voting

To illustrate the strategies and findings empirically, I consider a study of ideological voting. Following the literature on spatial voting theories (e.g. Hinich and Enelow. 1984), voting behaviors can be modeled in an ideological issue space. Since voter i's utility for party j or candidate j is a function of the distance between i's ideology and the candidate's,

$$U_{ij} = \alpha - \beta_j ||Self_i - C_{ij}|| \tag{2.32}$$

where C_{ij} denotes the ideological position of candidate j presented to individual i. A voter can maximize the expected utility by choosing the candidate whose ideological position is the closest to his/her own. For simplicity, let us assume that there are two candidates, a Republican and a Democrat. Voters then calculate

$$U_{iR} - U_{iD} = \beta \left(||Self_i - C_{iR}|| - ||Self_i - C_{iD}|| \right) \tag{2.33}$$

where $\beta \equiv -(\beta_R - \beta_D)$.

To estimate this equation, we need a survey that asks respondents to place the ideologies of themselves and the perceived ideologies of candidates. Many surveys have such items which are usually seven- or nine-point scaled. Here I use the American National Election Studies (ANES) 2000 panel data where the ideological disposition items of the respondents and of presidential candidates Bush and Gore (dropping all minor candidates), are seven-point scaled, ranging from "1. extremely liberal" to "7. extremely conservative."

The problem is that these seven-point scaled ideological placement items are highly likely to contain the concentrated measurement error. Accordingly, the ideological distance measure, $||Self_i - C_{iR}|| - ||Self_i - C_{iD}||$, may also suffer from such error. Recall that the concentrated measurement error arises for the ambiguities of questions and response categories, the lack of respondent's ability, attentiveness, involvements, and more generally, certainty, etc. The liberal-conservative (or left-right) ideology may be too vague to define for some respondents, who may also find difficulty in distinguishing among the response categories of leftist-neutral-rightist positions. The question and response categories may be even more ambiguous to politically unsophisticated respondents than to sophisticated ones. Any or all of these difficulties may cause some respondents to choose the middle categories for reasons other than the true neutral position.[9]

For the purpose of illustrating this paper's findings in a practical setting, the ANES 2000 panel study offers two extra advantages (other than the availabilities of the seven-point scaled ideological placement items). First, it provides follow-up certainty questions. After the placement of each presidential candidate, respondents were asked to answer "How certain are you of this? Very certain, pretty certain or not very certain?" These follow-up items are useful to show that the ideological placement items are indeed plagued with concentrated measurement error as uncertainty is an important factor affecting the middling tendency (Alvarez and Franklin, 1996; 1994; Bassili and Krosnick, 2000). Moreover, one can use the information from these certainty variables to construct tighter bounds, as discussed above in the section on the bounds on the parameter. The second advantage lies in the short panel structure. The ideological placement items, along with certainty questions, were repeatedly asked before and after the presidential election. Thus, the items in the post-election wave can be used to construct an instrument for the independent variable in estimating parameters for the pre-election wave, and vice versa.

2.5.1 The Presence of Concentrated Measurement Error

To show that the ideological placement items are plagued with concentrated measurement error, I use probit and ordered probit models, predicting the placements around the middle as a function of uncertainty. As an alternative, I also employed the degree of political sophistication in explaining the middling tendency, since the lack of a respondent's ability also corresponds to the gravitated choice around the middle (Bassili and Krosnick, 2000; Krosnick, 1991). Following the previous literature, two proxies for the levels of political sophistication are used: the levels of education (Converse, 1964) and of political information using interviewer's assessment (Zaller, 1992).[10]

[9]This bias toward the middle has also been recognized by the previous literature on ideological voting theories (e.g. Westholm, 1997; MacDonald and Rabinowitz, 1993).

[10]In ANES surveys, educational attainment consists of seven ordered categories, "1. 8 grades or less and no diploma or equivalency," "2. 9-11 grades, no further schooling (incl. 12 years without diploma or equivalency)," "3. high school diploma or equivalency test," "4. more than 12 years of schooling, no higher degree," "5. junior

For the dependent variables of the probit models, I create binary variables from the seven-point scaled placements of Bush and Gore. These variables are coded as 1 for the middle categories (3, 4, 5 among 1-7 categories) and 0 for the extreme categories 1, 2, 6, 7. The concentrated measurement error in these ideological placement items may lead the ideological distance measure in the right hand side (RHS) of equation (33) to have values gravitated toward 0. From the variable constructed by $|Self_i - C_{iR}| - |Self_i - C_{iD}|$, I similarly create a binary variable coded 1 for the middle categories, which are -1,0,1 in the ideological distance measure, and 0 for other categories. In the equation with $|Self_i - C_{iR}| - |Self_i - C_{iD}|$ as the dependent variable, the uncertainty variable is constructed by simply adding the uncertainty levels for the placements of both candidates. Thus, the certainty level ranges from "0. not very certain" to "2. very certain" in the equation using each candidate placement as the dependent variable, and it ranges from "0. not very certain for any of the candidates' placements" to "4. very certain for both" in the equation using the ideological distance measure as the dependent variable.

Table 2 presents the estimated coefficients and discrete changes in probabilities from probit regression analyses using the pre- and post-election wave data in the upper and lower panel, respectively. The discrete changes, $\triangle prob(y = 1)$, are obtained by computing the differences in probabilities of choosing the responses near the middle as an independent variable changes from its minimum to maximum. For the certainty variable, for instance, the discrete change estimates the effect of moving from "not very certain" to "very certain"; for education, the effect of moving from "less than 8 years of education" to "advanced degree including LLB and MA"; for information, the effect of moving from "very low level of political knowledge" to "very high level." The first four columns report the estimates from the probit of Bush placement. The fifth to eighth columns report the results using Gore placement as the dependent variable. The last four columns display the coefficients and discrete changes in probabilities obtained by running a probit of the ideological distance measure.

The bivariate probit regression results are reported in the first three columns for each of the three dependent variables. For all three dependent variables, uncertain respondents

or community college level degrees," "6. BA level degrees; 17+ years, no advanced degree," and "7. advanced degree, including LLB." The level of political information is inferred from the interviewer's assessment on each respondent's "general level of information about politics and public affairs," ranging from "1. very high" to "5. very low."

have uniformly higher probabilities of choosing the responses near the middle than certain ones. Moreover, the coefficient estimates are all significant at the .01 level. With the two proxies for the levels of political sophistication, the probabilities of choosing the middle are also higher in the group of low education/information than in the high education/information group. However, not all coefficient estimates are statistically significant. In all bivariate probits (except one case of Bush placement in the pre-election wave), all coefficient estimates for the information levels are statistically significant. Respondents with low education tend to choose the middle in the post-election wave, but this tendency is weak in the pre-election wave. I also run probit regressions with all three right hand side (RHS) variables of uncertainty, information, and education levels together. Thus, I run a "horse-race" among the different factors affecting the middling tendency. To aid the comparison, all independent variables are standardized. The results are reported in the last column for each dependent variable. Here, only the uncertainty variable remains to be powerful in predicting the middling tendency. Though the estimated coefficients are not all significant, the general picture from Table 2 is clear enough: some respondents tend to choose the middle when they are uncertain, and therefore those mid-point choices seem not to be the true neutral placements.[11]

In addition to the probits, I run the ordered probit regressions. The dependent variables are coded as 0 for the most extreme categories ("1. extremely liberal" and "7. extremely conservative"), 1 for ("2. liberal" and "6. conservative"), 2 for the categories of ("3 and 5"), and 3 for the exact middle. I also recode the ideological distance measure, $|Self_i - C_{iR}| - |Self_i - C_{iD}|$, into a five-point scaled variable coded 4 for the exact middle, 3 for the categories of "-1 and 1," 2 for "-2 and 2," 1 for "-3 and 3," and 0 for the extreme categories. Table 3 presents the coefficient estimates analogous to those in Table 2, by running the ordered probit

[11]One might raise the concern that choosing "not very certain" could mean that the respondent is moderate. In other words, some respondents might express that they are truly moderate by saying that they are uncertain. This is possible, but only if a respondent reports his *own* ideology as moderate and chooses "not very certain" in a follow-up question. The survey this chapter employs asked respondents to choose the presidential candidates' ideology. Therefore, it is highly unlikely for respondents to use uncertainty as another expression for a truly moderate ideology. To augment this argument, I estimated equation (33) after excluding the independent variable's middle categories. Regressions using subsamples of individuals with high certainty levels gave generally less attenuated coefficient estimates than regressions using those of low certainty groups (not shown). For example, -12.41 using all samples and -13.89 using the subset of individuals with the highest certainty level were obtained from the regressions using post-election data (the coefficients' substantive interpretations are unnecessary in this footnote, but curious readers will find Section 2.5.2 useful). This difference indicates that the responses of uncertain individuals contain more measurement errors than those of certain ones even if we analyze only the non-middle responses.

regressions on the three independent variables. In Table 4, I report the discrete changes in the probabilities of choosing each category of the dependent variables as a respondent is assumed to move from the minimum to the maximum value of each independent variable, using the pre- and post-election interviews in the upper and lower panel, respectively. In each panel, unadjusted changes are obtained from the bivariate ordered probits while the "horse-racing" changes are estimated with the three RHS variables all together in one equation.

The results of ordered probit regressions also point to the high middling tendency of uncertain respondents. For all three dependent variables, uncertain respondents choose the midpoints more than certain ones. Moreover, the uniformly decreasing changes in probabilities from the first column to the last one for each dependent variable in Table 4 indicate that uncertain respondents are more likely to choose not only the exact middle position but also the positions near the middle. The same pattern is found using the two proxies of political sophistication in the unadjusted changes and coefficients estimated by running the bivariate ordered probits though the effects are less favorable in terms of statistical significance, especially with the levels of education as the independent variable. The coefficient and probability change estimates from the horse-racing also give essentially identical results with those obtained from the probit regressions. From the findings in Table 3 and Table 4, one can clearly see that some respondents have high affinities toward choosing the positions concentrated around the middle when they are uncertain, and these choices should not be treated the same as those of truly neutral respondents.

Given some evidence of concentrated measurement error in Table 2-4, I now turn to the suggested strategies in estimating the relationship represented by equation (33).

2.5.2 Illustrating the Bounds on the Parameter and Trichotomization

Equation (33) indicates that a voter prefers a candidate whose ideology is similar to his own and thus gains more utilities by voting for that candidate. Following from the previous applications in the spatial voting theories, a voter's utility from a candidate is measured with "feeling thermometers," ranging from 0 (least favorable evaluation) to 100 (most favorable evaluation) (e.g. Macdonald and Rabinowitz, 1993; Westholm, 1997). The difference between the ratings of Bush and Gore therefore serves as the left hand side (LHS) variable of the equation (33), ranging from -100 (most favorable to Gore) to 100 (most favorable to Bush). The evaluation scores for Bush and Gore are available in both the pre- and post-election surveys. There are many reasons why the previous works have used the evaluation scores instead of the actual vote, although they claim to be spatial "voting" theories.[12] This paper employs the candidate ratings simply because the methods are based on the linearity of the outcome variable. While they can be extended to the binary choice models such as logit and probit, it is nevertheless impossible to obtain closed-form expressions.

I first consider the bounding parameter strategy. The results are reported in Table 5 where the first six columns present the results from the regressions using the pre-election data while the results using the post-election data are displayed in the last six columns. Entries are the unstandardized coefficient estimates obtained by regressing the difference between the ratings of Bush and Gore on the ideological distance measure. The estimates in the first and seventh columns are obtained by simple regressions with all available observations, so each of them constitutes a lower bound on the parameter β.[13] The sixth and twelfth columns present the IV estimates constituting upper bounds. As mentioned above, the ideological distance measure using the pre-election data serves as an instrument for the same measure using the post-election data, and vice versa. Note that the F-statistics from the first-stage regressions are at least 159, as reported under the R^2 in the table. Finally, the second to fifth and eighth to eleventh columns display the estimates obtained from running regressions with subsets of individuals with high levels of certainty. The uncertainty variable, ranging from "0. not very certain for any of the candidates' placements" to "4. very certain for both," enable us to find subsets of individuals with lower middling propensity. Using this variable, I run the regressions after confining the sample to the group of individuals with the certainty levels greater than or equal to one, two, three, four, and the estimates are reported in the second, third, fourth, and fifth columns using the pre-election data and in the eighth, ninth, tenth, and eleventh columns using the post-election data.

[12] According to Westholm (1997), the evaluation scores allow us to gauge the comprehensive preference ordering while the actual vote keeps to the peak preference. Moreover, the size of utility difference can only be approximated with the evaluation scores. Finally, the evaluation score has long been used as a proxy for the vote because it is the most powerful predictor (e.g. Brody and Page, 1973).

[13] Throughout the paper, a lower bound refers to a bound closer to 0 as the attenuation bias occurs in a multiplicative form. Conversely, an upper bound refers to a bound further from 0.

Note that the uncertainty variable is chosen over the other two variables for finding tighter lower bounds. In the horse-race results of probits and ordered probits in Tables 2-4, the effect of the uncertainty variable dominates those of the other two variables. Table 2, for instance, indicates that uncertain respondents have a 31.32 (34.84) percentage point higher chance of placing Bush (Gore) near the middle than certain respondents in the pre-election interviews. The middle categories in the ideological distance measure are chosen by uncertain respondents with a 31.78 percentage point higher probability than by certain ones. In contrast, the other two variables hardly change the probabilities at all.[14] As a respondent is assumed to move from certain to uncertain in the post-election wave, the probability of placing Bush (Gore) in the middle is significantly increased by 31.97 (47.74) percentage points, and an increase of 38.93 percentage points in the probability for the ideological distance measure. Again, the influences of the other two variables are almost zero.[15] The results from the ordered probit analyses are essentially identical. In the upper (lower) panel of Table 4, the probability that the exact middle category of the ideological distance measure is chosen increases by 24 (30.15) percentage points as a respondent is assumed to move from certain to uncertain. Uncertain respondents also gravitate toward the categories near the middle with a 10.81 (12.84) percentage point higher chance than certain respondents. On the other hand, the information and education variables do not change any of the probabilities. The horse-race results may be due to the post-treatment effect: education and/or political information tend to make a respondent certain. Nevertheless, it is obvious that the certainty variable is better than the two variables at predicting the middling tendency.[16]

[14]The coefficient estimate for the education variable in the eighth column even has a positive sign (though statistically insignificant), indicating that respondents with higher education tend to choose the middle more than those with lower education. This may be due to the post-treatment bias, or simply suggest that the education variable is not a good proxy in predicting the middling tendency.

[15]The coefficient estimate for the political information variable is statistically significant in the last column. However, the insignificant coefficient estimates and the tiny percentage point increase in probabilities in the fourth and eighth columns are sufficient to choose the certainty over the information variable.

[16]Researchers may use the education and/or information variable(s) in the absence of the follow-up questions, but they should be aware that these variables are imperfect proxies. While the certainty levels of respondents are directly measured by the follow-up survey instruments, the education and interviewer's assessment indirectly measure the levels of political sophistication which undoubtedly vary case by case. In order to use these proxies for finding better bounds, researchers must also decide whether these variables satisfy condition (17), that the true parameter is constant across the groups differentiated by a survey instrument, i.e. low vs. high levels in this context, but the estimates differ due to the heterogeneity of measurement error. This condition may be violated if, say, education magnifies the degree to which the true ideological proximity affects the evaluations of the candidates and voting decisions. However, the condition can be satisfied if, say, respondents with higher

Initially, the true coefficient β ranges from the lower bound obtained by the OLS in the first or seventh column to the upper bound obtained by the IV in the sixth or twelfth column. Adding to this, the probit and ordered probit results in Table 2-4 suggest that individuals with high levels of certainty may have lower middling propensities, and thus, one may find tighter lower bounds from the estimates obtained using the subsets of individuals with high levels of certainty. Therefore, we may obtain bounds on the parameter β using the results reported in Table 5.

The top panel in Table 5 presents the unadjusted estimates obtained by the bivariate regressions. The estimated range is [-13.89, -16.20] for the pre-election data and [-14.19, -15.19] for the post-election data.[17] The bounds are fairly tight. Most previous research on the spatial voting theories used this unadjusted specification without controlling for other variables (e.g. Merrill, 1994; Westholm, 1997). Was the previous literature too optimistic about the omitted variable bias? Maybe not. The second panel from the top reports the regression results after holding socio-economic variables constant.[18] The results are not very different from those in the top panel. An exception is the work by MacDonald and Rabinowitz (1993), where they controlled for the party identification.[19] They noted that including the party identification in the specification systematically changed the results, but also noted that it is controversial to include the control for party ID due to the possibility of post-treatment bias. The same changes are found in the third panel from the top in Table 5, where the reported estimates are obtained from the regressions after controlling for party identification. In this specification, the estimated range is computed as [-10.04, -12.39] and [-9.136, -12.26] for the pre- and post-election data, respectively. Similar bounds can be found in the lowest panel which presents the regression results holding both party ID and socio-economic characteristics constant.

Next, I consider the trichotomization strategy which involves creating two binary variables from the observed response and comparing the differences-in-means among three groups, with

education merely take tests (i.e. survey questions) better than those with lower education.

[17]Note the paucity of individuals with the certainty level equal to 4 in the pre-election interviews. Instead, I chose the estimate in the fourth column (certainty levels $\geqq 3$) as the lower bound.

[18]The control variables include age, gender, educational attainment, household income, and race dummies indicating whether the respondent is white, black, or other.

[19]They also included a dummy for black candidates and a dummy indicating whether the candidate is southern white or not. Since this study excludes all minor candidates from the beginning, these candidate characteristics are not necessarily included in the control variable structure.

one group being the base. In order to illustrate this strategy empirically, I divide the sample into three groups based on the values of the ideological distance measure, $|Self_i - C_{iR}| - |Self_i - C_{iD}|$. First, a group of individuals perceive both candidates to be similarly proximate. These individuals have -1,0,1 in the ideological distance measure and thus are highly likely to contain the concentrated measurement errors. Second, the base group consists of the individuals who are congruent more with Gore than with Bush in the ideological position. These individuals have less than or equal to -2 in the ideological distance measure. Third, Bush's ideology is perceived to be closer to a group of individuals compared to Gore's. These respondents have greater or equal to 2 in the ideological distance measure. With these three groups, I estimate the difference-in-means and report the results in Table 6.

In an earlier section, the theoretical discussion and the Monte Carlo simulation show that trichotomization reduces the bias from the concentrated measurement error. Unfortunately, we cannot directly compare the biases in the estimates reported in Tables 5 and 6 because we can observe only estimates, but not true parameters, i.e., we cannot compare $\hat{\gamma}/\gamma$ to $\hat{\beta}/\beta$ without knowing γ and β. If the certainty variable could *perfectly* isolate individuals whose responses were purely true, with no middling tendency *and* no random measurement error, then the estimates using the subset of individuals with the highest certainty level might substitute for γ and β. However, the certainty variable can only partially represent the middling tendency.

With this limitation in mind, the estimates in Table 6 serve as an illustration of the trichotomization strategy. The justification for using this strategy relies on the mathematical discussion and the simulation. Given that the dependent variable ranges from -100 to 100, the differences-in-means estimates reported in Table 6 clearly distinguish one group's behavior from another's.

2.5.3 Further Remarks

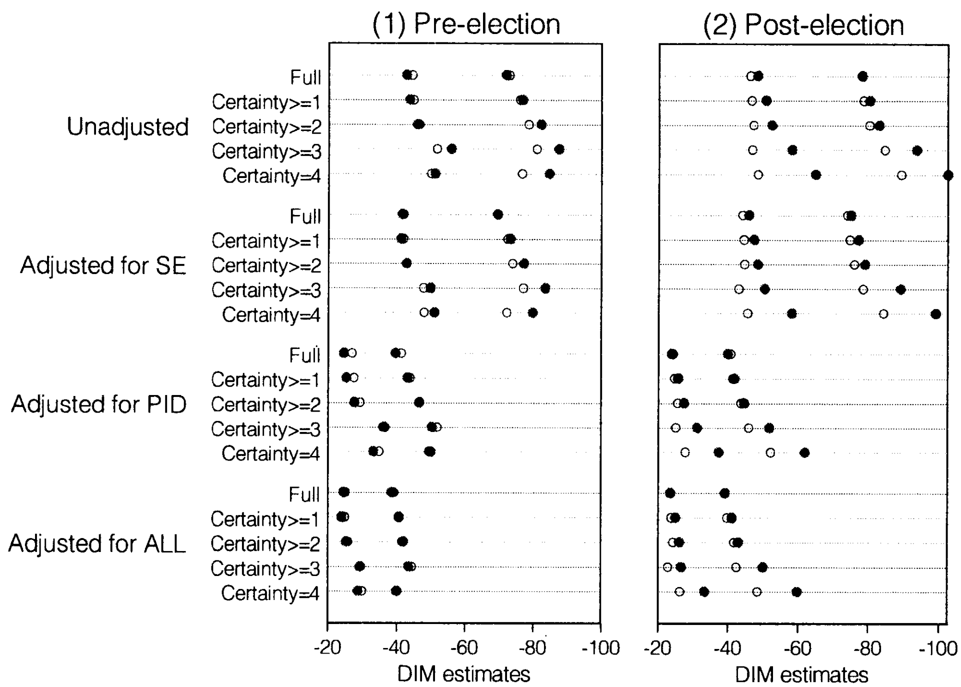
The purpose of Section 2.5 is to illustrate this chapter's strategies and findings in an applied setting. Under no circumstances does this section attempt to verify the spatial voting theories; rather, it has taken the previous theories as given and, therefore, has assumed equation (33) to be flawless.

Interestingly, however, further analyses reveal quadratic relationships between the LHS and

RHS variables of equation (33). Table 7 supports the linearity of equation (33) in only the last two columns. In contrast, the other eight columns reveal quadratic relationships; yet, the problem of measurement errors in nonlinear models is beyond the scope of the present study.[20]

If we *impose* linearity when a relationship is nonlinear up to a certain degree, the least square estimator is still expected to be biased toward zero under the concentrated measurement error assumption. The bounding strategy may also be valid in this circumstance. However, the underlying nonlinearity can easily affect differences in means, and thus, the trichotomization strategy may not always yield better estimates.

Figure 5. Comparison between Least Squares and Trichotomization



A rough comparison between least squares and trichotomization using the ANES data will help explain the relationship between nonlinearity and improvement by trichotomization. Figure 5 plots the DIM estimates analogous to those in Table 6 using predicted values obtained

[20]Only recently has research begun to analyze the measurement error problem in nonlinear models (e.g. Hong and Tamer, 2003; Hsiao and Wang, 2000), and this problem is far too complicated under the concentrated measurement error assumption.

from least squares and trichotomization as dependent variables. The solid circles represent the DIM estimates using the trichotomization strategy and the hollow circles stand for those using the predicted values obtained from least squares as dependent variables. The horizontal axis has its scale reversed so that DIM estimates farther from zero are located farther to the right on the axis.

Figure 5 shows that trichotomization yields the clearest improvements when the underlying model is linear. Going back to Table 7, the linear relationship appears only in the last two columns, where the estimates are obtained using the post-election data with the subsets of individuals whose certainty level is greater than or equal to 3. In the other eight columns of Table 7, quadratic relationships are present which in turn may affect the differences in means. As displayed in Figure 5, it is not clear whether trichotomization reduces the bias when a quadratic relationship occurs. Therefore, it might be necessary to mildly tone down the argument about the advantage of trichotomization. Researchers should carefully check whether nonlinearity hampers the model before they use the trichotomization strategy.

2.6 Conclusions

It has long been recognized that some respondents are prone to choose the categories around the middle for reasons other than a true moderate attitude. Scholars have addressed this issue in the context of measurement error and tried to improve the quality of both the data and of the estimates resulting from the data. However, they have not recognized that this middling tendency leads to the non-classical measurement error that is negatively correlated with the true variables. This unique type of error in variables is also different from the typical non-classical EIV, as the errors are concentrated around the mid-point.

To my knowledge, this study is the first attempt to model the concentrated measurement error. Based on the qualitative theories that have been developed by previous scholars, this paper formally analyzes the way in which the middling tendency of some respondents biases the coefficient estimates in the regression framework. More specifically, I have demonstrated that the regression coefficient estimates can be biased in any direction, but the attenuation bias is more common in political surveys. Moreover, the degree of attenuation gets worse as the more people are gravitated to choose the middle response categories.

Searching for a cure for this problem, this chapter first evaluates an intuitive way of excluding the middle. Since the errors are concentrated around the middle, this end-group strategy, including the old-fashioned Bartlett estimator, seems reasonable to consider. However, it turns out that it is not an ideal solution, as the asymptotic biases of the end-group estimators are the same as those of least squares. Note that this paper is open to the possibility that this strategy may improve the estimation in finite samples. All theoretical results in this paper are based on large-sample theories.

While this study could not identify a consistent point estimation, it can exploit some information from biased estimates. If a panel structure is available where the survey items that are plagued with concentrated measurement error are repeatedly asked, then we can use the observed response of the identical survey item at one wave as an instrument for the observed response at the other wave. While the obtained IV estimate is not asymptotically unbiased, it constitutes an upper bound on the parameter of interest. If a follow-up question on, for instance, certainty or involvement is available, we can use the auxiliary information obtained from it to find a tighter lower bound. In this chapter, I also mathematically prove that trichotomization reduces the bias from the concentrated measurement error while keeping substantive meaning in political research. A Monte Carlo simulation also supports the theoretical findings. Before employing the trichotomization strategy, however, researchers need to carefully evaluate whether the relationship is linear or not.

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Appendix A: The Asymptotic Biases of End-Group Estimators

Notations and assumptions are given in the main text. By Khinchin's law of large numbers,

$$plim \frac{1}{N \cdot prob(x_i \in S_H)} \sum_{i=1}^{N} y_i \mathbb{1}\{x_i \in S_H\} = E(y|x \in S_H) \tag{A1}$$

$$plim \frac{1}{N \cdot prob(x_i \in S_L)} \sum_{i=1}^{N} y_i \mathbb{1}\{x_i \in S_L\} = E(y|x \in S_L) \tag{A2}$$

$$plim \frac{1}{N \cdot prob(x_i \in S_H)} \sum_{i=1}^{N} x_i \mathbb{1}\{x_i \in S_H\} = E(x|x \in S_H) \tag{A3}$$

$$plim \frac{1}{N \cdot prob(x_i \in S_L)} \sum_{i=1}^{N} x_i \mathbb{1}\{x_i \in S_L\} = E(x|x \in S_L) \tag{A4}$$

$$plim \frac{1}{N[1 - prob(x_i \in S_M)]} \sum_{i=1}^{N} \tilde{x}_i \tilde{y}_i \mathbb{1}\{x_i \notin S_M\} = Cov(x, y|x \in S_M^c) \tag{A5}$$

$$plim \frac{1}{N[1 - prob(x_i \in S_M)]} \sum_{i=1}^{N} \tilde{x}_i^2 \mathbb{1}\{x_i \notin S_M\} = Var(x|x \in S_M^c) \tag{A6}$$

The equation (A1), (A2), and (A5) can be extended to

$$E(y|x \in S_H) = \alpha + \beta E(x^*|x > \kappa_H) + E(\varepsilon|x > \kappa_H) \tag{A7}$$

$$E(y|x \in S_L) = \alpha + \beta E(x^*|x < \kappa_L) + E(\varepsilon|x < \kappa_L) \tag{A8}$$

$$Cov(x, y|x \in S_M^c) = \beta Cov(x, x^*|x \in S_M^c) + Cov(x, \varepsilon|x \in S_M^c) \tag{A9}$$

Let σ_{jk} be the $(j,k)^{th}$ element of variance-covariance matrix of the variables x and x^*. According to Aitken (1934), the vector of expected values of x and x^* after a general selection of x can be obtained by

$$\mathbf{\Psi} = (\psi, \psi \sigma_{11}^{-1} \sigma_{12}) \tag{A10}$$

The variance-covariance matrix after selection is

$$\mathbf{\Sigma} = \begin{bmatrix} \lambda & \lambda\sigma_{11}^{-1}\sigma_{12} \\ \sigma_{21}\sigma_{11}^{-1}\lambda & \sigma_{22} - \sigma_{21}(\sigma_{11}^{-1} - \sigma_{11}^{-1}\lambda\sigma_{11}^{-1})\sigma_{12} \end{bmatrix} \tag{A11}$$

Applying the result in (A11) to (A5), (A6), and (A9) proves the first part of equation (12) that $\frac{\hat{\beta}_E}{\beta} = \frac{1-\mu_v}{(1-\mu_v)^2+\sigma_v^2+\sigma_r^2}$. Applying the result in (A10) to (A1)-(A4), (A7), and (A8) proves the second part that $\frac{\hat{\beta}_W}{\beta} = \frac{1-\mu_v}{(1-\mu_v)^2+\sigma_v^2+\sigma_r^2}$.

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Appendix B: The Asymptotic Biases of $\hat{\gamma}_d$, $\hat{\gamma}_{T1}$, and $\hat{\gamma}_{T2}$ when the Observed Variable has a Pearson type-VII Distribution

From the formula in the equation (A10),

$$E(x^*|x \geqq \kappa) = \frac{1-\mu_v}{(1-\mu_v)^2+\sigma_v^2+\sigma_r^2}E(x|x \geqq \kappa) \tag{B1}$$

$$E(x^*|x < \kappa) = \frac{1-\mu_v}{(1-\mu_v)^2+\sigma_v^2+\sigma_r^2}E(x|x < \kappa) \tag{B2}$$

$$E(x^*|-\kappa \leqq x \leqq \kappa) = \frac{1-\mu_v}{(1-\mu_v)^2+\sigma_v^2+\sigma_r^2}E(x|-\kappa \leqq x \leqq \kappa) \tag{B3}$$

$$E(x^*|x < -\kappa) = \frac{1-\mu_v}{(1-\mu_v)^2+\sigma_v^2+\sigma_r^2}E(x|x < -\kappa) \tag{B4}$$

Thus, equation (18) becomes

$$plim\frac{\hat{\gamma}_d}{\gamma} = \frac{1-\mu_v}{(1-\mu_v)^2+\sigma_v^2+\sigma_r^2}\frac{E(x|x \geqq \kappa)-E(x|x<\kappa)}{E(x^*|x^* \geqq \kappa)-E(x^*|x^*<\kappa)} \tag{B5}$$

Similarly, equations (27) and (28) are taken to be

$$plim\frac{\hat{\gamma}_{T1}}{\gamma_1} = \frac{1-\mu_v}{(1-\mu_v)^2+\sigma_v^2+\sigma_r^2}\frac{E(x|-\kappa \leqq x \leqq \kappa)-E(x|x<-\kappa)}{E(x^*|-\kappa \leqq x^* \leqq \kappa)-E(x^*|x^*<-\kappa)} \tag{B6}$$

$$plim\frac{\hat{\gamma}_{T2}}{\gamma_2} = \frac{1-\mu_v}{(1-\mu_v)^2+\sigma_v^2+\sigma_r^2}\frac{E(x|x > \kappa)-E(x|x<-\kappa)}{E(x^*|x^* > \kappa)-E(x^*|x^*<\kappa)} \tag{B7}$$

To obtain functional forms of the equations (B5)-(B7), it is required to compute the differences between conditional moments of the observed variable x_i. First,

$$
\begin{aligned}
&E(x|x \geqq \kappa) - E(x|x < \kappa)\\
&= \frac{1}{prob(x \geqq \kappa)} \int_{\kappa}^{\infty} \frac{x\Gamma(Q-1/2)}{\sqrt{m\pi}\Gamma(Q-1)} \left[1 + \frac{x^2}{m}\right]^{-(Q-1/2)} dx\\
&- \frac{1}{prob(x < \kappa)} \int_{-\infty}^{\kappa} \frac{x\Gamma(Q-1/2)}{\sqrt{m\pi}\Gamma(Q-1)} \left[1 + \frac{x^2}{m}\right]^{-(Q-1/2)} dx\\
&= \frac{\Gamma(Q-1/2)}{\sqrt{m\pi}\Gamma(Q-1)} \frac{m\left(1+\frac{\kappa^2}{m}\right)^{-(Q-3/2)}}{2(Q-3/2)} \left[\frac{1}{prob(x \geqq \kappa)} + \frac{1}{prob(x < \kappa)}\right] \qquad \text{(B8)}\\
&= p_{VII}(\kappa; Q, m) \left(\frac{m+\kappa^2}{2Q-3}\right) \left[\frac{1}{2} I\left(\frac{m}{m+\kappa^2}; Q-1, \frac{1}{2}\right) \left[1 - \frac{1}{2} I\left(\frac{m}{m+\kappa^2}; Q-1, \frac{1}{2}\right)\right]\right]^{-1}
\end{aligned}
$$

Combining (B5) and (B8) gives the equations (25) and (26).

Similarly,

$$
\begin{aligned}
&E(x| -\kappa \leqq x \leqq \kappa) - E(x|x < -\kappa)\\
&= \frac{1}{prob(-\kappa \leqq x \leqq \kappa)} \int_{-\kappa}^{\kappa} \frac{x\Gamma(Q-1/2)}{\sqrt{m\pi}\Gamma(Q-1)} \left[1 + \frac{x^2}{m}\right]^{-(Q-1/2)} dx\\
&- \frac{1}{prob(x < -\kappa)} \int_{-\infty}^{-\kappa} \frac{x\Gamma(Q-1/2)}{\sqrt{m\pi}\Gamma(Q-1)} \left[1 + \frac{x^2}{m}\right]^{-(Q-1/2)} dx\\
&= \frac{\Gamma(Q-1/2)}{\sqrt{m\pi}\Gamma(Q-1)} \frac{m\left(1+\frac{\kappa^2}{m}\right)^{-(Q-3/2)}}{2(Q-3/2)} \frac{1}{prob(x < -\kappa)} \qquad \text{(B9)}\\
&= p_{VII}(\kappa; Q, m) \left(\frac{m+\kappa^2}{2Q-3}\right) \left[\frac{1}{2} I\left(\frac{m}{m+\kappa^2}; Q-1, \frac{1}{2}\right)\right]^{-1}
\end{aligned}
$$

and

$$
\begin{aligned}
&E(x|x>\kappa) - E(x|x<-\kappa) \\
&= \frac{1}{prob(x|x>\kappa)} \int_{\kappa}^{\infty} \frac{x\Gamma(Q-1/2)}{\sqrt{m\pi}\Gamma(Q-1)} \left[1+\frac{x^2}{m}\right]^{-(Q-1/2)} dx \\
&- \frac{1}{prob(x<-\kappa)} \int_{-\infty}^{-\kappa} \frac{x\Gamma(Q-1/2)}{\sqrt{m\pi}\Gamma(Q-1)} \left[1+\frac{x^2}{m}\right]^{-(Q-1/2)} dx \\
&= \frac{\Gamma(Q-1/2)}{\sqrt{m\pi}\Gamma(Q-1)} \frac{m\left(1+\frac{\kappa^2}{m}\right)^{-(Q-3/2)}}{2\,(Q-3/2)} \left[\frac{1}{prob(x>\kappa)} + \frac{1}{prob(x<-\kappa)}\right] \qquad \text{(B10)} \\
&= p_{VII}(\kappa;Q,m)\left(\frac{m+\kappa^2}{2Q-3}\right)\left[\frac{1}{4} I\left(\frac{m}{m+\kappa^2};Q-1,\frac{1}{2}\right)\right]^{-1}
\end{aligned}
$$

Combining (B6), (B7), (B9), and (B10) yields the equations (29)-(31). The equality of $plim \frac{\hat{\gamma}_{T1}}{\gamma_1} = plim \frac{\hat{\gamma}_{T2}}{\gamma_2}$ follows from

$$
E(x^*|x^*>\kappa) - E(x^*|x^*<-\kappa) = 2\left[E(x^*|-\kappa \leqq x^* \leqq \kappa) - E(x^*|x^*<-\kappa)\right] \qquad \text{(B11)}
$$

⊠

Table 1. Simulated Effect of Concentrated Measurement Error on the Variance and Kurtosis of an Observed Variable

σ_r^2	q	p	σ_x^2	θ	σ_r^2	q	p	σ_x^2	θ
0.25	0.2	0.001	0.93	0.18	0.8	0.2	0.001	1.48	0.18
0.25	0.2	0.2	0.66	0.97	0.8	0.2	0.2	1.2	0.3
0.25	0.2	0.4	0.57	1.56	0.8	0.2	0.4	1.14	0.42
0.25	0.2	0.6	0.53	1.99	0.8	0.2	0.6	1.03	0.32
0.25	0.2	0.8	0.46	2.34	0.8	0.2	0.8	1	0.34
0.25	0.4	0.001	1.06	0.15	0.8	0.4	0.001	1.58	0.1
0.25	0.4	0.2	0.77	0.95	0.8	0.4	0.2	1.35	0.3
0.25	0.4	0.4	0.73	1.35	0.8	0.4	0.4	1.25	0.4
0.25	0.4	0.6	0.66	1.7	0.8	0.4	0.6	1.2	0.48
0.25	0.6	0.001	1.11	0.13	0.8	0.6	0.001	1.74	0.13
0.25	0.6	0.2	0.93	0.77	0.8	0.6	0.2	1.48	0.29
0.25	0.6	0.4	0.88	1.03	0.8	0.6	0.4	1.43	0.33
0.25	0.8	0.001	1.22	0.07	0.8	0.8	0.001	1.74	-0.04
0.25	0.8	0.2	1.08	0.47	0.8	0.8	0.2	1.63	0.17
0.25	0.999	0.001	1.27	0.02	0.8	0.999	0.001	1.75	0.01
0.5	0.2	0.001	1.18	0.15	1.2	0.2	0.001	1.93	-0.02
0.5	0.2	0.2	0.89	0.69	1.2	0.2	0.2	1.59	0.15
0.5	0.2	0.4	0.81	0.62	1.2	0.2	0.4	1.47	0.13
0.5	0.2	0.6	0.73	0.81	1.2	0.2	0.6	1.48	0.27
0.5	0.2	0.8	0.67	1.02	1.2	0.2	0.8	1.4	0.15
0.5	0.4	0.001	1.33	0.1	1.2	0.4	0.001	2	0.05
0.5	0.4	0.2	1.04	0.56	1.2	0.4	0.2	1.74	0.17
0.5	0.4	0.4	0.95	0.87	1.2	0.4	0.4	1.68	0.33
0.5	0.4	0.6	0.9	1.06	1.2	0.4	0.6	1.6	0.46
0.5	0.6	0.001	1.42	0.07	1.2	0.6	0.001	2.13	0.11
0.5	0.6	0.2	1.14	0.4	1.2	0.6	0.2	1.9	0.16
0.5	0.6	0.4	1.09	0.41	1.2	0.6	0.4	1.77	0.34
0.5	0.8	0.001	1.44	0.03	1.2	0.8	0.001	2.2	0.02
0.5	0.8	0.2	1.31	0.27	1.2	0.8	0.2	1.98	0.23
0.5	0.999	0.001	1.52	0.03	1.2	0.999	0.001	2.17	-0.09

Note: Simulated values of x_i are obtained by the equations (1)-(3). The number of observations is set to 10000, $x_i^* \sim N(0,1), w_i \sim N(0,1)$ and $r_i \sim N(0,\sigma_r^2)$.

Table 2. Probit Models of Middling Tendency

	Bush				Gore				$\lvert$Self−Bush$\rvert$ − $\lvert$Self−Gore$\rvert$			
Pre-Election Interviews												
Coefficients												
Certainty	-0.293**			-0.294**	-0.322**			-0.321**	-0.267**			-0.257**
	[0.046]			[0.050]	[0.047]			[0.049]	[0.054]			[0.056]
Political Info.		-0.082		0.016		-0.093*		-0.051		-0.121*		-0.026
		[0.046]		[0.053]		[0.046]		[0.053]		[0.055]		[0.063]
Education			-0.068	-0.011			0.029	0.089			-0.076	-0.042
			[0.045]	[0.051]			[0.045]	[0.052]			[0.053]	[0.058]
$\triangle$ $prob(y = 1)$												
Certainty	-0.3120			-0.313	-0.349			-0.348	-0.329			-0.317
Political Info.		-0.113		0.022		-0.129		-0.072		-0.167		-0.036
Education			-0.099	-0.016			0.044	0.131			-0.112	-0.061
N	777	777	776	775	779	778	777	777	615	615	615	614
Post-Election Interviews												
Coefficients												
Certainty	-0.299**			-0.298**	-0.460**			-0.470**	-0.381**			-0.334**
	[0.035]			[0.037]	[0.036]			[0.038]	[0.042]			[0.044]
Political Info.		-0.087*		0.023		-0.134**		-0.000		-0.254**		-0.135**
		[0.036]		[0.042]		[0.035]		[0.042]		[0.043]		[0.048]
Education			-0.078*	-0.025			-0.035	0.059			-0.137**	-0.026
			[0.035]	[0.039]			[0.034]	[0.039]			[0.039]	[0.043]
$\triangle$ $prob(y = 1)$												
Certainty	-0.320			-0.319	-0.477			-0.486	-0.440			-0.389
Political Info.		-0.121		0.032		-0.191		-0.000		-0.346		-0.184
Education			-0.109	-0.035			-0.051	0.086			-0.192	-0.037
N	1,379	1,376	1,376	1,373	1,385	1,383	1,384	1,380	1,156	1,154	1,156	1,152

All variables are standardized. Robust standard errors in brackets. ** $p<0.01$, * $p<0.05$.

Table 3. Coefficient Estimates from the Ordered Probit Models of Middling Tendency

	Bush				Gore				|Self−Bush| − |Self−Gore|			
Pre-Election Interviews												
Certainty	-0.266**			-0.269**	-0.332**			-0.334**	-0.299**			-0.284**
	[0.043]			[0.047]	[0.043]			[0.045]	[0.044]			[0.046]
Political Info.		-0.066		0.037		-0.086*		-0.035		-0.152**		-0.067
		[0.038]		[0.045]		[0.039]		[0.045]		[0.046]		[0.054]
Education			-0.082*	-0.041			0.020	0.075			-0.048	0.003
			[0.036]	[0.041]			[0.037]	[0.042]			[0.045]	[0.050]
N	777	777	776	775	779	778	777	777	615	615	615	614
Post-Election Interviews												
Certainty	-0.273**			-0.276**	-0.443**			-0.452**	-0.405**			-0.374**
	[0.032]			[0.033]	[0.031]			[0.034]	[0.034]			[0.036]
Political Info.		-0.068*		0.035		-0.130**		-0.011		-0.218**		-0.093*
		[0.031]		[0.036]		[0.030]		[0.035]		[0.034]		[0.039]
Education			-0.072*	-0.031			-0.028	0.066*			-0.114**	-0.016
			[0.029]	[0.032]			[0.028]	[0.032]			[0.033]	[0.036]
N	1,379	1,376	1,376	1,373	1,385	1,383	1,384	1,380	1,156	1,154	1,156	1,152

All variables are standardized. Robust standard errors in brackets. ** $p<0.01$, * $p<0.05$.

Table 4. Discrete Changes in Probabilities for the Ordered Probit Models of Middling Tendency

	Bush				Gore				|Self−Bush| − |Self−Gore|				
$\triangle prob(choosing)$	1 or 7	2 or 6	3 or 5	4	1 or 7	2 or 6	3 or 5	4	$\|y\| >= 4$	$\|y\| = 3$	$\|y\| = 2$	$\|y\| = 1$	$y = 0$
Pre-Election Interviews													
Unadjusted													
Certainty	.1335	.1514	-.127	-.1578	.1701	.1898	-.0890	-.2709	.2422	.096	.0267	-.1123	-.2530
Political Info	.0422	.0493	-.0388	-.052	.0558	.0650	-.0258	-.0950	.1301	.0570	.0217	-.0581	-.1507
Education	.0554	.0641	-.0508	-.0687	-.0144	-.0152	.0070	.0226	.0472	.0187	.0051	-.0221	-.0489
Horse-Racing													
Certainty	.1350	.1525	-.1282	-.1593	.1704	.1911	-.0894	-.2722	.2287	.0932	.0260	-.1081	-.2399
Political Info	-.0236	-.0276	.0239	.0273	.0214	.0280	-.0123	-.037	.0589	.0263	.0078	-.0301	-.0630
Education	.026	.0333	-.0271	-.032	-.0505	-.0604	.0300	.081	-.0035	-.0014	-.0003	.0018	.0034
Post-Election Interviews													
Unadjusted													
Certainty	.1246	.1695	-.1407	-.1534	.2398	.2212	-.1913	-.2698	.3718	.0941	-.0026	-.1342	-.3291
Political Info	.0399	.0549	-.0444	-.0505	.1000	.0857	-.0739	-.1118	.2349	.0609	.0025	-.0839	-.2145
Education	.0425	.0590	-.0469	-.0545	.0227	.0181	-.0169	-.0239	.1302	.0315	-.0017	-.0480	-.1119
Horse-Racing													
Certainty	.1262	.1706	-.1426	-.1542	.2436	.2258	-.1940	-.2753	.3447	.0895	-.0042	-.1284	-.3015
Political Info	-.0202	-.0285	.0245	.0241	.0081	.0083	-.0077	-.0086	.1025	.0283	-.0036	-.0430	-.0841
Education	.0174	.0259	-.0214	-.0218	-.0486	-.0463	.0458	.0492	.0186	.0048	-.0010	-.0079	-.0144

All variables are standardized. Robust standard errors in brackets. ** $p<0.01$, * $p<0.05$.

Table 5. Bounds on the Coefficient for the Ideological Distance Measure

	Pre-Election Interviews						Post-Election Interviews					
	Full	$C \geqq 1$	$C \geqq 2$	$C \geqq 3$	$C = 4$	IV	Full	$C \geqq 1$	$C \geqq 2$	$C \geqq 3$	$C = 4$	IV
Unadjusted												
$\hat{\beta}$	-12.22**	-12.71**	-13.14**	-13.89**	-13.69**	-16.20**	-12.54**	-12.64**	-12.90**	-13.53**	-14.19**	-15.19**
	[0.481]	[0.492]	[0.513]	[0.688]	[0.985]	[0.713]	[0.330]	[0.333]	[0.340]	[0.494]	[0.549]	[0.602]
N	606	534	447	210	98	464	1,146	1,029	908	385	226	466
R^2	0.490	0.517	0.556	0.613	0.595	0.453	0.527	0.552	0.575	0.626	0.694	0.519
F-stat						722.8						950.0
Adjusted for Socio-Economic Characteristics												
$\hat{\beta}$	-11.83**	-12.26**	-12.53**	-13.31**	-12.97**	-16.37**	-12.06**	-12.16**	-12.37**	-12.69**	-13.58**	-15.36**
	[0.518]	[0.533]	[0.556]	[0.788]	[0.954]	[0.791]	[0.372]	[0.377]	[0.383]	[0.552]	[0.617]	[0.691]
N	511	448	375	175	81	393	968	877	773	321	184	393
R^2	0.488	0.513	0.546	0.592	0.612	0.463	0.538	0.564	0.582	0.650	0.726	0.527
F-stat						535.8						762.6
Adjusted for Party Identification												
$\hat{\beta}$	-7.676**	-8.132**	-8.665**	-10.04**	-9.951**	-12.39**	-7.225**	-7.309**	-7.724**	-8.087**	-9.136**	-12.26**
	[0.671]	[0.725]	[0.819]	[1.243]	[1.761]	[1.261]	[0.527]	[0.558]	[0.592]	[0.989]	[1.144]	[1.193]
N	602	530	444	208	97	460	1,134	1,018	899	380	221	462
R^2	0.586	0.603	0.619	0.649	0.644	0.559	0.624	0.643	0.654	0.696	0.744	0.587
F-stat						270.2						192.4
Adjusted for All												
$\hat{\beta}$	-7.336**	-7.590**	-7.847**	-8.454**	-8.029**	-12.58**	-7.026**	-7.133**	-7.477**	-7.572**	-8.674**	-12.47**
	[0.747]	[0.808]	[0.906]	[1.328]	[1.688]	[1.422]	[0.586]	[0.620]	[0.663]	[1.069]	[1.296]	[1.423]
N	509	446	373	174	80	391	959	868	766	318	181	391
R^2	0.583	0.604	0.617	0.653	0.683	0.567	0.627	0.647	0.655	0.711	0.768	0.593
F-stat						188.5						159.0

Entries are unstandardized regression coefficient estimates. Robust standard errors in brackets. ** $p<0.01$, * $p<0.05$.

Table 6. Trichotomization of the Ideological Distance Measure

	Pre-Election Interviews					Post-Election Interviews				
	Full	$C \geqq 1$	$C \geqq 2$	$C \geqq 3$	$C = 4$	Full	$C \geqq 1$	$C \geqq 2$	$C \geqq 3$	$C = 4$
Unadjusted										
$\mathbb{1}\{-1 \leqq x_i \leqq 1\}$	-42.80**	-43.76**	-46.00**	-55.96**	-51.27**	-48.49**	-50.74**	-52.41**	-58.27**	-64.86**
	[3.316]	[3.551]	[3.858]	[6.103]	[8.800]	[2.392]	[2.536]	[2.738]	[4.881]	[6.479]
$\mathbb{1}\{2 \leqq x_i\}$	-72.15**	-76.83**	-82.26**	-87.42**	-84.72**	-78.34**	-80.49**	-83.02**	-93.68**	-102.4**
	[3.596]	[3.713]	[3.875]	[5.930]	[9.040]	[2.476]	[2.478]	[2.597]	[3.775]	[4.246]
N	606	534	447	210	98	1,146	1,029	908	385	226
R^2	0.408	0.438	0.482	0.514	0.448	0.470	0.505	0.525	0.592	0.671
Adjusted for Socio-Economic Characteristics										
$\mathbb{1}\{-1 \leqq x_i \leqq 1\}$	-41.64**	-41.25**	-42.85**	-49.83**	-50.94**	-45.87**	-47.40**	-48.46**	-50.50**	-58.22**
	[3.647]	[3.970]	[4.302]	[6.880]	[8.734]	[2.631]	[2.760]	[2.972]	[5.290]	[6.953]
$\mathbb{1}\{2 \leqq x_i\}$	-69.46**	-73.35**	-77.23**	-83.46**	-79.94**	-74.97**	-77.12**	-78.98**	-89.13**	-99.08**
	[3.890]	[4.096]	[4.318]	[7.088]	[9.756]	[2.769]	[2.793]	[2.958]	[4.253]	[4.710]
N	511	448	375	175	81	968	877	773	321	184
R^2	0.421	0.449	0.485	0.513	0.496	0.489	0.523	0.536	0.621	0.704
Adjusted for Party Identification										
$\mathbb{1}\{-1 \leqq x_i \leqq 1\}$	-24.49**	-25.20**	-27.68**	-36.55**	-33.27**	-23.93**	-25.78**	-27.37**	-31.18**	-37.35**
	[3.420]	[3.734]	[4.236]	[7.096]	[10.27]	[2.656]	[2.894]	[3.147]	[5.516]	[7.611]
$\mathbb{1}\{2 \leqq x_i\}$	-39.72**	-43.27**	-46.76**	-50.38**	-49.98**	-39.99**	-41.94**	-44.54**	-51.82**	-61.94**
	[4.381]	[4.798]	[5.636]	[9.254]	[12.31]	[3.476]	[3.694]	[3.991]	[6.722]	[9.032]
N	602	530	444	208	97	1,134	1,018	899	380	221
R^2	0.553	0.569	0.584	0.602	0.587	0.600	0.624	0.632	0.683	0.727
Adjusted for All										
$\mathbb{1}\{-1 \leqq x_i \leqq 1\}$	-24.49**	-23.92**	-25.33**	-29.36**	-28.71**	-23.66**	-25.06**	-26.17**	-26.75**	-33.41**
	[3.825]	[4.146]	[4.706]	[7.740]	[9.874]	[2.876]	[3.116]	[3.370]	[5.928]	[7.987]
$\mathbb{1}\{2 \leqq x_i\}$	-38.57**	-40.87**	-42.10**	-43.63**	-40.07**	-39.23**	-41.21**	-43.09**	-50.05**	-59.84**
	[4.931]	[5.373]	[6.262]	[10.35]	[13.58]	[3.796]	[4.029]	[4.342]	[6.858]	[9.303]
N	509	446	373	174	80	959	868	766	318	181
R^2	0.558	0.581	0.592	0.624	0.654	0.608	0.631	0.636	0.704	0.754

Entries are unstandardized difference-in-means estimates. Robust standard errors in brackets. ** $p<0.01$, * $p<0.05$.

Table 7. Quadratic Relationship

	Pre-Election Interviews					Post-Election Interviews				
	Full	$C \geqq 1$	$C \geqq 2$	$C \geqq 3$	$C = 4$	Full	$C \geqq 1$	$C \geqq 2$	$C \geqq 3$	$C = 4$
Unadjusted										
|Self−Bush| − |Self−Gore|	-11.85**	-12.42**	-12.83**	-13.06**	-12.33**	-12.20**	-12.29**	-12.57**	-13.40**	-14.18**
	[0.490]	[0.505]	[0.532]	[0.760]	[1.197]	[0.348]	[0.353]	[0.359]	[0.563]	[0.564]
$(\|Self-Bush\| - \|Self-Gore\|)^2$	0.501**	0.355*	0.381*	0.791**	0.873*	0.388**	0.405**	0.368**	0.102	0.0108
	[0.160]	[0.169]	[0.179]	[0.245]	[0.348]	[0.121]	[0.127]	[0.131]	[0.210]	[0.245]
N	606	534	447	210	98	1,146	1,029	908	385	226
R^2	0.497	0.520	0.559	0.628	0.615	0.530	0.556	0.578	0.626	0.694
Adjusted for Socio-Economic Characteristics										
|Self−Bush| − |Self−Gore|	-11.57**	-12.06**	-12.31**	-12.75**	-11.80**	-11.71**	-11.82**	-12.05**	-12.62**	-13.59**
	[0.519]	[0.539]	[0.567]	[0.864]	[1.082]	[0.393]	[0.399]	[0.406]	[0.632]	[0.628]
$(\|Self-Bush\| - \|Self-Gore\|)^2$	0.436*	0.297	0.307	0.634*	0.955*	0.403**	0.399**	0.350*	0.0520	-0.00466
	[0.172]	[0.184]	[0.198]	[0.304]	[0.433]	[0.135]	[0.141]	[0.147]	[0.233]	[0.269]
N	511	448	375	175	81	968	877	773	321	184
R^2	0.493	0.515	0.548	0.602	0.635	0.542	0.568	0.585	0.650	0.726
Adjusted for Party Identification										
|Self−Bush| − |Self−Gore|	-7.334**	-7.820**	-8.261**	-9.132**	-8.708**	-7.051**	-7.130**	-7.561**	-7.992**	-9.115**
	[0.664]	[0.726]	[0.823]	[1.251]	[1.828]	[0.538]	[0.569]	[0.602]	[1.041]	[1.151]
$(\|Self-Bush\| - \|Self-Gore\|)^2$	0.476**	0.373*	0.436*	0.822**	0.844**	0.252*	0.267*	0.236	0.0774	0.0162
	[0.151]	[0.160]	[0.168]	[0.232]	[0.321]	[0.117]	[0.123]	[0.127]	[0.198]	[0.238]
N	602	530	444	208	97	1,134	1,018	899	380	221
R^2	0.591	0.606	0.624	0.666	0.662	0.625	0.645	0.655	0.696	0.744
Adjusted for All										
|Self−Bush| − |Self−Gore|	-7.095**	-7.368**	-7.570**	-8.024**	-7.154**	-6.838**	-6.946**	-7.314**	-7.527**	-8.590**
	[0.741]	[0.810]	[0.906]	[1.345]	[1.727]	[0.591]	[0.625]	[0.667]	[1.098]	[1.254]
$(\|Self-Bush\| - \|Self-Gore\|)^2$	0.409*	0.309	0.346	0.570*	0.859*	0.270*	0.268*	0.224	0.0354	0.0497
	[0.165]	[0.176]	[0.187]	[0.283]	[0.385]	[0.128]	[0.133]	[0.139]	[0.213]	[0.252]
N	509	446	373	174	80	959	868	766	318	181
R^2	0.587	0.606	0.620	0.660	0.701	0.629	0.649	0.656	0.711	0.768

Entries are unstandardized regression coefficient estimates. Robust standard errors in brackets. ** p<0.01, * p<0.05.

Chapter 3

Public Ideological Preferences and Governement Welfare Spending

3.1 Introduction

Today, one of the most intriguing questions in political economy concerns why some countries have more generous welfare policies than others. Alesina and colleagues have appropriately categorized the existing literature on this issue into three groups - economic, political, and behavioral explanations (Alesina, Glaeser, and Sacerdote, 2001). Using this categorization, the existing theories on the determinants of the size of welfare spending include economic factors such as income inequality and trade openness, political factors such as the electoral system and the form of government, and behavioral/attitudinal factors such as racial prejudice and beliefs about fairness and the poor, to name a few.[1] Among the theories mentioned above, the behavioral approaches unanimously assume that government welfare policies reflect public preferences. For example, Alesina and Angeletos showed that social beliefs on income distribution determine the size of a country's welfare system (Alesina and Angeletos, 2005). More specifically, a country is likely to spend highly on welfare if more people in the country believe that poor people are poor not because they are lazy but because they are unlucky, or the more that people believe that luck, rather than effort, determines income. This argument strongly depends on the assumption that mass preferences influence government decisions. The

[1] The literature is enormous. Here is a sample. Alesina and Angeletos (2005), Alesina and Glaeser (2004), Alesina, Glaeser, and Sacerdote (2001), Bradley et al. (2003), Iversen and Soskice (2006), Iversen and Cusack (2000), Luttmer (2001), Lindert (2004; 1996; 1994), Moene and Wallerstein (2001), Meltzer and Richard (1981), Persson and Tabellini (2003; 2000), Rodrik (1998).

logical basis for the argument is that if more people believe that income is unfairly distributed, then more people will feel for the poor, and therefore they will demand that the government redistribute income. If, however, a government does not respond to public preferences, then it is less convincing to make the association between social beliefs on income distribution and the size of welfare spending. Another example of a behavioral theory is the relationship between ethnic or racial prejudice and welfare policy generosity. Many scholars have argued that people do not like to pay taxes that will eventually be used to help poor people of different races or ethnicities, and thus ethnic, linguistic, or racial fractionalization is negatively associated with the size of welfare-state programs (Alesina, Baqir, and Easterly, 2000; 1999; Alesina and Glaeser, 2004). I believe that racial/ethnic differences are relevant, but that there is a missing link. If government welfare policies do not reflect public preferences, then there is no reason to expect any decrease in welfare spending due to an increase of ethnic fractionalization. Can we assume that public opinion necessarily and inevitably influences government welfare policies?

In the political science literature, policy responsiveness has long been understood as a mechanism of democracy. Many theories have been based on the belief that policies reflect the collective wishes of the public through the political system of inputs and outputs. In a fair and open election process, candidates who are committed to implement policies favorable to the public are likely to be elected. Moreover, elected politicians who are not responsive to public preferences will eventually be replaced by other candidates who are responsive to such opinion in the following election. Utilizing a variety of survey data as a measure of public opinion, many scholars have found that this democratic mechanism indeed works, mostly in an American context (Erikson, Wright, and McIver, 1994; 1989; McIver, Erikson, and Wright, 2001; Wright, Erikson, and McIver, 1987; Stimson, McKuen, and Erikson, 1995; Hill and Hinton-Anderson, 1995; Monroe, 1998; Page and Shapiro, 1983). In other advanced democracies such as the UK, Canada, and Germany, scholars have also provided evidence of this link between public opinion and government policy (Brettschneider, 1996; Petry and Mendelsohn, 2004; Petry, 1999; Soroka and Wlezien, 2005; 2004).

Because scholarly studies in democratic jurisdictions clearly indicate the linkage between opinion and policy, it is reasonable to assume that mass preferences do exert an influence on government welfare spending policies. However, it is important to note that the previously mentioned studies all relate to countries in which the democratic system of representation is fully operational. The question then arises regarding whether there are any differences between fully democratic countries and less or non-democratic ones. According to democratic representation theories, policy responsiveness is a quality of a properly functioning democratic system (Powell, 2004; Pitkin, 1967). Therefore, it would be important to empirically test whether and to what extent countries with lower levels of democratic qualities differ from fully democratic countries in terms of welfare policy responsiveness. These systematic differences may, in turn, serve as better evidence to support the mechanisms that traditional representation theories offer.

The purpose of this study is two-fold. First, I aim to analyze whether public ideological preferences are associated with welfare policy generosity around the world. I believe that there is a missing link in the existing behavioral explanations of why some countries spend more on welfare than others. Behavioral variations, such as the social perception of fairness and ethnic/racial fractionalization, may indirectly influence the size of welfare, through the fact that welfare spending policies reflect public preferences. Second, I investigate whether the effects of public preferences are conditioned by the level or quality of democracy. To my knowledge, this study is the first attempt to explore this conditional effect of democracy in welfare state literature. Scholarly knowledge on policy responsiveness is largely limited to the advanced democracies where democratic political systems are fully operational. If one believes that public preferences influence the size of welfare due to the operation of an effective democratic political system, the strength of the effects should, ceteris paribus, depend on the level of democracy.

3.2 Democratic Responsiveness and Welfare Spending

We often think that public preferences influence government decisions in advanced democracies. This responsiveness has been empirically tested by many scholars using survey data as indicators of public preferences. For example, Erikson and colleagues found that there is a strong opinion-policy linkage in the 50 American states (Erikson, Wright, and McIver, 1994; 1989). Hill and Hinton-Anderson further found that this opinion-policy linkage is enhanced when there is an opinion shared between the public and the elites (Hill and Hinton-Anderson, 1995). Outside the US, a number of studies have found similar evidences in other advanced democracies (e.g., Petry, 1999; Petry and Mendelsohn, 2004; Brettschneider, 1996).

It is pertinent to note that several studies have considered welfare policy issues. First, in American state-level democracy, Erikson and associates investigated the relationship between state opinion liberalism, as measured by mean values from a survey-based liberal-conservative ideological identification, and a number of welfare policies (McIver, Erikson, and Wright, 2001; Wright, Erikson, and McIver, 1987). Wright et al. (1987) showed that the more liberal people there are in a state, the higher the state spends on per-pupil education. This opinion liberalism is also positively associated with the scope of Medicaid, AFDC, and tax progressivity measures. McIver et al. (2001) also found that state opinion liberalism positively correlated with AFDC, unemployment insurance, and education spending. A second group of scholars has examined the influence of public preferences on public expenditure, relying on over-time variations and time-series analyses. Employing the thermostatic model of representation, these scholars found that public expenditures increase when people prefer more spending. Not all spending domains show clear relationships, but the scholars in this account found convincing evidence for welfare spending in the US, UK, and Canada (Soroka and Wlezien, 2005; 2004; Soroka and Lim, 2003).

The vast majority of the democratic representation literature has been conducted within the context of individual fully democratic countries, and policy responsiveness has largely been ignored in cross-country comparative studies until recently. Using the OECD social expenditure database and several International Social Survey Program (ISSP) modules, Brooks and Manza find that citizens' policy preferences significantly influenced welfare efforts in 15 advanced countries (Brooks and Manza, 2006a; 2006b). They further argue that their results help understand why welfare states in many countries have persisted in contrast to the widely accepted prediction of the demise of the welfare state; the liberal public may oppose any attempt to shrink the welfare state.

This paper lies in the category of cross-country comparative studies on welfare policy responsiveness. This paper differs from the above-mentioned studies in many ways. By utilizing IMF's GFS, multiple waves of WVS, and other cross-country social surveys, the data incorporates a wider set of countries, including not only OECD countries but also developing ones.

This, in turn, allows us to test not only whether public ideological preferences influence the size of welfare spending, but also whether the effects are conditioned by the level of democracy.

3.3 Why Does Democracy Matter?

As mentioned in the previous section, government policies may reflect public opinion through the inputs and outputs of a democratic system, and welfare policies are no exception to this rule. Theoretically, however, only high-quality democracies can provide an institutionalized connection between the opinions of citizens and policy-makers in general (Powell, 2004; Pitkin, 1967). If this is true, we can reasonably expect there to be significant differences between fully democratic countries and less democratic or non-democratic ones in the extent to which government welfare policies respond to public preferences. What makes fully democratic and less democratic countries different from each other? Through what mechanism does this difference lead to the differences in the extent of responsiveness?

Universal agreement does not exist on the definition of the level of democracy. Like the various definitions of democracy, there are various definitions of the "degree of democraticness" or "quality of democracy." According to the most widely used definition from the Polity project, there are three interdependent essences that determine the level of democracy (Marshall and Jaggers, 2005). The first element is the degree of institutionalization of popular election as a process of selection of alternative policies and leaders. As institutionalized are the free and competitive elections, a fully democratic polity differs in the extent to which all citizens have equal opportunities to be in power through legitimate and regularized procedure. The second element is the degree of institutionalized constraints on executives' power. By having institutionalized constraints, the executives may have to consider others' ideas and preferences in making and implementing policies. The last element is the degree of political freedom such that all citizens can express their preferences in their day-to-day lives and in the political sphere as well. Countries in the world cannot be categorized solely by full-scale democracies and authoritarian regimes. Polities usually have varying degrees of the three elements, and thus political theorists have tried to develop the concepts such as "semidemocracy," "hybrid regime," "pseudodemocracy," and so on (Karl, 1995; Case, 1996).

The three dimensions of "democraticness" help us to understand the mechanism through which democracy operates to link between public policy implementation and mass preferences. Citizens can choose whom to be in political position among many alternative candidates through more institutionalized and competitive elections. In the meantime, politicians offer future action or policies once elected. Voters have opportunities to choose politicians who are credibly committed to providing pleasant policies, and can choose rivals in a competitive election if incumbents do not please them. This legitimate threat by citizens holds politicians more accountable for their policy implementation and limits their power. Therefore, under a fully democratic system, politicians may have incentives to respond to the preferences of the public in order to remain in office.

Citizens may have a high degree of political freedom to express their preferences under a high level of democracy. This in turn leads to a high degree of information available to the general public through, for example, the freedom of mass media. When voters have more fluent information, they can make politicians even more accountable because politicians may be less able to deceive citizens on the policy implementations or their possibly adverse outcomes.

Politicians may also need to please a wider group of people under a higher level of democracy. To understand this mechanism, we need the concepts of "selectorate" and "winning coalition." According to Bueno de Mesquita and his colleagues (Bueno de Mesquita, et al., 2002; 1999), the selectorate is defined as a group of people who have legitimate rights to participate in choosing political leaders. The winning coalition is a subset of selectorate whose support is essential for leaders to remain in the office. These concepts and the level of democracy share some aspects in common. For example, non-existence of universal suffrage means not only the low level of democracy, but that the size of selectorate and that of winning coalition is small. Less institutionalized constraints on the executives' power indicates that a country is less democratic, and that the size of winning coalition is small as well. For this reason, Bueno de Mesquita et al. used the Polity data to test their theories of the link between the political survival and the size of the selectorate/winning coalition (Bueno de Mesquita et. al., 1999). In this sense, the higher level of democracy may entail a larger selectorate and winning coalition. Politicians need support from a wider group of people in order to remain in power, and thus need to please more broad range of people under a high level of democracy.

In sum, a higher level of democracy may indicate that politicians are held more accountable for public policies, and that politicians may need to please a more extensive range of people in order to remain in political positions. Therefore, politicians do have incentives to offer and implement policies that are closer to what the general public wants.

In contrast, a lower level of democracy denotes a lower level of political accountability and smaller size of winning coalition necessary to keep political positions. In a less competitive or unfair election, citizens may lose some degree of opportunities to control politicians. If the political freedom to express views is limited, then the information flows must be bounded, and thus politicians may be more able to utilize the asymmetric information to deviate from what the citizens want. Less institutionalized constraints on executives' power, along with less competitive elections, may indicate the smaller size of a winning coalition. Politicians need to please the smaller group only, and therefore, the political map will be aligned with clientelism or factionalism. All of these imply that politicians in a less democratic country may be less responsive to the general public preferences.

3.4 Data and Measurement

In the present study, the research questions require that we need to rely on the country-level dataset. Obviously, we want the maximum number of observations in the dataset as well. For these reasons, this study uses the International Monetary Fund's Government Finance Statistics (GFS) for the measure of welfare spending. It contains the most extensive government expenditure data in the world. However, I had to restrict the samples because public ideological preferences data for each country comes from cross-country social surveys that are unfortunately not conducted in every country every year. This imposes a strong condition on the dataset employed in this study. I maintain two separate datasets depending on which social surveys are used; one with the multiple waves of World Value Survey (WVS) and the other with a combination of multiple social surveys. Merging the WVS and GFS yields 80 country-year observations from 44 countries. Using a combination of multiple surveys, instead of the WVS alone, increases the country-year observations to 180, but I still have 45 countries due to several reasons including reliability of surveys and the availability of survey items this study needs.[2] Because all of these surveys have been conducted in different countries in different years, these countries are not observed every year. I treat these as random, and do not try to account for any sample selection problem throughout the research.

(1) The Size of Total Welfare Spending

As the dependent variable, I computed total welfare spending from the sum of consolidated central government expenditures on education, health, and social protection obtained from IMF's GFS CD-ROM.[3] I then divided this sum by the current price GDP from IMF's International Financial Statistics On-line, and multiplied it by 100. Note that it is important to use consolidated expenditure data as it eliminates all intergovernmental transactions and creditor-debtor relationships.

(2) Public Ideological Preferences

From the second, third, and fourth waves of the World Value Survey, which cover the years 1989 to 2004,[4] I employed a 10-point scaled item on the self-placement of political orientation, ranging from 1 (Left) to 10 (Right). This item was asked in all three waves. I oriented these values so that higher scores represented more liberal (leftist) views. To get a representative measure of country-level public ideological preferences from this individual survey data, I computed the mean values for each country and standardized them to have a mean of 0 and a standard deviation of 1.[5] I obtained another measure using the relative fraction of the liberal population. After orienting all data so that the higher values represented liberal views, I first computed the fraction of people who chose from 7 to 10, and subtracted from this the fraction of people who chose from 1 to 4 in the question, and then multiplied it by 100.[6]

[2]The list of countries along with the mean values of the key variables is presented in Appendix.

[3]Government expenditures on social protection include cash and in-kind services and transfers in the categories of sickness and disability, old age, survivor, family and children, unemployment, housing, and social exclusion (socially excluded persons not classifiable elsewhere, such as low-income earners, immigrants, indigenous people, victims of criminal violence or natural disasters, etc.)

[4]The first wave was excluded because there is a 5-year gap between the first and second wave.

[5]Note that I tried to use median values instead of mean values, but the variation was too small to satisfy the identification condition. Often the lack of variation resulted in unrealistically inflated variance, and thus the coefficients became highly unreliable when I estimated the empirical model, though the signs of the coefficients were usually the same as the coefficients obtained using mean values.

[6]Note that I also tried to use the fraction of people who chose from 6 to 10 on the scale, minus the fraction of people who chose from 1 to 5. The results of including 5 and 6 on the scale were weaker than those obtained excluding 5 and 6, though both were similar. It is reasonable to expect stronger results using the latter scale, since 5 and 6 may represent neutral positions. Note also that I tried to use an "absolute fraction," by computing

This study assumes that the ideological preference variable captures the welfare dimension. This is a reasonable assumption because a person with more liberal (leftist) political views tends to support tax progressivity, welfare, and redistribution. Welfare and redistribution issues are one of the most important factors in determining one's placement on the left-right ideology scale in most countries. Even if other issues, such as gay marriage or religion, could also be important factors, more liberal (leftist) people still tend to support larger welfare spending.[7] For this reason, many previous studies have used this political orientation as a measure of preferences with regard to welfare spending (Alesina and Angeletos, 2005; Alesina, Glaeser, and Sacerdote, 2001). Note that, as pointed out by Alesina and colleagues, other WVS questions on welfare and redistribution have the problem of "status quo bias." The design of these questions involves the appropriateness of the current level of inequality and redistribution, and therefore the responses to these questions are influenced by widely different levels of inequality and spending across different countries in different years.[8]

The data with WVS in this study has 80 observations, which is two times larger than those of other studies that merged country-level and survey data (e.g. 32 observations in Brooks and Manza, 2006a; 43 observations in Brooks and Manza, 2006b). However, a sample size of 80 is still small from a methodological perspective. This study therefore attempts to increase the sample size by combining multiple cross-country social surveys. This is a feasible option because many cross-country social surveys contain ideological disposition items. Using the 1989-2003 WVS as an anchor, I added the observations from the Comparative Study of Electoral Systems (CSES), Eurobarometer, Central and Eastern Eurobarometer (CEEB), European Election Studies (EES), and Post-Communist Public Study (PCP). I carefully examined and confirmed that the wordings of these surveys' questions on political orientation were not significantly different from the same question in WVS. I also checked the correlation

the fraction of people who chose 7-10 without subtracting the fraction of people who chose 1-4. This measure ended up having multicollinearity problems when interaction terms were included in the empirical model. For this reason, I excluded this absolute fraction measure.

[7]Religiosity may turn voters into conservatives even if they are poor. De La O and Rodden provide strong evidence that religion distracts the poor so that poor religious people are less likely to vote for leftist parties (De La O and Rodden, 2008). These religious and conservative voters are less likely to support welfare programs (Scheve and Stasavage, 2006).

[8]For example, there is a 10-point scale on the issue of income inequality, ranging from 1 ("income should be made more equal") to 10 ("we need larger income differences as incentives for individual efforts"). This question involves the reaction to the current level of inequality and welfare in each respondent's country in a specific year.

between the observations extracted from each survey and those from the rest of the surveys to filter out unreliable data. The correlations among these surveys were sufficiently high to take their reliability as satisfactory (higher than .7).[9] One important thing to mention is that, unlike WVS, the ideological preference question in some surveys is on an 11-point scale.[10] In order to make the ideological preference measure as comparable as possible, I rescaled the 11-point scaled question to a 10-point scale so that it would have the same units and range as the WVS political orientation question. I then computed mean values for each country as before. Next, I combined the data from multiple surveys and standardized these to have a mean of 0 and a standard deviation of 1. Note that I have not measured the relative fraction of the liberal population this time due to the difficulty in rendering the relative fraction measures from 10-point scales comparable to those from 11-point scales.

(3) Level of Democracy

If the quality of the operation of the democratic system is a key to explaining why welfare policies reflect public preferences, then it can be expected that a government in a strong democracy would respond to her own citizens' collective voice more than a government in a less democratic country. This hypothesis is modeled in this study by including the interaction term between public ideological preferences and the different level of democracy.

Here, a question arises: how do we categorize the level of democracy? In this study, I rely on the democracy scores from Polity IV, ranging from 0 (no democracy) to 10 (strongest democracy).[11] The democracy scores represent the varying degrees of the three dimensions that we have discussed above. Instead of treating the democracy score as linear, I cut the samples into subgroups, mainly because of the identification problem with the interaction term in the model specified below. As Table 1 shows, the dataset in this study does not have a sufficiently large number of observations in each democracy score cell; the observations are concentrated around a polity score of 10. Therefore, the dataset was categorized into 2 using a certain threshold $\bar{\theta}_j$. For example, a dummy was set to 0 for countries having a democracy

[9]Note that I did not include all available cross-country social surveys. For example, the European Social Survey (ESS) was not included because there were no overlapping observations with which to check the correlation. I included only the surveys that showed high correlation with other surveys to preserve the reliability of the dataset resulting from the combination of multiple surveys.

[10]CSES and two countries from CEEB (Estonia and Hungary in 1993) have an 11-pt scaled political orientation question.

[11]Note that using a composite measure of the polity score, ranging from -10 to 10, gives similar results.

score of 9 or 10, and they were categorized as the fully democratic countries. Countries having a score of 8 or lower were assigned a score of 1, and these were indicated as the less democratic countries. Note, however, that the estimation using sub-grouped variables may depend on the threshold $\bar{\theta}_j$. I, thus, used all possible thresholds to check that whether the results were driven by a specific threshold. First, I chose countries having a democracy score of 10 (9) (8) as being fully democratic, and those having a democracy score of 9 (8) (7) and below as being less democratic countries (A. 10 vs. 9 and below, B. 10-9 vs. 8 and below, and C. 10-8 vs. 7 and below). Second, I tossed out the unclear middle when categorizing the samples (D. 10 vs. 8 and below, excluding the unclear 9, and E. 10-9 vs. 7 and below, throwing out 8).[12] Because the borderline countries may not be much different from strongly democratic countries, the latter two ways of categorization were expected to give clearer relationships, if any.

The means, inter-quartiles, and distributions of the dependent and independent variables are displayed in Figure 1 and Figure 2. In the figures, the fully democratic samples represent the countries having a democracy score of 10 or 9, and others are considered less democratic countries. Note that most of the welfare spending comes from social protection expenditures, but the health and education expenditures are not negligible.

(4) Other Variables

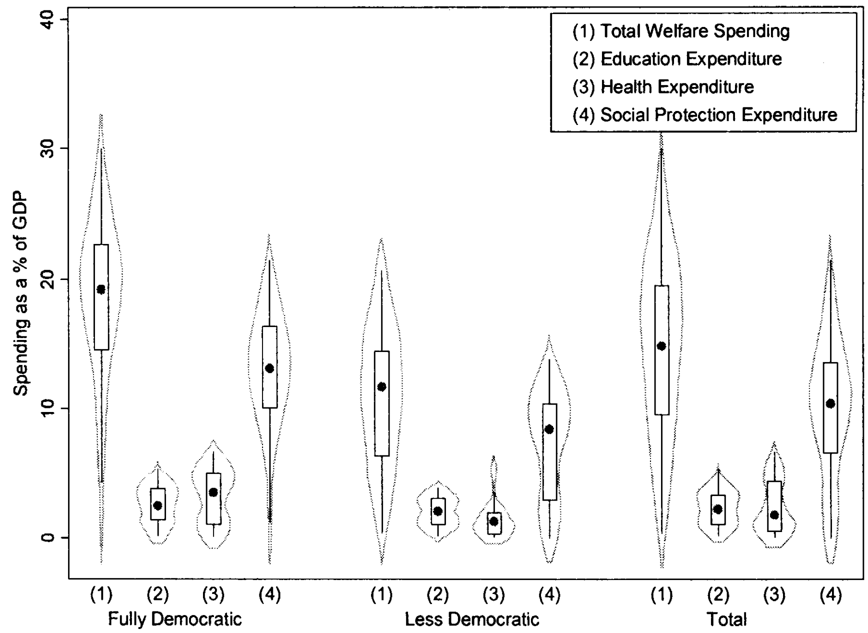
Because welfare policy generosity is affected by many other factors, we need to include economic, geographic, and political variables to control: *GDP per capita* measured in natural log of real GDP per capita in PPP-adjusted constant dollars (chain index),[13] *trade openness* measured by the sum of total exports and imports as a % of GDP, *population aged from 15 to 64* as a % of total population, *population aged 65 and older* as a % of total population,[14] and several political variables from the World Bank's Database of Political Institutions (DPI). These political variables are (1) dummies indicating whether a country in a specific year has the *presidential, assembly-elected presidential, or parliamentary system*, (2) dummies indicating whether a country has the *plurality, mixed PR, or PR electoral system*, (3) a dummy for *federalism*, (4) a dummy for the *leftist partisan control* of the chief executive, and (5) a dummy for the *Christian partisan control* of the chief executive. All regression includes these control

[12]More thresholds are not desirable due to the small sample size.
[13]The original source of this data is Penn World Table 6.2.
[14]Trade and population data were extracted from the World Development Indicators.

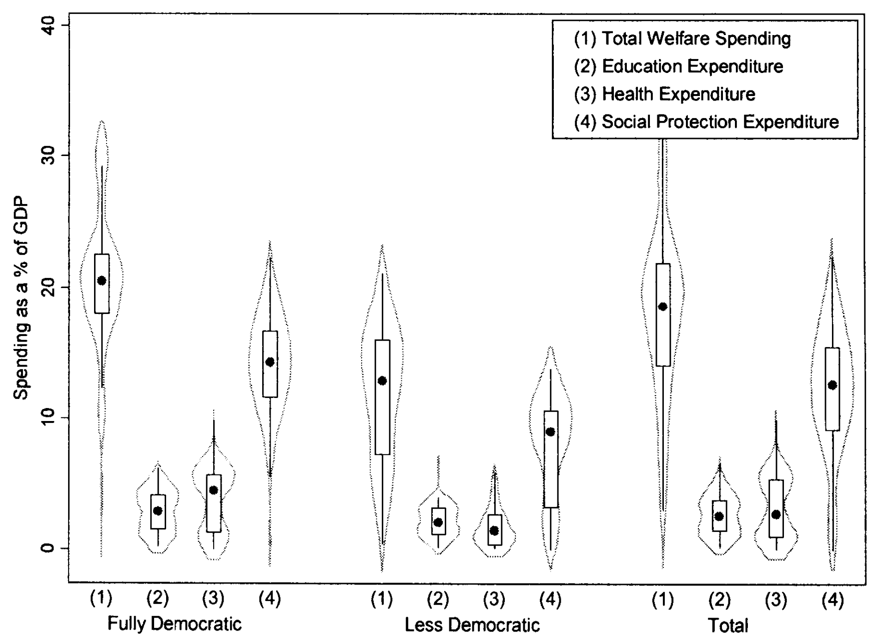
variables, but the last two partisan control variables are excluded in some specifications.

Figure 1. Distributions of Welfare Spending

A. Dataset with WVS (N = 80)



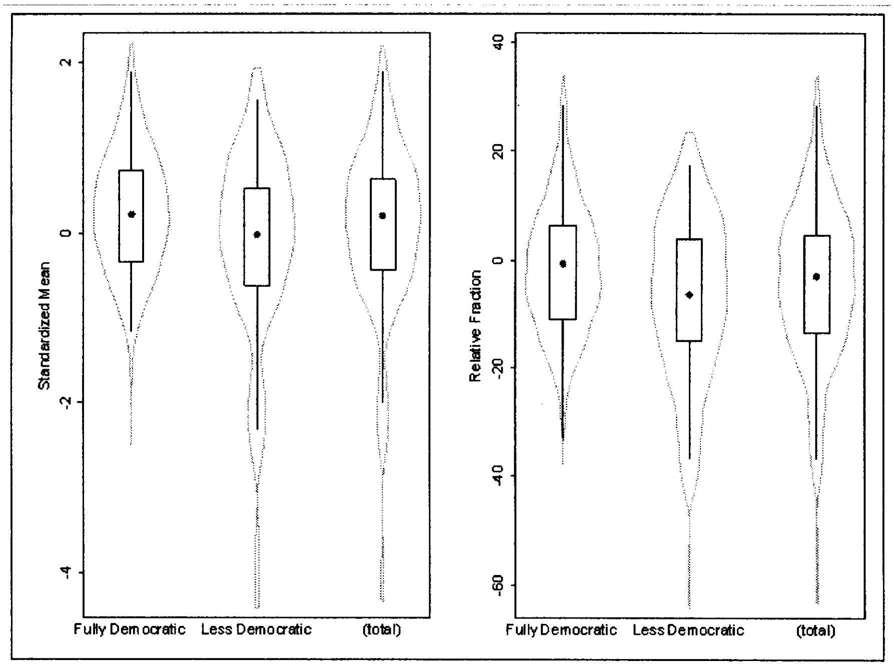
B. Dataset with Combined Surveys (N = 180)



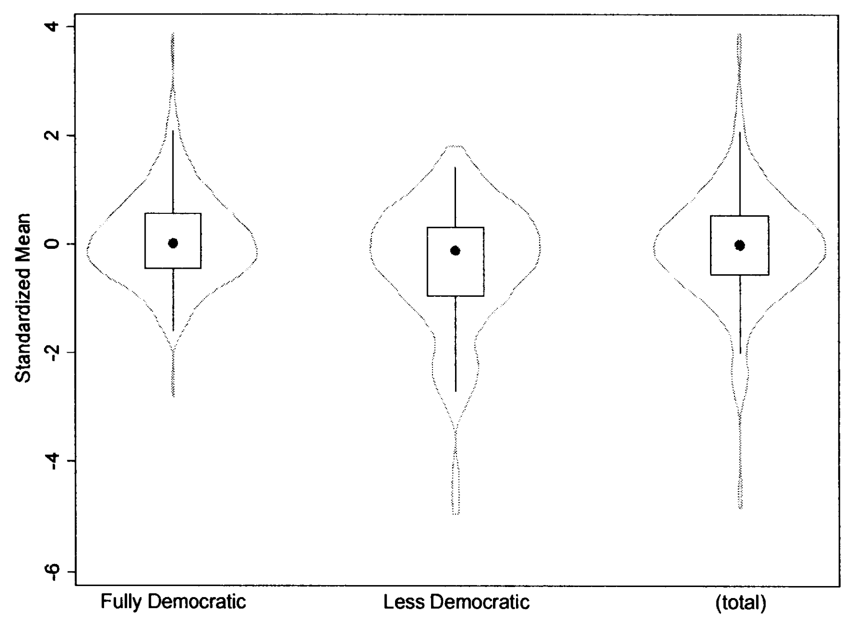
Note: The dot indicates the median. Box shows the interquartile range. Overlaid on the box is a kernel density estimation.

Figure 2. Distributions of the Public Ideological Preferences

A. Dataset with WVS (N = 80)



B. Dataset with Combined Surveys (N = 180)



Note: On the vertical axis, higher scores are associated with more liberal (leftist) preferences. The dot indicates the median. Box shows the interquartile range. Overlaid on the box is a kernel density estimation.

3.5 Empirical Strategy

Like most other datasets that merge country-level and survey data, the dataset used in this study is an unbalanced panel. This study uses OLS with time fixed effects so that we can rely on the cross-country variation within time periods. For the time fixed effects, a vector of dummies for the WVS waves is included. I add year fixed effects when I use the dataset with combined surveys. I also employed robust and clustered standard error, so that heteroskedasticity and correlations among observations within a country are allowed.[15] We also need to control for the regional heterogeneities. It is widely known that OECD countries spend more on welfare than other countries. Following Persson and Tabellini (2003), a dummy variable was included for OECD members before 1993, except for Turkey. Due to the legacies of the communist/socialist regimes, post-socialist countries are also known to have higher levels of welfare expenditures, and so a dummy for these post-socialist countries was also included.

The empirical model is, therefore, as follows:

$$Y_{it} = P_{it}'\beta_1 + D_{it}'\beta_2 + (P_{it} \times D_{it})'\beta_3 + \mathbf{X}_{it}'\alpha + \delta_I + \delta_T + \epsilon_{it}, \qquad (3.1)$$

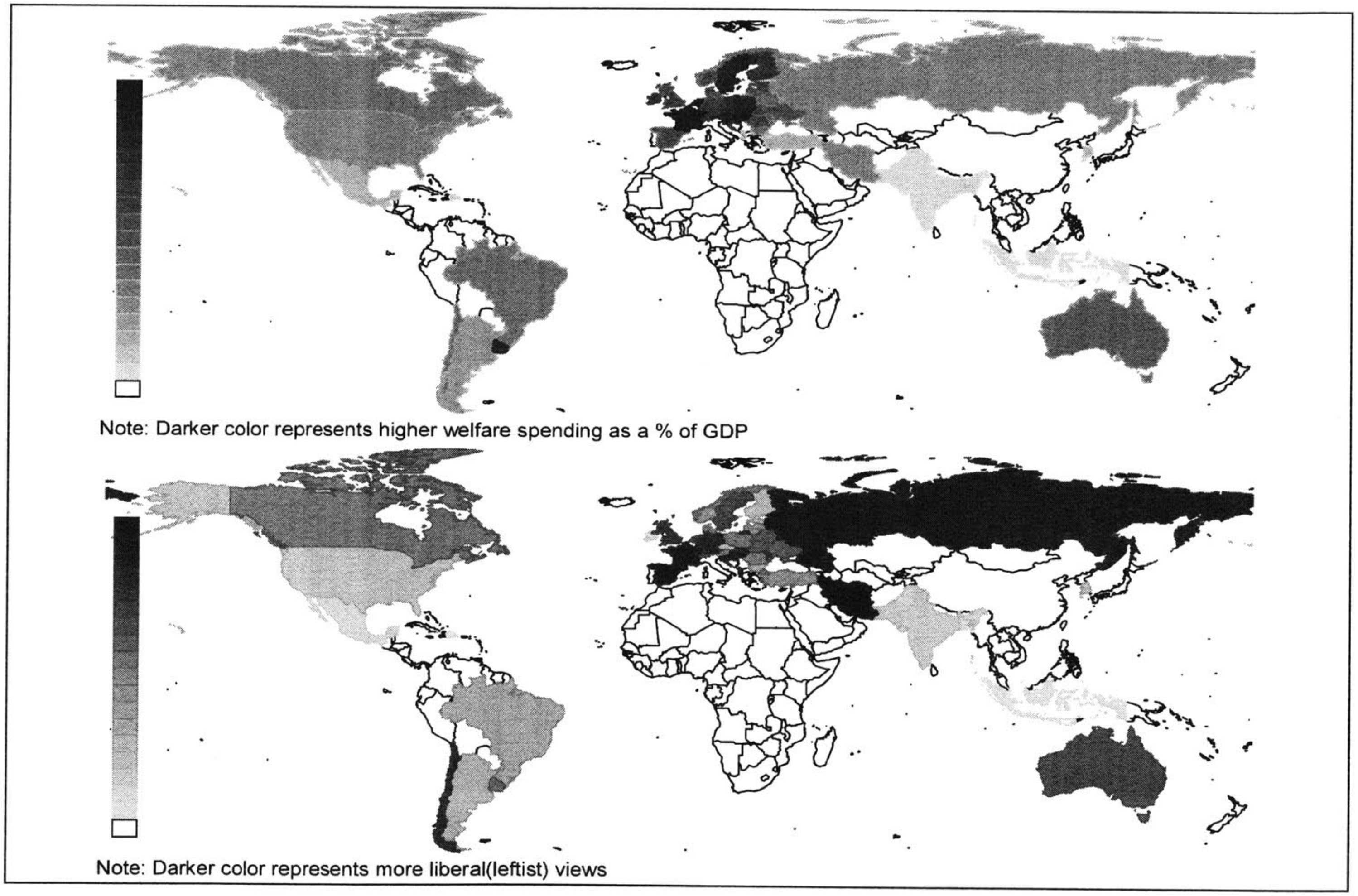
where, Y_{it} is a dependent variable of the size of total welfare spending, P_{it} is a measure of public ideological preferences, D_{it} is a dummy indicating whether a country is a fully or a less democratic country,[16] $P_{it} \times D_{it}$ is an interaction term, $\mathbf{X}_{it}$ is a vector of economic, geographic, and political controls described in the previous section, and δ_I and δ_T are vectors of dummies for regional and time fixed effects.

Along with this benchmark model, I specified several other models to fix some important potential problems. First, I tried to separate out the conditional effects of wealthiness from the conditional effects of democracy as the two may be highly correlated. Second, I estimated the equation (1) after excluding some countries in which the left-right ideological preferences

[15]In a large-N and small-T set-up, the robust-cluster standard error has advantages over other alternatives such as Newey-West estimator or Beck and Katz' panel-corrected standard error. While the former relies on large-N asymptotics, the latter two approaches rely on large-T asymptotics. I note that the results in this study are not sensitive to using the other standard error estimations.

[16]For any value of democracy score θ_{it}, $D_{it} = \begin{cases} 0 \text{ if } \theta_{it} \geq \bar{\theta}_j \\ 1 \text{ if } \theta_{it} < \bar{\theta}_j \end{cases}$ for $\bar{\theta}_j = 10$, 9, and 8. By throwing out the unclear middle, $D_{it} = \begin{cases} 0 \text{ if } \theta_{it} > \bar{\theta}_j \\ 1 \text{ if } \theta_{it} < \bar{\theta}_j \end{cases}$ for $\bar{\theta}_j = 9$, and 8. Therefore, I estimate equation (1) five times using different categorization methods.

Figure 3. Atlas: Welfare Spending as a % of GDP and Public Ideological Preferences



Note: Darker color represents higher welfare spending as a % of GDP

Note: Darker color represents more liberal(leftist) views may not capture the welfare dimensions. Third, I carefully attempted to fix the potentially detrimental measurement error problem by employing several Instrumental Variable estimators. These three alternative specifications will be explained in a more detailed fashion in the next section.

3.6 Findings

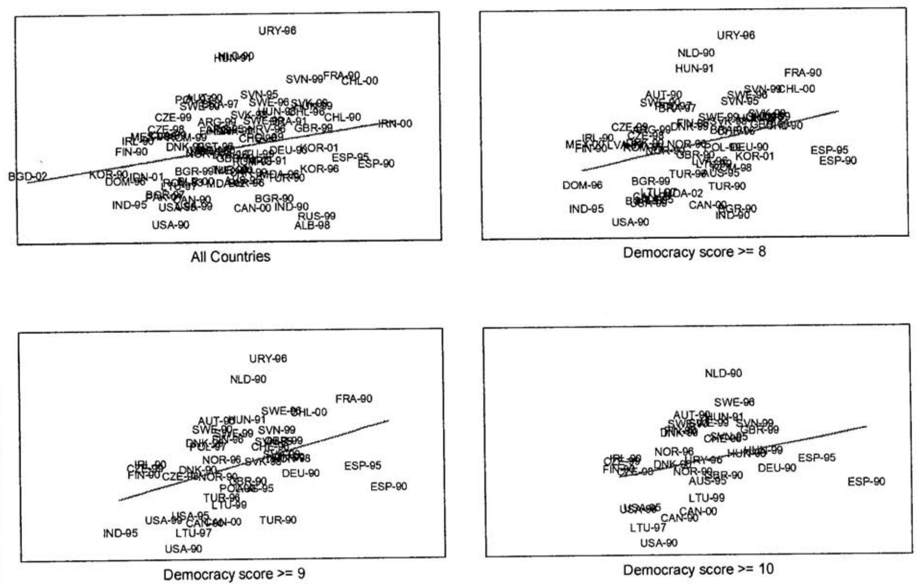
Let us begin with a simple preliminary picture that summarizes the relationship between the public ideological preferences and the welfare policy generosity. Figure 3 displays the atlas of the data. Darker colors represent higher mean values of welfare spending as a % of GDP and those of more liberal (leftist) views in the upper and lower panel, respectively.[17] Although it does not show a 100% match, liberal countries tend to have higher welfare spending in general.

The next pictures clearly demonstrate the conditional effects of democracy. Figures 4 and 5 exhibit the scatterplots of the relationship between the size of total welfare as a % of GDP and the relative fraction of liberal population after controlling for region and time fixed effects. The plots of the data with WVS are displayed in Figure 4 and those of one with combined surveys are displayed in Figure 5. In the upper panels of both figures, I plot the same relationship after gradually excluding more and more of the less democratic countries. More specifically, the scatterplot in the upper-left (UL) position includes all samples; only samples that have a democracy score of higher than or equal to 8 are included in the upper-right (UR) position; I further exclude the countries having a democracy score of 8 and below, and display the scatterplots in the lower-left (LL) position; and finally, the scatterplot in the lower-right (LR) position includes only the samples having a democracy score of 10. In the lower panels of both figures, I do the same process but this time exclude more and more of the highly democratic countries; excluding the countries having a democracy score of 10 in UR; excluding the countries having a democracy score of 9 or 10 in LL; and excluding the countries having democracy score of 8 and below in LR.

[17] White color simply means missing.

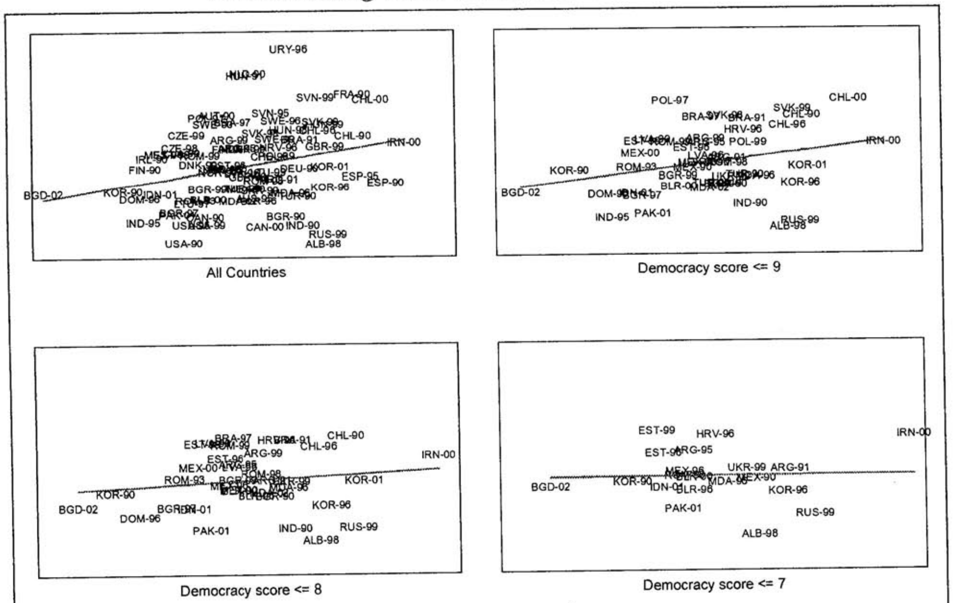
Figure 4. Public Ideological Preferences and the Size of Welfare Spending: Dataset with WVS

A. Excluding Less Democratic Countries



Notes: 1. y-axis represents total welfare spending as a % of GDP
2. x-axis represents relative fraction of liberal(leftist) population

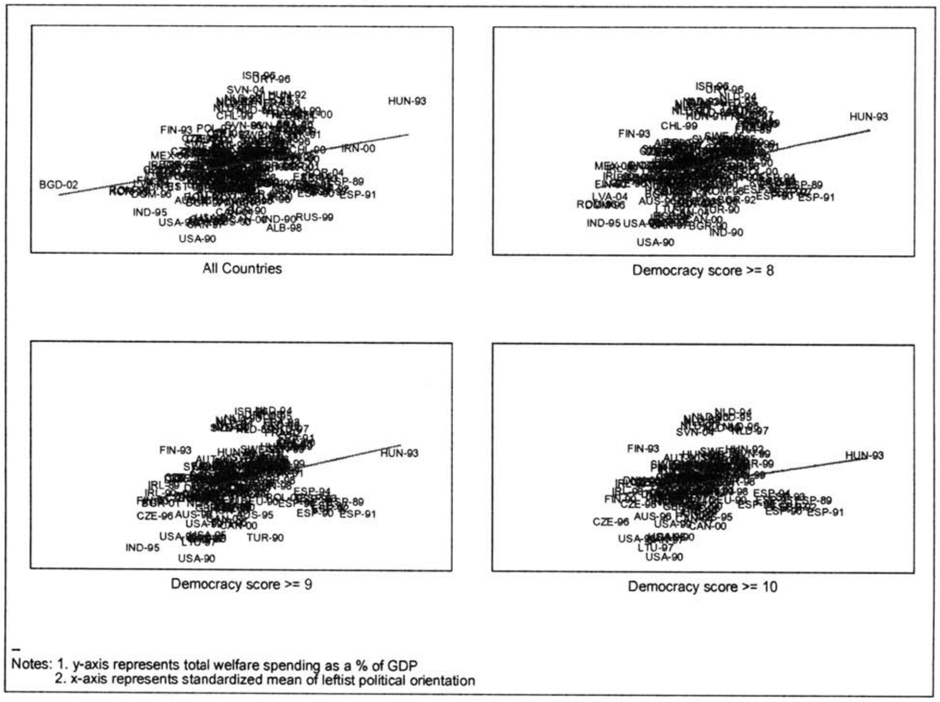
B. Excluding More Democratic Countries



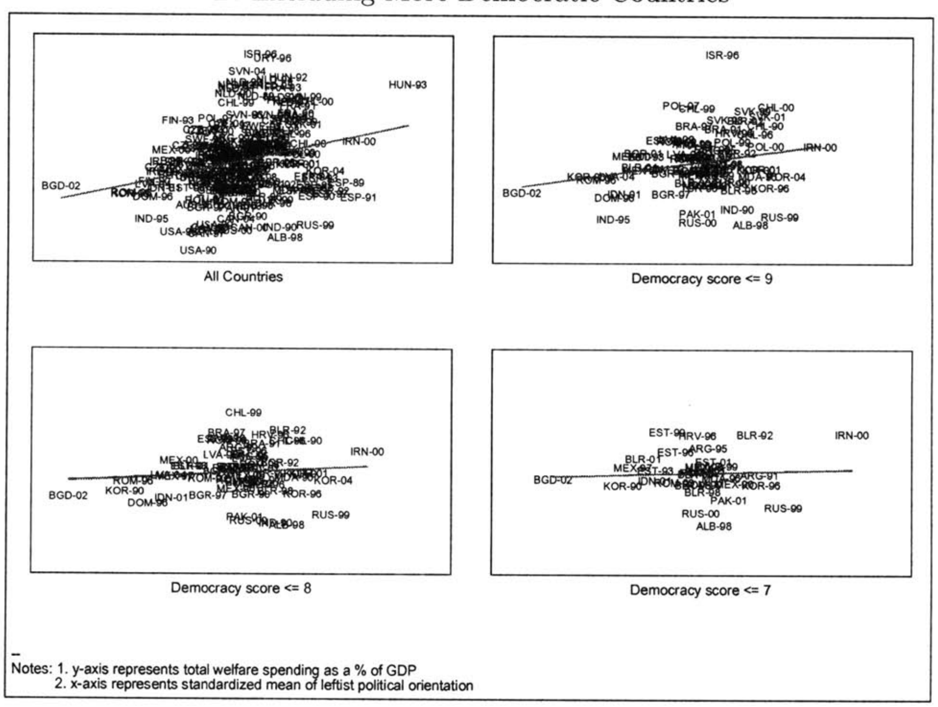
Notes: 1. y-axis represents total welfare spending as a % of GDP
2. x-axis represents relative fraction of liberal(leftist) population

Figure 5. Public Ideological Preferences and the Size of Welfare Spending: Dataset with Combined Surveys

A. Excluding Less Democratic Countries



B. Excluding More Democratic Countries



By excluding more and more of less democratic countries, the scatterplots are getting clearer in revealing the relationship between the public ideological preferences and the size of social welfare. On the other hand, the slope is getting flatter as more highly democratic countries are excluded. These simple pictures may indicate that the public ideological preferences affect the welfare policy generosity, but only in fully democratic countries. Politicians may not respond to the public preferences in less democratic countries.

I next turn to the multivariate analysis to check whether the pictures depicted above merely show a spurious relationship due to the effects of other political, economic, and geographic variables. Mainly due to the space limit, I report the regression tables using only two out of the five different ways of classifying level of democracy; [10-9 vs. 8 and below] and [10-9 vs. 7 and below excluding 8]. For the other categorizations, I use graphs to compactly display the results.[18]

3.6.1 Baseline Results

The regression results estimating the equation (1) are presented in Table 2 (data with WVS) and Table 3 (data with combined surveys). In Table 2, the first four columns report the coefficients estimated from regression on the standardized mean of the public leftist orientation. The last four columns then show the regression estimates using the relative fraction of the liberal population as an independent variable. All regressions include wave dummies to control for time fixed effects. I also include all standard political, economic, and geographic controls that I mentioned above. The coefficients reported in the third, fourth, seventh, and eighth columns are obtained from regression including region fixed effects. I relaxed the regional differences in the specifications reported in the first, second, fifth, and sixth columns. I ran regressions with several different specifications, by including and excluding the Leftist and Christian party controls of the chief executives, because public opinion could be affected by some indoctrination by the governing party or public opinion may indirectly affect the welfare policies only through the party politics. If either were the case, then the effects of public preferences would disappear once partisan control variables were included. The odd columns

[18]There is another reason to report the results using the two categorizations above. With some other ways of classification, it was technically impossible to distinguish the conditional effect of democracy from that of wealthiness. It will be discussed in a later section.

report regression estimates, without including any partisan control variable. The estimates from the regression including party controls are reported in the even columns. The coefficient estimates provide little evidence either that public political orientation is a by-product of a governing party's indoctrination or that the liberal public leads to the leftist or religious party control over the chief executives, which in turn increases the size of welfare spending.

Since the regression estimates of the main independent variables of interest are not significantly changed by different specifications, the results in the fourth and eighth columns with full control variables will be interpreted in this section. The upper and lower panels differ in the way in which fully and less democratic countries are classified. In the upper panel, countries having a democracy score of 9 or 10 are viewed as fully democratic, while countries scoring 8 and below are considered as less democratic samples. In the lower panels, I exclude the unclear middle so that the less democratic samples indicate the countries having democracy scores of 7 or below. Countries having a democracy score of 8 may not be seriously different from those scoring 9. I report not only the estimated coefficients but also the estimates of the marginal effect of the public preferences in less democratic countries. The marginal effects are reported immediately below the row indicating R^2 in each panel.

In the upper panel, the fourth column indicates that a fully democratic country that has a higher public leftist orientation by 1 standard deviation in a given year has a 2.552% higher expenditure on total welfare, expressed as a % of GDP. In less democratic countries, this effect does not exist as the estimated marginal effect shows. From the eighth column, one can find that welfare spending is about 1.64% higher in a fully democratic country that has a 10% more liberal population in the relative fraction (e.g. a 5% greater fraction of liberal population and a 5% lesser fraction of conservatives). This effect also seems non-existent among the less democratic countries.

The lower panel presents coefficients and marginal effects estimated from the regressions after removing the unclear middle. In this case, the effects of public preferences in fully democratic countries are compared with the same effects in less democratic countries, discarding the countries having a democracy score of 8. The effects of public ideological preferences in fully democratic countries are similar to those reported in the first panel. Moreover, the significant interaction effects and the marginal effects indicate that there is a meaningful difference between fully democratic and less democratic countries in terms of the effects of public preferences on the size of welfare.

Table 3 reports coefficient estimates of the main, interaction, and marginal effects, analogous to Table 2, but using the public preferences measured from the combined dataset. As I previously mentioned, I did not compute the relative fraction of liberal population with combined surveys because we may not want to treat the relative fraction measures computed from 10-point scales and those from 11-point scales comparably. All regressions include the standard controls and year fixed effects as well. The first two columns report the coefficient estimates from the regressions whose specification do not include region dummies. The regional differences are controlled in the regression results reported in the last two columns. As before, the coefficient estimates reported in the odd columns are obtained from the regression excluding partisan controls over the chief executives while these partisan controls are included in the regression results reported in the even columns.

The results are remarkably similar to the ones obtained by using WVS only; public opinion influences the welfare policy generosity among fully democratic countries, but the effect does not exist among less democratic countries.[19] The regression results in both Table 2 and Table 3 clearly indicate the significant effects of the public leftist orientation on the size of welfare-state programs, as well as the significant interaction effects with respect to the level of democracy.

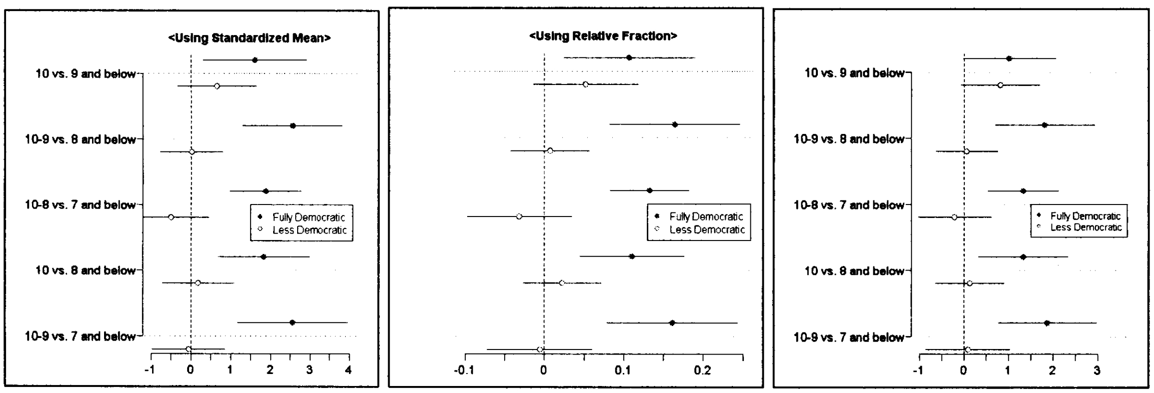
For the results using other categorizations, I draw graphs that concisely present the effects of the public preferences on the size of welfare spending and the conditional effects of democracy. The coefficient estimates and 90% confidence intervals are reported in Figure 6. The solid circles represent the coefficient estimates of the effects of the public preferences among fully democratic countries while the hollow circles stand for the same effects among less democratic countries. The graphs are drawn with the data with WVS in the first two panels; using standardized mean and relative fraction as a measure of independent variable, respectively, in the first and second graphs. The last panel displays the estimates using the data with combined surveys.

The coefficient estimates and the confidence intervals in all panels show that differences in public ideological preferences may explain why some countries spend more on welfare than do

[19]The magnitudes of the main effects are different from those in the analysis using WVS only, but this difference is not important as it is simply driven by different sets of data.

others. Moreover, the graphs point out that the effect of the mass preferences are conditioned by the level of democracy. From the first categorization of the graph in the last panel, we may notice that the countries having a democracy score of 9 may not be much different from those having a democracy score of 10. Given the results using other categorizations, it is nevertheless argued that the quality of a democratic political system is a key to explain the difference in welfare policy responsiveness. Unlike in fully democratic countries, liberal orientation of the public in less democratic countries may not affect welfare spending policies since politicians care less about what the general public want.

Figure 6. Marginal Effect Plots



Note: The solid circles represent the coefficient estimates of the effects of the public preferences among fully democratic countries while the hollow circles stand for the same effects among less democratic countries. The lines indicate 90% confidence intervals. The graphs are drawn with the data with WVS in the first two panels; using standardized mean and relative fraction as a measure of independent variable, respectively, in the first and second graphs. The last panel displays the estimates using the data with combined surveys.

3.6.2 Separating Out the Income Effect

Welfare policy responsiveness could also be conditioned by the level of a country's wealthiness, because government may be able to respond to public opinion only when it has sufficient resources to redistribute. One concern is that the level of democracy may be correlated with the level of wealthiness. If this were the case, then the interaction effects and marginal effects in Tables 2 and 3 would capture the union set of the conditional effect of democracy and that of affluence. I therefore try to separate out the latter.

Table 4 presents summary statistics of the natural log of real GDP per capita according to level of democracy for all 5 categorizations, using the data with WVS and one with combined surveys in the upper and lower panels, respectively. From Table 4, we can see that fully democratic countries are generally richer than less democratic ones, and thus the estimated conditional effect of democracy reported in Tables 2 and 3 may have included some effects of affluence.

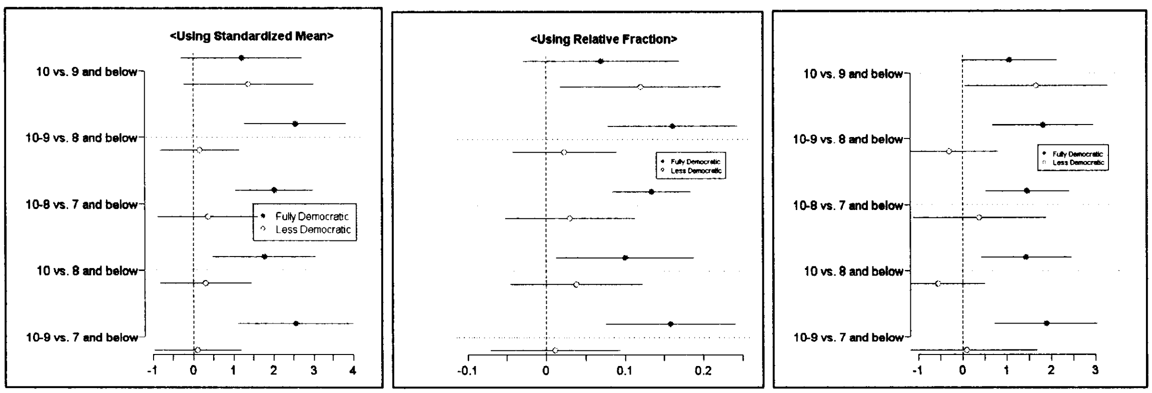
Two different methods are employed to separate out the effect of wealthiness from the conditional effect of democracy. First, I additionally specify the conditional effect of wealthiness to partial it out. Second, I exclude high-income countries and estimate equation (1) again. For the first method, the benchmark model of equation (1) is slightly modified as follows:

$$Y_{it} = P_{it}'\beta_1^P + D_{it}'\beta_2^P + (P_{it} \times D_{it})'\beta_3^P + \tilde{G}_{it}'\beta_4^P + (P_{it} \times \tilde{G}_{it})'\beta_5^P + \mathbf{X}_{it}'\alpha^P + \delta_I + \delta_T + \varepsilon_{it}, \quad (3.2)$$

Equation (2) is only slightly different from equation (1) in the sense that the interaction between the public ideological preferences (P_{it}) and the natural log of GDP per capita (G_{it}) is added. All coefficients are affected by this addition because the coefficient estimates are not unconditional marginal effects in a multiplicative interaction model. To make β_1^P, β_2^P, and β_3^P in equation (2) comparable to β_1, β_2, and β_3 in equation (1), the *demeaned* GDP per capita ($\tilde{G}_{it}$) is included so that we can control for the conditional effect of wealthiness and hold the GDP per capita at its mean.

Tables 5 and 6 report the regression results analogous to those in Tables 2 and 3, respectively, separating out the conditional effect of wealthiness. The results in Tables 5 and 6 show that the coefficient estimates are greatly similar to those without partialling out the conditional wealthiness effect. Figure 7 compactly displays the results using all 5 different categorizations analogous to those in Figure 6. Here, too, the results are essentially the same; public preferences matter but only among fully democratic countries. Note that there were collinearity issues in the specifications using the first (10 vs. 9 and below) and fourth (10 vs. 8 and below, excluding 9) categorizations; the variance inflation factor of interaction term was higher than the conventional threshold of 10. This may be due to the high correlation between the level of democracy and GDP per capita in the specifications using those two categories. Readers may easily find from Table 4 that the fully and less democratic countries have clearly distinct Min-Max ranges of GDP per capita in the first and fourth rows both in the upper and lower panels. Therefore, it may not be useful to interpret the results using these two categorizations in Figure 7.

Figure 7. Partialling Out the Income Effects



Note: The solid circles represent the coefficient estimates of the effects of the public preferences among fully democratic countries while the hollow circles stand for the same effects among less democratic countries. The lines indicate 90% confidence intervals. The graphs are drawn with the data with WVS in the first two panels; using standardized mean and relative fraction as a measure of independent variable, respectively, in the first and second graphs. The last panel displays the estimates using the data with combined surveys.

For the second method of separating out the income effects, I simply exclude high-income countries and estimate equation (1). From the Min-Max range of GDP per capita displayed in Table 4, the countries whose log of per capita GDP is higher than 9.6 are excluded, so that we can keep a similar range of wealth in both fully and less democratic countries for most of the categorization methods.[20] The results using this sample restriction are presented in Appendix due to the space limit.

[20]Upper truncation does not seem sufficient for [10 vs. 9 and below] or [10 vs. 8 and below] to keep the same range in fully and less democratic countries. The minimum log per capita GDP for a country having a democracy score of 10 is already above 9.02. I do not perform a double truncation as this causes too great a reduction in sample size.

3.6.3 What if Ideological Preferences do not Capture the Welfare Dimension?

This study assumes that the ideological preference measure captures the dimension of welfare and redistribution. This is a reasonable assumption, but it may not hold in every country.

We can systematically check whether the ideological preferences indeed capture the welfare dimension in each country by utilizing the 10-point scaled item on income inequality.[21] This item has a status quo bias and thus is not relevant to the main analysis of this study. However, we can utilize it to see if the ideological preferences capture the preferences with regard to welfare and redistribution. If ideology represents such preferences well enough, then liberal people should always be more pro-welfare than the conservatives, no matter what the current levels of inequality and welfare are.

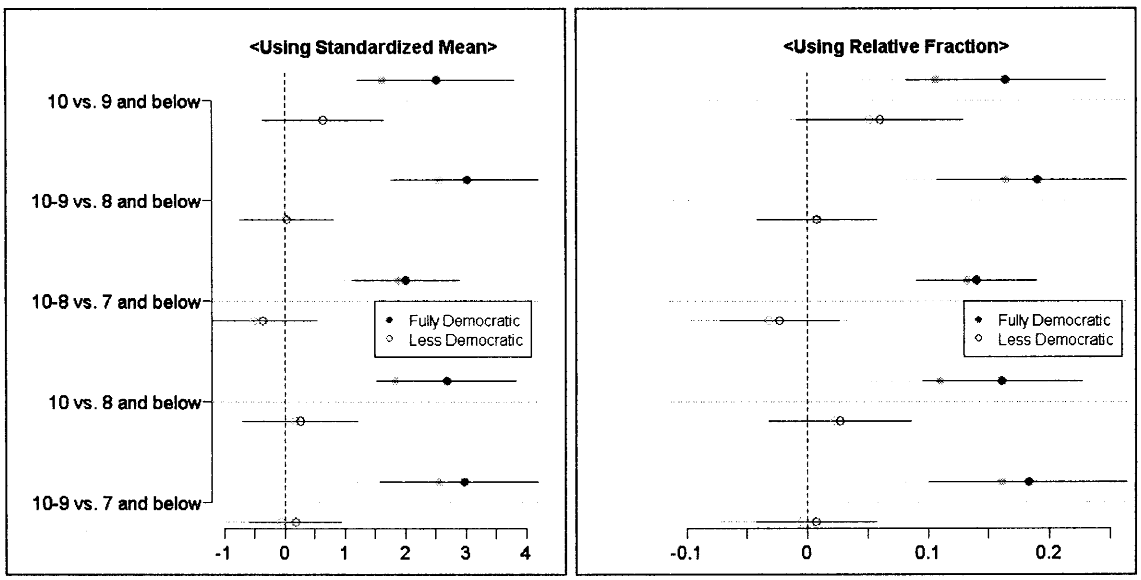
I first oriented the ideology and income inequality variables to have higher scores represent more leftist and pro-equality. For each country, I run the regression of the income inequality item on the ideological preference and other socioeconomic variables to control such as household income, unemployment status, age, sex, marital status, number of children, church attendance, and WVS wave fixed effects. It is important to control for such socioeconomic variables to be prevented from finding a spurious relationship. Table 7 reports the coefficient estimates for the leftist orientation for each country. Noticeably, 40 of the 44 coefficient estimates are positive and significant, suggesting that the ideological preferences do capture welfare and redistribution dimension in most countries.

In some countries, the ideological preferences may not represent the preferences with regard to welfare and redistribution. I re-estimate equation (1) after excluding these problematic countries. Doing so can be considered as eliminating noises, and thus should give clearer results if the level of democracy is important in determining the degree to which welfare policies reflect public preferences. We can systematically exclude the problematic countries based on the results in Table 7. I estimate equation (1) after, first, excluding the countries showing negative coefficient estimates for the leftist ideological preferences or its p-values greater than .1. I then additionally exclude the countries having p-values higher than .05. The results are reported in Tables 8 and 9, and Figure 8.

[21] See footnote 8.

The first and fourth columns in Table 8 display the results that are identical to those in the third and fourth columns in Table 2. The first and fourth columns in Table 9 correspond to the seventh and eighth columns in Table 2. The second and fifth columns in both tables report the coefficient estimates from the regression after excluding the countries having p-values greater than .1 in the coefficient estimates in Table 7. I further exclude the countries with p-values higher than .05 and the estimated coefficients are reported in the third and sixth columns.

Figure 8. Excluding Countries (p>.05) Where the Ideological Preferences May Not Capture the Dimension of Welfare and Redistribution



Note: The circles and lines light in color show the coefficient estimates and 90% confidence intervals without excluding any country. Those dark in color indicate the coefficients and confidence intervals estimated after excluding the countries having p-values greater than .05.

It is quite impressive to observe the changes in the coefficient estimates of main and interaction effects. Both effects are getting larger when we explore from the coefficient estimates in the first and fourth, to those in the second and fifth, to those in the third and sixth. By excluding more and more of the noisy observations, the results are getting clearer and clearer. Figure 8 markedly illustrates this. The circles and dashed lines light in color represent the coefficient estimates and 90% confidence intervals without excluding any country. Those dark in color indicate the coefficients and confidence intervals estimated after excluding the countries having p-values greater than .05. In all categorizations, such exclusion of noisy observations provides a refinement of the results.[22]

3.6.4 Fixing Measurement Error Problem

One concern remains to be addressed: the problem of measurement error. It is a well-known fact that the responses to individual survey items are plagued with large amounts of measurement error (Achen, 1975; Ansolabehere, Rodden, and Snyder, 2008; 2006). Because the independent variable of public ideological preferences comes from survey data, we need to pay attention to the possibility of mismeasurement that may seriously bias the coefficient estimates. I carefully attempt to fix this by employing Instrumental Variable (IV) estimators.

The most widely used IV estimator is Two-Stage Least Squares (2SLS). However, it is not suitable for the analysis due to the "problem of weak instruments" that is salient in finite sample data and multiplicative interaction models. Instead of 2SLS, I employed alternative IV estimators such as Fuller and Lewbel. The theoretical and empirical discussions on the problem of 2SLS and alternative IV estimators are of highly technical natures, and thus are presented in Appendix.

In brief, the use of the several IV estimators unanimously increases the coefficient estimates of main effects, indicating that the attenuation biases are mitigated, if not eliminated. More importantly, the interaction effects and marginal effects of the public ideological preferences among less democratic countries confirm that there is a significant difference between fully and less democratic countries even after fixing the downward bias. Essentially the same results are obtained no matter what estimator, what measure of the independent variable (standardized mean or relative fraction), and what data (one with WVS or one with combined surveys) are used. The main results are broadly robust after fixing the attenuation bias caused by potential measurement errors.

[22] I also conduct a similar process involving the exclusion of noisy country-year modules instead of countries. I first run the regression with individual data for each country-year module, and then use those results to exclude the problematic cases. This process gives essentially the same results and these are reported in Appendix.

3.7 Conclusions

The results in this chapter show that the public opinion, measured by the ideological disposition of the general public, explains why some countries spend more on welfare than others; politicians do reflect the ideological preferences of the citizens in fully democratic countries. This result is consistent with past findings (Brooks and Manza, 2006; Erikson, Wright, and McIver, 1994; 1987). Furthermore, this study finds a meaningful difference in the welfare spending policy responsiveness. The effect of public preferences on the size of welfare was conditioned by the quality of the democratic representation system; among less democratic countries, welfare spending policies have little been affected by the public preferences. This difference can be explained by the mechanism that traditional democratic representation theories offer; in a higher level of democracy, citizens can make politicians more accountable and politicians may need to please a wider group of people than in a lower level of democracy. Politicians may have incentives to please the general public in fully democratic countries, but they may not do so in less democratic ones as they are held less accountable, enjoy asymmetric information, and only need to provide policies that satisfy a smaller group of people.

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Appendix A: Measurement Error, Weak Instrument Problem, and Instrument Variable Estimation

I begin with the classical errors-in-variable (EIV) model. After partialling out the correctly measured variables, we can write the model as the following:

$$y = \beta x^* + v_1 \tag{3.3a}$$

$$x = x^* + v_2 \tag{3.3b}$$

where x^* is the unobserved true regressor and x is the observed measure of x^* with error v_2. Following the classical EIV assumptions, $E(v_1|x^*) = 0$, $E(v_2|x^*) = 0$, $E(v_1|x) = 0$, but $E(v_2|x) \neq 0$. The true regressor x^* is assumed to be uncorrelated with v_2, and thus x must be correlated with v_2. Because only x is available, the equation (3) can be rewritten as follows:

$$y = \beta x + v_1 - \beta v_2 \equiv \beta x + \epsilon \tag{3.4}$$

The problem is that $E(\epsilon|x)$ is not 0 as x is correlated with v_2. Estimating equation (4) using OLS will be biased due to the violation of the orthogonality condition. In this situation, the typical solution is the use of instrumental variables. The most widely used IV estimator is 2SLS, but it is not unbiased in finite sample data. Hahn and Hausman showed that the bias of 2SLS is

$$E(\hat{\beta}_{2SLS}) - \beta \approx \frac{k\rho(1 - R^2_{1st})}{nR^2_{1st}} \tag{3.5}$$

where k is the number of instruments, ρ is the measure of extent to which x is correlated with ϵ, R^2_{1st} is the partial R^2 from the first-stage regression of x on instruments z's, and n is the number of observations (Hahn and Hausman, 2003). From equation (5), one may notice that there is no problem in using 2SLS as long as n is large.[23] In contrast, 2SLS would be biased when n is small. Most of the cross-country analyses have finite samples, in either pure cross-section or panel format. When any non-orthogonality condition is suspicious in a finite

[23] In criticizing Angrist and Krueger (1991), Bound and his colleagues found that large k and low partial R^2 could still lead to huge biases even when sample size is sufficiently large (n = 329,509) (Bound, Jaeger, and Baker, 1995).

sample dataset, then it is not a good idea to use 2SLS unless there is a super instrument that gives a very high partial R^2 or F-statistics from the first-stage regression. In practice, however, when there are small numbers of problematic variables in a finite sample dataset, the partial R^2 is often low even when the coefficient estimates from the first-stage regression are very significant, i.e. p-values are smaller than 0.01. This "problem of weak instruments" is particularly troublesome in a multiplicative interaction model which is employed in this study. If a model includes an interaction term, then the first-stage regression of the interaction term usually yields low F-statistics. From the benchmark model of equation (1), we need to find relevant instruments to instrument not only P_{it}, but also $P_{it} \times D_{it}$ even though P_{it} is assumed to be the only mismeasured variable. A natural choice is to find a valid instrument W_{it} that is correlated with P_{it} and add $W_{it} \times D_{it}$ in the matrix of instruments $\mathbf{Z}_{it}$ (Wooldridge, 2001).[24] However, the correlation between W_{it} and P_{it} does not guarantee a full identification of interaction term, and often gives low F-statistics in practice. A conventional choice to increase R^2 or F is to add more instruments, but then that increases k in equation (5) so that the bias of 2SLS would not be improved in a finite sample dataset. To solve the weak instrument problem discussed so far, we need to find a small number of instruments that give high partial R^2 from the first-stage regressions of the main and of the interaction term as well. However, it is unrealistically difficult to find such instruments. Therefore, instead of the typical 2SLS, this study employs alternative estimators to mitigate, if not eliminate, the bias from measurement error.

Lewbel's Estimator

Lewbel's IV estimator exploits properties of third-moments of the data at hand (Lewbel, 1997). To briefly introduce this estimator, let us slightly modify the equation (3) by the following expression,

$$y = \beta x^* + \gamma \xi + v_1 \tag{3.6a}$$

$$x = x^* + v_2 \tag{3.6b}$$

[24] $W_{it} \times D_{it}$ in the matrix $\mathbf{Z}_{it}$ prevents "forbidden regression."

In equation (6), all variables are partialled out except x^* and a perfectly measured variable ξ. Lewbel's estimator can be obtained by

$$\hat{\beta}_L = (x'\mathbf{P}_z x)^{-1} x'\mathbf{P}_z y \qquad \text{where } \mathbf{P}_z = \mathbf{Z}(\mathbf{Z}'\mathbf{Z})^{-1}\mathbf{Z}' \tag{3.7}$$

Let $C_{i\cdot} = C_i - \bar{C}$ for a variable C where $\bar{C}$ denotes its sample mean. $\mathbf{Z}$ is a matrix of instruments consisting of $Z_{1i} = f(\xi_{i\cdot})$ for any given function of f which is not linear in ξ, $Z_{2i} = x_{i\cdot} f(\xi_{i\cdot})$, $Z_{3i} = y_{i\cdot} f(\xi_{i\cdot})$, and $Z_{4i} = x_{i\cdot} y_{i\cdot}$. Note that all $Z_i's$ can be expressed as the following. For any $k = 1, \cdots, 4$,

$$Z_{ki} = f(\xi_{i\cdot})^{I_\xi} x_{i\cdot}^{I_x} y_{i\cdot}^{I_y} \tag{3.8a}$$

$$= f(\xi_{i\cdot})^{I_\xi} (x_{i\cdot}^* + v_{2i})^{I_x} (\beta x_{i\cdot}^* + \gamma \xi_{i\cdot} + v_{1i})^{I_y} \tag{3.8b}$$

where $I_c = 1$ or 0 for a variable C. The classical EIV assumptions along with nondifferential measurement error suffice for the validity of $\mathbf{Z}$, i.e. $E(Z_{ki}v_{1i}) = E(Z_{ki}v_{2i}) = 0 \; \forall \; k = 1, \cdots, 4$. For the identification, $E(\mathbf{Z}'Q_\xi x) \neq 0$ where $Q_\xi = I - \xi(\xi'\xi)^{-1}\xi'$, assuming that $E[f(\xi_{i\cdot})Q_\xi x_i^*] \neq 0$ if the matrix $\mathbf{Z}$ has Z_1, or that $E[f(\xi_{i\cdot})Q_\xi x_i^* x_{i\cdot}^*] \neq 0$ if $\mathbf{Z}$ has Z_2, or that $\beta E[f(\xi_{i\cdot})Q_\xi x_i^*] + \gamma E[f(\xi_{i\cdot})\xi_{i\cdot} Q_\xi x_i^*] \neq 0$ if the matrix $\mathbf{Z}$ has Z_3, or that $\beta E[(x_{i\cdot}^*)^2 Q_\xi x_i^*] + \gamma E[x_{i\cdot}^* \xi_{i\cdot} Q_\xi x_i^*] \neq 0$ if Z_4 is included. Note that the identification condition relies on the third moments of the joint distribution of $(f(\xi), \xi, x^*)$. This means that the identification condition would fail if data followed the multivariate normal distribution where all third moments disappear. We need a skewed distribution, and it is not a strong requirement in political science as there are few variables that are perfectly normally distributed with 0 skewness. This identification condition is, in fact, testable using F-statistics or partial R^2 from the first-stage regression. If F or R^2 is low, then we fall into the weak instrument problem again. Using the identification test statistics and following the procedure that Lewbel himself uses, the Lewbel's instrument $\mathbf{Z}_L$ in this study includes a constant, all regressors, demeaned welfare spending times demeaned preferences, and demeaned trade times demeaned preferences. I utilize the *Trade as a % of GDP* as a perfectly measured variable since it is relatively easy to record without errors.

Fuller's Estimator

As discussed above, 2SLS would not be a desirable solution in this study. However, recent development of methodological techniques enables us to lessen the weak instrument problem. Hahn, Hausman, and Kuersteiner suggested using either a Jackknifed 2SLS or Fuller estimator (Hahn, Hausman, and Kuersteiner, 2004). These estimators basically minimize the possible finite sample bias while keeping the principles of instrumental variable approaches.[25] This study employed the Fuller estimator. From equations (3) and (4), the Fuller estimator can be obtained by the following:

$$\hat{\beta}_{F(\alpha)} = [x'\mathbf{P}_z x - (\lambda^* - \frac{\alpha}{n-k})x'\mathbf{Q}_z x]^{-1}[x'\mathbf{P}_z y - (\lambda^* - \frac{\alpha}{n-k})x'\mathbf{Q}_z y] \tag{3.9}$$

where, $\mathbf{P_z}= \mathbf{Z}(\mathbf{Z}'\mathbf{Z})^{-1}\mathbf{Z}'$, $\mathbf{Q_z}= \mathbf{I} - \mathbf{P_z}$, λ^* is the smallest root that satisfies $|\mathbf{M}'\mathbf{P}_z\mathbf{M} - \lambda\mathbf{M}'\mathbf{Q}_z\mathbf{M}| = 0, \mathbf{M} \equiv [y \quad x]$, and $\alpha > 0$ is a parameter to be chosen by researcher (Fuller, 1977). Fuller himself suggested choosing $\alpha = 1$ if one wishes to obtain an unbiased estimator. $\alpha = 4$ should be used if one wants to minimize mean squared error, and several other researchers advocated choosing the same value of α (e.g. Rothenberg, 1983). I chose both $\alpha = 1$ and $\alpha = 4$ in this study. For the instruments in the matrix $\mathbf{Z}$, I followed Wooldridge's suggestion that one should use either different measures of the mismeasured variable or different variables that are correlated with the mismeasured variable (Wooldridge, 2001). The mismeasurement of instruments does not matter as long as the measurement errors in instruments are not correlated with that in political orientation variable and with model error. I chose two WVS items that are correlated with the independent variable of public ideological orientation. First, I chose a 10-point scale on whether competition is good or harmful, ranging from 1 ("Competition is good. It stimulates people to work hard and develop new ideas") and 10 ("Competition is harmful. It brings the worst in people"). From this item, a "social perception on competition" can be measured by computing a standardized mean or relative fraction. Second, I chose an item asking whether owners, the state, or employees should run the business.[26] From this

[25]There have been many other alternative IV estimators proposed to fix weak instrument problem such as GMM, LIML, JIVE, and Nagar-type estimators. However, Hahn and his colleagues showed that those estimators do not have finite moments, and therefore suffer from small sample variabilities (Hahn, Hausman, and Kuersteiner, 2004).

[26]Respondents were asked to choose one of the four statements below that comes the closest to their opinion. 1 "Owners should run their business," 2 "Owners/Employees participate in selection of managers" 3 "The State

item, I computed the fraction of people who think employees should own and run the business. The chosen instrumental variables are clearly correlated with the political orientation; conservatives (rightists) are more likely to favor competition and have distastes for the business under the thumbs of employees/labors. The question is, though, whether the measurement errors in the chosen instruments are uncorrelated with that in the political orientation variable. If the sources of measurement error are some factors that each individual survey items differently pose, for example, respondents' varying levels of understanding different questions, vague question wordings of some items, and/or vague response categories, then we may assume that the measurement errors in the chosen WVS items are relatively not correlated with that in the ideological preference item. However, if measurement errors arose from sampling errors or some mood factors (for example, bad weather that hit some respondents throughout the survey), then the required lack of correlations may not be assumed. We can test whether the measurement errors in instruments are correlated with that in the political orientation variable by utilizing internal instruments in addition to the external instruments. Specifically, Lewbel's instruments can be added to the two WVS items. Given that Lewbel's instruments $\mathbf{Z}_L$ satisfy $E(\mathbf{Z}'_L v_1) = E(\mathbf{Z}'_L v_2) = 0$ in equations (3), (4), and (6), we can assess this (lack) of correlations by the tests of over-identification restriction. This study used Hansen's J-statistics.

Estimation Results

The various IV estimation results are now presented. In Tables A1 and A2, odd columns report the estimated coefficients for the specification without partisan control variables. The coefficients estimated from the regressions including partisan controls are presented in even columns. All regressions include region and time fixed effects. The title of each table indicates which dataset is used (i.e., data with WVS in Table A1 and data with combined surveys in Table A2), which measure of independent variable is used (i.e., standardized mean or relative fraction), and which categorization is employed. In Tables for estimation utilizing internal information (A1-1, A1-2, A1-3, A1-4, A2-1 and A2-2), I report the estimates obtained by OLS and Lewbel. Because I slightly modified Lewbel's method by additionally employing Fuller's,

should be the owner," and 4 "Employees should own the business and elect managers."

I report the results using both $\alpha = 1$ (unbiased Fuller) and $\alpha = 4$ (Fuller minimizing MSE).[27] In Tables for estimation utilizing external information (A1-5, A1-6, A1-7, and A1-8), the estimated coefficients are reported employing OLS, Fuller estimator using the two WVS items as instruments, and Fuller estimator using the two WVS items and Lewbel's instruments. I also report the partial R^2 and F-statistics from the first-stage regressions of the main and of the interaction term. When internal information is utilized, these statistics are all sufficiently large and significant, indicating identification conditions are satisfied. When external information is utilized as shown in Tables A1-5 to A1-8, however, the F-statistics are not sufficiently high in the third, fourth, fifth, and sixth columns. This may indicate the situation of weak instruments, and thus the choice of Fuller is appropriate as a "weak-instrument-robust" estimator. Finally, I test whether the measurement errors in the instruments are (un)correlated with those in the independent variables by using Hansen J-statistics reported in the last four columns in Tables A1-5 to Al-8. Relying on the theoretical properties that the Lewbel's instruments are valid, the Hansen J-statistics test the null hypothesis of the required lack of correlations between the measurement errors in instruments and those in mismeasured variables. All these statistics are small enough not to reject the null hypothesis, and thus we can reasonably assume that the WVS items may have measurement errors (or negligible mismeasurement) that are not correlated with those in the ideological preference measure.

The use of several IV estimators unanimously increases the coefficient estimates of main effects, indicating that the attenuation biases are mitigated, if not eliminated. More importantly, the interaction effects and marginal effects of the public ideological preferences among less democratic countries confirm that there is a significant difference between fully and less democratic countries even after fixing the downward bias. From the estimated coefficients in the last column in Table A1-1, for example, a country whose people have more liberal preferences by 1 standard deviation in a given year spend 3.848% more on welfare than other countries (higher than OLS estimate of 2.552%). However, such effect of public leftist orientation does not seem to exist among less democratic countries as the interaction effect and marginal effect indicate. No matter what estimator, what measure as the independent variable,

[27] The identification of Lewbel's estimator depends on the third moments of the data, and the Fuller estimator guarantees the existence of finite moments and thus has good finite sample properties. Therefore, I modified Lewbel's estimator by combining Lewbel's instruments given in equation (8) with Fuller's modification in equation (9).

and what data are used, the general picture is the same. Another example would be that, from the last column in Table A1-7 where relative fraction is used as a measure of public ideological preferences and external information is utilized for instruments, the expenditure on welfare is about 2.58% higher in a fully democratic country where there is a 10% more relative fraction of liberal population (higher than OLS estimate of 1.64%). Furthermore, there is a significant difference between fully and less democratic countries in the degree to which welfare spending policies reflect public ideological preferences as the interaction effect estimate of -0.249 and the marginal effect estimate of 0.009 indicate. The main results of this study are broadly robust after fixing the attenuation bias from potential measurement errors.

Appendix B: List of Countries and Mean Values of Public Ideological Preferences, Total Welfare Spending as a % of GDP, and Democracy Score

In Tables A3 and A4, countries are listed along with the mean values of public ideological preferences, total welfare spending as a % of GDP, and democracy score.

Appendix C: Separating Out the Income Effects by Sample Restriction

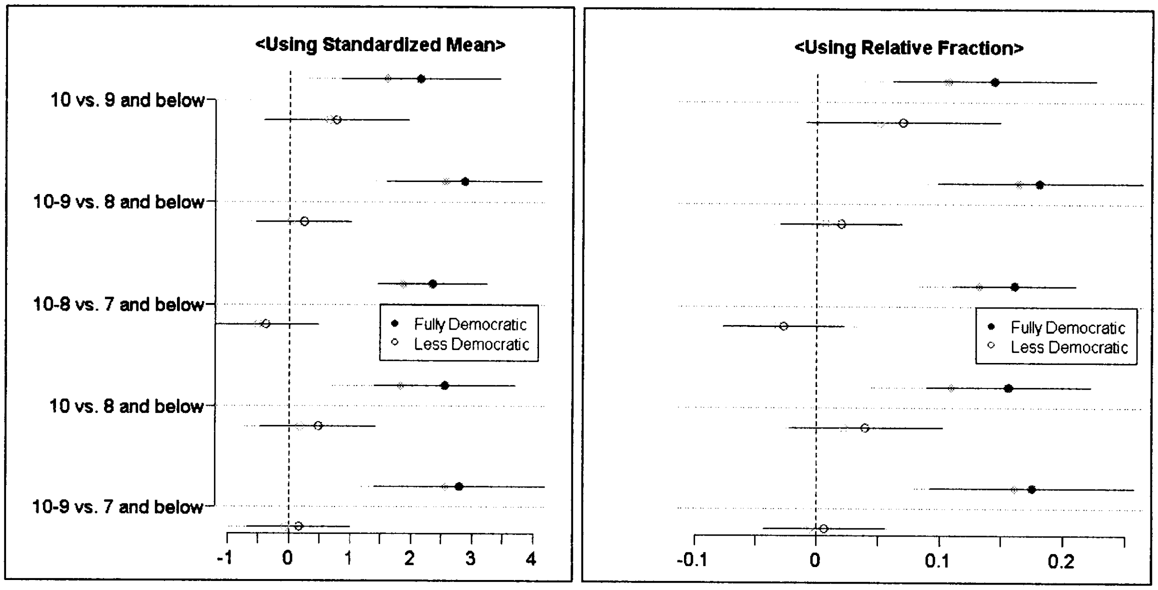
Two methods are employed to separate out the income effects from the conditional effects of democracy. Here in this Appendix, the results using the second method are reported: estimating equation (1) after restricting samples. I excluded the countries whose log of per capita GDP is higher than 9.6, which is the threshold chosen from the Min-Max range displayed in Table 4. Tables A5 and A6 display coefficient and marginal effect estimates analogous to those seen in Tables 2 and 3, but now which exclude the high-income countries from the estimation. Note that sample size in all panels diminishes, and therefore the variances of estimators are large. Although the statistical significances are weaker than for the estimators shown in Tables 2 and 3, all main, interaction, and marginal effects indicate essentially the same relationship between the public leftist orientation, size of welfare, and the level of democracy. The regression results shown in Tables A5 and A6 may add some credibility to the results reported in Tables 5 and 6.

Appendix D: Excluding Country-Year Modules Where the Ideological Preferences May Not Capture the Dimension of Welfare and Redistribution

For each WVS country-year module, I first run the regression of income inequality item on the ideological preference variable and other socio-economic variables to controls such as household income, sex, age, unemployment, marital status, number of children, and church attendance. The coefficient estimates for the political orientation are reported in Table A7. Note that some country-year modules do not contain income inequality or control variable items, and thus do not appear in Table A7 as we cannot estimate coefficients. These are DENMARK (1999), HUNGARY (1998, 1999), SOUTH KOREA (1990), LATVIA (1999), SLOVAK REPUBLIC (1999), SLOVENIA (1995), and SWEDEN (1990, 1999).

The regression results reported in Tables A8 and A9 and Figure A1 are parallel to those in Tables 8 and 9 and Figure 8, respectively, excluding the noisy country-year modules based on the results in Table A7. The consequences of this process are essentially the same; excluding noisy observations clarifies the relationship between the public ideological preferences, size of welfare spending, and the level of democracy.

Figure A1. Excluding Country-Year Modules (p>.05) Where the Ideological Preferences May Not Capture the Dimension of Welfare and Redistribution



Note: The circles and lines light in color show the coefficient estimates and 90% confidence intervals without excluding any country. Those dark in color indicate the coefficients and confidence intervals estimated after excluding the country-year modules having p-values greater than .05.

Table 1. Democracy Scores and Frequencies

A. Dataset with WVS

Democracy score	0	2	4	5	6	7	8	9	10	Total
Frequencies	4	1	2	2	2	9	17	9	34	80

B. Dataset with Combined Surveys

Democracy score	0	2	4	5	6	7	8	9	10	Total
Frequencies	6	1	2	5	3	13	25	17	108	180

Note: In the dataset, a few countries were observed with varied democracy scores as they have experienced a change in the quality of democracy. In the dataset with WVS, these are Argentina (7 to 8), Chile (8 to 9), South Korea (7 to 8), Moldova (7 to 8), Romania (5 to 8), Mexico (2 to 4 to 6 to 8), and India (8 to 9). In addition to these countries, Bulgaria (8 to 9) and Russia (5 to 7) are also found to have varied democracy scores in the dataset with combined surveys.

Table 2-1. Ideological Preferences, Democracy, and Welfare Policy Generosity - Data with WVS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(1) 10-9 vs. 8 and below								
Leftist Orientation:	2.730**	2.853***	2.504***	2.552***				
Standardized Mean	[1.017]	[0.924]	[0.808]	[0.770]				
Leftist Orientation	-2.718**	-2.717**	-2.575***	-2.541***				
×Less Democratic	[1.127]	[1.015]	[0.944]	[0.861]				
Leftist Orientation:					0.168**	0.177***	0.160***	0.164***
Relative Fraction					[0.070]	[0.063]	[0.056]	[0.053]
Leftist Orientation					-0.162**	-0.162**	-0.158**	-0.157**
×Less Democratic					[0.078]	[0.071]	[0.067]	[0.061]
Less Democratic	-4.034***	-3.239***	-4.335***	-3.329***	-4.650***	-3.726***	-4.928***	-3.846***
	[1.321]	[1.133]	[1.210]	[1.080]	[1.475]	[1.256]	[1.301]	[1.156]
Observations	73	73	73	73	73	73	73	73
R^2	0.771	0.813	0.796	0.830	0.769	0.813	0.797	0.832
dy/dx - Less Dem	0.012	0.137	-0.071	0.011	0.006	0.014	0.002	0.007
	[0.514]	[0.483]	[0.512]	[0.481]	[0.035]	[0.032]	[0.035]	[0.032]
Partisan Controls	No	Yes	No	Yes	No	Yes	No	Yes
Regional FE	No	No	Yes	Yes	No	No	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Robust and country-clustered standard errors in brackets

*** p<0.01, ** p<0.05, * p<0.1

Table 2-2. Ideological Preferences, Democracy, and Welfare Policy Generosity - Data with WVS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(2) 10-9 vs. 7 and below								
Leftist Orientation:	2.694**	2.805***	2.428**	2.559***				
Standardized Mean	[1.131]	[0.999]	[0.928]	[0.859]				
Leftist Orientation	-2.670**	-2.713**	-2.578**	-2.617***				
×Less Democratic	[1.161]	[1.034]	[1.002]	[0.919]				
Leftist Orientation:					0.162**	0.170**	0.152**	0.161***
Relative Fraction					[0.075]	[0.067]	[0.062]	[0.058]
Leftist Orientation					-0.162*	-0.165**	-0.165**	-0.167**
×Less Democratic					[0.080]	[0.071]	[0.073]	[0.068]
Less Democratic	-5.486***	-4.620***	-5.212***	-4.431***	-6.009***	-5.054***	-5.776***	-4.938***
	[1.834]	[1.544]	[1.732]	[1.551]	[1.996]	[1.694]	[1.882]	[1.692]
Observations	57	57	57	57	57	57	57	57
R^2	0.803	0.848	0.826	0.862	0.799	0.846	0.826	0.863
dy/dx - Less Dem	0.024	0.092	-0.149	-0.058	0.000	0.005	-0.012	-0.006
	[0.583]	[0.472]	[0.641]	[0.569]	[0.041]	[0.034]	[0.046]	[0.041]
Partisan Controls	No	Yes	No	Yes	No	Yes	No	Yes
Regional FE	No	No	Yes	Yes	No	No	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Robust and country-clustered standard errors in brackets

*** p<0.01, ** p<0.05, * p<0.1

Table 3. Public Ideological Preferences, Democracy, and Welfare Policy Generosity - Data with Combined Surveys

	(1)	(2)	(3)	(4)
(1) 10-9 vs. 8 and below				
Leftist Orientation:	2.139**	1.899**	2.064**	1.813**
Standardized Mean	[0.890]	[0.816]	[0.777]	[0.680]
Leftist Orientation	-2.349**	-2.042**	-2.197**	-1.879**
×Less Democratic	[1.038]	[0.963]	[0.922]	[0.818]
Less Democratic	-5.418***	-4.365***	-5.809***	-4.715***
	[1.290]	[1.337]	[1.149]	[1.240]
Observations	166	166	166	166
R^2	0.746	0.767	0.770	0.792
dy/dx - Less Dem	-0.209	-0.143	-0.133	-0.065
se	[0.451]	[0.474]	[0.423]	[0.423]
(2) 10-9 vs. 7 and below				
Leftist Orientation:	2.158**	1.920**	2.085**	1.870***
Standardized Mean	[0.900]	[0.816]	[0.792]	[0.676]
Leftist Orientation	-2.238**	-1.831*	-2.189**	-1.782**
×Less Democratic	[1.057]	[1.033]	[0.930]	[0.860]
Less Democratic	-6.422***	-5.391***	-6.364***	-5.363***
	[1.699]	[1.630]	[1.550]	[1.525]
Observations	144	144	144	144
R^2	0.732	0.756	0.757	0.782
dy/dx - Less Dem	-0.079	0.089	-0.104	0.087
se	[0.515]	[0.528]	[0.579]	[0.574]
Partisan Controls	No	Yes	No	Yes
Regional FE	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Robust and country-clustered standard errors in brackets

*** p<0.01, ** p<0.05, * p<0.1

Table 4. (log) GDP per Capita and the Level of Democracy

A. Data with WVS

	Mean	Std. Dev.	Min	Max	N
(1) 10 vs. 9 and below					
10	9.817094	.3859531	9.023938	10.41761	34
9 and below	8.776289	.5596902	7.548747	9.968466	45
(2) 10-9 vs. 8 and below					
10-9	9.629532	.5762337	7.652761	10.41761	43
8 and below	8.74012	.5406917	7.548747	9.690308	36
(3) 10-8 vs. 7 and below					
10-8	9.398825	.6752004	7.548747	10.41761	59
7 and below	8.709176	.5681285	7.625332	9.557682	20
(4) 10 vs. 8 and below					
10	9.817094	.3859531	9.023938	10.41761	34
8 and below	8.74012	.5406917	7.548747	9.690308	36
(5) 10-9 vs. 7 and below					
10-9	9.629532	.5762337	7.652761	10.41761	43
7 and below	8.709176	.5681285	7.625332	9.557682	20

B. Data with Combined Surveys

	Mean	Std. Dev.	Min	Max	N
(1) 10 vs. 9 and below					
10	9.868714	.3075268	9.023938	10.50231	108
9 and below	8.936377	.5779343	7.548747	9.983765	69
(2) 10-9 vs. 8 and below					
10-9	9.784652	.4275788	7.652761	10.50231	125
8 and below	8.833646	.5153675	7.548747	9.821395	52
(3) 10-8 vs. 7 and below					
10-8	9.648309	.5447678	7.548747	10.50231	148
7 and below	8.775223	.5112715	7.625332	9.557682	29
(4) 10 vs. 8 and below					
10	9.868714	.3075268	9.023938	10.50231	108
8 and below	8.833646	.5153675	7.548747	9.821395	52
(5) 10-9 vs. 7 and below					
10-9	9.784652	.4275788	7.652761	10.50231	125
7 and below	8.775223	.5112715	7.625332	9.557682	29

Table 5-1. Partialling Out the Income Effects - Data with WVS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(1) 10-9 vs. 8 and below								
Leftist Orientation:	2.733**	2.856***	2.503***	2.545***				
Standardized Mean	[1.012]	[0.913]	[0.821]	[0.778]				
Leftist Orientation	-2.818***	-2.753***	-2.483***	-2.389***				
×Less Democratic	[0.993]	[0.884]	[0.873]	[0.802]				
Leftist Orientation:					0.170**	0.178***	0.157***	0.161***
Relative Fraction					[0.067]	[0.061]	[0.055]	[0.052]
Leftist Orientation					-0.172**	-0.167**	-0.141**	-0.137**
×Less Democratic					[0.072]	[0.065]	[0.066]	[0.061]
Less Democratic	-4.082***	-3.263**	-4.274***	-3.216**	-4.761***	-3.785***	-4.705***	-3.559**
	[1.367]	[1.217]	[1.297]	[1.233]	[1.551]	[1.365]	[1.431]	[1.376]
Observations	73	73	73	73	73	73	73	73
R^2	0.771	0.813	0.796	0.830	0.769	0.813	0.797	0.833
dy/dx - Less Dem	-0.085	0.103	0.020	0.155	-0.002	0.011	0.016	0.023
se	[0.657]	[0.588]	[0.658]	[0.601]	[0.048]	[0.042]	[0.047]	[0.044]
Partisan Controls	No	Yes	No	Yes	No	Yes	No	Yes
Regional FE	No	No	Yes	Yes	No	No	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Robust and country-clustered standard errors in brackets

*** p<0.01, ** p<0.05, * p<0.1

Table 5-2. Partialling Out the Income Effects - Data with WVS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(2) 10-9 vs. 7 and below								
Leftist Orientation:	2.697**	2.808***	2.423**	2.552***				
Standardized Mean	[1.132]	[0.995]	[0.945]	[0.878]				
Leftist Orientation	-2.727**	-2.742***	-2.400**	-2.450**				
×Less Democratic	[1.146]	[0.973]	[1.006]	[0.911]				
Leftist Orientation:					0.164**	0.172**	0.149**	0.158**
Relative Fraction					[0.074]	[0.066]	[0.061]	[0.058]
Leftist Orientation					-0.173*	-0.173**	-0.143*	-0.147*
×Less Democratic					[0.086]	[0.074]	[0.079]	[0.073]
Less Democratic	-5.496***	-4.628***	-5.156***	-4.376**	-6.097***	-5.121***	-5.568***	-4.751**
	[1.841]	[1.566]	[1.749]	[1.608]	[1.999]	[1.730]	[1.896]	[1.785]
Observations	57	57	57	57	57	57	57	57
R^2	0.803	0.848	0.826	0.862	0.800	0.846	0.826	0.863
dy/dx - Less Dem	-0.030	0.066	0.023	0.101	-0.009	-0.001	0.006	0.011
se	[0.838]	[0.639]	[0.819]	[0.657]	[0.063]	[0.051]	[0.061]	[0.053]
Partisan Controls	No	Yes	No	Yes	No	Yes	No	Yes
Regional FE	No	No	Yes	Yes	No	No	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Robust and country-clustered standard errors in brackets

*** p<0.01, ** p<0.05, * p<0.1

Table 6. Partialling Out the Income Effects - Data with Combined Surveys

	(1)	(2)	(3)	(4)
(1) 10-9 vs. 8 and below				
Leftist Orientation:	2.122**	1.883**	2.054**	1.803**
Standardized Mean	[0.911]	[0.829]	[0.807]	[0.703]
Leftist Orientation	-2.651***	-2.352***	-2.321***	-1.993**
×Less Democratic	[0.897]	[0.792]	[0.826]	[0.778]
Less Democratic	-5.547***	-4.504***	-5.885***	-4.783***
	[1.308]	[1.401]	[1.152]	[1.353]
Observations	166	166	166	166
R^2	0.747	0.767	0.770	0.792
dy/dx - Less Dem	-0.529	-0.469	-0.267	-0.191
se	[0.828]	[0.750]	[0.742]	[0.684]
(2) 10-9 vs. 7 and below				
Leftist Orientation:	2.156**	1.906**	2.111**	1.893**
Standardized Mean	[0.927]	[0.843]	[0.817]	[0.707]
Leftist Orientation	-2.285**	-2.026**	-1.861*	-1.616
×Less Democratic	[1.079]	[0.992]	[1.080]	[1.030]
Less Democratic	-6.445***	-5.481***	-6.149***	-5.268***
	[1.760]	[1.665]	[1.613]	[1.623]
Observations	144	144	144	144
R^2	0.732	0.756	0.757	0.782
dy/dx - Less Dem	-0.129	-0.120	0.250	0.276
se	[1.074]	[0.943]	[1.091]	[0.984]
Partisan Controls	No	Yes	No	Yes
Regional FE	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Robust and country-clustered standard errors in brackets

*** p<0.01, ** p<0.05, * p<0.1

Table 7. Ideological Preferences and Support for More Income Equality in 44 Countries - Individual Country Estimates

Country	Coeffcient Estimates	Robust S.E.	Observations
ALBANIA	0.229***	0.038	871
ARGENTINA	0.075*	0.043	1806
AUSTRALIA	0.340***	0.035	1605
AUSTRIA	0.160**	0.068	1211
BANGLADESH	0.273***	0.034	1109
BELARUS	0.164***	0.037	1627
BRAZIL	0.097***	0.031	1876
BULGARIA	0.223***	0.029	2067
CANADA	0.200***	0.034	2734
CHILE	0.170***	0.029	2639
CROATIA	0.062	0.054	1025
CZECH REPUBLIC	0.311***	0.027	2400
DENMARK	0.528***	0.042	812
DOMINICAN REPUBLIC	0.057	0.062	315
ESTONIA	0.078*	0.045	1327
FINLAND	0.443***	0.035	1302
FRANCE	0.466***	0.053	676
GERMANY	0.266***	0.036	2842
HUNGARY	0.205***	0.064	688
INDIA	0.174***	0.023	2131
INDONESIA	0.190***	0.049	725
IRAN	-0.052	0.036	1070
IRELAND	0.216***	0.058	805
KOREA, REPUBLIC OF	0.071**	0.031	2354
LATVIA	0.243***	0.052	892
LITHUANIA	0.196***	0.043	1257
MEXICO	0.108***	0.024	3185
MOLDOVA	0.226***	0.032	1373
NETHERLANDS	0.428***	0.041	728
NORWAY	0.432***	0.027	1980
PAKISTAN	0.183*	0.097	197
POLAND	0.141***	0.035	1643
ROMANIA	0.354***	0.029	2397
RUSSIA	0.153***	0.036	1485
SLOVAK REPUBLIC	0.182***	0.053	831
SLOVENIA	-0.047	0.072	479
SPAIN	0.299***	0.029	2843
SWEDEN	0.435***	0.031	885
SWITZERLAND	0.241***	0.065	740
TURKEY	0.138***	0.048	1763
UK	0.368***	0.041	1527
UKRAINE	0.090*	0.052	666
URUGUAY	0.225***	0.065	838
US	0.231***	0.028	3665

All regressions include household income, unemployment status, sex, age, marital status, number of hildren, church attendance, and WVS wave fixed effects.

Table 8. Excluding Countries Where the Ideological Preferences May Not Capture the Dimension of Welfare and Redistribution - Standardized Mean

	(1)	(2)	(3)	(4)	(5)	(6)
(1) 10-9 vs. 8 and below						
Leftist Orientation:	2.504***	2.700***	2.979***	2.552***	2.687***	3.027***
Standardized Mean	[0.808]	[0.844]	[0.873]	[0.770]	[0.768]	[0.806]
Leftist Orientation	-2.575***	-2.926***	-3.057***	-2.541***	-2.806***	-2.993***
×Less Democratic	[0.944]	[0.975]	[1.006]	[0.861]	[0.871]	[0.946]
Less Democratic	-4.335***	-4.735***	-4.736***	-3.329***	-3.683***	-3.710***
	[1.210]	[1.168]	[1.116]	[1.080]	[1.015]	[0.819]
Observations	73	69	63	73	69	63
R^2	0.796	0.788	0.808	0.830	0.826	0.852
dy/dx - Less Dem	-0.071	-0.225	-0.078	0.011	-0.119	0.034
se	[0.512]	[0.543]	[0.539]	[0.481]	[0.496]	[0.471]
(2) 10-9 vs. 7 and below						
Leftist Orientation:	2.428**	2.604***	2.882***	2.559***	2.649***	2.978***
Standardized Mean	[0.928]	[0.942]	[0.987]	[0.859]	[0.862]	[0.934]
Leftist Orientation	-2.578**	-2.739***	-2.803**	-2.617***	-2.671***	-2.803**
×Less Democratic	[1.002]	[0.999]	[1.029]	[0.919]	[0.937]	[1.029]
Less Democratic	-5.212***	-5.885***	-5.746***	-4.431***	-5.051***	-4.955***
	[1.732]	[1.521]	[1.756]	[1.551]	[1.330]	[1.287]
Observations	57	54	49	57	54	49
R^2	0.826	0.830	0.842	0.862	0.870	0.887
dy/dx - Less Dem	-0.149	-0.135	0.078	-0.058	-0.022	0.175
se	[0.641]	[0.608]	[0.559]	[0.569]	[0.544]	[0.460]
Excluding	None	p>.1	p>.05	None	p>.1	p>.05
Partisan Controls	No	No	No	Yes	Yes	Yes
Regional FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Robust and country-clustered standard errors in brackets
*** p<0.01, ** p<0.05, * p<0.1

Table 9. Excluding Countries Where the Ideological Preferences May Not Capture the Dimension of Welfare and Redistribution - Relative Fraction

	(1)	(2)	(3)	(4)	(5)	(6)
(1) 10-9 vs. 8 and below						
Leftist Orientation:	0.160***	0.170***	0.186***	0.164***	0.171***	0.190***
Relative Fraction	[0.056]	[0.058]	[0.059]	[0.053]	[0.053]	[0.055]
Leftist Orientation	-0.158**	-0.180**	-0.185**	-0.157**	-0.175***	-0.182**
×Less Democratic	[0.067]	[0.069]	[0.071]	[0.061]	[0.062]	[0.067]
Less Democratic	-4.928***	-5.422***	-5.402***	-3.846***	-4.272***	-4.283***
	[1.301]	[1.277]	[1.234]	[1.156]	[1.101]	[0.916]
Observations	73	69	63	73	69	63
R^2	0.797	0.788	0.807	0.832	0.828	0.854
dy/dx - Less Dem	0.002	-0.011	0.000	0.007	-0.004	0.008
se	[0.035]	[0.037]	[0.036]	[0.032]	[0.033]	[0.031]
(5) 10-9 vs. 7 and below						
Leftist Orientation:	0.152**	0.162**	0.177***	0.161***	0.166***	0.183***
Relative Fraction	[0.062]	[0.062]	[0.063]	[0.058]	[0.058]	[0.061]
Leftist Orientation	-0.165**	-0.179**	-0.178**	-0.167**	-0.175**	-0.177**
×Less Democratic	[0.073]	[0.072]	[0.072]	[0.068]	[0.068]	[0.072]
Less Democratic	-5.776***	-6.493***	-6.273***	-4.938***	-5.587***	-5.439***
	[1.882]	[1.672]	[1.843]	[1.692]	[1.472]	[1.364]
Observations	57	54	49	57	54	49
R^2	0.826	0.830	0.841	0.863	0.872	0.887
dy/dx - Less Dem	-0.012	-0.017	-0.000	-0.006	-0.009	0.007
se	[0.046]	[0.044]	[0.041]	[0.041]	[0.040]	[0.035]
Excluding	None	p>.1	p>.05	None	p>.1	p>.05
Partisan Controls	No	No	No	Yes	Yes	Yes
Regional FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Robust and country-clustered standard errors in brackets

*** p<0.01, ** p<0.05, * p<0.1

Table A1-1 . IV Utilizing Internal Information - Data with WVS: **Standardized Mean and 10-9 vs. 8 and Others**

VARIABLES	OLS	OLS	Lewbel (Fuller1)	Lewbel (Fuller1)	Lewbel (Fuller4)	Lewbel (Fuller4)
Leftist Orientation:	2.504***	2.552***	4.834*	4.233	4.332*	3.848*
Standardized Mean	[0.808]	[0.770]	[2.712]	[2.607]	[2.183]	[2.111]
Leftist Orientation	-2.575***	-2.541***	-4.640*	-3.915	-4.175*	-3.580*
$\times$Less Democratic	[0.944]	[0.861]	[2.740]	[2.490]	[2.248]	[2.060]
Less Democratic	-4.335***	-3.329***	-4.523***	-3.802***	-4.485***	-3.698***
	[1.210]	[1.080]	[1.230]	[1.374]	[1.182]	[1.257]
Observations	73	73	73	73	73	73
R^2	0.796	0.830	0.762	0.814	0.775	0.821
Partisan Controls	No	Yes	No	Yes	No	Yes
Regional FE	Yes	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes	Yes
dy/dx - Less Dem	-0.071	0.011	0.194	0.318	0.158	0.267
se	[0.512]	[0.481]	[0.540]	[0.543]	[0.536]	[0.526]
Partial R^2- Main			0.556	0.538	0.556	0.538
Partial R^2- Interaction			0.844	0.841	0.844	0.841
F-stat - Main			36.059	24.089	36.059	24.089
p > F - Main			0.000	0.000	0.000	0.000
F-stat - Interaction			87.983	82.365	87.983	82.365
p > F - Interaction			0.000	0.000	0.000	0.000
Hansen J-stat			1.197	0.655	1.224	0.673
p-val			0.550	0.721	0.542	0.714

Robust and country-clustered standard errors in brackets.

*** p<0.01, ** p<0.05, * p<0.1

Table A1-2. IV Utilizing Internal Information - Data with WVS: **Standardized Mean and 10-9 vs. 7 and Others**

VARIABLES	OLS	OLS	Lewbel (Fuller1)	Lewbel (Fuller1)	Lewbel (Fuller4)	Lewbel (Fuller4)
Leftist Orientation:	2.428**	2.559***	4.282*	3.969	3.846*	3.596*
Standardized Mean	[0.928]	[0.859]	[2.309]	[2.378]	[1.896]	[1.902]
Leftist Orientation	-2.578**	-2.617***	-4.560*	-4.143*	-4.126**	-3.769*
$\times$Less Democratic	[1.002]	[0.919]	[2.286]	[2.337]	[1.905]	[1.893]
Less Democratic	-5.212***	-4.431***	-5.682***	-4.965**	-5.565***	-4.818***
	[1.732]	[1.551]	[1.898]	[1.877]	[1.829]	[1.759]
Observations	57	57	57	57	57	57
R^2	0.826	0.862	0.800	0.849	0.811	0.855
Partisan Controls	No	Yes	No	Yes	No	Yes
Regional FE	Yes	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes	Yes
dy/dx - Less Dem	-0.149	-0.058	-0.278	-0.174	-0.280	-0.173
se	[0.641]	[0.569]	[0.710]	[0.630]	[0.683]	[0.619]
Partial R^2- Main			0.534	0.536	0.534	0.536
Partial R^2- Interaction			0.885	0.885	0.885	0.885
F-stat - Main			9.469	11.067	9.469	11.067
p > F - Main			0.000	0.000	0.000	0.000
F-stat - Interaction			49.421	48.030	49.421	48.030
p > F - Interaction			0.000	0.000	0.000	0.000
Hansen J-stat			0.140	0.020	0.161	0.020
p-val			0.932	0.990	0.923	0.990

Robust and country-clustered standard errors in brackets.

*** p<0.01, ** p<0.05, * p<0.1

Table A1-3. IV Utilizing Internal Information - Data with WVS: **Relative Fraction and 10-9 vs. 8 and Others**

VARIABLES	OLS	OLS	Lewbel (Fuller1)	Lewbel (Fuller1)	Lewbel (Fuller4)	Lewbel (Fuller4)
Leftist Orientation:	0.160***	0.164***	0.266**	0.249*	0.249**	0.234*
Relative Fraction	[0.056]	[0.053]	[0.128]	[0.133]	[0.113]	[0.117]
Leftist Orientation	-0.158**	-0.157**	-0.256*	-0.232*	-0.240*	-0.219*
×Less Democratic	[0.067]	[0.061]	[0.136]	[0.134]	[0.122]	[0.120]
Less Democratic	-4.928***	-3.846***	-5.395***	-4.416***	-5.318***	-4.315***
	[1.301]	[1.156]	[1.471]	[1.562]	[1.418]	[1.460]
Observations	73	73	73	73	73	73
R^2	0.797	0.832	0.779	0.822	0.784	0.825
Partisan Controls	No	Yes	No	Yes	No	Yes
Regional FE	Yes	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes	Yes
dy/dx - Less Dem	0.002	0.007	0.010	0.017	0.009	0.016
se	[0.035]	[0.032]	[0.040]	[0.038]	[0.039]	[0.037]
Partial R^2- Main			0.527	0.504	0.527	0.504
Partial R^2- Interaction			0.776	0.772	0.776	0.772
F-stat - Main			20.876	15.421	20.876	15.421
p > F - Main			0.000	0.000	0.000	0.000
F-stat - Interaction			48.244	45.437	48.244	45.437
p > F - Interaction			0.000	0.000	0.000	0.000
Hansen J-stat			0.792	0.221	0.814	0.230
p-val			0.673	0.895	0.666	0.891

Robust and country-clustered standard errors in brackets.
*** p<0.01, ** p<0.05, * p<0.1

Table A1-4. IV Utilizing Internal Information - Data with WVS: **Relative Fraction and 10-9 vs. 7 and Others**

VARIABLES	OLS	OLS	Lewbel (Fuller1)	Lewbel (Fuller1)	Lewbel (Fuller4)	Lewbel (Fuller4)
Leftist Orientation:	0.152**	0.161***	0.249**	0.245*	0.230**	0.226*
Relative Fraction	[0.062]	[0.058]	[0.119]	[0.130]	[0.105]	[0.112]
Leftist Orientation	-0.165**	-0.167**	-0.288**	-0.275*	-0.267**	-0.254**
$\times$Less Democratic	[0.073]	[0.068]	[0.127]	[0.137]	[0.114]	[0.119]
Less Democratic	-5.776***	-4.938***	-6.594***	-5.800***	-6.445***	-5.621***
	[1.882]	[1.692]	[2.111]	[2.087]	[2.037]	[1.959]
Observations	57	57	57	57	57	57
R^2	0.826	0.863	0.807	0.850	0.813	0.855
Partisan Controls	No	Yes	No	Yes	No	Yes
Regional FE	Yes	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes	Yes
dy/dx - Less Dem	-0.012	-0.006	-0.039	-0.030	-0.037	-0.028
se	[0.046]	[0.041]	[0.051]	[0.047]	[0.050]	[0.046]
Partial R^2- Main			0.534	0.527	0.534	0.527
Partial R^2- Interaction			0.824	0.824	0.824	0.824
F-stat - Main			9.067	9.740	9.067	9.740
p > F - Main			0.000	0.000	0.000	0.000
F-stat - Interaction			31.554	30.765	31.554	30.765
p > F - Interaction			0.000	0.000	0.000	0.000
Hansen J-stat			0.134	0.358	0.151	0.358
p-val			0.935	0.836	0.927	0.836

Robust and country-clustered standard errors in brackets.

*** p<0.01, ** p<0.05, * p<0.1

Table A1-5. IV Utilizing External Information - Data with WVS: **Standardized Mean and 10-9 vs. 8 and Others**

VARIABLES	OLS	OLS	External (Fuller1)	External (Fuller1)	External (Fuller4)	External (Fuller4)	Int-Ext (Fuller1)	Int-Ext (Fuller1)	Int-Ext (Fuller4)	Int-Ext (Fuller4)
Leftist Orientation:	2.504***	2.552***	4.075**	4.012**	3.959**	3.939**	4.580**	4.435**	4.418**	4.311**
Standardized Mean	[0.808]	[0.770]	[1.700]	[1.574]	[1.590]	[1.486]	[2.096]	[1.872]	[1.951]	[1.759]
Leftist Orientation	-2.575***	-2.541***	-2.556	-3.163*	-2.638	-3.168**	-4.450**	-4.164**	-4.292**	-4.048**
×Less Democratic	[0.944]	[0.861]	[1.801]	[1.565]	[1.663]	[1.465]	[2.030]	[1.741]	[1.902]	[1.649]
Less Democratic	-4.335***	-3.329***	-5.983***	-4.761**	-5.926***	-4.708**	-6.023***	-4.891**	-5.968***	-4.822**
	[1.210]	[1.080]	[1.869]	[1.809]	[1.832]	[1.772]	[1.824]	[1.859]	[1.791]	[1.808]
Observations	73	73	57	57	57	57	57	57	57	57
R^2	0.796	0.830	0.345	0.516	0.360	0.519	0.372	0.514	0.379	0.517
Partisan Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Regional FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
dy/dx - Less Dem	-0.071	0.011	1.519	0.849	1.321	0.771	0.131	0.271	0.126	0.262
se	[0.512]	[0.481]	[1.111]	[0.936]	[0.996]	[0.875]	[0.559]	[0.552]	[0.553]	[0.543]
Partial R^2- Main			0.454	0.529	0.454	0.529	0.772	0.767	0.772	0.767
Partial R^2- Interaction			0.425	0.474	0.425	0.474	0.920	0.921	0.920	0.921
F-stat - Main			10.085	13.572	10.085	13.572	38.671	20.178	38.671	20.178
p > F - Main			0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
F-stat - Interaction			4.049	5.232	4.049	5.232	76.004	83.803	76.004	83.803
p > F - Interaction			0.008	0.002	0.008	0.002	0.000	0.000	0.000	0.000
Hansen J-stat			0.634	0.111	0.659	0.112	5.029	2.606	5.108	2.674
p-val			0.728	0.946	0.719	0.946	0.540	0.856	0.530	0.849

Robust and country-clustered standard errors in brackets.

*** p<0.01, ** p<0.05, * p<0.1

Table A1-6. IV Utilizing External Information - Data with WVS: **Standardized Mean and 10-9 vs. 7 and Others**

VARIABLES	OLS	OLS	External (Fuller1)	External (Fuller1)	External (Fuller4)	External (Fuller4)	Int-Ext (Fuller1)	Int-Ext (Fuller1)	Int-Ext (Fuller4)	Int-Ext (Fuller4)
Leftist Orientation:	2.428**	2.559***	3.910*	3.894**	3.772**	3.820**	4.653**	4.294**	4.418**	4.166**
Standardized Mean	[0.928]	[0.859]	[1.969]	[1.758]	[1.807]	[1.640]	[2.132]	[1.921]	[1.972]	[1.800]
Leftist Orientation	-2.578**	-2.617***	-3.516*	-4.485**	-3.453*	-4.314**	-5.100**	-4.606**	-4.829**	-4.443**
×Less Democratic	[1.002]	[0.919]	[2.049]	[1.818]	[1.904]	[1.677]	[2.058]	[1.839]	[1.905]	[1.723]
Less Democratic	-5.212***	-4.431***	-6.648**	-5.880**	-6.579**	-5.822**	-7.069**	-6.146**	-6.945***	-6.053**
	[1.732]	[1.551]	[2.595]	[2.411]	[2.544]	[2.363]	[2.595]	[2.366]	[2.535]	[2.318]
Observations	57	57	44	44	44	44	44	44	44	44
R^2	0.826	0.862	0.427	0.589	0.432	0.592	0.389	0.585	0.404	0.589
Partisan Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Regional FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
dy/dx - Less Dem	-0.149	-0.058	0.395	-0.591	0.318	-0.494	-0.447	-0.312	-0.410	-0.277
se	[0.641]	[0.569]	[1.359]	[1.104]	[1.249]	[1.017]	[0.697]	[0.534]	[0.680]	[0.525]
Partial R^2- Main			0.532	0.632	0.532	0.632	0.798	0.818	0.798	0.818
Partial R^2- Interaction			0.540	0.591	0.540	0.591	0.949	0.950	0.949	0.950
F-stat - Main			8.297	11.162	8.297	11.162	8.459	10.328	8.459	10.328
p > F - Main			0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
F-stat - Interaction			3.497	4.299	3.497	4.299	232.973	291.030	232.973	291.030
p > F - Interaction			0.018	0.007	0.018	0.007	0.000	0.000	0.000	0.000
Hansen J-stat			2.528	1.431	2.592	1.429	3.707	4.599	3.730	4.551
p-val			0.283	0.489	0.274	0.489	0.716	0.596	0.713	0.603

Robust and country-clustered standard errors in brackets.

*** p<0.01, ** p<0.05, * p<0.1

Table A1-7. IV Utilizing External Information - Data with WVS: **Relative Fraction and 10-9 vs. 8 and Others**

VARIABLES	OLS	OLS	External (Fuller1)	External (Fuller1)	External (Fuller4)	External (Fuller4)	Int-Ext (Fuller1)	Int-Ext (Fuller1)	Int-Ext (Fuller4)	Int-Ext (Fuller4)
Leftist Orientation:	0.160***	0.164***	0.241**	0.241**	0.236**	0.239**	0.265**	0.264**	0.258**	0.258**
Relative Fraction	[0.056]	[0.053]	[0.108]	[0.098]	[0.101]	[0.093]	[0.124]	[0.115]	[0.117]	[0.110]
Leftist Orientation	-0.158**	-0.157**	-0.129	-0.180	-0.141	-0.183	-0.268**	-0.255**	-0.260**	-0.249**
×Less Democratic	[0.067]	[0.061]	[0.132]	[0.119]	[0.119]	[0.109]	[0.127]	[0.111]	[0.121]	[0.107]
Less Democratic	-4.928***	-3.846***	-6.143***	-5.100**	-6.161***	-5.084**	-6.785***	-5.509**	-6.718***	-5.443***
	[1.301]	[1.156]	[2.086]	[2.030]	[2.037]	[1.977]	[2.077]	[2.039]	[2.032]	[1.985]
Observations	73	73	57	57	57	57	57	57	57	57
R^2	0.797	0.832	0.356	0.530	0.373	0.532	0.390	0.529	0.394	0.531
Partisan Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Regional FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
dy/dx - Less Dem	0.002	0.007	0.112	0.061	0.095	0.055	-0.003	0.008	-0.002	0.009
se	[0.035]	[0.032]	[0.096]	[0.083]	[0.083]	[0.075]	[0.038]	[0.036]	[0.038]	[0.036]
Partial R^2- Main			0.407	0.499	0.407	0.499	0.753	0.753	0.753	0.753
Partial R^2- Interaction			0.321	0.376	0.321	0.376	0.899	0.899	0.899	0.899
F-stat - Main			6.075	12.468	6.075	12.468	25.307	15.378	25.307	15.378
p > F - Main			0.001	0.000	0.001	0.000	0.000	0.000	0.000	0.000
F-stat - Interaction			3.262	4.286	3.262	4.286	56.391	60.435	56.391	60.435
p > F - Interaction			0.022	0.006	0.022	0.006	0.000	0.000	0.000	0.000
Hansen J-stat			0.885	0.427	0.900	0.430	4.720	2.773	4.748	2.804
p-val			0.642	0.808	0.638	0.807	0.580	0.837	0.577	0.833

Robust and country-clustered standard errors in brackets.

*** p<0.01, ** p<0.05, * p<0.1

Table A1-8. IV Utilizing External Information - Data with WVS: **Relative Fraction and 10-9 vs. 7 and Others**

VARIABLES	OLS	OLS	External (Fuller1)	External (Fuller1)	External (Fuller4)	External (Fuller4)	Int-Ext (Fuller1)	Int-Ext (Fuller1)	Int-Ext (Fuller4)	Int-Ext (Fuller4)
Leftist Orientation:	0.152**	0.161***	0.234*	0.246**	0.227**	0.240**	0.272**	0.263**	0.260**	0.257**
Relative Fraction	[0.062]	[0.058]	[0.117]	[0.110]	[0.108]	[0.102]	[0.125]	[0.123]	[0.117]	[0.116]
Leftist Orientation	-0.165**	-0.167**	-0.234	-0.314**	-0.229*	-0.295**	-0.325**	-0.304**	-0.308**	-0.293**
×Less Democratic	[0.073]	[0.068]	[0.149]	[0.127]	[0.134]	[0.113]	[0.130]	[0.124]	[0.122]	[0.117]
Less Democratic	-5.776***	-4.938***	-7.300**	-7.052**	-7.233**	-6.885**	-8.061***	-7.124**	-7.897***	-7.000**
	[1.882]	[1.692]	[2.919]	[2.756]	[2.835]	[2.642]	[2.864]	[2.630]	[2.796]	[2.564]
Observations	57	57	44	44	44	44	44	44	44	44
R^2	0.826	0.863	0.438	0.587	0.441	0.594	0.407	0.592	0.417	0.595
Partisan Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Regional FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
dy/dx - Less Dem	-0.012	-0.006	-0.001	-0.068	-0.003	-0.055	-0.053	-0.041	-0.048	-0.037
se	[0.046]	[0.041]	[0.109]	[0.084]	[0.095]	[0.072]	[0.049]	[0.036]	[0.048]	[0.035]
Partial R^2- Main			0.484	0.585	0.484	0.585	0.778	0.790	0.778	0.790
Partial R^2- Interaction			0.387	0.429	0.387	0.429	0.917	0.918	0.917	0.918
F-stat - Main			5.152	8.231	5.152	8.231	9.761	10.897	9.761	10.897
p > F - Main			0.003	0.000	0.003	0.000	0.000	0.000	0.000	0.000
F-stat - Interaction			2.613	3.279	2.613	3.279	104.064	128.481	104.064	128.481
p > F - Interaction			0.054	0.023	0.054	0.023	0.000	0.000	0.000	0.000
Hansen J-stat			2.119	0.817	2.141	0.836	3.070	4.146	3.137	4.107
p-val			0.347	0.665	0.343	0.659	0.800	0.657	0.791	0.662

Robust and country-clustered standard errors in brackets.
*** p<0.01, ** p<0.05, * p<0.1

Table A2-1. IV Estimation Utilizing Internal Information - Data with Multiple Surveys: **10-9 vs. 8 and Others**

VARIABLES	OLS	OLS	Lewbel (Fuller1)	Lewbel (Fuller1)	Lewbel (Fuller4)	Lewbel (Fuller4)
Leftist Orientation:	2.064**	1.813**	2.989*	2.481	2.918**	2.421
Standardized Mean	[0.777]	[0.680]	[1.499]	[1.577]	[1.409]	[1.460]
Leftist Orientation	-2.197**	-1.879**	-2.859*	-2.293	-2.800*	-2.245
×Less Democratic	[0.922]	[0.818]	[1.538]	[1.607]	[1.462]	[1.510]
Less Democratic	-5.809***	-4.715***	-5.876***	-4.980***	-5.868***	-4.954***
	[1.149]	[1.240]	[1.086]	[1.269]	[1.083]	[1.247]
Observations	166	166	166	166	166	166
R^2	0.770	0.792	0.485	0.543	0.488	0.545
Partisan Controls	No	Yes	No	Yes	No	Yes
Regional FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
dy/dx - Less Dem	-0.133	-0.065	0.129	0.188	0.118	0.176
se	[0.423]	[0.423]	[0.482]	[0.493]	[0.480]	[0.490]
Partial R^2- Main			0.435	0.414	0.435	0.414
Partial R^2- Interaction			0.829	0.828	0.829	0.828
F-stat - Main			25.457	25.899	25.457	25.899
p > F - Main			0.000	0.000	0.000	0.000
F-stat - Interaction			115.853	110.336	115.853	110.336
p > F - Interaction			0.000	0.000	0.000	0.000
Hansen J-stat			1.094	0.367	1.104	0.371
p-val			0.579	0.832	0.576	0.831

Robust and country-clustered standard errors in brackets.

*** p<0.01, ** p<0.05, * p<0.1

Table A2-2 . IV Estimation Utilizing Internal Information - Data with Multiple Surveys: **10-9 vs. 7 and Others**

VARIABLES	OLS	OLS	Lewbel (Fuller1)	Lewbel (Fuller1)	Lewbel (Fuller4)	Lewbel (Fuller4)
Leftist Orientation:	2.085**	1.870***	2.966**	2.549	2.894**	2.482*
Standardized Mean	[0.792]	[0.676]	[1.455]	[1.542]	[1.368]	[1.418]
Leftist Orientation	-2.189**	-1.782**	-3.231**	-2.647	-3.160**	-2.576*
×Less Democratic	[0.930]	[0.860]	[1.460]	[1.615]	[1.389]	[1.506]
Less Democratic	-6.364***	-5.363***	-6.602***	-5.735***	-6.584***	-5.700***
	[1.550]	[1.525]	[1.415]	[1.476]	[1.414]	[1.457]
Observations	144	144	144	144	144	144
R^2	0.757	0.782	0.491	0.552	0.495	0.554
Partisan Controls	No	Yes	No	Yes	No	Yes
Regional FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
dy/dx - Less Dem	-0.104	0.087	-0.265	-0.098	-0.266	-0.094
se	[0.579]	[0.574]	[0.608]	[0.580]	[0.603]	[0.581]
Partial R^2- Main			0.398	0.384	0.398	0.384
Partial R^2- Interaction			0.925	0.925	0.925	0.925
F-stat - Main			11.361	13.975	11.361	13.975
p > F - Main			0.000	0.000	0.000	0.000
F-stat - Interaction			133.477	135.254	133.477	135.254
p > F - Interaction			0.000	0.000	0.000	0.000
Hansen J-stat			0.797	1.952	0.816	1.960
p-val			0.671	0.377	0.665	0.375

Robust and country-clustered standard errors in brackets.

*** p<0.01, ** p<0.05, * p<0.1

Table A3. Using World Value Surveys second-fourth Waves (1989-2004)

Country	Ideological Preferences: Std Mean	Rel Fraction	Total Welfare	Dem Score
ALBANIA	1.049855	16.28665	8.656703	6
ARGENTINA	-0.27335	-12.0228	8.604914	7 and 8
AUSTRALIA	0.482312	0.782122	14.14536	10
AUSTRIA	-0.01506	-6.90994	24.89485	10
BANGLADESH	-4.04623	-58.2322	3.148216	6
BELARUS	0.224775	-4.94192	13.62227	0
BRAZIL	-0.18931	-6.61522	12.07664	8
BULGARIA	-0.14154	-6.04841	13.23855	8
CANADA	0.033134	-5.78522	10.92088	10
CHILE	0.758383	8.724428	12.69467	8 and 9
CROATIA	0.73508	7.196262	20.69704	0
CZECH REPUBLIC	-0.73807	-15.4884	20.94351	10
DENMARK	-0.09544	-8.2712	19.05793	10
DOMINICAN REPUBLIC	-2.13731	-33.5	3.491213	8
ESTONIA	-0.20529	-11.9655	18.65345	7
FINLAND	-0.60935	-13.3987	19.31645	10
FRANCE	1.499372	21.59091	27.42495	9
GERMANY	0.859287	10.56063	18.33552	10
HUNGARY	0.877368	7.192981	25.07211	10
INDIA	-0.82217	-15.9206	0.602441	8 and 9
INDONESIA	-2.12406	-33.9853	2.960627	7
IRAN	1.568409	17.53333	9.206429	4
IRELAND	-1.01995	-20.6013	19.64473	10
KOREA, REPUBLIC OF	-0.35143	-9.36973	5.33781	7 and 8
LATVIA	-0.08069	-9.39165	18.26958	8
LITHUANIA	-0.09571	-5.55076	15.12558	10
MEXICO	-0.83165	-19.6118	6.811121	2, 4, and 8
MOLDOVA	0.587819	1.742018	13.99201	7 and 8
NETHERLANDS	0.309954	-0.21505	30.00905	10
NORWAY	-0.07038	-6.38244	17.85569	10
PAKISTAN	-0.72892	-30.4878	0.402625	0
POLAND	0.139111	-3.09877	21.46068	9
ROMANIA	-0.06006	-6.90387	16.66785	5 and 8
RUSSIA	1.450951	12.53165	8.745363	5
SLOVAK REPUBLIC	0.746971	7.318048	22.55703	9
SLOVENIA	0.992354	8.002602	25.30071	10
SPAIN	1.752783	25.86604	16.68833	10
SWEDEN	0.294168	-0.20164	22.6363	10
SWITZERLAND	0.497375	4.049844	19.32206	10
TURKEY	-0.06413	-5.9375	4.556898	9
UK	0.73833	5.213118	18.44157	10
UKRAINE	0.235883	-1.63044	14.27508	7
URUGUAY	0.100247	-1.73913	21.81466	10
US	-0.40021	-13.6801	9.937492	10

Table A4. Using Multiple Surveys (1989-2004)

Country	Ideological Preferences	Total Welfare	Dem Score
ALBANIA	0.876642	8.656703	6
ARGENTINA	-0.54124	8.604914	7 and 8
AUSTRALIA	-0.34916	14.13781	10
AUSTRIA	-0.26447	24.89485	10
BANGLADESH	-4.5841	3.148216	6
BELARUS	0.04882	15.3542	0
BRAZIL	-0.45119	12.07664	8
BULGARIA	-0.25955	14.96737	8 and 9
CANADA	-0.36633	10.70639	10
CHILE	0.15593	13.10163	8 and 9
CROATIA	0.539342	20.69704	0
CZECH REPUBLIC	-0.75906	20.86448	10
DENMARK	-0.43395	20.43276	10
DOMINICAN REPUBLIC	-2.53858	3.491213	8
ESTONIA	-0.4718	17.11957	7
FINLAND	-0.79834	20.25976	10
FRANCE	1.268306	28.59589	9
GERMANY	0.398457	21.0328	10
HUNGARY	1.188927	26.17238	10
INDIA	-1.12934	0.602441	8 and 9
INDONESIA	-2.52439	2.960627	7
IRAN	1.432301	9.206429	4
IRELAND	-0.51365	20.58135	10
ISRAEL	-0.38681	22.35206	9
KOREA, REPUBLIC OF	-0.19653	5.512547	7 and 8
LATVIA	-0.78303	17.12216	8
LITHUANIA	-0.3509	15.12558	10
MEXICO	-1.35555	6.88157	2, 4, 6, and 8
MOLDOVA	0.381544	13.99201	7 and 8
NETHERLANDS	0.482164	29.98822	10
NORWAY	-0.0837	18.50794	10
PAKISTAN	-1.02942	0.402625	0
POLAND	0.272628	20.31218	9
ROMANIA	-0.40569	15.88787	5 and 8
RUSSIA	0.477662	8.354366	5 and 7
SLOVAK REPUBLIC	0.718656	22.35235	9
SLOVENIA	0.398085	26.12289	10
SPAIN	1.839903	17.11967	10
SWEDEN	0.156638	22.46765	10
SWITZERLAND	-0.10805	16.89414	10
TURKEY	-0.31705	4.556898	9
UK	0.249535	20.25339	10
UKRAINE	0.004424	14.27508	7
URUGUAY	-0.14092	21.81466	10
US	-0.82696	10.09731	10

Table A5-1. Partialling Out the Income Effects by Sample Restriction - Data with WVS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(1) 10-9 vs. 8 and below								
Leftist Orientation:	2.860***	2.124*	3.041**	2.634				
Standardized Mean	[0.913]	[1.189]	[1.471]	[1.794]				
Leftist Orientation	-2.752**	-1.897	-3.232*	-2.595				
×Less Democratic	[1.069]	[1.240]	[1.652]	[1.976]				
Leftist Orientation:					0.173**	0.133*	0.187*	0.167
Relative Fraction					[0.064]	[0.077]	[0.109]	[0.124]
Leftist Orientation					-0.159**	-0.110	-0.197	-0.159
×Less Democratic					[0.075]	[0.078]	[0.125]	[0.137]
Less Democratic	-4.514***	-2.516**	-4.482***	-2.939**	-5.021***	-2.669*	-5.206***	-3.271*
	[1.433]	[1.164]	[1.396]	[1.320]	[1.594]	[1.361]	[1.544]	[1.647]
Observations	47	47	47	47	47	47	47	47
R^2	0.805	0.835	0.821	0.842	0.799	0.835	0.816	0.840
dy/dx - Less Dem	0.109	0.228	-0.191	0.039	0.014	0.023	-0.010	0.008
se	[0.493]	[0.475]	[0.565]	[0.545]	[0.034]	[0.032]	[0.041]	[0.037]
Partisan Controls	No	Yes	No	Yes	No	Yes	No	Yes
Regional FE	No	No	Yes	Yes	No	No	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Robust and country-clustered standard errors in brackets

*** p<0.01, ** p<0.05, * p<0.1

Table A5-2. Partialling Out the Income Effects by Sample Restriction - Data with WVS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(2) 10-9 vs. 7 and below								
Leftist Orientation:	3.001***	1.796	3.547	1.867				
Standardized Mean	[1.037]	[1.250]	[2.452]	[2.731]				
Leftist Orientation	-3.067**	-1.960	-3.952*	-2.137				
×Less Democratic	[1.164]	[1.320]	[2.208]	[2.725]				
Leftist Orientation:					0.187**	0.113	0.218	0.114
Relative Fraction					[0.076]	[0.087]	[0.193]	[0.215]
Leftist Orientation					-0.194**	-0.126	-0.254	-0.134
×Less Democratic					[0.089]	[0.094]	[0.192]	[0.225]
Less Democratic	-5.310**	-2.657	-5.580***	-2.971	-5.910**	-2.879	-6.412***	-3.178
	[2.115]	[1.664]	[1.729]	[2.102]	[2.134]	[1.878]	[1.794]	[2.760]
Observations	32	32	32	32	32	32	32	32
R^2	0.874	0.900	0.881	0.901	0.868	0.899	0.876	0.900
dy/dx - Less Dem	-0.066	-0.164	-0.405	-0.270	-0.007	-0.013	-0.036	-0.020
se	[0.718]	[0.687]	[0.893]	[0.901]	[0.051]	[0.046]	[0.064]	[0.063]
Partisan Controls	No	Yes	No	Yes	No	Yes	No	Yes
Regional FE	No	No	Yes	Yes	No	No	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Robust and country-clustered standard errors in brackets

*** p<0.01, ** p<0.05, * p<0.1

Table A6. Partialling Out the Income Effects by Sample Restriction - Data with Combined Surveys

	(1)	(2)	(3)	(4)
(1) 10-9 vs. 8 and below				
Leftist Orientation:	2.427***	1.688**	2.338***	1.680*
Standardized Mean	[0.426]	[0.656]	[0.706]	[0.894]
Leftist Orientation	-2.282***	-1.484	-2.319**	-1.504
×Less Democratic	[0.723]	[0.895]	[0.920]	[1.163]
Less Democratic	-5.462***	-3.069**	-5.518***	-3.198**
	[1.063]	[1.214]	[1.134]	[1.217]
Observations	69	69	69	69
R^2	0.854	0.876	0.859	0.876
dy/dx - Less Dem	0.146	0.204	0.019	0.176
se	[0.492]	[0.491]	[0.534]	[0.526]
(2) 10-9 vs. 7 and below				
Leftist Orientation:	2.964***	2.384**	2.870**	2.340
Standardized Mean	[0.519]	[0.964]	[1.148]	[1.476]
Leftist Orientation	-3.169***	-2.525*	-3.367***	-2.602
×Less Democratic	[0.760]	[1.280]	[1.104]	[1.706]
Less Democratic	-5.883***	-4.337**	-6.167***	-4.597***
	[1.222]	[1.571]	[1.367]	[1.342]
Observations	50	50	50	50
R^2	0.911	0.917	0.913	0.917
dy/dx - Less Dem	-0.205	-0.141	-0.497	-0.262
se	[0.494]	[0.614]	[0.840]	[0.830]
Partisan Controls	No	Yes	No	Yes
Regional FE	No	No	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Robust and country-clustered standard errors in brackets
*** p<0.01, ** p<0.05, * p<0.1

Table A7. Ideological Preferences and Support for More Income Equality - Estimates from Individual Data by Country-Year Modules

Module	Coef. Est.	S.E.	N	Module	Coef. Est.	S.E.	N
ALBANIA (98)	0.229***	0.038	871	S. KOREA (96)	0.128***	0.044	1159
ARGENTINA (91)	-0.038	0.145	241	S. KOREA (01)	0.015	0.043	1195
ARGENTINA (95)	0.280***	0.068	717	LATVIA (96)	0.243***	0.052	892
ARGENTINA (99)	-0.033	0.057	848	LITHUANIA (97)	0.226***	0.051	689
AUSTRALIA (95)	0.340***	0.035	1605	LITHUANIA (99)	0.169**	0.078	568
AUSTRIA (90)	0.160**	0.068	1211	MEXICO (90)	0.213***	0.058	614
BGD (02)	0.273***	0.034	1109	MEXICO (96)	0.106***	0.032	1707
BELARUS (96)	0.207***	0.043	1125	MEXICO (00)	0.066	0.046	864
BELARUS (00)	0.055	0.072	502	MOLDOVA (96)	0.217***	0.046	759
BRAZIL (91)	0.141***	0.045	919	MOLDOVA (02)	0.227***	0.045	614
BRAZIL (97)	0.057	0.042	957	NLD (90)	0.428***	0.041	728
BULGARIA (90)	0.151***	0.052	669	NORWAY (90)	0.482***	0.038	960
BULGARIA (97)	0.269***	0.047	742	NORWAY (96)	0.374***	0.039	1020
BULGARIA (99)	0.234***	0.051	656	PAKISTAN (01)	0.183*	0.097	197
CANADA (90)	0.216***	0.048	1242	POLAND (97)	0.129***	0.044	824
CANADA (00)	0.186***	0.047	1492	POLAND (99)	0.148***	0.052	819
CHILE (90)	0.174***	0.051	915	ROMANIA (93)	0.495***	0.046	902
CHILE (96)	0.111*	0.057	767	ROMANIA (98)	0.320***	0.043	910
CHILE (00)	0.214***	0.046	957	ROMANIA (99)	0.265***	0.061	585
CROATIA (96)	0.062	0.054	1025	RUSSIA (99)	0.153***	0.036	1485
CZECH REP (98)	0.340***	0.043	847	SLOVAK REP (98)	0.182***	0.053	831
CZECH REP (99)	0.297***	0.035	1553	SLOVENIA (99)	-0.047	0.072	479
DENMARK (90)	0.528***	0.042	812	SPAIN (90)	0.334***	0.032	2210
DOM. REP (96)	0.057	0.062	315	SPAIN (95)	0.164**	0.067	633
ESTONIA (96)	-0.033	0.061	752	SWEDEN (96)	0.435***	0.031	885
ESTONIA (99)	0.243***	0.062	575	SWISS (96)	0.241***	0.065	740
FINLAND (90)	0.531***	0.056	503	TURKEY (90)	0.267***	0.069	606
FINLAND (96)	0.375***	0.045	799	TURKEY (96)	0.094	0.059	1157
FRANCE (90)	0.466***	0.053	676	UK (90)	0.358***	0.05	1005
GERMANY (90)	0.266***	0.036	2842	UK (99)	0.399***	0.072	522
HUNGARY (91)	0.205***	0.064	688	UKRAINE (99)	0.090*	0.052	666
INDIA (90)	0.189***	0.036	1180	URUGUAY (96)	0.225***	0.065	838
INDIA (95)	0.174***	0.03	951	US (90)	0.183***	0.047	1415
INDONESIA (01)	0.190***	0.049	725	US (95)	0.270***	0.051	1193
IRAN (00)	-0.052	0.036	1070	US (99)	0.234***	0.046	1057
IRELAND (90)	0.216***	0.058	805				

All regressions include household income, sex, age, unemployment status, marital status, number of children, and church attendence.

Table A8. Excluding Country-Year Modules Where the Ideological Preferences May Not Capture the Dimension of Welfare and Redistribution - Standardized Mean

	(1)	(2)	(3)	(4)	(5)	(6)
(1) 10-9 vs. 8 and below						
Leftist Orientation:	2.504***	2.636***	2.719***	2.552***	2.812***	2.870***
Standardized Mean	[0.808]	[0.783]	[0.811]	[0.770]	[0.757]	[0.769]
Leftist Orientation	-2.575***	-2.382**	-2.561**	-2.541***	-2.483***	-2.632***
×Less Democratic	[0.944]	[0.940]	[0.956]	[0.861]	[0.885]	[0.887]
Less Democratic	-4.335***	-4.941***	-5.361***	-3.329***	-4.154***	-4.523***
	[1.210]	[1.105]	[1.076]	[1.080]	[0.905]	[0.863]
Observations	73	64	62	73	64	62
R^2	0.796	0.821	0.828	0.830	0.860	0.866
dy/dx - Less Dem	-0.071	0.254	0.158	0.011	0.328	0.238
se	[0.512]	[0.555]	[0.562]	[0.481]	[0.479]	[0.478]
(2) 10-9 vs. 7 and below						
Leftist Orientation:	2.428**	2.633***	2.713***	2.559***	2.742***	2.801***
Standardized Mean	[0.928]	[0.937]	[0.931]	[0.859]	[0.869]	[0.857]
Leftist Orientation	-2.578**	-2.575**	-2.624**	-2.617***	-2.607***	-2.636***
×Less Democratic	[1.002]	[1.004]	[0.994]	[0.919]	[0.945]	[0.945]
Less Democratic	-5.212***	-5.426***	-5.776***	-4.431***	-4.655***	-4.975***
	[1.732]	[1.499]	[1.447]	[1.551]	[1.247]	[1.182]
Observations	57	53	52	57	53	52
R^2	0.826	0.840	0.843	0.862	0.881	0.885
dy/dx - Less Dem	-0.149	0.058	0.089	-0.058	0.135	0.166
se	[0.641]	[0.608]	[0.610]	[0.569]	[0.515]	[0.514]
Excluding	None	p>.1	p>.05	None	p>.1	p>.05
Partisan Controls	No	No	No	Yes	Yes	Yes
Regional FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Robust and country-clustered standard errors in brackets
*** p<0.01, ** p<0.05, * p<0.1

Table A9. Excluding Country-Year Modules Where the Ideological Preferences May Not Capture the Dimension of Welfare and Redistribution - Relative Fraction

	(1)	(2)	(3)	(4)	(5)	(6)
(1) 10-9 vs. 8 and below						
Leftist Orientation:	0.160***	0.167***	0.172***	0.164***	0.178***	0.182***
Relative Fraction	[0.056]	[0.053]	[0.055]	[0.053]	[0.051]	[0.053]
Leftist Orientation	-0.158**	-0.143**	-0.158**	-0.157**	-0.150**	-0.163**
×Less Democratic	[0.067]	[0.067]	[0.069]	[0.061]	[0.063]	[0.064]
Less Democratic	-4.928***	-5.410***	-5.905***	-3.846***	-4.564***	-4.994***
	[1.301]	[1.239]	[1.196]	[1.156]	[1.065]	[1.001]
Observations	73	64	62	73	64	62
R^2	0.797	0.822	0.830	0.832	0.863	0.869
dy/dx - Less Dem	0.002	0.023	0.014	0.007	0.028	0.020
se	[0.035]	[0.038]	[0.039]	[0.032]	[0.033]	[0.033]
(2) 10-9 vs. 7 and below						
Leftist Orientation:	0.152**	0.163**	0.169***	0.161***	0.171***	0.175***
Relative Fraction	[0.062]	[0.061]	[0.061]	[0.058]	[0.058]	[0.057]
Leftist Orientation	-0.165**	-0.163**	-0.168**	-0.167**	-0.166**	-0.170**
×Less Democratic	[0.073]	[0.072]	[0.071]	[0.068]	[0.068]	[0.068]
Less Democratic	-5.776***	-5.939***	-6.317***	-4.938***	-5.121***	-5.468***
	[1.882]	[1.633]	[1.552]	[1.692]	[1.378]	[1.288]
Observations	57	53	52	57	53	52
R^2	0.826	0.839	0.843	0.863	0.882	0.886
dy/dx - Less Dem	-0.012	-0.000	0.001	-0.006	0.005	0.006
se	[0.046]	[0.044]	[0.044]	[0.041]	[0.038]	[0.038]
Excluding	None	p>.1	p>.05	None	p>.1	p>.05
Partisan Controls	No	No	No	Yes	Yes	Yes
Regional FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Robust and country-clustered standard errors in brackets

*** p<0.01, ** p<0.05, * p<0.1

Chapter 4

Immigrants, Socialization, and Political Views on Welfare Spending

4.1 Introduction

Many countries are experiencing massive immigration in the era of globalization. Immigrants in the US, for example, account for about 12% of the total population and almost 40% of the current population growth is due to immigration (Camarota, 2004; 2001). In the UK, approximately 10.1% of the population is foreign-born.[1] In Canada, New Zealand, and many other OECD countries, immigrants constitute a rising share of the population. The flow of immigration highlights the importance of understanding immigrant political behavior as immigrants will eventually impact the politics of their adopted country. But the political experience of immigrants in a *new* country also provides researchers a unique opportunity to examine the effects of socialization and acculturation. Unlike natives, immigrants are born and grow up in foreign countries and are exposed to a new political culture once they emigrate. Therefore, analyzing their political behavior and comparing it with that of natives allows us to better understand the influences of socialization and assimilation.

During the past decades, scholars have produced a vast body of literature on immigration in developed countries. While studies have extensively focused on natives' views (mostly hostile)

[1] Estimate from http://www.statistics.gov.uk, the Annual Population Survey of the Office for National Statistics.

toward immigrants, research on the political views of immigrants themselves is in its infancy. Only a few scholars have analyzed immigrants' party preferences and voting behaviors in order to evaluate the possibility of their political integration (Ramakrishnan and Espenshade, 2001; Wong, 2000; Cain, Kiewiet, and Uhlaner, 1991). In contrast, research on immigrants' political preferences in *other* major policy areas, such as welfare, national security, and crime, is rare although it is a growing consensus that attention to these other policy preferences of immigrants is crucial in order to understand future political developments in a receiving society.[2]

The present study examines the political preferences of first-, second-, and third-generation immigrants regarding welfare spending in the US. The welfare policy issue defines the left-right political ideology in the electoral politics of most countries. Accordingly, social scientists have long been devoted to understanding why some people support welfare policies more than others (Fox, 2004; Luttmer, 2001; Gilens, 1995; Hasenfeld and Rafferty, 1989). This paper begins by analyzing whether immigrants systematically differ from natives in their political views on welfare spending. This is an empirical question, but the choice of US leads us to start with a prior belief that first-generation immigrants are more liberal than US natives. Research on "American exceptionalism" suggests that Americans are distinctively individualistic and conservative in their welfare attitudes. Many authors have stressed that Americans strongly favor equal opportunity over equal outcome, and have a strong belief in the value of individual efforts instead of the socialist value of redistribution (Lipset, 1996; 1979; 1977; de Tocqueville, 1835). Individualism has long been the dominant ideology in the US (Kluegel and Smith, 1986). Accordingly, a group of scholars has used the idea of individualism to explain why Americans do not support a large welfare state (Alesina, Glaeser, and Sacerdote, 2001; Feldman and Zaller, 1992; Marmor, Mashaw, and Harvey, 1990). Since first-generation immigrants grow up and are socialized in foreign countries, they may not have the strong inclination toward individualism that most Americans have. Therefore, they are expected to support welfare spending more than US natives. Despite other theoretical possibilities—that immigrants as a self-selected group of highly motivated people may be less supportive of welfare programs than US natives or that variation in socioeconomic factors may drive immigrant-native differences—this study finds that first-generation immigrants are indeed more liberal than US natives, even

[2]An important exception is the study of Dancygier and Saunders (2006) on immigrants' preferences regarding social spending in the UK and Germany.

after controlling for socioeconomic variables.[3]

After we establish the systematic differences between first-generation immigrants and US natives, this study will then examine the effects of assimilation and socialization on immigrants' political preferences regarding welfare spending. Once immigrants cross the border, they are exposed to a *new* political system through which they experience and possibly adopt America's dominant political values and norms. Therefore, tracking the development of political preferences of first-generation immigrants and comparing them with those of natives allows us to analyze the effects of assimilation in American society. Note that age has often been employed as a proxy for political experience of natives (Cain, Kiewiet, and Uhlaner, 1991; Converse, 1969). However, age also represents the stage in the life cycle that may influence political behavior (Niemi et. al., 1985). For example, people in their 20s are more likely to be socially and economically vulnerable than middle aged people and thus are more likely to support welfare programs. Therefore, age may have an effect that is different from that of political experience. By focusing on first-generation immigrants, we can explore the effect of political experience while partialling out the age effect because the two factors do not directly correspond to each other (Wong, 2000).

Generational status is also useful in analyzing the effect of socialization (Ramakrishnan and Espenshade, 2001; Cain, Kiewiet, and Uhlaner, 1991). Second-generation immigrants grow up in the US, just like natives. Thus, they have similar experiences in daily life. However, at the same time, they are influenced by their non-native parents at home. Therefore, comparing the political views of first-, second-, and third-generation immigrants with those of natives contributes to our understanding of the political socialization of immigrants.

Note that this study does not assume that immigrants are a homogeneous group in the US. Immigrants come from many countries and have diverse cultural, linguistic, and political backgrounds. However, we can posit that immigrants are a politically meaningful group that will eventually impact US welfare politics. Immigrants have often been treated as one group when discussing natives' attitudes toward immigrants and in policy debates about restricting immi-

[3]Although there are various definitions of the conservative-liberal spectrum, this study will employ a political sense of the government's responsibility of welfare. Therefore, "a conservative person" in this context refers to someone who believes that the government should spend less on transfers and that individuals ought to work for their own welfare. Conversely, "a liberal person" refers to someone who believes that the government has the responsibility for welfare transfer in order to reduce inequality and take care of citizens' welfare.

gration. We have also observed that not only Latino and Asian immigrants, but immigrants as a whole have acted collectively on immigration-related issues (e.g., lobbying and petitioning for a federal lawsuit after Proposition 187). In social science research, some scholars have treated immigrants as one category (Borjas, 2002; Ramakrishnan and Espenshade, 2001; Cho, 1999; Borjas and Hilton, 1996) while others have limited their scope to sub-ethnic immigrant populations (Wong, 2000; Cain, Kiewiet, and Uhlaner, 1991).[4]

The present study uses three nationwide surveys to investigate the political views of immigrants—the 2004 National Annenberg Election Survey, the 1992-2000 American National Election Studies Cumulative Data File, and the 2006 Cooperative Congressional Election Study. First, I show that first-generation immigrants are more liberal than US natives; they support welfare spending to a greater extent. This finding is consistent with our prior belief based on the combination of American exceptionalism literature and the fact that first-generation immigrants are born and grow up outside the US. Second, as the first-generation immigrants stay longer in the US, their political views more closely resemble those of US natives. This suggests the assimilation of immigrants into US society. Third, the political views of second-generation immigrants fall between those of first-generation immigrants and those of US natives, but are much closer to the latter. I also find that first-generation immigrants in the 1960s and 1970s, a group of whom are parents of current second-generation immigrants, were at least as liberal as today's first-generation immigrants. Taken together with the finding that third-generation immigrants have almost identical views to US natives, this study suggests that the more liberal views of first-generation immigrants are not necessarily adopted by the next generation. This paper provides further evidence that second-generation immigrants are socialized in the same way as US natives, and concludes that the more liberal political views of immigrants regarding welfare spending fade away within one generation or so due to the effects of assimilation and socialization.

[4]Outside of the American context, Dancygier and Saunders (2006) and Chui, Curtis, and Lambert (1991) have analyzed the political behavior of immigrants without limiting their scope to particular ethnic groups.

4.2 Determinants of Political Views on Welfare Spending

Despite the relatively extensive literature on the determinants of American welfare attitudes, immigration and socialization have almost completely been ignored. Instead, most attention has been paid to explanations based on individualism and economic self-interest (Fox, 2004; Pereira and Van Ryzin, 1998; Gilens, 1995; Hasenfeld and Rafferty, 1989).

Many scholars have used individualism to explain variations *within* Americans' welfare attitudes (Bobo, 1991; Hasenfeld and Rafferty, 1989; Feldman, 1988; Kluegel and Smith, 1986; Williamson, 1974). They argue that the dominant ideology of individualism, which emphasizes limited government intervention and individual opportunity and responsibility for one's own well-being, is an underlying principle in determining specific policy preferences. Accordingly, they show that more individualistic respondents are less supportive of means-tested (meaning that eligibility to benefits is determined solely by income level) or progressive (meaning that benefits do not depend on the amount of contributions to programs) welfare policies.[5]

Economic self-interest is another widely used factor in explaining Americans' welfare attitudes. Some theories suggest that the benefit-cost calculus can explain why some people support welfare spending more than others. Although variations exist within this school of thought, the most widely accepted idea is that socially and/or economically vulnerable groups support welfare spending more because they are more likely to receive its benefits. Empirical studies have shown that non-whites—mostly blacks—are much more pro-welfare than whites (Alesina, Glaeser, and Sacerdote, 2001; Alesina and La Ferrara, 2001). People in the low-income bracket are also known to support the welfare state more (AuClaire, 1984). The impact of income mobility could also be included in this school. Lipset (1992, 1977) has long been a proponent of the theory that popular beliefs about social mobility may explain the differences between US and Western European welfare states. Alesina and La Ferrara (2001) argued that the more upward income mobility people expect, the less likely they are to support welfare. Piketty (1995) theorized that low-income individuals whose parents have high

[5]Throughout this paper, the term "welfare" refers to these means-tested or progressive welfare programs. These may include Temporary Assistance to Needy Families (TANF), Aid to Families with Dependent Children (AFDC), Food Stamps, and other transfer programs that provide benefits directly to the needy. The government expenditure on public education may also be included in this definition of welfare due to its progressive characteristic. In contrast, social security is not a part of this strictly defined welfare because it is regressive; the benefits depend on how much one contributes to the fund.

income support less redistribution than low-income people whose parents also have low income. Likewise, high-income individuals whose parents have low income support more redistribution than high-income people whose parents also have high income. Finally, individuals with less education, females, and the young have all been reported to be more liberal in their welfare attitudes (Pereira and Van Ryzin, 1998; Hasenfeld and Rafferty, 1989).

Beyond the two mainstream schools of thought mentioned above, a rapidly growing body of literature attempts to link racial politics with views on welfare spending. Gilens (1999; 1996; 1995) is the pioneer of this school of thought, having revealed that the racial stereotype of blacks being lazy is still prominent in US politics and is inversely correlated with whites' views on welfare spending. Numerous other studies have similarly found that whites want to spend less for welfare when the benefits are likely to go to people belonging to non-white ethnic groups (Luttmer, 2001; Alesina and La Ferrara, 2001; Alesina, Baqir, and Easterly, 1999).

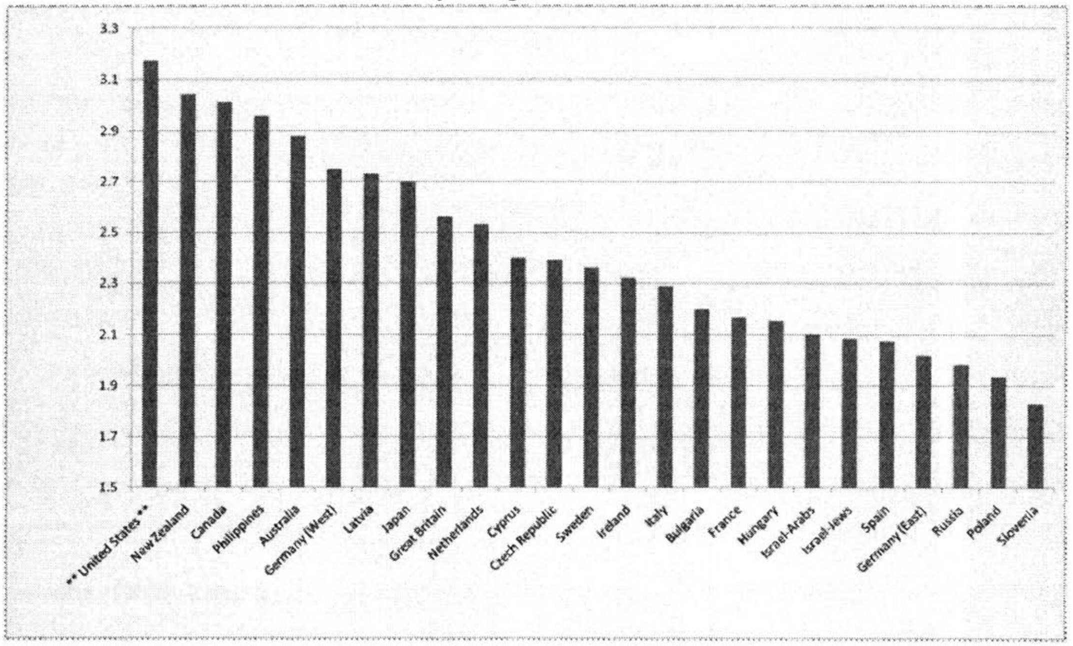
Existing literature has focused on individualism, economic self-interest, racial politics, party identification (Cook and Barrett, 1992; Chew, 1992; Eismeier, 1982), and even religiosity (Scheve and Stasavage, 2005). In contrast, immigrants have largely been marginalized when researchers have explored welfare attitudes. However, it is increasingly important to understand immigrants' political views as the immigrant population is growing and influencing US welfare politics in various ways. Furthermore, large-scale immigration affords us a great opportunity to analyze the effects of political socialization and assimilation, something that previous studies have also failed to consider.

4.3 Theoretical Expectations

According to the American exceptionalism literature, Americans are distinctively individualistic and have distaste for redistribution (Lipset, 1996; 1979; de Tocqueville, 1835). This observation provides an insight into immigrants' political preferences with regard to welfare spending; immigrants, having been raised and socialized in foreign countries, are expected to be more liberal than US natives who have had life-long exposure to deeply-rooted individualism (McClosky and Zaller, 1984).

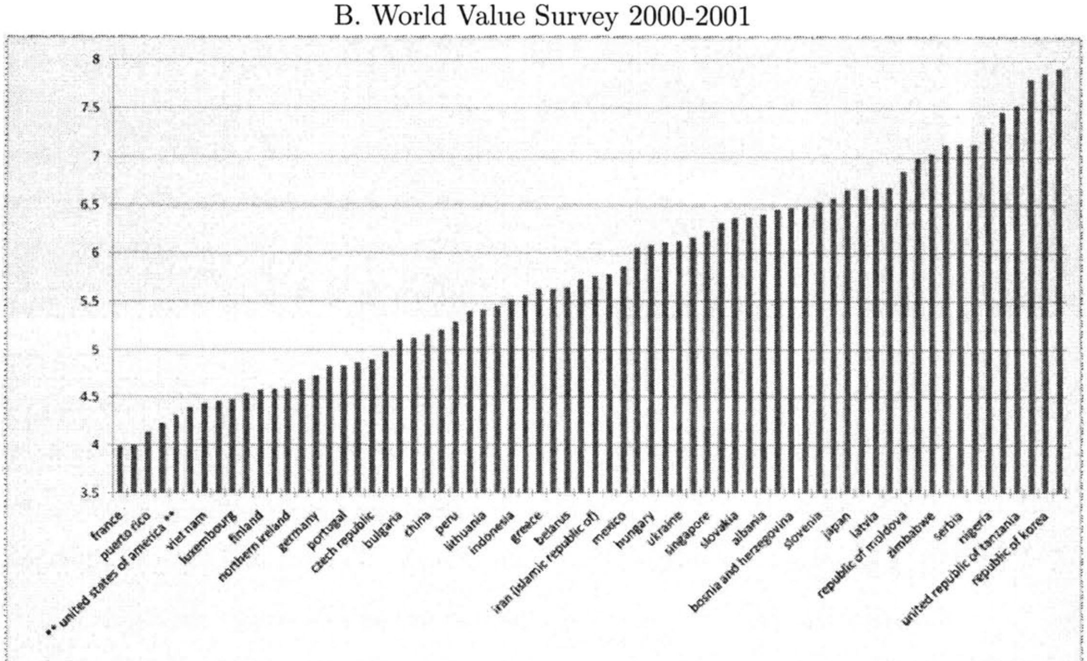
Figure 1. Conservative Americans

A. International Social Survey Program: Role of the Government III 1996



Note: On the y-axis, higher values represent more conservative views, ranging from 1 (agree strongly with the idea of the government's responsibility to reduce income inequality) to 5 (disagree strongly).

B. World Value Survey 2000-2001



Note: On the y-axis, higher values represent more liberal views, ranging from 1 (agree completely with the statement "People should take more responsibility to provide for themselves") to 10 (agree completely with the statement "The government should take more responsibility").

Using multi-country survey data, we can quickly confirm whether Americans are actually more conservative than people in other countries. The upper panel in Figure 1 contains information from the International Social Survey Program (ISSP), Role of Government 1996 module. The question asked if respondents agreed or disagreed that the government had the responsibility to reduce income inequality. To get a brief picture, I simply computed the mean value of the position for each country. A higher score means that more people disagree that the government has such a responsibility and thus represents a more conservative view. As one can see, Americans are more conservative than people in all the other 24 countries where the ISSP was conducted. We get a similar picture from the 2000-2001 World Value Survey. In this survey, the question asked respondents' 10-point position on the government's responsibility for the well-being of its citizens. Again, I calculated the mean value of the answer for each country. This time, a higher score indicates that more people believe that the government should take more responsibility and are thus more liberal. As the lower panel of Figure 1 illustrates, Americans are more conservative than people in most of the other 67 countries. Since immigrants were socialized in other countries before coming to the US, we expect they should be more liberal than US natives. This is referred to as the *American exceptionalism hypothesis.*

The American exceptionalism hypothesis, however, does not go unchallenged. Some researchers suggest that immigrants are a self-selected group who are highly motivated and seek more opportunities abroad (Freeman, 1995; Borjas, 1987). This argument suggests that immigrants are more likely to hunt for equal opportunity and support less taxation and redistribution. Given their willingness to cross borders to find better opportunities, they may even be more conservative than US natives. On the other hand, we can hypothesize that immigrants do not differ from US natives, except in their socioeconomic profiles. The popular image of immigrants is that they tend to be less skilled and poorer than natives. In turn, they are expected to be more liberal than natives; but this difference is expected to disappear once we control for socioeconomic variables. Due to these competing possibilities, whether immigrants are systematically different from US natives in their political views on welfare spending remains an empirical question.

Unlike the above two alternatives that run counter to the American exceptionalism hypothesis, what Borjas called the welfare magnet hypothesis could lead to the same expectation that immigrants should be more liberal; immigrants might move to the US because they are attracted by its social safety nets (Borjas, 1999a; 1999b; 1996). If immigrants are purely welfare seekers, then they are necessarily more liberal regardless of how they were socialized in their countries of origin. This possibility will also be considered in the analysis.

Despite the competing alternative hypotheses, I find that first-generation immigrants are more liberal than US natives. This finding in turn allows us to document whether immigrants retain or modify their political views on welfare while they live in the US. Immigrants may retain a set of values they internalized before emigrating, but they may also be assimilated after they have come. Therefore, immigrants might maintain their more liberal political views or become more like US natives the longer they live in the US.

Once in the US, immigrants acquire political information and become familiar with core norms and values. In the US, individualism is a dominant ideology (McClosky and Zaller, 1984). Scholars argue that even the most egalitarian elites support the idea of equal opportunity (Verba and Orren, 1985). Using open-ended questions in a survey, Feldman and Zaller (1992) found that most people readily come up with the traditional values of individualism and laissez-faire when expressing their welfare policy preferences. In this political culture, immigrants are highly likely to absorb the individualistic ideology through acculturation or socialization. Despite the liberal preferences immigrants generally have, the longer they live in the US the more closely they are expected to adopt the conservative views of US natives. This is referred to as the *first-generation immigrant assimilation hypothesis.*

In line with this interest in how the more liberal views of immigrants change, I analyze the political views of second-generation immigrants. These Americans are in an interesting position. Unlike first-generation immigrants, they have grown up in the US and have thus been socialized in the same way as US natives. Unlike US natives, however, they were raised by non-native parents and thus might have been influenced in various ways. If both socialization and family shape one's political views, then second-generation immigrants are expected to have welfare views that fall between the political views of first-generation immigrants and those of US natives on the conservative-liberal spectrum. This is referred to as the *generational* *socialization hypothesis.*

If the political views of second-generation immigrants are similar to those of first-generation immigrants but not to those of US natives, it means that the more liberal views of first-generation immigrants carry over to the next generation. In this case, first-generation immigrants may not be assimilated into US society and may strongly influence the views of second-generation immigrants. Conversely, if second-generation immigrants have similar views to those of US natives, the political views of first-generation immigrants do not persist in the next generation. This may be due to the socialization of second-generation immigrants, the assimilation of first-generation immigrants into US society, or both. We may have similar expectations about third-generation immigrants; their political views are expected to fall between those of natives and those of second-generation immigrants.

4.4 Data and Measures

To test the hypotheses, this study makes use of three nationwide survey datasets: the 2004 National Annenberg Election Survey (Annenberg hereafter), the American National Election Study (ANES) Cumulative Data File, and the 2006 Cooperative Congressional Election Study (CCES). Each dataset is useful in different ways. First, Annenberg provides the largest foreign-born sample, almost 2,000 observations, so that it can be compared with native-born samples without worrying about representativeness. More importantly, Annenberg's "years of residence in the US" variable enables us to analyze how the political views of these foreign-born respondents change the longer they live in the US. However, this dataset does not distinguish between second-generation immigrant Americans and US natives. Second, we can identify samples of US natives, first-generation immigrants, and second-generation immigrants using the ANES. Unlike the other two datasets, the ANES allows us to measure political views using a wide battery of questions related to welfare spending. However, it does not provide information about how many years immigrants have lived in the US, and questions used to identify native-immigrant status were not asked after 2000. Lastly, CCES has more detailed information about respondents' immigrant status—whether a respondent is a US native or a first-generation, second-generation, or third-generation immigrant. It also distinguishes respondents who are first-generation immigrant citizens from non-citizens. However, it contains only one question relating to welfare spending and does not include the years of residence variable.

For the dependent variable, I picked every question related to support for welfare spending. I then oriented each variable to have more liberal views reflected as higher scores, aggregated them, and created a scale to capture the political views on "welfare spending in general."[6] From Annenberg, I selected two 4-point scaled questions on "government health insurance" and "federal assistance to public schools," and two 2-point and two 5-point questions on the issues of government health insurance for "children" and "workers." From the ANES, I chose two 7-point questions on "government health insurance" and "government guaranteed jobs," and six 3-point questions on "federal spending on the poor, childcare, public schools, welfare programs, solving the problem of the homeless, and food stamps."[7] There was only one relevant question from the CCES, a 5-point scaled question about "guaranteed health insurance for all Americans."

It is important to include all relevant questions in this analysis because there is too much room for manipulation if one selects just one or two particular survey questions. Furthermore, analyzing overall welfare attitudes based on one specific ANES survey item, the 3-point scaled question about "federal spending on welfare programs," is very likely to be misleading. The wording "welfare programs" is too broad for people to define consistently. Most people do not have complete knowledge about what comprises US welfare programs. The vagueness and indefinableness of this wording causes respondents to interpret the question differently.

By aggregating all relevant questions, we can create a scale that represents the political views on welfare spending in general. Scaling also provides an advantage over individual survey items; reducing measurement error. It is well known that measurement error plagues survey responses (Achen, 1975). If the left-hand side variable is measured with error, nonlinear estimators are biased and inconsistent. Even in the case of linear estimation, measurement error in the dependent variable can cause bias and inconsistency if the error is correlated with the error of the model. Even if the correlation might be low, it is always better to construct

[6] I focus on the regression of the scale, but I also report item-by-item regression results in Appendix A.

[7] Note that each question is available for different years from the ANES Cumulative Data File. See Appendix C for the data types and survey years of all ANES, Annenberg, and CCES questions.

a scale because proper scaling reduces unknown measurement errors and prevents arbitrary results (Ansolabehere, Rodden, and Snyder, 2008; 2006; Achen, 1975).

To construct the scale of political views on welfare spending, this study employed factor analysis with all relevant questions, and then standardized the factor score to have a mean of 0 and a standard deviation of 1. For Annenberg data, I used one health insurance and one public schools question to construct a scale. Note there are five items related to health insurance. Of these, I chose a 4-point health insurance question because it provided the largest number of observations and respondents answering this question also answered the 4-point public schools question. For ANES data, scaling was somewhat complicated. While factor analysis is a useful method of scaling, it suffers from many missing values because values missing from any one individual item will leave the factor score blank. This problem becomes acute in the ANES cumulative data file due to changes in content over time. For this reason, I imputed each question using the rest of the questions that were used for aggregation. This was a feasible option because there were eight welfare-related questions in the ANES. However, one must be careful as too many imputations would understate the standard error and thus overstate the test statistics (Allison, 2002). In the end, I used 10 different imputations and subsequent scaling methods while adhering to two important rules: keep as many questions as possible and avoid too many imputations. It turned out that the differences in imputation and scaling methods did not significantly affect the major findings. Therefore, I report the regression results using one method of scaling; the missing values of all eight questions were imputed and scaled for the survey years of 1992, 1994, 1996, and 2000.[8]

4.5 Findings

4.5.1 Political Views of Immigrants Relative to those of US-born Respondents

I first report the findings from the Annenberg survey data. The benchmark model is

$$Y_i = D_i'\alpha + X_i'\beta + \varepsilon_i \tag{4.1}$$

[8]See Appendix B for detailed procedures of all 10 imputations and the subsequent scaling methods. I also provide the regression results using these different scalings.

where Y_i is the dependent variable of political views on welfare spending; X_i is a vector of socioeconomic variables; and D_i is a dummy variable indicating whether a respondent was born in the US or in a foreign country. Therefore, α measures the difference-in-mean (DIM) estimator for the political views on welfare spending of foreign-born respondents relative to those of US-born respondents.

Table 1 presents the coefficient estimates$\hat{\alpha}$ from the regression of the scale constructed using the two 4-point Annenberg items. The unadjusted coefficients, presented in the first column, were obtained simply by differencing the unconditional means.[9] The second column reports the adjusted DIM coefficients estimated by differencing the conditional means, holding other socioeconomic variables constant. The regression model includes a fairly large set of control variables: respondent's age, gender, race dummies (White, Black, Asian, Hispanic, or Native American), dummies of educational attainment, family income, dummies of employment status, household union membership, dummies of church attendance, number of children, marital status, and dummies indicating whether a respondent lives in a central city or a suburban or rural area.

The DIM estimates in Table 1 provide answers to the study's first empirical question. From the upper panel, the unadjusted estimators show that foreign-born respondents are 0.251 units more liberal than US-born respondents (25.1% of the dependent variable's standard deviation). To get a sense of the substantive significance, the estimated difference of 0.251 is equivalent to the effect of a significant reduction in household income from 100K (about 80th-percentile) to 25K (20th-percentile). Compared to socioeconomically equivalent US-born respondents, foreign-born respondents are still more liberal by 0.123 units. The Annenberg survey distinguishes between foreign-born citizens and non-citizens, so that in the lower panel I additionally report the DIM estimates of foreign-born citizens and non-citizens relative to US-born respondents. One can see that foreign-born non-citizens are more liberal than US-born respondents by 0.178 units, even after holding other socioeconomic variables constant. Foreign-born citizens also turn out to be more liberal than US-born respondents, though not as much as foreign-born non-citizens.[10]

[9]See Appendix C for the unconditional mean and the number of observations for the US-born and foreign-born samples.

[10]I also provide regression results of individual items. See Appendix A for the linear probability model (LPM)

Next, I analyze whether the more liberal political views of immigrants are sustained or disappear as they live longer in the US. This can be conjectured, based on the information in Table 1. I test whether the coefficient estimates for foreign-born non-citizens are different from those for foreign-born citizens. The resulting F-statistics are reported next to the DIM estimates for the foreign-born non-citizen dummy. It turns out that foreign-born non-citizens are more liberal than foreign-born citizens, and the difference between them is statistically significant. Generally, foreign-born citizens have lived longer in the US than foreign-born non-citizens; thus, the difference between the two groups may imply that the political views of immigrants are getting closer to those of US-born respondents the longer they live in the US.[11] To analyze the assimilation effect more rigorously, the following model is specified,

$$Y_{fb,i} = R'_{fb,i}\gamma + X'_{fb,i}\beta + u_i \tag{4.2}$$

where, $Y_{fb,i}$ is the dependent variable of political views of immigrants on welfare spending; $R_{fb,i}$ is an immigrant's years of residence in the US, included as a second-order polynomial, and $X_{fb,i}$ is a vector of socioeconomic variables. Note that I first excluded the age variable in $X_{fb,i}$, and then included it as a second-order polynomial to estimate γ as the effect of the years of residence after partialling out the aging effect. The DIM estimates reported in Table 1 verify that foreign-born respondents are more liberal than US natives. Therefore, γ in equation (2) indicates whether the more liberal views of immigrants persist or become more like those of US-born respondents the longer they live in the US, implying assimilation affects their attitudes. The results of estimating equation (2) are reported in Table 2. The unadjusted coefficients appear in the first four columns, while the last four columns contain the adjusted coefficients, holding other socioeconomic variables constant.

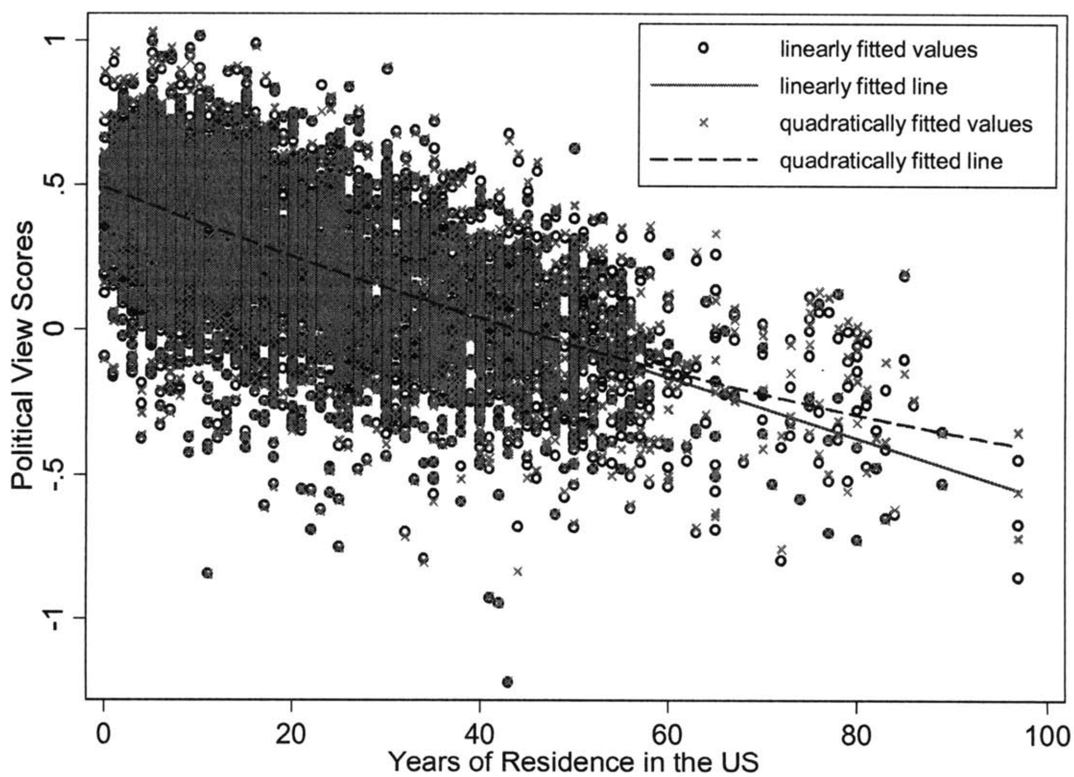
The results displayed in Table 2 clearly show the effect of assimilation on political views on welfare. Immigrants become more conservative the longer they live in the US. This inference remains valid when the specification does not include the age control, as presented in the first, second, fifth, and sixth columns, and even after partialling out the age effect, as shown in the third, fourth, seventh, and eighth columns. The coefficients for the years of residence

and the ordered probit results.

[11]Calculated from Annenberg data, foreign-born citizens and non-citizens have lived in the US for 28.8 and 11.1 years on average (15.5 and 9.3 years standard deviation), respectively.

variable are all significant and are in the same direction, both when it is included as a linear term (displayed in the odd columns), and as a second-order polynomial (reported in the even columns).[12] Figure 2 shows the relationship between immigrants' political views on welfare spending and their years of residence in the US after partialling out all other effects, including age.[13]

Figure 2. Assimilation of Foreign-born Respondents in Political Views on Welfare Spending



Note: On the y-axis, higher values represent more liberal views on welfare spending.

[12]The estimated coefficients for the years of residence squared indicate that the relationship is not curvilinear.

[13]Note that little heterogeneity was found between the foreign-born citizen and non-citizen samples in the coefficient estimates for the years of residence. In linear specifications, the coefficient estimates were -0.00649 and -0.0065 in the restricted sample of foreign-born citizens and that of non-citizens, respectively. Both estimators were significant at the 95% level. In quadratic specifications, the difference was about 0.003, but the relationship does not seem to be curvilinear.

4.5.2 Do Political Views of Immigrants Persist in the Next Generation?

Although foreign-born respondents appear to become more conservative the longer they live in the US, they are in general more liberal than US-born respondents, as the DIM estimates in Table 1 indicate. Now, the question is whether these more liberal views persist in the next generation. If second-generation immigrants' political views are influenced by their non-native parents, then we may say that first-generation immigrants have a long-run impact on US welfare politics by transferring their views to new Americans. However, second-generation immigrants were born in the US and thus have been socialized in American political culture. The socialization and family effects may coexist and one may outweigh the other.

To address this question, I estimate the following equation using the ANES and CCES.

$$Y_i = D_i'\alpha + X_i'\beta + \varepsilon_i \qquad (1')$$

This equation is slightly different from equation (1) in that D_i is now a vector of dummies indicating whether a respondent is a US native, a first-generation immigrant, or a second-generation immigrant. In the ANES, the immigrant dummies were obtained based on two questions, "Where did the respondent grow up?" and "Were the respondent's parents born in the US?" The two questions were available only until 2000, giving a time boundary for the analysis using ANES. From the 2-by-2 typology, I categorized the native-immigrant status of each respondent, as shown in Table 3. I thus set up two dummies, so that $D = [d_1, d_2]'$ where d_1 and d_2 denote whether the respondent is a first-generation immigrant and whether one is a second-generation immigrant, respectively. Unlike the ANES, the CCES contains a question about respondents' immigrant status. Moreover, it reveals two more pieces of information than the ANES does regarding third-generation immigrants and a distinction between first-generation immigrant citizens and non-citizens.[14] Therefore, more immigrant dummies are set up in the vector D_i using CCES.

[14]The actual wording of the question in the CCES has different coding and labels from those I use in this study. Respondents were asked to check one of the following: 1) Immigrant Citizen (if the respondent is an immigrant and a naturalized citizen), 2) Immigrant non-citizen, 3) First generation (if the respondent was born in the US but at least one parent is an immigrant), 4) Second generation (if the respondent and his/her parents were born in the US but at least one grandparent was an immigrant), and 5) Third generation (if the respondent, his/her parents, and grandparents were all born in the US). In this study, I label the codes 1 and 2 above as first-generation immigrants. Likewise, the codes 3 and 4 are labeled as second-generation and third-generation, respectively. Code 5 is US natives.

The vector X_i consists of almost the same control variables as those used in Annenberg. In the ANES, I additionally included a home ownership dummy, but I could not include the number of children variable because it was not asked every year.[15] In the CCES, the number of children variable was available without inducing too many missing values. However, there was no variable indicating whether respondents live in an urban or rural area.

Turning first to the findings from the ANES cumulative data, Table 4 presents the coefficient estimates from the regression of the scale constructed with all eight ANES items. The unadjusted estimates were obtained by the difference in unconditional means, while including survey year dummies in the model.[16] These are presented in the first column. The second column contains the adjusted estimates, holding other socioeconomic variables and time fixed effects constant.

The DIM estimators in Table 4 verify what was already found by Annenberg. This time, the base category is US natives, which does not include second-generation immigrants. The unadjusted estimates show that first-generation immigrants are 0.214 units more liberal than US natives.[17] After controlling for other socioeconomic differences, immigrants are still more liberal by 0.186 units. The political views of second-generation immigrants on welfare spending are in between those of first-generation immigrants and those of US natives, but much closer to the latter. By computing the difference between the DIM estimators of the first- and second-generation dummies relative to US natives, one can see that second-generation immigrants are 0.181 units more conservative than first-generation immigrants. Even after holding other socioeconomic differences constant, second-generation immigrants are 0.136 units more conservative than first-generation immigrants. As shown in the F-statistics next to the coefficients on the second-generation immigrant dummy, the difference between first- and second-generation immigrants' political views are statistically significant, both in unadjusted and adjusted estimates. In contrast, the difference between the political views of second-generation immigrants and those of US natives is small and statistically insignificant. Second-generation immigrants turn out to be 0.033 units more liberal than natives and, after controlling for socioeconomic

[15] Including the number of children variable caused too many missing values, up to 80% data loss.

[16] See Appendix C for the unconditional mean and sample size of each category of immigrants and natives.

[17] The coefficient estimate of 0.214 can be compared to the effect of losing family income. In the ANES, moving from the "33th-67th percentile" income category to the "17th-33th percentile" category increases support for welfare spending by 0.161.

backgrounds, 0.037 units more liberal.[18]

Next, I report the findings from the CCES. The CCES has only one health insurance question available, but it has more detailed information on respondents' immigrant status—whether the respondent is a US native, a first-generation, second-generation, or third-generation immigrant. It also distinguishes respondents who are first-generation immigrant citizens from non-citizens.[19]

Table 5 shows the linear probability model results of the standardized response for the question on "guaranteed health insurance for all Americans."[20] The unadjusted coefficients are reported in the first column, and the second column contains the adjusted coefficients.[21] Next to the estimated coefficients for the second-generation immigrant dummy, I report the F-statistics of whether first-generation immigrants differ from second-generation immigrants in their political views. Likewise, the F-statistics located next to the coefficients for the third-generation dummy show whether second-generation immigrants are different from third-generation immigrants. Finally, I test whether immigrant citizens are different from immigrant non-citizens, and report the resulting F-statistics next to the coefficients for the immigrant citizen dummy.

The results shown in Table 5 are very similar to the ones from the ANES data. Both unadjusted and adjusted estimators show that first-generation immigrants are more liberal than US natives. Of the first-generation immigrants reported in the lower panel, immigrant non-citizens turn out to be much more liberal than US natives. Immigrant citizens are also more liberal than US natives, though not as much as immigrant non-citizens. The difference between immigrant citizens and non-citizens in their political views was statistically significant, as the F-statistics indicate. The political views of second-generation immigrants are between those of first-generation immigrants and those of US natives, but again are closer to the latter. The third-generation immigrants turn out to be almost identical to US natives in their political views.

[18]See Appendix A for the regression results of the eight individual items. I provide both LPM and ordered probit estimates.

[19]Note that immigrant non-citizens were under-sampled in the CCES; there were 95 samples in the unadjusted specification and only 73 in the adjusted one. This is due to the sample design of the CCES that oversampled registered voters. For the sample design of the CCES, go to the CCES website at http://web.mit.edu/polisci/portl/cces/sampledesign.html.

[20]For the results of the ordered probit model, please see Appendix A.

[21]See Appendix C for the unconditional mean and sample size of each category of immigrants and natives.

The findings from the Annenberg, ANES, and CCES have suggested answers to the empirical puzzles of this study. First, first-generation immigrants are more liberal than US natives in their political views on welfare spending, as shown in Tables 1, 4, and 5. Second, the more liberal political views of first-generation immigrants are apparently not sustained. As presented in Table 2, first-generation immigrants become more conservative the longer they live in the US, suggesting the effects of assimilation. Third, the political views of second-generation immigrants lie between those of first-generation immigrants and those of US natives, but are much closer to the latter. Second-generation immigrants are influenced by their non-native parents at home, but at the same time are socialized from birth in the same way as US natives. Therefore, these findings imply that the assimilation of first-generation immigrants and/or the socialization of second-generation immigrants hinder the more liberal political views of first-generation immigrants from persisting in the next generation. Fourth, results from the CCES in Table 5 demonstrate that third-generation immigrants are almost identical to US natives in their political views. This may strengthen the argument that the more liberal views of immigrants may completely vanish in one generation or so.

4.5.3 First-generation Immigrants in the 1960s and 1970s

One may raise the concern that the group of current first-generation immigrants does not perfectly overlap with that of the non-native parents of current second-generation immigrants. The conclusion that immigrants' political views do not persist in the next generation would not change if the political views of these non-native parents were more than or equal to those of the current first-generation immigrants. However, if these non-native parents were not as liberal as the current first-generation immigrants, one might counter that the second-generation immigrants' conservative views (relative to those of the first-generations') could have been derived not only from socialization but also from their non-native parents' relatively conservative views. This suggests that immigrant views might indeed persist in the next generation.

To address this concern, we need to measure the political views of first-generation immigrants using 1960s or 1970s datasets and compare the views of this group of possible non-native parents of current second-generation immigrants with those of current first-generation immigrants measured using 1990s datasets. Unfortunately, not many welfare-related questions were asked in the 1960s and 1970s; however, I was able to use some ANES questions that were asked both in the 1970s and 1990s surveys—those covering the respondents' 7-point positions on "government health insurance" and "government guaranteed jobs." Moreover, two essentially same ANES questions were asked in the 1960s surveys: these questions are binary-coded and this is the only difference from the abovementioned 7-point questions.[22] These questions allow us to determine whether the views of first-generation immigrants in the 1960s and 1970s were as liberal as those of current first-generation immigrants. The factor analysis is again employed to aggregate these two questions in each of the 1960s, 1970s, and 1990s datasets.[23]

Table 6 reports the regression results of the scales. The first and second columns present the estimated coefficients using 1960s datasets. The DIM estimators in the third and fourth columns are obtained using the 1970s datasets. Likewise, the last two columns represent the DIM estimators using 1990s surveys. Note that the estimated coefficients of the scales in Table 6 cannot be compared to those in Table 4 because the scales in each table are constructed using different items.

Table 6 is useful to check whether first-generation immigrants in the 1960s and 1970s were as liberal as those in the 1990s. The regression results show that first-generation immigrants were more liberal than US natives not only in the 1990s, but also in the 1960s and 1970s. Note that the gap in political views between first-generation immigrants and US natives was far larger in the 1970s. This may be due to the noticeable stagflation during the 1970s in the US. Previous research has shown that anti-poor sentiment increased in the 1970s because the macro-economic problem resulted in psychological stress and self-defensiveness (Schlozman and Verba, 1979). The very large DIM coefficients in the 1970s may reflect the anti-poor sentiments of US natives in general or the more desperate welfare needs of immigrants caused by natives' self-defensiveness, or both.[24] Also note that in the 1960s datasets, the political views of second-generation immigrants were closer to those of first-generation immigrants.[25]

[22]For the data types and survey years of these questions, please see Appendix C.

[23]See Appendix A for the individual item-by-item regression results.

[24]Note that the standardized unconditional mean of the scale for US natives is smaller in the 1970s than in the 1990s (reported in Appendix C). On the other hand, those of the scale for immigrants are larger in the 1970s. I also note that the regression coefficients for the other control variables using 1970s datasets are generally higher than those using 1990s datasets, reflecting the generally different historical context in the 1970s. Readers may obtain the regression table reporting all coefficients for the control variables upon request.

[25]It might simply reflect the more liberal welfare views of second-generation immigrants in the 1960s, or the binary-coding of the questions in the 1960s datasets might affect the magnitude of coefficients as it cannot

Even after taking all this into consideration, at least one thing can be argued: first-generation immigrants in the 1960s and 1970s, which is the group of possible non-native parents of current second-generation immigrants, were at least as liberal as current first-generation immigrants. This helps conclude that the more liberal views of immigrants do not persist in the next generation; the political views of second-generation immigrants are conservative even though their non-native parents have liberal political views.

Table 6 is also useful to rule out a possibly different interpretation of the coefficient estimates of the years of residence reported in Table 2. One might claim thatγ in equation (2) does not measure the effect of assimilation. Rather, the significant and negative$\hat{\gamma}$ could indicate that immigrants who came to the US in an earlier period were more conservative than those who came in a later period. The results reported in Table 6 show that immigrants who came to the US 30 years ago were actually more liberal than US natives, excluding this possibly different interpretation of $\hat{\gamma}$ in Table 2.

4.5.4 Socialization of Second-generation Immigrants

The more liberal political views of immigrants may not persist in the next generation. Second-generation immigrants are influenced by non-native parents at home, and at the same time are socialized in the same way as US natives. Therefore, the transience of the first-generation immigrants' political views may be attributed to the assimilation of first-generation immigrants and/or the socialization of second-generation immigrants. In Table 2, we have already seen that first-generation immigrants are increasingly assimilated into US society the longer they live in the US. The next question is whether second-generation immigrants are in fact as socialized as US natives in terms of political views on welfare spending.

To address this question, I utilized cross-state differences in the political views of US natives. The basic idea is that if second-generation immigrants are socialized in the same way as US natives, they should be more liberal in states where US natives are more liberal than second-generation immigrants in states where US natives are conservative in their political views on welfare spending.[26]

capture the variations of different opinions.

[26]In previous research on political incorporation, the age variable has often been employed to measure the socialization of US-born populations (Cain, Kiewiet, and Uhlaner, 1991, Shively, 1979). However, there is an

I proceeded through the following steps. First, I restricted samples to US natives only, and ran the regression of the political views on a vector of state dummies and the usual socioeconomic controls. Second, ranking was given to states, from the most liberal state (the state where US natives are the most liberal in their political views on welfare spending) to the most conservative state (the state where US natives are the most conservative). Third, based on the ranking, the samples were divided into two categories: liberal states and conservative states. The conventional wisdom of half-and-half was used, but I also chose the top 40% and bottom 40%, disregarding the unclear middle. Fourth, I created a dummy that equaled 1 if a respondent was a resident of a liberal state (the top 50% when using half-and-half, or top 40% when disregarding the unclear middle), and that otherwise equaled 0 (the bottom 50% or bottom 40%). Finally, I ran the regression of the political views on the liberal state dummy for each set of US native, first-generation immigrant, and second-generation immigrant samples. The specification is thus

$$\begin{aligned} Y_{us,i} &= L'_{us,i}\delta_{us} &&+ X'_{us,i}\beta_{us} &&+ w_{0,i} \\ Y_{first,i} &= L'_{first,i}\delta_{first} &&+ X'_{first,i}\beta_{first} &&+ w_{1,i} \\ Y_{second,i} &= L'_{second,i}\delta_{second} &&+ X'_{second,i}\beta_{second} &&+ w_{2,i} \end{aligned} \tag{4.3}$$

where, Y_i is the dependent variable of the political views on welfare; L_i is a dummy indicating whether a respondent lives in a liberal state (the top 50 % or top 40%) or not (bottom 50% or bottom 40%); and X_i is the socioeconomic control structure. Using restricted samples of US natives, I estimated the first line of equation (3). Likewise, I estimated the second and third lines of equation (3), being restricted to using the first- and second-generation immigrant samples.[27]

Table 7 presents the estimates of coefficients in equation (3). All estimates are adjusted,

inevitable limitation of this variable since age in and of itself, representing a social and economic position in the life cycle, affects political views on welfare spending. As an alternative, I tried to trace the changes in second-generation immigrants' political views using the 1972-1976, 1992-1996, and 2000-2004 ANES panels. However, the picture was unclear; there was little change in political views while the associated variances were large. These panels are not useful for measuring socialization anyway, because we have only four years to track changes.

[27]One can think of the above equation structure as a Seemingly Unrelated Regression (SUR) set-up, but since all equations have exactly the same right-hand side variables, SUR estimates are numerically the same as equation-by-equation OLS estimates.

holding other socioeconomic variables constant. The upper panel reports$\hat{\delta}$'s using the criterion of $L = 1$ if a respondent lives in a liberal state (top 50%); otherwise, $L = 0$. The DIM estimators of the respondents residing in the top 40% relative to the respondents in the bottom 40% are displayed in the lower panel. Using the Annenberg survey, the first and second columns report the $\hat{\delta}_{us}$, the estimated coefficients for US-born respondents residing in liberal states relative to US-born respondents in conservative states, and the $\hat{\delta}_{first}$, coefficients for first-generation immigrants in liberal states relative to first-generation immigrants in conservative states, respectively. Estimators using ANES are displayed in the third, fourth, and fifth columns, and CCES results are in the last four columns. Note the $\hat{\delta}_{us}$'s presented in the first, third, and sixth columns. Since L was constructed based on the ranking of the liberality of US natives, $\hat{\delta}_{us}$'s are necessarily positive and significant.

From the coefficient estimates using ANES in the third and fifth columns, we see a clear pattern that second-generation immigrants are (socialized to be) more liberal in states where US natives are more liberal than second-generation immigrants in states where US natives are conservative. This pattern remains valid when the top 40% and bottom 40% are compared, tossing out the unclear middle. This may indicate that second-generation immigrants are socialized in a similar way as US natives.

First-generation immigrants may also be (assimilated to be) more liberal in the states where US natives are liberal than are first-generation immigrants in conservative states. However, this pattern is not expected to be clear because first-generation immigrants may also retain the norms and values they internalized while growing up in foreign countries before emigrating. The estimates of $\hat{\delta}_{first}$ using Annenberg and the ANES all show this pattern. First-generation immigrants tend to be more liberal in states where US natives are more liberal than first-generation immigrants in conservative states, but the pattern is less clear, as reported in the second and fourth columns.

Unfortunately, the findings in this section are less robust when the CCES is used. The estimates presented in the last four columns do not clearly show that first- and second-generation immigrants are more liberal in liberal states. Yet, the third-generation immigrants' cross-state difference in political views seems to follow the pattern of US natives; they are more liberal in states where US natives are more liberal than third-generation immigrants in conservative states.

4.5.5 Welfare Tourists?

In previous sections, the reasoning as to why first-generation immigrants are expected to be more liberal than US natives was based on the facts that (1) Americans are more conservative than people in other countries in their welfare attitudes and (2) immigrants are born and socialized in other countries. However, there is another possibility that may have the same empirical expectation. Some economists and populist commentators have proposed the welfare magnet hypothesis, claiming the US welfare system may affect immigrants' decision to move (e.g. Borjas, 1996); immigrants are single-minded welfare-seekers, or at least believe that US social safety nets would protect them from potential failure. If immigrants behave in this way, they must necessarily support welfare spending.

In this section, I provide rough evidence against this possibility. Note the aim is not to assess if the welfare magnet hypothesis itself is true. Rather, the aim is to test whether the more liberal views of first-generation immigrants could be explained by the welfare magnet hypothesis. Two ways of testing were employed, first utilizing low-income immigrants' political views and then utilizing the availability of state assistance to immigrants.

Though not perfect, low-income first-generation immigrants are a good proxy for the group of immigrants who might be attracted by the US welfare system. We can analyze whether these low-income immigrants are driving the more liberal political views of immigrants. If this group is a driving force of the DIM estimates reported in Tables 1, 4, and 5, it might be difficult to rule out the possibility of the welfare magnet hypothesis.

The first panel in Table 8 shows DIM estimates analogous to those in Table 1, split by low versus high income using the Annenberg survey. A threshold of $50,000 was chosen to distinguish between low and high income because it was the median both in the Annenberg and the Current Population Survey (CPS) samples. Using the ANES data, the second panel presents the DIM estimators corresponding to those in Table 4, divided by low versus medium-high income levels. The income level in the ANES cumulative data file was recoded to a 5-point scale, where 0 = "0-16 percentile," 1 = "17-33 percentile," 2 = "34-67 percentile," 3 = "68-95 percentile," and 4 = "96-100 percentile." The 33rd percentile was chosen as the threshold for the low versus medium-high income, because a median is not available and the 67th percentile is not really a good threshold to collect low-income respondents. The two lower panels report the DIM estimators analogous to those in Table 5, broken down by low versus medium-high income using the CCES data. I chose two thresholds for the income level; one is $50,000, which is about the 33rd percentile in the CCES and the median in the CPS, and the other is $70,000, which is approximately the median in the CCES and the 70th percentile in the CPS.[28]

All datasets give essentially the same results regarding this issue; the more liberal views of first-generation immigrants are not driven by low-income immigrants. This indicates that the more liberal views of immigrants cannot be explained by the welfare magnet hypothesis. The adjusted DIM coefficients reported in the third and fourth columns in the two upper panels of Table 8 show that there is little heterogeneity. In the two lower panels, the adjusted coefficients in the third and fourth columns show that there is noticeable heterogeneity, but not in the direction of our concern. The difference between medium-high income immigrants and medium-high income natives is even greater than the immigrant-native difference in low-income samples.

For the second way of testing, I utilized the availability of state assistance to immigrants. The basic idea is similar to the one in the previous section: if first-generation immigrants are more liberal because they are welfare seekers, then they should be more liberal, particularly in the states where assistance is available to immigrants and non-citizens, than first-generation immigrants in states where social safety nets are available exclusively to citizens. Following this logic, the specification is almost the same as equation (3), except that $L = [L_{us}, L_{first,} L_{second}]'$ is now a vector of dummies indicating whether respondents live in states where welfare benefits are less or more available to immigrants.

Two sources from the Urban Institute were used to determine the level of state assistance to immigrants and non-citizens. First, Zimmerman and Tumlin (1999) categorized all states' safety nets available to immigrants into four levels: "Most Available," "Somewhat Available," "Less Available," and "Least Available."[29] Second, I constructed an index of state welfare avail-

[28]More high-income respondents were sampled in the CCES than the population data (CPS) suggested. This may be due to the sample design of the CCES. Although high-income respondents were oversampled, there are no population coverage or sample selection problems in this survey. For the survey design of the CCES, please refer to the website at http://web.mit.edu/polisci/portl/cces/sampledesign.html.

[29]Zimmerman and Tumlin's index is based on welfare rules of all states as of 1998.

ability to immigrants based on three tables from Rowe, Murphy, and Williamson (2006).[30] Each table indicated whether a state's assistance was fully available, partially available, or not available at all to 4 to 5 categories of immigrants.[31] I assigned a 1 to each category of immigrants if a state's assistance was fully available, 0.5 if partly available, and 0 if not available. All scores were then added up for each state. I set $L = 1$ if a respondent lives in a state that had total scores of 11 and above, and$L = 0$ if a respondent lives in a state having total scores of 10 and below. By choosing this threshold, we can split all states into the top 50% and bottom 50% in terms of the availability of state assistance to immigrants. We can also compare a group of states having 12 and above with another having 9 and below, discarding the unclear middle.

The upper panel in Table 9 reports the coefficient estimates from the regression of the political views on a vector of dummies indicating whether a respondent lives in a state where safety nets are the least, less, somewhat, or the most available to immigrants, using the categories of Zimmerman and Tumlin (1999). Using the index created based on the three tables from Rowe, Murphy, and Williamson (2006), the lower panel shows the estimates from the regression on a dummy that equals 1 if a respondent lives in a state where assistance is more available, and that equals 0 otherwise.

One can clearly see that there is not much shift in the political views of first-generation immigrants. As shown in the second, fourth, and seventh columns, first-generation immigrants are not more liberal in states where safety nets are more available to immigrants and non-citizens than first-generation immigrants in states where assistance is less available. Coefficients estimated from the ANES even indicate that first-generation immigrants are more conservative in states where safety nets are more available than first-generation immigrants in states where safety nets are less available. These findings from Table 9 may suggest that the welfare magnet

[30]The three tables are "Eligibility of Nonexempt, Pre-PRWORA, Qualified Aliens," "State Funds to Help Non-citizens Who Entered after Enactment and Are Ineligible for TANF," and "Eligibility of Nonexempt, Post-PRWORA, Qualified Aliens after Five Years." All were created based on the Urban Institute's Welfare Rules Data as of 2004. See Appendix D for a list of states in each Zimmerman and Tumlin (1999) category and the three tables from Rowe, Murphy, and Williamson (2006).

[31]Categories of immigrants in the first and third tables mentioned in the previous footnote are (1) lawful permanent residents, (2) parolees, (3) deportees, (4) asylees/refugees, and (5) battered non-citizens. Categories in the second table are (1) lawful permanent residents, (2) parolees, (3) battered non-citizens, and (4) non-qualified aliens. Please see Appendix D for the definitions of each category.

hypothesis is not the reason first-generation immigrants are more liberal.[32]

4.5.6 The Effects of National Origin

Immigrants are born and raised in many different countries before moving to the US. Accordingly, they are socialized in different ways, depending on their national origin. Therefore, I run an additional regression, as follows.

$$Y_i = C_i'\zeta + X_i'\beta + v_i \tag{4.4}$$

where Y_i is the scale of political views on welfare spending; C_i is a vector of national origin dummies indicating whether a respondent grew up in the US, Western Europe, Latin America, Asia, Africa, or Eastern Europe; and X_i is a vector of socioeconomic variables.

The national origins of respondents were identified using the ANES questions "Where did the respondent grow up?" and "Where was the respondent born?" National origin was categorized by continent primarily because of the small sample size of first-generation immigrants in the ANES. Even so, each continent of origin dummy does not have a large sample size, and therefore, any finding in this section should be considered tentative.[33]

Table 10 reports a set of DIM coefficients. The first column presents unadjusted estimators. Adjusted estimators are displayed in the second column.

The reported unadjusted estimators for the Western Europe dummy indicate that the political views of Western European immigrants are almost the same as those of US natives. Interestingly, however, these immigrants are more liberal than socioeconomically equivalent US natives. This finding supports the idea that individualism distinguishes Americans' views on welfare from those of Western Europeans (Alesina, Glaeser, and Sacerdote, 2001). There is an interesting finding about immigrants from Latin America. They have large positive unadjusted coefficients, but these disappear after controlling for other socioeconomic variables. Therefore,

[32]Note the estimates in US native samples reported in the first, third, and sixth columns. Interestingly, they are generally more liberal in the states where assistance is more available to immigrants. This may indicate that these relatively more liberal US natives may have supported, or at least may not have opposed, their state's assistance to immigrants and non-citizens. They may have been more liberal concerning welfare, not only for themselves but also for immigrants and non-citizens.

[33]The survey contains 94 immigrant samples from Asia, 90 from Latin America, and 42 from Western Europe. Other than these, it includes fewer than 25 samples for the rest of the continent categories. See Appendix C for the sample size and unconditional mean of immigrants from different continents.

it should be said that Latin American immigrants are more liberal than US natives, but this is entirely due to differences in socioeconomic status. The rest of the estimated coefficients are statistically insignificant and thus inconclusive. From the adjusted estimators, one can only surmise that immigrants from Eastern Europe and Asia (as well as other unidentified countries) are (insignificantly) more liberal than socioeconomically equivalent US natives.

4.6 Conclusions

This chapter contributes to our understanding of three important areas of research: welfare attitudes, immigrant political behavior, and socialization. Previous research on why some people support welfare policies more than others has mostly focused on individualistic ideology and economic self-interest while ignoring the immigration context. In the meantime, immigration literature has extensively addressed natives' attitudes toward immigrants. Only recently have researchers analyzed the political behavior of immigrants themselves, but the focus has mostly been on partisan preferences and voting behavior. In this chapter, I examined whether and to what extent first-, second-, and third-generation immigrants differ from US natives on the crucial issue of welfare policy preferences. Furthermore, I exploited the unique situations of immigrants that are distinguishable from those of natives—first-generation immigrants' exposure to a new political system in the US and second-generation immigrants' having non-native parents at home—to analyze the effects of assimilation and socialization.

Several hypotheses were tested using three nationally representative surveys. Beginning with the first empirical puzzle, research on American exceptionalism offers a prior belief that immigrants are more liberal than US natives. Despite other alternative hypotheses, such as self-selection and no native-immigrant difference, the American exceptionalism hypothesis is well substantiated, establishing that first-generation immigrants are more liberal than US natives in their political views on welfare spending. I also provided evidence that the more liberal views of first-generation immigrants are not the result of immigrants' being welfare tourists. Low-income immigrants, who may be attracted by welfare benefits, are not particularly pulling the DIM estimates in the liberal direction. In some data, high-income immigrants turn out to be even more liberal than comparably situated natives. Similarly, no difference was found in welfare attitudes between immigrants living in states where welfare assistance is available exclusively to citizens and those living in states where immigrants and non-citizens are also eligible for benefits.

Once the native-immigrant difference was established, I used the amount of time immigrants have lived in the US and their generational status to document the effects of assimilation and socialization. In line with the first-generation immigrant assimilation hypothesis, I found that the longer first-generation immigrants live in the US, the more conservative they become, similar to US natives. Regarding the generational socialization hypothesis, I found that the political views of second-generation immigrants fall between those of first-generation immigrants and those of US natives, but are much closer to the latter. Together with the finding that third-generation immigrants have almost identical views to those of US natives, this study suggests that the more liberal political views of first-generation immigrants do not persist in the next generation; they fade away in one generation or so. Where the data permits, I presented some evidence that second-generation immigrants are socialized in the same way as US natives. Therefore, the transience of the more liberal views of immigrants may be due to (1) the assimilation of first-generation immigrants and (2) the socialization of subsequent-generation immigrants.

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Appendix A: Analysis with Individual Survey Items

I provide the results of the item-by-item linear probability model (LPM) and the ordered probit model as well, since the data types of all questions are ordered discrete.

Again, the basic model is

$$Y_i = D_i'\alpha + X_i'\beta + \varepsilon_i \tag{A1}$$

where, Y_i is each individual dependent variable of the support for welfare spending; D_i is a vector of immigrant dummies indicating whether the respondent is a US native, a first-generation immigrant, a second-generation immigrant, or a third-generation immigrants; and X_i is a vector of socioeconomic variables.

In Tables A1 through A3, I report the coefficients and marginal effects of changes in the immigrant dummies on the probabilities of

$$\begin{aligned}
\Pr(Y = 0|Z) &= \Phi(\lambda_0 - Z'\gamma) \\
\Pr(Y = 1|Z) &= \Phi(\lambda_1 - Z'\gamma) - \Phi(\lambda_0 - Z'\gamma) \\
\vdots \qquad & \qquad \vdots \qquad\qquad\qquad \vdots \\
\Pr(Y = M|Z) &= 1 - \Phi(\lambda_{M-1} - Z'\gamma)
\end{aligned}$$

where, $Z = \left[\begin{smallmatrix} D \\ X \end{smallmatrix}\right]$ and X does not include a constant term. I excluded a constant term in the ordered probit model, even in the presence of a binary dependent variable; $\gamma = \left[\begin{smallmatrix} \alpha \\ \beta \end{smallmatrix}\right]$; M is an integer, ranging from 1 to 6, depending on how many orders each individual dependent variable has; and λ_i's are unknown cut-point parameters to be estimated. Note that $-\lambda_0$ in a no-constant specification is equal to a constant term in a model with a constant.

The marginal effects of interest are

$$\Pr(Y=0|Z,D=1)-\Pr(Y=0|Z,D=0)=\Phi(\hat{\lambda}_0-\bar{X}'\hat{\beta},D=1)-\Phi(\hat{\lambda}_0-\bar{X}'\hat{\beta},D=0)$$

$$\Pr(Y=1|Z,D=1)-\Pr(Y=1|Z,D=0)$$
$$=[\Phi(\hat{\lambda}_1-\bar{X}'\hat{\beta},D=1)-\Phi(\hat{\lambda}_0-\bar{X}'\hat{\beta},D=1)]-[\Phi(\hat{\lambda}_1-\bar{X}'\hat{\beta},D=0)-\Phi(\hat{\lambda}_0-\bar{X}'\hat{\beta},D=0)]$$

$$\vdots \qquad \vdots \qquad \vdots \qquad \vdots$$

$$\Pr(Y=M|Z,D=1)-\Pr(Y=M|Z,D=0)=-[\Phi(\hat{\lambda}_{M-1}-\bar{X}'\hat{\beta},D=1)-\Phi(\hat{\lambda}_{M-1}-\bar{X}'\hat{\beta},D=0)]$$

It is up to readers how to interpret the marginal effects. Note, however, that the interpretation of the marginal effect on the probability in the middle (neutral position; $\Pr(Y=3)$ for the 7-point DVs, $\Pr(Y=2)$ for the 5-point DVs, and $\Pr(Y=1)$ for the 3-point DVs) is not clear enough.

Another model specification is

$$Y_{fb,i}=R'_{fb,i}\gamma+X'_{fb,i}\beta+u_i \tag{A2}$$

where, $R_{fb,i}$ is the variable indicating a first-generation immigrant's years of residence in the US that is included as a second-order polynomial.

The LPM estimates are also reported in Tables A4 through A7. Note that I standardized each individual item to have a mean of 0 and a standard deviation of 1 for LPM estimates.

Appendix B: Imputation and Scaling for ANES Cumulative Data

For imputation and subsequent scaling, this study kept two crucial rules: (1) as many questions as possible to address concern over the possible manipulation and measurement errors of individual items, and (2) avoiding too many imputations that would cause an understated standard error and overstated test statistics. As one can see in Table C1, this study selected 8 ANES questions that were asked in different survey years. While I maintained the two important rules above, I used 10 different methods of imputation based on the availability of questions in each survey year.

92 and 96: All 8 questions are available in 92 and 96.

1) No imputation.

2) With imputation for all 8 questions.

92, 96, and 00: Excluding the question of "Respondent's support for federal spending on solving the problem of the homeless," all the other 7 questions are available in 92, 96, and 00.

1) No imputation.

2) With imputation for the 7 available questions and no imputation for the excluded question.

3) With imputation for all 8 questions including the excluded one.

92, 94, 96, 00: Excluding the questions of "Respondent's support for federal spending on the poor" and "Respondent's support for federal spending on solving the problem of the homeless," all the other 6 questions are available in 92, 94, 96, and 00.

1) No imputation.

2) With imputation for the 6 available questions and no imputation for the excluded questions.

3) With imputation for the 7 questions, including the excluded "Homeless question" and no imputation for the excluded "Poor question."

4) With imputation for the 7 questions, including the excluded "Poor question" and no imputation for the excluded "Homeless question."

5) With imputation for all 8 questions, including both excluded questions.

I report the data recovery rate by each imputation in Table B1. No further expansion of survey years is suggested because doing so would not only impute too much data but also cause too many questions to be lost and thereby hinder the representative quality of the scale. The regression results using all 10 different imputation and subsequent scaling methods are reported in Table B2-B3. Note that I standardized each scale to have a mean 0 and a standard deviation 1. The differences in imputation and scaling did not significantly affect the major findings.

Appendix C: Dependent Variables

Table C1 presents the data types and survey years of all ANES, Annenberg, and CCES questions. The unconditional means along with the numbers of observations for the US-born and foreign-born samples are displayed in Tables C2, C3, and C4.

Appendix D: Availability of State Assistance to Immigrants

This study used two sources from the Urban Institute to determine the level of state assistance to immigrants. First, Zimmerman and Tumlin (1999) categorized the availability to immigrants of all states' safety nets into four levels; "Most available," "Somewhat available," "Less available," and "Least available." Here the states in each category are listed.

Most Available: California, Illinois, Maine, Maryland, Massachusetts, Missouri, Nebraska, Rhode Island, Washington

Somewhat Available: Connecticut, Florida, Hawaii, Minnesota, New Jersey, New York, Oregon, Pennsylvania, Vermont, Wisconsin

Less Available: Alaska, Arizona, Colorado, Delaware, District of Columbia, Georgia, Iowa, Kansas, Kentucky, Michigan, Montana, Nevada, New Hampshire, New Mexico, North Carolina, North Dakota, Tennessee, Utah, Virginia, Wyoming

Least Available: Alabama, Arkansas, Idaho, Indiana, Louisiana, Mississippi, Ohio, Oklahoma, South Carolina, South Dakota, Texas, West Virginia

Second, I constructed an index of state welfare availability to immigrants based on the three tables from Rowe, Murphy, and Williamson (2006). The three tables are combined and displayed in Table D.

Table 1. Difference-in-Mean Estimates of the Scale: National Annenberg Election Survey

	Unadjusted		Adjusted	
US-Born Vs Foreign-Born				
Foreign-born	0.251***		0.123***	
	(0.0202)		(0.025)	
TWO Foreign-Born Respondents				
Foreign-born Citizens	0.156***		0.097***	
	(0.0272)		(0.0303)	
Foreign-born Noncitizens	0.401***	F-stat = 46.75	0.178***	F-stat = 4.40
	(0.0253)		(0.0319)	
Observations	21483		19094	
R^2	0.005		0.101	

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. Estimates of unadjusted specification are the same as the differences in unconditional mean. Adjusted specification holds other socioeconomic variables constant. The other socioeconomic variables include respondent's age, gender, race dummies, dummies of educational attainment, family income, dummies of employment status, union membership, dummies of church attendance, the number of children dummies, marital status, and dummies indicating whether the respondent lives in an urban, a suburban, or a rural area.

Table 2. Assimilation of first-generation Immigrants

	Unadjusted				Adjusted			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Yrs of Residence	-0.0098***	-0.0090***	-0.0097***	-0.0103***	-0.0091***	-0.0101***	-0.0089***	-0.0109***
in the US	(0.0013)	(0.0034)	(0.0017)	(0.0036)	(0.0016)	(0.0038)	(0.0019)	(0.0041)
Yrs squared		-0.000014		0.0000145		0.000018		0.000036
		(0.000060)		(0.000066)		(0.000067)		(0.000071)
Age Control			Yes	Yes			Yes	Yes
Age squared Control				Yes				Yes
Socioeconomic Controls					Yes	Yes	Yes	Yes
Observations	1871	1871	1860	1860	1643	1643	1638	1638
R^2	0.037	0.037	0.037	0.037	0.12	0.12	0.121	0.121

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3. Immigrant Dummies Classification

		Parents Native Born	
		US Native Born	Foreign Born
Respondent Grew up in	US	US Natives	Second Generation Immigrants
	Foreign country	Not classified	First Generation Immigrants

Note: The data of "Where was the respondent born?" were interpolated into "Where did the respondent grow up?" if the data are missing. Answers to both questions are coded as one group out of the 76 states/regions of the US and 85 foreign countries. The question of whether the respondent's parents were native born is binary-coded.

Table 4. Difference-in-Mean Estimates of the Scale: American National Election Studies

	Unadjusted		Adjusted	
first generation	0.214		0.186	
	(0.056)***		(0.072)***	
second generation	0.033	F-stat = 8.31	0.05	F-stat = 3.14
	-0.033		-0.038	
Observations	7684		5930	
R^2	0.02		0.22	

Robust standard errors in parentheses.

* significant at 10%; ** significant at 5%; *** significant at 1%.

All regressions include a vector of survey year dummies to control time fixed effects. Estimates of unadjusted specification are almost the same as the differences in unconditional mean. Adjusted specification holds other socio-economic variables constant. The other socioeconomic variables include respondent's age, gender, race dummies, dummies of educational attainment, family income, dummies of employment status, union membership, dummies of church attendance, home ownership, marital status, and dummies indicating whether the respondent lives in an urban, a suburban, or a rural area.

Table 5. Difference-in-Mean Estimates of the "Guaranteed Health Insurance for All Americans": Cooperative Congressional Election Study

	Unadjusted		Adjusted	
WITH ONE 1st-Generation				
first generation	0.237		0.137	
	(0.037)***		(0.041)***	
second generation	0.091	F-stat = 10.63	0.058	F-stat = 2.72
	(0.029)***		(0.032)*	
third generation	-0.013	F-stat = 10.78	-0.002	F-stat = 3.07
	(0.019)		(0.02)	
TWO 1st-Generation Immigrants				
Immigrant noncitizen	0.483		0.353	
	(0.083)***		(0.093)***	
Immigrant citizen	0.202	F-stat = 9.51	0.106	F-stat = 6.08
	(0.040)***		(0.044)**	
Observations	15980		12629	
R^2	0.01		0.14	

Robust standard errors in parentheses.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Entries are estimated by the Linear Probability Model. Estimates of unadjusted specification are the same as the differences in unconditional mean. Adjusted specification holds other socio-economic variables constant. The other socioeconomic variables include respondent's age, gender, race dummies, dummies of educational attainment, family income, dummies of employment status, union membership, dummies of church attendance, home ownership, marital status, and number of children dummies.

Table 6. Difference-in-Mean Estimates of the Scale: the 1960s, 1970s, and 1990s American National Election Studies

	1960s		1970s		1990s	
	Unadj	Adj	Unadj	Adj	Unadj	Adj
1st-gen	0.322	0.378	0.745	0.721	0.278	0.257
	(0.072)***	(0.109)***	(0.088)***	(0.091)***	(0.068)***	(0.079)***
2nd-gen	0.08	0.232	0.222	0.276	0.068	0.093
	(0.047)*	(0.062)***	(0.047)***	(0.049)***	(0.038)*	(0.042)**
N	2674	1758	4054	3666	5756	5001
R^2	0.06	0.23	0.02	0.18	0.01	0.15

Robust standard errors in parentheses.

* significant at 10%; ** significant at 5%; *** significant at 1%.

All regressions include a vector of survey year dummies to control time fixed effects. Estimates of unadjusted specification are almost the same as the differences in unconditional mean. Adjusted specification holds other socioeconomic variables constant. None of the three scales above can be compared with the scale in Table 4.

Table 7. Socialization of second-generation Immigrants

	Annenberg		ANES			CCES			
	US	Foreign	US Nat	1st-gen	2nd-gen	US Nat	1st-gen	2nd-gen	3rd-gen
50 vs. 50	0.166***	0.059	0.233***	0.093	0.156**	0.173***	0.029	0.01	0.063*
	(0.0146)	(0.0407)	(0.0258)	(0.142)	(0.0678)	(0.021)	(0.079)	(0.063)	(0.033)
N	17447	1647	4991	201	727	7654	610	1026	3337
R^2	0.108	0.113	0.245	0.31	0.22	0.147	0.189	0.163	0.152
40 vs. 40	0.193***	0.0718*	0.275***	0.121	0.209***	0.222***	0.067	-0.065	0.107***
	(0.0407)	(0.0433)	(0.0288)	(0.154)	(0.0721)	(0.0253)	(0.101)	(0.0791)	(0.0405)
N	14537	1501	4128	181	656	5477	415	680	2318
R^2	0.107	0.106	0.238	0.325	0.23	0.158	0.219	0.191	0.15

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

All regressions hold other socioeconomic variables constant.

Table 8. Difference-in-Mean Estimates by Income Level

	Unadjusted		Adjusted	
	Low Inc.	High Inc.	Low Inc.	High Inc.
Annenberg (Threshold: $50,000 - median both in Annenberg and in CPS)				
Foreign-born	0.226	0.211	0.103	0.14
	(0.0245)***	(0.0359)***	(0.0312)***	(0.0398)***
Observations	9891	9543	9709	9385
R^2	0.006	0.003	0.098	0.087
ANES (Threshold: 33 %)				
first generation	0.204	0.274	0.156	0.166
	(0.111)*	(0.067)***	(0.129)	(0.087)*
second generation	-0.227	0.136	-0.029	0.078
	(0.054)***	(0.045)***	(0.06)	(0.048)
Observations	2398	4528	2045	3885
R^2	0.02	0.03	0.21	0.17
CCES (Threshold 1: $50,000 - 33% in CCES and median in CPS)				
first generation	0.107	0.313	0.037	0.187
	(0.068)	(0.047)***	(0.078)	(0.052)***
second generation	0.065	0.129	0.033	0.079
	(0.052)	(0.039)***	(0.058)	(0.042)*
third generation	0.007	0.01	0.023	0.008
	(0.032	(0.025	(0.035	(0.027
Observations	5019	8895	3990	7292
R^2	0.01	0.01	0.1	0.13
CCES (Threshold 2: $70,000 - median in CCES and 70% in CPS)				
first generation	0.201	0.303	0.121	0.176
	(0.056)***	(0.054)***	(0.065)*	(0.058)***
second generation	0.069	0.145	0.033	0.09
	(0.043)	(0.046)***	(0.047)	(0.050)*
third generation	-0.013	0.036	-0.03	0.056
	(0.027)	(0.03)	(0.029)	(0.032)*
Observations	7565	6349	6075	5207
R^2	0.01	0.01	0.12	0.14

Robust standard errors in parentheses.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Estimates of unadjusted specification are the same as the differences in unconditional mean. Adjusted specification holds other socioeconomic variables constant.

Table 9. State Assistance to Immigrants and Political Views on Welfare Spending

	Annenberg		ANES			CCES			
	US	Foreign	US Nat	1st-gen	2nd-gen	US Nat	1st-gen	2nd-gen	3rd-gen
BASED ON the Urban Institute's Report as of 1998									
Less	0.0408**	-0.009	0.0549	-0.139	0.0849	0.0409	-0.0447	-0.0202	0.0606
	(0.0204)	(0.0637)	(0.0343)	(0.263)	(0.12)	(0.029)	(0.144)	(0.105)	(0.0539)
Somewhat	0.0857***	0.0476	0.164***	0.0946	0.125	0.0596*	-0.162	0.0163	0.0314
	(0.0203)	(0.0616)	(0.0383)	(0.264)	(0.109)	(0.0314)	(0.133)	(0.096)	(0.0521)
Most	0.0237	-0.031	0.131***	-0.22	-0.028	0.0650**	-0.00251	-0.109	0.0484
	(0.0217)	(0.0622)	(0.0396)	(0.25)	(0.115)	(0.0309)	(0.129)	(0.0986)	(0.0526)
N	17447	1647	4998	201	728	7655	610	1027	3337
R^2	0.101	0.106	0.235	0.321	0.218	0.141	0.193	0.166	0.152
BASED ON the Urban Institute's Welfare Rules Data as of 2004									
(10 AND BELOW) VS. (11 AND ABOVE)									
11+	-0.0205	0.0315	0.0536**	-0.228	0.0269	0.0370*	0.0736	-0.0538	0.00362
	(0.0145)	(0.0412)	(0.0258)	(0.149)	(0.0699)	(0.0217)	(0.0802)	(0.0631)	(0.0338)
N	17447	1647	4998	201	728	7635	607	1024	3333
R^2	0.1	0.105	0.233	0.317	0.214	0.141	0.189	0.162	0.152
(9 AND BELOW) VS. (12 AND ABOVE)									
12+	0.0126	0.0017	0.0453	-0.336*	-0.044	0.0281	0.00057	-0.0066	0.0138
	(0.0188)	(0.0487)	(0.0313)	(0.185)	(0.098)	(0.0269)	(0.108)	(0.0809)	(0.0447)
N	11502	1160	3433	145	474	5079	393	693	2124
R^2	0.107	0.132	0.254	0.395	0.236	0.145	0.204	0.162	0.169

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

All regressions hold other socioeconomic variables constant.

Table 10. Continents of Origin

	Unadjusted	Adjusted
Western Europe	0.047	0.364
	(0.129)	(0.141)***
Latin America	0.403	-0.004
	(0.092)***	(0.105)
Eastern Europe	0.081	0.211
	(0.151)	(0.228)
Asia	0.113	0.209
	(0.093)	(0.158)
Africa	0.236	0.007
	(0.45)	(0.362)
Not Codeable Else	0.181	0.368
	(0.301)	(0.372)
Observations	7683	5929
R-squared	0.02	0.22

Robust standard errors in parentheses. *** significant at 1%.

Table A1-1. Difference-in-Mean Coefficients of the Individual Items in National Annenberg Election Survey – Estimated by Ordered Probit Model; Unadjusted Coefficients and Marginal Effects

(1) GOVT SPENDING ON HEALTH INSURANCE (4-PT) (21752 obs)

	COEFFICIENT	MARGINAL EFFECT FOR			
		Pr(y=0) None	Pr(y=1) Less	Pr(y=2) Same	Pr(y=3) More
US-Born Vs Foreign-Born					
Foreign-born	0.308***	-0.0201***	-0.0205***	-0.0526***	0.0932***
	(0.0339)	(0.00179)	(0.00203)	(0.00566)	(0.00927)
TWO Foreign-Born Respondents					
Foreign-born Citizens	0.209***	-0.0144***	-0.0144***	-0.0360***	0.0648***
	(0.0416)	(0.00242)	(0.00262)	(0.00709)	(0.0120)
Foreign-born Noncitizens	0.488***	-0.0266***	-0.0289***	-0.0807***	0.136***
	(0.0561)	(0.00200)	(0.00257)	(0.00839)	(0.0126)
Cut-points		λ_0	λ_1	λ_2	
		-1.754***	-1.337***	-0.586***	
		(0.0158)	(0.0123)	(0.00945)	

(2) FEDERAL ASSISTANCE TO PUBLIC SCHOOLS (4-PT) (31275 obs)

	COEFFICIENT	MARGINAL EFFECT FOR			
		Pr(y=0) None	Pr(y=1) Less	Pr(y=2) Same	Pr(y=3) More
US-Born Vs Foreign-Born					
Foreign-born	0.383***	-0.0171***	-0.0242***	-0.0768***	0.118***
	(0.0288)	(0.00101)	(0.00156)	(0.00551)	(0.00780)
TWO Foreign-Born Respondents					
Foreign-born Citizens	0.238***	-0.0116***	-0.0160***	-0.0485***	0.0761***
	(0.0347)	(0.00137)	(0.00206)	(0.00693)	(0.0103)
Foreign-born Noncitizens	0.658***	-0.0221***	-0.0342***	-0.123***	0.180***
	(0.0497)	(0.00103)	(0.00174)	(0.00773)	(0.00996)
Cut-points		λ_0	λ_1	λ_2	
		-1.913***	-1.433***	-0.524***	
		(0.0149)	(0.0108)	(0.00778)	

(3) GOVT HEALTH INSURANCE FOR CHILDREN (BINARY) (11864 obs)

	COEFFICIENT	dy/dx
Us-Born Vs Foreign-Born		
Foreign-born	0.520***	0.108***
	(0.0597)	(0.00938)
Two Foreign-Born Respondents		
Foreign-born Citizens	0.348***	0.0765***
	(0.0707)	(0.0129)
Foreign-born Noncitizens	0.871***	0.145***
	(0.114)	(0.0100)
Cut-points		λ_0
		-0.887***
		(0.0139)

(4) GOVT HEALTH INSURANCE FOR CHILDREN (5-PT) (9824 obs)

	COEFFICIENT	MARGINAL EFFECT FOR				
		Pr(y=0) Oppose	Pr(y=1)	Pr(y=2) Neutral	Pr(y=3)	Pr(y=4) Favor
Us-Born Vs Foreign-Born						
Foreign-born	0.601***	-0.0719***	-0.0495***	-0.00780***	-0.0797***	0.209***
	(0.0516)	(0.00425)	(0.00386)	(0.000871)	(0.00750)	(0.0153)
Two Foreign-Born Respondents						
Foreign-born Citizens	0.426***	-0.0549***	-0.0366***	-0.00564***	-0.0559***	0.153***
	(0.0610)	(0.00581)	(0.00479)	(0.000893)	(0.00879)	(0.0198)
Foreign-born Noncitizens	1.054***	-0.0890***	-0.0708***	-0.0118***	-0.139***	0.310***
	(0.0951)	(0.00369)	(0.00408)	(0.00116)	(0.0107)	(0.0173)
Cut-points		λ_0	λ_1	λ_2	λ_3	
		-1.265***	-0.871***	-0.815***	-0.203***	
		(0.0176)	(0.0150)	(0.0148)	(0.0133)	

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table A1-1. Difference-in-Mean Coefficients of the Individual Items in National Annenberg Election Survey – Estimated by Ordered Probit Model; Unadjusted Coefficients and Marginal Effects (Continued.)

	(5) GOVT HEALTH INSURANCE FOR WORKERS (BINARY) (10959 obs)		(6) GOVT HEALTH INSURANCE FOR WORKERS (5-PT) (9752 obs)					
	COEFFICIENT	dy/dx	COEFFICIENT	MARGINAL EFFECT FOR				
				Pr(y=0) Oppose	Pr(y=1)	Pr(y=2) Neutral	Pr(y=3)	Pr(y=4) Favor
Us-Born Vs Foreign-Born								
Foreign-born	0.424***	0.116***	0.470***	-0.0793***	-0.0468***	-0.00955***	-0.0485***	0.184***
	(0.0527)	(0.0122)	(0.0455)	(0.00592)	(0.00447)	(0.00112)	(0.00626)	(0.0171)
Two Foreign-Born Respondents								
Foreign-born Citizens	0.307***	0.0871***	0.326***	-0.0584***	-0.0330***	-0.00658***	-0.0312***	0.129***
	(0.0642)	(0.0161)	(0.0544)	(0.00802)	(0.00544)	(0.00122)	(0.00668)	(0.0211)
Foreign-born Noncitizens	0.632***	0.156***	0.775***	-0.105***	-0.0717***	-0.0157***	-0.0961***	0.289***
	(0.0899)	(0.0159)	(0.0794)	(0.00611)	(0.00610)	(0.00174)	(0.0124)	(0.0252)
Cut-points		λ_0		λ_0	λ_1	λ_2	λ_3	
		-0.650***		-1.082***	-0.653***	-0.565***	0.102***	
		(0.0135)		(0.0162)	(0.0142)	(0.0139)	(0.0132)	

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table A1-2. Difference-in-Mean Coefficients of the Individual Items in National Annenberg Election Survey – Estimated by Ordered Probit Model; Adjusted Coefficients and Marginal Effects

(1) GOVT SPENDING ON HEALTH INSURANCE (4-PT) (19302 obs)

	COEFFICIENT	MARGINAL EFFECT FOR Pr(y=0) None	Pr(y=1) Less	Pr(y=2) Same	Pr(y=3) More
Us-Born Vs Foreign-Born					
Foreign-born	0.201***	-0.0104***	-0.0127***	-0.0366***	0.0596***
	(0.0443)	(0.00195)	(0.00254)	(0.00788)	(0.0123)
Two Foreign-Born Respondents					
Foreign-born Citizens	0.160***	-0.00843***	-0.0102***	-0.0292***	0.0479***
	(0.0501)	(0.00230)	(0.00295)	(0.00900)	(0.0142)
Foreign-born Noncitizens	0.303***	-0.0140***	-0.0178***	-0.0541***	0.0858***
	(0.0717)	(0.00246)	(0.00350)	(0.0120)	(0.0179)
Cut-points		λ_0	λ_1	λ_2	
		-1.629***	-1.185***	-0.372***	
		(0.121)	(0.121)	(0.120)	

(2) FEDERAL ASSISTANCE TO PUBLIC SCHOOLS (4-PT) (27789 obs)

	COEFFICIENT	MARGINAL EFFECT FOR Pr(y=0) None	Pr(y=1) Less	Pr(y=2) Same	Pr(y=3) More
Us-Born Vs Foreign-Born					
Foreign-born	0.166***	-0.00585***	-0.0100***	-0.0357***	0.0516***
	(0.0373)	(0.00114)	(0.00206)	(0.00786)	(0.0110)
Two Foreign-Born Respondents					
Foreign-born Citizens	0.126***	-0.00454***	-0.00772***	-0.0271***	0.0394***
	(0.0418)	(0.00134)	(0.00238)	(0.00890)	(0.0126)
Foreign-born Noncitizens	0.270***	-0.00843***	-0.0150***	-0.0569***	0.0803***
	(0.0633)	(0.00149)	(0.00292)	(0.0126)	(0.0170)
Cut-points		λ_0	λ_1	λ_2	
		-2.129***	-1.615***	-0.629***	
		(0.0948)	(0.0941)	(0.0937)	

(3) GOVT HEALTH INSURANCE FOR CHILDREN (BINARY) (10475 obs)

	COEFFICIENT	dy/dx
Us-Born Vs Foreign-Born		
Foreign-born	0.255***	0.0525***
	(0.0797)	(0.0144)
Two Foreign-Born Respondents		
Foreign-born Citizens	0.222**	0.0459***
	(0.0895)	(0.0164)
Foreign-born Noncitizens	0.341**	0.0660***
	(0.146)	(0.0228)
Cut-points		λ_0
		-1.159***
		(0.200)

(4) GOVT HEALTH INSURANCE FOR CHILDREN (5-PT) (8833 obs)

	COEFFICIENT	MARGINAL EFFECT FOR Pr(y=0) Oppose	Pr(y=1)	Pr(y=2) Neutral	Pr(y=3)	Pr(y=4) Favor
Us-Born Vs Foreign-Born						
Foreign-born	0.330***	-0.0362***	-0.0296***	-0.00464***	-0.0487***	0.119***
	(0.0639)	(0.00559)	(0.00527)	(0.000938)	(0.0100)	(0.0214)
Two Foreign-Born Respondents						
Foreign-born Citizens	0.245***	-0.0279***	-0.0224***	-0.00347***	-0.0357***	0.0894***
	(0.0716)	(0.00681)	(0.00614)	(0.00103)	(0.0111)	(0.0249)
Foreign-born Noncitizens	0.612***	-0.0530***	-0.0486***	-0.00797***	-0.0930***	0.203***
	(0.112)	(0.00577)	(0.00676)	(0.00137)	(0.0170)	(0.0301)
Cut-points		λ_0	λ_1	λ_2	λ_3	
		-1.416***	-0.983***	-0.923***	-0.245	
		(0.170)	(0.170)	(0.170)	(0.170)	

Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.10$

All regressions are adjusted for other socioeconomic differences.

Table A1-2. Difference-in-Mean Coefficients of the Individual Items in National Annenberg Election Survey – Estimated by Ordered Probit Model; Adjusted Coefficients and Marginal Effects (Continued.)

	(5) GOVT HEALTH INSURANCE FOR WORKERS (BINARY) (9791 obs)		**(6) GOVT HEALTH INSURANCE FOR WORKERS (5-PT) (8771 obs)**					
	COEFFICIENT	dy/dx	COEFFICIENT	MARGINAL EFFECT FOR				
				Pr(y=0) Oppose	Pr(y=1)	Pr(y=2) Neutral	Pr(y=3)	Pr(y=4) Favor
Us-Born Vs Foreign-Born								
Foreign-born	0.338***	0.0903***	0.277***	-0.0453***	-0.0304***	-0.00593***	-0.0285***	0.110***
	(0.0690)	(0.0160)	(0.0564)	(0.00789)	(0.00605)	(0.00130)	(0.00715)	(0.0221)
Two Foreign-Born Respondents								
Foreign-born Citizens	0.319***	0.0851***	0.236***	-0.0391***	-0.0260***	-0.00504***	-0.0238***	0.0940***
	(0.0775)	(0.0179)	(0.0626)	(0.00899)	(0.00676)	(0.00142)	(0.00770)	(0.0247)
Foreign-born Noncitizens	0.386***	0.0995***	0.387***	-0.0580***	-0.0416***	-0.00840***	-0.0446***	0.153***
	(0.117)	(0.0249)	(0.0971)	(0.0111)	(0.00976)	(0.00220)	(0.0142)	(0.0370)
Cut-points		λ_0		λ_0	λ_1	λ_2	λ_3	
		-0.775***		-1.217***	-0.749***	-0.657***	0.0703	
		(0.184)		(0.154)	(0.154)	(0.154)	(0.153)	

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.10

All regressions are adjusted for other socioeconomic differences.

It was too messy to present the ordered probit model estimates from the regression of each individual item on the first-generation immigrant's years of residence in the US. Readers may obtain such tables upon request.

Table A2-1. Difference-in-Mean Coefficients of the Individual Items in American National Election Studies – Estimated by Ordered Probit Model; Unadjusted Coefficients and Marginal Effects

(1) R POSITION 7PT GOVT HEALTH INSURANCE (14492 obs)

	COEFFICIENT	MARGINAL EFFECT FOR						
		Pr(y=0) no_gov't spending	Pr(y=1) 1	Pr(y=2) 2	Pr(y=3) Neutral	Pr(y=4) 4	Pr(y=5) 5	Pr(y=6) more_gov't spending
first generation	0.527***	-0.100***	-0.0443***	-0.0355***	-0.0267***	0.00365***	0.0212***	0.182***
	(0.056)	(0.0076)	(0.0046)	(0.0043)	(0.0048)	(0.0013)	(0.0011)	(0.021)
second generation	0.223***	-0.0511***	-0.0187***	-0.0130***	-0.00556***	0.00568***	0.0125***	0.0701***
	(0.026)	(0.0055)	(0.0023)	(0.0017)	(0.0011)	(0.00052)	(0.0013)	(0.0086)
Cut-points		λ_0	λ_1	λ_2	λ_3	λ_4	λ_5	
		-0.951***	-0.592***	-0.270***	0.185***	0.476***	0.787***	
		(0.031)	(0.030)	(0.030)	(0.030)	(0.030)	(0.031)	

(2) R POSITION 7PT GOVT GUARANTEED JOBS (21648 obs)

	COEFFICIENT	MARGINAL EFFECT FOR						
		Pr(y=0) no_gov't spending	Pr(y=1) 1	Pr(y=2) 2	Pr(y=3) Neutral	Pr(y=4) 4	Pr(y=5) 5	Pr(y=6) more_gov't spending
first generation	0.185***	-0.0410***	-0.0209***	-0.0111***	0.00704***	0.0136***	0.0139***	0.0384***
	(0.045)	(0.0090)	(0.0052)	(0.0033)	(0.00091)	(0.0030)	(0.0034)	(0.010)
second generation	0.00782	-0.00190	-0.000851	-0.000372	0.000439	0.000618	0.000589	0.00147
	(0.021)	(0.0051)	(0.0023)	(0.0010)	(0.0012)	(0.0017)	(0.0016)	(0.0040)
Cut-points		λ_0	λ_1	λ_2	λ_3	λ_4	λ_5	
		-0.926***	-0.433***	0.0297	0.625***	0.983***	1.298***	
		(0.035)	(0.034)	(0.034)	(0.034)	(0.035)	(0.035)	

(3) POOR/POOR PEOPLE - FEDERAL SPENDING (5824 obs)

	COEFFICIENT	MARGINAL EFFECT FOR		
		Pr(y=0) Decreased	Pr(y=1) Same	Pr(y=2) Increased
first generation	0.254***	-0.0350***	-0.0648***	0.0999***
	(0.083)	(0.0096)	(0.022)	(0.032)
second generation	0.115**	-0.0176***	-0.0281**	0.0457**
	(0.047)	(0.0068)	(0.012)	(0.019)
Cut-points		λ_0	λ_1	
		-1.323***	-0.0158	
		(0.033)	(0.029)	

(4) CHILD CARE - FEDERAL SPENDING (11311 obs)

	COEFFICIENT	MARGINAL EFFECT FOR		
		Pr(y=0) Decreased	Pr(y=1) Same	Pr(y=2) Increased
first generation	0.231***	-0.0343***	-0.0552***	0.0894***
	(0.064)	(0.0081)	(0.016)	(0.024)
second generation	0.0256	-0.00433	-0.00578	0.0101
	(0.033)	(0.0055)	(0.0074)	(0.013)
Cut-points		λ_0	λ_1	
		-1.197***	-0.0270	
		(0.031)	(0.029)	

Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.10$

A vector of survey year dummies is included to control time fixed effects.

Table A2-1. Difference-in-Mean Coefficients of the Individual Items in American National Election Studies – Estimated by Ordered Probit Model; Unadjusted Coefficients and Marginal Effects (Continued.)

(5) PUBLIC SCHOOLS - FEDERAL SPENDING (13314 obs)

	COEFFICIENT	MARGINAL EFFECT FOR Pr(y=0) Decreased	Pr(y=1) Same	Pr(y=2) Increased
first generation	0.226***	-0.0193***	-0.0593***	0.0786***
	(0.065)	(0.0046)	(0.017)	(0.021)
second generation	-0.0233	0.00241	0.00616	-0.00857
	(0.031)	(0.0033)	(0.0082)	(0.011)
Cut-points		λ_0	λ_1	
		-1.671***	-0.431***	
		(0.036)	(0.032)	

(6) THE HOMELESS - FEDERAL SPENDING (7871 obs)

	COEFFICIENT	MARGINAL EFFECT FOR Pr(y=0) Decreased	Pr(y=1) Same	Pr(y=2) Increased
first generation	0.0911	-0.0111	-0.0214	0.0325
	(0.078)	(0.0088)	(0.018)	(0.027)
second generation	0.0455	-0.00576	-0.0107	0.0164
	(0.042)	(0.0052)	(0.0099)	(0.015)
Cut-points		λ_0	λ_1	
		-1.499***	-0.434***	
		(0.032)	(0.029)	

(7) WELFARE PROGRAMS - FEDERAL SPENDING (7512 obs)

	COEFFICIENT	MARGINAL EFFECT FOR Pr(y=0) Decreased	Pr(y=1) Same	Pr(y=2) Increased
first generation	0.0112	-0.00448	0.00189	0.00258
	(0.073)	(0.029)	(0.012)	(0.017)
second generation	-0.0407	0.0162	-0.00707	-0.00916
	(0.040)	(0.016)	(0.0071)	(0.0089)
Cut-points		λ_0	λ_1	
		0.141***	1.263***	
		(0.030)	(0.033)	

(8) FOOD STAMPS SPENDING -FEDERAL SPENDING (14963 obs)

	COEFFICIENT	MARGINAL EFFECT FOR Pr(y=0) Decreased	Pr(y=1) Same	Pr(y=2) Increased
first generation	0.0848	-0.0304	0.00795*	0.0224
	(0.055)	(0.019)	(0.0043)	(0.015)
second generation	0.00689	-0.00251	0.000750	0.00176
	(0.027)	(0.0099)	(0.0029)	(0.0070)
Cut-points		λ_0	λ_1	
		-0.0834***	1.287***	
		(0.029)	(0.030)	

Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.10$

A vector of survey year dummies is included to control time fixed effects.

Table A2-2. Difference-in-Mean Coefficients of the Individual Items in American National Election Studies – Estimated by Ordered Probit Model; Adjusted Coefficients and Marginal Effects

(1) R POSITION 7PT GOVT HEALTH INSURANCE (12809 obs)

	COEFFICIENT	MARGINAL EFFECT FOR						
		Pr(y=0) no gov't spending	Pr(y=1) 1	Pr(y=2) 2	Pr(y=3) Neutral	Pr(y=4) 4	Pr(y=5) 5	Pr(y=6) more gov't spending
first generation	0.516***	-0.0931***	-0.0445***	-0.0375***	-0.0280***	0.00427***	0.0236***	0.175***
	(0.067)	(0.0086)	(0.0055)	(0.0055)	(0.0060)	(0.0015)	(0.0015)	(0.025)
second generation	0.229***	-0.0496***	-0.0200***	-0.0147***	-0.00660***	0.00616***	0.0141***	0.0707***
	(0.029)	(0.0058)	(0.0027)	(0.0021)	(0.0014)	(0.00061)	(0.0017)	(0.0096)
Cut-points		λ_0	λ_1	λ_2	λ_3	λ_4	λ_5	
		-1.391***	-1.022***	-0.683***	-0.207***	0.0980	0.430***	
		(0.078)	(0.078)	(0.077)	(0.077)	(0.077)	(0.077)	

(2) R POSITION 7PT GOVT GUARANTEED JOBS (19227 obs)

	COEFFICIENT	MARGINAL EFFECT FOR						
		Pr(y=0) no gov't spending	Pr(y=1) 1	Pr(y=2) 2	Pr(y=3) Neutral	Pr(y=4) 4	Pr(y=5) 5	Pr(y=6) more gov't spending
first generation	0.202***	-0.0407***	-0.0242***	-0.0145***	0.00775***	0.0170***	0.0173***	0.0373***
	(0.054)	(0.0097)	(0.0065)	(0.0046)	(0.00094)	(0.0042)	(0.0047)	(0.011)
second generation	0.0730***	-0.0159***	-0.00860***	-0.00450***	0.00396***	0.00647***	0.00620***	0.0124***
	(0.024)	(0.0051)	(0.0029)	(0.0016)	(0.0012)	(0.0021)	(0.0021)	(0.0043)
Cut-points		λ_0	λ_1	λ_2	λ_3	λ_4	λ_5	
		-1.346***	-0.834***	-0.338***	0.303***	0.702***	1.062***	
		(0.068)	(0.067)	(0.067)	(0.067)	(0.067)	(0.067)	

(3) POOR/POOR PEOPLE - FEDERAL SPENDING (4330 obs)

	COEFFICIENT	MARGINAL EFFECT FOR		
		Pr(y=0) Decreased	Pr(y=1) Same	Pr(y=2) Increased
first generation	0.176	-0.0209*	-0.0488	0.0697
	(0.12)	(0.012)	(0.034)	(0.046)
second generation	0.134**	-0.0167**	-0.0366**	0.0533**
	(0.060)	(0.0069)	(0.017)	(0.024)
Cut-points		λ_0	λ_1	
		-1.725***	-0.287**	
		(0.14)	(0.14)	

(4) CHILD CARE - FEDERAL SPENDING (9205 obs)

	COEFFICIENT	MARGINAL EFFECT FOR		
		Pr(y=0) Decreased	Pr(y=1) Same	Pr(y=2) Increased
first generation	0.305***	-0.0366***	-0.0797***	0.116***
	(0.085)	(0.0081)	(0.023)	(0.031)
second generation	0.0627	-0.00896	-0.0157	0.0247
	(0.040)	(0.0056)	(0.010)	(0.016)
Cut-points		λ_0	λ_1	
		-1.948***	-0.679***	
		(0.10)	(0.10)	

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.10

All regressions are adjusted for socioeconomic differences. A vector of survey year dummies is included as well to control time fixed effects.

Table A2-2. Difference-in-Mean Coefficients of the Individual Items in American National Election Studies – Estimated by Ordered Probit Model; Adjusted Coefficients and Marginal Effects (Continued.)

(5) PUBLIC SCHOOLS - FEDERAL SPENDING (10927 obs)

	COEFFICIENT	MARGINAL EFFECT FOR		
		Pr(y=0) Decreased	Pr(y=1) Same	Pr(y=2) Increased
first generation	0.184**	-0.0134***	-0.0509**	0.0643**
	(0.083)	(0.0051)	(0.022)	(0.028)
second generation	0.0425	-0.00351	-0.0118	0.0153
	(0.038)	(0.0030)	(0.011)	(0.014)
Cut-points		λ_0	λ_1	
		-2.444***	-1.115***	
		(0.10)	(0.097)	

(6) THE HOMELESS - FEDERAL SPENDING (6902 obs)

	COEFFICIENT	MARGINAL EFFECT FOR		
		Pr(y=0) Decreased	Pr(y=1) Same	Pr(y=2) Increased
	0.218**	-0.0202**	-0.0529**	0.0730**
	(0.10)	(0.0080)	(0.024)	(0.032)
	0.0927*	-0.00959*	-0.0227*	0.0323*
	(0.051)	(0.0049)	(0.012)	(0.017)
		λ_0	λ_1	
		-1.501***	-0.376***	
		(0.12)	(0.11)	

(7) WELFARE PROGRAMS - FEDERAL SPENDING (5816 obs)

	COEFFICIENT	MARGINAL EFFECT FOR		
		Pr(y=0) Decreased	Pr(y=1) Same	Pr(y=2) Increased
first generation	-0.147	0.0585	-0.0306	-0.0279
	(0.10)	(0.041)	(0.023)	(0.018)
second generation	-0.0668	0.0266	-0.0133	-0.0134
	(0.051)	(0.020)	(0.011)	(0.0100)
Cut-points		λ_0	λ_1	
		-0.635***	0.550***	
		(0.12)	(0.12)	

(8) FOOD STAMPS SPENDING -FEDERAL SPENDING (12470 obs)

	COEFFICIENT	MARGINAL EFFECT FOR		
		Pr(y=0) Decreased	Pr(y=1) Same	Pr(y=2) Increased
	0.0456	-0.0161	0.00520	0.0109
	(0.070)	(0.025)	(0.0074)	(0.017)
	-0.00225	0.000805	-0.000277	-0.000528
	(0.033)	(0.012)	(0.0041)	(0.0078)
		λ_0	λ_1	
		-0.589***	0.908***	
		(0.083)	(0.083)	

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.10

All regressions are adjusted for socioeconomic differences. A vector of survey year dummies is included as well to control time fixed effects.

Table A3. Difference-in-Mean Coefficients of the Health Insurance Item in Cooperative Congressional Election Studies – Estimated by Ordered Probit Model; Unadjusted Coefficients and Marginal Effects

UNADJUSTED (15980 obs)	COEFFICIENT	MARGINAL EFFECT FOR				
		Pr(y=0) no_gov't spending	Pr(y=1) 1	Pr(y=2) Neutral	Pr(y=3) 3	Pr(y=4) more_gov't spending
WITH ONE 1st-Generation						
first generation	0.294***	-0.0886***	-0.0154***	-0.00486***	-0.00540**	0.114***
	(0.044)	(0.012)	(0.0026)	(0.00093)	(0.0022)	(0.018)
second generation	0.113***	-0.0361***	-0.00538***	-0.00155***	0.0000571	0.0430***
	(0.033)	(0.010)	(0.0017)	(0.00052)	(0.00041)	(0.013)
third generation	-0.00705	0.00233	0.000314	0.0000848	-0.0000803	-0.00265
	(0.021)	(0.0068)	(0.00091)	(0.00025)	(0.00024)	(0.0077)
TWO 1st-Generation Immigrants						
Immigrant noncitizen	0.555***	-0.149***	-0.0319***	-0.0111***	-0.0261**	0.218***
	(0.12)	(0.024)	(0.0074)	(0.0030)	(0.011)	(0.045)
Immigrant citizen	0.257***	-0.0785***	-0.0133***	-0.00414***	-0.00389*	0.0998***
	(0.047)	(0.013)	(0.0028)	(0.00095)	(0.0020)	(0.019)
Cut-points		λ_0	λ_1	λ_2	λ_3	
		-0.592***	-0.328***	-0.221***	0.362***	
		(0.013)	(0.012)	(0.012)	(0.012)	

ADJUSTED (12629 obs)	COEFFICIENT	MARGINAL EFFECT FOR				
		Pr(y=0) no_gov't spending	Pr(y=1) 1	Pr(y=2) Neutral	Pr(y=3) 3	Pr(y=4) more_gov't spending
WITH ONE 1st-Generation						
first generation	0.193***	-0.0576***	-0.0112***	-0.00325***	-0.00189	0.0740***
	(0.053)	(0.015)	(0.0033)	(0.0011)	(0.0017)	(0.021)
second generation	0.0802**	-0.0249**	-0.00439*	-0.00121*	0.000251	0.0303**
	(0.040)	(0.012)	(0.0023)	(0.00065)	(0.00028)	(0.015)
third generation	0.00645	-0.00205	-0.000339	-0.0000894	0.0000701	0.00241
	(0.024)	(0.0076)	(0.0013)	(0.00033)	(0.00025)	(0.0090)
TWO 1st-Generation Immigrants						
Immigrant noncitizen	0.464***	-0.123***	-0.0295***	-0.00961***	-0.0197	0.182***
	(0.14)	(0.029)	(0.0096)	(0.0036)	(0.013)	(0.055)
Immigrant citizen	0.156***	-0.0472***	-0.00891***	-0.00255**	-0.000941	0.0596***
	(0.056)	(0.016)	(0.0035)	(0.0011)	(0.0014)	(0.022)
Cut-points		λ_0	λ_1	λ_2	λ_3	
		-0.230.	0.0592	0.171	0.809	
		(0.71)	(0.71)	(0.71)	(0.71)	

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table A4. Difference-in-Mean Coefficients from the Regression of Individual Items in National Annenberg Election Survey – Estimated by LPM

	Used for Scale		Others			
	(1)	(2)	(3)	(4)	(5)	(6)
Unadjusted Coefficients						
[US-born vs Foreign-born]						
FB	0.200***	0.245***	0.282***	0.383***	0.269***	0.350***
	(0.0204)	(0.0166)	(0.0245)	(0.0281)	(0.0283)	(0.0316)
[Two Foreign-born Respondents]						
FB Citizens	0.136***	0.161***	0.207***	0.282***	0.205***	0.249***
	(0.0276)	(0.023)	(0.0351)	(0.0388)	(0.038)	(0.0413)
FB Noncitizens	0.299***	0.373***	0.387***	0.575***	0.366***	0.536***
	(0.0259)	(0.0199)	(0.027)	(0.0251)	(0.0375)	(0.0409)
Observations	21752	31275	11864	9824	10959	9752
R-squared	0.003	0.005	0.006	0.011	0.006	0.009
Adjusted Coefficients						
[US-born vs Foreign-born]						
FB	0.122***	0.0978***	0.115***	0.186***	0.190***	0.184***
	(0.0256)	(0.021)	(0.0331)	(0.0365)	(0.0364)	(0.0406)
[Two Foreign-born Respondents]						
FB Citizens	0.103***	0.0836***	0.119***	0.159***	0.195***	0.161***
	(0.0311)	(0.0258)	(0.04)	(0.0439)	(0.0433)	(0.0463)
FB Noncitizens	0.160***	0.126***	0.105***	0.250***	0.180***	0.240***
	(0.0325)	(0.026)	(0.0403)	(0.0413)	(0.0488)	(0.0576)
Observations	19302	27789	10475	8833	9791	8771
R-squared	0.069	0.085	0.076	0.113	0.075	0.109

Robust standard errors in parentheses.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Estimates of unadjusted specification are the same as the differences in unconditional mean. Adjusted specification holds other socioeconomic variables constant. The other socioeconomic variables include respondent's age, gender, race dummies, dummies of educational attainment, family income, dummies of employment status, union membership, dummies of church attendance, marital status, number of children dummies, and dummies indicating whether the respondent lives in an urban, a suburban, or a rural area.

Dependent Variables:

(1) Favor Government Spending More on Health Insurance (4-pt)

(2) Favor Federal Assistance to Public Schools (4-pt)

(3) Favor Government Health Insurance for Children (binary)

(4) Favor Government Health Insurance for Children (5-pt)

(5) Favor Government Health Insurance for Workers (binary)

(6) Favor Government Health Insurance for Workers (5-pt)

Table A5-1. Assimilation of first-generation Immigrants: Individual Items in Annenberg

	Unadjusted				Adjusted			
Govt Spending on Health Insurance (4-Pt)								
Yrs of Residence	-0.00783***	-0.00952**	-0.0106***	-0.0130***	-0.00883***	-0.0123***	-0.0105***	-0.0148***
in the US	(0.00137)	(0.0039)	(0.00185)	(0.0042)	(0.00178)	(0.00424)	(0.00208)	(0.00466)
Yrs squared		0.0000288		0.0000448		0.0000605		0.0000752
		(0.0000715)		(0.0000781)		(0.0000797)		(0.0000837)
Observations	1893	1893	1882	1882	1662	1662	1657	1657
R-squared	0.023	0.023	0.026	0.026	0.088	0.088	0.09	0.09
Federal Assistance to Public Schools (4-Pt)								
Yrs of Residence	-0.00864***	-0.0038	-0.00628***	-0.00381	-0.00629***	-0.00214	-0.00445***	-0.00139
in the US	(0.00114)	(0.00302)	(0.00147)	(0.00318)	(0.00154)	(0.00358)	(0.00168)	(0.00376)
Yrs squared		-0.0000832		-0.0000383		-0.0000737		-0.0000503
		(0.0000544)		(0.0000598)		(0.0000658)		(0.0000683)
Observations	2700	2700	2681	2681	2358	2358	2348	2348
R-squared	0.029	0.03	0.032	0.034	0.077	0.077	0.08	0.082
Age Control			Yes	Yes			Yes	Yes
Age squared				Yes				Yes
Socioeconomic					Yes	Yes	Yes	Yes

Table A5-2. Assimilation of first-generation Immigrants: Individual Items in Annenberg

	Unadjusted				Adjusted			
Govt Health Insurance for Children (Binary)								
Yrs of Residence	-0.00974***	-0.00387	-0.00966***	-0.00439	-0.00600***	0.000655	-0.00603**	0.000704
in the US	(0.00178)	(0.00498)	(0.00215)	(0.00496)	(0.00209)	(0.00529)	(0.00245)	(0.00568)
Yrs squared		-0.0001		-0.000094		-0.00012		-0.00012
		(0.0000923)		(0.0000987)		(0.0000991)		(0.00011)
Observations	985	985	978	978	840	840	835	835
R-squared	0.05	0.052	0.05	0.052	0.101	0.104	0.102	0.105
Govt Health Insurance for Children (5-Pt)								
Yrs of Residence	-0.0105***	-0.0135***	-0.0128***	-0.0171***	-0.00937***	-0.0119**	-0.0104***	-0.0183***
in the US	(0.00183)	(0.00449)	(0.00231)	(0.00512)	(0.00248)	(0.00476)	(0.00275)	(0.00525)
Yrs squared		0.0000487		0.0000745		0.0000409		0.00014
		(0.0000782)		(0.0000903)		(0.0000806)		(0.0000876)
Observations	811	811	805	805	718	718	714	714
R-squared	0.053	0.054	0.056	0.057	0.158	0.158	0.159	0.167
Govt Health Insurance for Workers (Binary)								
Yrs of Residence	-0.00843***	-0.0107**	-0.00844***	-0.0111**	-0.00819***	-0.00719	-0.00907***	-0.00927
in the US	(0.00184)	(0.00507)	(0.00232)	(0.00532)	(0.00252)	(0.00621)	(0.00273)	(0.00637)
Yrs squared		0.0000398		0.0000493		-0.0000182		0.00000405
		(0.0000912)		(0.0000999)		(0.000109)		(0.000113)
Observations	926	926	919	919	817	817	814	814
R-squared	0.027	0.027	0.026	0.027	0.098	0.098	0.098	0.098
Govt Health Insurance for Workers (5-Pt)								
Yrs of Residence	-0.0112***	-0.0152***	-0.0103***	-0.0226***	-0.00602**	-0.0135**	-0.00645*	-0.0227***
in the US	(0.00204)	(0.00522)	(0.00284)	(0.00519)	(0.00301)	(0.00577)	(0.00339)	(0.00586)
Yrs squared		0.0000642		0.000218**		0.000121		0.000283***
		(0.0000865)		(0.0000887)		(0.0000977)		(0.0000939)
Observations	800	800	794	794	708	708	704	704
R-squared	0.047	0.048	0.049	0.063	0.183	0.185	0.186	0.204
Age Control			Yes	Yes			Yes	Yes
Age squared				Yes				Yes
Socioeconomic					Yes	Yes	Yes	Yes

Table A6. Difference-in-Mean Coefficients from the Regression of the Individual Items in American National Election Studies – Estimated by Linear Probability Model

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Unadjusted Coefficients								
1st-gen	0.206	0.185	0.166	0.072	0.013	0.471	0.194	0.076
	(0.064)***	(0.049)***	(0.044)***	(0.059)	(0.063)	(0.047)***	(0.043)**	(0.049)
2nd-gen	0.095	0.022	-0.018	0.032	-0.036	0.207	0.015	0.006
	(0.039)**	(0.028)	(0.025)	(0.033)	(0.034)	(0.024)***	(0.02)	(0.024)
N	5824	11311	13314	7871	7512	14492	21648	14963
R^2	0.01	0.01	0.01	0.01	0.02	0.02	0.01	0.02
Adjusted Coefficients								
1st-gen	0.132	0.224	0.147	0.173	-0.118	0.443	0.201	0.038
	(0.085)	(0.060)***	(0.057)***	(0.072)**	(0.085)	(0.055)***	(0.048)***	(0.058)
2nd-gen	0.101	0.049	0.026	0.07	-0.052	0.21	0.072	-0.002
	(0.047)**	(0.033)	(0.03)	(0.038)*	(0.041)	(0.026)***	(0.022)***	(0.027)
N	4330	9205	10927	6902	5816	12809	19227	12470
R^2	0.13	0.1	0.08	0.09	0.13	0.09	0.13	0.15

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

All regressions include a vector of survey year dummies to control time fixed effects. Estimates of unadjusted specification are almost the same as the differences in unconditional mean. Adjusted specification holds other socioeconomic variables constant. The other socioeconomic variables include respondent's age, gender, race dummies, dummies of educational attainment, family income, dummies of employment status, household union membership, dummies of church attendance, home ownership, marital status, and dummies indicating whether the respondent lives in a central city, a suburban area, or a rural one.

Dependent Variables:

(1) Federal Spending on the Poor

(2) Federal Spending on the Child Care

(3) Federal Spending on the Public Schools

(4) Federal Spending on the Aids to Homeless

(5) Federal Spending on the Welfare Programs

(6) R Position Government Health Plan

(7) R Position Government Guaranteed Job

(8) Federal Spending on the food Stamp

Table A7. Individual Items in 1960s, 1970s, and 1990s

	1960s		1970s		1990s	
	(1)	(2)	(3)	(4)	(5)	(6)
Unadjusted Coefficients						
1st-gen	0.397	0.177	0.849	0.257	0.252	0.13
	(0.063)***	(0.076)**	(0.078)***	(0.074)***	(0.063)***	(0.069)*
2nd-gen	0.176	-0.02	0.346	-0.029	0.078	0.04
	(0.044)***	(0.044)	(0.039)***	(0.033)	(0.035)**	(0.038)
N	3292	3270	5876	8027	6179	6211
R^2	0.02	0.08	0.03	0.01	0.02	0.01
Adjusted Coefficients						
1st-gen	0.407	0.197	0.784	0.337	0.251	0.119
	(0.095)***	(0.110)*	(0.088)***	(0.080)***	(0.075)***	(0.082)
2nd-gen	0.306	0.099	0.343	0.056	0.096	0.072
	(0.058)***	(0.056)*	(0.042)***	(0.035)	(0.039)**	(0.041)*
N	2211	2155	5297	7269	5348	5390
R^2	0.15	0.17	0.12	0.14	0.1	0.12

Robust standard errors in parentheses.

* significant at 10%; ** significant at 5%; *** significant at 1%.

All regressions include a vector of survey year dummies to control time fixed effects. Estimates of unadjusted specification are almost the same as the differences in unconditional mean. Adjusted specification holds other socioeconomic variables constant.

Dependent Variables:

(1) R Position Government Health Plan (binary)

(2) R Position Government Guaranteed Job (binary)

(3) R Position Government Health Plan (7pt)

(4) R Position Government Guaranteed Job (7pt)

(5) R Position Government Health Plan (7pt)

(6) R Position Government Guaranteed Job (7pt)

Table B1. Imputation Methods and Data Recovery Rates for the Scale of Political views on Welfare Spending

Scale	Before Scaling			
	Years Chosen	Imputation	Recovery of Excluded Question	Data Recovery Rate by Imputation
(1)	When all 8 questions are available **(92, 96)**	No	No	0%
(2)		Yes	No	4.665%
(3)	When all the other 7 questions excluding "Spending on the Homeless" question are available **(92, 96, 00)**	No	No	0%
(4)		Yes	No	8.05%
(5)		Yes	Yes: "Spending on the Homeless" is recovered	11.015%
(6)	When all the other 6 questions excluding "Spending on the Homeless" and "on the Poor" questions are available **(92, 94, 96, 00)**	No	No	0%
(7)		Yes	No	8.117%
(8)		Yes	Yes: "Spending on the Poor" is recovered	10.451%
(9)		Yes	Yes: "Spending on the Homeless" is recovered	13.726%
(10)		Yes	Yes: "Spending on the Poor" and "on the Homeless" are recovered	15.068%

Table B2. Difference-in-Mean Estimates of the Scale by Different Scaling Methods

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Unadjusted Coefficients										
1st-gen	0.284	0.26	0.255	0.216	0.216	0.227	0.191	0.212	0.196	0.214
	(0.078)***	(0.073)***	(0.072)***	(0.063)***	(0.063)***	(0.067)***	(0.059)***	(0.057)***	(0.057)***	(0.056)***
2nd-gen	0.004	0.023	0.012	0.054	0.056	-0.012	0.014	0.032	0.018	0.033
	(0.051)	(0.044)	(0.046)	(0.038)	(0.037)	(0.039)	(0.033)	(0.033)	(0.033)	(0.033)
N	3153	4156	4006	5938	5938	5502	7684	7684	7683	7684
R^2	0.03	0.03	0.02	0.02	0.02	0.02	0.02	0.02	0.02	0.02
Adjusted Coefficients										
1st-gen	0.207	0.213	0.144	0.143	0.178	0.136	0.144	0.156	0.185	0.186
	(0.104)**	(0.091)**	(0.087)*	(0.080)*	(0.080)**	(0.080)*	(0.075)*	(0.072)**	(0.074)**	(0.072)***
2nd-gen	0.038	0.047	0.049	0.06	0.069	0.024	0.027	0.044	0.038	0.05
	(0.055)	(0.048)	(0.05)	(0.045)	(0.045)	(0.042)	(0.038)	(0.038)	(0.038)	(0.038)
N	2764	3579	3477	4399	4399	4803	5930	5930	5930	5930
R^2	0.25	0.24	0.24	0.23	0.22	0.22	0.22	0.22	0.22	0.22

Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

All regressions include a vector of survey year dummies to control time fixed effects. Estimates of unadjusted specification are almost the same as the differences in unconditional mean. Adjusted specification holds other socioeconomic variables constant.

Scaling Methods:

(1) All 8 Variables are Available for the Scale: 92, 96 – without imputation

(2) All 8 Variables are Available for the Scale: 92, 96 – with imputation

(3) All 7 Variables are Available for the Scale if "Homeless" Variable is excluded: 92, 96, 00 - without Imputation

(4) All 7 Variables are Available for the Scale if "Homeless" Variable is excluded: 92, 96, 00 - with Imputation

(5) All 7 Variables are Available for the Scale if "Homeless" Variable is excluded: 92, 96, 00 - With Imputation and the excluded "Homeless" variable is recovered

(6) All 6 Variables are Available for the Scale if "Homeless" and "Poor" Variables are excluded: 92, 94, 96, 00 – Without imputation

(7) All 6 Variables are Available for the Scale if "Homeless" and "Poor" Variables are excluded: 92, 94, 96, 00 – With imputation

(8) All 6 Variables are Available for the Scale if "Homeless" and "Poor" Variables are excluded: 92, 94, 96, 00 – With Imputation and the excluded "Poor" variable is recovered

(9) All 6 Variables are Available for the Scale if "Homeless" and "Poor" Variables are excluded: 92, 94, 96, 00 – With Imputation and the excluded "Homeless" variable is recovered

(10) All 6 Variables are Available for the Scale if "Homeless" and "Poor" Variables are excluded: 92, 94, 96, 00 – With Imputation and the excluded "Homeless" and "Poor" variables are recovered

Table B3. Continents of Origin by Different Scaling Methods

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Unadjusted Coefficients										
W. Europe	0.113	0.109	0.06	0.036	0.043	0.041	0.037	0.035	0.034	0.047
	(0.147)	(0.149)	(0.156)	(0.15)	(0.142)	(0.147)	(0.139)	(0.135)	(0.133)	(0.129)
Latin Am.	0.409	0.404	0.381	0.384	0.371	0.393	0.396	0.413	0.394	0.403
	(0.128)***	(0.119)***	(0.111)***	(0.102)***	(0.104)***	(0.102)***	(0.094)***	(0.090)***	(0.094)***	(0.092)***
E. Europe	0.628	0.424	0.597	0.222	0.222	0.26	0.068	0.076	0.091	0.081
	(0.256)**	(0.229)*	(0.218)***	(0.181)	(0.166)	(0.224)	(0.176)	(0.16)	(0.165)	(0.151)
Asia	0.121	0.076	0.1	0.107	0.11	0.096	0.084	0.106	0.091	0.113
	(0.132)	(0.123)	(0.127)	(0.11)	(0.107)	(0.115)	(0.099)	(0.096)	(0.095)	(0.093)
Africa	0.105	0.076	0.156	0.105	0.188	0.274	0.181	0.195	0.218	0.236
	(0.619)	(0.635)	(0.525)	(0.448)	(0.435)	(0.508)	(0.458)	(0.462)	(0.455)	(0.45)
Elsewhere	0.659	0.639	0.429	0.176	0.175	0.392	0.114	0.187	0.103	0.181
	(0.393)*	(0.353)*	(0.335)	(0.305)	(0.302)	(0.351)	(0.314)	(0.302)	(0.31)	(0.301)
N	3152	4155	4005	5937	5937	5501	7683	7683	7682	7683
R^2	0.03	0.03	0.03	0.02	0.02	0.02	0.02	0.02	0.02	0.02
Adjusted Coefficients										
W. Europe	0.411	0.436	0.309	0.326	0.344	0.325	0.359	0.343	0.392	0.364
	(0.159)***	(0.158)***	(0.161)*	(0.163)**	(0.155)**	(0.154)**	(0.154)**	(0.147)**	(0.147)***	(0.141)***
Latin Am.	-0.195	-0.113	-0.107	-0.073	-0.064	-0.039	0.011	-0.014	0.024	-0.004
	(0.146)	(0.125)	(0.111)	(0.104)	(0.11)	(0.112)	(0.106)	(0.101)	(0.107)	(0.105)
E. Europe	0.697	0.526	0.602	0.382	0.473	0.187	0.022	0.131	0.116	0.211
	(0.375)*	(0.281)*	(0.338)*	(0.242)	(0.232)**	(0.297)	(0.246)	(0.227)	(0.243)	(0.228)
Asia	0.159	0.184	-0.019	0.074	0.13	0.058	0.153	0.163	0.217	0.209
	(0.244)	(0.211)	(0.194)	(0.18)	(0.184)	(0.17)	(0.16)	(0.158)	(0.158)	(0.158)
Africa	-0.139	-0.117	-0.213	-0.14	-0.039	-0.067	-0.039	-0.058	0.04	0.007
	(0.471)	(0.48)	(0.392)	(0.4)	(0.387)	(0.36)	(0.359)	(0.37)	(0.356)	(0.362)
Elsewhere	0.586	0.495	0.674	0.344	0.353	0.582	0.236	0.367	0.226	0.368
	(0.454)	(0.42)	(0.404)*	(0.399)	(0.387)	(0.443)	(0.422)	(0.38)	(0.41)	(0.372)
N	2763	3578	3476	4398	4398	4802	5929	5929	5929	5929
R^2	0.25	0.24	0.24	0.23	0.23	0.22	0.22	0.23	0.22	0.22

A vector of survey year dummies is included to control time fixed effects. See Appendix C for the unconditional means and sample sizes of each immigrant group from different continents. See the notes in Table B2 for 10 different scaling methods.

Table C1. Questions on the Political Views in Welfare Spending: Data Type and Years

Variable name	Data type	years available
National Annenberg Election Survey		
CCC02 (4pt) Favor Government Spending on Health Insurance	3. More 2. Same 1. Less 0. None	04
CCC40 (4pt) Favor Federal Assistance to Public Schools		
CCC03 (binary) Favor Government Health Insurance for Children	1. Favor 0. Oppose	04
CCC05 (binary) Favor Government Health Insurance for Workers		
CCC04 (5-pt) Favor Government Health Insurance for Children	4. Strongly Favor 1-3. 0. Strongly Oppose	04
CCC06 (5-pt) Favor Government Health Insurance for Workers		
American National Election Studies		
VCF0806 (7pt) R Position on Government Health Insurance	6. Government insurance plan 1-5 0. Private insurance plan	70, 72, 76, 78, 84, 88, 92, 94, 96, 00
VCF0809 (7pt) R Position on Government Guaranteed Jobs	6. Government see to job and good living 1-5 0. Government let each person get ahead	72, 74, 76, 78, 80, 82, 84, 86, 88, 90, 92, 94, 96, 98, 00
VCF0886 (3pt) Poor – Federal Spending (needs to be)	2. Increased 1. Same 0. Decreased or cut out entirely	92, 96 00, 02
VCF0887 (3pt) Child Care - Federal Spending (needs to be)	Same as above	88, 90, 92, 94, 96, 00, 02
VCF0890 (3pt) Public Schools - Federal Spending (needs to be)	Same as above	84, 88, 90, 92, 94, 96, 00, 02
VCF0893 (3pt) Aids to Homeless – Federal Spending (needs to be)	Same as above	88, 90, 92, 96
VCF0894 (3pt) Welfare Programs - Federal Spending (needs to be)	Same as above	92, 94, 96, 00, 02
VCF9046 (3pt) Food Stamp - Federal Spending (needs to be)	Same as above	84, 86, 88, 90, 92, 94, 96, 00
Old Items available in 1960s		
VCF0805 (binary) R Position on Government Health Insurance	1. Government should help 0. Government should stay out	56, 60, 62, 64, 68
VCF0808 (binary) R Position on Government Guaranteed Jobs	1. Government see to job and good living 0. Government let each person get ahead	56, 58, 60, 64, 68
Cooperative Congressional Election Study		
Support - Guaranteed Health Insurance for All Americans (5pt)	4. Strongly support 1-3 0. Strongly oppose	06

Table C2. [Annenberg] Sample Size and Unconditional Mean of the Natives and Immigrants

SCALE AND KEY VARS	Scale		Health Insurance (4-pt)		Assistance to Public Schools	
	N	Mean	N	Mean	N	Mean
US-born	19597	-.0220	19844	-.0174	28552	-.0210
Foreign-born	1886	.2286	1908	.1821	2723	.2240
FB citizen	1163	.1344	1175	.1190	1644	.1397
FB non-citizen	719	.3786	729	.2820	1074	.3515

OTHER VARS*)	Government Health Insurance for							
	Children (2-pt)		Children (5-pt)		Workers (2-pt)		Workers (5-pt)	
	N	Mean	N	Mean	N	Mean	N	Mean
US-born	10872	-.0247	9010	-.0342	10027	-.0224	8949	-.0297
Foreign-born	992	.2571	814	.3485	932	.2469	803	.3203
FB citizen	581	.1818	530	.2481	561	.1827	522	.2188
FB non-Citizen	407	.3625	282	.5403	370	.3432	279	.5067

*: The four health insurance items were not used to construct the scale above because these items did not much overlap with the public schools item (fewer than 40% of the respondents). The two 5-pt health insurance items did not overlap with the public schools item at all.

Table C3-1. [ANES] Sample Size and Unconditional Mean of the Natives and Immigrants

SCALE	N	Mean
US Natives	6448	-.0126
1st-Generation Immigrants	274	.2049
2nd-Generation Immigrants	962	.0304

INDIVIDUAL ITEMS	7pt R Position on				Federal Spending on the			
	Govt Health Plan		Govt Guaranteed Job		Poor		Childcare	
	N	Mean	N	Mean	N	Mean	N	Mean
US Natives	11973	-.0249	18062	-.0162	4889	-.0249	9464	-.0119
1st-Generation Immigrants	458	.4328	645	.1775	213	.1838	386	.1727
2nd-Generation Immigrants	2061	.1620	2941	-.0020	722	.0809	1461	.0085
	Federal Spending on the							
	Public Schools		Aids to Homeless		Food Stamps		Welfare Programs	
	N	Mean	N	Mean	N	Mean	N	Mean
US Natives	11138	-.0084	6566	-.0070	12580	-.0054	6315	-.0508
1st-Generation Immigrants	428	.1714	270	.0661	462	.0601	264	-.0337
2nd-Generation Immigrants	1748	-.0335	1035	.0373	1921	.003	933	-.0802

SMALL SCALES in 1960s, 1970s, 1990s	1960s		1970s		1990s	
	N	Mean	N	Mean	N	Mean
US Natives	2061	-.1027	3305	-.1056	4809	.0075
1st-Generation Immigrants	129	.2907	113	.6439	223	.2900
2nd-Generation Immigrants	484	-.0045	636	.1162	724	.0846

Table C3-2. [ANES] Sample Size and Unconditional Mean of the Natives and Immigrants

INDIVIDUAL ITEMS in 1960s, 1970s, 1990s	1960s				1970s				1990s			
	Health Plan		Jobs		Health Plan		Jobs		Health Plan		Jobs	
	N	Mean	N	Mean	N	Mean	N	Mean	N	Mean	N	Mean
US Natives	2535	-.0708	2529	-.1051	4787	-.1089	6634	-.0447	5164	.0156	5188	-.0355
1st-Generation Immigrants	167	.3429	146	.1488	157	.7376	214	.2175	233	.2735	232	.0931
2nd-Generation Immigrants	590	.1089	595	-.1067	932	.2373	1179	-.0765	782	.1072	791	.0072

SCALE BY CONTINENTS	N	Mean
USA	7410	-.0070
Western Europe	42	.0673
Latin America	94	.4010
Eastern Europe	25	.0671
Asia	90	.1083
Africa	8	.1723
Not Codeable Elsewhere	14	.1273

Table C4. [CCES] Sample Size and Unconditional Mean of the Natives and Immigrants

With One First-Generation Immigrants	Health Insurance for All Americans	
	N	Mean
US Natives	9757	-.0141
First-Generation Immigrants	749	.2231
Second-Generation Immigrants	1286	.0771
Third-Generation Immigrants	4188	-.0272
Two First-Generation Immigrants	N	Mean
Immigrant Non-citizen	95	.4686
Immigrant Citizen	654	.1874

Table D-1. Availability of State Assistance to Immigrants – Urban Institute's Welfare Rules Data as of 2004

State	Eligibility of Nonexempt, Pre-PRWORA, Qualified Aliens					State Funds to Help Noncitizens Who Entered after Enactment and Are Ineligible for TANF				Eligibility of Nonexempt, Post-PRWORA, Qualified Aliens after Five Years				
	Lawful Perm. Resid.[1]	Asylee/ Refugee[2]	Deportee[3]	Parolee[4]	Bat. Non-citizen[5]	Lawful Perm. Resid.	Parolee	Bat. Non-citizen	Non-qual. Alien[6]	Lawful Perm. Resid.	Asylee/ Refugee	Deportee	Parolee	Bat. Non-citizen
AL	All	All	All	All	None	No	No	No	No	All	None	None	None	None
AK	All	All	All	All	All	No	No	No	Yes	All	All	All	All	Some
AZ	All	All	All	All	All	No	Yes	No	No	All	All	All	All	All
AR	All	All	All	All	None	No	No	No	No	None	None	None	None	None
CA	All	All	All	All	Some	Yes	Yes	Yes	Yes	All	All	All	All	All
CO	All	All	All	All	Some	No	No	No	Yes	All	All	All	All	Some
CT	All	All	All	All	All	Yes	Yes	Yes	Yes	All	All	All	All	All
DE	All	All	All	All	All	No	No	Yes	No	All	All	All	All	All
D.C.	All	All	All	All	All	No	No	No	No	All	All	All	All	All
FL	All	All	All	All	All	No	No	No	No	All	All	All	All	All
GA	All	All	All	All	All	Yes	Yes	Yes	No	All	All	All	All	All
HI	All	All	All	All	All	Yes	Yes	Yes	Yes	All	All	All	All	All
ID	All	All	All	All	All	No	No	No	No	None	None	None	None	All
IL	All	All	All	All	Some	No	No	Yes	Yes	All	All	All	All	Some
IN	All	All	All	All	None	No	No	No	No	None	All	All	None	None
IA	All	All	All	All	All	No	No	Yes	No	All	All	All	All	All
KS	All	All	All	All	All	No	No	No	No	All	None	None	All	All
KY	All	All	All	All	All	No	No	No	No	All	All	All	All	All
LA	All	All	All	All	All	No	No	No	No	All	All	All	All	All
ME	All	All	All	All	None	Yes	Yes	Yes	No	All	All	All	All	All
MD	All	All	All	All	Some	Yes	Yes	Yes	No	All	All	All	All	All
MA	All	All	All	All	All	No	No	No	No	All	All	All	All	All
MI	All	All	All	All	All	No	No	No	No	All	All	None	All	All
MN	All	All	All	All	All	No	No	No	Yes	All	All	All	All	All
MS	None	All	All	None	None	No	No	No	No	None	None	None	None	None
MO	All	All	All	All	All	No	No	No	No	All	All	All	All	All

Source: Rowe, Murphy, and Williamson (2006) - Table I.B.5, I.B.6, and I.B.7.

Note: According to the source, the definitions of each category of immigrant status are as follows:

1) Lawful permanent residents: permanently admitted.

2) Asylees and refugees: immigrants who escaped their countries due to persecution for political reasons.

3) Deportees: individuals who are granted a stay of deportation or who have had their deportation withheld.

4) Parolees: permitted entry in cases of emergency.

5) Battered noncitizens: meet the definition of 8 USC 1641 (c).

6) Non-qualified aliens: not qualified as defined by federal law.

Table D-2. Availability of State Assistance to Immigrants – Urban Institute's Welfare Rules Data as of 2004

State	Eligibility of Nonexempt, Pre -PRWORA, Qualified Aliens					State Funds to Help Noncitizens Who Entered after Enactment and Are Ineligible for TANF				Eligibility of Nonexempt, Post-PRWORA, Qualified Aliens after Five Years				
	Lawful Perm. Resid.[1]	Asylee/ Refugee[2]	Deportee[3]	Parolee[4]	Bat. Non-citizen[5]	Lawful Perm. Resid.	Parolee	Bat. Non-citizen	Non-qual. Alien[6]	Lawful Perm. Resid.	Asylee/ Refugee	Deportee	Parolee	Bat. Non-citizen
MT	None	All	All	All	Some	No	No	No	No	None	All	All	All	Some
NE	All	All	All	All	All	Yes	Yes	Yes	No	All	All	All	All	All
NV	All	None	None	All	None	No	No	No	No	All	None	None	All	None
NH	All	All	All	All	All	No	No	No	No	All	All	All	All	All
NJ	All	All	All	All	All	No	No	Yes	No	All	All	All	All	All
NM	All	All	None	All	None	Yes	Yes	Yes	No	All	All	All	All	All
NY	All	All	All	All	Some	Yes	Yes	Yes	Yes	All	All	All	All	Some
NC	All	All	All	All	Some	No	No	No	No	All	All	All	All	Some
ND	All	All	All	All	None	No	No	No	Yes	None	None	None	None	None
OH	All	All	All	All	All	No	No	No	No	All	All	All	All	All
OK	All	All	All	All	All	No	No	No	No	All	All	All	All	All
OR	All	All	All	All	All	Yes	Yes	Yes	Yes	All	All	All	All	All
PA	All	All	All	All	All	Yes	Yes	Yes	No	All	All	All	All	All
RI	All	All	All	All	All	Yes	Yes	Yes	No	All	All	All	All	All
SC	All	All	All	All	None	No	No	No	No	All	All	None	All	None
SD	All	None	None	None	None	No	No	No	No	All	All	All	All	All
TN	All	All	All	All	None	Yes	Yes	Yes	Yes	All	All	All	All	None
TX	All	All	All	All	Some	No	No	No	No	None	None	None	None	Some
UT	All	All	All	All	Some	Yes	Yes	Yes	No	All	All	All	All	Some
VT	All	All	All	All	All	Yes	Yes	Yes	Yes	All	All	All	All	All
VA	All	All	All	All	All	No	No	No	No	All	All	All	None	All
WA	All	All	All	All	Some	Yes	Yes	Yes	Yes	All	All	All	All	All
WV	All	All	All	All	None	No	No	No	No	All	All	All	All	None
WI	All	All	All	All	All	Yes	Yes	Yes	No	All	All	All	All	All
WY	All	All	All	All	All	Yes	Yes	Yes	No	All	None	None	All	All

Source: Rowe, Murphy, and Williamson (2006) - Table I.B.5, I.B.6, and I.B.7.

Note: According to the source, the definitions of each category of immigrant status are as follows:

1) Lawful permanent residents: permanently admitted.

2) Asylees and refugees: immigrants who escaped their countries due to persecution for political reasons.

3) Deportees: individuals who are granted a stay of deportation or who have had their deportation withheld.

4) Parolees: permitted entry in cases of emergency.

5) Battered noncitizens: meet the definition of 8 USC 1641 (c).

6) Non-qualified aliens: not qualified as defined by federal law.

Chapter 5

Concluding Remarks

This dissertation explored two broad questions regarding the political and economic determinants of welfare policy and attitudes. I began with the methodological issue of the bias from measurement error that needs to be addressed in substantive chapters. While Chapter 2 discussed the measurement error in general, the focus was on the problem of the middling tendency that occurs in some survey items. Existing scholarship has been confined to traditional CEV assumptions; thus, researchers have been unable to offer solutions that are based on systematic analyses of the bias from this concentrated measurement error.

In Chapter 2, I proposed two estimation strategies: bounds on the parameters of interest and trichotomization. I demonstrated that researchers can construct informative bounds on the parameters using OLS and IV estimates, and I also showed that follow-up questions about things such as certainty and involvement help researchers to determine tighter bounds. This suggests that we benefit from inserting the follow-up questions at the survey design stage. However, I would like to point out that it may not be a good idea to keep asking "Are you sure...?" to respondents every time they finish each question. Doing so may agitate respondents and thus increase measurement error. Therefore, researchers designing surveys should carefully place follow-up questions for only a few selected items.

The strategy of finding bounds on the parameters requires panel surveys with which the items measured with error are repeatedly asked. However, panel surveys are not always available, and the number of repeated items is limited. Follow-up questions are even more rare and should not be inserted too often. The simple idea of trichotomization may address the problem of concentrated measurement error without relying on this pricey information. Of course, benefits never come without costs: by trichotomizing the variables, researchers lose variation and need to change the interpretation of the coefficients.

Chapter 2 analyzed the problems caused by the middling tendency and suggested solutions to reduce their impact. This approach may provide one possible way to put to rest a long-debated issue in the field of survey design. The problem of middling tendency has led to a debate on the choice of odd-number vs. even-number categories. Because some middle responses lower the quality of surveys, several survey design experts suggest excluding the moderate choice so that the middling problem is reduced. However, this suggestion is challenged, because it also forces true attitudes out of the genuine neutral position. This ongoing debate is far from reaching a consensus. Given the obvious strength and weakness of including and excluding the middle response category, the arguments of the two camps have relied entirely on the calculus of benefits and costs. Both camps compare reliability (mostly by comparing the temporal stability in a short panel) and validity (mostly by comparing predictions from a regression). Each camp supports its claim with its own data. This dissertation suggested that we retain the middle response to capture true moderate attitudes and then control for the problems that are caused by the middling tendency. This may help with the development of a compromise between the two competing sides of the debate.

Turning to the substantive research questions, I provided empirical evidence that the generosity of the government welfare policy is associated with public ideological preferences. There have been considerable scholarly efforts to clarify why some countries spend more on welfare than others, and this has been certainly one of the most challenging questions in the field of political economy during the last decade. In the meantime, political scientists have long been trying to answer whether and to what extent the democratic representation system operates so that public policies respond to the preferences of general citizens. In Chapter 3, I sought to synthesize the findings of welfare spending literature with theories of democratic policy responsiveness.

Chapter 3 emphasized the difference in the effects of public preferences between fully operating democratic countries and less democratic ones. According to democratic representation theories, policy responsiveness is a product of a high-quality democratic system: citizens can make politicians accountable, and politicians have to provide policies that are pleasant to the general public in order to gain or retain political office. By contrast, in less democratic countries, politicians only need a small size of winning coalition to keep their political positions due to many issues including less competitive and unfair elections.

The ideal pictures of fully democratic and less democratic countries about the degree of responsiveness may not go unchallenged. In a high level of democracy, elected politicians can ignore what they promised to do during the campaign, because voters may pay little attention to them anyway. Politicians may allocate resources for policies that are favorable to a few specific interest groups as well. In a low level of democracy, a lack of checks and balances may lead powerful executives to pursue what constituents want, even when opposition parties strongly resist these efforts.

Despite these plausible mechanisms, this study provided sophisticated evidence of democratic responsiveness in the welfare dimension, which is a policy area that receives relatively intense public attention. The difference between fully and less democratic countries with regard to the effects of public opinion clearly suggests that welfare policy responsiveness only holds true in countries in which politicians have the incentive to not deviate from the preferences of the general public.

Chapter 4 contributes to the literature on the individual-level determinants of welfare attitudes. To explain why some people support welfare policies more than others, previous literature has mostly focused on individualistic ideology and economic self-interest. This study sees the flood of immigration as a unique opportunity to assess the effects of socialization and assimilation.

The main empirical results in Chapter 4 were as follows:

1. First-generation immigrants show greater support for welfare than US-born natives.
2. The political views of first-generation immigrants more closely resemble those of US-born natives the longer the immigrants stay in the US, thereby suggesting the assimilation of the immigrants into US society.
3. The political views of second-generation immigrants fall somewhere between those of first-generation immigrants and those of US-born natives, but they are much closer to

those of the latter. Taken together with the finding that third-generation immigrants have almost identical political views as those of US-born natives, this study suggests that the more liberal views of first-generation immigrants do not persist into the next generation as a result of the effects of assimilation and socialization in the US.

These findings carry important implications for US welfare politics. The foreign-born makeup of the US population is approximately 12%. Moreover, 40% of current population growth is a result of immigration. Thus, immigrants clearly constitute a large minority group. Aside from the political clout wielded by interest groups, immigrants are not a negligible group in local electoral politics. Although immigrants account for only about 6% of the total eligible voters at the federal level, there are a number of states in which the immigrant voter population is large, such as California, New York, and New Jersey. According to the CPS, immigrant voters account for about 16% of all eligible voters in California, where more than 40% of the state population is comprised of immigrants. In New York, about 14% of eligible voters are immigrants, and immigrants comprise about 45% of the total population. Immigrants in New Jersey make up about 45% of the total population and about 10% of all eligible voters. Because immigrants are more supportive of welfare programs than US-born natives, the large number of immigrants may augment the pro-welfare forces that have historically been limited in the US.[1]

However, long-run changes in the generosity of the US welfare state are expected to remain small; the liberal views of immigrants do not persist as a result of assimilation and socialization. Going back to de Tocqueville, a long tradition of research on American political culture has pointed out that individualism is a core belief in US society. Deeply-rooted and pervasive, the dominant ideology of individualism may easily permeate immigrant populations as they learn political norms and values; it may even go unnoticed while immigrants are exposed to political and social life in the US. The US has experienced massive immigration, but it has also maintained limited welfare policies. Socialization and assimilation may be the keys to explaining why the US has not enlarged the welfare system despite the large influx of more liberal immigrants and their children.

[1]Huber and Stephens (2000) found that well-organized women affect not only women-friendly policies but also gender-neutral welfare state services. Immigrants may also influence welfare policies that lie outside the domain of immigration policy.

The empirical findings presented in this dissertation provide fertile ground for future research. A possible project that is closely related to the empirical results of Chapter 3 would be to examine whether and to what extent a country experiencing a democratic jump shows differences in the degree to which welfare spending policies reflect public preferences before and after such a jump. Although the dataset compiled for this dissertation does not include much information from countries that have undergone noticeable democratic changes, a careful time-series analysis would likely elucidate whether the shock of a democratic jump produces a persistently different response with regard to the effects of public opinion on welfare policy generosity. More scholarly efforts to uncover the relationship between democratic representation and welfare policies will enrich our understanding of both theories of the welfare state and responsiveness literature.

While this dissertation includes some interesting findings and implications, there is also substantial room for improvement. One issue is the indirect method that was employed in Chapter 4 to determine whether second-generation immigrants are socialized in the same way as US-born natives. I found some evidence of second-generation immigrants' socialization with the use of cross-state differences in US-born natives' political views. However, the study would have been more robust if there was longitudinal data that traced changes in political views over a long period. I tried to use the 1972-1976, 1992-1996, and 2000-2004 ANES panels; however, these four-year spans were not sufficient to analyze the pattern of change. I believe that the use of longer-term panel data to examine the persistence of immigrants' political views will improve the assessment and understanding of the links among immigration, socialization, and welfare attitudes.