The ‘Social’ Part of Social Desirability: How Social Networks Influence the Survey Response*

Matthew T. Pietryka | mpietryka@fsu.edu
Dennis F. Langley | dfl12@my.fsu.edu
Florida State University | Department of Political Science

January 3, 2019

Survey researchers have dedicated increasing attention toward understanding how social networks influence individual-level outcomes like attitudes, political participation, and other forms of collective action. This work implicitly assumes that errors in individuals’ self-reported attitudes and behaviors are unrelated to the composition of their social networks. We evaluate this assumption, developing a theory explaining how social networks influence the survey response by shaping the social desirability of various behaviors and attitudes. We apply our theory to the study of political participation, examining evidence from three observational datasets and an experiment conducted on a national sample. We demonstrate that non-voting respondents’ tendency to falsely report having voted is driven by political participation levels among their close friends and family. We show that this tendency can artificially inflate estimates of social influence. This study therefore suggests that survey researchers must account for social influence on the survey response to avoid biasing their conclusions.

*We thank the FSU political science department for funding the CCES module; Brad Gomez for coordinating and administering the module; Anand Sokhey for sharing data from his CCES module; Jessica Parsons for research assistance; and Stephen Goggin, Philip Paolino, and Stefan Wojcik for thoughtful suggestions. This research also benefited from feedback during presentations at the University of British Columbia, the 2017 American Political Science Association annual meeting, and the 2017 Midwest Political Science Association annual meeting.

Scholars have increasingly portrayed political participation as a social, rather than an individualistic act. This work suggests voter turnout depends on social influence arising from one's political discussion network (Knoke 1990; Straits 1990; Mutz 2002; Huckfeldt, Mendez and Osborn 2004; Nir 2005; McClurg 2006; Jang 2009; Klofstad, Sokhey and McClurg 2013; Nir 2011), voluntary group associations (Sallach, Babchuk and Booth 1972; Uhlaner 1989), local neighborhood context (Olsen 1972; Eulau and Rothenberg 1986; Huckfeldt and Sprague 1995; Timpone 1998; Oliver 1996), and family structure (Glaser 1959; Wolfinger and Wolfinger 2008). These social forces seem to influence other forms of political participation (Alford and Scoble 1968; Huckfeldt 1979; Pollock 1982; Lake and Huckfeldt 1998; Tam Cho 1999; Kim, Wyatt and Katz 1999; McLeod, Scheufele and Moy 1999; Scheufele 2000; Scheufele et al. 2004; Eveland and Hively 2009; Klofstad, Sokhey and McClurg 2013), persisting across a variety of countries and contexts (Fitton 1973; Zipp and Smith 1979; Harell 2009).

Like most political behavior research, studies of social influence tend to rely on survey data. Unfortunately, surveys may present overlooked problems for this body of work. As scholars are widely aware, survey reports are often biased by social desirability; respondents falsely report holding attitudes and behaviors that others will view favorably while obscuring their attitudes and actions that others might deem distasteful. Yet no research examines how individuals' immediate social networks contribute to social desirability bias in survey reports. This problem can be mitigated by relying on validated outcomes, but existing work typically does not. Indeed, *all* of the studies cited above examine self-reported participation.

By overlooking the social network's influence on social desirability bias, previous survey research may have suggested incorrect conclusions about the magnitude of social influence. When measures that predict biased reporting are used as explanatory variables in models of self-reported attitudes and behaviors, the estimated relationship will often be biased (Ansolabehere and Hersh 2012; Bernstein, Chadha and Montjoy 2001). If social pressure encourages biased responses, apparent social influence on self-reported behavior may instead reflect only social influence on the survey response. Such a confound would present a serious problem because much of the current literature on social influence in political behavior relies on self-reported outcomes.[1]

We therefore explore the extent to which self reports may bias our understanding of social influence in political behavior. We focus on electoral turnout as our outcome variable because scholars, journalists, and the public at large widely recognize its essential role in democratic governance. And unlike most survey measures, self-reported turnout can be checked against government voter files, allowing us to examine individual variation in misreporting. Across three surveys administered in different times and locations, we demonstrate that overreports—cases where non-voters report having voted—are more common among respondents whose associates participate at greater rates. We then show that this pattern can lead to biased estimates of social influence when the socially-desirable outcome variable is self-reported. Finally, we present an experiment conducted with a large, national sample, demonstrating a causal link between the salience of associates' participation and one's own tendency to overreport. This experiment evaluates several possible mechanisms by which social influence might arise. Our research thus provides insight for elections scholars about the determinants of participation. For survey researchers, it reveals unexplored sources of social desirability bias. And for network scholars, it provides insight into the mechanisms driving social influence.

Social desirability encourages biased survey responses

When responding to surveys, people often attempt to present themselves in a manner they feel would be most favorable to others. This impression management strategy (Goffman 1959) encourages survey respondents to misreport their true beliefs and actions that others

[1]A skeptical reader may argue that the relationship between social networks and social desirability is too obvious to merit attention. Yet the widespread publication of social influence studies relying on self-reported data suggests this relationship and its implications have *not* been obvious to prominent social influence scholars, journal editors, or reviewers.

might deem distasteful. Such dissembling is most common on items measuring behaviors and beliefs that are widely practiced or commonly abhorred. For instance, Ansolabehere and Hersh (2012) demonstrate that survey respondents misreport most frequently on items like turnout and voter registration where a socially desirable option is available. But respondents rarely misreport on items like racial identification, where no unambiguous societal norm exists (Ansolabehere and Hersh 2012). Of course, tastes vary; not everyone agrees on which practices are desirable and which should be avoided. Individuals who fail to recognize or endorse a survey option's social desirability should feel little need to misreport.

Why do some people feel compelled to misreport socially-undesirable traits while others lack this compulsion? Extant explanations focus on respondents' personal attributes, demonstrating that overreporting increases with respondents' education, income, and political interest (Ansolabehere and Hersh 2012; Silver, Anderson and Abramson 1986). These same characteristics also predict actual participation, suggesting that people who participate most frequently also feel the strongest pressure to overreport (Bernstein, Chadha and Montjoy 2001). Following this logic, the strong relationship between social networks and turnout (e.g., Kenny 1992; Leighley 1990; Mcclurg 2003; Nickerson 2008; Rolfe 2012) implies an analogous relationship between social networks and turnout *misreports*. Though work has yet to examine this relationship directly, contextual analysis provides suggestive evidence. People overreport voting more frequently in electoral contexts that tend to have greater aggregate participation (Karp and Brockington 2005). This pattern suggests that the social desirability of turnout may depend on societal norms. Unfortunately, no work on social desirability bias examines the mechanisms by which individuals internalize these norms.

A rich history in social influence suggests that individuals look to their close friends and family members to determine the normative appeal of actions and beliefs (Duncan, Haller and Portes 1968; Erickson 1988; Festinger, Schachter and Back 1950; Homans 1961). Yet this intimate social group, which scholars often label the core network, has received little attention in theories or empirical analyses of survey misreports.[2] This inattention presents a problem because so much research on social influence relies on self reported data.

Previous studies of social influence may be biased

If the social network affects the survey response, we must reevaluate studies of social influence that rely on self reports. As Bernstein, Chadha and Montjoy (2001, 22) explain, "using reported votes in place of validated votes substantially distorts standard multivariate explanations of voting, increasing the apparent importance of independent variables that are related in the same direction to both overreporting and voting and sharply decreasing the apparent importance of independent variables related in opposing directions to those two variables." If social pressure increases the social desirability of identifying in surveys as a voter, previous studies of voting may have overstated the effect of individuals' social networks. And, indeed, most existing research has relied on self-reports (e.g., Huckfeldt, Johnson and Sprague 2004; Klofstad, Sokhey and McClurg 2013; McClurg 2006).

This problem may not only impact studies of social influence, but also behavior studies that have ignored the social network entirely. By failing to control for the network, previous work may have misestimated the impact of other voting predictors that are correlated with social networks. The inattention to the social basis of social desirability will be inconsequential if wealth, education, and other individual attributes commonly used as controls account fully for variation in the social pressure generated by network members. If rich, educated people comprise the majority of individuals with participatory networks, then

[2]Two studies address this issue indirectly, though their substantive interests lie elsewhere. Fowler (2005, 287) suggests, but does not test, the possibility that social networks influence turnout overreports. And Bond et al. (2012) explore a related premise on p. 4 of their online supplementary information: whether perceptions of network turnout encourage false reports of voting in social media. The analysis suggests only a weak effect. While this study may suggest the network plays only a marginal role in misreporting, we expect a stronger relationship for *survey* misreports. Brenner (2012) argues that turnout overreporting is driven by respondents' social identification as voters rather than nonvoters. In Fowler et al.'s study, Facebook users chose whether to provide a message to their friends stating that they voted; there was no option to advertise that they did not vote. Thus Facebook users may have felt less pressure to dissemble because dismissing the message without advertising their participation did not identify them as "nonvoters."

previous studies of turnout overreports may have captured this potential influence by proxy. And, by controlling for these characteristics, studies of social influence in turnout can purge the bias created by self-reports. If these individual attributes provide insufficient proxies for network-induced social pressure, however, the social influence estimates are likely to be biased. To help researchers evaluate the potential that these problems arise, we need a theory that can explain how social networks influence the survey response.

A *social* theory of the survey response

The social network can influence individual attitudes and beliefs through two mechanisms (Deutsch and Gerard 1955). First, *informational influence* occurs when the network provides new information that changes an individuals' beliefs. Second, the network may change an individual's behavior by creating *social pressure* to conform, even in the absence of informational influence (Carlson and Settle 2016). Social pressure arises from one's desire to win peers' approval or avoid condemnation.

In the context of turnout misreports, the network may create informational influence by changing people's beliefs about broad, societal norms. For instance, someone might use an availability heuristic (Tversky and Kahneman 1973), leading them to infer that more people participate nationally if they are surrounded by people wearing "I voted" stickers than they would if surrounded by people lacking these stickers. Or more localized informational effects arise if they infer that many of their peers vote, regardless of the societal norm.

Alternatively, the network may influence turnout misreports through social pressure, altering an individual's perception of the social rewards of voting or the social sanctions for abstaining (Anoll 2018). To influence survey reports, this pressure must operate even in anonymous survey settings. Drawing from Goffman (1967), Scheff (1988) argues that social pressure arises because people *anticipate* the pride or shame they would feel if their beliefs or actions were made public. And thus they perpetually feel this social pressure—even in situations where their peers are absent.[3]

Both mechanisms suggest a common expectation: People with more participatory networks should be more likely to overreport turnout. If informational influence drives this process, people with more participatory networks should infer a stronger societal norm toward participation. If social pressure drives this process, people with more participatory networks should envision these social rewards or sanctions more intensely. In either case, the social desirability arises from people's mental reconstructions of their networks.

For these mechanisms to operate, people's perceptions of their network need not be accurate. Indeed, work on social influence in participation suggests that when perceptions of network characteristics diverge from reality, their perceptions are often more consequential than their networks' actual characteristics (Huckfeldt 2007; Ryan 2011). Like many political perceptions, people's mental reconstructions of their networks should be governed by recent interactions and cues in the immediate context (Zaller and Feldman 1992), which may lead people to over- or under-estimate their associates' participation. On one hand, political discussion is typically initiated by the most politically engaged (Huckfeldt and Mendez 2008). When this group is salient, people may overestimate the frequency of political participation in their network, increasing the social desirability of reporting turnout. On the other hand, a growing body of the public perceives politics as distasteful, and systematically conceal their political involvement from their peers (Klar and Krupnikov 2016). When this group is salient, people may underestimate the frequency of political participation in their network, decreasing the social desirability of reporting turnout.

To summarize, we posit that people rely on a mental reconstruction of their social network to ascertain the social desirability of actions or beliefs. Just as people leaving their house might bring along their phone, keys, purse, or wallet, they also bring a mental image of their network. This mental image, we argue, governs the social desirability of particular responses

[3]This perspective provides a plausible explanation for the persistence of turnout overreporting even in self-administered, online surveys (Ansolabehere and Hersh 2012).

to survey questions. Our empirical analysis therefore begins with observational analysis of three datasets, examining the relationship between the core network and turnout misreports. Our theory suggests two mechanisms by which the network may influence misreports. This mental image might create social desirability bias through informational influence, altering individuals' awareness of societal norms. Or it may create social desirability bias through social pressure by altering people's perceptions about the social rewards of voting. After the observational analysis, we therefore present an experiment designed to evaluate these mechanisms and the causal logic underpinning our theory.

Observational study

For empirical evidence, we begin by asking two questions to assess the potential for networks to bias the conclusions drawn from observational survey data. First, do people with more participatory networks overreport voting at greater rates? Second, do models relying on self-reported turnout suggest greater social influence than would models relying on validated turnout? As we show below, the answer to both questions is yes.

Observational data

To address these questions, we need individual-level measures of self-reported turnout, validated turnout, and turnout within respondents' immediate social networks. In survey research, social networks are typically measured with a *name generator* battery (see e.g., Laumann 1973; Marsden 1987), which ask respondents to identify the names or initials of their closest friends, family, or other associates. Name generators remain rare on political surveys, despite increasing scholarly attention to social networks. And most surveys with name generators—like the 2000 American National Election Study—offer only self-reported turnout measures. Only two preexisting studies fit our needs: The 1984 South Bend Study and the 2010 Williamsburg Study. To expand the temporal and geographic scope of our study, and to examine our theory on a nationally-representative sample, we added a name generator battery to the 2016 Cooperative Congressional Election Study (CCES).

The South Bend Study: Huckfeldt and Sprague (2006) combined a three-wave panel survey with validated records of respondents' participation in elections. These data have been used in many prominent studies of social influence (e.g., Huckfeldt and Sprague 1995; Kenny 1992; Klofstad, McClurg and Rolfe 2009; Mutz and Mondak 1997). The survey's first wave contacted approximately 1,500 respondents within the South Bend metropolitan area and the second and third waves of the study attempted to recontact these individuals, supplementing panel attrition with new respondents. We restrict our analysis to the 1,510 individuals who responded to the third wave, which included both the turnout measure and name generator.[4]

Respondents were randomly selected within 16 South Bend neighborhoods. Thus, with over 90 respondents per neighborhood, the sample is intended to be representative within neighborhoods, but not of South Bend as a whole.[5] Huckfeldt and Sprague accessed the Indiana voter file to provide a validated turnout measure for these respondents.

The Williamsburg Study: This multiwave panel, collected by Miller et al. (2015), surveys William & Mary students around the time of the May 2010 Williamsburg municipal elections. The survey targeted all 5,726 students on the college master email list. We restrict our analysis to the 1,735 students older than 18 years who responded to both the first and third waves. The first wave, collected from late February to early March, included measures of respondents' social networks, demographics, and political attitudes.[6] The third wave,

[4]Self-reported turnout was measured with the item, "In talking to people about the election, we find that a lot of people weren't able to vote for one reason or another. How about you? Did you vote this fall, or did something keep you from voting?" [with response options: "Yes, voted"/ "No, did not vote"]. The name generator focused on up to three people with whom the main respondent discussed politics: "Can you give me the FIRST names of the three people you talked with most about the events of the past election year? These people might be from your family, from work, from the neighborhood, from church, from some other organization you belong to, or they might be from somewhere else."

[5]In all South Bend analyses, we cluster the standard errors on neighborhoods to account for this sampling strategy.

[6]The name generator focused on up to five friends among the students at William & Mary: "One of the purposes of this survey is to examine the flow of political information on campus between people who know each other. We are particularly interested in knowing whether people who are friends have similar opinions and thoughts

collected immediately following the election, included the self-reported turnout measure.[7] The data also provide validated turnout measures from the Williamsburg voter file.

CCES: Our new data come from a name generator battery included on two modules of the 2016 CCES pre-election wave.[8] A post-election wave collected self-reported turnout, yielding 1,512 valid responses from across the US.[9]

Together, these three datasets offer useful variation in both measurement and context, as summarized in Table 1. The South Bend Study and the CCES both collected up to three names, targeting associates with whom they discussed politics. The Williamsburg Study collects up to five names, targeting close friends. Though political discussion networks differ from friendship networks, they often overlap because people tend to discuss politics most frequently with those whom they discuss other important matters: their significant others, close friends, and family (Huckfeldt, Johnson and Sprague 2004; Klofstad, McClurg and Rolfe 2009). The South Bend Study included both the name generator and turnout measure on the same post-election wave. The other two datasets collected the name generator before the election and turnout after, establishing temporal precedence. The South Bend Study and the CCES each rely on respondents' perceptions of network participation while the Williamsburg Study provides validated turnout for network members.[10] The South Bend Study relied on

about politics. To help us answer this question, we are asking you to give first and last names of up to five of your closest friends who attend William and Mary."

[7] The survey text read,"Did you vote in the May 4th city council election?" ["Yes"/"No"]. The survey only asks the turnout question to respondents who reported that they were registered to vote in the City of Williamsburg. In the analyses we present below, respondents who reported that they were not registered are treated as non-voters. Restricting the sample to registered voters yields analogous conclusions.

[8] The name generator collected up to three names with the prompt, "From time to time, people discuss government, elections, and politics with other people. Who are the people with whom you discuss these matters? These people might or might not be relatives. Can you think of anyone? Please enter their first name in the box below."

[9] We draw the turnout and control variables from the CCES common content data posted to `https://doi.org/10.7910/DVN/GDF6Z0` on 2018-02-10. The post-election turnout item read, "Which of the following statements best describes you?" [respondents are coded as voters if they chose, "I definitely voted in the General Election on November 6."].

[10] The South Bend PIs did not validate the participation of discussants identified by the main respondents and the identities of these discussants have been anonymized, preventing a new validation effort. The South Bend study PIs also attempted to interview some of the discussants identified by the main respondents. In principle, we could instead rely on discussants' self-reported turnout. Unfortunately, this "snowball" sample was much smaller than the main respondent sample. Main respondent's provided turnout reports of 4,153 discussants while only 891 of these discussants provided self-reports. Given the similarity between main

Table 1: Comparing the observational datasets

	South Bend	Williamsburg	CCES
Year	1984	2011	2016
Validated turnout	Yes	Yes	Yes
Self-reported turnout	Yes	Yes	Yes
Name generator	Yes	Yes	Yes
Maximum number of names elicited	3	5	3
Type of network	Political discussion	Friendship	Political discussion
Network measured prior to turnout	No	Yes	Yes
Network participation	Perceived	Validated	Perceived
Survey mode	Face-to-face	Online	Online
Sampling Frame	Local	Local	National
Electoral context	Presidential	Municipal	Presidential
N - total	1,510	1,735	1,512
N - validated voters	1,010	400	960
N - nonvoters	500	1,335	552

face-to-face interviews while the other two studies were self-administered online. Since self-administered online studies may reduce social desirability bias (Holbrook and Krosnick 2010), these two datasets provide hard tests for the theory. The South Bend Study and Williamsburg Studies focus on specific communities while the CCES draws respondents from across the country. Finally, the Williamsburg Study focuses on a low-turnout local election while the remaining two studies focus on high-turnout national elections. Despite these broad differences, each dataset suggests a heretofore overlooked social component of turnout overreports, as we show below.

Observational results

Using these data, the analysis proceeds in two stages. First, we examine overreporting by restricting the sample to nonvoters and regressing self-reported turnout on network participation. We define nonvoters as respondents who were not identified as voters in

respondents' perceptions of discussant characteristics and discussants' own self-reported characteristics (Huckfeldt 2001), we rely on the larger sample. The Williamsburg Study provides validated turnout measures for both respondents and their friends. To avoid simultaneity bias, we use the friends' validated turnout in the 2009 VA gubernatorial election. The CCES name-generator battery collected only first names. Asking only for first names increases response rates but prevents a validation effort.

the public voter file for the focal election.[11] Second, relying on the entire sample, we estimate turnout models using alternately self-reported and validated turnout as the outcome measures. This second analysis allows us to assess whether these two measures of turnout lead to differing conclusions about the strength of association between network participation and respondent voting.

In all models, we control for common predictors of overreporting and turnout. These measures are drawn from Ansolabehere and Hersh (2012), a recent comprehensive study of turnout overreporting on surveys.[12] We also include a variable indicating whether the respondent failed to identify any associates with the name generator battery. We do not exclude these individuals from analysis because respondents failing to name discussion partners are nonetheless typically embedded in meaningful political communication networks (Eveland, Hutchens and Morey 2013). Details about item wording and variable values for these and other items can be found in section A of the online Supporting Information (SI); summary statistics can be found in SI B. To avoid bias emerging from listwise deletion, we use 50 imputations of the explanatory variables.[13]

Network participation predicts overreporting

Table 2 presents logistic regressions of self-reported turnout among validated nonvoters. The first model in each dataset presents the bivariate relationship between overreporting and `Network Participation`—the proportion of the main respondent's discussants identified

[11]Like previous work, we do not examine underreporting because many nonvoters falsely report voting, but voters seldom report abstaining (e.g., Ansolabehere and Hersh 2012; Silver, Anderson and Abramson 1986).

[12]For the Williamsburg analyses, we must deviate from the Ansolabehere and Hersh (2012) controls in several ways. Since student samples provide little variation in respondents' age, education, income, or marital status, the survey omitted these items. It also omitted a measure of respondents' church attendance. We proxy for age and education with indicators of respondents' academic class standing. We proxy for income with an item asking about respondents' socioeconomic status.

[13]The practice of dropping all cases with even a single missing value, commonly referred to as listwise deletion, reduces the model's degrees of freedom unnecessarily. Further, it biases the coefficient estimates anytime people with missing responses differ systematically from those with complete responses. Multiple imputation addresses both problems, preserving all valid responses and reducing the bias arising from differences between observed and missing cases (for details, see Rubin 2009). Though many imputation methods exist, we rely on conditional multiple imputation because it tends to perform better than alternative approaches on the categorical and ordinal variables commonly included in surveys (Kropko et al. 2014). We generated the imputations using the `mice 2.30` package (Buuren and Groothuis-Oudshoorn 2011) in `R 3.3.3` (R Core Team 2017). For details about the imputations, see SI C.

as voters. The second model introduces the controls. Since the sample is restricted to nonvoters, positive coefficients associated with network participation indicate people with more participatory networks are more likely to *overreport* turnout.

In all three datasets, the first model suggests that nonvoters with more participatory networks are more likely to report that they voted. The substantive impact of these results can be seen by using the estimates to generate predicted probabilities. In South Bend, nonvoters who believed all of their discussants voted are 17 percentage points more likely to report turnout than nonvoters who believed none of their discussants voted. The analogous difference is 43 percentage points in Williamsburg and 26 points in the CCES.[14]

The second model for each dataset introduces the controls. In South Bend and Williamsburg, the difference between nonvoters in participatory and non-participatory networks is weaker after conditioning on the other predictors, but remains substantively and statistically significant. After conditioning, the South Bend model predicts that nonvoters in fully-participatory networks will be eight percentage points more likely to report voting than nonvoters in fully-abstaining networks.[15] The Williamsburg model predicts a 20 percentage-point difference. Compared to the first model, the second CCES model predicts a substantively larger difference between people in fully participatory or fully non-participatory networks—34 percentage points.

In all three cases, a substantively important relationship remains after introducing the controls, suggesting that the literature on overreporting has missed the *social* aspect of social desirability bias. The controls in our models have been used in this literature to identify citizens who feel the strongest social pressure to participate—and hence social

[14]The CCES provides two ways to measure validated turnout, which differ based on how respondents who are not matched to a voter file are handled. Here, we follow convention by treating these unmatched respondents as nonvoters. Berent, Krosnick and Lupia (2016) argue that this approach introduces too much error because respondents may go unmatched due to poor record keeping rather than abstention. To address this concern, an alternative approach is to exclude unmatched respondents from analysis, identifying respondents as nonvoters only if they were explicitly reported as such in the voter file. In SI D, we reestimate our models with this alternative specification, yielding almost identical results.

[15]To reduce ceiling effects, the reported estimates are based on covariate profiles where ordinal covariates are set to their minimum values and dummy-coded variables are set to their baseline values.

Table 2: Nonvoters embedded in more participatory networks are more likely to report that they voted.

	Bivariate			Controls		
	Estimate	(SE)	p value	Estimate	(SE)	p value
South Bend						
Intercept	0.17	(0.249)	.505	-3.25	(0.705)	$<$ .001
Network Participation (as a proportion)	0.73	(0.345)	.036	1.05	(0.445)	.019
Controls?	No			Yes		
AIC	630.29			473.04		
N - Observations	500			500		
N - Imputations	50			50		
Williamsburg						
Intercept	-2.17	(0.107)	$<$ .001	-3.56	(0.469)	$<$ .001
Network Participation (as a proportion)	2.29	(0.523)	$<$ .001	2.08	(0.538)	$<$ .001
Controls?	No			Yes		
AIC	1008.46			984.47		
N - Observations	1335			1335		
N - Imputations	50			50		
CCES						
Intercept	0.59	(0.512)	.247	-2.18	(0.962)	.023
Network Participation (as a proportion)	1.20	(0.585)	.040	1.64	(0.724)	.024
Controls?	No			Yes		
AIC	487.17			409.23		
N - Observations	552			552		
N - Imputations	50			50		

Note: This table displays coefficients from logistic regressions of turnout on network participation. The models include only individuals who are nonvoters according to the public voter files. In each dataset, the first model displays the bivariate relationship and the second model displays the relationship after conditioning on controls for gender, marital status, residential mobility, church attendance, political interest, party identification strength, ideology strength, education, income, race/ethnicity, age, and whether the respondent failed to identify a discussant using the name generator (SI D reports the complete results, including the coefficients from these control variables). The standard errors in the South Bend models are corrected for clustering at the neighborhood level. The CCES estimates rely on post-election weights.

pressure to report participating even when they have not. Nonetheless, a strong relationship between overreporting and network participation persists in the presence of these controls. By comparison, education provides the most consistent predictor of overreporting in past work (Ansolabehere and Hersh 2012, 441) and our results reaffirm that pattern (see SI D for our complete model results). In our data, people in the top half of education tend to overreport at rates 10-20 percentage points greater than the least educated—differences that are similar in magnitude to those associated with a minimal to maximal shift in network participation. Thus, prior work has been ignoring substantial variation in individuals' propensity to overreport turnout. To account for this pattern, these analyses suggest common controls serve as insufficient proxies for the network. Controlling for this bias will be easier if researchers explicitly incorporate network participation into their models.

Self-Reported turnout can bias estimates of social influence

Since network participation predicts overreporting—even after controlling for commonly-used explanatory variables—it has the potential to bias estimates of social influence. To determine whether this potential is realized, Table 3 reports models regressing individual-level turnout on network participation. Unlike the previous results, these models are not restricted to voters. The first model for each dataset relies on self-reported turnout as the outcome variable. The second model uses validated turnout.

The table suggests that using self-reported turnout rather than validated turnout can lead to distinct conclusions about the extent of social influence in voting. In all three models based on self-reported turnout, the coefficient associated with network participation is positive and significant. In two of the three models based on validated turnout, these coefficients are close to zero and lack statistical significance. In South Bend, the model predicts that people in fully-participatory networks report turnout at rates 12 percentage points greater than those with fully-abstaining networks. The corresponding difference for validated turnout is only one percentage point. In the CCES, the self-reported model suggests a 26 percentage-point

Table 3: Models relying on self-reported turnout suggest greater social influence than do models relying on validated turnout.

	Self-Reported turnout			Validated turnout		
	Estimate	(SE)	p value	Estimate	(SE)	p value
South Bend						
Intercept	-2.05	(0.458)	< .001	-0.82	(0.418)	.048
Network Participation (as a proportion)	0.73	(0.397)	.066	0.05	(0.234)	.827
Controls?	Yes			Yes		
AIC	834.75			1694.83		
N - Observations	1510			1510		
N - Imputations	50			50		
Williamsburg						
Intercept	-1.18	(0.273)	< .001	-1.07	(0.299)	< .001
Network Participation (as a proportion)	2.26	(0.382)	< .001	1.91	(0.381)	< .001
Controls?	Yes			Yes		
AIC	2123.64			1804.75		
N - Observations	1735			1735		
N - Imputations	50			50		
CCES						
Intercept	-0.47	(0.870)	.592	-0.58	(0.747)	.442
Network Participation (as a proportion)	1.13	(0.545)	.038	-0.28	(0.445)	.530
Controls?	Yes			Yes		
AIC	590.78			1661.27		
N - Observations	1512			1512		
N - Imputations	50			50		

Note: This table displays coefficients from logistic regressions of turnout on network participation. In each dataset, the first model measures turnout with self-reports and the second model uses validated records. All models control for gender, marital status, residential mobility, church attendance, political interest, party identification strength, ideology strength, education, income, race/ethnicity, age, and whether the respondent failed to identify a discussant using the name generator (SI D reports the complete results, including the coefficients from these variables). The standard errors in the South Bend models are corrected for clustering at the neighborhood level. The CCES estimates rely on post-election weights.

difference, compared to the validate model's predicted -6 point difference, which is in the opposite direction than one would expect and lacks statistical significance.

Williamsburg serves as the exception because the self-reported and validated models suggest the same conclusion. Compared to respondents with no participation in their networks, those with fully-participatory networks are 51 percentage-points more likely to *report* turnout and 44 percentage points more likely to *actually* turn out.

The different substantive conclusions supported by the models indicates that researchers may be mislead if they rely only on self-reports. Using the South Bend and CCES data, researchers without access to validated turnout may conclude that individuals' turnout decisions depend on their networks; with validated turnout, they would be unable to reject the null of no influence.[16] These disparate results across the three datasets suggests that researchers cannot predict ex ante the extent to which relying on self-reported turnout will bias their conclusions.

Experimental study

The analyses above establish the relationship between networks and overreporting, but leave several questions unanswered. First, do more participatory networks *cause* overreporting or is the relationship spurious, caused perhaps by aspects of the broader context that encourage people to overreport and encourage their networks to participate? For instance, groups of friends may all be targeted by the same geographically-focused mobilization drive, increasing their chances of voting and the pressure they feel to report voting on surveys. Second, if networks cause overreporting, through what mechanisms do they operate? As we explain above, social influence may arise from informational effects or social pressure. We therefore designed an experiment that allows us to explore these questions.

[16]The inability to reject the null in the South Bend and CCES studies does *not* provide strong evidence for an absence of social influence (see Rainey 2014). Though the validated coefficients are smaller than the self-reported coefficients, they are not estimated with enough precision to rule out important substantive effects.

Experimental design

An ideal experiment would compare overreporting among individuals who have been randomly assigned to more or less participatory discussion networks. Random assignment is infeasible here, however, since real-world discussion networks tend to include individuals' closest friends and family (Huckfeldt, Mendez and Osborn 2004). Nonetheless, random assignment provides a means to exogenously influence a central mechanism in our theory, which posits that respondents carry with them a mental representation of their network which conditions the social desirability of various responses. We can therefore examine this mechanism by exogenously influencing their mental representation of the network, rather than influencing the network itself.

Network participation prime

We embedded the experiment within a standard name-generator battery, which we included on a post-election module of the 2016 CCES, yielding a national sample of 841 respondents. We influence respondents' perceptions of their networks by priming them to think of more or less participatory associates. The network turnout prime thus asked respondents to think of someone, "who you believe [voted / did not vote] in the General Election on November 8? What is that person's first name?". Our theory suggests respondents will overreport at greater rates if they are primed to think of an associate who voted (we refer to these respondents as the `associate voted` group, N = 439) rather than an associate who did not vote (the `associate abstained` group, N = 402). Our key outcome is therefore the respondent's self-reported turnout, which we asked shortly after the name generator.[17]

If individuals in the `associate voted` group report higher levels of turnout, we can conclude that these individuals are *overreporting* at greater rates. This conclusion is warranted because the experiment occurred after the election and therefore cannot have influenced

[17] Some people may have been unable to think of a voter or non-voter in their network. Others gave obviously false names like "Hillary Clinton." Since we cannot be certain respondents imagined a real associate, our results represent intention-to-treat (ITT) effects, providing a more conservative test of our theory.

whether respondents actually voted. Moreover, by randomizing the treatment, we can be confident that no differences exist between treatments in their baseline propensity to vote *or* misreport voting.[18] The observational analyses above assume that the validated measures are free of error, an assumption that Berent, Krosnick and Lupia (2016) call into question. Since a validation effort is not necessary for the experimental analysis, the design allows us to test our theory without this restrictive assumption. To see whether any observed effects depended on the type of relationship the name generator elicited, we asked respondents to name either a close associate ("close friend or family member"), a less intimate associate ("coworker or casual acquaintance"), or someone they identified with ("person like you"). All three wordings yielded similar effects, as shown in SI F.

National turnout cue

As we explain above, networks may influence misreports through information effects or social pressure. Information plays a role if their network provides information about how typical people behave, helping them make inferences about societal norms. If the network influences overreporting through this mechanism, there must be a causal link between higher perceptions of national turnout levels and greater overreporting. To provide causal leverage on this relationship, we randomly embedded one of three cues about national participation levels immediately prior to the name generator. One group received no cue (`no cue` group, N = 293). The second group received a cue stating that, "Nationwide, approximately 37% of eligible voters cast votes in the 2014 U.S. elections" (`low national turnout` group, N = 268). The third group received a cue stating "Nationwide, approximately 59% of eligible voters cast votes in the 2012 U.S. elections" (`high national turnout` group, N = 264).[19]

[18]We present balance statistics for pre-treatment demographic variables in SI section E. Only small differences emerge between treatment groups. In SI E, we nonetheless replicate our analyses after controlling for the covariates that show even moderate imbalance. These models suggest the same substantive conclusions as those reported below.

[19]These turnout estimates come from McDonald (2016).

The national turnout cue was assigned independently from the name-generator text. Thus, a subject in the `associate voted` and `low national turnout` groups received the following prompt:

> Lots of people vote in federal elections, but many others do not. Nationwide, approximately 37% of eligible voters cast votes in the 2014 U.S. elections. Think about a specific [close friend or family member / coworker or casual acquaintance / person like you] who you believe voted in the General Election on November 8? What is that person's first name?

Dependent variables

This design allows us to compare the relative impact of interpersonal relationships and societal norms on misreports by comparing the effect of the network turnout prime to the effect of the national turnout cue.

Further, we included several questions to help us distinguish information effects from social pressure. If social influence occurs through information, the network treatment must change respondents' perceptions about national turnout or turnout within their own networks. We therefore examine whether the treatments changed these perceptions, asking about turnout nationally ("Thinking about all eligible voters in the U.S., what percentage of these people do you think voted in the General Election on November 8?") and within respondents' own networks ("Thinking about all the people you talk with about matters that are important to you, what percentage of these people do you think voted in the General Election on November 8?").[20] Asking about perceptions of national turnout also serves as a manipulation check for the national turnout cue. If social influence occurs through social pressure, respondents should anticipate greater social sanctions if a peer were to learn that they did not vote. We therefore asked, "Thinking about [FIRST NAME], how disappointed would they be if they learned that you did not vote in the election this year?". If participation levels in one's immediate social network increase social pressure, then

[20] We randomized the order of these two questions.

individuals in the `associate voted` group should anticipate greater disappointment than those in the `associate abstained` group.

Experimental results

Figure 1 displays the results.[21] In the figure and the text below, we report p-values from difference in means tests, estimated via OLS with heteroskedasticity-robust standard errors, but we find substantively identical results with randomization inference (see Keele, McConnaughy and White 2012).

Figure 1A shows the effect of the network participation prime on the proportion of respondents who reported voting in the 2016 election.[22] Both groups reported high levels of turnout as is typical in surveys, but this tendency was particularly strong for respondents primed to think about an associate who voted. Almost 90% of these `associate voted` respondents reported turnout, compared to 82% of the `associate abstained` respondents, who were primed to think of an associate who did not vote ($p < .01$). To put this effect in perspective, this difference is almost as large as the twelve percentage point difference in overreporting between weak partisans and pure independents shown in the CCES column of the observational analysis (Table D1 in SI D). And it is roughly half as large as the twenty percentage point difference between the most and least educated individuals.[23]

Figure 1B shows that the cue about national turnout levels had little effect on self-reported turnout. If perceptions of societal norms cause overreporting, we would expect people

[21] The survey allowed responses to skip questions and thus two people did not respond to the turnout report and 10-16 others did not respond to the other dependent variables. SI G documents this non-response and provides sensitivity analysis demonstrating that the conclusions below are robust to any possible assumptions about the true values of the missing variables.

[22] Applying survey weights can bias estimates of treatment effects and we therefore present unweighted results, as recommended by Franco et al. (2017).

[23] To verify that these results arise from *overreporting*, rather than actual differences in turnout, we have replicated this analysis after excluding validated voters from the sample. Within this subset (N = 367), subjects primed to think of a voter were ten percentage points more likely to report turnout than those primed to think of a non-voter (p = 0.051). Given the concerns over the accuracy of government records (Berent, Krosnick and Lupia 2016), our primary analysis focuses on the full sample. Reassuringly, there is only a one percentage point treatment effect for the validated voters in the sample (p = 0.34), suggesting the effect is not driven by the voters in the sample.

primed with the lower turnout estimate to be less likely to report voting. Instead, the `low national turnout` group reported voting at a *greater* rate than the other groups, though these differences are small in magnitude ($p_{\text{low national turnout}} = .23$; $p_{\text{high national turnout}} = .61$).

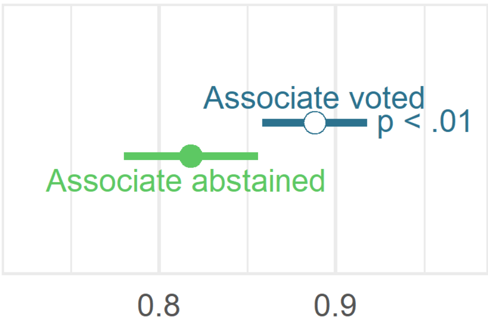
Figure 1C plots the mechanisms through which the network participation prime may effect reported turnout. The two leftmost panels examine information effects, plotting the treatment effect on perceptions of national turnout (left panel) and turnout within their own networks (middle panel). These panels suggest the network participation prime had little effect on these perceptions. Therefore, in this experiment the network's effect on overreporting does not appear to arise by changing respondents' perceptions of participation either nationally or within their own networks. Rather, the apparent mechanism appears to be social pressure, as shown in the far right panel, which plots how disappointed the associate would be to learn that the respondent did not vote. The plot shows that individuals expect non-voting associates to feel little disappointment, averaging only 0.6 on the three-point scale which ranged from 0 = "Not at all disappointed" to 3 = "Very disappointed." In contrast, respondents whose associate voted reported an average of 1.2—just over the midpoint on the scale and double that of respondents whose associate did not vote ($p < .001$).

Figure 1D provides analogous plots for the national turnout cue. Though the national turnout cue had no effect on overreporting, it succeeded in altering respondents' perceptions of national participation levels, as shown in the left panel. Respondents who received no cue believed 60% of eligible voters turned out in 2016, on average, compared to 55% among the `low national turnout` group ($p < .001$). Thus, priming the low turnout in the 2014 midterm elections reduced respondents' estimates of aggregate participation in the 2016 presidential election. Those in the `high national turnout` group estimated a 58% national turnout rate, which was similar to those receiving no cue ($p = .21$) and greater than the `low national turnout` group ($p = .01$). As the last two panels show, this treatment had no apparent effects on respondents' perceptions of turnout within their own networks nor perceived social pressure.

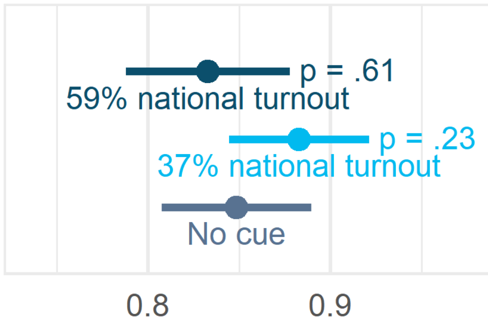
In summary, the experiment suggests that respondents' immediate social networks influence overreports. In contrast, perceptions of societal norms about participation do not. In this case, the network tends to operate through social pressure rather than information. The network treatment had no systematic effect on perceptions of turnout either nationally or within respondents' own networks. Instead, its primary effect was on expected levels of disappointment.

Our results suggest that the apparent effect of societal norms on overreporting (Karp and Brockington 2005) may instead reflect an aggregation of more localized social influence. This conclusion aligns with recent evidence suggesting that people feel pressure to conform to the behavior of their ingroup peers, but not outgroup members (Suhay 2015). And perhaps as a result, the social rewards of voting depend heavily on the local context (Anoll 2018). This conclusion requires further research, however, because the high turnout rate in the sample may imply a ceiling effect, suppressing the effect of the national turnout cue. Nonetheless, turnout reports in the experiment are consistent with typical survey estimates in U.S. presidential elections. Thus, this study examines effects using variation that reflects common real-world conditions. Still, we hope future work explores whether perceptions of national turnout matter more in low-turnout elections.

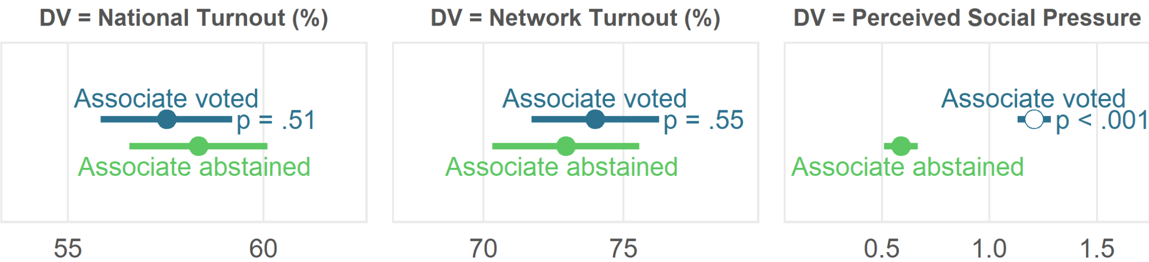
Figure 1: Experimental treatment effects on self-reported turnout (Panels A & B), and perceptions of national turnout, network turnout, and social pressure to vote (Panels C & D).



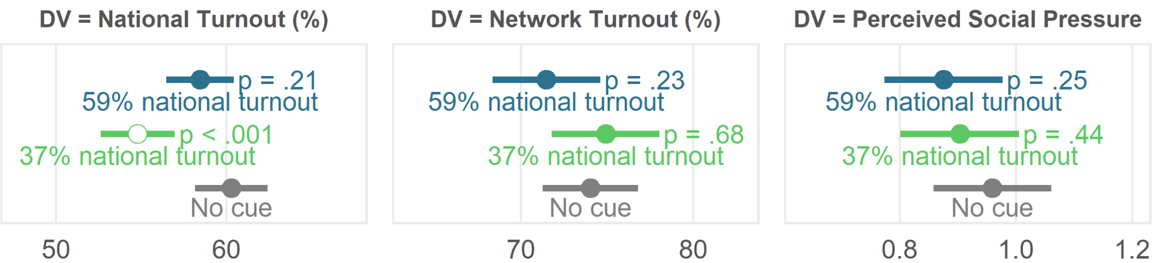
(A) The effect of the network participation prime on self-reported turnout. People are more likely to report having voted if they are reminded of an associate who voted, rather than one who abstained.



(B) The effect of the national turnout cue on self-reported turnout. Knowledge about national turnout rates has little effect on the likelihood that people report having voted.



(C) The effect of the network participation prime on perceptions of national turnout, network turnout, and social pressure to vote. The network treatment increases a respondent's perceived social pressure.



(D) The effect of the national turnout cue on perceptions of national turnout, network turnout, and social pressure to vote. The national treatment affects a respondent's expectation about national turnout.

Note: The plots show means and 95% confidence intervals for each experimental group. The outcome in panels A and B is self-reported turnout. The outcomes in panels C and D reflect a respondent's belief about the percentage of eligible voters who cast votes, the percentage of their discussion network who cast votes, and the level of disappointment their associate would feel if they learned that the respondent did not vote. To the right of each estimate, differences-in-means p-values compare the focal estimate to the reference category, which is the `associate abstained` group for the network turnout prime and the `no cue` group for the national turnout prime.

Conclusion

The results provide evidence demonstrating how the social network can magnify or reduce the social desirability bias of turnout self reports. The experiment suggests that individuals feel greater social pressure to vote and, subsequently, overreport turnout at a greater rate when they are asked to think about acquaintances who themselves voted (Figure 1A). These results indicate that individuals with more participatory networks should be more prone toward misreporting because they will tend to perceive greater social pressure (Figure 1B). Using observational data from three different elections, we find support for that expectation. Individuals with more participatory networks are more likely to falsely report voting (Table 2). This relationship creates the potential to bias estimates of social influence in voting. Researchers using self-reported turnout would find evidence of a substantively large, and statistically significant relationship between network participation and individual-level turnout. Researchers using validated turnout would find such evidence only in one of our three datasets (Table 3).

The social desirability of turnout arising in participatory networks presents a great confound for studies of social influence relying on self reports. For most attitudinal outcomes and many behavioral outcomes, validated measures remain unavailable and thus the bias of the estimates cannot be checked. If analyses using self-reported measures always overestimated the magnitude of social influence relative to analyses using estimates from validated measures, researchers could correct for this bias by adjusting the self-report estimates toward zero. Unfortunately the evidence presented above suggests that such a procedure may instead lead to *underestimates* of social influence in some cases. This correction would be warranted in the South Bend and CCES studies, but in Williamsburg, it would underestimate the apparent social influence observed in the validated voting model. When validation is infeasible, we therefore recommend that scholars subject their estimates to sensitivity analysis to help readers understand the range of estimates they would recover after correcting for plausible levels of bias (see e.g., VanderWeele 2011).

While the analyses reported above focus on turnout, our results suggest that similar problems will arise when researchers study other outcomes that may be biased by social desirability. For instance, many scholars are interested in understanding how individuals' social networks influence their partisan identification and candidate preferences. But for each of these outcomes, social desirability has powerful effects on reported values (Klar and Krupnikov 2016). Even studies that are not interested in social influence must address this problem. Since these outcomes can rarely be validated, the challenge for scholars will be to remove the bias generated by social desirability. As Table 2 suggests, individual covariates serve as insufficient proxies for the social network. Therefore, removing the bias associated with social desirability will be easier if researchers can directly measure and control for the network.

This paper adds to the growing literature exploring the confounds to causal inference that emerge in observational analyses of social influence. Such analyses often cannot distinguish cases of social influence from cases where social interaction arises because of shared interests. Nor can they disentangle social influence from the various contextual pressures that may produce similar attitudes or behaviors within the network, such as mobilization drives or media coverage. In light of these many challenges, can observational data provide *any* insight into social influence? We believe they can, particularly as a compliment to experimental designs, which overcome these confounds, but often lack external validity. To eliminate threats to internal validity, many experimental studies of social influence occur in artificial laboratory settings (Ahn, Huckfeldt and Ryan 2014; Carlson and Settle 2016; Pietryka 2016; Ryan 2011) or focus on online interaction with newly generated social ties (Klar and Shmargad 2017; Carlson 2018). Abstract experimental environments provide well-identified causal estimates, but these precise treatment effects often lack a specific real-world analogue. In light of these many challenges to both internal and external validity, some scholars may be tempted to abandon the study of social influence altogether. Rather than run from these challenges, we believe scholars should explore their theories with multiple methods, leveraging the advantages of each. At the same time, reviewers must recognize that no single dataset, regression, or experimental design can address all of these challenges at once. Evaluating theories of social influence requires both experimental work demonstrating a clear causal mechanism and observational work demonstrating a substantively significant real-world relationship.

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