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FROM THE GENERAL TO THE SPECIFIC: HOW SOCIAL TRUST MOTIVATES RELATIONAL TRUST

Blaine G. Robbins
University of California, Berkeley

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Abstract

When people form beliefs about the trustworthiness of others with respect to particular matters (i.e., when individuals trust), theory suggests that they rely on preexistent cognitive schemas regarding the general cooperativeness of individuals and organizations (i.e., social trust). In spite of prior work, the impact of social trust on relational trust—or what Russell Hardin (2002) calls *trust as a three-part relation* where actor A trusts actor B with reference to matter Y—is not well established. Four vignette experiments were administered to Amazon.com Mechanical Turk workers ($N = 1,388$ and $N = 1,419$) and to public university undergraduate students ($N = 995$ and $N = 956$) in order to investigate the relationship between social trust and relational trust.

Measures of general social trust and particular social trust produced statistically equivalent effects that were positively associated with relational trust. Political trust, however, was statistically unrelated to relational trust. These results support the idea that people rely on schemas and stereotypes concerned with the general cooperativeness and helpfulness of others when forming beliefs about another person's trustworthiness with respect to a particular matter at hand.

Keywords: social trust, general social trust, particular social trust, political trust, relational trust, web-based vignette experiment, correlated random-effects model

From the General to the Specific:
How Social Trust Motivates Relational Trust

1. Introduction

Trust is one of the rare concepts in the social sciences that catalyzes a range of dynamic social processes, including economic exchange (Arrow 1972; Coleman 1990; North 1990), organizational efficiency (Mayer et al., 1995; Rousseau et al. 1998), economic development (Tabellini 2010; Roth 2010), and institutional quality (Putnam 1993; Robbins 2012a). In fact, many scholars consider trust to be *the* foundational building block of modern societies (Putnam 2000; Fukuyama 1995), where social conflict and disorder are commonplace in its absence (Banfield 1958; Gambetta 1993). It is imperative, then, for social scientists to identify mechanisms concerned with the genesis of trust.

In the political and sociological sciences, much of our knowledge about the origins of trust (see Bjørnskov 2007; Delhey and Newton 2005; Fairbrother and Martin 2013; Glanville et al. 2013; Herreros 2004; Paxton 2002, 2007; Robbins 2012b; Wilkes 2011) is derived from a small number of survey instruments intended to measure *social trust* (Rosenberg 1956). While consensus has yet to be reached, social trust is often defined as an expectation about the general cooperativeness and helpfulness of individuals and organizations that is open ended with respect to possible matters (Hardin 2002); this includes such sister concepts as generalized trust (Nannestad 2008), out-group trust (Putnam 2007), particularized trust (Uslaner 2002), and political trust (Levi and Stoker 2000) among others.

During the past twenty years, Hardin (1993, 2002) and colleagues (Cook et al. 2005) began to challenge the utility of social trust as a theoretical construct as well as the empirical validity of the survey items used to measure social trust and its related concepts (Delhey et al.

2011; Miller and Mitamura 2002; Sturgis and Smith 2010). According to Hardin and colleagues, concepts and measures centered on generalized trustees and unspecified matters for which trust is placed yield few analytical insights into the dynamics of trust. As a result, it has become clear that conceptualizations of trust should move away from trust as a two-part relation—where actor A trusts persons or organizations of general type B (e.g., friends, strangers, institutions) without any constraint on the scope of trust—to a form of *relational trust* consisting of three parts: actor A's beliefs (the truster), specific actor B's perceived trustworthiness (the trustee), and particular matter Y of concern to actor A.¹

In spite of theoretical arguments against social trust and its ancillary concepts, can social trust—broadly defined and measured—influence relational trust? According to Hardin's (2002) *learned capacity to trust* model, relational trust stems, in part, from a person's "optimistic expectations of trustworthiness (p. 118)." These general expectations are rooted in prior experiences (Paxton and Glanville 2015; Rotter 1971; Stack 1978) and "...past encounters with other people (Hardin 2002: 113)," which manifest as subjective beliefs and cognitive schemas. In line with this model, psychologists have long noted how beliefs and expectations, regardless of their generality or specificity, mutually reinforce each other (Allport 1954; Bandura 1977; Festinger 1957), while contemporary research on social cognition shows that schemas are causally interrelated (Fiske and Taylor 2013; Kunda 1999; Moskowitz 2005). For instance, role schemas are often used as a basis for attributions about person schemas (Fiske 1998), and self-schemas emerge, to some extent, from relational schemas (Baldwin 1992).

Based on this logic, there is reason to believe that cognitive schemas concerned with trust in categories of people and organizations should positively affect the trust we form in specific

¹ I thus define relational trust as a belief about another person's trustworthiness with respect to a particular matter at hand that emerges under conditions of unknown outcomes (see Hardin 2002; Cook et al. 2005).

people about particular matters. As theory suggests, people are imperfect decision makers and employ various cognitive shortcuts, such as stereotypes and heuristics, when establishing trust in others (Brewer 2008; Kramer 2001). My trust in you is a function of my beliefs about your trustworthiness—largely derived from my experiences with you—as well as my cognitive schemas about prototypical individuals and organizations. This process implies that the more optimistic a person is about the general cooperativeness of individuals and organizations, the more likely that person is to trust another individual with respect to a particular matter.

Although there is evidence that generalized trust, particularized trust, and political trust are causally interrelated (Glanville and Paxton 2007; Freitag and Traunmüller 2009; Newton and Zmerli 2011), existing work has not yet examined whether relational trust can spring from these alternative microfoundations. In economics, scholars have shown that social trust is associated with beliefs in other people's trustworthiness (Sapienza et al. 2013) as well as the behavior and preferences of trusters (Bellemare and Kroeger 2007; Thöni et al. 2012). Yet, this research has relied on behavioral measures of trust with suspect discriminant validity (Cox 2004; Cox et al. 2008) and laboratory conditions with questionable ecological validity (Levitt and List 2007; Guala 2012). That is, the ultimate source of observed behavior in a laboratory experiment—be it trust, other-regarding preferences or some other social psychological mechanism—is difficult to ascertain, and to complicate matters even further, the experimental conditions in which such behavior is observed rarely occurs in the real world. Therefore, to investigate the link between social trust and relational trust it is necessary to conduct controlled experiments that use self-reports of relational trust solicited under richly detailed conditions simulating real life.

Here, I present data from four studies in which I investigated the influence of social trust on relational trust. I developed two novel vignette experiments of simulated 'car repair' and

‘group project’ scenarios where unknown outcomes were inherent to the situation. Each scenario consisted of a truster (the participant), a trustee (an auto mechanic or a student), and a particular matter (auto repairs or task completion). Numerous vignette dimensions were administered in each experiment, including randomized values for age, race and gender of a trustee. Participants assessed ten vignettes in total and reported their levels of relational trust for each vignette. Afterwards, participants were asked classic demographic questions and survey items concerned with social trust (general social trust, particular social trust, and political trust), social preferences, age, gender, and the like. The two vignette experiments were administered to Amazon.com Mechanical Turk workers ($N = 1,388$ and $N = 1,419$) and to public university undergraduate students ($N = 995$ and $N = 956$).

My research design accomplishes two important tasks. First, by using two different sampling methods (purposive and random sampling) and experimental contexts (car repair and group project scenarios) to field four large- N experiments in two separate populations (Mechanical Turk workers and undergraduate students), I minimize type 1 *and* type 2 errors. As a consequence, my findings are robust to false positive and false negative results, which are a growing concern in the psychological sciences (Murayama et al. 2013; Simmons et al. 2011). Second, with the use of multilevel models I am able to parse within-individual variation in relational trust from between-individual variation. This procedure allows me to identify whether—and to what extent—relational trust originates from characteristics of the situation (i.e., randomized vignette dimensions) and or characteristics of the individual (e.g., personality traits and individual propensities). Decomposing within- from between-individual variation in relational trust to empirically identify sources of between-individual variation have yet to be done in the trust literature.

With this research design, correlated random-effects models showed that general social trust and particular social trust was positively associated with relational trust (general social trust = particular social trust) across all four experiments, while political trust was statistically unrelated. These results are especially noteworthy as they describe the relationship between social trust and relational trust under controlled conditions with high saliency and concreteness—the first such set of findings on the subject with strong internal validity and ecological validity.

2. Materials and Methods

2.1. Design and Participants

Each experiment features a 5 (Age: 20, 30, 40, 50, or 60 year old) x 4 (Race: white, black, Hispanic, or Asian) x 2 (Gender: male or female) x 2 (Reputation: no reputation or positive reputation) x 3 (Halo: blank, bad used computer, or good used computer) x 2 (Competence: blank or competent) x 2 (Exertion: blank or hard-working) x 6 (Perceived internal motivations: uncooperative, no prior interaction, prior interaction, encapsulated interests, goodwill, or virtuous dispositions) x 3 (Contract: blank, non-binding contract, or binding contract) x 3 (Regulation: no regulations, non-monetary regulations, or monetary regulations) multifactorial vignette design, which yields a factorial object universe of 51,840 ($2^4 \times 3^3 \times 4^1 \times 5^1 \times 6^1$) unique vignettes where all possible combinations of dimensions were included in the factorial object universe (Jasso 2006; Rossi and Nock 1982). Note that the effects of these dimensions and levels will be explored in alternative manuscripts.

For studies 1a and 1b, 1,388 Amazon.com Mechanical Turk (MTurk) workers participated in study 1a from 1,340 unique IP addresses (IPs), while 1,354 of these workers completed study 1a from 1,311 unique IPs. Likewise, 1,419 MTurk workers participated in study 1b from 1,388 unique IPs, while 1,369 of these workers completed study 1b from 1,342

unique IPs (see the Supplemental Material online for descriptive statistics). In both studies, workers were paid \$2 for their participation. Each study consisted of a unique Mechanical Turk sample with no redundant workers across samples. Overall sample size was determined by a power analysis, and my data collection stopping rule for each study consisted of reaching a target sample size of 1,350 respondents who completed *and* submitted a Human Intelligence Task (HIT).² Although crowdsource-recruited samples such as Mechanical Turk are convenient and non-probabilistic, parameter estimates and standard errors for vignette experiments implemented on Mechanical Turk parallel those drawn from population-based random samples (see Weinberg et al. 2014).

For studies 2a and 2b, 10,000 undergraduate students at a large public university were contacted. All students were randomly selected from a publicly available student directory. Of these 10,000 students, 5,000 were randomly selected for each experiment. 995 undergraduate students participated in study 2a from 987 unique survey tokens and 964 unique IPs, while 895 of these students completed study 2a from 895 unique survey tokens and 877 unique IPs. Likewise, 956 undergraduate students participated in study 2b from 946 unique survey tokens and 926 unique IPs, while 851 of these students completed study 2b from 851 unique survey tokens and 836 unique IPs. With respect to response rates, 13 students did not meet study 2a's eligibility requirements (e.g., reported not being legal adults), which produced an 18% response

² The data collection stopping rule of 1,350 completed and submitted HITs was exceeded by 4 in study 1a and 19 in study 1b. There are two reasons for this discrepancy. First, upon completing the web-based vignette experiment, MTurk workers were assigned a unique survey code to submit as a completed HIT to Amazon.com. In some cases, workers completed the experiment but did not submit their HIT. Second, some workers completed the experiment but were barred from submitting their HIT by Amazon.com since the target goal of 1,350 submitted HITs was achieved. In all of these instances, workers who completed the experiment but failed to or were unable to submit their HIT were compensated \$2 upon contacting the researcher. Importantly, the results presented in studies 1a and 1b do not substantively change when excluding those workers who completed the experiment but failed to submit their HIT. Results are available upon request.

rate (895 completed/4,987 eligible); while 16 students did not meet study 2b's eligibility requirements, which produced a 17% response rate (851 completed/4,984 eligible).

In studies 2a and 2b, students who participated were entered into a lottery for the chance to win one of six \$50 bills. Overall sample size was determined by a power analysis, and my data collection stopping rule consisted of one full academic quarter of recruitment with one recruitment e-mail sent a week.

2.2. Procedure

For studies 1a and 1b, participants were contacted over Amazon.com's Mechanical Turk via a HIT (Human Intelligence Task) advertising \$2 payment for participating in a web-based experiment. To be eligible, MTurk participants must have been legal adults residing in the U.S. with approval rates 90% or above on previous MTurk tasks. In contrast, participants in studies 2a and 2b were contacted over e-mail advertising lottery payment of \$50 for participating in a web-based sociology experiment, where each sampled student was provided with a web-link to the experiment and a unique survey token. Eligibility was restricted to undergraduate students with legal adult status.

After consenting to participate, participants were shown a cover sheet for a hypothetical 'car repair' scenario:

"You just started a new job that requires you to commute every day to work. A couple of days before the job starts you decide to take your car in to an automotive repair shop called The Autoshop for an oil change. In the middle of the oil change, the auto mechanic tells you about a major issue with your car's engine that will cost about \$1,000 to repair, including parts and labor. You would like your car to be in working order, but you want to spend your money wisely since the amount of money required to restore the car's engine is large compared to your budget.

Additionally, you have some concern: the auto mechanic might suggest fraudulent (or intentionally deceptive) services that are unnecessary and quite costly to you but beneficial to the auto mechanic. You're also concerned since the auto mechanic might

provide low quality services at an exaggerated price that would require additional and costly repairs in the future. Plus, you know people who've paid for needless repairs and have had recurrent car problems because of poor quality auto-mechanic services"

Or a hypothetical 'group project' scenario:

"You are a college student who is enrolled in a course that requires a group project assignment. Each group project involves two separate tasks, one student to analyze data and one student to write a technical report of the results. For your specific group, you were assigned the task of writing a technical report of the results, while your partner was assigned the task of analyzing the data.

But you have some concern: students receive the same grade regardless of their contribution to the group project. You're also concerned since the group project is difficult to complete alone and your partner's contribution to the group project is unknown and uncertain. In other words, if your partner doesn't complete the data analysis for the group project, you might not finish in time and fail the assignment as you'll have to analyze the data without help and write a technical report of the results alone. But if you manage to analyze the data and write a technical report of the results, your partner will receive the same grade as you without having contributed to the group project. Unfortunately, you know people who have failed group projects in the past because other group members did not complete their assigned tasks"

The 'car repair' and 'group project' scenarios were constructed to maximize differences across a number of theoretically relevant parameters while maintaining congruency of dimensions between scenarios. These differences include the following (with 'car repair' versus 'group project' comparisons): bilateral versus unilateral exchange, market versus non-market exchange, tangible versus intangible resources, mutual dependence versus interdependence, and transferable versus non-transferable goods. As a result, the two scenarios were employed to explore the robustness of the hypotheses, rather than test hypothesized differences in parameter estimates across scenarios.

After this, participants were informed of how they would be assessing a series of hypothetical situations based on the initial scenario. Participants were then quizzed on the respective scenarios outlined above with two of three screener questions (see Berinsky et al. 2014) and then shown ten vignettes randomly drawn with replacement from the vignette object

universe of 51,840 unique vignettes (only one respondent in all four studies—study 2b—assessed two of the same vignettes). While the levels of each dimension were randomized, the order of dimensions was fixed from vignette-to-vignette.³ An example vignette for the ‘car repair’ scenario is as follows:

- The auto mechanic is a 30 year old, white female.
- As far as you know, none of your friends have used the auto mechanic’s services before.
- The auto mechanic has serviced your car before and always provides justifiable repairs because the auto mechanic genuinely cares for and is concerned about your interests. You know this since the auto mechanic has repaired your car for free when you couldn’t afford the cost of repairs. And, as best you can tell, the auto mechanic is a hard-working, competent machinist.
- The auto mechanic signs a limited warranty outlining how the auto mechanic will be subject to costly professional penalties if the repairs and new engine parts do not last 50,000 miles.
- An automotive agency regulates services at The Autoshop by fining auto mechanics who provide fraudulent services to their customers.
- The auto mechanic sold you a used computer before but you were dissatisfied with the computer.

After assessing the ten vignettes, participants filled out a demographic questionnaire and a series of survey questions, which included measures of general social trust, particular social trust, political trust, inequality aversion, betrayal aversion, risk aversion, trusting behavior, and social value orientations. Afterwards, participants were debriefed, thanked for their participation, and paid \$2 (studies 1ab) or entered into a lottery (studies 2ab). The median time to complete any study ranged between 18.12 and 19.65 minutes (study 1a: 18.12 min.; study 1b:

³ Vignette methodologists find that respondents perform well with ten vignettes per person and no more than twelve randomized dimensions per vignette (Auspurg and Jäckle 2015; Sauer et al. 2011). With this design, the number of vignettes and dimensions is small enough to avoid biases that might jeopardize validity and reliability, such as cognitive overload, learning effects, and order effects, but the number of vignettes is large enough to explore within- *and* between-individual variation in the outcome variables of interest. And although order effects have been observed with vignette experiments using more than one evaluation task (i.e., outcome variable), the bias is unique to complex evaluation tasks and is relatively minor and marginally significant (Auspurg and Jäckle 2015). Moreover, recent research reveals very few differences in data quality between subjects who receive treatments in a laboratory or an online setting (Clifford and Jerit 2014). In other words, the present research falls within design parameters known to minimize bias.

18.87 min.; study 2a: 19.57 min.; study 2b: 19.65 min.). See the Supplemental Material available online for further details about the vignettes, dimensions, and measures.

2.3. Vignette-Level Dependent Variables

At the bottom of each vignette participants were asked a relational trust question, which I conceptualize as a belief about another person's trustworthiness with respect to a particular matter at hand that emerges under conditions of unknown outcomes (see Hardin 2002; Cook et al. 2005). The question was structured as an 11-point bipolar scale ('group project' elements in parentheses): "Given the conditions above, to what extent do you trust the auto mechanic (student) to provide justifiable and quality auto repairs (to complete the assigned data analysis task)?" ranging from "complete distrust" (0 value) to "neither trust nor distrust" (50 value) to "complete trust" (100 value) with a "don't know" option at the end of the scale (study 1a: $M = 6.51$, $SD = 2.68$, $\min = 0$, $\max = 10$; study 1b: $M = 7.49$, $SD = 2.30$, $\min = 0$, $\max = 10$; study 2a: $M = 6.74$, $SD = 2.46$, $\min = 0$, $\max = 10$; study 2b: $M = 7.52$, $SD = 2.25$, $\min = 0$, $\max = 10$). The relational trust scale was then divided by 10 to produce a variable ranging from 0 to 10, with the "don't know" response treated as missing. In Appendix A, Figures A1 through A4 depict the distributions of relational trust and individual-specific means across studies 1ab and 2ab.

2.4. Individual-Level Independent Variables

Following prior research (Newton and Zmerli 2011), I used a number of survey items to measure indicators of social trust, which included dimensions for general social trust, particular social trust, and political trust.

2.4.1. General Social Trust

I conceptualize general social trust as an expectation about the general cooperativeness and helpfulness of strangers with respect to all matters, which I operationalize and measure with

five items. The first item consisted of the classic binary trust question from the General Social Survey: “Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people?” Responses included “most people can be trusted”, “need to be very careful”, and “don’t know.” The remaining four items were responses to the following question: “We’d like to ask you how much you trust people from various groups. Could you tell us for each whether you trust people from this group completely, distrust people from this group completely, or neither trust nor distrust people from this group?” Participants answered this question for “people you meet for the first time”, “strangers”, “people of another nationality”, and “people of another religion.” Responses were made on an 11-point scale ranging from 0 (*complete distrust*) to 50 (*neither trust nor distrust*) to 100 (*complete trust*), with a “don’t know” option at the end of each scale (each item was divided by 10 to produce variables ranging from 0 to 10). These four items were summed and divided by four while ignoring missing values (i.e., row means) to form the general social trust scale (study 1a: $\alpha = .85$; study 1b: $\alpha = .84$; study 2a: $\alpha = .85$; study 2b: $\alpha = .83$). Those who answered “don’t know” were treated as missing.

2.4.2. *Particular Social Trust*

Four items measured particular social trust, which I conceptualize as an expectation about the general cooperativeness and helpfulness of known others with respect to all matters. Embedded within the same matrix grid as items for the general social trust scale were “your family”, “your friends”, “your neighbors”, and “people you know personally” (each item was divided by 10 to produce variables ranging from 0 to 10). These four items were summed and divided by four while ignoring missing values (i.e., row means) to form the particular social trust

scale (study 1a: $\alpha = .78$; study 1b: $\alpha = .77$; study 2ab: $\alpha = .72$). Those who answered “don’t know” were treated as missing.

2.4.3. *Political Trust*

I conceptualize political trust an expectation about the general cooperativeness and helpfulness political institutions. I measured political trust using a matrix grid consisting of four items, which were responses to the following question: “Now we'd like to ask you how much you trust certain organizations and institutions. Could you tell us for each whether you trust these organizations and institutions completely, distrust these organizations and institutions completely, or neither trust nor distrust these organizations and institutions?” These four items included “Congress and the Senate”, “Public authorities”, “Courts”, and “Police.” Responses were made on an 11-point scale ranging from 0 (*complete distrust*) to 50 (*neither trust nor distrust*) to 100 (*complete trust*), with a “don’t know” option at the end of each scale (each item was divided by 10 to produce variables ranging from 0 to 10). The items were summed and divided by four while ignoring missing values (i.e., row means) to form the political trust scale (studies 1ab: $\alpha = .86$; studies 2ab: $\alpha = .84$). Those who answered “don’t know” were treated as missing.

In studies 1ab and 2ab, none of the correlations between general social trust, particular social trust, and political trust were greater than .50. Taken together, α -values support construct validity within factors and intercorrelations support discriminant validity between factors.

2.5. *Individual-Level Control Variables*

2.5.1. *Social Value Orientations*

Participants were provided with and completed the triple dominance measure of social values (Van Lange et al. 1997; Van Lange 1999), which I used to gauge other-regarding

preferences. This measure consisted of nine hypothetical decision scenarios, where participants decided for each scenario how to divide resources between themselves and a hypothetical stranger, or “other”. Each scenario included three options corresponding to one of three social values: a *cooperative* choice, which maximized joint gain; an *individualist* choice, which maximized personal gain without regard to the other’s outcome; and a *competitive* choice, which maximized the difference between gains to self and other. Participants were classified as cooperative, individualist, or competitive if they made six or more choices corresponding to one of the social value orientations. From this, I constructed three dummy variables: social preferences (consisting of the cooperative orientation), egoistic preferences (consisting of the individualist and competitive orientations), and unclassified preferences (consisting of those not classified as either cooperative, individualist, or competitive). It is common practice in the literature to combine individualist and competitive orientations given the small number of competitive orientations in the general population (De Cremer and Van Lange 2001). This scale was included to control for possible false consensus effects whereby A’s own trustworthiness influences the trust that s/he forms about B’s trustworthiness with respect to a particular matter (Butler et al. 2013).

2.5.2. *Preference Aversions*

I measured risk aversion with one item: “Are you, generally speaking, a person who avoids taking risks, or a person who is fully prepared to take risks?” Responses were made on a scale from 0 (*avoid taking risks*) to 10 (*fully prepared to take risks*), with a “don’t know” option at the end of the scale. Inequality aversion and betrayal aversion were measured in response to “To what degree do you agree with the following statements?” using the following statements, respectively: “Wages and incomes should be made more equal” and “If someone offends me, I

will also offend him or her.” Each item ranged from 0 (*strongly disagree*) to 5 (*neutral*) to 10 (*strongly agree*), with a “don’t know” option at the end of the scale (those who answered “don’t know” were treated as missing). These items were included since various measures of preference aversions have been shown to confound the relationship between social trust and behavioral trust (see Fehr 2009).

2.5.3. *Trusting Behavior*

Three items—lend possessions, lend money, and unlocked door—were used to capture prior trusting behavior: (i) “How often do you lend personal possessions to your friends (tools, books, your car or bicycle, etc.)?”, (ii) “How often do you lend money to your friends?”, and (iii) “How often do you leave your door unlocked?” Each item was treated as a continuous variable (1 = never, 2 = infrequently, 3 = sometimes, 4 = often, and 5 = very often). Those who answered “prefer not to say” were treated as missing. These variables were included as prior trusting behaviors might confound the relationship between social trust and relational trust (Glaeser et al. 2000; Naef and Schupp 2009).

2.5.4. *Demographic Variables*

I treated age as a continuous variable. Gender was coded 0 for female (and other gender) and 1 for male.

2.6. *Individual-Level Model Covariates*

Since respondents vary with respect to when, where, and how they participated in studies 1ab and 2ab, I control for a number of individual-level covariates intended to reduce model noise and address non-independence of observations that can arise from such unsystematic variation. First, it is possible for subjects to participate in studies 1ab and 2ab from the same IP address. In this case, data are likely correlated and standard errors downwardly biased since clustered

observations contain less unique information. To address this issue, I include dummy variables where the referent category represents all IP addresses with a single partial or complete experiment and the indicator categories represent a vector of IP addresses with multiple partial or complete experiments.⁴ Second, I include two binary variables for each true-or-false screener question administered directly after the coversheet (and prior to the ten vignettes), where the referent category (“0”) represents an incorrect answer and the indicator category (“1”) represents a correct answer.⁵ Third, I further reduce model noise attributed to issues of attention by controlling for length of participation in a study (natural log time in minutes).⁶ Fourth, to account for history effects, I include dummy variables for the day in which participants partially or fully completed an experiment.⁷

Fifth, in studies 2a and 2b it is not only possible to observe units from the same IP address but also units from the same survey token, which further violates the assumption of independence of observations. To address this problem, I include dummy variables where the referent category represents all survey tokens with a single partial or complete experiment and the indicator categories represent a vector of survey tokens with multiple partial experiments or multiple partials and a complete experiment.⁸ Sixth, recruitment in studies 2a and 2b lasted approximately one academic quarter with one recruitment e-mail sent a week. To further reduce model noise and account for individual-level variation in the number of recruitment e-mails

⁴ Study 1a: 11% of participants from similar IP addresses; study 1b: 7% of participants from similar IP addresses; study 2a: 11% of participants from similar IP addresses; study 2b: 12% of participants from similar IP addresses.

⁵ Study 1a: screener 1 = 96% correct, screener 2 = 98% correct; study 1b: screener 1 = 95% correct, screener 2 = 94% correct; study 2a: screener 1 = 90% correct, screener 2 = 96% correct; study 2b: screener 1 = 93% correct, screener 2 = 93% correct.

⁶ Study 1a: $M = 2.90$, $SD = 0.39$, min = 0.31, max = 4.70; study 1b: $M = 2.94$, $SD = 0.42$, min = 0.41, max = 5.47; study 2a: $M = 2.96$, $SD = 0.53$, min = 0.34, max = 4.71; study 2b: $M = 2.97$, $SD = 0.51$, min = 0.56, max = 4.46.

⁷ Studies 1ab: 2 unique date stamps; studies 2ab: 50 unique date stamps.

⁸ Studies 2ab: 17% of participants from similar survey tokens.

required for respondents to participate, I include dummy variables where the referent category represents respondents who participated upon the initial recruitment e-mail and the indicator categories represent each additional recruitment e-mail sent before a respondent participated.⁹

2.7. Analytic Strategy

My research design yields data in which i vignettes ($i = 1, \dots, 10$) are nested within j individuals ($j = 1, \dots, J$), resulting in a pooled time-series cross-sectional data set. As a result, I estimated two-level correlated random-effects models (Mundlak 1978; Woolridge 2010) to explore the relationship between social trust and relational trust.

My level-1 (or within-level) model takes the following form:

$$Y_{ij} = \beta_{0j} + \beta_{1j}X_{ij} + \delta_i V_i + e_{ij} \quad (1)$$

where Y_{ij} is a continuous measure of relational trust in the i th vignette for the j th individual, β_{0j} is a random intercept term capturing unobserved heterogeneity varying across individuals but not vignettes, β_{1j} is a non-random slope for X_{ij} (which is a vector of vignette dimensions treated as dummy variables that vary across both individuals and vignettes), δ_i is a non-random slope for V_i (which is the i th vignette treated as $i - 1$ dummy variables), and e_{ij} is a disturbance term that varies over the population of vignettes (assumed normal and *iid*).

I specify a level-2 model of between-individual variation in relational trust by modeling the random intercept, β_{0j} , from equation 1:

$$\beta_{0j} = \gamma_{00} + \gamma_{01}\bar{X}_j + \gamma_{02}W_j + u_{0j} \quad (2)$$

where β_{0j} is a random intercept term capturing individual-level variation in relational trust, γ_{00} is an overall population intercept for relational trust, γ_{01} is a non-random slope for $\bar{X}_j$ (which is

⁹ Studies 2ab: median = 2, min = 1, max = 8.

a vector of individual-specific means for X_{ij} that vary across individuals but not vignettes), γ_{02} is a non-random slope for W_j (which is a vector of individual-level independent variables that vary across individuals but not vignettes), and u_{0j} is a random disturbance term that varies over the population of individuals (assumed normal and *iid*).

For purposes of the present study, equation 2 is of central importance that requires further explication. First, I include $\bar{X}_j$ in equation 2 to decompose (or deconflate) the variances of X_{ij} into within- and between-individual components (see Mundlak 1978) (Hausman specification tests estimating fixed- and random-effects econometric models for panel data support this procedure). Although levels of X_{ij} were randomly assigned to vignettes *and* to individuals, the variances of X_{ij} consist of within- and between-individual components for one key reason: unbalanced distributions of levels of dimensions between individuals. That is, individuals with greater (lower) proportions of certain levels of a dimension across the 10 vignettes may produce higher (lower) mean-levels of relational trust via learning and fatigue effects for instance, which could produce a correlation between X_{ij} and u_{0j} captured at the between-individual level. Ultimately, then, a higher proportion of vignettes favorable to the formation of relational trust might confound the relationship between W_j and β_{0j} (e.g., social trust and relational trust). The inclusion of $\bar{X}_j$ reduces this possible sources of bias.

Second, because W_j were not manipulated nor randomly assigned to individuals, W_j are likely correlated with u_{0j} . Under these conditions, one can only assume that γ_{02} are causal if W_j capture all relevant confounding variables at the between-individual level. Although I

include numerous theoretically motivated control variables, I do not interpret γ_{02} as causal effects but as upwardly biased conditional associations that risk omitted variable bias.

Finally, tests revealed that the model estimates presented below were neither unduly influenced by outliers nor first-order serial autocorrelation of the level-1 disturbance terms, but that heteroscedastic level-1 errors were observed. As a result, robust standard errors are used throughout. All modeling procedures were executed in Stata 13.1.

For a larger discussion and justification of my research design, especially in regards to common methods used in behavioral economics, see Appendix B.

3. Results

To start, null models yield small to moderately sized intra-class correlation coefficients (ICC) (study 1a: .10; study 1b: .14; study 2a = .18; study 2b = .26), demonstrating some variation between individuals with most variation occurring within individuals (or between vignettes). ICCs thus support the notion that investigating between-individual variation in relational trust is worth consideration.

[Insert Table 1 About Here]

Table 1 provides estimates for models specified in equations 1 and 2. Across all four studies, Table 1 shows that greater general social trust and particular social trust (both measured as scales) are directly associated with higher mean-levels of relational trust at the between-individual level. Substituting the general social trust scale for the classic binary general social trust question yields similar results (study 1a: $b = 0.17$, $SE = 0.06$, $p < .01$; study 1b: $b = 0.19$, $SE = 0.06$, $p < .01$; study 2a: $b = 0.19$, $SE = 0.09$, $p < .05$; study 2b: $b = 0.42$, $SE = 0.09$, $p < .001$). Except for study 2a, political trust is statistically unrelated to relational trust (see Table 1).

With respect to comparative effects, the slopes for general social trust and particular social trust (measured as scales) are statistically equivalent across all four studies (H_0 : general social trust = particular social trust; study 1a: $\chi^2(1) = 0.06, p > .05$; study 1b: $\chi^2(1) = 1.40, p > .05$; study 2a: $\chi^2(1) = 0.01, p > .05$; study 2b: $\chi^2(1) = 0.12, p > .05$). I thus observe inequality of effects among the social trust measures, where general social trust = particular social trust > political trust (ns).

Regarding effect sizes, the general social trust scale (study 1a: $\beta = 0.04$; study 1b: $\beta = 0.05$; study 2a: $\beta = 0.04$; study 2b: $\beta = 0.11$), the binary general social trust question (study 1a: $\beta = 0.03$; study 1b: $\beta = 0.04$; study 2a: $\beta = 0.05$; study 2b: $\beta = 0.09$), and the particular social trust scale (study 1a: $\beta = 0.04$; study 1b: $\beta = 0.08$; study 2a: $\beta = 0.04$; study 2b: $\beta = 0.08$) consistently generate small effects sizes in the form of standardized β s (see Cohen 1988). Moreover, the total level-2 variance explained by the control variables is small (study 1a: level-2 $R^2 = .07$; study 1b: level-2 $R^2 = .03$) to moderate (study 2a: level-2 $R^2 = .13$; study 2b: level-2 $R^2 = .13$), while the addition of the social trust scales accounts for small (study 1a: level-2 $\Delta R^2 = .05$; study 1b: level-2 $\Delta R^2 = .08$) to moderate (study 2a: level-2 $\Delta R^2 = .09$; study 2b: level-2 $\Delta R^2 = .12$) changes in the explained level-2 variance.

With respect to the control variables, only social preferences generate statistically significant results across three of the four studies, while inequality aversion and betrayal aversion reach statistical significance in at least two of the four studies. A few control variables are statistically significant in at least one study (e.g., male in study 1a), while none of the control variables are statistically significant in each of the four studies.

3.1. Robustness and Sensitivity Checks

I also subject the results to a number of alternative modeling procedures. First, I find strong evidence for a positive relationship between social trust and relational trust. All else equal, relational trust is greater for “high social trusters” than for “low social trusters”. Yamagishi et al. (1999) assert the opposite: “low social trusters” are more gullible when faced with risky unknown outcomes, and “high social trusters” are more adept at using information revealing a lack of trustworthiness. In other words, conditions favorable to betrayal and deceit should have a greater negative impact on relational trust for “high social trusters” than for “low social trusters.” To explore this competing hypothesis, I estimate models specified in equations 1 and 2 but with cross-level interactions between general social trust (both the scale and binary question) and vignette dimensions that explicitly operationalize prior untrustworthy behavior (i.e., Halo and Perceived internal motivations dimensions). Regardless of the measure, I do not observe consistent directions of effect nor statistically significant cross-level interactions across all four studies, thus supporting the notion that—all else equal—relational trust is greater for “high social trusters” than for “low social trusters”

Second, I include other classic demographic control variables to the models presented in Table 1, including race, income, and education (see Supplemental Material available online for information regarding these questions). Across all four studies, none of the alternative control variables substantively alter the impact of general social trust, particular social trust, or political trust on relational trust. Second, excluding respondents who failed screener (i.e., attention) questions, who assessed multiple vignettes from the same IP address and or survey token, or who did not complete the study yields substantive conclusions similar to those presented in Table 1.

The results for all robustness and sensitivity checks are available upon request.

4. Discussion

The current research addressed a longstanding issue in the literature on trust and cooperation by investigating the extent to which social trust—expectations about the general cooperativeness and helpfulness of individuals and organizations—influences relational trust (or A’s belief about B’s trustworthiness with respect to matter Y). Two vignette experiments of simulated ‘car repair’ and ‘group project’ scenarios administered to two separate populations—two purposive samples (Amazon.com’s Mechanical Turk workers) and two random samples (public university undergraduate students)—produced five key findings.

First, my results replicated prior work revealing a positive relationship between general social trust and belief-, preference-, and behavioral-based measures of trust (Sapienza et al. 2013; Thöni et al. 2012). Respondents who reported high levels of general social trust also reported high mean-levels of relational trust across both experiments and populations. In these cases, a respondent’s trust in a simulated trustee with respect to a particular matter (‘car repairs’ or ‘project completion’) was a function of the expectations that a respondent held about the general cooperativeness of strangers. Importantly, these results were consistent across various measures of general social trust, including the classic binary general social trust question.

Second, the current research generated novel findings in that it was the first study to find a statistically significant relationship between particular social trust and relational trust. Across simulated scenarios and populations, higher levels of particular social trust were consistently related to higher mean-levels of relational trust. Interestingly, the positive effects for particular social trust were statistically equivalent to general social trust across all four studies. These results imply that my trust toward specific people about particular matters of interest is influenced to a similar degree by my beliefs in the general cooperativeness and helpfulness of friends, family, *and* strangers. This finding is unexpected given the literature’s emphasis on

general social trust in promoting cooperation, social order, and collective action (Fukuyama 1995) and on particular social trust in undermining these outcomes (Banfield 1958). The present findings thus provided evidence for the power of particular social trust *and* general social trust in fostering everyday interactions, exchanges, and transactions.

Third, I found weak evidence for the role of political trust in generating relational trust. In only one study (study 2a) was a positive and statistically significant effect between political trust and relational trust observed, while the remaining three studies consistently generated null findings. These results are important in light of current trends in social psychology and the research design used in the present manuscript. Recent publications in the social sciences (Murayama et al. 2013) and the medical sciences (Ioannidis 2005) have called for the collection of large samples and the replication of studies within a single manuscript to reduce false positive findings, which currently plague the psychological sciences (Simmons et al. 2011). If study 2a was the sole experiment, a number of false positives would have been reported; by creating two distinct vignette experiments and fielding these experiments in two separate populations, a positive relationship between political trust and relational trust was greeted with greater skepticism.

Fourth, I observed that much of the variation in relational trust—as dictated by the ICCs—occurred within-individuals (or at the vignette level), and that a small fraction of the variance resided between-individuals. In spite of this, I found that social trust did account for small to moderate amounts of variation at the individual level (as indicated by level-2 R^2 s), with effect sizes for particular social trust statistically equivalent to that of general social trust: anywhere from 0.04 to 0.11 standard-deviation-unit increases in relational trust were accounted for by one standard-deviation-unit increases in social trust. The implication being that the level

of relational trust A forms about B with respect to matter Y is contingent on A's beliefs about the general cooperativeness and helpfulness of friends, family, and strangers. These beliefs act as reference points and anchors that people employ when forming trust in specific actors for particular matters. And actors deviate from these anchors when characteristics of the trustee, the exchange relationship, and the situation change or evolve. In other words, the situation determines the extent to which an individual departs from their base level of relational trust, which is in part determined by their personal level of social trust. This, interestingly enough, provides support for the recent cognitive culturalist turn in sociology (Lizardo 2004; Lizardo and Strand 2010; Vaisey 2008) insofar as social trust—cognitive schemas partly determined by one's culture (Uslaner 2002, 2008)—shapes and molds relational trust.

Fifth, the observed relationship between social trust and relational trust was highly robust, emerging across studies with varying participant samples (MTurk workers and undergraduate students), sampling methods (purposive and random sampling), experimental contexts (car repair and group project scenarios), and control variables (attitudinal and demographic controls). With respect to the latter, a large number of control variables were statistically related to relational trust across the four experiments, such as betrayal aversion, inequality aversion, and risk aversion. But only social preferences reached statistical significance in at least three of the four experiments. This finding aligns with recent research in behavioral economics showing how false consensus effects can bias trust formation (Butler et al. 2013): the trust that A forms in B is a function of A's own trustworthiness; that is, one might ask oneself "How would I behave as a trustee under these circumstances?" when deciding whether to trust. If a truster is interested in how resources are allocated to the self *and* others, then false consensus effects could lead a truster to overestimate the extent to which a trustee cares about the

truster's interests, which would upwardly bias perceptions of trustworthiness and relational trust. Altogether, the methods employed in the present manuscript avoided possible false positive effects, such as men generating greater trust than women or risk takers producing greater trust than the risk averse, but supported the role of social preferences in forming trust.

4. Conclusion

All else equal, my findings suggest that the trust we form in specific people about particular matters stems from general social trust and particular social trust. In the absence of social trust, people are less likely to trust others. I conclude that general social trust and particular social trust are central to the development and maintenance of relational trust. The key implication being that individuals from regions around the world with high levels of social trust (e.g., Sweden) should, on average, produce stronger beliefs about the trustworthiness of another person with respect to a particular matter than individuals from regions with low levels of social trust (e.g., Brazil).

While my findings provide important insights into the processes that shape relational trust, areas for future research are available. Social scientists should investigate how alternative measures of social trust influence relational trust, as well as the extent to which social trust impacts relational trust in dynamic situations where trusters exchange with the same or different trustees through time. Future studies should also identify the extent to which relational trust mediates the link between social trust and exchange. In other words, does social trust indirectly affect exchange via relational trust or does social trust have a unique direct effect on cooperation? Finally, person X situation interactions addressing whether and how social trust moderates the situation-trust link would be a welcome addition to the trust literature.

APPENDIX A

Fig. A1. Distribution of Relational Trust (Panel A) and Individual-Specific Means (Panel B) in Study 1a.

Fig. A2. Distribution of Relational Trust (Panel A) and Individual-Specific Means (Panel B) in Study 1b.

Fig. A3. Distribution of Relational Trust (Panel A) and Individual-Specific Means (Panel B) in Study 2a.

Fig. A4. Distribution of Relational Trust (Panel A) and Individual-Specific Means (Panel B) in Study 2b.

APPENDIX B

Social scientists who conduct laboratory experiments and vignette experiments commonly employ two distinct measures of relational trust: self-reports and behavioral indicators. Because of their hypothetical nature, self-reports are common to vignette experiments, while behavioral indicators are common to laboratory experiments and economic games. Unsurprisingly, economists generally espouse the latter. Drawing on the revealed preferences literature in economics, behavioral economists have long measured relational trust (an internal cognitive process) with observed behavior in an investment or trust game (Berg et al. 1995; see Johnson and Mislin 2011 for a review). In the trust game, participants play the role of either a *sender* or a *receiver*. At the start of the game, the sender and the receiver are endowed with the same amount of monetary units (MUs). The sender moves first and decides how many MUs to send or not to send the receiver, which is normally doubled (or sometimes tripled) if sent. The receiver must then decide how many MUs to return to the sender, which marks the end of the game or that particular iteration once returned (or not returned). Economists typically regard the amount of MUs sent by the sender as a behavioral indicator of trust (what I refer to as *revealed trust*), and the amount of MUs returned by the receiver as a behavioral indicator of trustworthiness.

Since people often lack the self-awareness and self-knowledge to understand and identify the sources of their own attitudes, beliefs, and behaviors via introspection (Nisbett and Wilson 1977), economists generally regard behavioral indicators as more valid and reliable than self-reports. More specifically, self-reports of relational trust are less immune to response biases—including the attitudinal fallacy, social desirability bias, recency bias, and self-perception bias—

than revealed trust. In spite of these strengths, I argue that revealed trust captured in a trust game exhibits a number of critical shortcomings.

First, relational trust is a belief about another person's trustworthiness with respect to a particular matter at hand that emerges under conditions of unknown outcomes (see Hardin 2002; Cook et al. 2005). Since relational trust is a belief (or an internal cognitive processes) that serves as a sufficient condition—not a necessary *and* sufficient condition—for cooperation and exchange (Cook et al. 2005; Molm et al. 2007), revealed trust constitutes an indirect measure of relational trust that should be used with caution due to its low discriminant validity (see Cox 2004; Cox et al. 2008). Revealed trust, in other words, conflates relational trust with any number of internal psychological mechanisms that might account for investments in a trust game such as risk aversion, reciprocal altruism, and other-regarding preferences to name a few (see Bohnet and Zeckhauser 2004; Cox 2004; Sapienza et al. 2013). As Sen (1973) writes, economists "...have been too prone, on the one hand, to overstate the difficulties of introspection and communication and, on the other hand, to underestimate the problems of studying preferences revealed by observed behavior (p. 258)".

Second, investments in a trust game constitute tangible monetary stakes (Berg et al. 1995), which are relevant for generalizing to economic exchange in the real world. However, the conditions that characterize a trust game (or any other economic game) are artificial and the number of manipulated treatments in a trust game is usually small (e.g., iterative exchange, reputation, gender, etc.). While this is often considered a strength of behavioral experiments (the exploration of one or two causal factors in a controlled environment), it also limits their generalizability and ecological validity (Levitt and List 2007). For instance, there is a great difference between investing 10 MUs in a trust game and loaning \$10,000 to a friend. For the

former, stakes are low and information about the trustworthiness of a receiver is controlled and restricted to a small number of conditions (e.g., gender and reputation). For the latter, stakes are high and information about the trustworthiness of a receiver is uncontrolled, unrestricted, and nearly limitless. When people form trust beliefs in the wild they draw on countless bits of information—some with greater effects than others—ranging from nominal social categories to perceived motivations to the institutional environment among others. And when people form trust beliefs in the lab, they draw on the conditions provided to them. The consequence is a discontinuity between what is discovered in the lab and what actually occurs in the wild. For instance, lab subjects will use a trustee's race to predict behavior if that is the only information available to them, but the same effect will disappear in the presence of other information about a trustee (Castillo and Petrie 2010).

Guala (2012) recently applied this reasoning to research on strong reciprocity. In reviewing the literature, he showed that the link between strong reciprocity and cooperation was strong in the lab but weak in the wild: instances where social order emerges from or is sustained by strong reciprocity escape the anthropological record. Instead, "...social dilemmas in the wild are typically solved by institutions that coordinate punishment, reduce its cost, and extend the horizon of cooperation (Guala 2012: 1)". In other words, race and strong reciprocity matter for trust and cooperation, respectively, when these are the *only* conditions available to respondents. But once other treatments and conditions of greater importance are introduced, their effects approach zero. This is what I refer to as a *lab-wild discontinuity effect*.

In contrast, my research design relies on self-reports of relational trust measured in two distinct hypothetical situations, a 'car repair' scenario and a 'group project' scenario, where each scenario consists of numerous randomly assigned treatments. Instead of using indirect

behavioral indicators of relational trust, I employ direct introspective self-reports. I do so for a number of reasons.

First, my relational trust scale measures a respondent's self-reported strength of relational trust (or *what is*), not *why* a respondent reported a particular level of relational trust. This is a key distinction. Questions of *why* suffer from introspection biases (Nisbett and Wilson 1977), while questions of *what* do not (Vaisey 2014). For instance, imagine two actors, B and C, each of whom hold any number varying characteristics (e.g., class, gender, race, etc.). Person A is asked the extent to which s/he trusts both to perform particular matter Y. Because the question concerns *what* and not *why*, Person A should have little difficulty engaging this level of introspection. Now imagine that Person A is asked *why* s/he selected a particular level of trust. This is where a lack of self-knowledge and self-awareness biases Person A's response (see Jerolmack and Khan 2014). To answer questions of *why* across multiple levels of analysis, I eschew introspective accounts and, instead, employ numerous theoretically motivated vignette dimensions and individual-level variables. Note, however, that other response biases such as social desirability effects can unduly influence questions of *what* (Paulhus 1991), especially “sensitive items” like self-reports of drug use or sexual activity. Since self-reports of relational trust are not “sensitive questions” and vignette experiments provide subjects with multiple reasons to justify any particular judgement or item response (Wallander 2009), some response biases—such as social desirability bias—likely exert minimal influence on self-reports of relational trust.

These considerations inform my second reason for using vignette experiments with self-reports of relational trust: multifactorial research designs that go beyond five or six treatments are difficult—if not impossible—to implement in laboratory settings. In contrast, it is common

for—and considered a strength of—vignette experiments to investigate a large number of treatments in a single study (Hainmueller et al. 2014). Vignette experiments grant researchers the freedom to construct experimental settings that mirror conditions of trust formation found in the wild where numerous causal factors might influence trust. This allows researchers to conduct comparative analyses (e.g., investigate whether treatment A has a greater effect on outcome Y than treatment B) and to minimize lab-wild discontinuity effects, which should ultimately produce levels of relational trust that parallel those found in real-world ‘car repair’ and ‘group project’ situations. As such, vignette experiments are able to produce ecologically valid results with strong predictive validity when properly constructed (see Hainmueller et al. 2015; Hechter et al. 2005; Horne 2003)

To summarize, I argue that revealed trust—or investments in a trust game—is an indirect measure of relational trust, which is all too often observed under controlled and restricted conditions that rarely mirror those found in the wild. As consequence, the common “off the shelf” economic game used to understand the origins of relational trust—the trust game—exhibits poor discriminant validity, generalizability, and ecological validity. To address these issues, I propose and use self-reports of relational trust within the context of vignette experiments, which is an ideal method for capturing belief formation in controlled environments (Jasso 2006). The self-reports I use allow me to address questions of *what*, while the randomized vignette dimensions allow me to address questions of *why*. And doing so in the context of simulated real-world situations rich in detail increases the generalizability of my findings and the ecological validity of my research design.

ACKNOWLEDGEMENTS

Acknowledgments: the author thanks Maria Grigoryeva, Edgar Kiser, Ross Matsueda, Jerald Herting, Darryl Holman, Steven Pfaff, Aimée Dechter, Katie Corcoran, David Pettinicchio, Michelle Maroto, and the anonymous reviewers for comments and suggestions, Richard Callahan for coding assistance, Alan Li and Matt Weatherford for technical assistance, and the Department of Sociology, the Center for Studies in Demography and Ecology, and the Graduate School at the University of Washington for funding and software support. Author contributions: B. G. Robbins is the sole author of this article and is responsible for its content. Declaration of conflicting interests: the author declared that he had no conflicts of interest with respect to his authorship or the publication of this article. Funding: this research was supported by the National Science Foundation grant SES-1303577, which bears no responsibility for the analysis and interpretations drawn here.

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Table 1. Two-Level Correlated Random-Effects Models Exploring the Relationship Between Social Trust and Relational Trust

	Studies 1ab				Studies 2ab			
	Model 1		Model 2		Model 3		Model 4	
	<i>b</i> (<i>SE</i>)	β	<i>b</i> (<i>SE</i>)	β	<i>b</i> (<i>SE</i>)	β	<i>b</i> (<i>SE</i>)	β
General social trust	0.07** (0.03)	0.04	0.08** (0.03)	0.05	0.08* (0.04)	0.04	0.18*** (0.04)	0.11
Particular social trust	0.08** (0.03)	0.04	0.13*** (0.03)	0.08	0.08* (0.04)	0.04	0.16*** (0.04)	0.08
Political trust	0.02 (0.02)	0.01	-0.01 (0.02)	-0.01	0.11*** (0.03)	0.08	0.04 (0.03)	0.03
Egoistic preferences (ref.)								
Social preferences	0.20** (0.07)	0.03	0.20** (0.07)	0.04	0.42*** (0.11)	0.08	0.14 (0.10)	0.03
Unclassified preferences	-0.15 (0.14)	-0.01	-0.04 (0.14)	-0.004	0.17 (0.18)	0.02	-0.29 (0.17)	-0.04
Risk aversion	0.01 (0.01)	0.01	0.003 (0.01)	0.004	0.04* (0.02)	0.04	0.03 (0.02)	0.03
Inequality aversion	0.01 (0.01)	0.01	0.01 (0.01)	0.01	0.05*** (0.02)	0.06	0.04* (0.02)	0.05
Betrayal aversion	-0.03* (0.01)	-0.03	-0.01 (0.01)	-0.01	-0.03 (0.02)	-0.03	-0.06*** (0.02)	-0.07
Lend possessions	0.03 (0.04)	0.01	0.02 (0.03)	0.01	-0.02 (0.05)	-0.01	0.12* (0.05)	0.05
Lend money	0.01 (0.04)	0.004	-0.01 (0.03)	-0.01	0.04 (0.05)	0.01	-0.03 (0.05)	0.02
Door unlocked	0.01 (0.02)	0.002	-0.01 (0.02)	-0.002	-0.03 (0.03)	-0.02	-0.04 (0.04)	0.02
Age	-0.006 (0.003)	-0.02	-0.001 (0.003)	-0.01	-0.01 (0.01)	-0.02	0.01 (0.01)	0.02
Male	0.18** (0.06)	0.03	0.04 (0.06)	0.01	-0.07 (0.09)	-0.01	0.04 (0.09)	0.01
Constant	3.89***	0.56	3.86***	0.55	4.22***	0.68	3.05***	0.83
$\text{var}(e_{ij})$	0.57***	0.05	0.60***	0.04	0.58***	0.05	0.58***	0.05
$\text{var}(u_{0j})$	2.94***	0.07	2.13***	0.06	2.62***	0.09	1.97***	0.07
Vignette dimensions	Yes		Yes		Yes		Yes	
Vignette dummies	Yes		Yes		Yes		Yes	
Individual-specific mean dimensions	Yes		Yes		Yes		Yes	
Individual-level covariates	Yes		Yes		Yes		Yes	
Observations	12559		12757		8070		7558	
Individuals	1262		1281		817		765	

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$ (two-tailed)

Note: b = unstandardized slopes, SE = (robust standard errors in parantheses), β = standardized slopes. Model 1 = study 1a, model 2 = study 1b, model 3 = study 2a, model 4 = study 2b.