**Emotion, Efficacy, and Protest Intentions:
Testing a Multilevel Dual Pathway Model of Collective Action**

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Abstract

What are the causes of anger and efficacy, and their consequences for protest intentions? Here we propose a multilevel dual pathway model of collective action where anger and efficacy operate at multiple levels of analysis. To test our model, we administer a factorial survey experiment of student protest to a disproportionate stratified random sample of undergraduate students ($N = 880$). We find that the indirect effect of anger on protest intentions follows two routes—one dispositional and one situational—while the indirect effect of efficacy flows through a situational channel. We also find that the dual pathways of anger and efficacy are triggered by a broad set of situational conditions (incidental grievances, selective rewards and punishments, collective action frames, and size of the protest), while anger is also a function of a narrow set of dispositional factors (protest norms and attitudes). Our results imply that understanding the multilevel nature of anger and efficacy can help social movement organizations better coordinate collective action.

Introduction

In the spring of 2020, millions of Americans took to the streets as part of a passionate movement protesting police brutality and racial injustice. One strand of contemporary social movement theory might see the movement as growing out of citizens being motivated to exploit the political opportunity to make a difference in the context of the COVID crisis, a divided polity, and a discredited president (McAdam 1983; McAdam et al. 1996; Meyer 2004). From this point of view, mobilization ultimately springs out of people's perception of collective efficacy. A second line of thought might see the movement as a response to mounting anger and frustration as institutional and economic breakdown occurs during a period of deepening crisis (Goodwin et al. 2000, 2001; Jasper 1998, 2011, 2018; Useem 1998). From this point of view, mobilization might grow out of popular indignation and outrage in response to grievances. Indeed, for much of the history of the study collective behavior, competing theories have stressed either the rationality of protest or its emotionality to the exclusion of the other (Berkowitz 1972; Gurr 1970; Jasper 2018; McCarthy and Zald 1977; Moore 1966; Oliver et al. 1985; Olson 1965; Opp 2009; Smelser 1962). This begs the question: Can these perspectives be reconciled?

Over the past twenty years, social psychologists have been developing a family of dual pathway models of collective action that treat efficacy *and* emotion as complementary routes to mobilization (Stürmer and Simon 2009; Tausch et al. 2011; Tausch and Becker 2013; van Zomeren et al. 2004; van Zomeren et al. 2008; van Zomeren et al. 2010; van Zomeren et al. 2012). The central insight of these models is that actors are “passionate economists” (van Zomeren et al. 2012) who “think through emotions” (Jasper 2018: 16); purposive actors simultaneously driven by feelings *and* instrumental interests (van Zomeren et al. 2004; van Zomeren et al. 2008; van Zomeren et al. 2010; van Zomeren et al. 2012). Actors, in these

models, respond to incentives and calculate advantages, but are also motivated by feelings that influence the intensity of grievances and protest intentions. Explicitly synthetic, dual pathway models of collective action integrate key insights from relative deprivation theory (Walker and Smith 2002), resource mobilization theory (McCarthy and Zald 1977), and social identity theory (Tajfel and Turner 1979) by proposing that neither grievances nor potential benefits are sufficient triggers of protest intentions. Besides the cognitive dimensions of motivation, group members must also identify with the group and feel that they are unfairly suffering as part of it. Instrumental considerations and emotions simultaneously influence decision-making, especially when the experience of group-based deprivation increases the salience of social identity. This understanding of collective actors has several advantages, including its consistency with a great body of cognitive research, its ability to integrate subjective utility models with social identity models, and its ability to make predictions about the situations and processes that incline actors toward protest.

Despite the appeal of dual pathway approaches and the body of empirical evidence in support of them, theoretical and empirical questions remain when applied as an explanatory model of collective action. First, the dual pathway model outlined by van Zomeren and colleagues underscores a limited number of causes. While parsimonious, we contend that anger, efficacy, and protest intentions have foundations beyond collective disadvantage and social identity. In addition to these key inputs, political and sociological theories of protest and collective action identify a number of additional conditions sufficient for the growth and decay of collective action: material incentives (Oliver 1980; Medina 2007; Opp 2009), social incentives (Chong 1991; Muller and Opp 1986; Opp and Gern 1993; Willer 2009), production functions and thresholds (Granovetter 1978; Oliver et al. 1985), collective action frames (Gamson 1992;

Snow et al. 1986), and individual predispositions and preferences (Fehr and Gintis 2007; Henrich et al. 2001; Oliver et al. 1985; Mondak and Halperin 2008; Tilman et al. 2018). With these additional initial conditions in mind, our goal is to integrate dual pathway models with the sociological research on collective action so as to arrive at a fuller account of anger, efficacy, and protest intentions.

Second, we use a factorial survey experiment of student protest coupled with a multilevel structural equation model to decompose between-individual effects from within-individual effects. In our design, respondents evaluate twelve vignettes in which characteristics of a hypothetical protest situation are manipulated and randomly assigned from vignette-to-vignette. By mapping the consistency of responses across different protest situations, we can assess the degree to which protest intentions and the dual pathways of anger and efficacy are a function of the person and/or the situation. This grants us the freedom to see whether individuals anchor on mean levels of anger, efficacy, and protest intentions, and to identify how much variation there is around these individual-specific means depending on the situation. With our within-subjects factorial survey experiment, we also address some of the endogeneity bias inherent to mediation analysis (Morgan and Winship 2007). We can effectively bolster causal identification of the indirect effects at the within-individual level by statistically controlling for confounders at the between-individual level.

We examine our theoretical and empirical questions by administering a factorial survey experiment to a disproportionate stratified random sample of 880 undergraduate students at a large, public university in the Pacific Northwest. Our research design yields four principal discoveries. First, we find that the dual pathways of anger and efficacy operate at multiple levels of analysis. The effect of anger on protest intentions follows two routes—one dispositional and

one situational—while the effect of efficacy flows through a situational channel. This implies that specific changes to the situation activate anger *and* efficacy, which in turn affect protest intentions. And, unlike efficacy, some individuals are prone to anger, and this dispositional anger (or mood) is associated with propensities for protest. Second, we discover mixed support for the initial conditions of social identity and collective disadvantage. Social identity plays a negligible role in driving anger, efficacy, and protest intentions, while collective disadvantage accounts for all three. Third, we observe strong support for initial conditions not commonly espoused by the dual pathway literature, such as selective rewards and punishments, collective action frames, and size of the protest. Interestingly, each of the initial conditions function differently: sometimes they operate via both pathways, sometimes they operate via one, and sometimes they exhibit countervailing effects. Fourth, our identification strategy allows us to claim—with some confidence—that the indirect effects of anger and efficacy at the within-individual level are weakly biased.

Taken together, we find strong support for the dual pathways of anger and efficacy, but we complicate when and how these pathways operate, and we expand and broaden the initial conditions of these pathways.

The Dual Pathway Model of Collective Action

Classic theoretical accounts of collective action often treated emotions such as frustration, disgust, and contempt as motives sufficient for the development of political protest (Berkowitz 1972; Gurr 1970; Moore 1966; Smelser 1962). In the wake of Olson's (1965) seminal *Logic of Collective Action*, however, the study of protest took a distinctively rationalist turn by explicitly discounting emotions as causes. Scholarship in the rational choice, political opportunities, and resource mobilization traditions that followed, underscored instrumental

motivations, material incentives, and the deliberate exploitation of political opportunities to account for protest (Diani 1997; Diani and McAdam 2003; Jenkins 1983; Klandermans 1984; Klandermans and Oegema 1987; McAdam et al. 1996; McAdam et al. 2001; Tilly 1978).

Although dominated by a narrowly rationalist paradigm for decades, more recently there has been a renaissance of thinking in sociology (Goodwin et al. 2000, 2001; Kemper 2001; DeCelles et al. 2019; Jasper 1998, 2013, 2018; Saab et al. 2016) and social psychology (Klandermans 1997; Leach et al. 2006; Livingstone et al. 2011; Tausch et al. 2011; van Stekelenburg and Klandermans 2013; van Zomeren et al. 2004) about the affective microfoundations of collective action. Perhaps the clearest statements concerning the role of emotions can be found in dual pathway models of collective action (Shi et al. 2015; Simon et al. 1998; Stürmer and Simon 2009; Tausch et al. 2011; Tausch and Becker 2013; van Zomeren et al. 2008; van Zomeren et al. 2010; van Zomeren et al. 2012; van Zomeren et al. 2004). Early manifestations of these models distinguished between an instrumental pathway, guided by deliberate, calculative reasoning of the costs and benefits to participation, and an identity pathway, guided by processes of identification and self-categorization (Simon et al. 1998). Although empirical findings supported the idea that interests and identification uniquely predicted protest intentions (Simon et al. 1998), alternative dual pathway models followed.

Van Zomeren et al. (2004) proposed a dual pathway model that consisted of an efficacy pathway and an emotion pathway, while Stürmer and Simon (2009)—building from the seminal work of Simon et al. (1998)—explored group-based anger as an additional path. An impressive body of empirical research testing dual pathway models came on the heels of these theories (Leach et al. 2006; Shi et al. 2015; Tausch et al. 2011; Tausch and Becker 2013; van Stekelenburg et al. 2011; van Zomeren et al. 2008; van Zomeren et al. 2010). At the same time,

other researchers offered theoretical statements modifying, integrating, and adding initial conditions and paths to the model such as grievances, ideology, and social embeddedness (Klandermans et al. 2008; van Stekelenburg 2013; van Stekelenburg and Klandermans 2013; van Stekelenburg et al. 2011).

The dual pathway model originally espoused by van Zomeren et al. (2004) conceptualized group-based anger (pathway 1) and group efficacy (pathway 2) as different forms of managing disadvantage (Lazarus 1991). According to this model, protest intentions result from *problem-focused* and *emotion-focused* attempts to cope with *group-based* grievances in the form of procedural unfairness and injustice (Shi et al. 2015; Tausch et al. 2011; van Zomeren et al. 2008; van Zomeren et al. 2010; van Zomeren et al. 2012; van Zomeren et al. 2004). In the first pathway, actors appraise their disadvantage and determine whether this disadvantage is fair, legitimate, and just. If the disadvantage is appraised as unfair and unjust—and if this disadvantage resides at the group-level (i.e., perceived as collective disadvantage)—then actors experience group-based anger which motivates collective action. The second pathway, by contrast, emphasizes appraisals of a group's efficacy to solve common problems. This pathway focuses on how actors take part in collective action when they believe that their group has sufficient resources to succeed in mobilization efforts (Bandura 2000; Finkel and Muller 1998; Finkel et al. 1989; Klandermans 1984, 1997; Opp 2009), which—to some degree—is a function of collective disadvantage.

Drawing on relative deprivation theory and social identity theory, van Zomeren's (2004) dual pathway model assumes that anger and efficacy are "group-based". That is, groups must be disadvantaged for collective action to emerge, and group members must perceive their disadvantage as collective *and* view their identity in collective terms. With respect to group-

based anger, "...collective disadvantage makes social identity salient, resulting in group-based appraisal and emotion. If the event is appraised as unfair, then group-based anger should be likely (van Zomeren et al. 2004: 650)", thereby increasing the odds of collective action.

Likewise, van Zomeren and colleagues readily admit that social identity approaches downplay instrumental explanations, but nonetheless go on to argue that group efficacy—to some degree—is a function of perceived collective disadvantage and social identity (van Zomeren et al. 2004).

It is important to note that the dual pathway model, regardless of the path, assumes purposive action (van Zomeren et al. 2012). It also does not assume that emotion-focused action is necessarily spontaneous and unreflective, or that problem-focused action is necessarily deliberative and calculative. Rather, the central insight of the dual pathway model is that both emotion *and* reason are fundamentally cognitive processes.

The Present Research

The present research extends previous work in a number of ways, both theoretically and empirically. First, building from Bert Klanderman's and Jacqueliën van Stekelenburg's work (Klandermans et al. 2008; van Stekelenburg 2013; van Stekelenburg and Klandermans 2013; van Stekelenburg et al. 2011), we integrate a broader set of sociologically informed predictors to van Zomeren and colleagues' dual pathway model of collective action. Second, we focus exclusively on anger as the key emotion motivating collective action (Leach et al. 2006; Livingstone et al. 2011; Tausch et al. 2011; Tausch and Becker 2013), and move away from group-based conceptions of anger. Third, we use a random sample of undergraduate students to minimize total survey error, and we employ a within-subjects experimental design to reduce endogeneity bias of the anger and efficacy pathways. We briefly review each of the proposed extensions below.

Dual pathway model of collective action: A multilevel approach

In the present research, we keep with van Zomeren et al.'s (2004) original dual pathway model, but explore a broader set of predictors. Most dual pathway models focus on identity, procedural fairness, grievances, and/or injustice as initial conditions of protest intentions (Leach et al. 2006; Shi et al. 2015; Tausch et al. 2011; Tausch and Becker 2013; van Stekelenburg et al. 2011; van Stekelenburg 2013; van Stekelenburg et al. 2011; van Zomeren et al. 2008; van Zomeren et al. 2010). Yet, the social movement and collective action traditions in political science and sociology identify a number of additional sources of protest intentions that may operate via the dual pathways of anger and efficacy (e.g., Klandermans et al. 2008; Stürmer and Simon 2004; van Stekelenburg and Klandermans 2013). To frame our discussion, we adopt a typology commonly used in social psychology, namely the distinction between person-centered and situation-centered explanations of intentions and behavior (Fleeson 2001).

The person-situation debate refers to the controversy over whether the effect of an individual's characteristics—such as their personality—affect intentions and behavior consistently across situations, or whether intentions and behavior are best understood by changes to the situation. Person-centered explanations focus on the stable personality traits, acquired dispositions, preferences, and even biographical characteristics that people possess. Applied to collective action, research shows that dispositions and characteristics of individuals do in fact spur mobilization and protest, leading Snow and Oliver (1995) to conclude that “there is something to be gained by reconsidering ‘personality’ (p. 578).”

Furthermore, previous mobilization efforts and participation in political protests may spur future protest via changes to beliefs and attitudes (Opp 2009). Participation in protests may also be more likely when individuals hold favorable attitudes toward collective action (e.g., protests

as a justifiable tool for fostering change), harbor unfavorable attitudes (or grievances) toward the target of a protest, and/or have internalized protest norms (Jasso and Opp 1997; Marwell and Oliver 1993; Oliver et al. 1985; Opp et al. 1996; Opp 2009). Finally, collective action may be more likely among individuals who are other-regarding (versus self-regarding) and less likely among individuals who are risk averse (Lichbach 1995; Fehr and Gintis 2007; Henrich et al. 2001; Tilman et al. 2018). In sum, the literature on person-centered explanations of collective action is rich and full of insights, and can be categorized as follows: (1) self and social identities, (2) prior social and political action, (3) structural grievances and protest attitudes, and (4) preferences and beliefs.

Situation-centered explanations, in contrast, underscore how features of context and circumstance—rather than personality traits or acquired dispositions—influence intentions and behavior. As Medina (2013) argues, explanations of collective action are “no longer grappling to explain high levels of participation; their challenge is rather to discern how such levels respond to changes in the environment where they take place (p. 280).” Most theoretical and empirical work in the social movement and collective action traditions, as a consequence, is situated within this framework. For instance, material incentives (Oliver 1980; Olson 1965), social incentives (Opp 2009; Willer 2009), collective action frames (Gamson 1992; Snow et al. 1986), and production functions (Oliver et al. 1985) are capable of motivating bystanders to participate in a protest, where the goal of the protest is to produce a collective good (e.g., spur policy changes) or to avoid a public bad (e.g., avert tuition increases). Each of these factors derives their motivating power from relational and structural foundations, all of which are situation-centered accounts of collective action.

In sum, we draw on the extensive social movement and collective behavior literature to explore whether and to what extent classic person-centered *and* situation-centered explanations of collective action influence protest intentions and are mediated by the dual paths of anger and efficacy. Those paths mediated in part (or not at all) by anger and efficacy provide us with an opportunity to theorize alternative pathways, and assess the degree to which initial conditions that theoretically affect collective action are a function of anger and efficacy. Below, we detail the paths of anger and efficacy, and offer a number of expectations and hypotheses.

A reexamination of anger and efficacy

Unlike van Zomeren and colleagues, we contend that anger and efficacy *can* motivate collective efforts in the *absence* of perceived collective disadvantage and identification with a group. Following Klandermans and van Stekelenburg (Klandermans et al. 2008; van Stekelenburg and Klandermans 2013; van Stekelenburg et al. 2011), we treat collective disadvantage and social identity as sufficient (but not necessary) conditions for anger and efficacy. For us, anger and efficacy have roots beyond relative deprivation and social identity.

Anger. Emotions cover numerous types of automatic responses, appraisals, feelings, and evaluations, including urges, reflex emotions, moods, affective commitments, and moral emotions (Goodwin et al. 2000; Jasper 2011, 2018). “Most emotions are cultural labels that correspond imperfectly to the many feeling-thinking processes going on, such as adrenaline and other chemicals, blood flow and pulse rates,..., and dozens or even hundreds of other small changes in our bodies (Jasper 2018: 6).” As such, different emotions have different implications for collective protest. Not all emotions cause intentions or behavior. Some reflex emotions, such as fear, may lead to resignation and withdrawal (Holmes 2004; Kemper 2001; Scheff 1990; Turner and Stets 2005), whereas other reflex emotions like anger uniquely translate grievances

into action. As Jasper (2013) notes: “Anger and indignation, the morally grounded form of anger, are crucial to many aspects of protest. They not only motivate participation but they direct blame for social problems, create sympathy and admiration for protesters, and guide strategic choices (p. 208).” Furthermore, anger is the “prototypical protest emotion” by which we assert our rights and social status, such as through righteous indignation at injuries or insults (Jasper 2011: 296-7). Such feelings produce “states of action readiness”, or as van Zomeren et al. (2008) argue, “...emotions like anger...motivate collective action because they invoke specific action tendencies to confront those responsible in order to redress their unfair deprivation (p. 506).”

We argue that protest flows, in part, incidentally, that is, from the feeling that one’s self or group has been unjustly injured by the disregard of the authorities or by deliberate actions against it (Pfaff and Hechter 2020; Hechter et al. 2016). According to Jasper (2011), anger and indignation directed toward authorities “...can be especially moving, as it involves a sense of betrayal (p. 292).” Confronted with incidental grievances, people experience injuries to their personal interests *and* the social standing of their group. Individually experienced anger can generate the collective indignation that motivates protest (Bar-Tal et al. 2007); and anger should be especially potent when members of an affected in-group perceive their deprivation as arising from actions that reflect the contempt of a powerful outgroup (Mackie et al. 2000; Goldenberg et al. 2016). In this way, anger can motivate action in the presence or absence of group identification, and thereby account for interest-based collective action (e.g., student protests against tuition increases) as well as identity-oriented social movements (e.g., Black Lives Matter, Me Too, Gay Rights Movement, etc.; Polletta and Jasper 2001).

Collective efficacy. In one of the most widely known micro-level research programs on collective action, Bert Klandermans and colleagues (Klandermans 1984, 1997; Klandermans and

Oegema 1987; Klandermans et al. 2008) integrate value-expectancy theory with resource mobilization theory to show how situations influence aggrieved people to undertake group action. In early models proposed by Klandermans (1984, 1997), protest participation arises from individuals who—because of grievances—are sympathetic to the goals of a movement, have been targeted for mobilization, and have become motivated to participate. Individuals participate in protest given the necessary resources, opportunities, and numbers for success. These elements inform perceived *collective efficacy*, which is not merely the personal efficacy of group members but rather an emergent feature of their situational interdependence: “Being socially situated, and often interdependently so, individuals’ judgements of their personal efficacy are not detached from other members’ enabling or impeding activities (Bandura 2000: 76).” A large body of research shows that “...the higher the perceived collective efficacy, the higher the groups’ motivational investment in their undertakings” and that “People who believe they can achieve desired changes through their collective voice, and who view their governmental systems as trustworthy, are active participants in conventional political activities (Bandura 2000: 78).”

Drawing directly from Klandermans and Bandura, van Zomeren and colleagues conceptualize collective efficacy (or what they call group efficacy) as “...one’s collective belief group-related problems can be solved by collective efforts (van Zomeren et al. 2004: 651).” We view group efficacy in a similar regard as van Zomeren, but place less emphasis on coping with disadvantage as an initial condition of collective efficacy. In relation to collective action and protest, we define group (or collective) efficacy as an individual’s subjective probability that a common good will be provided by other members of the group if the individual does not contribute to the production of the common good (Klandermans 1984, 1997; Opp 2009). In this way, group efficacy has the power to motivate protest intentions and collective action, and arise

as a function of (a) identities (van Stekelenburg 2013; van Stekelenburg and Klandermans 2013; van Stekelenburg et al. 2011; van Zomeren et al. 2004), (b) opportunities, resources and expected number of participants (Klandermans 1984), and (c) previous experience and success with protest (Opp 2009).

Hypotheses and expectations. In the current study, we systematically test the indirect effects of perceived collective efficacy and anger on protest intentions. Given the discussion above of dual pathway models, we anticipate that collective efficacy and anger constitute separate but complimentary pathways to protest intentions.

First, we expect that instrumentally motivated actors respond to incentives (H1). More specifically, individuals (H1a) consider the *material value of prospective public goods* when they decide to participate in collective action, (H1b) *perceive the costs and benefits of collective action* not in absolute terms but in reference to their own interests and resources, and (H1c) possess *stable attitudes, preferences, and beliefs* that reflect their individual dispositions toward collective action.

Second, we argue that the link between incentives and intentions are partly mediated by emotionally driven actors who respond to feelings of anger (H2) based on: (H2a) the *intensity of shared grievances*; (H2b) the *perceptions of threat toward the group* for which they are a member; (H2c) the *presence of collective action frames that define and highlight collective disadvantage*; and (H2d) *identification with the collective action group*.

Third, we argue that incentives and intentions are also partly mediated by perceptions of collective efficacy to the extent that decision-making is interdependent (H3). More specifically, actors alter perceived collective efficacy based on *the contributions of others* (H3a), *the perceived costs and benefits of collective action to others* (H3b), *the presence of collective action*

frames that provide solutions to collective disadvantage (H3c), and the strength of identification with the collective action group (H3d). Figure 1 provides an illustration of the integrated dual pathway model we intend to test.

[Figure 1 about here]

Causal identification of the dual pathways

Empirically, we see two outstanding problems facing the literature testing dual pathway models of collective action. The first empirical problem is one of *representational survey error* (Groves et al. 2009; Weisberg 2005). This form of error occurs when estimated statistics depart from the unobserved population parameter. While this form of survey error undermines external validity and the generalizability of empirical findings, it also unduly affects the “representation” of statistical estimates (Groves et al. 2009).

In the literature on dual pathway models of collective action (broadly defined), issues of representational survey error are ubiquitous. While some studies in this literature sample adults from the general population in different cultural contexts (Klandermans et al. 2008; Leach et al. 2006; Simon et al. 1998; Tausch et al. 2011; van Stekelenburg et al. 2011), almost all are non-probability samples, most of which consist of undergraduate students in China (Shi et al. 2015), Germany (Stürmer and Simon 2009; Tausch et al. 2011; Tausch and Becker), India (Tausch et al. 2011), Netherlands (van Zomeren et al. 2004; van Zomeren et al. 2010), United States (Miller et al. 2009), and the United Kingdom (Livingstone et al. 2011). Moreover, the identification of target populations, units in the sampling frame, and distinctions between samples and respondent pools are rarely discussed (see Leach et al. 2006; Stürmer and Simon 2004; van Stekelenburg et al. 2011 for exceptions), which generates uncertainty about the severity of coverage error and non-response error.

To address these longstanding issues of representational survey error in the dual pathway literature, we focus on a target population of undergraduate students enrolled at a large, public university in the pacific northwest of the United States. We also employ a clearly defined sampling frame, use probability sampling techniques, and identify minimal amounts of non-response error. Finally, we follow a common design practice in the dual pathway literature and focus on a specific type of collective disadvantage salient to all undergraduate students: tuition increases (Tausch et al. 2011; van Zomeren et al. 2004).

The second empirical problem, beyond representational survey error, is the *causal identification of indirect effects*. Most of the research testing dual pathway models of collective action rely on cross-sectional survey data or laboratory experiments, treat anger and efficacy as endogenous variables, and estimate indirect effects using classic methods of testing for mediation (Miller et al. 2009; Simon et al. 1998; van Zomeren et al. 2010) or structural equation path models (Klandermans et al. 2008; Leach et al. 2006; Shi et al. 2015; Tausch et al. 2011; Tausch and Becker 2013; van Stekelenburg et al. 2011; van Zomeren et al. 2004).

In the present study, we use a factorial survey experiment of student protest coupled with a two-level structural equation model to decompose between-individual effects (i.e., person) from within-individual effects (i.e., situation). In our design, respondents evaluate twelve vignettes where features of a hypothetical protest situation are manipulated and randomly assigned from vignette-to-vignette. This within-subjects design allows us to address some—but not all—of the omitted variable bias inherent to mediation analysis (Bullock et al. 2010; Imai et al. 2010). At the within-individual level, we effectively reduce bias of the indirect effects by statistically controlling for vignette invariant confounders via differencing (i.e., we control for between-individual unobserved heterogeneity such as dispositions and preferences). We also

follow recent studies and manipulate anger (Livingstone et al. 2011) and efficacy (van Zomeren et al. 2010) directly in our experimental design (see Shi et al. 2015), a strategy that parallels the classic “encouragement design” (Holland 1988). By using a within-subjects design and manipulating anger and efficacy independently and simultaneously, we provide a rich test of—and direct evidence for—the dual pathway model of collective action.

Data and Methods

Factorial Survey Experiment

To test our dual pathway model of collective action, we use a factorial survey experiment (FSE) of student protest (Auspurg and Hinz 2015). Our FSE approach presents respondents with hypothetical protest scenarios in narrative form. Embedded in the narratives are vignette *dimensions* and *levels*. Vignette dimensions characterize different aspects of the social context, such as grievances and the number of participants, that affect protest intentions. Vignette levels are the values of the vignette dimensions (e.g., 3 protestors versus 3000 protestors) used to elicit judgments, decisions, and/or behavioral intentions from respondents. We thus experimentally manipulate different aspects of the conditions in which protests occur (*dimensions*) by randomly assigning attribute values of those conditions (*levels*) to protest scenarios; conditions that external organizations, such as university administrations and student political groups, manipulate in the real world. In terms of estimation, vignette dimensions are the independent variables in our statistical models that vary according to the values of the vignette levels. For each vignette, subjects were asked whether they are angry toward the target of the protest, whether they believe the depicted protest will succeed, and whether they are willing to participate in the protest. Anger toward administration, likelihood of success, and intention to

protest are dependent variables in our statistical models, and correspond exactly to the outcomes specified by our dual pathway model of collective action.

Each respondent assessed twelve vignettes. We randomly assigned values of vignette dimensions to vignettes, which ensures that vignette dimensions and levels are uncorrelated. We also randomly assigned vignettes to respondents, which ensures that vignette dimensions are uncorrelated with respondent characteristics. The result is a pooled person-vignette data set generated from two sampling processes: a random sample of undergraduates drawn from a target population of students, and a random sample of vignettes drawn from the vignette population or factorial object universe (i.e., all possible combinations of dimensions and levels). Our statistical models disentangle person-specific effects (person-centered, between-individual) from vignette-specific effects (situation-centered, within-individual), and test for multilevel mediation of the dual pathways at the between- *and* within-individual levels.

While our static research design grants us the ability to deeply probe the dynamics of student protest—an important type of collective action in the history of social movements—it nonetheless limits the scope of our findings to local populations addressing incidental grievances with peaceful methods of mobilization. This is a particularly important caveat given that anger and efficacy undermine some kinds of non-normative mobilization efforts (see Stürmer and Simon 2009; Tausch et al. 2011). Our results may have more to say about organized labor protests and demonstrations than nationwide political campaigns or spontaneous riots. That being said, all experimental research must focus on a particular type of collective action, where the primary goal is to test theory and the secondary goal is to triangulate the findings in relation to prior research.

Population and Sample of Respondents

During the autumn of 2014, we administered a web-based version of our FSE to a sample of undergraduate students enrolled at a large public research university in the Pacific Northwest. This university is the state's flagship and has a relatively diverse student body. Moreover, recent tuition increases have spurred discontent among students, who are at risk of protest. In response to an announced tuition increase of 14 percent, hundreds of students at this university staged a protest on campus in March of 2010 to coincide with a "National Day of Action" organized by a coalition of American student groups. The following year, another tuition increase of 20 percent elicited a protest involving only a few dozen students. Clearly, the issue of tuition is salient for students at this university but there has been temporal variation in the magnitude of protests against tuition increases.

Using the registrar's list of students and email addresses as a sampling frame (20,241 elements), we drew a disproportionate stratified random sample of freshmen, sophomores, and juniors ($N = 3000$). We excluded seniors from the sampling frame as they are immune to the collective good (or grievance), which was a proposed tuition increase taking effect the following academic year. Our respondent pool of 880 students reflects a CASRO response rate of 31.67 percent and an AAPOR cooperation rate of 64.99 percent. We observed negligible amounts of coverage error, sampling error, and unit non-response error; further discussion of each type of error can be found in the Supplemental Materials online. The median study length was approximately 29.41 minutes.

Vignette Dimensions and Levels

A crucial task for designing FSEs is to create dimensions and levels that not only capture key theoretical concepts such as selective incentives and collective action frames, but to also craft vignettes that reflect theoretically plausible protest scenarios. In creating the vignette

characteristics, we drew upon (a) the sociological literature on student protest, collective action, and rational choice, (b) the social psychological literature on dual pathway models of collective action, and (c) findings from focus groups and a pilot study (see the Supplemental Materials online). In our scenarios, we assume two exogenously given actors—a university administration and a student organization—which set incentive levels and provide information. The dimensions and levels described below constitute our situation-centered, or within-individual level, variables that generate vignette-specific effects. All vignette dimensions and levels can be found in the Appendix (see Table A1).

Collective good. The first dimension we manipulate is the magnitude of the *incidental grievance* (or collective good) imposed on students. In our vignettes, the grievance is the size of the *tuition increase* threatened by the university administration. We distinguish between four levels of the grievance—1, 10, 50, and 100 percent tuition increase—that are plausible and in the range of real-life tuition increases. Quebec and Puerto Rico, for instance, have proposed 75 percent and 100 percent tuition increases in the past, respectively. To state it differently, preventing a tuition hike determines the value of the collective good: the greater the grievance (i.e., tuition increase), the greater the value of the collective good if the tuition hike is thwarted. In the dual pathway literature, it is common to design experiments around tuition increases (Shi et al. 2015; Stürmer and Simon 2009; Tausch and Becker 2013; van Zomeren et al. 2004). Our study, however, is the first to manipulate the magnitude of a tuition hike—not merely the presence or absence of a tuition increase—and explore its effects on anger, efficacy, and protest intentions.

Selective incentives. We include two dimensions of selective incentives, or tangible punishments and rewards given for contributing to, or refraining from, collective action. The first

dimension, *selective rewards*, operationalizes benefits awarded by a student organization for participating in the protest. Following Olson (1965), we include four levels of up-front monetary rewards to participants, including a blank level (i.e., respondents shown nothing), \$10, \$50, and \$200. Here, \$10 is a plausible return from a sponsoring organization, such as a labor union. We include \$50 and \$200 to test counterfactual conditions of strong incentives. Our second dimension captures the university administration's attempt to increase the costs of protesting, which historically has ranged from minor informal sanctions (e.g., reprimands) to serious academic sanctions (e.g., expulsion) to legal sanctions (arrest) (McCormick 1990). We focus on a salient cost to students easily imposed by administrators—punitive grading and expulsion. This dimension, *selective punishments*, ranges from no penalty to incomplete grades to failing grades to expulsion from the university. In our operationalization, selective rewards and punishments are available to, and imposed upon, all participants regardless of group size, making them structurally similar (Oliver 1980).

Social incentives. We employ two dimensions of social incentives, or intangible punishments and rewards given for contributing to, or refraining from, collective action (such as the status, respect, and reputation among peers). The first, *social inducement*, operationalizes the social influence of peers who try to recruit the respondent, including a blank level and a level that consists of “some friends” who urge the respondent to participate in the protest (Opp and Gern 1993). The second dimension, *reputation*, captures respect and admiration from significant others for participating (Chong 1991; Willer 2009). This consists of two levels: a blank level and a level in which friends and family will respect anyone who participates in the protest. For both dimensions, we assume (1) that respondents can imagine hypothetical recruitment scenarios

(which might depart from their actual expectations), and (2) that respondents can overcome the cost of others' disappointment if they refrain from participating.

Collective action frames. Organizers can frame grievances in a number of ways. Political actors often act as signifying agents, who actively “produce, maintain, and fight for meanings” they find important and “struggle to have certain meanings and understandings gain ascendance over others” (Snow and Oliver 1995: 587). Collective action frames are ways of characterizing a situation that is likely to mobilize others to participate. Members of political organizations often intentionally manipulate frames to maximize the number of participants by aligning collective action frames with the interests of potential constituencies (Snow et al. 1986). Gamson (1992) argued that successful frames are those that define the problem and solution in collective terms, define antagonists as “them” in opposition to “us,” and define an injustice that collective action can correct. Cognitive frames, in other words, are forms of information actors use to make decisions.

We thus embed three dimensions in the protest scenario that could produce effective frames. For each dimension, we assume that subjective frames are rooted in objective reality, and that respondents will subjectively frame the situation in ways consistent with the objective dimension. The first frame, *solidarity frame*, depicts employees of the University (i.e., service workers) who are sympathetic to the cause. This dimension consists of three levels: a blank level, a “declaration of unity” level, and a “declaration of unity founded on moral principles” level. We expect both levels that involve declarations of unity to increase the protest’s expected likelihood of success (i.e., collective efficacy).

The second frame, *efficacy frame*, manipulates two levels concerned with the success of walk-outs aimed at stopping tuition increases at other universities. For the first level, respondents

do not know how effective walk-outs are at preventing tuition hikes at other universities, while, for the second level, respondents do know how effective walk-outs are at preventing tuition hikes. The second level depicts a situation with high collective efficacy that could provide a motivational solution for collective action and thereby increase the expected likelihood of success (Nelson et al. 1997).

The third frame, *injustice frame*, diagnoses the problem and attributes wrongdoing by depicting a violation of fairness norms: “administrators are planning a 10% increase in their salaries.” Although the administrative action does not affect the size of the incidental grievance (tuition increase), the injustice frame depicts contempt for students as a group by increasing administrative salaries while imposing hardships on students. This is precisely the perception of contempt that is prone to cause group-based anger (Tausch et al. 2011: 132-5). Such an injustice should produce anger, and consequently, increase the expected gain (justice) from a successful protest (Gamson 1992).

Number of participants. Dual pathway models of collective action often treat the group to which an individual is a member as static. Most collective action, however, is dynamic. Granovetter’s (1978) threshold model, for instance, posited that individuals, when making the decision to join a protest, would consider the total number of protesters who have joined up to that point. Granovetter showed that if potential protesters have different thresholds for protesting based on the cumulative number of joiners, an individual’s decision depends on the distribution of thresholds in the population at risk. Oliver et al. (1985) extended this work by specifying a theory of the critical mass, exploring the implications of different production functions, using simulation methods, and examining the effects of heterogeneity of resources in populations at risk of collective action.

Building from this seminal sociological research, we explore whether and how protest size affects anger, efficacy, and protest intentions by imposing exogenous variation on the *interdependence of actors*. To do this, we manipulate the cumulative number of protestors who have already joined the hypothetical protest, and randomly assign *without replacement* 12 levels of the size of the protest to each respondent, which ranged in scope from 0 to 22000 students: 0, 1, 3, 7, 20, 55, 150, 400, 1100, 3000, 8100, and 22000.¹ The values we selected for the number of protest participants were based on an exponentiated quasi-log scale. When logged, non-zero values would approximate a vector of integers ranging from 0 to 10. This is an important strategy since we assume that small differences at low numbers (e.g., between 0 and 1) are more important than small differences at large numbers (e.g., 10000 and 10001). The exponentiated values of the quasi-log scale allow us to capture meaningful variation at the low-end of the scale and to observe values toward the high-end of the total target population. By having each respondent evaluate vignettes with all 12 levels of the size of the protest, we can investigate how collective efficacy and protest intentions change as a function of the number of participants.²

The final two vignette dimensions, *success manipulation* and *anger manipulation*, manipulate the core pathway variables of anger and efficacy more directly to examine their causal effects on collective action (Livingstone et al. 2011; Shi et al. 2015; van Zomeren et al. 2010). For *success manipulation*, we use a modified version of van Zomeren et al.'s (2010) manipulation whereby an "expert" stated that collective action would or would not achieve the group's end (see also Shi et al. 2015). In our manipulation, it is a local newspaper predicting that

¹ We randomly assigned without replacement the size of the protest to map out the between- and within-individual variation in production and mobilization functions of collective action..

² A pilot study ($N = 207$) using a different scaling method for the size of the protest (0, 1, 5, 20, 100, 250, 500, 1000, 5000, 10000, 20000, and 25000) yielded similar results observed in the present manuscript (Author Blinded).

the student walk-out has a (1) small or (2) large chance of succeeding (respondents were also shown a blank level). For *anger manipulation*, we manipulate other group members' emotional reactions to incidental grievances (a blank level and a "friends have expressed disdain" level), which is similar to the manipulation used by Livingstone et al. (2011).

These eleven vignette dimensions and their respective levels generate a factorial object universe of 221,184 ($2^5 \times 3^2 \times 4^3 \times 12^1$) unique vignettes.³ We showed respondents twelve vignettes, which yielded a vignette-level sample of 10,560 vignettes and an individual-level sample of 880 respondents.⁴ Note that we randomly assigned *without replacement* levels of the *number of participants* dimension to respondents from vignette-to-vignette, while the levels of all other dimensions were randomly assigned *with replacement* from vignette-to-vignette.

Endogenous Variables

We contacted sampled participants over e-mail and invited them to complete in a web-based survey for a payment of 10 USD. After consenting to participate, respondents were informed of the following hypothetical scenario:

"You are a college student enrolled at this University. The state legislature has just approved a tuition increase that will affect all students and will become effective at the beginning of the next academic quarter. The University administration supports the proposed increase in tuition. In response, a new student group has formed to oppose the approved legislation.

The student group is organizing students to take part in a walk-out during final exams week. The group's goal is to compel the administration and the state legislature to abandon the tuition hike. Students who participate in the walk-out agree not to take any exams or to turn in any papers or other graded work during the grading period. If the walk-out succeeds, no students will pay increased tuition.

³ We provide further justification for our design choices in the Supplemental Materials online.

⁴ Using simple random sampling implies that our factorial object universe is much greater than the overall sample size (our sampling fraction is .047). The resulting sparseness can produce "aliasing", or false negative tests of higher-order interaction effects. Note, however, that "One strength of simple random designs is their low susceptibility to systematic biases as might be caused for fractional factorial designs and for D-efficient designs by their aliasing structure (Dölmer 2016: 340)."

The student group acknowledges that the walk-out will not be easy and wants to mobilize as many students as possible to help meet its goal. You have some concerns about the walk-out because it can only succeed if many students take part, but those who do not participate will enjoy the same benefit as those who do.”

We then told respondents that they would evaluate a series of hypothetical situations based on the background information provided above, and that each situation would end with questions about the student protest. We then showed participants a series of twelve vignettes, with an example vignette as follows (embolden, italicized descriptions of each vignette dimension were not included in the FSE):

“The approved legislation will amount to a 50% increase in the tuition you will pay (***Collective good***). The University threatens that students who participate in the walk-out will receive failing grades in their courses (***Selective punishments***). A national student federation will provide \$50 to every student who participates in the walk-out (***Selective rewards***).

A few days before the walk-out, some of your friends approach you and urge you to participate in the walk-out with them (***Social inducement***). In conversing with your friends and family, the clear consensus among them is that they will respect anyone who takes part in the walk-out (***Reputation***). In the days before the walk out is to begin, the University’s service workers declare that they will unite with the students and join the walk-out (***Solidarity frame***).

Days after the student group formed, Seattle newspapers predicted that the student walk-out had a small chance of succeeding (***Success manipulation***). Your friends expressed disdain toward the University administration for their support of the tuition increase (***Anger manipulation***).

News reports indicate that comparable walk-outs at other American universities have succeeded in preventing tuition increases (***Efficacy frame***). The University’s president and top administrators have announced that they are not going to adjust their salaries for the coming year (***Injustice frame***). In the morning on the first day of the walk-out, you learn that fifty-five students (or 0.24% of 28,000 possible students) have participated in the walk-out (***Number of participants***).”

After each vignette, respondents completed three evaluation tasks—each involving 0 to 100 visual analogue scales (VAS) with a slider bar—that constitute our endogenous variables. First, respondents were asked the following: “Given this set of conditions, to what extent do you feel anger toward the University administration and state legislature?” We treat this variable, *anger toward administration*, as a potential mediator between our initial conditions (at the

between- and within-individual level) and protest intentions. We use a VAS to measure anger for two reasons: (1) to keep the look of the scale consistent with the other two evaluation tasks; and (2) VASs are more valid and reliable measures of feelings, emotional states, moods, and sensations than classic verbal scales and category scales (McCormack et al. 1988). Regardless of how emotions are measured (e.g., VAS versus Likert scale), FSEs run the risk of invoking “feelings rules”, or how one ought to feel in certain circumstances (Holmes 2004), and not necessarily arouse real emotional responses like anger (Collett and Childs 2011). A recent review, however, suggests that there is strong self-report and physiological evidence for the successful induction of emotions, like anger, using imagined hypothetical scenarios (Siedlecka and Denson 2019). We explore this issue in greater detail in the discussion and conclusion.

Second, respondents were then asked “Given this set of conditions, what is the percent chance that the student walk-out will succeed?” Like anger, we treat this variable, *likelihood of success*, as a potential mediator. We employ expectations elicited in probabilistic form for three reasons. First, probabilities represent the level of the collective good provided in many theories of collective action (Francisco 2010; Medina 2007; Oliver et al. 1985). Second, probabilities provide a well-defined numerical scale that allows interpersonal comparisons. Third, probabilities allow respondents to express uncertainty when assessing scenarios that lack full information. We thus use probabilistic scales for both likelihood of success and intention to protest.⁵ The mean probability of expected success averaged across our vignettes and respondents is .36 (see Table 1), which supports the realism of our vignette universe.

⁵ In the broader dual pathway literature, efficacy is commonly measured as a fixed or static entity (e.g., Shi et al. 2015; van Zomeren et al. 2010). Two common survey items used to measure group efficacy include “I think together we are able to change this situation” and “I think that students can really influence these decisions”. We argue that our operationalization is more valid since it captures the probabilistic nature of efficacy (Opp 2009).

[Table 1 about here]

Third, and finally, respondents were asked “Given this set of conditions, what is the percent chance that you would take part in the student walk-out?” This variable, *intention to protest*, captures a respondent’s willingness to join the protest, which is our primary dependent variable. The mean intention to protest averaged across vignettes is .36. Thus, given our vignettes, students report a substantial willingness to protest tuition hikes, which suggests that tuition increases are a salient political issue and there is no universal tendency toward free riding.

Following the literature on dual pathway models of collective action, we are interested in explaining individual differences in protest intentions rather than actual protest behavior. Although some sociologists have criticized the use of intentions or expectations—such measures are imperfectly associated with behavior (Finkel and Muller 1998)—others argue that intentions are appropriate to model (Fishbein and Ajzen 1975). To address these known reservations about the validity of protest intentions, we provide estimates from a follow-up study assessing the predictive validity of our FSE (see the Supplemental Materials online).

Identities, Attitudes, Preferences, and Demographic Characteristics

Table 1 presents brief descriptions and summary statistics of the person-centered, or between-individual level, variables that constitute our person-specific effects (wording of each survey item can be found in the Supplemental Materials online). Among *self and social identities*, we use four measures: identification as a student (3-item scale, $\alpha = .832$), identification as a student activist (4-item scale, $\alpha = .960$), self-efficacy (10-item scale, $\alpha = .866$; Schwarzer and Jerusalem 1995), and values education (7-item scale, $\alpha = .629$). *Prior social and political action* is measured in four ways: prior membership in organizations (9-item scale, $\alpha =$

.604), prior volunteerism (1 = yes, 0 = no), prior participation in various forms of activism (9-item scale, $\alpha = .687$), and ever organized a protest (1 = yes, 0 = no).

We use multiple measures of *structural grievances and protest attitudes*. We capture perceived structural grievances toward the university with a 4-item scale ($\alpha = .906$). We use four 7-point items to measure protest attitudes: (1) the goal of a protest is more important than the act of protest, (2) will protest despite likely failure, (3) protests are an effective tool for social change, and (4) disruptive protests are justified. We also measure protest attitudes with willingness (or lack thereof) to be a first mover. Finally, we measure protest attitudes with a 3-item scale of protest norms ($\alpha = .427$), and use a single 7-point item to measure attitudes about the tuition increase (incidental grievance) by asking whether tuition increases are justified.

Among our *preferences and beliefs* variables, we use three measures. First, we include two four-item scales of generalized trust ($\alpha = .791$) and particularized trust ($\alpha = .688$) since both are related to collective action (Sonderskov 2011). Second, we measure social value orientations with the triple dominance scale of social values, a well-researched construct in psychology (Van Lange et al. 1997). Third, we capture risk-taking (or risk aversion) with a well-established single-item measure (Dohmen et al. 2010).

We also control for a number of demographic and political characteristics, including political orientation, age, race and ethnicity, gender, education, income, economic independence, and state residency (see the Supplemental Materials online for summary statistics). Finally, we control for design-based effects by including measures of study duration, attention checks, perceived vignette realism, and other variables in our statistical models.

Analytic Strategy

Multilevel structural equation model. The research design yields person-vignette panel data in which i vignettes ($i = 1, \dots, 12$) are nested within j individuals ($j = 880$). To test our model specified in Figure 1, we estimate a two-level structural equation model (SEM) where anger toward administration and likelihood of success mediate the effects of individual *and* vignette characteristics on intention to protest. To undermine potential confounding, our multilevel SEM decomposes the effects of anger toward administration and likelihood of success into within- *and* between-individual components. With this decomposition, our within-individual effects of anger and success on intentions are akin to coefficients in fixed-effects panel models (Allison 2009).

Thus, if the assumption of conditional independence is violated, bias would arise under reverse causality, or if a potential within-person confounder that covaried with the (temporal) process of assessing vignettes was correlated with anger and/or success at the within-individual level. We argue that the assumption of conditional independence is weak in our case since (1) a recent meta-analysis comparing observational studies to experimental studies suggests that the reflection problem is largely absent from the empirical literature testing dual pathway models of collective action (van Zomeren et al. 2008), and (2) potential within-person confounders are few and likely exert minimal bias. Taken together, the reduced form (total) effects at the within-individual level are causal effects, but the separated direct and indirect effects are not causal, and, instead, should be interpreted as within-individual conditional associations (Holland 1988; Morgan and Winship 2007).

By contrast, estimates at the between-individual level neither identify causal effects nor completely control for unobserved heterogeneity across individuals. Therefore, we must rely on the assumption, typically made in observational research, that the disturbance terms are uncorrelated with between-individual variables once we include level-2 control variables in the

model. Since this is a strong assumption, we interpret individual-level coefficients as conditional associations and not as causal effects.

Further details about the multilevel SEM, including reduced form equations and discussions of causal inference, can be found in the Supplemental Materials online.

Estimation. We estimate our multilevel SEM simultaneously as a single system using robust full-information maximum likelihood estimation (MLR). MLR provides consistent and asymptotically efficient estimates of parameters and consistent estimates of standard errors. Under the assumption of “data missing completely at random” or “missing at random”, MLR for missing data provides asymptotically unbiased estimates (Allison 2001). We have no reason to believe that the data are “missing not at random” given the non-sensitive nature of the study. As a result, we use MLR to model missingness throughout, and use Mplus 8.2 to estimate all models found in the present manuscript.

[Figure 2 about here]

Finally, given that our endogenous variables are bounded between 0 and 100, and exhibit data “clumping” at 0 and 100 (see Figure 2), three modeling procedures are possible: linear, Tobit, and fractional logit. We ultimately treat anger toward administration, likelihood of success, and intention to protest as continuous variables, and model them within a linear framework since (a) we observed minor substantive differences in estimates of total effects between linear, Tobit, and fractional logit models, (b) Tobit models yield biased estimates in the presence of heteroskedasticity (Greene 2003), and (c) our multilevel SEM failed to converge when the endogenous variables were estimated as limited dependent variables.

Results

We decompose the variance of our endogenous variables, *anger toward administration*, *likelihood of success*, and *intention to protest*, into within- and between-person variation by estimating an empty random intercept model for each endogenous variable. This yields an intraclass correlation of .31 for *anger*, .18 for *success*, and .28 for *intention*. Thus, 31 percent of the variance of *anger*, 18 percent of the variance of *success*, and 28 percent of the variance of *intention* is between individuals, with the remainder within individuals (i.e., across vignettes). The observed variation between individuals and vignettes justifies the use of multilevel models.

Vignette Level: Grievances, Incentives, and Frames

Our level-1 equations regress anger toward administration, likelihood of success, and intention to protest on vignette characteristics. We first present independently estimated multilevel models for anger toward administration, likelihood of success, and intention to protest in Table 2, followed by direct and indirect effects of vignette characteristics on intention to protest through anger and success (Table 3). In all models, we include dummy variables for vignette number (vignette 1 is the referent category) at the within-individual level (i.e., level-1) and all variables at the between-individual level (i.e., level-2).

[Tables 2 and 3 about here]

Anger toward administration: Total effects. Model 1 in Table 2 presents total effects of vignette characteristics on anger toward administration. Our first vignette dimension—collective good—operationalizes incidental grievances as tuition increases. We find that tuition increases strongly influence anger toward administration, and reject the null hypothesis that the three dummies are jointly equal to zero, $\chi^2(3) = 1424.46, p < .001$.⁶ Thus, as expected (see H1 and

⁶ Wald χ^2 tests of joint significance are used throughout.

H2a), tuition increases spark anger directed toward the University administration, which is the source of incidental grievances in our FSE.

In terms of incentives, selective rewards have a non-significant effect on anger toward administration, $\chi^2(3) = 1.33, p = .72$, while selective punishments have a significant effect, $\chi^2(3) = 141.91, p < .001$. As expected (see H2b), a university crackdown threatening students with salient penalties causes anger and indignation in students, especially as the costs of a looming threat increase. By contrast, the two dimensions operationalizing social incentives—social inducement and reputation—have little effect on anger (see Table 2). Similarly, two of the three collective action frames are non-significant, including the efficacy frame (see Table 2) and solidarity frame ($\chi^2(2) = 2.77, p = .250$). The injustice frame, however, is significant and in the expected direction (see H2c).

[Figure 3 about here]

Next, we explore the functional form between the size of the protest and anger toward administration. Using dummy variables for number of participants to provide a non-parametric estimate, we observe a relatively constant functional form (see Figure 3, Panel A). Thus, collective behavior directed toward redressing incidental grievances does not appear to spark individually experienced anger and indignation. Methodologically, we found that by treating the categorical values of number of participants as real numbers and assuming a continuous distribution, a quadratic term captured the functional form nearly as well as the non-parametric dummies for all three endogenous variables. We therefore report the more parsimonious parametric quadratic models throughout the manuscript. Regardless, we discuss the non-parametric functional forms for likelihood of success and intention to protest in their respective sub-sections.

Finally, we find that the anger manipulation is non-significant (see Table 2). This suggests that the method developed by Livingstone et al. (2011) was not successful at directly manipulating anger, and that we must rely on the assumption of conditional independence to identify the level-1 within-person effect of anger on intention to protest. The success manipulation, as expected, was statistically non-significant, $\chi^2(2) = 2.77, p = .250$.

Likelihood of success: Total effects. Model 2 in Table 2 presents total effects of vignette characteristics on likelihood of success. Like anger, our collective good dimension is statistically significant, $\chi^2(3) = 146.42, p < .001$, and monotonically related to likelihood of success. Thus, as expected (see H1a and H3b), students recognize that grievances are effective in galvanizing student protest.

We also find that selective rewards have a significant but modest effect on likelihood of success, $\chi^2(3) = 14.15, p < .01$. The effect implies that small incentives used in the real-world, such as providing magazine subscriptions or tote bags for participation, have little effect on the perceived collective efficacy of a protest. Of greater practical importance are selective punishments threatened by the university, $\chi^2(3) = 68.71, p < .001$. Taken together, selective rewards increase perceptions of collective efficacy, while selective punishments decrease perceptions of collective efficacy, both sets of findings support H1a and H3b.

By contrast, the two operationalizations of social incentives—social inducement and reputation—have non-significant effects on likelihood of success (see Table 2). And unlike anger toward administration, the efficacy frame (see Table 2) and solidarity frame ($\chi^2(2) = 8.13, p < .05$) have significant effects on likelihood of success. The injustice frame, in contrast, yields a non-significant result. Thus, the effects of collective action frames on perceived likelihood of success meet the expectations of H3c.

Having established the functional form between the size of the protest and anger toward administration, we now investigate the functional form for likelihood of success. Again, using dummy variables for number of participants, we find a decelerating functional form: increasing the number of participants from 0 to 1100 precipitously increases the average probability of success from 16 to about 42 (see Figure 3, Panel B). Between 400 and 1100 participants, the increase reaches an inflection point, leveling off until about 22,000 participants, with a maximum value of 79. Thus, when about 1100 students (or 4.78% of the student body) are mobilized, respondents perceive that the student walk-out has about a 42 percent chance of success, approximating a tipping point at which problems of “optimization, strategic action, surplus, and free riding” might kick in, causing the probability of success to flatten (Oliver and Marwell 2001: 295).” When 3000 students participate (13.04% of the student body), the likelihood of success is just under a 49 percent chance. This decelerating functional form parallels prior research (Matsueda et al. 2020; Robbins et al. 2020), and suggests that perceptions of collective efficacy are dynamic and interdependent (see H1 and H3a): contributions by others alters the perceived collective efficacy of a group. Like anger toward administration, the decelerating functional form is nicely captured by the quadratic term (see model 2, Table 2).

Finally, as expected, we find that the anger manipulation is non-significant, while the success manipulation is significant, $\chi^2(2) = 83.29, p < .001$. This suggests that the method developed by van Zomeren et al. (2010) was effective at directly manipulating success. To gain traction on the identification of the estimate, we need to investigate whether the success manipulation affects intention to protest *and* whether likelihood of success is statistically related to intention to protest. This is where we turn to next.

Intention to protest: Total effects. Model 3 in Table 2 presents total effects of vignette characteristics on intention to protest. As expected, most of the vignette characteristics causing anger toward administration and likelihood of success have similar effects on intention to protest. For instance, the collective good ($\chi^2(3) = 374.16, p < .001$) and selective punishment ($\chi^2(3) = 68.71, p < .001$) dimensions are significant, both of which monotonically influence intention to protest.

Moreover, those vignette dimensions that uniquely affect anger or success also affect intentions. Selective rewards, for example, significantly influence success as well as intentions, $\chi^2(3) = 63.93, p < .001$. Likewise, the dynamics between size of the protest and intention to protest parallel those observed for likelihood of success: increasing the number of participants from 0 to 1100 precipitously increases the average intention to protest from about a 21 to a 35 percent chance of participating, and at around 400 participants the increase reaches an inflection point (yielding a maximum average intention of 69 at 22,000 participants). Overall, the results are expected and support our hypotheses outlined in H1 and H3.⁷

Finally, the success manipulation is significant, $\chi^2(2) = 32.38, p < .001$, and in the expected direction. This finding supports the method of directly manipulating likelihood of success (van Zomeren et al. 2010). By contrast, the anger manipulation is non-significant (see Table 2), which is expected given that it is statistically unrelated to anger.

Intention to protest: Direct and indirect effects. Next, we regressed intention to protest on all vignette and individual characteristics as well as anger toward administration and likelihood

⁷ Critics claim that FSEs create unrealistic situations, which can bias responses (Faia 1980). Under some conditions, respondents may deem our scenarios as unrealistic (e.g., 20,000 protestors who are each paid \$200). To test for this, we estimated total effects models that did not include extreme counterfactual conditions (e.g., \$200 incentives, expulsion from school, and more than 3000 student protesters), thus eliminating a potential loss of legitimacy. Our results remained similar.

of success. Results of the multilevel SEM with mediation (at the vignette level) can be found in the Appendix (Table A2). Of particular note are the direct effects of anger toward administration and likelihood of success. For anger, we find a significant but modest effect: a change of 10 (on a scale of 0 to 100) in the magnitude of anger toward administration is associated with a 1.07 change in the probability of intention to protest. By contrast, the effect of likelihood of success on intention to protest is substantial and significant: a change of 10 in the probability of success is associated with a 6.72 change in the probability of intention to protest. Thus, as hypothesized (see H2 and H3), students are more likely to participate when they harbor indignation toward the university and expect the protest to succeed.

In Table 3, we decompose the total effects of vignette characteristics into direct and indirect components.⁸ To start, we consider the magnitude of the collective good (or incidental grievance). For this dimension, the greater the threatened tuition increase, the greater the public bad averted by a successful protest. What we find is that larger increases in tuition generate anger and cause students to believe that the protest will succeed, both of which induce protest intentions. The indirect effect of anger explains around one-fifth to a third of the total effect of tuition increases on intentions, while the indirect effect of success explains around a third to two-fifths of the total effect of tuition increases (see Table 3). Thus, large incidental grievances induce protest intentions via the dual pathways of anger and efficacy.

For selective rewards and punishments, the story is slightly different. Regarding the former, \$10 and \$50 incentives yield negligible effects on intention to protest, but an incentive of \$200 has a significant effect. For a \$200 incentive, about one-fifth of this effect operates indirectly through likelihood of success. Thus, selective rewards directly motivate students to

⁸ For space considerations, we do not report indirect effects that are statistically non-significant. Full models are available upon request.

participate by offsetting the personal costs of protesting, but also indirectly as students recognize that offering rewards to all students increases the likelihood that a protest will succeed. For selective punishments, we observe countervailing effects: threatened sanctions—regardless of their content—indirectly *increase* protest intentions via anger toward administration, but indirectly *decrease* intentions via likelihood of success. A negligible proportion of the total effects of selective punishments are accounted for by the anger pathway, while one-fifth to a quarter of the total effects are accounted for by the success pathway. Thus, threatened punishments deter participation directly by increasing the personal costs of protesting, but also indirectly by sparking anger and undermining perceived collective efficacy.

In terms of collective action frames, each respective frame is almost entirely mediated by a particular pathway. For instance, almost three-fourths of the efficacy frames operates indirectly through likelihood of success, while none of the effect operates indirectly through anger toward administration. Conversely, the majority of the effect of the injustice frame operates indirectly through anger toward administration, while a small non-significant proportion operates through likelihood of success. Unlike the other collective action frames, the solidarity frame yields “competitive mediation” where the total effect of the solidarity frame on intentions is non-significant (Zhao et al. 2010). But the indirect effects are countervailing: the likelihood of success pathway is positive and significant, while the anger toward administration pathway is negative and non-significant.

With respect to the size of the protest, recall that we observed a decelerating functional form (of varying degrees) between the number of participants dimension and the three endogenous regressors. When we decompose the total effect of number of participants on intention to protest into direct and indirect effects, we find that the indirect effect of success

dominates. And when we consider the indirect effects of anger and success, the direct effect of number of participants is negligible and non-significant. These results show that the relation between efficacy and intentions does not always imply successful collective action, since their decelerating functional forms reflect decreasing marginal returns to protesting.

Finally, we investigate the effectiveness of our anger and success manipulations. Because the effect of the anger manipulation was negligible and non-significant, we do not decompose its effects in Table 3. By contrast, the effect of the success manipulation on likelihood of success and intention to protest was non-negligible and significant. We find that the success manipulation increases or decreases intentions (depending on the manipulated level) with a large proportion of the positive or negative effects operating indirectly through likelihood of success, while none of the effects operate indirectly through anger toward administration. The direct effect of the success manipulation on intention to protest is also negligible and non-significant.

The results of our anger manipulation and success manipulation in conjunction with our analytic strategy yield two implications for the interpretation of our results. First, we must interpret the indirect effects of anger as within-person conditional associations (similar to coefficients generated by fixed effects panel models). Second, we can make strong claims about the causal interpretation of the indirect effects of success. These claims are further supported by a supplemental analysis that treats the success manipulation as an instrumental variable. In this supplemental analysis, we find that the indirect effects of likelihood of success are strikingly similar to those presented in Table 3 (see the Supplemental Materials online). In short, we firmly believe that a minimal amount of bias—if any—is present in the level-1 estimates of the (a) *direct effects* of the vignette characteristics on intention to protest, and the (b) *indirect effects* of

the vignette characteristics on intention to protest operating through anger toward administration and likelihood of success.

Individual Level: Identities, Attitudes, Preferences, and Demographic Characteristics

Next, we estimate our level-2 equations and analyze the effects of individual-level characteristics on individual-specific means (i.e., random intercepts) of anger toward administration, likelihood of success, and intention to protest. We first present independently estimated multilevel models in Table 4, followed by direct and indirect effects of individual characteristics on individual-specific means of intention to protest through anger and success (Table 5). In all models, we include vignette characteristics and dummy variables for vignette number (vignette 1 is the referent category) at the within-individual level (i.e., level-1).

[Tables 4 and 5 about here]

Anger toward administration: Total effects. Across the four sets of variables, measures of *structural grievances and protest attitudes* appear to drive individual-specific mean levels of anger (see Table 4). While self-efficacy (a measure of *self and social identities*) is significant and positively related to anger, all other variables (besides *structural grievances and protest attitudes*) are non-significant. Students with greater grievances directed toward the university, students who believe that disruptive protests are justifiable and who would protest despite probable failure, and students who harbor stronger normative obligations to protest, have greater baseline levels of anger toward the administration than those without. By contrast, increases in positive attitudes toward tuition increase, decreases baseline levels of anger. These results, when taken together, provide mixed support for dual pathway models of collective action. Only one antecedent condition—structural grievances and protest attitudes—affect anger (see H1c and

H2a), while the other—self and social identities—are largely unrelated to anger (see H1c and H2d).

Likelihood of success: Total effects. For likelihood of success, we observe similar findings: operationalizations of *structural grievances and protest attitudes* are the only variables that predict individual-specific mean levels of success. All other variables are non-significant. We find that students with no propensity to protest (i.e., “never movers”) have lower average levels of success than those with high propensities (i.e., “followers” and “first movers”). Conversely, attitudes toward protest are positively related to individual-specific mean levels of success; that is, respondents who would protest despite failure, who think that protests are an effective tool for social and political change, and who believe that protests are a justifiable form of collective action have greater average levels of success. Again, these results yield mixed support for dual pathway models of collective action: measures of self and social identity are statistically unrelated to efficacy (see H1c and H3d), while structural grievances and protest attitudes are positively related to efficacy (see H1c and H3a).

Intention to protest: Total effects. For intention to protest, we again observe similar findings: operationalizations of *structural grievances and protest attitudes* consistently predict individual-specific mean levels of intentions, while measures of *self and social identities* as well as *prior social and political action* are statistically unrelated to intentions. We find that students with no propensity to protest (i.e., “never movers”) have lower average levels of intentions than those with high propensities (i.e., “followers” and “first movers”). Likewise, students who hold favorable attitudes toward protest and harbor strong protest norms have greater individual-specific mean levels of intentions than those who do not. And those respondents who hold favorable attitudes toward tuition increases also report weaker average levels of intentions than

those who harbor unfavorable attitudes. Finally, we find that risk-taking—a measure of *preferences and beliefs*—is positively associated with average levels of intentions (i.e., risk takers have greater levels of intentions than respondents who are risk averse). Overall, we find that the intention to protest largely arises from structural grievances and protest attitudes and not necessarily self and social identities.

Intention to protest: Direct and indirect effects. Next, we regressed intention to protest on all vignette and individual characteristics as well as anger toward administration and likelihood of success. Results of the multilevel SEM with mediation (at the individual level) can be found in the Appendix (Table A3). Of particular note are the direct effects of anger toward administration and likelihood of success. For anger, we find a significant but modest effect: a change of 10 (on a scale of 0 to 100) in the average level of anger toward administration is associated with a 0.89 change in the average level of intention to protest. By contrast, the relation between the average level of success and average level of intention is non-significant. These results suggest that anger operates through two multilevel pathways: one dispositional (students with greater average levels of anger have greater average levels of intention) and one situational (students who experience anger as a result of the situation are more likely to participate). The efficacy pathway, by contrast, operates exclusively at the vignette (or situational) level.

In Table 5, we decompose the total effects of individual characteristics into direct and indirect components. For anger, we find that the indirect effects of self-efficacy and structural grievances are statistically non-significant at the $p < .05$ level. This suggests that the direct effect of anger on intentions at the individual-level is (a) substantively negligible to yield significant indirect effects, and (b) offset by countervailing direct effects of self-efficacy and structural grievances on average levels of intention to protest. By contrast, the statistically significant

individual-level variables that operate indirectly are protest norms and attitudes toward tuition increases. In these cases, protest norms bolster intentions via anger, and attitudes toward tuition increases dampen intentions via anger. For efficacy, we find that none of the individual-level variables operate indirectly via likelihood of success. This finding is expected given the statistically non-significant direct effect of success on intentions (see Table B2). In the end, few individual-level variables operate indirectly via average levels of anger and success. The bulk of the statistically significant individual-variables detected in Table 4, instead, operate directly on average levels of intention to protest.

Discussion and Conclusion

Social scientists have been paying greater attention to emotion and affect in understanding the dynamics of social movements and collective action (DeCelles et al. 2019; Goodwin et al. 2000, 2001; Jasper 2011, 2018; Useem 1998). Emotions, as a result, have gained theoretical prominence for scholars of protest, rebellion, and revolutions, with some social psychologists identifying anger as one of two key pathways—the other being collective efficacy—linking social identity and collective disadvantage to collective action (Klandermans et al. 2008; Leach et al. 2006; Stürmer and Simon 2009; Tausch et al. 2011; van Stekelenburg et al. 2011; van Zomeren et al. 2004). These dual pathway models of collective action exhibit strong empirical support, but a number of questions remain, namely the breadth of initial conditions and issues of casual identification.

In using a FSE of student protest with a disproportionate stratified random sample of undergraduate students at a large, public university, we provide a test of a multilevel dual pathway model of collective action. Our empirical analysis yields four principal results. First, the dual pathways of anger and efficacy operate at multiple levels of analysis: the effect of anger

follows two routes—one dispositional and one situational—while the effect of efficacy flows through a situational channel. In both instances, anger and efficacy indirectly affect protest intentions. Second, self and social identities play a negligible role in producing anger, efficacy, and protest intentions, while grievances and collective disadvantage account for all three. A set of findings that simultaneously questions and supports the initial dual pathway models of collective action. Third, consistent with social movement and collective action theories, the dual pathways of anger and efficacy are triggered by a broad set of situational conditions (incidental grievances, selective rewards and punishments, collective action frames, and size of the protest), while anger is a function of a narrow set of dispositional factors (protest norms and attitudes). Fourth, our identification strategy allows us to claim—with some confidence—that the indirect effects of anger and efficacy at the within-individual level are weakly biased.

The implications of these findings are manifold. We discovered important initial conditions for both pathways, how each pathway operates given a specific initial condition, and what the initial conditions imply for dual pathway models of collective action. While dual pathway models do differ with respect to the content and number of pathways, most underscore social identity and collective disadvantage as important initial conditions (van Stekelenburg 2013; van Stekelenburg and Klandermans 2013; van Stekelenburg et al. 2011; van Zomeren et al. 2004; van Zomeren et al. 2008; van Zomeren et al. 2012). Yet, we only observe empirical support for one: collective disadvantage. At the individual level, we find that perceived structural grievances directly relate to anger, but not protest intentions. Only at the vignette (or within-individual) level do we confirm the role of collective disadvantage: incidental grievances—in the form of tuition increases—positively influence anger and efficacy, which in turn influence protest intentions.

This is an important finding for the larger literature on grievances (Hechter et al. 2016; Turner and Killian 1957; Useem 1998). Individuals tend to tolerate longstanding, structural grievances like poor wages and difficult living conditions, but incidental ones—like major court decisions affecting the status of cultural and ethnic minorities, state repression, episodes of police brutality, and sudden tuition increases—are sufficient to cause spontaneous mobilization (Bergstrand 2014; Pfaff and Hechter 2020; Tausch et al. 2011; van Stekelenburg and Klandermans 2013; van Zomeren et al. 2008). Recall that Mancur Olson (1965) discounted the role of grievances in large groups, which is also consistent with many resource mobilization theories (Oberschall 1973; Zald and McCarthy 1979). Contrary to the Olsonian paradigm, we find that grievances increase protest intentions, and do so via affective *and* instrumental mechanisms (van Zomeren et al. 2004; van Zomeren et al. 2008; van Zomeren et al. 2010; van Zomeren et al. 2012). Grievances spark anger and indignation, and heighten perceptions of collective efficacy in averting the collective bad. Moreover, the direct effect of grievances on protest intentions can be explained by rational decision making (Hechter et al. 2016): the greater the threatened tuition increase, the greater the individual returns to thwarting the tuition increase. In short, incidental grievances matter, and are crucial to fueling mobilization.

While collective disadvantage is an important situational driver of mobilization that operates via anger and efficacy, we fail to observe comparable support for the role of self and social identities. Across multiple measures of identity—ranging from identification with the student body to identification as a student activist—we consistently observe null effects. While we do not claim that self and social identities are unimportant for all types of collective action, our results do suggest that social identity—operationalized herein—is negligible for the particular issue of student protests against tuition increases. Given identity’s prominence in

theories of mobilization across the social sciences (Gould 1995; Melucci 1988; Pfaff 1996; Stürmer and Simon 2004; van Stekelenburg and Klandermans 2013; van Zomeren et al. 2004), what accounts for this null effect? First, categorical membership as a student is relatively permeable, and does not imply a life-long commitment. It is an identity that people expect to exit at some point in their lives. And although students are a subordinate group, they are relatively advantaged (Hensby 2017). As van Zomeren et al. (2008) note, the effect of social identity on protest intentions is strongest when people belong to a disadvantaged group that is difficult to exit (p. 507). Second, the effects of social identity on protest intentions may be especially potent when the social standing and life chances of members of a group are threatened. Given that students occupy multiple social identities, perhaps general “student” or “activist” identities lack salience compared to other nominal collective identities—such as gender, race, and sexual orientation—within which grievances are differentially experienced. With these caveats in mind, future work should investigate whether—and to what extent—social identity spurs protest participation for different types of disadvantaged groups.

Beyond collective disadvantage and incidental grievances, we identify a number of additional initial conditions sufficient to activate the dual pathways of anger and efficacy. The positive and negative effects of selective rewards and punishments, respectively, provide the clearest evidence in support of Olson (1965), but also contradict some of his core assumptions. While Olson (1965) fundamentally recognized the instrumental path through which selective rewards and punishments operate, Olson overlooked the explanatory power of affect and emotion. With respect to selective rewards, note that a \$10 incentive exerts a negligible, non-significant effect on protest intentions, whereas an incentive of \$50 directly affects protest intentions and an incentive of \$200 directly *and* indirectly influences intentions via efficacy. In

the real world, though, organizers are unable to offer selective incentives to all participants of large protests, while political authorities can *and* do credibly coerce all participants. Oliver (1980) referred to this as an asymmetry between rewards and punishments. And what we find is that the effect sizes of selective punishments are much greater than selective rewards, where punishments operate through multiple channels. The participants in our study responded to all forms of manipulated coercion, with each level of severity having a monotonic effect on anger, efficacy, and protest intentions. Unlike rewards, punishments exhibit countervailing effects: they increase protest intentions by causing anger, but decrease intentions by reducing perceptions of success. The negative efficacy channel, however, is much stronger than the positive emotion channel. This finding helps to account for the paradox of repression so often observed in studies of protest mobilization. Whereas repression is generally quite effective in squelching unrest, under some conditions it triggers indignation sufficient to overcome expected sanctions (Goldstone and Tilly 2001).

What our analysis of selective rewards and punishments suggests is that collective action frames highlighting the emotional impact of repression may be able to counteract the negative effects of coercion on collective efficacy. This type of framing process goes unobserved in our study, but we do find overwhelming support for the effects of other collective action frames, namely frames that highlight injustice, solidarity, and efficacy (Snow and Oliver 1995; Snow et al. 1986). In all three cases, our frame manipulations positively and indirectly affect protest intentions via one of the theorized pathways: the solidarity and efficacy frames operate through collective efficacy, whereas the injustice frame operates through anger. The cognitive frames and framing literature have long theorized about such effects (Gamson 1992), and our study is one of the first to test these expectations with a research design that: (a) manipulates perceived

meanings in an experimental setting; and (b) operationalizes frames without inferring meanings from social interaction. Yet, what we capture are snapshots of collective action frames and not dynamic frame alignment processes (Snow et al. 1986). In spite of findings in support of collective action frames, future research should examine the frame alignment process in a dynamic, experimental setting.

Our study has important implications for rational choice models of collective behavior. The present findings demonstrate that individuals consider their own interests as well as the interests of others, with their protest intentions partly determined by what they believe others would do in the same situation. The key result in support of this claim is the finding that the functional form between protest size and protest intentions is decelerating and almost completely mediated by collective efficacy. These results imply that each individual's contribution makes the next individuals' less worthwhile and thus less likely (Oliver et al. 1985). This process is largely instrumental and lacks strong affective foundations: students consider the actions of other students in perceiving the group's success and in forming their own intentions, but the emotions of students are only weakly activated by the participation of others.

These results resonate with recent research by Robbins et al. (2020) who find that the population-average production and mobilization functions of collective action are decelerating. Where Robbins et al. (2020) merely map the contours of these functions, we provide clear evidence for a causal link between the two (see also Matsueda et al. 2020). By doing so, we illuminate the mechanisms of social influence and contagion in collective action: witnessing collective behavior fosters beliefs about the likelihood of success (McPhail 1991), it does not heighten emotional arousal as some early theorists of crowd behavior were apt to claim (Le Bon 1895). What this discussion conveys, overall, is that our findings fundamentally undercut

Olson's (1965) assumption of independent actors, but supports theories that assume interdependence between instrumentally motivated actors (e.g., Centola 2013; Macy 1991; Marwell and Oliver 1993; Granovetter 1978).

Beyond situational sources of protest intentions, we find that perceived structural grievances and protest attitudes matter for average levels of anger, efficacy, and/or protest intentions. In particular, we find that propensities to protest (i.e., would be a "first mover"), favorable attitudes toward protest (e.g., protesting is an end-in-itself), and strong protest norms are associated with greater individual-specific mean levels of protest intentions. And individuals who firmly believe that tuition increases are justifiable, report weaker average levels of intentions than those who believe that tuition increases are unjustifiable. Importantly, we discover that only a few of these variables operate indirectly via anger and efficacy, while most directly affect average levels of protest intentions.

These findings have two implications for the study of collective action. First, most models of collective action contend that attitudes, preferences, and interests are necessary for the emergence of collective behavior (Fehr and Gintis 2007; Granovetter 1978; Hardin 1982; Henrich et al. 2001; Jasso and Opp 1997; Medina 2007; Opp 2009; Tilman et al. 2018). Theories of the critical mass, for instance, propose that production functions are determined by individual interest in (or desire for) the common good and by resources available to produce the common good (Marwell and Oliver 1993). Our findings support this line of theorizing, and question Olson's (1965) original claim that preferences and interests are insufficient to produce collective action. Second, with the exception of protest norms and attitudes toward tuition increases, most measures of perceived structural grievances and protest attitudes directly affect average levels of protest intentions. In other words, most person-centered variables do not operate via individual

propensities for anger and efficacy. While some other unobserved mechanism besides anger and efficacy may account for the direct effects of protest attitudes, we contend that the likely explanation is instrumental: interest in—or desire for—protest as a means-to-an-end or an end-in-itself serves as an important condition for one’s willingness to protest.

Taken together, our results support the idea that individuals think through emotions, grapple with feelings, contend with the actions of others, and act on their own interests (Jasper 2018; van Zomeren et al. 2012). Nevertheless, several caveats are in order. First, our null findings for social incentives must be approached with caution. We operationalized social incentives with vignette dimensions representing reputational effects and social inducement. In both cases, we assume that respondents are capable of imagining hypothetical situations in which friends attempt to induce participation or family members respect them for their participation (even if the respondent’s actual friends and family are unlikely to act in this way). If respondents are unable to imagine such a scenario, the manipulations fail and effect sizes are underestimated.

We contend that our assumptions likely held, as research suggests that individuals are adept at imagining “what if” counterfactuals even if their real-life situations do not parallel those described in the hypothetical situation (Wallander 2009). Moreover, it is one thing “to think” about how one should feel under certain conditions, and quite another to feel real anger. Our FSE may have invoked “feeling rules” (Holmes 2004) applicable to the hypothetical situation without arousing real emotions. If this is the case, estimates of social incentives would be downwardly biased (Childs and Collett 2011). Even though a recent review shows that hypothetical situations are capable of inducing emotions with real physiological responses (Siedlecka and Denson 2019), future research should probe the validity of our assumptions and explore whether respondents react to our hypothetical social incentives as hypothesized.

Second, our research design is hypothetical and investigates protest intentions rather than protest participation. We see this is a strength for a number of reasons. Identifying when and how individuals intend to protest is key to understanding actual protest behavior (Klandermans 1984). A claim generally supported by the theory of reasoned action (Fishbein and Ajzen 1975), which suggests that intentions are appropriate to model. Moreover, protest intentions are a frequent outcome under study in the literature on dual pathway models of collective action (see Shi et al. 2015; Stürmer and Simon 2009; van Zomeren et al. 2004). Restricting our study to protest intentions thus facilitates cross-study comparisons. Our research design also allows us to move beyond the logistical constraints of lab experiments and field experiments. With our FSE, we experimentally test and comparatively evaluate operationalizations of the university administration, the student organization, as well as the protest environment facing respondents (Auspurg and Hinz 2015). All of which are difficult to manipulate with any other design. Finally, when the choices and conditions of an FSE parallel those observed in the real world, behavioral intentions strongly predict behavior (Hainmueller et al. 2015). Our analysis of predictive validity supports this claim (see the Supplemental Materials online). In spite of the arguments and evidence in favor of FSEs, assessing the degree to which our findings apply to actual protest behavior is an important endeavor for future research.

Finally, the present study focused on a particular type of collective action, albeit an important and pivotal one in the history of social movements. Our results are therefore limited to local populations addressing incidental grievances with normative—or peaceful—methods of mobilization. Such findings are relevant for scholars interested in student protests, labor protests, and demonstrations against suddenly-imposed grievances, and less so for scholars who wish to understand nationwide political campaigns, spontaneous riots, and non-institutional mobilization.

The latter is particularly relevant since research indicates that anger and efficacy are negatively related to extreme, non-normative types of collective action (Stürmer and Simon 2009; Tausch et al. 2011), and that anger reduces protest intentions for institutional insiders who are sympathetic to the goals of a social movement (DeCelles et al. 2019). Because of the circumscribed nature of our design, we welcome future research that investigates dual pathway models of collective action for different types of grievances, collective actions, and institutional actors.

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Table 1. Sample Descriptive Statistics

Name	Definition	Mean	SD	Min/Max	N
<i>Vignette-level dependent variables</i>					
Anger toward administration	Extent to which respondent angry toward administration (0-100 Visual Analog Scale)	63.67	29.849	0/100	10454
Likelihood of success	Percent chance student walk-out will succeed (0-100 Visual Analog Scale)	36.253	30.635	0/100	10483
Intention to protest	Percent chance take part in student walk-out (0-100 Visual Analog Scale)	36.578	34.924	0/100	10462
<i>Individual-level independent variables</i>					
First mover: Student protest					
Follower (omitted)	Would participate in a student walk-out if others participated as well	.706	---	0/1	878
First mover	Would be the first person to participate in a student walk-out	.087	---	0/1	878
Never mover	Would never participate in a student walk-out	.079	---	0/1	878
DKPNS	Don't know or prefer not to say	.126	---	0/1	878
Protest goal-alignment	Goal more important than act of protest (1 = strongly disagree, 7 = strongly agree)	5.745	1.500	1/7	858
Protest despite failure	Will protest despite probable failure (1 = strongly disagree, 7 = strongly agree)	3.076	1.596	1/7	850
Protest effectiveness	Protest effective tool for social change (1 = strongly disagree, 7 = strongly agree)	5.061	1.466	1/7	849
Protests justified	Disruptive protests justified (1 = strongly disagree, 7 = strongly agree)	4.765	1.603	1/7	851
Protest norms	3-item scale of norms of student protest ($\alpha = .427$)	4.868	0.940	2/7	823
Attitudes toward tuition increases	Tuition increases justified (1 = strongly disagree, 7 = strongly agree)	3.212	1.534	1/7	849
Prior activism ^a	9-item scale of prior participation in different forms of activism ($\alpha = .687$)	1.468	1.557	0/9	844
Protest coordination					
Never organized (omitted)	Never organized or coordinated a demonstration, social protest, or walk-out?	.946	---	0/1	876
Organized	Has organized or coordinated a demonstration, social protest, or walk-out	.036	---	0/1	876
DK/PNS	Don't know or prefer not to say	.017	---	0/1	876
Associational membership ^b	9-item scale of belonging to different associations and organizations ($\alpha = .604$)	2.485	1.644	0/9	855
Volunteerism					
Does not volunteer (omitted)	Does not spend time volunteering for clubs, social services, or charities during the week	.525	---	0/1	878
Volunteers	Spends time volunteering for clubs, social services, or charities during the week	.373	---	0/1	873
DK/PNS	Don't know or prefer not to say	.100	---	0/1	878
Student identity	3-item scale of student identification ($\alpha = .832$)	5.661	1.263	1/7	861
Student activist identity	4-item scale of student activist identification ($\alpha = .960$)	2.328	1.375	1/7	842
Values education ^a	7-item scale of higher-education a respondent values ($\alpha = .629$)	4.782	1.716	0/7	863
Structural grievances	4-item scale of perceived structural grievances toward university ($\alpha = .906$)	3.393	1.167	1/7	824
Self-efficacy	10-item Generalized Self-Efficacy Scale ($\alpha = .866$)	3.196	0.418	1/4	863
Generalized trust	4-item scale of generalized trust ($\alpha = .791$)	4.523	1.394	0/10	830
Particularized trust	4-item scale of particularized trust ($\alpha = .688$)	7.440	1.187	0.75/10	838
Social value orientations					
Proself: Individualist (omitted)	Individualist social value orientation	.198	---	0/1	880
Proself: Competitor	Competitive social value orientation	.060	---	0/1	880
Prosocial	Cooperative social value orientation	.644	---	0/1	880
Unclassified	Not classified social value orientation	.095	---	0/1	880
Risk-taking	7-point item where 1 = avoids taking risks, and 7 = fully prepared to take risks	4.541	1.432	1/7	841

Note : DK = don't know; PNS = prefer not to say; ^a = row sums of the items were used.

Table 2. Two-Level Models of Anger toward Administration, Likelihood of Success, and Intention to Protest, Vignette-Level Total Effects

	Model 1	Model 2	Model 3
	Anger toward Administration	Likelihood of Success	Intention to Protest
<i>Within Level: Vignettes</i>			
Collective good			
1% tuition increase (omitted)			
10% tuition increase	14.392*** (0.712)	2.850*** (0.643)	5.006*** (0.742)
50% tuition increase	28.688*** (0.858)	5.636*** (0.688)	11.169*** (0.835)
100% tuition increase	36.786*** (0.997)	8.300*** (0.712)	17.790*** (0.957)
Selective rewards			
No monetary incentive (omitted)			
\$10 incentive	-0.459 (0.565)	0.546 (0.605)	1.129 (0.705)
\$50 incentive	-0.290 (0.550)	0.318 (0.597)	1.310 (0.694)
\$200 incentive	0.132 (0.566)	2.072** (0.609)	5.544*** (0.727)
Selective punishments			
No penalty (omitted)			
Incomplete grades	2.871*** (0.567)	-3.633*** (0.678)	-10.105*** (0.810)
Failing grades	5.810*** (0.598)	-4.789*** (0.670)	-16.010*** (0.859)
Expulsion	7.131*** (0.650)	-5.306*** (0.686)	-16.945*** (0.920)
Social inducement			
No approached (omitted)			
Some friends	0.507 (0.402)	0.228 (0.434)	0.729 (0.500)
Reputation			
No reputation (omitted)			
Reputation	0.127 (0.405)	0.169 (0.423)	0.026 (0.489)
Efficacy frame			
No efficacy (omitted)			
Efficacy	0.342 (0.386)	2.237*** (0.440)	2.042*** (0.509)
Injustice frame			
No injustice (omitted)			
Injustice	10.425*** (0.526)	0.597 (0.433)	1.403** (0.516)
Solidarity frame			
No solidarity (omitted)			
Solidarity	-0.788 (0.475)	1.361** (0.515)	0.668 (0.595)
Moral solidarity	-0.462 (0.482)	1.207* (0.512)	0.529 (0.627)
Number of participants	0.321* (0.126)	5.867*** (0.168)	4.023*** (0.179)
Number of participants ²	-0.014* (0.006)	-0.159*** (0.007)	-0.101*** (0.008)
Success manipulation			
No news (omitted)			
Bad news	-0.056 (0.481)	-1.589** (0.527)	-1.572* (0.623)
Good news	0.539 (0.486)	3.538*** (0.542)	2.051** (0.622)
Anger manipulation			
No anger (omitted)			
Friends' anger	0.627 (0.377)	-0.067 (0.445)	0.126 (0.498)
Constant	-3.668 (12.686)	18.597 (10.112)	-8.020 (11.087)
Individual-level variables	Yes	Yes	Yes
Vignette dummy variables	Yes	Yes	Yes
Intercept variance	213.737*** (12.572)	155.447*** (8.990)	182.403*** (11.592)
Residual variance	372.472*** (9.403)	446.436*** (10.504)	596.576*** (13.381)
Vignettes	10560	10560	10560
Individuals	880	880	880
Level-1 R^2	0.389	0.398	0.300

*** $p < .001$, ** $p < .01$, * $p < .05$

Notes: Maximum likelihood for missing data with robust standard errors used throughout. Values of the number of participants is divided by 1000 to ease model convergence.

Table 3. Total, Direct, and Indirect Effects of Vignette Dimensions on Intention to Protest

	Total (SE)	Direct (SE)	Indirect _{anger} (SE)	Indirect _{success} (SE)
Collective good				
1% tuition increase (omitted)				
10% tuition increase	5.030*** (0.738)	1.559** (0.599)	1.546*** (0.216)	1.924*** (0.437)
50% tuition increase	11.180*** (0.835)	4.320*** (0.712)	3.080*** (0.417)	3.780*** (0.477)
100% tuition increase	17.813*** (0.955)	8.249*** (0.851)	3.950*** (0.533)	5.604*** (0.503)
Selective rewards				
No monetary incentive (omitted)				
\$10 incentive	1.105 (0.702)	0.786 (0.553)	-0.048 (0.061)	0.367 (0.407)
\$50 incentive	1.296 (0.694)	1.109* (0.566)	-0.030 (0.059)	0.218 (0.401)
\$200 incentive	5.510*** (0.727)	4.113*** (0.598)	0.014 (0.061)	1.384** (0.413)
Selective punishments				
No penalty (omitted)				
Incomplete grades	-10.061*** (0.808)	-7.921*** (0.658)	0.308*** (0.070)	-2.447*** (0.459)
Failing grades	-15.941*** (0.857)	-13.336*** (0.698)	0.624*** (0.099)	-3.229*** (0.456)
Expulsion	-16.889*** (0.917)	-14.094*** (0.753)	0.765*** (0.114)	-3.560*** (0.471)
Efficacy frame				
No efficacy (omitted)				
Efficacy	2.026*** (0.507)	0.503 (0.397)	0.037 (0.042)	1.486*** (0.297)
Injustice frame				
No injustice (omitted)				
Injustice	1.412** (0.516)	-0.122 (0.425)	1.119*** (0.157)	0.415 (0.291)
Solidarity frame				
No solidarity (omitted)				
Solidarity	0.685 (0.594)	-0.136 (0.486)	-0.082 (0.053)	0.903** (0.346)
Moral solidarity	0.522 (0.628)	-0.248 (0.511)	-0.047 (0.053)	0.817* (0.345)
Number of participants	4.025*** (0.179)	0.048 (0.142)	0.034* (0.014)	3.943*** (0.144)
Number of participants ²	-0.101*** (0.008)	0.008 (0.006)	-0.001* (0.001)	-0.107*** (0.005)
Success manipulation				
No news (omitted)				
Bad news	-1.549* (0.622)	-0.488 (0.478)	-0.005 (0.052)	-1.056** (0.356)
Good news	2.047** (0.622)	-0.398 (0.502)	0.059 (0.053)	2.386*** (0.363)

*** $p < .001$, ** $p < .01$, * $p < .05$

Notes : Maximum likelihood for missing data with robust standard errors used throughout. Values of the number of participants is divided by 1000 to ease model convergence.

Table 4. Two-Level Models of Anger toward Administration, Likelihood of Success, and Intention to Protest, Individual-Level Total Effects

	Model 1	Model 2	Model 3
	Anger toward Administration	Likelihood of Success	Intention to Protest
<i>Between Level: Individuals</i>			
<i>Self and social identities</i>			
Student identity	0.781 (0.560)	0.058 (0.472)	-0.070 (0.488)
Student activist identity	0.828 (0.547)	-0.094 (0.431)	0.615 (0.515)
Self-efficacy	3.731* (1.633)	-0.098 (1.341)	0.293 (1.567)
Values education	0.116 (0.364)	-0.072 (0.325)	-0.034 (0.356)
<i>Prior social and political action</i>			
Associational membership	0.138 (0.401)	-0.134 (0.356)	0.359 (0.416)
Volunteerism			
Does not volunteer (omitted)			
Volunteers	-0.637 (1.229)	0.732 (1.134)	-1.821 (1.176)
Prior activism	0.033 (0.437)	0.093 (0.404)	0.808 (0.476)
Protest coordination			
Never organized (omitted)			
Organized	0.884 (3.158)	4.071 (3.317)	3.652 (3.820)
<i>Structural grievances and protest attitudes</i>			
Structural grievances	1.588** (0.575)	0.598 (0.525)	0.570 (0.540)
First mover: Student protest			
Follower (omitted)			
First mover	1.721 (2.119)	1.831 (2.033)	12.308*** (2.317)
Never mover	-1.303 (2.454)	-4.066* (1.791)	-11.585*** (1.956)
Protest goal-alignment	-0.074 (0.412)	-0.311 (0.394)	0.369 (0.407)
Protest despite failure	0.805* (0.410)	1.359*** (0.370)	3.690*** (0.430)
Protest effectiveness	0.251 (0.491)	1.445*** (0.404)	1.140** (0.435)
Protests justified	1.146** (0.391)	0.776* (0.351)	1.144** (0.383)
Protest norms	3.573*** (0.766)	-0.409 (0.713)	1.972* (0.804)
Attitudes toward tuition increases	-2.399*** (0.400)	-0.229 (0.361)	-1.178** (0.384)
<i>Preferences and beliefs</i>			
Generalized trust	0.055 (0.465)	0.078 (0.399)	0.146 (0.469)
Particularized trust	0.488 (0.500)	0.432 (0.490)	0.346 (0.582)
Social value orientations			
Proself: Individualist (omitted)			
Proself: Competitor	-4.760 (2.728)	-0.084 (2.283)	-1.850 (2.366)
Prosocial	-2.165 (1.456)	0.365 (1.307)	0.535 (1.454)
Unclassified	-4.045 (2.192)	2.001 (2.018)	2.042 (2.221)
Risk-taking	-0.354 (0.463)	0.483 (0.366)	1.010* (0.434)
Constant	-3.668 (12.686)	18.597 (10.112)	-8.020 (11.087)
Individual-level control variables	Yes	Yes	Yes
Vignette-level variables	Yes	Yes	Yes
Vignette dummy variables	Yes	Yes	Yes
Intercept variance	213.737*** (12.572)	155.447*** (8.990)	182.403*** (11.592)
Residual variance	372.472*** (9.403)	446.436*** (10.504)	596.576*** (13.381)
Vignettes	10560	10560	10560
Individuals	880	880	880
Level-2 R^2	0.238	0.177	0.364

*** $p < .001$, ** $p < .01$, * $p < .05$

Notes: Maximum likelihood for missing data with robust standard errors used throughout.

Table 5. Total, Direct, and Indirect Effects of Individual-Level Variables on Intention to Protest

Individual-level variable	Total (SE)	Direct (SE)	Indirect _{anger} (SE)	Indirect _{success} (SE)
<i>Self and social identities</i>				
Self-efficacy	0.054 (1.222)	-0.279 (1.240)	0.328° (0.197)	0.004 (0.052)
<i>Structural grievances and protest attitudes</i>				
Structural grievances	-0.006 (0.415)	-0.124 (0.413)	0.140° (0.075)	-0.022 (0.032)
First mover: Student protest				
Follower (omitted)				
First mover	11.060*** (2.066)	10.961*** (2.011)	0.161 (0.201)	-0.063 (0.109)
Never mover	-8.795*** (1.577)	-8.828*** (1.607)	-0.123 (0.221)	0.155 (0.178)
Protest goal-alignment	0.578* (0.291)	0.573* (0.292)	-0.007 (0.037)	0.012 (0.020)
Protest despite failure	2.704*** (0.361)	2.694*** (0.366)	0.064 (0.045)	-0.054 (0.061)
Protest effectiveness	0.127 (0.332)	0.160 (0.330)	0.024 (0.045)	-0.056 (0.062)
Protests justified	0.501° (0.288)	0.430 (0.287)	0.101° (0.054)	-0.030 (0.035)
Protest norms	1.811** (0.587)	1.479* (0.591)	0.314* (0.147)	0.017 (0.033)
Attitudes toward tuition increases	-0.760** (0.293)	-0.556° (0.297)	-0.213* (0.096)	0.009 (0.017)
<i>Preferences and beliefs</i>				
Risk-taking	0.716* (0.342)	0.764* (0.342)	-0.029 (0.043)	-0.019 (0.024)

*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, ° $p < 0.10$ (two-tailed test)

Note: normal-theory standard errors in parentheses (SE). The total, direct, and indirect effects are unique between-level effects minus the within-level effects of individual-level variables on random intercepts. Thus, the discrepancy between total effects of individual-level variables found here and in Table 4.

Figure 1. A Multilevel Dual Pathway Model of Collective Action

Figure 2. Histograms of Anger toward Administration (A), Likelihood of Success (B), and Intention to Protest (C)

Figure 3. Graph of Predicted Anger toward Administration (**A**), Likelihood of Success (**B**), and Intention to Protest (**C**) by Number of Participants (Dummies).

Note: Solid black markers indicate predicted values with 95% confidence intervals in black.

Appendix

Table A1. Dimensions and Levels of the Factorial Survey Experiment of Student Protest

Dimension	Levels	Text
<i>Collective good</i>	1	The approved legislation will amount to a 1% increase in the tuition you will pay.
	2	The approved legislation will amount to a 10% increase in the tuition you will pay.
	3	The approved legislation will amount to a 50% increase in the tuition you will pay.
	4	The approved legislation will amount to a 100% increase in the tuition you will pay.
<i>Selective rewards</i>	1	Blank
	2	A national student federation will provide \$10 to every student who participates in the walk-out.
	3	A national student federation will provide \$50 to every student who participates in the walk-out.
	4	A national student federation will provide \$200 to every student who participates in the walk-out.
<i>Selective punishments</i>	1	The University states that it will not penalize students who participate in the walk-out.
	2	The University threatens that students who participate in the walk-out will receive incomplete grades in their courses and will have to make up missed work within one week.
	3	The University threatens that students who participate in the walk-out will receive failing grades in their courses.
	4	The University threatens that students who participate in the walk-out will be subject to expulsion.
<i>Social inducement</i>	1	Blank
	2	A few days before the walk-out, some of your friends approach you and urge you to participate in the walk-out with them.
<i>Reputation</i>	1	Blank
	2	In conversing with your friends and family, the clear consensus among them is that they will respect anyone who takes part in the walk-out.
<i>Solidarity frame</i>	1	Blank
	2	In the days before the walk-out is to begin, the University's service workers declare that they will unite with the students and join the walk-out.
	3	In the days before the walk-out is to begin, the University's service workers declare that they will unite with the students and join the walk-out because it is the right thing to do.
<i>Efficacy frame</i>	1	You do not know how student walk-outs have affected increases in tuition at other American universities.
	2	News reports indicate that comparable walk-outs at other American universities have succeeded in preventing tuition increases.
<i>Injustice frame</i>	1	The University's president and top administrators have announced that they are not going to adjust their salaries for the coming year.
	2	The student group reveals that the University's president and top administrators are planning a 10% increase in their salaries for the coming year.
<i>Number of participants</i>	1	In the morning on the first day of the walk-out, you learn that no students (or 0% of 23,000 possible students) have participated in the walk-out.
	2	In the morning on the first day of the walk-out, you learn that one student (or 0.004% of 23,000 possible students) has participated in the walk-out.
	3	In the morning on the first day of the walk-out, you learn that three students (or 0.01% of 23,000 possible students) have participated in the walk-out.
	4	In the morning on the first day of the walk-out, you learn that seven students (or 0.03% of 23,000 possible students) have participated in the walk-out.
	5	In the morning on the first day of the walk-out, you learn that twenty students (or 0.09% of 23,000 possible students) have participated in the walk-out.
	6	In the morning on the first day of the walk-out, you learn that fifty-five students (or 0.24% of 23,000 possible students) have participated in the walk-out.
	7	In the morning on the first day of the walk-out, you learn that one hundred and fifty students (or 0.65% of 23,000 possible students) have participated in the walk-out.
	8	In the morning on the first day of the walk-out, you learn that four hundred students (or 1.74% of 23,000 possible students) have participated in the walk-out.
	9	In the morning on the first day of the walk-out, you learn that one thousand one hundred students (or 4.78% of 23,000 possible students) have participated in the walk-out.

	10	In the morning on the first day of the walk-out, you learn that three thousand students (or 13.04% of 23,000 possible students) have participated in the walk-out.
	11	In the morning on the first day of the walk-out, you learn that eight thousand one hundred students (or 35.22% of 23,000 possible students) have participated in the walk-out.
	12	In the morning on the first day of the walk-out, you learn that twenty-two thousand students (or 95.65% of 23,000 possible students) have participated in the walk-out.
<i>Success manipulation</i>	1	Blank
	2	Days after the student group formed, Seattle newspapers predicted that the student walk-out had a small chance of succeeding.
	3	Days after the student group formed, Seattle newspapers predicted that the student walk-out had a great chance of succeeding.
<i>Anger manipulation</i>	1	Blank
	2	Your friends have expressed disdain toward the University administration for their support of the tuition increase.

Note: 'Blank' indicates that a level for a particular dimension was not mentioned in a vignette.

Table A2. Two-Level Model of Anger toward Administration, Likelihood of Success, and Intention to Protest, Vignette-Level Mediation Model

	Anger toward Administration	Likelihood of Success	Intention to Protest
<i>Within Level: Vignettes</i>			
<i>Endogenous variables</i>			
Anger toward administration			0.107*** (0.014)
Likelihood of success			0.672*** (0.014)
<i>Vignette dimensions</i>			
Collective good			
1% tuition increase (omitted)			
10% tuition increase	14.411*** (0.711)	2.863*** (0.643)	1.559** (0.599)
50% tuition increase	28.703*** (0.858)	5.626*** (0.689)	4.320*** (0.712)
100% tuition increase	36.809*** (0.997)	8.341*** (0.711)	8.249*** (0.851)
Selective rewards			
No monetary incentive (omitted)			
\$10 incentive	-0.433 (0.565)	0.546 (0.605)	0.786 (0.553)
\$50 incentive	-0.284 (0.550)	0.324 (0.597)	1.109* (0.566)
\$200 incentive	0.126 (0.566)	2.059** (0.609)	4.113*** (0.598)
Selective punishments			
No penalty (omitted)			
Incomplete grades	2.868*** (0.567)	-3.643*** (0.678)	-7.921*** (0.658)
Failing grades	5.818*** (0.598)	-4.806*** (0.670)	-13.336*** (0.698)
Expulsion	7.126*** (0.650)	-5.298*** (0.686)	-14.094*** (0.753)
Social inducement			
No approached (omitted)			
Some friends	0.503 (0.402)	0.228 (0.434)	0.512 (0.404)
Reputation			
No reputation (omitted)			
Reputation	0.134 (0.405)	0.177 (0.423)	-0.082 (0.398)
Efficacy frame			
No efficacy (omitted)			
Efficacy	0.347 (0.386)	2.212*** (0.439)	0.503 (0.397)
Injustice frame			
No injustice (omitted)			
Injustice	10.427*** (0.526)	0.618 (0.433)	-0.122 (0.425)
Solidarity frame			
No solidarity (omitted)			
Solidarity	-0.762 (0.475)	1.343** (0.515)	-0.136 (0.486)
Moral solidarity	-0.442 (0.482)	1.217* (0.513)	-0.248 (0.511)
Number of participants	0.320* (0.126)	5.868*** (0.168)	0.048 (0.142)
Number of participants ²	-0.014* (0.006)	-0.159*** (0.007)	0.008 (0.006)
Success manipulation			
No news (omitted)			
Bad news	-0.047 (0.480)	-1.572** (0.527)	-0.488 (0.478)
Good news	0.546 (0.486)	3.551*** (0.542)	-0.398 (0.502)
Anger manipulation			
No anger (omitted)			
Friends' anger	0.631 (0.377)	-0.080 (0.445)	0.124 (0.403)
Constant	-3.158 (12.879)	18.721 (10.096)	-18.459 (9.497)
Individual-level variables	Yes	Yes	Yes
Vignette dummy variables	Yes	Yes	Yes
Intercept variance	214.232*** (12.604)	155.631*** (9.002)	101.791*** (7.452)
Residual variance	372.497*** (9.404)	446.506*** (10.501)	385.020*** (11.527)
Cov(Intercept _{success} , Intercept _{anger})		40.320*** (7.782)	
Cov(Residual _{success} , Residual _{anger})		40.136*** (5.717)	
Vignettes		10560	
Individuals		880	
Level-1 R^2	0.389	0.398	0.548

*** $p < .001$, ** $p < .01$, * $p < .05$

Notes: Maximum likelihood for missing data with robust standard errors used throughout. Values of the number of participants is divided by 1000 to ease model convergence.

Table A3. Two-Level Model of Anger toward Administration, Likelihood of Success, and Intention to Protest, Individual-Level Mediation Model

	Anger toward Administration	Likelihood of Success	Intention to Protest
<i>Between Level: Individuals</i>			
<i>Endogenous variables</i>			
Anger toward administration			0.089* (0.036)
Likelihood of success			-0.039 (0.042)
<i>Self and social identities</i>			
Student identity	0.764 (0.562)	0.050 (0.472)	-0.259 (0.353)
Student activist identity	0.861 (0.547)	-0.117 (0.430)	0.531 (3.81)
Self-efficacy	3.707* (1.636)	-0.111 (1.340)	-0.279 (1.240)
Values education	0.128 (0.364)	-0.067 (0.325)	-0.021 (0.272)
<i>Prior social and political action</i>			
Associational membership	0.121 (0.401)	-0.137 (0.356)	0.418 (0.307)
Volunteerism			
Does not volunteer (omitted)			
Volunteers	-0.645 (1.231)	0.782 (1.133)	-2.222* (0.900)
Prior activism	0.052 (0.438)	0.106 (0.405)	0.759* (0.351)
Protest coordination			
Never organized (omitted)			
Organized	0.897 (3.177)	4.155 (3.316)	0.806 (2.994)
<i>Structural grievances and protest attitudes</i>			
Structural grievances	1.584** (0.574)	0.578 (0.525)	-0.124 (0.413)
First mover: Student protest			
Follower (omitted)			
First mover	1.820 (2.105)	1.610 (2.032)	10.961*** (2.011)
Never mover	-1.388 (2.105)	-3.987* (1.794)	-8.828*** (1.607)
Protest goal-alignment	-0.078 (0.413)	-0.306 (0.394)	0.573* (0.292)
Protest despite failure	0.718 (0.408)	1.387*** (0.370)	2.694*** (0.366)
Protest effectiveness	0.269 (0.492)	1.452*** (0.405)	0.160 (0.330)
Protests justified	1.143** (0.391)	0.775* (0.352)	0.430 (0.287)
Protest norms	3.548*** (0.765)	-0.436 (0.712)	1.479* (0.591)
Attitudes toward tuition increases	-2.408*** (0.400)	-0.230 (0.362)	-0.556 (0.297)
<i>Preferences and beliefs</i>			
Generalized trust	0.052 (0.467)	0.074 (0.399)	0.072 (0.354)
Particularized trust	0.480 (0.500)	0.429 (0.489)	0.005 (0.444)
Social value orientations			
Proself: Individualist (omitted)			
Proself: Competitor	-4.751 (2.724)	-0.120 (2.284)	-0.825 (2.110)
Prosocial	-2.141 (1.459)	0.348 (1.305)	0.702 (1.085)
Unclassified	-4.055 (2.189)	2.042 (2.018)	1.612 (1.519)
Risk-taking	-0.332 (0.464)	0.481 (0.366)	0.764* (0.342)
Constant	-3.158 (12.879)	18.721 (10.096)	-18.459 (9.497)
Individual-level control variables	Yes	Yes	Yes
Vignette-level variables	Yes	Yes	Yes
Vignette dummy variables	Yes	Yes	Yes
Intercept variance	214.232*** (12.604)	155.631*** (9.002)	101.791*** (7.452)
Residual variance	372.497*** (9.404)	446.506*** (10.501)	385.020*** (11.527)
Cov(Intercept _{success} , Intercept _{anger})		40.320*** (7.782)	
Cov(Residual _{success} , Residual _{anger})		40.136*** (5.717)	
Vignettes		10560	
Individuals		880	
Level-2 R^2	0.177	0.236	0.645

*** $p < .001$, ** $p < .01$, * $p < .05$

Notes: Maximum likelihood for missing data with robust standard errors used throughout.

Supplemental Materials

Emotion, Efficacy, and Protest Intentions: Testing a Multilevel Dual Pathway Model of Collective Action

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1 Factorial Survey Experiment

1.1. Designing the factorial survey experiment

The validity of our FSE rests on the weak assumption that our hypothetical scenarios realistically depict protest situations relevant to the dual pathway model of collective action being tested. The scenarios may include a mix of factual and counterfactual conditions, but together respondents must perceive them as realistic. Relatedly, respondents must perceive the levels of the vignette dimensions and consider those levels in their responses. Given our experimental design, in which levels of vignette dimensions are manipulated (i.e., randomly assigned) by the researcher, the treatment manipulations must be “successful.” In addition, the manipulation of vignette levels and presentation to respondents must be consistent with how such levels would appear in actual protest situations. Finally, vignettes must minimize social desirability and other response effects.

To ensure that our FSE of student protest is realistic and that the manipulations worked in authentic ways, we designed initial hypothetical scenarios to be consistent with previous vignette studies of protest (e.g., Jasso and Opp 1997), and then modified the scenarios based on a series of focus groups, pretests, and a pilot study. First, we pre-tested an early draft of the survey using all members of the research team, and modified the survey to increase realism. Second, we administered the modified survey to a dozen social science graduate students, half of whom had expertise in survey methods, and conducted a post-survey focus group session to receive feedback and suggestions. After this illuminating session, we substantially revised our FSE. Third, we administered a pilot study where we asked respondents at the end of the survey to provide open-ended comments about their survey experience, including any concerns they had about the survey (see Author Blinded). After the pilot study, we again substantially revised our FSE. Finally, we explore a number of additional checks of invalidity in the forthcoming sections of the Supplemental Materials (e.g., attention checks, survey duration, etc.). The results, when taken together, bolster the validity of our FSE.

1.2. Tests of assumptions for unbiased causal inference

Temporal stability and causal transience. First, under two weak assumptions, the assessment of twelve vignettes per respondent allows us to estimate unit causal effects in a counterfactual framework and not merely treatment effects on the treated. The two assumptions—temporal stability and causal transience—state that potential outcomes are unaffected by the anticipation of treatments administered in the future (sometimes referred to as anticipation effects) and that potential outcomes in one period are unaffected by treatments administered in prior periods (sometimes referred to as spillover effects) (Holland 1986).

To test these assumptions, we estimated (a) two-level models in which each vignette dimension manipulated in the factorial survey experimented interacted with dummy variables for vignette number, and (b) two-level models with vignette dimensions and lagged $t - 1$ vignette dimensions (Hainmueller et al. 2014). We found that (a) joint tests of the interaction terms rejected the null hypothesis that the vignette dimensions were identical across the 12 vignettes (anger: $\chi^2(319) = 488.85, p < .001$; success: $\chi^2(319) = 372.04, p = .021$; intention: $\chi^2(319) = 383.60, p = .007$), and (b) joint tests of the lagged $t - 1$ vignette dimensions failed to reject the null hypothesis for anger toward administration ($\chi^2(29) = 24.08, p = .725$) and intention to protest ($\chi^2(29) = 39.53, p = .092$) but not likelihood of success ($\chi^2(29) = 59.71, p = .0007$).

While the findings appear to challenge the assumptions of temporal stability and casual transience, we discovered that the results were driven by the ratings produced in vignette 1. If we excluded vignette 1, joint tests of the interaction terms failed to reject the null hypothesis that the vignette dimensions were identical across the remaining 11 vignettes (anger: $\chi^2(290) = 312.84, p = .170$; success: $\chi^2(290) = 301.08, p = .314$; intention: $\chi^2(290) = 300.18, p = .328$). Furthermore, restricting our analysis to the first vignette assessed—even though we lose statistical power—generated substantive findings similar to those estimated from a two-level model that pooled vignettes 2 through 12.

Given the results, we argue that the assumptions of temporal stability and causal transience were likely maintained in the present study. But if the assumptions were in fact violated, we contend that the magnitude of this bias was negligible.

SUTVA. The stable unit treatment value assumption (SUTVA) states that unit A's potential outcome is solely a function of whether he or she receives the treatment; assignments or treatments applied to units B through Z have no effect on unit A's potential outcome (Rosenbaum 2007). A given outcome then is unaffected by treatments assigned to or received by other units. SUTVA, in other words, implies non-interference between units. This assumption is generally maintained in laboratory settings but should be assessed for experimental methods with less control, such as web-based survey experiments, field experiments, and natural experiments.

We first assess SUTVA by exploring the date in which respondents completed the study. The idea here is that respondents who completed the study at a later date could have been influenced by respondents who completed the study at an earlier date. Joint tests failed to reject the null hypothesis that dummy variables for the date in which respondents completed the study were jointly equal to zero for likelihood of success: $\chi^2(83) = 76.06, p = .692$) and intention to protest ($\chi^2(83) = 81.23, p = .534$) but not anger toward administration ($\chi^2(83) = 115.67, p < .05$); these results also imply an absence of history effects. Furthermore, joint tests failed to reject the null hypothesis that interaction terms between the vignette dimensions and a continuous measure of date of study completion were jointly equal to zero for anger toward administration ($\chi^2(29) = 37.10, p = .143$) and intention to protest ($\chi^2(29) = 32.53, p = .297$) but not likelihood of success ($\chi^2(29) = 50.10, p = .008$).

We also assess SUTVA by investigating the effects of shared IP addresses. Given that we conducted our experiment online, we have less control over communication between participants. Shared IP addresses for respondents serves as a way for participants to communicate with each other about different treatment assignments. Likelihood ratio tests comparing two- and three-level models clustering on respondent ID and IP address, respectively, failed to reject the null hypothesis that the ratio of likelihoods of nested models are equivalent (anger: $\chi^2(1) = 1.66, p = .197$; success: $\chi^2(1) = 0.000, p = 1.000$; intention: $\chi^2(1) = 0.000, p = 1.000$). Moreover, interacting a dummy variable for shared IP address (0 = not shared IP address, 1 = shared IP address) and each vignette dimension generated statistically non-significant joint tests (anger: $\chi^2(29) = 36.62, p = .156$; success: $\chi^2(29) = 19.37, p = .911$; intention: $\chi^2(29) = 29.17, p = .456$).

Taken together, the results suggest that SUTVA was likely maintained.

Conditional independence and ignorability. Conditional independence—sometimes referred to as ignorability—is the assumption that values assigned to treatment conditions are independent of potential outcomes (i.e., values taken by dependent variables), where “assigning values” describes the process by which treatments obtain their particular values. By manipulating and randomly assigning vignette dimensions to respondents, we satisfy the assumption of conditional independence as well as three other assumptions that underlie conditional independence: (1) treatment assignment is independent of potential outcomes and values of the dependent variables (i.e., no simultaneity or reflection problems), (2) treatment selection bias is absent, and (3) omitted variable bias is also absent.

To test the assumption of conditional independence we first conducted randomization checks. The goal was to look for unexpected correlations between experimental manipulations and observed covariates (e.g., gender). Our randomization checks consisted of regressing each vignette dimension—either logistic or multinomial logistic regression depending on the number of levels per dimension—on the individual-level covariates presented in the main document. We found that individual-level covariates with less than 1 percent item non-response were jointly insignificant (p -values $> .05$) for each vignette dimension, indicating support for the assumption of conditional independence and that the vignette dimensions were jointly balanced. The latter also suggests that the unit homogeneity assumption—all units with the same value of the treatment have the same potential outcome—is plausible (Morgan and Winship 2007).

Second, we tested the assumption of conditional independence with a classic Hausman specification test estimating fixed- and random-effects econometric models for panel data. With the Hausman test, we can evaluate the assumption that the vignette dimensions, X_{ij} , at level-1 are orthogonal to the individual-level (i.e., level-2) disturbance term, u_{0j} . Hausman tests failed to reject the null hypothesis of no systematic differences in coefficients for anger toward administration ($\chi^2(29) = 34.69, p = .214$), likelihood of success ($\chi^2(29) = 22.35, p = .805$), and intention to protest ($\chi^2(29) = 11.27, p = .998$). Correlations between X_{ij} and u_{0j} were negligible for anger toward administration ($r = -0.031$), likelihood of success ($r = 0.0009$), and intention to protest ($r = -0.005$), which further supports the assumption of conditional independence.

Monotonicity. The assumption of monotonicity states that there are no units that receive a treatment if assigned to the control group (Angrist et al. 1996); or, more generally, that respondents do not do the opposite of his or her assignment, no matter what the assignment. Angrist et al. (1996) classify four types of subjects: (1) compliers: subjects who take the treatment if assigned to the treatment condition; (2) never-takers: subjects who avoid the treatment if assigned to the treatment condition; (3) always-takers: subjects who take the treatment if assigned to the control condition; and (4) defiers: subjects who do the opposite of their assignment; they avoid the treatment if assigned to it, and they take the treatment if

assigned to the control group. Angrist et al. (1996) refer to never-takers, always-takers, and defiers jointly as non-compliers. The presence of non-compliers—and defiers in particular—can downwardly bias average treatment effects. In some experimental designs (e.g., natural experiments like the Vietnam War draft) and in most observational studies, the assumption of monotonicity is frequently violated. But, like classic laboratory experiments, our research design rules out non-compliers by making treatment unavailable to those in the control group and the control unavailable to those in the treatment group. Thus, our assumption of monotonicity is likely valid.

Measurement equivalence. Although not an explicit assumption of causal inference, we assess measurement equivalence for different types of survey takers. Measurement equivalence (or invariance) is the property of measurement which indicates that a construct (or treatment) is being measured the same across respondents (Vandenberg and Lance 2000). We performed a number of tests to assess whether different types of survey takers responded similarly to the vignette dimensions. First, we excluded, from the models, respondents who were 1.96 standard deviations above or below the mean of a survey duration variable (measured in total minutes). With these alternative models, we found that the estimates of vignette dimensions were substantively similar to model estimates in which all survey takers, regardless of duration to complete the survey, were included.

Second, our factorial survey experiment contained two T/F screener questions used to gauge the attention of respondents. We found that 9.79 percent of the sample failed one or both of the screener questions. Joint tests of interaction effects between the vignette dimensions and a dummy variable for the screener questions (0 = answered both screener questions correctly, 1 = answered one or both of the screener questions incorrectly) rejected the null hypothesis that the interaction effects were jointly equal to 0 (anger: $\chi^2(29) = 56.26, p < .01$; success: $\chi^2(29) = 102.70, p < .0001$; intention: $\chi^2(29) = 49.55, p = .01$). In spite of these results, only minor substantive differences were observed between models with and without respondents who failed one or both screener questions.

In short, although we discovered that treatment effects varied across different types of survey takers, we found that the size of this bias was negligible.

1.3. Assessment of errors of non-observation

One goal of survey methodology is to generate unbiased inferences of the target population, which is done by minimizing errors of non-observation (Groves et al. 2009). Errors of non-observation include coverage error (the statistical gap between the target population and the sampling frame), sampling error (the statistical gap between the sampling frame and the sample), and non-response error (the statistical gap between the sample and the respondent pool). Although not covered in this sub-section, another goal of survey methodology is to undermine errors of observation. These errors involve differences between reported and recorded values of a survey variable and a true value or score. Errors of observation include invalidity and unreliability, measurement error (that can stem from social desirability bias, intentional misreporting, etc.), and processing error.

Below, we review and address each of the possible errors of non-observation. First starting with coverage error.

Coverage error. Our target population of interest was freshmen, sophomore, and junior undergraduate students who were legal adults (age 18+) enrolled at the University of Washington (UW) during the AY 14-15. We restrict our target population to freshmen, sophomores, and juniors since we were uninterested in drawing inferences about undergraduate students with senior class standing. This was the case since graduating seniors were immune to the underlying grievance motivating the organization of the hypothetical student walk-out in the factorial survey experiment: tuition increases taking place the following academic year. Graduating seniors, in other words, were free from experiencing the hypothetical tuition increase.

After receiving IRB approval from the UW Human Subjects Division, the registrar's office provided us with a .csv file of all undergraduate students registered at the University of Washington and its satellite campuses for Autumn quarter of 2014. This list included first and last names, email addresses, class standing, and declared 1st and 2nd majors. Ideally, the sampling frame—the list of all elements in the target population that can be sampled—would completely overlap with the target population. Foreign elements (or ineligible units) and missing elements (or undercoverage) are two of the most common problems that prohibit convergence between the target population and sampling frame. For the present study, we dealt with foreign elements as follows. First, using information provided by the registrar's office, we excluded students with senior class standing as well as students from the satellite campuses (e.g., Bothell, Tacoma, etc.). This resulted in a sampling frame that consisted of 20,241 elements. Second, during the process of informed consent, we asked students whether they were legal adults, students at UW, and graduating during the AY 14-15. If students reported not being legal adults, not being a student at UW, and/or graduating during the AY 14-15, they were prohibited from participating in the study. Third, during the data collection process, we asked students to report their age and whether they planned on graduating during the AY 14-15. Those who reported being less than 18 years of age or planning to graduate during the AY 14-15 were excluded from the analysis. Regarding missing elements, or the problem of undercoverage, we admit that some students might not have been registered at UW during Autumn quarter of 2014 but were nonetheless matriculated students (e.g., on medical leave). We contend that these missing elements constitute a small portion of the target population. And, in all likelihood, the missing elements do not systematically differ from registered students, especially in relation to possible differences in survey responses and vignette ratings.

Taken together, we provide evidence and argumentation for overlap between the target population and sampling frame; suggesting negligible coverage error in the present study. As a final note, while coverage error is minor, our target population of interest prohibits inferences to (a) other populations of

undergraduate students proximal to (e.g., Western Washington University) and distal from (e.g., Oxford University) the University of Washington, and (b) senior-standing undergraduate students. In other words, generalizations from our sample are limited to freshmen, sophomore, and junior undergraduate students at the University of Washington.

Sampling error. We randomly sampled 3000 elements from the 20,241 possible elements in the sampling frame. As noted earlier, sampling error is the statistical gap between the sampling frame and the sample. Sampling error occurs when elements of the sampling frame do not have an equal probability of selection into the sample, and when key subgroups of the sampling frame are not represented in the sample. To reduce sampling error, we used a simple random sample that provided each element of the sampling frame an equal probability of selection into the sample. Furthermore, we used observed variables found in the sampling frame to check the representativeness of our sample. In Table S1, we present the distributions of observed variables found in the sampling frame, sample, factorial survey experiment respondent pool, and follow-up survey respondent pool. We can see that the simple random sample produced similar distributions of class standing and declared major between the sampling frame and sample, with an average difference of 0.56. In short, the evidence suggests that our simple random sample yielded minimal sampling error.

Non-response error. Non-response error is the statistical gap between the sample and respondent pool. The statistical gap, more specifically, is the product of the response rate *and* differences between respondent and non-respondent variables of interest (e.g., difference in means). Response rates alone, in other words, are not quality indicators of non-response error. According to the survey methodology literature, there are two types of non-response: unit non-response and item non-response. Unit non-response (or “total failure”) obtains when a sampled unit refuses to cooperate with the survey request. Item non-response (“partial failure”, “missing data”), in contrast, occurs when a sampled unit refuses to answer one or more survey items. In the present sub-section, we discuss unit non-response, and reserve the discussion of item non-response for the main document.

We address the degree of non-response error by presenting evidence of (a) the response rate, and (b) the extent to which the respondent pool of the factorial survey experiment differs from the sample. Again, to accomplish the latter, we rely on the observed variables provided in our sampling frame. First, the CASRO response rate was 31.67 percent and the AARPO cooperation rate was 64.99 percent. While our CASRO response rate was moderate-to-low, it is relatively high for a web-based survey (Manfreda et al. 2008). Second, according to distributions of the variables provided in our sampling frame (i.e., class standing and declared major), unit non-response did not produce an unrepresentative respondent pool despite our 31.67 percent response rate (see Table S1). In fact, the respondent pool of our factorial survey experiment yielded distributions of class standing and declared major similar to distributions observed in our sample, with an average difference of 0.94. Furthermore, a logit model regressing respondent pool status (0 = element in sample, 1 = element in respondent pool) on dummy variables for class standing and declared major yielded statistically non-significant joint tests for class standing ($\chi^2(2) = 4.78, p = .091$) and declared major ($\chi^2(95) = 82.64, p = .813$).

In short, the evidence suggests that unit non-response produced a negligible gap between the sample and the respondent pool of the factorial survey experiment.

Other considerations. In this sub-section, we address design issues unique to factorial survey experiments that may—if present—bias treatment effects. Factorial survey methodologists recommend that respondents rate no more than ten vignettes where each vignette contains no more than ten manipulated vignette dimensions (Auspurg and Hinz 2015; Auspurg and Jäckle 2017; Robbins and Kiser 2018). This “10 and 10” rule undermines order effects (i.e., bias that results from the order in which a dimension is presented in a vignette), reduces respondent boredom and fatigue effects, and facilitates

unbiased evaluation of vignettes for respondents who are of higher age (> 60 years old), lower educational background, and little familiarity with the topic under evaluation. While our research design skirts the boundaries of these recommendations (i.e., twelve evaluated vignettes where each vignette contained eleven manipulated vignette dimensions), we justify our design choices as follows: (1) the quasi-log scale used for our number of participants dimension required 12 levels (and, hence, 12 vignettes to rate) in order to investigate within-individual ratings of small-, medium-, and large-scale social protest; (2) selection of the remaining vignette dimensions was informed by general theory (e.g., dual pathway models, theories of the critical mass, etc.); (3) decision-making in real-life collective action situations requires individuals to consider multiple pieces of information (e.g., size of the protest, monetary incentives, social incentives, collective action frames, etc.), which is captured by our research design; and (4) less complex pilot studies and pretests manipulating similarly operationalized vignette dimensions generated comparable treatments effects.

Table S1. Distribution of observed variables in sampling frame, sample, factorial survey experiment, and follow-up survey

Variables	Sampling Frame	Sample	FSE Respondent Pool	FUS Respondent Pool
Class standing				
Freshman	31.8 percent	33.2 percent	32.6 percent	38 percent
Sophomore	26.4 percent	26.2 percent	29.4 percent	28.2 percent
Junior	41.8 percent	40.6 percent	38 percent	33.8 percent
Declared major				
Pre sciences	19.5 percent	19.3 percent	20.2 percent	20.4 percent
Pre major	12.8 percent	14.1 percent	13.6 percent	14.8 percent
Pre engineering	10.3 percent	9.8 percent	9.9 percent	9.3 percent
Pre social sciences	8.3 percent	8.2 percent	8.7 percent	9.7 percent
Computer science	1.1 percent	1.2 percent	1 percent	1.2 percent
Electrical engineering	1 percent	0.9 percent	0.6 percent	0.5 percent
Other major	47 percent	46.5 percent	46 percent	44.1 percent
N	20,241	3000	880	432

Note: FSE = factorial survey experiment. FUS = follow-up survey.

2 Analytic Strategy

2.1. Multilevel structural equation model

The research design yields person-vignette panel data in which i vignettes ($i = 1, \dots, 12$) are nested within j individuals ($j = 880$). To test our model specified in Figure 1, we estimate a two-level structural equation model (SEM) where anger toward administration and likelihood of success mediate the effects of individual *and* vignette characteristics on intention to protest (see Figure S1). The first level models within-person variation by regressing anger toward administration, likelihood of success, and intention to protest on vignette characteristics. If we let N_{ij} be our observed measure of anger toward administration for vignette i and individual j , our first-level model for anger toward administration takes the following form:

$$\text{Anger toward administration: } N_{ij} = \alpha_j^N + X_{ij}\alpha + \epsilon_{ij}, \quad (1)$$

where N_{ij} is a continuous measure of anger toward administration, α_j^N is a random intercept term capturing unobserved heterogeneity varying across individuals but not vignettes, α is a (non-random) coefficient vector, X_{ij} is a vector of vignette characteristics varying across both individuals and vignettes, and ϵ_{ij} is a disturbance term (assumed normal and *iid*). This defines our first level model of anger toward administration, which is a between-vignette, within-individual model.

Similarly, if we let M_{ij} be our observed measure of likelihood of success for vignette i and individual j , our first-level model for likelihood of success takes the following form:

$$\text{Likelihood of success: } M_{ij} = \alpha_j^M + X_{ij}\gamma + \delta_{ij}, \quad (2)$$

where M_{ij} is a continuous measure of likelihood of success, α_j^M is a random intercept term capturing unobserved heterogeneity varying across individuals but not vignettes, γ is a (non-random) coefficient vector, X_{ij} is a vector of vignette characteristics varying across both individuals and vignettes, and δ_{ij} is a disturbance term (assumed normal and *iid*). To control for unobserved common causes, we covary the disturbance terms of anger toward administration and likelihood of success (see Figure S1), yielding $\text{cov}(\epsilon_{ij}, \delta_{ij}) \neq 0$. This defines our first level model of likelihood of success.

In parallel with Eq. 1 and 2, if we let Y_{ij} be our observed measure of intention to protest for vignette i and individual j , our first-level model for intention to protest takes the following form:

$$\text{Intention to protest: } Y_{ij} = \alpha_j^Y + N_{ij}\theta + M_{ij}\beta + X_{ij}\phi + \pi_{ij}, \quad (3)$$

where Y_{ij} is a continuous measure of intention to protest, α_j^Y is a random intercept term capturing unobserved heterogeneity varying across individuals but not vignettes, ϕ is a (non-random) coefficient vector, X_{ij} is a vector of vignette characteristics varying across both individuals and vignettes, and π_{ij} is a disturbance term (assumed normal and *iid*). This defines our first level model of intention to protest.

We can decompose the total effects of our vignette characteristics, X_{ij} , on intention to protest, Y_{ij} , into direct and indirect effects through anger toward administration, N_{ij} , and likelihood of success, M_{ij} , by substituting Eq. 1 and 2 into Eq. 3., which yields the following intention to protest reduced-form equation:

$$Y_{ij} = \alpha_j^Y + \alpha_j^N \theta + \alpha_j^M \beta + X_{ij}(\alpha \theta + \gamma \beta + \phi) + \epsilon_{ij} \theta + \delta_{ij} \beta + \pi_{ij}, \quad (4)$$

where $\alpha_j^T = \alpha_j^Y + \alpha_j^N \theta + \alpha_j^M \beta$ is the total effect of intercepts, $\eta = \alpha \theta + \gamma \beta + \phi$ is the total effect of vignette characteristics, and $\varphi_{ij} = \epsilon_{ij} \theta + \delta_{ij} \beta + \pi_{ij}$ is the reduced-form disturbance term. Following convention, we first test for the total effect, η , and then decompose it into direct and indirect effects. The hypothesis of total mediation by N_{ij} or M_{ij} is the test of $\phi = 0$. We use the delta method to obtain asymptotic standard errors for indirect and total effects (Sobel 1982).

We specify second-level models of between-person variation in anger toward administration, likelihood of success, and intention to protest by modeling our random intercepts as follows (see Figure S1):

$$\text{Anger toward administration: } \alpha_j^N = \alpha^N + Z_j A + \tau_j, \quad (5)$$

$$\text{Likelihood of success: } \alpha_j^M = \alpha^M + Z_j \Gamma + u_j, \quad (6)$$

$$\text{Intention to protest: } \alpha_j^Y = \alpha^Y + \alpha_j^N \Theta + \alpha_j^M B + Z_j \Phi + \omega_j, \quad (7)$$

where α_j^N , α_j^M , and α_j^Y are random intercept terms from above capturing individual variation in anger toward administration, likelihood of success, and intention to protest, respectively, α^N , α^M , and α^Y are non-random intercepts, Z_j is a vector of individual-level variables, A , Γ , Θ , B , and Φ are non-random coefficients, τ_j , u_j , and ω_j are disturbance terms (assumed normal and *iid*). To control for unobserved common causes, we covary the second-level disturbance terms of anger toward administration and likelihood of success (see Figure S1), yielding $\text{cov}(\tau_j, u_j) \neq 0$. Our second-level models estimate effects of individual-level covariates on variation in anger toward administration, likelihood of success, and intention to protest across individuals.¹ Thus, A and Γ represent the effects of individual characteristics, Z_j , on anger toward administration and likelihood of success, respectively, and Φ provides a similar effect on intention to protest (Figure S1). Similarly, Θ and B represent the effects of individual feelings of anger and individual judgments of success (averaged across vignettes), respectively, on individual intention to protest (averaged across vignettes).² By substituting equations 5 and 6 into equation 7, we can decompose our individual-level effects into direct and indirect effects through anger and success (averaged across vignettes).

Strength of causal inference. Our level-1 and level-2 equations imply that the parameters of our model vary in how close they approximate causal effects. The most important parameter are the total effects of vignette characteristics on anger toward administration (equation 1), likelihood of success (equation 2), and intention to protest $\eta = \alpha \theta + \gamma \beta + \phi$ (equation 4). Our FSE design affects the properties of estimates of α , γ , and η : because we randomly assigned levels of vignette dimensions to vignettes, we can safely assume that our level-1 independent variables (i.e., vignette characteristics), X_{ij} , are orthogonal to each other (increasing statistical power of tests of α , γ , and η by reducing multicollinearity) and to the disturbance terms ϵ_{ij} and δ_{ij} (reducing bias in estimates of α , γ , and η) (see Figure S1). Moreover, because we also randomly assigned vignettes to individuals, we can safely assume that X_{ij} are orthogonal to α_j^N (equation 1), α_j^M (equation 2), and α_j^T (equation 4), which increases efficiency of estimates of α , γ , and η (equation 4). These design features eliminate bias due to the effects of observed and unobserved

¹ To estimate contextual effects in classic multilevel models, individuals (level-1) are nested within groups (level-2). For us, vignettes (level-1), or the “contextual effects” in our models, are nested within individuals (level-2).

² The aggregate effect of, for example, anger toward administration on intention to protest is the sum of the level-1 and level-2 coefficients ($\theta + \Theta$).

variables (or heterogeneity) across individuals, including self-selection bias. This implies that we can interpret α , γ , and η as causal effects under relatively weak assumptions, such as temporal stability and causal transience (Holland 1986; Morgan and Winship 2007). We explore the validity of these assumptions in the Supplemental Materials online.

This does not hold for the structural form of intention to protest (equation 3). Because N_{ij} and M_{ij} were neither randomly assigned nor manipulated, but instead are endogenous regressors, N_{ij} and M_{ij} could conceivably be correlated with the disturbance term π_{ij} . Under these conditions, we can only assume that θ and β are causal if (a) the disturbance terms ϵ_{ij} and δ_{ij} are uncorrelated with π_{ij} , or (b) X_{ij} captures all relevant confounding variables at the within-individual level (conditional independence assumption). To undermine potential confounding, our multilevel structural equation model decomposes the effects of anger toward administration and likelihood success into within- *and* between-individual components with the inclusion of α_j^N and α_j^M in equation 7. Therefore, θ and β are within-person effects akin to coefficients in fixed-effects panel models (Allison 2009). If the assumption of conditional independence is violated, bias would arise under reverse causality—if Y_{ij} feeds back on N_{ij} or M_{ij} —or if a potential within-person confounder that covaried with the (temporal) process of assessing vignettes was also correlated with N_{ij} and Y_{ij} (or M_{ij} and Y_{ij}). We argue that the assumption of conditional independence is weak in our case since (a) a recent meta-analysis comparing observational studies to experimental studies suggests that the reflection problem is largely absent from the empirical literature testing dual pathway models of collective action (van Zomeren et al. 2008), and (b) potential within-person confounders are few and likely exert minimal bias. Taken together, the reduced form (total) effects ($\alpha\theta + \gamma\beta + \phi$) of X_{ij} are causal effects, but the separated direct (ϕ) and indirect effects ($\alpha\theta$ and $\gamma\beta$) are not causal, and, instead, should be interpreted as within-individual conditional associations (Holland 1988).

Unlike our equations at the within-individual level, equations 5, 6, and 7 at the between-individual level neither identify causal effects nor completely control for unobserved heterogeneity across individuals. Therefore, we must rely on the assumption, typically made in observational research, that the disturbance terms τ_j , u_j , and ω_j are uncorrelated with Z_j once we include key level-2 control variables in the model. Since this is a strong assumption, we interpret individual-level coefficients as conditional associations and not as causal effects.

Figure S1. Path Diagram of a Two-Level Structural Equation Model of Intention to Protest

3 Results: Factorial Survey Experiment

3.1. Instrumental variable analysis

Table S2. Two-Level Model of Anger toward Administration, Likelihood of Success, and Intention to Protest, Vignette-Level Mediation Model with IV Estimation

	Anger toward Administration	Likelihood of Success	Intention to Protest
<i>Within Level: Vignettes</i>			
<i>Endogenous variables</i>			
Anger toward administration			0.108*** (0.018)
Likelihood of success			0.677*** (0.099)
<i>Vignette dimensions</i>			
<i>Collective good</i>			
1% tuition increase (omitted)			
10% tuition increase	14.400*** (0.712)	2.859*** (0.643)	1.564** (0.601)
50% tuition increase	28.693*** (0.858)	5.623*** (0.689)	4.327*** (0.731)
100% tuition increase	36.794*** (0.997)	8.337*** (0.711)	8.279*** (0.924)
<i>Selective rewards</i>			
No monetary incentive (omitted)			
\$10 incentive	-0.447 (0.565)	0.543 (0.605)	0.789 (0.558)
\$50 incentive	-0.293 (0.550)	0.320 (0.597)	1.117* (0.567)
\$200 incentive	0.108 (0.566)	2.054** (0.609)	4.125*** (0.633)
<i>Selective punishments</i>			
No penalty (omitted)			
Incomplete grades	2.844*** (0.567)	-3.648*** (0.678)	-7.943*** (0.778)
Failing grades	5.808*** (0.598)	-4.810*** (0.670)	-13.365*** (0.875)
Expulsion	7.113*** (0.651)	-5.303*** (0.686)	-14.120*** (0.968)
<i>Social inducement</i>			
No approached (omitted)			
Some friends	0.499 (0.402)	0.227 (0.434)	0.522 (0.404)
<i>Reputation</i>			
No reputation (omitted)			
Reputation	0.128 (0.405)	0.175 (0.423)	-0.086 (0.399)
<i>Efficacy frame</i>			
No efficacy (omitted)			
Efficacy	0.339 (0.386)	2.210*** (0.439)	0.519 (0.458)
<i>Injustice frame</i>			
No injustice (omitted)			
Injustice	10.420*** (0.526)	0.616 (0.433)	-0.122 (0.428)
<i>Solidarity frame</i>			
No solidarity (omitted)			
Solidarity	-0.756 (0.476)	1.342** (0.515)	-0.128 (0.502)
Moral solidarity	-0.442 (0.482)	1.215* (0.513)	-0.240 (0.522)
Number of participants	0.320* (0.126)	5.868*** (0.168)	0.076 (0.598)
Number of participants ²	-0.014* (0.006)	-0.159*** (0.007)	0.007 (0.017)
<i>Success manipulation</i>			
No news (omitted)			
Bad news		-1.567** (0.533)	
Good news		3.492*** (0.546)	
<i>Anger manipulation</i>			
No anger (omitted)			
Friends' anger	0.631 (0.377)	-0.080 (0.445)	0.122 (0.403)
Constant	-2.904 (12.885)	18.772 (10.098)	-18.761* (9.449)
Individual-level variables	Yes	Yes	Yes
Vignette dummy variables	Yes	Yes	Yes
Intercept variance	214.283*** (12.607)	155.637*** (9.002)	101.836*** (7.454)
Residual variance	372.563*** (9.398)	446.504*** (10.501)	385.061*** (11.478)
Cov(Intercept _{success} , Intercept _{anger})		40.346*** (7.784)	
Cov(Residual _{success} , Residual _{anger})		40.134*** (5.717)	
Cov(Residual _{success} , Residual _{intention})		1.985 (44.740)	
Vignettes		10560	
Individuals		880	
Level-1 R ²	0.389	0.398	0.548

*** $p < .001$, ** $p < .01$, * $p < .05$

Notes: Maximum likelihood for missing data with robust standard errors used throughout. Values of the number of participants is divided by 1000 to ease model convergence. The success manipulation is used as an instrumental variable for likelihood of success.

Table S3. Total, Direct, and Indirect Effects of Vignette Dimensions on Intention to Protest with IV Estimation

	Total (SE)	Direct (SE)	Indirect _{anger} (SE)	Indirect _{success} (SE)
Collective good				
1% tuition increase (omitted)				
10% tuition increase	5.025*** (0.738)	1.564** (0.601)	1.552*** (0.271)	1.908*** (0.512)
50% tuition increase	11.172*** (0.835)	4.327*** (0.731)	3.092*** (0.529)	3.753*** (0.703)
100% tuition increase	17.808*** (0.955)	8.279*** (0.924)	3.965*** (0.676)	5.564*** (0.933)
Selective rewards				
No monetary incentive (omitted)				
\$10 incentive	1.103 (0.703)	0.789 (0.558)	-0.048 (0.062)	0.362 (0.410)
\$50 incentive	1.299 (0.694)	1.117* (0.567)	-0.032 (0.059)	0.214 (0.400)
\$200 incentive	5.508*** (0.727)	4.125*** (0.633)	0.012 (0.061)	1.371** (0.461)
Selective punishments				
No penalty (omitted)				
Incomplete grades	-10.071*** (0.808)	-7.943*** (0.778)	0.307*** (0.077)	-2.435*** (0.573)
Failing grades	-15.950*** (0.857)	-13.365*** (0.875)	0.626*** (0.118)	-3.210*** (0.650)
Expulsion	-16.893*** (0.917)	-14.120*** (0.968)	0.767*** (0.139)	-3.539*** (0.686)
Efficacy frame				
No efficacy (omitted)				
Efficacy	2.031*** (0.507)	0.519 (0.458)	0.037 (0.042)	1.475*** (0.368)
Injustice frame				
No injustice (omitted)				
Injustice	1.412** (0.515)	-0.122 (0.428)	1.123*** (0.196)	0.411 (0.299)
Solidarity frame				
No solidarity (omitted)				
Solidarity	0.686 (0.594)	-0.128 (0.502)	-0.081 (0.054)	0.896* (0.365)
Moral solidarity	0.523 (0.627)	-0.240 (0.522)	-0.048 (0.053)	0.811* (0.357)
Number of participants	4.027*** (0.179)	0.048 (0.142)	0.035* (0.015)	3.916*** (0.600)
Number of participants ²	-0.101*** (0.008)	0.008 (0.006)	-0.001* (0.001)	-0.106*** (0.017)

*** $p < .001$, ** $p < .01$, * $p < .05$

Notes : Maximum likelihood for missing data with robust standard errors used throughout. Values of the number of participants is divided by 1000 to ease model convergence. The success manipulation is used as an instrumental variable for likelihood of success.

4 Follow-Up Survey

4.1. Survey items of political action

Have you written a letter expressing a point of view to an editor or to a politician during the last six months?

Approximately how many times during the last six months have you written a letter to an editor or to a politician?
Type in number of times: _____

Have you signed a protest letter or petition during the last six months?

Approximately how many times during the last six months have you signed a protest letter or petition?
Type in number of times: _____

Have you hung a political poster or sign on a wall during the last six months?

Approximately how many times during the last six months have you hung a political poster or sign on a wall?
Type in number of times: _____

Have you painted a political slogan on a wall during the last six months?

Approximately how many times during the last six months have you painted a political slogan on a wall?
Type in number of times: _____

Have you joined a social or political advocacy group during the last six months?

Approximately how many times during the last six months have you joined a social or political advocacy group?
Type in number of times: _____

Have you attended a political rally during the last six months?

Approximately how many times during the last six months have you attended a political rally?
Type in number of times: _____

Have you participated in a demonstration or a protest march during the last six months?

Approximately how many times during the last six months have you participated in a demonstration or a protest march?
Type in number of times: _____

Have you taken part in a job action or a strike during the last six months?

Approximately how many times during the last six months have you taken part in a job action or a strike?
Type in number of times: _____

Have you occupied a building or a factory in protest during the last six months?

Approximately how many times during the last six months have you occupied a building or a factory in protest?
Type in number of times: _____

5 Results: Follow-Up Survey

5.1. Administration of the follow-up survey and predictive validity of intention to protest

Dual pathway models of collective action frequently explain intention to protest, which should be a key predictor of actual protest behavior. We conducted a follow-up study to determine whether intention to protest predicts actual protest behavior in the future.

Six months after administering the FSE, 432 respondents completed an online follow-up survey, yielding a response rate of 49 percent (further details about sampling error and nonresponse error can be found in subsection 5.2). As before, respondents read an informed consent document and were asked to voluntarily consent to participate. Respondents were then asked whether and how often they participated in nine different types of political action during the past six months. After providing self-reports of political action, respondents filled out a survey questionnaire, were shown a debriefing statement, thanked for their participation, and provided \$10USD for completing the follow-up survey. The median survey length of the follow-up survey was approximately 10.43 minutes.

With this data, we created four political behavior variables. First, we created two binary variables: one, *demonstration*, consists of whether a respondent—during the last six months—had participated in a demonstration or protest march ($N = 432$, $M = 0.07$, $SD = 0.25$), while the other, *political action*, is a binary index of whether a respondent had (i) written a letter expressing a point of view to an editor or a politician, (ii) signed a protest letter or petition, (iii) hung a political poster or sign on a wall, (iv) painted a political slogan on a wall, (v) joined a social or political advocacy group, (vi) attended a meeting or rally about politics, (vii) participated in a demonstration or protest march, (viii) taken part in a job action or strike, or (ix) occupied a building or a factory in protest during the past six months ($N = 432$, $M = 0.44$, $SD = 0.49$). Second, we constructed two count variables: one, *demonstrations*, consists of how many demonstrations or protest marches a respondent participated in during the last six months ($N = 432$, $M = 0.12$, $SD = 0.64$, $\text{Min} = 0$, $\text{Max} = 10$), while the other, *political actions*, is a count index of the total number of political actions across (i) through (ix) that a respondent participated in during the last six months ($N = 432$, $M = 2.04$, $SD = 7.43$, $\text{Min} = 0$, $\text{Max} = 112$).

To predict the probability of future political behaviors, we estimated individual-specific mean values of intention to protest ($N = 432$, $M = 36.55$, $SD = 20.54$, $\text{Min} = 0$, $\text{Max} = 100$). We hypothesize that if our FSE has predictive validity, then individual-specific mean intentions to protest will correlate with self-reports of actual political behaviors occurring six months after the FSE. With logit and negative binomial regression models, we find that intention to protest is significantly associated with actual protest behavior (see Figure S2). For instance, a one-unit increase in individual-specific mean intentions is associated with a 3 percent increase in the odds of participating in a demonstration or protest march six months after the FSE (see model 1, Table S4).

Overall, our analysis of predictive validity is best interpreted as a lower bound given that (1) the situations depicted in our FSE depart from real situations confronted by students, (2) our measures of political behavior are heterogeneous and include behaviors (e.g., signing a petition) not represented by the hypothetical scenario specified in our FSE, and (3) political opportunities and knowledge of opportunities in the real world drive political behavior. In spite of this lower bound, we observe strong support for the predictive validity for our FSE.

Table S4. Logistic Regression and Negative Binomial Regression Models of Self-Reports of Political Action

	Binary Outcomes		Count Outcomes	
	Demonstration	Political Action	Demonstrations	Political Actions
	Model 1	Model 2	Model 3	Model 4
Mean intentions	.03** (.01)	.03*** (.01)	.03*** (.01)	.03*** (.01)
Constant	-3.63*** (.48)	-1.15*** (.21)	-3.32*** (.45)	-.34 (.24)
Alpha			9.73	3.32
Controls	No	No	No	No
N	432	432	432	432
Wald χ^2	(1) = 7.45	(1) = 25.24	(1) = 14.68	(1) = 20.65
<i>p</i> -value	.006	.000	.000	.000
Pseudo-R ²	.04	.05	.03	.02

*** $p < .001$, ** $p < .01$, * $p < .05$, † $p < .10$

Note : log-odds (robust standard errors).

Figure S2. Plots of predicted probabilities and predicted counts

Note: (A) Predicted probabilities and 95% CIs of participating in a demonstration as a function of individual-specific mean intentions ($N = 432$). (B) Predicted probabilities and 95% CIs of participating in any form of political action as a function of individual-specific mean intentions ($N = 432$). (C) Predicted counts and 95% CIs of the number of demonstrations as a function of individual-specific mean intentions ($N = 432$). (D) Predicted counts and 95% CIs of the number of political actions as a function of individual-specific mean intentions ($N = 432$). Estimates derived from Table S4

5.2. Unit non-response

Building from subsection 1.3, we marshal four pieces of evidence detailing the magnitude of non-response error in the follow-up survey. We finish subsection 5.2 by discussing and providing a solution.

First, the follow-up survey observed some attrition, as evidenced by the 49.09 percent response rate ($N = 432$). Second, as shown in Table S1, distributions of class standing and declared major differed between the respondent pools of the factorial survey experiment and the follow-up survey. We found an average difference of 1.6 between the observed variables. Furthermore, a logit model regressing respondent pool status (0 = element in respondent pool of factorial survey experiment, 1 = element in respondent pool of follow-up survey) on dummy variables for class standing and declared major yielded statistically significant joint tests for class standing ($\chi^2(2) = 9.00, p = .011$) but not declared major ($\chi^2(52) = 29.15, p = .995$). Third, we investigated whether propensity for unit non-response in the follow-up survey was correlated with sociodemographic variables measured in the factorial survey experiment. Since most sociodemographic variables exhibit some item non-response, we restricted our analysis to variables with less than 1 percent item non-response. We estimated a logit model regressing propensity for non-response (0 = element in respondent pool of factorial survey experiment, 1 = element in respondent pool of follow-up survey) on sociodemographic variables and found statistically and marginally significant joint tests for gender ($\chi^2(2) = 7.41, p < .05$), race ($\chi^2(4) = 7.74, p < .10$), level of education ($\chi^2(2) = 8.50, p < .10$), state residency ($\chi^2(2) = 6.12, p < .05$), and log survey duration in minutes (log-odds = .488, Robust $SE = .181, p < .001$). Fourth, we found that a variable measured during the factorial survey experiment (prior activism), which is conceptually related to the dependent variables measured at the time of our follow-up survey, is statistically unrelated to propensity for unit non-response in the follow-up survey (log-odds = .012, Robust $SE = .044, p = .774$).

Taken together, the evidence suggests that non-response error is present in the follow-up survey. The propensity for unit non-response, however, is likely not a function of the dependent variable, which suggests that non-response error has a common cause or is “missing at random” (Groves et al. 2009). To handle this problem, we estimated alternative models that controlled for gender, race, level of education, etc. The results of these models can be found in Table S5, while plots of the predicted probabilities and predicted counts can be found in Figure S3. As shown, the results are not sensitive to controls that are correlated with propensity for unit non-response in the follow-up survey.

Table S5. Logistic Regression and Negative Binomial Regression Models of Self-Reports of Political Action with Control Variables

	Binary Outcomes		Count Outcomes	
	Demonstration	Political Action	Demonstrations	Political Actions
	Model 1	Model 2	Model 3	Model 4
Mean intentions	.02* (.01)	.03*** (.01)	.02** (.01)	.02*** (.01)
Constant	-4.27* (1.86)	-2.59** (.98)	-3.39* (1.57)	-1.81* (.85)
Alpha			4.48	2.75
Controls	Yes	Yes	Yes	Yes
N	424	429	429	429
Wald χ^2	(11) = 20.23	(13) = 51.22	(13) = 872.33	(13) = 45.77
<i>p</i> -value	.04	.000	.000	.000
Pseudo-R ²	.08	.09	.10	.05

*** $p < .001$, ** $p < .01$, * $p < .05$, † $p < .10$

Note : log-odds (robust standard errors). Control variables include gender, race, level of education, state residency, and survey duration.

Figure S3. Plots of Predicted Probabilities and Predicted Counts

Note: (A) Predicted probabilities and 95% CIs of participating in a demonstration as a function of individual-specific mean intentions and control variables ($N = 424$). (B) Predicted probabilities and 95% CIs of participating in any form of political action as a function of individual-specific mean intentions and control variables ($N = 429$). (C) Predicted counts and 95% CIs of the number of demonstrations as a function of individual-specific mean intentions and control variables ($N = 429$). (D) Predicted counts and 95% CIs of the number of political actions as a function of individual-specific mean intentions and control variables ($N = 429$). For panels A through D, predicted probabilities and predicted counts are estimated while holding the control variables at their mean. Estimates derived from Table S5.

6 References

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7 Appendix

5.1 Survey items used in the factorial survey experiment

First mover: Student Protest

Given the 12 situations you have just seen, would you ever be the first person to participate in a student walk-out?

Yes, I would be the first person to participate in a student walk-out

No, I would not be the first person to participate in a student walk-out

No, I would never participate in a student walk-out

Don't know

Prefer not to say

["No, I would not be the first person..."]

How many other people would need to participate in a student walk-out before you would as well?

[numerical box]

Protest goal-alignment, protest despite failure, protest effectiveness, protests justified, and attitudes toward tuition increases

To what degree do you agree with the following statements?

If I participate in a social protest, I am more concerned about the goal of the protest than the act of protesting itself.

I would participate in a social protest or walk-out even if the likelihood of achieving its goal was low.

Social protests, strikes, and walk-outs are an effective tool for social and political change.

It is justified to take part in a disruptive protest if it seems like the authorities don't respond to peoples' grievances.

It is justified for legislators to raise tuition at public universities when state finances are under strain.

Strongly disagree	1
	2
	3
	4
	5
	6
Strongly agree	7
Don't know	88

Protest norms

To what extent do you agree with the following statements?

If a student is dissatisfied with their university, he or she has a duty to do something about it.

I think it is wrong to do anything against tuition increases. This should be left to university administrators and state legislators.

If I were not active against tuition increases, I would have a bad conscience.

Strongly disagree	1
	2
	3
	4
	5
	6
Strongly agree	7
Don't know	88

Prior activism

Have you ever done any of the following activities? Check all that apply.

- Written a letter expressing a point of view to an editor or to a politician
- Signed a protest letter or petition
- Hung a political poster or sign on a wall
- Painted a political slogan on a wall
- Joined a social or political advocacy group
- Attended a meeting or rally about politics
- Participated in a demonstration or protest march
- Taken part in a job action or strike
- Occupied a building or factory in protest
- None of the above
- Prefer not to say

Protest coordination

Have you ever organized or coordinated a demonstration, social protest, or walk-out?

- Yes
- No
- Don't know

Associational membership

Have you ever belonged to one of the following kinds of organizations? Check all that apply.

- Student organization or club
- Fraternity or sorority
- Academic honor society or academic club
- Organized sports team
- Community group or neighborhood association
- Religious congregation or religious organization
- Labor union or professional association
- Political organization
- Other organization
- I have never belonged to any organization
- Prefer not to say

Volunteerism

Do you spend time volunteering for clubs, social services, or charities during the week?

- Yes
- No
- Don't know
- Prefer not to say

Student identity

To what extent do you agree with the following statements?

I like to be seen as a member of the University of Washington student community.

I feel connected to other students at the University of Washington.

I like to be seen as a student of the University of Washington.

- Strongly disagree 1
- 2
- 3
- 4

	5
	6
Strongly agree	7
Don't know	88

Student activist identity

To help you understand the next questions, please read the following broad definition of student activism:

“The goal of student activism is to advocate educational, curricular, or financial change at one’s school, university, or college, where the means of student activism can vary greatly (e.g., from hanging posters on a wall to organizing sit-ins).

Please indicate how strongly you agree or disagree with each of the following statements.

Being a student activist is central to who I am.

I identify myself as a student activist.

People who know me well would call me a student activist.

Being a student activist is an important reflection of who I am.

Strongly disagree	1
	2
	3
	4
	5
	6
Strongly agree	7
Don't know	88

Values education

Which of the following aspects of a university education do you value most? (Circle one)

A broad liberal arts education	1
Exposure to new ideas and new kinds of people	2
Preparation for a specific career	3
General preparation for employment	4
An enjoyable and exciting college experience	5
The opportunity to make friends and future associates	6
Preparation to be an informed citizen	7
None of the above	8
Prefer not to say	99

Structural grievances

To what extent do you agree with the following statements?

The University of Washington makes sure that I get what I need.

The outcomes of decisions that the University of Washington makes are fair to me.

The University of Washington treats me in a fair way.

The University of Washington respects students like me.

Strongly disagree	1
	2
	3
	4
	5
	6
Strongly agree	7

Don't know

88

Self-efficacy

For each of the following statements, please check the choice that is closest to how true you think it is for you. The questions ask about your opinion. There are no right or wrong answers.

I can always manage to solve difficult problems if I try hard enough.

If someone opposes me, I can find the means and ways to get what I want.

It is easy for me to stick to my aims and accomplish my goals.

I am confident that I could deal efficiently with unexpected events.

Thanks to my resourcefulness, I know how to handle unforeseen situations.

I can solve most problems if I invest the necessary effort.

I can remain calm when facing difficulties because I can rely on my coping abilities.

When I am confronted with a problem, I can usually find several solutions.

If I am in trouble, I can usually think of a solution.

I can usually handle whatever comes my way.

Not at all true	1
Hardly true	2
Moderately true	3
Exactly true	4

Generalized trust and particularized trust

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?

You family	
Your neighbors	
Your friends	
People you know personally	
Strangers	
People you meet for the first time	
People of another religion.	
Complete distrust	0
	1
	2
	3
	4
Neither trust nor distrust	5
	6
	7
	8
	9
Complete trust	10
Don't know	88

Social value orientations

In this next task we ask you to imagine that you have been randomly paired with another person, whom we will refer to simply as the "Other." This other person is someone you do not know and that you will not knowingly meet in the future. Both you and the "Other" person will be making choices by circling either the letter A, B, or C. Your own choices will produce points for both yourself and the "Other" person. Likewise, the other's choice will produce points for him/her and for you. Every point has value:

the more points you receive, the better for you, and the more points the "Other" receives, the better for him/her.

Here's an example of how this task works:

	A	B	C
You get	500	500	550
Other gets	100	500	300

In this example, if you chose A you would receive 500 points and the other would receive 100 points; if you chose B, you would receive 500 points and the other 500; and if you chose C, you would receive 550 points and the other 300. So, you see that your choice influences both the number of points you receive and the number of points the other receives.

Before you begin making choices, please keep in mind that there are no right or wrong answers—choose the option that you, for whatever reason, prefer most. Also, remember that the points have value. The more of them you accumulate, the better for you. Likewise, from the other's point of view, the more points s/he accumulates, the better for him/her.

For each of the nine choice situations, circle A, B, or C, depending on which column you prefer most:

	A	B	C
(1) You get	480	540	480
Other gets	80	280	480

	A	B	C
(2) You get	560	500	500
Other gets	300	500	100

	A	B	C
(3) You get	520	520	580
Other gets	520	120	320

	A	B	C
(4) You get	500	560	490
Other gets	100	300	490

	A	B	C
(5) You get	560	500	490
Other gets	300	500	90

	A	B	C
(6) You get	500	500	570
Other gets	500	100	300

	A	B	C
(7) You get	510	560	510
Other gets	510	300	110

	A	B	C
--	---	---	---

(8) You get	550	500	500
Other gets	300	100	500

	A	B	C
(9) You get	480	490	540
Other gets	100	490	300

Risk-taking

Are you, generally speaking, a person who avoids taking risks, or a person who is fully prepared to take risks?

Avoid taking risks	1
	2
	3
	4
	5
	6
Fully prepared to take risks	7
Don't know	88

Sociodemographic variables

Age

How old are you? (In years)

Years: _____

Race

Are you of Hispanic, Latino, or Spanish origin? (Please select at most one answer)

- No
- Yes, Mexican, Mexican American, Chicano
- Yes, Puerto Rican
- Yes, Cuban
- Yes, Central American
- Yes, South American
- Yes, some other Hispanic, Latino, or Spanish origin
- Don't know
- Prefer not to say

Please indicate what race(s) you consider yourself to be. (Check all that apply)

- White
- Black or African American
- American Indian or Alaska Native
- Asian Indian
- Chinese
- Filipino
- Japanese
- Korean
- Vietnamese
- Other Asian
- Native Hawaiian
- Guamanian or Chamorro
- Samoan
- Other Pacific Islander

Some other race
Don't know
Prefer not to say

Gender

What is your gender? (Circle one)

Male 1
Female 2
Other 3
Prefer not to say 99

Marital status

Are you currently married, widowed, divorced, separated, or never married? (Please select at most one answer)

Married 1
Widowed 2
Divorced 3
Separated 4
Never married 5
None of the above 6
Prefer not to say 99

Level of education

What is the highest educational level that you have attained? (Circle one)

HS DIPLOMA or the equivalent (GED) 1
1 year of college 2
2 years of college 3
3 years of college 4
4 years of college 5
5 years or more of college 6
Don't know 88

Major

In what general area does your first major or intended major field of study fall?

Arts & Humanities
Engineering & Information Sciences
Natural & Health Sciences
Professional Studies
Social Sciences
Don't know

Specifically, what is your major or intended major in Arts & Humanities?

Arts (Visual)
Classics
English
Foreign Language
Graphic Design / Digital Arts
History
Music
Performing Arts
Philosophy

Other

Specifically, what is your major or intended major in Engineering & Information Sciences?

Computer Science

Engineering

Information Science & Technology

Other

Specifically, what is your major or intended major in Natural & Health Sciences?

Biology

Mathematics

Statistics

Environmental Studies

Physics

Nursing

Other

Public Health

Chemistry

Specifically, what is your major or intended major in Professional Studies?

Accounting

Architecture

Business

Education

Landscape Architecture

Library Science

Regional Planning

Social Work

Urban Design

Other

Specifically, what is your major or intended major in Social Sciences?

Anthropology

Communication

Criminal Justice / Socio-Legal Studies

Economics

Ethnic Studies

International Studies

Political Science / Government

Psychology

Sociology

Women's Studies / Gender Studies

Other

Economic dependence

Are you considered financially independent for purposes of financial aid?

Yes

No

Don't know

Prefer not to say

Residency

For the purposes of determining tuition, does the University consider you an in-state, out-of-state, or international student?

- In-state
- Out-of-state
- International
- Don't know

Parent's income

What is your best estimate of your parents' total income last year? Consider income from all sources before taxes.

- Less than \$10,000
- \$10,000 to \$14,999
- \$15,000 to \$19,999
- \$20,000 to \$24,999
- \$25,000 to \$29,999
- \$30,000 to \$39,999
- \$40,000 to \$49,999
- \$50,000 to \$59,999
- \$60,000 to \$74,999
- \$75,000 to \$99,999
- \$100,000 to \$149,999
- \$150,000 to \$199,999
- \$200,000 to \$249,999
- \$250,000 or more
- Don't know
- Prefer not to say

Living arrangements

Do you currently live in your parents' household?

- Yes
- No
- Not sure

Employment status

Are you currently working for pay?

- Yes
- No
- Prefer not to say

Care status

Do you have responsibilities to take care of children or other family members?

- Yes
- No
- Prefer not to say

Political orientation

Generally speaking, do you usually think of yourself as a Republican, Democrat, Independent, or something else? Choose one of the following answers

- Strong Republican 1
- Moderate Republican 2

Leaning Republican	3
Undecided or independent	4
Leaning Democrat	5
Moderate Democrat	6
Strong Democrat	7
Other Party	8
Don't Know	88
Prefer not to say	99