How Protesting Depends on Peers: U.S. Students in the 1960s

Michael Biggs[*]

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There is an extensive literature on how individual voters are influenced by the actions and attitudes of others in their social network or geographical area. Comparable analyses of social movements are lacking. This paper uses a panel of freshmen entering American colleges in 1967, who were sampled again in 1970. Freshmen were asked to estimate their own chance of protesting. This provides a comprehensive control variable at the individual level, and is aggregated at the college level to measure social influence. From analysis of 11,355 students, the probability of protest during college increased markedly with the proportion of their peers who had anticipated that they would protest, controlling for over thirty variables pertaining to the individual, college, and state. This effect is plausibly attributed to social interaction within college, rather than selection into certain colleges by individuals already inclined to protest; the effect is strong even for students who had expected not to protest. Students protested, in part, because their peers did. Although most variation among colleges can be explained by the characteristics of entering freshmen, initial differences were amplified by social influence.

The literatures on social movements and on electoral politics rarely communicate. The separation is surprising given that one influential school emphasizes the continuity between "contentious politics" and institutionalized politics (McAdam, Tarrow, and Tilly 2001). The divergence reflects the trend towards academic specialization, but is compounded by differing theoretical predispositions and empirical sources. Voting is analyzed with sample surveys, whereas movements are investigated by looking at organizations or events. Useful data on individual participation in protest are scarce, because population surveys are unsuitable. Voting is theorized as an individual decision, whereas movements are treated as collective. The field of social movements was founded on the proposition that movements emerge from pre-existing networks and groups (Freeman 1973; Oberschall 1973; Tilly 1978), and scholars remain reluctant to consider individual action (an exception is Opp 2009). This tendency to view mobilization as something that happens at the group level is curious given that movements, aside from the labor movement, involve only a small minority of the group that they represent. We typically say, for example, that black college students in the American South protested against racial oppression in 1960; it is easily forgotten that most of them did not.[1] Therefore the question of who participates—what explains variation among individuals in similar social positions—cannot be ignored.

Analysis of individual participation does not imply that decisions are egocentric. Quite the reverse: only by dissolving collectivities into individuals does it become possible to investigate how people influence each other. Lazarsfeld pioneered the panel survey to understand how voters change their attitudes under the influence of friends and workmates (e.g. Berelson, Lazarsfeld, and McPhee 1954). This sociological research agenda was gradually ceded to political scientists (Bond et al. 2012; Klofstad 2011; Nickerson 2008; Sinclair 2012; Zuckerman ed. 2005). Comparable research on social movements has been hindered by two methodological problems. One-off surveys cannot distinguish the causes of protest from its effects, because participation in protest often reconfigures social relationships. In addition, surveys typically "measure" social context by eliciting the respondent's subjective perception—whether someone asked them to participate (e.g. Snow, Zurcher, and Olson 1980), or how many of their friends approve (e.g. Finkel and Muller 1998), or how many people they expect to protest (e.g. Klandermans 1984). It is impossible to tell whether protest actually depends on encouragement

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[1] One in four of these undergraduates had joined sit-in demonstrations by 1962 (Matthews and Prothro 1966: 412)

by others, or whether perceptions are molded to justify action. Few studies objectively measure social influence prior to participation (e.g. McAdam and Paulsen 1993), or provide multilevel analysis of individuals and their social contexts (Dixon and Roscigno 2003).

To investigate how an individual's involvement in protest depends on other people, this paper constructs a panel of American students. Freshmen were surveyed as they entered college in 1967, and they were sampled again at the beginning of 1970. This cohort had progressed through high school as the student movement emerged, spearheaded by black students in the South. Berkeley's Free Speech Movement in 1964 had attacked the university administration as a microcosm of American society, ousting the Chancellor. The escalating war in Vietnam had brought Students for a Democratic Society (SDS) into the national media spotlight. From 1967, just as this cohort entered college, protest accelerated. Many campuses were disrupted, with notable occupations and strikes at the University of Wisconsin in October 1967, Columbia University in April 1968, and at San Francisco State from November 1968 onwards. From SDS emerged the revolutionary Weather Underground, which organized the spectacular "Days of Rage" in October 1969. Only a tiny minority embraced revolution; more typical were those who joined the huge anti-war demonstration in Washington, D.C. in November 1969, which attracted two hundred thousand participants (McPhail and McCarthy 2004: 13). By 1970, almost half of the sample had protested.[2]

Aside from illuminating an historically important era, the panel used here is exceptional for providing comprehensive information on individuals *before* they interacted with their college peers and *before* they protested. My dependent variable is whether the student had protested since entering college. The focal independent variable is the proportion of other freshmen in college in 1967 who thought there was some chance that they would protest. The effect is estimated from 11,355 students in 171 colleges, controlling for over thirty individual, institutional, and geographical characteristics—including the freshman's own anticipated chance of protest.

The proportion of other freshmen who anticipated protesting did indeed have a strong positive effect on the student's probability of protest. Moving from the 10th percentile to the 90th percentile, the odds of protest almost doubled. This effect is plausibly interpreted as genuine social influence, after accounting for the initial similarity of students admitted to each college.

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[2] Participants-turned-scholars provide valuable insight into the student movement (Freeman 2004; Gitlin 1980).

The effect holds for the subset of freshmen who expected not to protest: those of them who attended a college with many potential protesters had not sought an opportunity for political activism. Conversely, there was no effect for students who switched college, and so did not experience sustained interaction with their fellow freshmen. Aside from identifying social influence at the individual level, this multilevel analysis also parses variation in protest among colleges. The largest portion reflected initial differences in the students admitted to each college. Social influence nevertheless amplified these initial differences, widening the gap between activist and quiescent campuses. Students were influenced by their peers rather than by faculty, and the presence of SDS had no apparent effect.

1. Social influence

One of the foundational propositions in the field of social movements is that "contentious politics … never mobilize without some significant grounding in ties created by previous contention and/or routine social life" (McAdam, Tarrow, and Tilly 2001: 58).[3] From this very general proposition can be derived two testable hypotheses, which are distinct though not antithetical. The first hypothesis, here called *social density*, can be found in Marx and Engels' analysis of class formation. A group of similarly situated people will be more likely to mobilize (all else being equal) if they interact intensively with each other. Conversely, they will be less likely to mobilize to the extent that they interact with those who exercise power over them. This hypothesis is persuasively illustrated by two elegant studies, one of variation over space and the other of change over time. In totalitarian China, students in Beijing could protest because they resided in tightly packed dormitories, sequestered in autonomous campuses, which clustered in a single district of the city (Zhao 1998). In British India, Bengali regiments tended to mutiny during or immediately after Hindu or Muslim festivals, because they provided occasions for sepoys to interact (Rao 2012).

The second hypothesis will be referred to as *social influence*. It is implicit in the research of Lazarsfeld and colleagues on electoral choice (e.g. Berelson, Lazarsfeld, and McPhee 1954). An individual will be more likely to mobilize if they are connected socially—as friends, neighbors, coworkers, and so on—with others who have mobilized, and less likely if they are connected socially with others who have not. Action is therefore positively correlated among individuals

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[3] This proposition seems hard to falsify, though there is some discordant evidence (Jasper and Poulsen 1995; Useem 1985).

who interact with each other. Note that positive correlation may predict opposite outcomes: if your friends protest, then you are likely to join them; if your friends do not, then you are unlikely to protest. Under this hypothesis, therefore, a greater density of social interaction will not necessarily increase mobilization, it just amplifies the similarity of connected individuals; this could either discourage or encourage protest. Note that social influence refers to similarity among connected individuals that is caused neither by their own individual characteristics nor by their common environment. What exactly constitutes social connection will vary, but it is worth distinguishing this from media representation. Many studies show the diffusion of protest across cities and countries after people read about protest elsewhere in newspapers or watch it on television (Andrews and Biggs 2006; Biggs 2013; Myers 2000). This kind of mediated diffusion is not considered here.

Another significant distinction is between the first-order social connections arising from the structures and routines of ordinary life, and the second-order connections fostered by movement organizations (Tilly 2008: 8–9).[4] For students of social movements, the former provide a terminal explanation, whereas the latter cannot. If we argue that workers struck because they belonged to a trade union, then we still need to explain the existence of that organization and their membership in it. If we argue that workers struck because they belonged to an isolated community dominated by a single occupation (Kerr and Siegel 1954[5]), then we have reached our stopping point—we hand over to an economist to explain industrial location. This paper focuses on social influence arising from ordinary life. Likewise, it compares participants to non-participants, rather than comparing degrees of commitment among participants.

Social influence implies that mobilization will cluster in space and time. In the Paris Commune, resistance by volunteer battalions was positively correlated according to the residence of recruits (Gould 1991). Strikes in the nineteenth century clustered in waves; the greater the number of workers who struck on one day, the more likely others were to strike on that day or the next (Biggs 2005). Analysis at this aggregate level is inherently limited, because it does not explain why some individuals participate while others do not. At the individual level, social influence has been detected in four ways.

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[4] The deliberate efforts of activists to connect different social groups—brokerage—is not considered here, though of course it is important (Han 2009; Hedstrom, Sandell, and Stern 2000).

[5] Kerr presided over the University of California during Berkeley's Free Speech Movement, and was ultimately fired for leniency towards protesters.

One is to ask about the attitudes of the respondent's significant others. In East Germany, people who reported having more friends who were critical of the Communist regime were more likely to join the demonstrations that toppled it in 1989 (Opp and Gern 1993). Establishing causation, however, is problematic. Individuals prefer to associate with those who are like them, and so it could be that those individuals who would be inclined to protest anyway associate with each other.[6] A panel survey in West Germany found that the attitudes of significant others had no effect on protest two years later (Finkel and Muller 1998). Using the respondent's perception of others' attitudes is also problematic. Some electoral surveys ask respondents to identify people with whom they discuss politics, and then sample those discussants. An individual's perception of their discussant's presidential vote often diverges from the discussant's actual vote; perception is biased in complex ways, depending on the local context (Huckfeldt and Sprague 1987). There is no comparable research on protest, though one study finds that protesters severely underestimated the negative attitudes of their opponents (Biggs 2006).

Another method of detecting social influence is to ask participants how they became involved. Most often they were asked by someone whom they already knew (Snow, Zurcher, and Olson 1980). In the United States, being asked to protest multiplies the odds eleven-fold (Schussman and Soule 2005: 1090); you are more likely to accede to a request from someone who shares organizational membership (Lim 2008: 972).[7] The same problem of causation arises. People who are trying to recruit others naturally target those who they expect to assent (Brady, Schlozman, and Verba 1999). In addition, it is not clear that people who refuse such a request will remember it and will report it honestly in a survey (Wallis and Bruce 1982).

A third method of detecting social influence is to ask the respondent how many others are expected to participate. Dutch trade unionists were more willing to join a campaign for shorter hours if they thought that many others in their plant would do so (Klandermans 1984: 592). By contrast, the expected number of friends and acquaintances going to a demonstration against cruise missiles in the Netherlands did not affect willingness to participate (Klandermans and Oegema 1987: 528). The problem with using subjective expectations about others is that they are

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[6] When these individuals are followed over four waves, reported protest does indeed increase subsequent "social incentives"—a scale that includes critical friends (Opp and Kittel 2010: 107).

[7] If an individual who belongs to a *movement* organization is asked to protest by another member, then both are already movement participants—this is not relevant for explaining the difference between participants and non-participants. Unfortunately the question refers to "someone who belongs to an organization you belong to" and does not query what type of organization (Brady, Schlozman, and Verba 1999: 165).

inevitably influenced by the individual's own inclination to participate. This problem is overcome in Dixon and Roscigno's (2003) exemplary multilevel analysis of a strike by service workers at Ohio State University. The probability of a worker going on strike increased markedly with the proportion of strikers in their work unit, controlling for twelve characteristics of the worker and unit. As soon as the proportion exceeded a small minority, the worker was more likely to strike than not. This study is the most convincing demonstration of social influence on protest at the individual level.

The strongest evidence for social influence is elicited by experimental manipulation, as used in recent studies of electoral politics. Individuals were more likely to vote when someone else in their household had been urged to vote (Nickerson 2008), or when Facebook notified them that their friends had voted (Bond et al. 2012). One quasi-experiment touches on social movements. Klofstad (2011) exploits the random assignment of roommates at the University of Wisconsin to identify the effect of being paired with a student who discusses politics. Such discussion increased involvement in organizations (including movement organizations), but there was no detectable effect on political participation (including protest). What is interesting is that students were unaware of social influence, understanding their actions as purely self-determined.

Robust identification of social influence poses four empirical challenges. First, individuals must be located within meaningful social contexts, such as workplaces or friendship networks. Second, the potential influence of other individuals within that context should be measured objectively. Third, social influence should be measured prior to its putative effect. Panel surveys demonstrate that participation in protest profoundly affects an individual's attitudes and social networks (Finkel and Muller 1998; Opp and Kittel 2010; Roy and Converse 1990). Ascertaining the direction of causation from a survey conducted retrospectively—after the action has taken place—is impossible. Fourth, even with proper temporal ordering, it is crucial to distinguish influence from selection. We choose, to some extent, our own social context; we prefer to interact with people who have similar characteristics to our own. A feminist, for example, naturally befriends other feminists. If she participates along with her friends in a pro-choice demonstration, we cannot necessarily conclude that she protested because her friends did. Measuring the influence of her friends requires taking into account the similarities that originally brought them together.

2. Protest by students

The upsurge in student protest in the 1960s naturally attracted the interest of social scientists. They deployed new techniques of quantitative analysis to focus either on individuals or on institutions. At the individual level, Matthews and Prothro (1966) pioneered the first sample survey to compare protesters with non-protesters, in black colleges in the South. Most subsequent surveys were conducted within a single institution (e.g. Blackstone, Gales, Hadley, and Lewis 1970; Flacks 1967; Mankoff and Flacks 1971; Wood 1974—an exception is Kahn and Bowers 1970). The Youth-Parent Socialization Panel (YPSP) sampled students at high school in 1965, with a second wave in 1973 (Kent and Niemi 1981). Of 1,300 respondents in the second wave, 900 attended college and thus experienced the peak of campus protest. These various surveys converge on similar findings (an incisive early review is Flacks 1970). Protesters tended to have relatively privileged parents, with views on the left of the political spectrum; they tended to be academically successful, and to study social sciences or the humanities. These findings are mainly derived from bivariate and trivariate tabulations, though YPSP has been reanalyzed with multivariate techniques (Paulsen 1991; Sherkat and Blocker 1994).

Analysis of students who volunteered for Freedom Summer in 1964 requires separate discussion, because it considers the transition from ordinary participation in a movement to extreme commitment (McAdam 1988).[8] Information from 800 applications to the Freedom Summer project is used to predict whether students subsequently took part or withdrew. Students were more likely to commit to the project if they belonged to an organization and expressed a consonant identity in their written application—for example, being affiliated with a church and describing their motivation with reference to Christianity (McAdam and Paulsen 1993: 660–1). The applications have the virtue of longitudinal data, capturing identity some months prior to the outcome. Unfortunately, they lack information on many characteristics identified in the literature on student protest, such as parental class and academic achievement.[9]

Systematic research on institutions began with surveys asking administrators about the incidence of protest on campus (Bayer and Astin 1969; Blau and Slaughter 1971; Hodgkinson 1970; Peterson 1970; Scott and El-Assal 1969). This tradition continues with analyses of the variation of protest or mobilization across colleges (Soule 1997; Van Dyke 1998; Van Dyke,

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[8] Three quarters of the applicants were already involved in the movement for Civil Rights (McAdam 1986: 82).

[9] McAdam (1988) ingeniously uses the average income of the census tract where the applicant or applicant's parents resided, but this estimate is not used in subsequent multivariate analysis.

Dixon, and Carlon 2007).[10] From the 1960s to the 1990s, protest was most likely in institutions with more students, that imposed higher admission standards, and that had a history of campus activism. Private non-denominational colleges seem to have been most prone to protest. It is impossible to ascertain whether these patterns reflect social processes occurring within the institution or simply the characteristics of students who attended. Only one multivariate study examines protest at the level of individuals and institutions: a reanalysis of protest by black Southern students (Biggs 2006), which is constrained by a small sample (under 300 students).

3. Data and method

The American Council on Education first surveyed freshmen in 1966, and this became an annual survey. Colleges and universities were selected from across the spectrum of higher education. (I will refer generically to colleges.) Institutions administered the survey "during the fall registration or campus orientation period—before the student had started classes, but after he had matriculated" (Panos, Astin, and Creager 1967: 1). In 1969, the Carnegie Commission's National Survey of Higher Education selected 189 institutions that had participated in the freshmen surveys.[11] It excluded any that "had achieved a response rate of less than 85% or had distributed questionnaires in a manner which was questionable (e.g., distribution at voluntary freshman orientation meetings)" (Carnegie 1972: 13). Students from the cohorts of 1966, 1967, 1968, and 1969 were sampled in this second wave. The questionnaire was mailed to the student's parents' address just before Christmas 1969. Those who did not reply were prompted again by postcard.

My analysis is confined to the 1967 cohort because that year's freshman survey included the key question on anticipated protest, and there is an adequate length of time—two and a third years—between the two waves. Two-year colleges are omitted, because their students had a different trajectory; most would have graduated in the summer of 1969 and then either left higher education or switched to a four-year institution. The main analysis is further restricted to students who were still attending the same college in 1970 (those who left will be analyzed separately).[12]

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[10] Related dependent variables are studied at the institutional level too, such as coalitions (Van Dyke 2003) and outcomes (Rojas 2006).

[11] The American Council of Education conducted its own follow-up surveys which would be equally valuable (Astin et al. 1975: 48–50). My enquiries to the Higher Education Research Institute (at the University of California at Los Angeles) and to Alexander Astin failed to locate these data.

[12] None of the freshmen who had entered San Luis Rey College and who responded in the second wave remained there; this institution is ommitted. One other exclusion should be noted: the small number of freshmen in 1967 who were transferring from another college.

The dataset for the second wave (Trow et al. 1978) omits some variables from the first wave, and provides only minimal information on institutions in order to conceal their identity. Therefore I match the second wave to the original dataset for the entire freshman survey (HERI and ACE 2001), as detailed in Online Appendix S1.[13] Conjoining the datasets makes it feasible, with considerable detective work, to identify the actual institutions and thus add institutional variables. Most importantly, it enables compositional variables to be derived from almost all the freshman who entered these institutions (126,589 students), rather than estimated from the much smaller subset who were chosen for the second wave and then responded. Unfortunately the attrition in the second wave was substantial. Out of the students who had entered four-year institutions in 1967 and were sent questionnaires in the second wave, only 40% mailed them back. This response rate is worse than the contemporaneous YSPS (81% between 1967 and 1973), but comparable to other panel surveys used to analyze protest (39%–57% in Finkel and Muller 1998; Opp and Kittel 2010; Roy and Converse 1990). This issue is discussed in Online Appendix S2. My sample comprises 12,899 students. Missing values reduce the estimation sample to 11,355.

The dependent variable is protest by the second wave. Students were asked a battery of questions on whether they had participated in "demonstrations or protests … since entering college." I construct an indicator for whether the student had protested against "U.S. military policy," "existing ethnic or racial policies," or "administrative policies of a college"—either on their own campus or elsewhere.[14] Of my sample, 45% of students had protested.[15] The proportion ranged from 4% at West Point to 88% at Bennington College. Not surprisingly, the Vietnam war was the most prominent issue. The least important was race, as white students by this time were reluctant to encroach on the black struggle for liberation (Peterson 1970: 66). Protesters most often participated on their own campus (95% had protested on campus, 36% elsewhere).

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[13] If the paper is published, the Stata programs to perform the matching and replicate the analysis will be included as supplementary information.

[14] A fourth issue was identified, protest "against college demonstrators." Only 3% of students responded affirmatively. An example would be the students at Columbia who beseiged the protesters who had occupied the President's office, preventing the passage of food and reinforcements (Trimberger 1968: 39). This question is not included in my protest indicator. The indicator is coded missing if there was no valid response to all eight questions.

[15] This proportion is not weighted to represent the national population of students. Weights are discussed in Online Appendix S3.

In predicting protest by 1970, multiple levels of analysis are required: individuals are nested within 171 colleges, in turn nested within 42 states. The probability of student i in college c and state s protesting, p_{ics}, is estimated as

$$\ln\left(\frac{p_{ics}}{1-p_{ics}}\right) = \alpha + \sum \beta_k^{(1)} X_{kics} + \sum \beta_l^{(2)} Y_{lcs} + \sum \beta_m^{(3)} Z_{ms} + \zeta_{cs}^{(2)}$$

where X_k, Y_l, Z_m are independent variables at the individual, institutional, and state levels, with coefficients $\beta^{(1)}$, $\beta^{(2)}$, and $\beta^{(3)}$ to be estimated. Any clustering of students within colleges that remains unexplained by independent variables is captured by the random intercept ζ, drawn from the normal distribution with mean zero; its variance ψ is to be estimated.[16]

The crucial independent variables pertain to students' expectations of protest, as recorded in the first wave. Freshmen were asked for "your best guess as to the chances that you will" do various things in college; one was "participate in student protests or demonstrations." This question captures the sum of influences on the student's predisposition to protest—insofar as they entered the student's awareness—*before* the student was immersed in the college environment. These influences include the college's reputation as known to the student (from media or friends and family). Figure 1 compares anticipated protest with actual protest, two and a third years later. Students predicted their future behavior fairly well. Out of those who had not expected to protest, one in four confounded their prediction. At the other extreme, out of those who thought that the chance was very good, one in five had not protested—or at least not yet. Anticipated protest will be used in two ways. It yields a compositional variable at the institutional level: the proportion of other freshman in the same college who anticipated some chance or a very good chance that they would protest. Besides the compositional variable, the student's anticipated protest will be entered as an independent variable at the individual level. This will provide the most comprehensive control for everything predisposing towards protest before the student experienced life at college. There was significant variation within institutions: every college had some freshmen who expected not to protest, and all but three had some who anticipated a very good chance.

At the individual level, three dozen variables represent 21 distinct characteristics. They are not listed here to conserve space, but will be presented in Table 1. (Appendix Tables A1 and A2 provide detailed definitions and summary statistics.) Because almost all these characteristics are

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[16] A random intercept at the state level is redundant because there are, on average, only four colleges per state. If it is added, it is statistically significant only in Model 2.

Figure 1. Association between anticipated and actual protest

As freshman, 1967: anticipated chance of protest
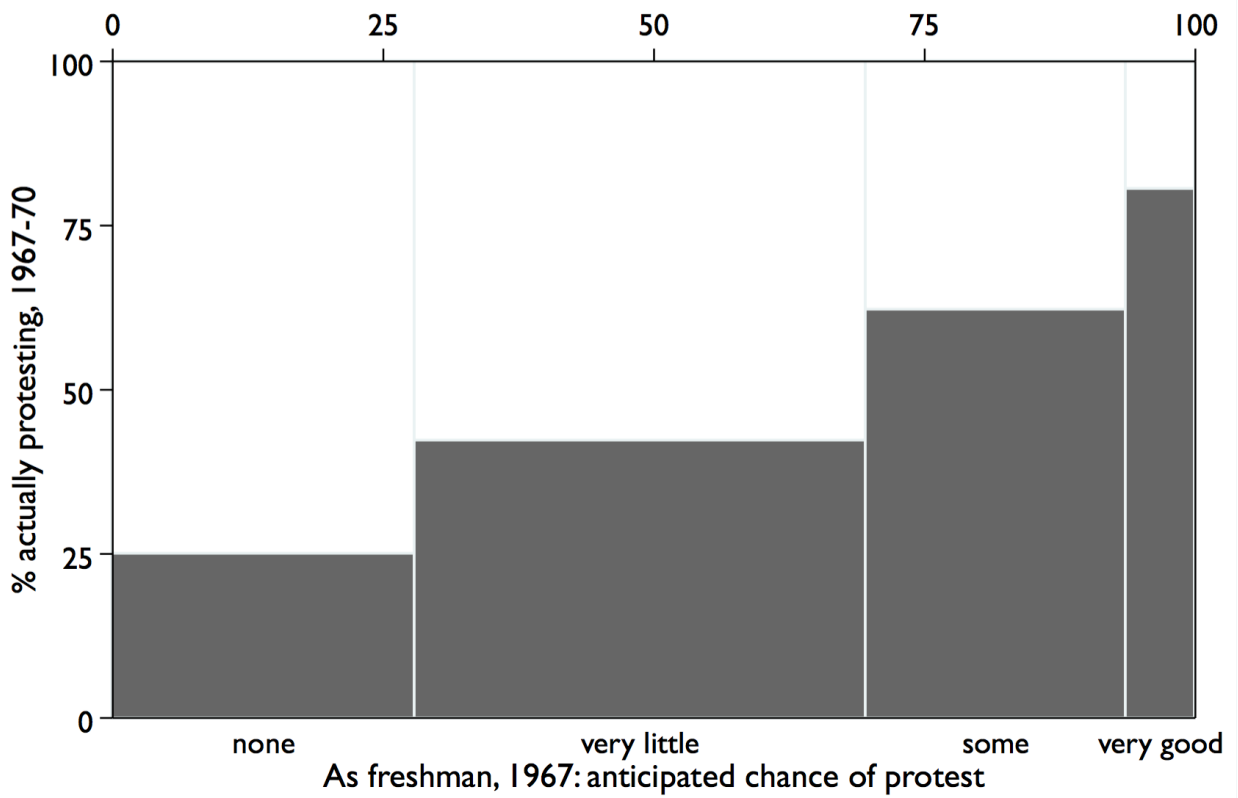
taken from the first wave, attitudes and experiences genuinely precede protest during college. Field of study, for example, is taken from "probable major" as declared by freshmen. An important control variable is prior protest. In a list of things that might have been done in the past year in school, one item was "participated in organized demonstrations." The response was either frequently, occasionally, or not at all. I combine the first two categories to construct an indicator.[17]

The freshman survey was extraordinarily comprehensive, with one exception: it did not ask about left-right political orientation. The closest was a question on whether "college officials have the right to ban persons with extreme views from speaking on campus." Disagreement serves to capture political affinity with the student movement, given the prominence of Berkeley's Free Speech Movement. Because student activists tended to be the children of liberal parents, it is worth taking parental "political leaning"—ranging from "strongly conservative" to "left"—from the second wave. This requires two caveats. The students' parents could have changed their views between 1967 and 1970, though political views tend to stabilize in middle age (e.g. Alwin, Cohen, and Newcomb 1992). In addition, students who became more radical could have reappraised their parents' views: a freshman who judged her parents as "liberal" in 1967 might have viewed them as "middle of the road" by 1970. Any bias would tend to reduce the apparent effect of liberalism on protest.

Several institutional characteristics are included. Size is measured by the total number of full-time undergraduates; this number is logged. The density of social interaction is proxied by the proportion of undergraduates living in dormitories. Rather than measuring selectivity or academic quality by institutional rankings (which lack transparency), I measure the proportion of freshmen for whom academic quality was a major influence on their choice of college.[18] This ranges from Alabama Agricultural and Mechanical College (13%) to California Institute of Technology (98%). Because graduate students were prominent in the student movement, an indicator is entered for institutions offering doctoral degrees. Public institutions are distinguished from privately controlled ones. Private colleges are further differentiated into non-religious (the reference category), Catholic, and Protestant. Identifying the last type requires subtlety because formal control by a Protestant denomination did not invariably correspond to the institution's

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[17] If indicators for occasionally and frequently are entered separately, their odds ratios are practically identical.

[18] The proportion is strongly associated with Cass and Birnbaum's (1968: 814–8) six-fold ordinal classification (used by Van Dyke 1998). Spearman's rank-correlation coefficient is .80.

character, as measured by freshmen stating that religious affiliation influenced their choice.[19] Indicators are entered for historically black institutions and for colleges for the armed services.

Variation at the state level must be considered. Measuring the proportion voting Democrat in the 1966 Congressional election is not appropriate because that vote meant something completely different in Mississippi and in Massachusetts. Public opinion—on the Vietnam War, for example—cannot be reliably estimated at the state level.[20] Therefore I use the entire freshman survey of 1967 (numbering a quarter million) to measure the proportion of students from the state who had participated in organized demonstrations in their last year of school.[21] This ranges from 10% in Wisconsin to 28% in Washington, D.C. An indicator is added for states of the former Confederacy.

4. Results

Table 1 shows results for individual characteristics, while Table 2 details the institutional level. Coefficients are expressed as odds ratios (e^β). Variables have roughly the same scale. Proportions are naturally bounded by 0 and 1. Responses to questions on attitudes and experiences are scaled on the interval from 0 to 1 (e.g. strongly disagree is 0, strongly agree is 1). Other continuous variables (e.g. income) are rescaled by dividing by twice their standard deviation: thus the odds ratio describes the effect of an increase by two standard deviations.[22] To assess a model's overall ability to discriminate between students who did protest and those who did not, we can consider the area under the "receiver operating characteristic" curve (AUC for short), which can range from 0.5 (no ability to discriminate between outcomes) to 1 (perfect discrimination). The predicted probability is calculated from the fixed portion of the model, excluding the random intercept (ζ).

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[19] In every Catholic institution, most students mentioned religious affiliation as an influence on their choice. The University of Denver was officially Methodist, but only 11% of freshmen mentioned this influence, and 40% did not even identify as Protestant. I treat this and similar institutions as non-religious. Conversely, I treat Berea College as Protestant: it was non-denominational but required Bible study and chapel attendance (Cass and Birnbaum 1968: 56).

[20] Six Gallup polls—asking whether the Vietnam War was a mistake—from May 1966 to July 1967 yield almost 16,000 respondents, but none from New Hampshire and only 14 from Vermont.

[21] The proportion for Texas, for example, is calculated from all freshmen whose home state was Texas, regardless of where they attended college. The minimum number is 393 in South Dakota.

[22] Suggested by Andrew Gelman, this scaling convention makes continuous variables roughly comparable to binary variables (for which the maximum standard deviation is .5). Note that the logged proportion of potential protesters is scaled in this manner.

Model 1 in Table 1 is comparable to previous analyses of students at the individual level. Variation at the institutional level is controlled by the full set of variables shown in Table 2; entering a separate intercept for each college provides essentially identical results. Black students are far more likely to protest. Students from a privileged class background—as measured by education—are more likely to protest. Students from the South are less likely to protest. Students with political knowledge and political experience are naturally more likely to protest. The most important experience was taking part in demonstrations. But efficacy—the belief that individuals can affect social change—is not significant. Jews and atheists are more likely to protest than are Protestants (the reference category) and Catholics. Political values provide the most powerful predictors of protest. A student who rejected the right of college officials to ban extremist speakers has twice the odds of protesting compared to a student who endorsed it. Students whose parents were radical have triple the odds of protest compared to those whose parents were conservative. Students with greater academic aspirations are more likely to protest. Protesters are more likely to expect to major in social science than arts and humanities (the reference category), and more likely to major in either than in natural sciences or professional subjects, let alone business or engineering.

Two unexpected effects are intriguing. Students who expressed concern over their funding are more likely to protest; this effect obtains controlling for parental income and indirectly for college fees (through public/private control). Thus protesters, while more privileged than the average student, were not from the wealthiest families. Students who went to a school in their home state are less likely to protest. This effect is one of the largest, reducing the odds of protest by a third. It can be interpreted as evidence for the importance of social density: those who remained close to home retained closer ties to family and to friends from school (some of whom would be in employment), and so were less involved in the denser network of relationships on campus. Another interpretation is that moving out of state reflects an aspect of personality, namely openness to experience, which also shapes the decision to protest.

Model 1 omits the student's own anticipated chance of protesting, because that predicts but does not explain subsequent protest. This variable is added in Model 4. Compared to a student who anticipated no chance of protest, one who anticipated a very good chance has a five-fold greater odds of actually protesting, even after controlling for all other individual and institutional characteristics. This variable is a better predictor than any other.

Table 1. Predicting protest during college, 1967-70: individual characteristics

	Model 1			Model 4		
	odds	*se*	*p*	*odds*	*se*	*p*
Male	1.11	.06	.06	1.08	.06	.18
Race: black	3.94	.87	.00 ***	3.94	.88	.00 ***
Race: Asian	1.16	.28	.55	1.25	.31	.37
Race: native	1.93	.98	.20	1.93	.99	.20
Race: other	.80	.13	.17	.85	.14	.32
Parents' income (logged)	1.08	.06	.18	1.11	.07	.09
Parents' political liberalism	3.01	.30	.00 ***	2.61	.27	.00 ***
Parents' highest education: some high school	1.26	.24	.22	1.24	.24	.27
Parents' highest education: high school graduate	1.31	.23	.12	1.28	.22	.17
Parents' highest education: some college	1.39	.24	.06	1.33	.24	.11
Parents' highest education: college degree	1.54	.27	.02 *	1.45	.26	.04 *
Parents' highest education: postgraduate degree	1.49	.27	.03 *	1.41	.26	.06
Home in the South	.73	.07	.00 ***	.76	.07	.00 **
School type: private non-religious	.87	.08	.15	.84	.08	.08
School type: private religious	1.11	.08	.16	1.04	.08	.60
Average grade in secondary school	.93	.11	.54	.93	.11	.54
Number of students known by name (logged)	1.06	.05	.19	1.05	.05	.27
Number of students considered close friends (logged)	.96	.04	.35	.98	.05	.72
Can use Robert's *Rules of Order*	1.21	.07	.00 ***	1.21	.07	.00 ***
Can describe freedoms in Bill of Rights	1.16	.05	.00 ***	1.14	.05	.00 **
Participated in organized demonstrations in last year of school	1.81	.11	.00 ***	1.32	.08	.00 ***
Present religion: Catholic	1.06	.07	.37	1.05	.07	.47
Present religion: Jewish	1.50	.14	.00 ***	1.45	.14	.00 ***
Present religion: none	1.70	.14	.00 ***	1.49	.13	.00 ***
Present religion: other	1.19	.13	.12	1.06	.12	.58
Importance of keeping up to date with political affairs	1.67	.13	.00 ***	1.55	.13	.00 ***
Whether an individual can affect social change	1.08	.08	.29	1.06	.08	.39
Whether college officials have right to ban extremist speakers	.44	.03	.00 ***	.57	.04	.00 ***
Intends to obtain graduate degree	1.14	.06	.01 *	1.14	.06	.01 *
Probable major: social science	1.20	.08	.01 **	1.15	.08	.04 *
Probable major: education	.79	.08	.02 *	.82	.08	.05 *
Probable major: biological and physical sciences	.83	.06	.01 **	.85	.06	.02 *
Probable major: business	.64	.07	.00 ***	.66	.07	.00 ***
Probable major: engineering	.64	.06	.00 ***	.66	.07	.00 ***
Probable major: professional	.81	.06	.01 **	.83	.06	.01 *
Probable major: other	.73	.08	.00 **	.75	.08	.01 **
Concern over financing college	1.31	.11	.00 **	1.28	.11	.00 **
Dropped out for at least one term	.78	.05	.00 ***	.77	.06	.00 ***
College in home state	.66	.03	.00 ***	.65	.03	.00 ***
Chance of protest: little				1.65	.09	.00 ***
Chance of protest: some				2.80	.18	.00 ***
Chance of protest: very good				5.00	.59	.00 ***
Institutional characteristics	yes, not shown			yes, see Table 2		
AUC (fixed only)	.742			.758		
N	11,355			11,355		

Multilevel logistic regression with 7 integration points

odds: odds ratio; se: standard error; p: p-value (two-tailed), *** p < .001, ** p < .01, * p < .05

Our focus now shifts to institutional characteristics. Model 2 in Table 2 considers only the institution's characteristics, and so is comparable to previous analyses at the ecological level. The probability of protest increases with the number of undergraduates. As expected, protest increases with academic quality. Moving from a college at the 10th percentile to one at the 90th almost doubles the odds. Protest increases with the proportion of students living in dormitories, in accordance with the hypothesis of social density. Moving from a college with 32% of undergraduates in dormitories (the 10th percentile) to one where all lived in dormitories (the 90th percentile) multiplies the odds by 1.5. The presence of doctoral students has the opposite effect from that expected, though this is not quite statistically significant. Protest is more likely at private institutions than in public ones, and more likely in Catholic than in Protestant colleges.[23] Obviously protest is far more likely at historically black institutions, and far less likely at military schools. Geographical location also matters. College students are more likely to protest outside the South and in states where many high-school students had demonstrated. Moving from Missouri (at the 10th percentile) to Maryland (at the 90th percentile) multiplies the odds by 1.5.

Model 3 introduces all the individual characteristics shown in Table 1, including anticipated protest. This severely attenuates the institutional variables; several are no longer statistically significant. There is one exception (which persists in the next model): protest is significantly more likely at Catholic institutions. The substantial increase in AUC from Model 2 to Model 3 demonstrates that protest was predicted mainly by individual characteristics that predated college, deriving from family background or school experiences. Institutions varied largely because they admitted different students.

Model 4 adds the crucial compositional variable, the proportion of other freshmen who thought there was at least some chance of protesting—for brevity, potential protesters. The variable is logged because the marginal effect diminishes as potential protesters increase. Its effect is strongly positive and statistically significant ($p = .001$). Moving from the 10th percentile to the 90th percentile, would multiply the odds of protest by a factor of 1.9 (the 95% confidence interval for this odds-rate ratio is 1.2 … 2.6). Figure 2 shows the effect for a typical freshman, setting all other variables to their median (mode for nominal variables). The vertical lines depict the 10th, 50th, and 90th percentiles; shading denotes the 95% predictive interval. A typical student, at a school where 15% of freshmen were potential protesters would have a .32 probability of

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[23] The coefficients for public and for private Protestant differ ($p = .02$), as do the coefficients for Protestant and Catholic ($p = .01$).

Table 2. Predicting protest during college, 1967-70: institutional characteristics

	Model 2			Model 3			Model 4			Model 5		
	odds	se	p	odds	se	p	odds	se	p	odds	se	p
Individual characteristics	no			yes, not shown			yes, see Table 1			yes, not shown		
Number of full-time undergraduates (logged)	1.40	.22	.03 *	1.11	.17	.51	1.07	.16	.67			
Freshmen choosing college for academic reputation	3.88	1.08	.00 ***	1.78	.49	.04 *	1.17	.35	.60			
Undergraduates in dormitories	1.85	.42	.01 **	1.23	.27	.35	1.12	.24	.60			
Doctoral program	.75	.11	.06	.84	.12	.22	.89	.13	.39			
Control: private Catholic	1.19	.18	.23	1.48	.23	.01 *	1.48	.22	.01 **			
Control: private Protestant	.80	.11	.12	.92	.13	.53	.98	.13	.91			
Control: public	.54	.09	.00 ***	.81	.13	.18	.92	.15	.61			
Institution for blacks	10.42	3.14	.00 ***	2.15	.76	.03 *	1.29	.50	.51			
Institution for armed services	.19	.08	.00 ***	.30	.12	.00 **	.93	.50	.89			
Other freshmen expecting some chance of protest (logged)							1.72	.29	.00 **	1.93	.19	.00 ***
Southern state				.86	.13	.31	1.06	.17	.71			
Proportion of state's students demonstrating at school				11.98	15.67	.06	3.65	4.85	.33			
Random intercept: college (variance)	.28	.04		.23	.04		.22	.04		.24	.04	
AUC (fixed only)	.657			.757			.758			.756		
N	11,355			11,355			11,355			11,355		
AIC	14,379			13,016			13,008			12,999		

Multilevel logistic regression with 7 integration points

odds: odds ratio; se: standard error; p: p-value (two-tailed), *** p < .001, ** p < .01, * p < .05

Figure 2. Effect of student composition on protest (Model 4)
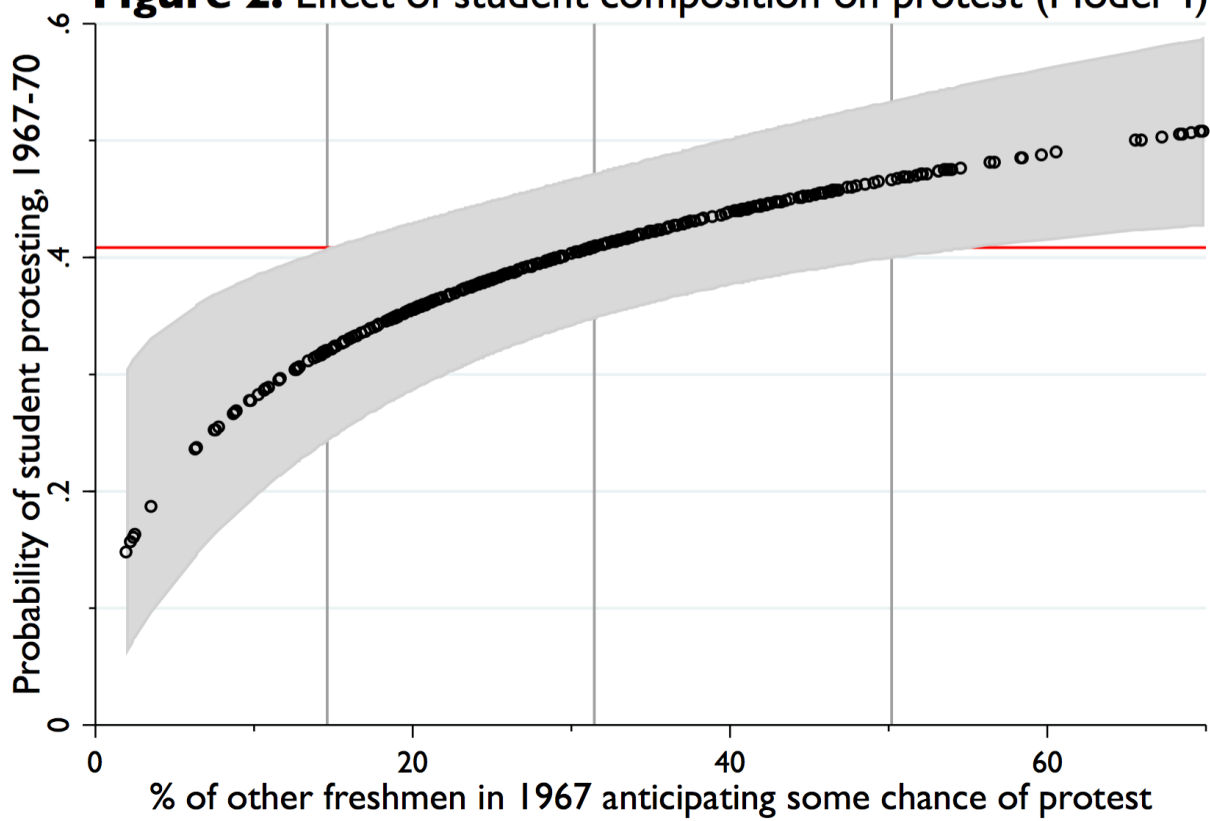
protesting. Increasing potential protesters to 50%—holding all other individual and institutional characteristics constant—would increase the probability to .47.

Compared to Model 3, the increased predictive accuracy of Model 4 (measured by AUC) is minimal. The Akaike Information Criterion (AIC) captures the tradeoff between accuracy and parsimony: a reduction by more than 3 points indicates a substantively better model.[24] Model 4 reduces AIC by 8 points. Model 5 drops all the other variables at the institutional level (including the two geographic variables). Remarkably, this reduces AIC by a further 9 points. Therefore the proportion of potential protesters is the key variable at the institutional level.

It is worth testing analogous compositional variables. One might suspect that the absolute number of potential protesters would be relevant; in a large university, a small proportion still represents a critical mass. Adding the number of potential protesters (logged) to Model 4, however, has no effect. One might also suspect that actual experience of protest would be more significant. The proportion of freshmen who had demonstrated in high school, however, has no discernible effect when substituted for potential protesters in Model 4. Demonstrating in high school must have depended on factors such as geographical location, and so absence did not necessarily indicate lack of inclination. Conversely, some of the demonstrators did not intend to continue: over a third anticipated little or no chance of protest during college.

How do we explain the importance of student composition? Despite the inclusion of so many individual characteristics, one might still suspect deliberate selection: someone who intended to join the burgeoning student movement would choose a school where it was already prominent. That implies that composition should have no effect on those freshmen who expected not to protest (and had not demonstrated in high school). Applying Model 4 to this subsample ($n = 3,012$), the estimated effect is even larger. Moving from the 10th percentile to the 90th would multiply the odds of protest by 2.5 (the 95% confidence interval for this odds-rate ratio is 1.1 … 3.9). Figure 3 shows this effect, setting all other variables at the median (or mode) for this subsample. Students who expected not to protest were strongly affected by their peers. The association cannot be explained by intentional selection, because students from this subsample

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[24] The correction for sample size is used:

$$AIC = -2\log(L) + 2p + \frac{2p(p+1)}{n-p-1}$$

where L is the likelihood, n is the number of observations, and p is the number of parameters (Burnham and Anderson 2004).

Figure 3. Effect of student composition on protest for students who expected not to protest
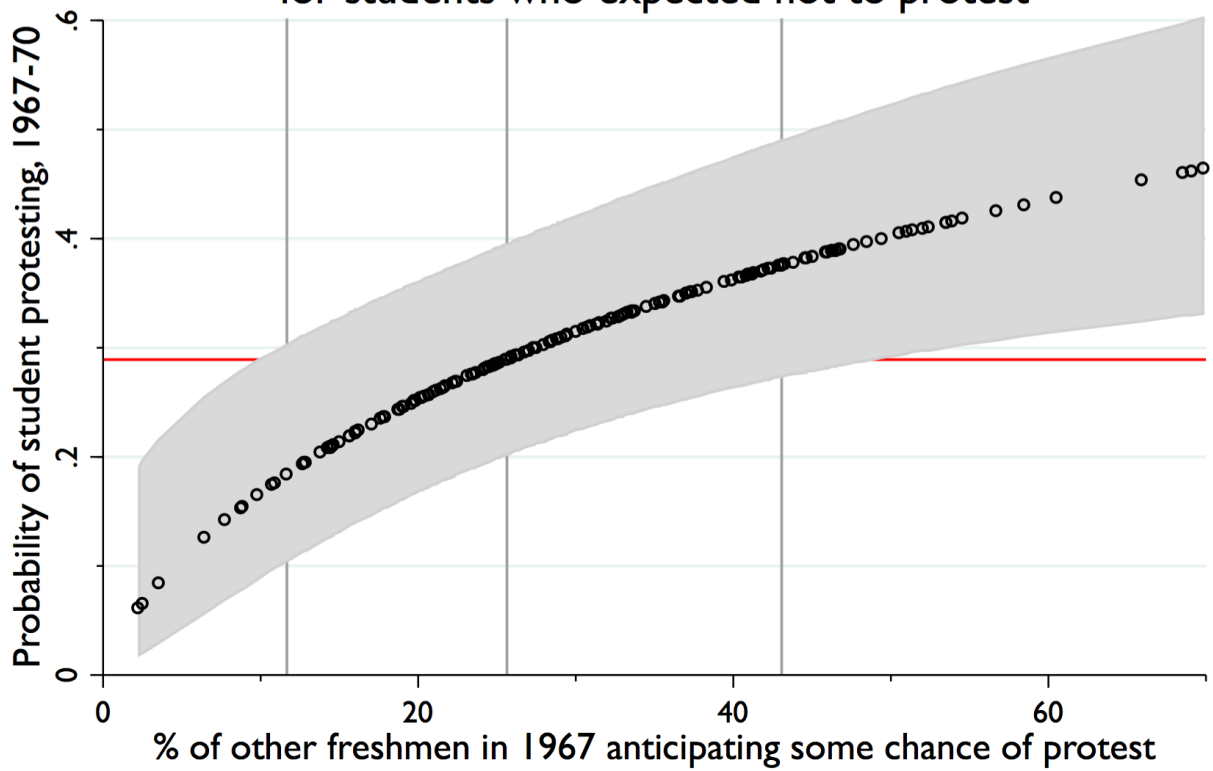
who found themselves in a college with a high proportion of potential protesters had not chosen it for that reason.

Selection, whether intentional or not, can be tested indirectly as well. If the proportion of potential protesters had an effect due to selection, then that effect should be the same whether the student remained in the same college or left for another. Recall that freshmen who changed schools are excluded from the estimation sample. Did the proportion of potential protesters have a similar effect on those who left their freshman college? When Model 4 is applied to this additional sample ($n = 3{,}292$), there is no discernible effect (the 95% confidence interval for the exponentiated coefficient is 0.7 … 1.5).[25] In short, the composition of freshmen strongly affected those students who remained, but not those who left. This is consistent with genuine social influence and inconsistent with selection.

These results for individual protest have important implications at the institutional level. Figure 4 displays histograms of the proportion of students in each college who are predicted to protest. These predictions are derived from Model 4 (the fixed portion, excluding ζ). The first graph provides a baseline, by randomly assigning students to colleges. In consequence, the proportion of potential protesters in each college varies randomly around the overall mean. Because other institutional factors—such as the college's academic standing or its location— have such little influence on the probability of protest, the resulting variation among colleges is minimal. The second graph locates students in their actual colleges, but sets social influence to the overall mean. Variation is now considerable, which emphasizes the fact that colleges differed mainly because they admitted different sorts of students. The characteristics of freshmen in 1967, before they even started class, substantially predicts the distribution of subsequent protest. The third graph adds the actual proportion of potential protesters to each college. This step noticeably increases the variance (the standard deviation rises from 13 to 17), though by less than the previous step. Thus social influence reinforced polarization among institutions. This effect applied especially where potential protesters comprised a small minority; they were much less likely to protest than individuals with the same characteristics in more supportive social

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[25] In a pooled model with potential protesters entered separately for students who moved and those who remained, the hypothesis of the same effect is rejected, $p = .002$. The second wave includes only minimal information on the student's current institution, and this is often missing. Therefore the trajectory of those who moved is not possible to analyze.
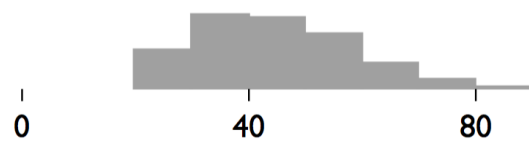
Figure 4. Variation in protest among colleges

A. Predicted: individuals
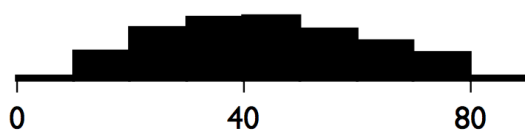randomly assigned to college

s.d. = 5.4

0   40   80
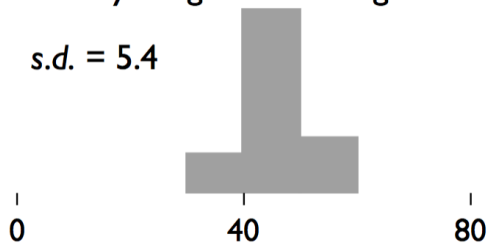
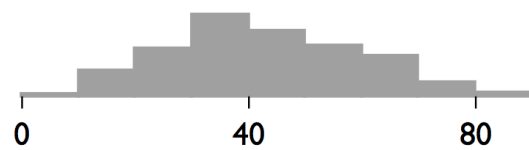B. Predicted: social influence
set to overall average

s.d. = 12.9

0   40   80

D. Actual

s.d. = 18.1

0   40   80

C. Predicted

s.d. = 16.8

0   40   80

% of 1967 freshmen protesting by 1970

environments. These predictions replicate almost the full extent of variation across colleges, as depicted in the final graph.

5. Discussion

Before discussing the results, their limitations should be underlined. Most importantly, the estimate of social influence is valid only insofar as there are no unmeasured characteristics that affect an individual's decision to protest, and that vary systematically across institutions—and so would be confounded with the proportion of potential protesters. Without randomly assigning students to colleges, we can never rule out unobserved heterogeneity. In mitigation, however, this analysis does incorporate a very large number of individual variables, along with several institutional variables. Exceptionally, those variables include the entering freshman's own prediction of her or his chance of protest, which comprehensively captures personal circumstances—as known to the individual—bearing on the decision to protest. The attrition rate between the first and second waves is a concern, but there was no relationship between a student's anticipated chance of protest and their probability of responding in the second wave (Online Appendix S2).

The results confirm the significance of many individual characteristics identified in the literature on student protesters. This confirmation is useful because it comes from multivariate analysis of longitudinal data. Consider, for example, the pronounced difference across disciplines—social scientists protested more than engineers—which is well known as a bivariate association.[26] One could suspect that this simply reflected the associations between parental class and protest on one hand, and discipline on the other; students from less privileged backgrounds were attracted to more vocational subjects. Or one could suspect reverse causation, as protesters switched to social science for its relevance to their political activism. Both conjectures turn out to be false. The disciplinary difference persists even controlling for parental class, and it is manifested when the student's probable major is elicited as they enter college.

The characteristics of incoming freshmen—before they were exposed to college—were the most important predictor of subsequent protest. This finding underscores the importance of selection into social contexts. Studies that take institutions as units of analysis are liable to overestimate their importance. Consider the proportion of students living in dormitories. At the

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[26] Gambetta and Hertog (2016) revive the study of disciplinary differences in political values, dormant since Ladd and Lipset (1975).

ecological level, this variable has a large and positive effect on protest (Model 2). The effect accords with theoretical expectations, for protest should increase with social density (Zhao 1998). Taking into account the characteristics of individual students, however, there is no effect for dormitory residence at the institutional level (Model 3). In other words, colleges that housed most students in dormitories simply admitted more students who were already inclined to protest.

Only by measuring the characteristics of incoming freshmen is it possible to estimate the effect of social influence. The central finding is that a student was far more likely to protest in a college where other freshmen expected to protest (Figures 2 and 3). The compositional variable used here does not measure actual social interaction, but it captures the peer group from whom the student would draw roommates, classmates, and friends. The median freshman's cohort numbered about 450 students. Students were surely influenced by earlier cohorts of students, but this does not make any material difference because variation across colleges—the difference between Baylor University in Texas and Oberlin College, for example—would be very similar for successive cohorts. Social influence could have operated gradually and imperceptibly, as students interacted through informal friendship networks and formal campus organizations. Or it could have occurred more eventfully, as students were drawn into campaigns over local issues— such as the presence of the Reserve Officers' Training Corps—or into one of the concerted moments of mobilization against the Vietnam War.

Could students have been influenced by faculty rather than by their peers? This source of enculturation was emphasized by the famous study of Bennington College during the 1930s (Newcomb 1943). The second wave of the survey asked students to assess the "political leaning" of their professors. It requires the same caveats as parents' political liberalism, discussed above. A contextual variable is created by taking the mean. Los Angeles Baptist College is the most conservative, while Bennington (appropriately enough) is the most liberal. Across colleges, faculty liberalism is positively correlated with the (logged) proportion of potential protesters (r = .68). When added to Model 4, however, this variable is not statistically significant (p = .59); potential protesters remains statistically significant (p = .03).

Movement organization is not entered as a separate independent variable because it is another outcome to be explained, alongside protest. Nevertheless, one could treat prior organization as a rival explanation to social influence. The leading organization was SDS, which had perhaps 25,000 members by late 1967. Scorning bureaucracy, the national office did not systematically record its constituent chapters. Nevertheless, a list of chapters known to exist in

the mid 1960s can be reconstructed.[27] SDS had a presence at a quarter of the institutions in my sample. It is only slightly correlated with the proportion of potential protesters (logged, $r = .38$). When added to Model 4, this variable has no discernible effect (the 95% confidence interval for the exponentiated coefficient is 0.7 … 1.5), while potential protesters remains statistically significant ($p = .002$).

The estimated magnitude of social influence is large: moving from the 10th to the 90th percentile almost doubles the odds of protest. This magnitude may seem suspiciously large. Most rigorous studies of political behavior measure lesser influences: how someone is affected by a single co-resident individual (Nickerson 2008; Klofstad 2011), or by nonsalient information about many friends (Bond et al. 2012). The variable measured here, by contrast, cumulates interaction with many people—the major portion of the individual's social world—extending over years. In addition, this study considers a stage in the life cycle and a social role in which individuals are especially open to influence by others. "College is a 'most likely' case of peer influence" (Klofstad 2011: 26). Finally, the historical context was exceptionally conducive to protest.[28] The escalating war in Vietnam violated the ideals of many students, while it also threatened men with conscription. This trend of increasing grievances does not explain variation among students and colleges, of course, but we may suspect that it was a necessary condition for social influence—especially for those who had initially not expected to protest. Had the Vietnam War ended in 1968, it is conceivable that few of this subset of students would have protested, no matter how many of their peers had anticipated protest as freshmen (thus the curve in Figure 3 would be a horizontal line just above zero).

6. Conclusion

Lazarsfeld (1948) invented the panel survey to trace the dynamics of social influence. Following individuals over time enables us to see how their attitudes and actions evolve in response to significant others. This paper uses the same analytical strategy. It takes advantage of the

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[27] The source is Sale's (1973) detailed history (following Van Dyke 1998). I start with the last comprehensive list of chapters at the end of 1964 and continue with the periodic listings of new and revived chapters until the end of the 1966/67 academic year (Sale 1973: 161, 193, 246, 306–7, 341). I also search the digitized text for the name of every institution in my sample.

[28] For the students studied by Klofstad (2011: 32), serious political engagement meant going to see Obama's speech during the presidential primary. Four decades earlier, their predecessors had shut down the University of Wisconsin and forced the Chancellor to resign.

transition from school to college, which plunges people into a new social context. Students exercise some choice over which college to attend, within constraints such as academic achievement and parental income. The sorting process ensures that students attending each college resemble each other more than they resemble the overall student population. Nevertheless, there is still considerable variation among students entering each college. Students—especially those who live on campus—spend much time in the company of their fellows in college. Conceivably, those who deviate significantly from the average could react by forming their own subgroup, which preserves or accentuates the initial deviation. More plausibly, they would tend to converge towards the average. Social influence within college would therefore amplify variation across institutions.

We have two snapshots of some 11,355 individuals: on entering college in the fall of 1967, and at the beginning of 1970. This is the largest panel survey, by an order of magnitude, ever used to study protest. Compositional variables are aggregated from almost the entire cohort of freshmen entering these 171 colleges, numbering over one hundred thousand. These data provide two crucial advantages. Social influence is measured by the actual responses of fellow students, rather than by the individual's own subjective perception. Characteristics of individual and institutions are measured before the outcome. Either feature is rare in the study of social movements; to my knowledge, this is the first to combine both.

What led almost half of these students to protest? Institutional characteristics were not significant, with the minor exception that protest was more likely in Catholic colleges. The individual characteristics of entering freshmen were most important in determining protest. It was most likely if their parents were highly educated and politically liberal, if they came from outside the South, if they valued politics and were skeptical of authority, if they were not Christian, if they wanted to study social sciences or humanities, and if they worried about financing college. Beyond the individual's own characteristics, however, the decision to protest was strongly influenced by peers within college. Social influence is measured by the proportion of freshmen who had anticipated that they had at least some chance of protesting. Moving from a college at the 10th percentile to one at the 90th percentile, the odds of someone taking part in protest almost doubled. This effect held, moreover, for the subset of students who had expected not to protest. There was no effect for students who left for another institution. Social influence had significant ramifications at the institutional level. It exacerbated initial differences in the

freshman cohort, and was particularly important in curbing protest in colleges where potential protesters comprised a small minority.

One conclusion of this paper is that social influence should be carefully distinguished from social density. Students who attended college outside their home state were far more likely to protest, and this suggests the importance of involvement in the dense web of relationships on campus. Unlike social density, however, social influence may either inflame or dampen dissent. The literature on social movements tends to focus on social networks as conducive to participation, without considering how they might also hinder it. The emphasis on individuals being recruited to movements through personal ties, though undoubtedly correct, is inherently asymmetrical; recruitment has no inverse. There is also a habit of sampling on the dependent variable, of studying protest without comparing its absence. To take one example, churches are viewed as the basis of the Civil Rights movement, and yet comparison shows that the students who took part in the sit-ins were those who attended church less frequently (Biggs 2006: 330). McAdam (2003) argues that social formations promote mobilization only if they are culturally appropriated. Dissolving collectivities into their constituent individuals provides a different (though not contradictory) perspective. Individuals become more like the peers with whom they interact, and so the outcome depends on the character of the peers. Social influence on protest was positive at Oberlin College, but negative at Oklahoma State University.

Another conclusion is that selection into social contexts should be taken seriously. Studies of social movements usually treat individuals as wholly the product of their social location, either because the analysis focuses exclusively on supra-individual structures or because any association between individual characteristics and social context is causally attributed to the latter (you protested *because* your friends approve of protest). Yet individuals are selected into social contexts, through their own choices and by means of institutional filters and structural constraints; people deliberately seek likeminded friends or end up in situations with people who resemble them (you have formed friendships with people who approve of protest *because* you are a potential protester). The claim is not that individuals somehow exist prior to social contexts, of course, but that individuality emerges from the process of navigating between them, as Simmel famously argued. For the college students studied here, their individual characteristics—as entering freshmen—had been shaped by earlier social contexts, especially family and school. Similar sorts of students were admitted into each college, and accounting for this process of selection is essential in order to understand what happened during college.

This paper demonstrates how the study of social movements will be enhanced by multilevel analysis, which considers individuals within their social context. Movements are usually studied through meso-level analysis of social aggregates and institutions. A parallel literature on political participation uses surveys that sever individuals from their social context. Both approaches are inherently limited. As this paper shows, it is misleading to explain protest at the level of the college—looking at the presence of movement organizations, for instance, or the number of students—without regard to the composition of the student body. It is equally misleading to consider only individual students. Admittedly, lack of suitable data restrict our ability to analyze individuals in context, given the nature of protest—normally rare, but flaring up unpredictably in certain times and places. Even when we lack data on individuals, however, there remains scope for disaggregation. Rather than taking cities as the unit of observation, for example, consider variation among neighborhoods (e.g. Kawalerowicz and Biggs 2015). Contemporary social media promise exciting opportunities to analyze social influence (e.g. Budak and Watts 2015). It is easy to overlook the wealth of data gathered by the pioneering generation of quantitative social scientists in the 1960s.[29] Those who had the foresight to archive their data left a legacy which deserves investigation with modern techniques. We still have much to learn from the past.

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[29] The earlier generation also benefitted from higher response rates. Two comparable surveys of undergraduates illustrate the decline over half a century: from 84% (Matthews and Prothro 1966: 497) to 23% (Klofstad 2011: 144).

Appendix Table A1. Independent variables

FQ indicates questions asked in the first wave, to freshman entering in 1967 (questionnaire in Panos, Astin, and Creager 1967: 59–62). Responses to most of these questions are included in Trow et al. (1978), but some are available only from HERI and ACE (2001). SQ indicates questions asked in the second wave, after Christmas 1969 (questionnaire in Trow 1975: 403–14). ACE indicates institutional data taken from American Council on Education (1968).

Male. FQ "Your sex." "Male" (1); "Female" (0)
Race. FQ "What is your racial background?" "Caucasian" (reference); "Negro"; "American Indian"; "Oriental"; "Other"
Parents' income (logged). FQ "What is your *best estimate* of the total income last year of your parental family? (not your own family if you are married). Consider annual income from all sources before taxes." <$4,000; $4,000–$5,999; $6,000–$7,999; $8,000–$9,999; $10,000–$14,999; $15,000–$19,999; $20,000–$24,999; $25,000–$29,999; $30,000 or more; "I have no idea." Midpoint of category used, with $2,500 for lowest and $40,000 for highest. Missing values (including "no idea") estimated from regressing the logarithm of income on indicator variables for race, father's education, mother's education, father's occupation, mother's occupation, home state, school type, concern over financing college, and public/private college ($R^2 = .49$). Natural logarithm taken. Scaled by dividing by two standard deviations
Parents' political liberalism. SQ "Indicate the political leaning which best describes your parents." "Strongly conservative" (0); "Moderately conservative" (.25); "Middle-of-the road" (.5); "Liberal" (.75); "Left" (1)
Parents' highest education. FQ "What is the highest level of formal education obtained by your parents?" Asked for father and for mother; the highest is coded here. "Grammar school or less" (reference); "Some high school"; "High school graduate"; "Some college"; "College degree"; "Postgraduate degree"
Home in the South. FQ "Your current home state." States of the Confederate South (1); elsewhere (0).
School type. SQ "From what kind of secondary school did you graduate?" "Public" or "Bureau of Indian Affairs" or "Other Federal Government" (reference); "Non-public, Catholic" or "Non-public, other religiously affiliated"; "Non-public, non-religiously affiliated"
Average grade in secondary school. FQ "What was your average grade in secondary school?" "A or A+" (1); "A-" (6/7); …; "D" (0).
Number of students known by name (logged). FQ "How many students in high school did you know by their first name or nicknames?" <5; 6–10; 11–50; 21–50; 51–100; 101–200; more. Midpoint of category used, with 3 for lowest and 250 for highest. Natural logarithm taken. Scaled by dividing by two standard deviations
Number of students considered close friends (logged). FQ "How many of these students did you consider *close friends*?" <5; 6–10; 11–50; 21–50; 51–100; 101–200; more. Midpoint of category used, with 3 for lowest and 250 for highest. Natural logarithm taken. Scaled by dividing by two standard deviations
Can use Robert's *Rules of Order*. FQ "For each of the following activities, indicate if you can presently perform the activity competently"—"Use Robert's *Rules of Order*." "No" (0); "Yes" (1)
Can describe freedoms in Bill of Rights. FQ "For each of the following activities, indicate if you can presently perform the activity competently"—"Describe the personal freedoms guaranteed by the Bill of Rights." "No" (0); "Yes" (1)
Participated in organized demonstrations in last year of school. FQ "Indicate which of these things you did during the *past year* in school"—"Participated in organized demonstrations." "Not at all" (0); "Occasionally" or "Frequently" (1)
Present religion. FQ "Your present religious preference." "Protestant" (reference); "Roman Catholic"; "Jewish"; "Other"; "None"
Importance of keeping up to date with political affairs. FQ "Indicate the importance to you personally of keeping up with the following"—"Keeping up to date with political affairs." "Essential" (1); "Very important" (2/3); "Somewhat important" (1/3); "Not important" (0).
Whether an individual can affect social change. FQ "Realistically, an individual person can do little to bring about changes in our society." "Agree strongly" (0); "Agree somewhat" (1/3); "Disagree somewhat" (2/3); "Disagree strongly" (1)
Whether college officials have right to ban extremist speakers. FQ "College officials have the right to ban persons with extreme views from speaking on campus." "Agree strongly" (1); "Agree somewhat" (2/3); "Disagree somewhat" (1/3); "Disagree strongly" (0)

Intends to obtain graduate degree. FQ "What is the highest academic degree that you intend to obtain?" "None" or "Associate" or "Bachelor's degree" or "B.D." or "Other" (0); "Master's degree" or "Ph.D. or Ed.D." or "M.D., D.D.S., or D.V.M." or "LL.B. or J.D." (1).
Probable major. F "Your probable major field of study." Groupings: "Arts and humanities" (reference); "Social science"—from this grouping, the field of education is given a separate indicator variable; "Biological science" or "Physical science"; "Business"; "Engineering"; "Professional"; "Other fields"
Concern over financing college. FQ "Do you have any concern about your ability to finance your college education?" "None (I am confident that I will have sufficient funds" (0) "Some concern (but I will probably have enough funds)" (0.5) "Major concern (not sure I will be able to complete college)" (1)
Dropped out for at least one term. SQ "Since first entering college, have you ever dropped out for a term or longer?" "No" (0); "Yes" (1)
College in home state. FQ "Your current home state." Same state as college (1); different (0)
Chance of protest. FQ "What is your best guess as to the chances that you will"— "Participate in student protests or demonstrations." "Very good chance"; "Some chance"; "Very little chance"; "No chance" (reference)
Number of full-time undergraduates (logged). ACE: full-time undergraduates enrolled in fall 1966. Natural logarithm taken. Scaled by dividing by two standard deviations
Freshmen choosing college for academic reputation. FQ "Indicate the importance to you personally … in your decision to enroll in *this college*"—"Academic reputation of the college". "Major influence" (1); "Minor influence" or "Not relevant" (0). Averaged by college. Total $n = 126{,}589$
Undergraduates in dormitories. ACE: capacity of dormitories or residence halls for undergraduates, divided by number of full-time undergraduates
Control. Private non-religious (reference); private Catholic; private Protestant; public. ACE. Formal affiliation checked against FQ "Indicate the importance to you personally … in your decision to enroll in *this college*"— "Religious affiliation." Five colleges officially controlled by a Protestant denomination reclassified as non-religious because less than a quarter of freshmen reported religion as influencing their choice; one non-sectarian college reclassified as Protestant because over half reported religion as influencing their choice
Black. Historically black institution (1); others (0)
Armed services. U.S. service institutions and Virginia Military Institute (1); others (0)
Other freshmen expecting some chance of protest (logged). FQ (see above). Proportion of freshmen in college—excluding the focal respondent—answering "Some chance" or "Very good chance." Total $n = 126{,}589$. Natural logarithm taken. Scaled by dividing by two standard deviations
Southern state. College located in the Confederate South (1); elsewhere (0)
Proportion of state's students demonstrating in last year of school. FQ (see above). Proportion of all freshmen (in every institution in the first wave), whose home was in this state, answering "Occasionally" or "Frequently." Total $n = 245{,}537$

Appendix Table A2. Summary statistics for independent variables

Variable	range			mean (std dev)
Individual				
Male	0	—	1	.51
Race: black	0	—	1	.03
Race: Asian	0	—	1	.01
Race: native	0	—	1	.00
Race: other	0	—	1	.02
Parents' income (logged)	6.42	—	8.77	7.72 (.50)
Parents' political liberalism	0	—	1	.36 (.23)
Parents' highest education: some high school	0	—	1	.06
Parents' highest education: high school graduate	0	—	1	.24
Parents' highest education: some college	0	—	1	.20
Parents' highest education: college degree	0	—	1	.29
Parents' highest education: postgraduate degree	0	—	1	.21
Home in the South	0	—	1	.15
School type: private non-religious	0	—	1	.06
School type: private religious	0	—	1	.18
Average grade in secondary school	0	—	1	.68 (.22)
Number of students known by name (logged)	0.79	—	3.95	3.55 (.50)
Number of students considered close friends (logged)	0.58	—	2.93	1.22 (.50)
Can use Robert's Rules of Order	0	—	1	.20 (.40)
Can describe freedoms in Bill of Rights	0	—	1	.55 (.50)
Participated in organized demonstrations in last year of school	0	—	1	.16
Present religion: Catholic	0	—	1	.28
Present religion: Jewish	0	—	1	.08
Present religion: none	0	—	1	.09
Present religion: other	0	—	1	.04
Importance of keeping up to date with political affairs	0	—	1	.57 (.28)
Whether an individual can affect social change	0	—	1	.64 (.31)
Whether college officials have right to ban extremist speakers	0	—	1	.36 (.34)
Intends to obtain graduate degree	0	—	1	.69
Probable major: social science	0	—	1	.18
Probable major: education	0	—	1	.06
Probable major: biological and physical sciences	0	—	1	.19
Probable major: business	0	—	1	.06
Probable major: engineering	0	—	1	.09
Probable major: professional	0	—	1	.13
Probable major: other	0	—	1	.06 (.23)
Concern over financing college	0	—	1	.36
Dropped out for at least one term	0	—	1	.11
College in home state	0	—	1	.55
Chance of protest: little	0	—	1	.42
Chance of protest: some	0	—	1	.24
Chance of protest: very good	0	—	1	.06
Institution				
Number of full-time undergraduates (logged)	2.47	—	5.16	3.79 (.50)
Freshmen choosing college for academic reputation	0.13	—	0.98	.68 (.19)
Undergraduates in dormitories	0	—	1	.67 (.25)
Doctoral program	0	—	1	.37 (.48)
Control: private Catholic	0	—	1	.14
Control: private Protestant	0	—	1	.21
Control: public	0	—	1	.26
Institution for blacks	0	—	1	.02
Institution for armed services	0	—	1	.02
Other freshmen expecting some chance of protest (logged)	-3.62	—	-0.33	-1.15 (.50)
Southern state	0	—	1	.16 (.37)
Proportion of state's students demonstrating at school	0.10	—	0.28	.17 (.04)

Note: standard deviation is not provided for binary variables

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How Protesting Depends on Peers: U.S. Students in the 1960s

ONLINE SUPPLEMENT

CONTENTS

ACCOMPANYING FILES

(1) Tab-delimited data: *institutions_key.txt*

(2) Tab-delimited data: *institutions_data.txt*

(3) Stata command file: *merge_waves.do*

(4) Stata command file: *results.do*

Appendix S1. Matching waves and identifying institutions

The datasets do not provide any variables to differentiate individual institutions or to link institutions and students across the datasets. My procedures are not easily described in words; the Stata program (merge_waves.do) and ancillary institutional data (institutions_key.txt, institutions_data.txt) are included with this supplementary information.

The first step is to match individuals in the Carnegie dataset (Trow et al. 1978) with their incarnations in the full freshman survey (HERI and ACE 2001). The former provided a special missing code where a student did not answer a question, but the latter did not do so for most variables, which complicates the matching process. I use fourteen variables having many values, such as mother's occupation, father's birth state, and the student's least favored major. In combination, these variables uniquely identify 99.8% of the students in the Carnegie dataset. The two datasets are then merged, matching on those fourteen variables. Out of 18,909 students in the Carnegie dataset, 18,689 (98.8%) can be merged with their incarnations in the freshman dataset.

The second step is to delimit the boundaries between students in one institution and students in another. In the freshman dataset, I delimit a new institution when an individual record differs from the previous record in at least one of these institutional particulars: state, type (2-year college, 4-year college, university[30]), sex (female, male, co-educational), race (regular, predominantly negro), public or private control, stratification cell (which classified 2-year colleges by enrollment and 4-year institutions by expenditure), male and female weights, and participation code (which denoted whether the institution was included in the national report). For students matched with the second wave, I check these institutional boundaries using this procedure for particulars in the Carnegie dataset: type and quality code, public or private control, enrollment, and weight. The procedures yield the same delimitation of institutions, except for a few universities (like the University of Pennsylvania) whose constituent schools are segmented in the freshman dataset.

The third step is to identify those institutions that were sampled by the second wave. I omit junior colleges from my analysis, and so the following discussion covers four-year institutions only. There is a list of 297 institutions participating in the 1967 freshman survey (Panos, Astin, Creager 1967: Appendix 4) but the 172 sampled in the second wave were not divulged.

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[30] The type variable erroneously classifies some 4-year colleges as 2-year; therefore I derive the threefold classification from stratification cell. This classification is confirmed by the type variable in the Carnegie dataset.

Identification is feasible precisely because the freshman dataset and the Carnegie dataset provide different institutional data. Because the second wave was taken from institutions that consistently participated in all three freshman surveys from 1966 to 1968, institutions that missed 1966 or 1968 can be ruled out (Creager et al. 1969: Appendix 4).[31] Data used to identify institutions include: number of students enrolled in 1966, which matches the variable in the Carnegie dataset (U.S. Department of Health, Education, and Welfare 1967a); quality code (Carnegie 1972: 97–116); whether it admitted both sexes or only one (ACE 1967); whether it was founded for black students (Creager et al. 1969: 12 and Appendix 4); and religious affiliation (ACE 1967).

In the smaller states, identification is usually straightforward. Alabama serves as an example. The second wave included three colleges: a private college for blacks, a public college for blacks, and a private college. We know that four colleges in the state participated in the freshman survey in 1967: Alabama Agricultural and Mechanical (A&M) College, Athens College, Spring Hill College, and Talladega College. A&M must be the public black college, and Talladega must be the private black college. Spring Hill and Athens were both in the same size range (1,000 to 2,500) and have the same quality code (6). But the former was a Catholic institution while the latter was affiliated with the Methodist Church. The private college that was selected for the second wave must be the former, because 94% of its freshmen were Catholic and 57% chose the college because of its religious affiliation.

In larger states, of course, there are sometimes two or three institutions that share identical characteristics. These situations are often resolved by further information. One is the intended majors of freshmen. In New Jersey, this serves to differentiate Newark College of Engineering from Montclair State College (primarily for training teachers). A second is the number of students in the freshman survey, which provides a lower bound on the size of the freshman cohort. A third is expenditure per student, which is contained in the stratification cell. Although these figures were not published for individual institutions, I use comparable financial information (ACE 1968) to discriminate between a pair of institutions that differ significantly in expenditure. In Massachusetts, Mount Holyoke College can be differentiated from Wellesley

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[31] The Technical Report (Carnegie 1972: 9) states that 4-year institutions are excluded unless they participated in all four years from 1966–69, but this is wrong. The sampling was planned in the fall of 1968 (p. 8). "Information for the ACE 1969 freshman survey was not yet available at the time of sampling for the present survey" (p. 9). Hence the inclusion of Brandeis University—unambiguously identified because 73% of its students were Jewish—in the Carnegie dataset, even though it did not participate in the freshman survey in 1969.

College because the latter spent 40% more per student, and its endowment was three times as large.

Once almost all the institutions are identified, it becomes apparent that the great bulk of the freshman file (containing the first 259,923 student records) was alphabetically ordered first by state and then by the institution's name. (California and Maine were exceptional, as their institutions were not consistently sorted by name.) This ordering validates my procedures for identification. It also enables resolution of otherwise intractable pairs, such as Amherst College and Williams College in Massachusetts—wealthy private colleges for men, with similar numbers of students.

Appendix S2. Attrition in the second wave

From students who had been freshmen in 1967, the Carnegie Commission intended to sample a quota of 275 from each institution—or all the students, if fewer than 275. Computer errors led to the sampling of up to 50% more from each institution (Carnegie 1972: 18–19). I estimate the average maximum quota—after these errors—as 299, because this reproduces the total number of students sampled in the second wave (Carnegie 1972: 75). This estimate is used in turn to estimate the total number of students sampled in four-year institutions, which yields the response rate of 40%.

To systematically investigate factors shaping response, I use all the uniquely identified students in the first wave (99.9%), and ascertain whether they appeared in the second wave. (Some students in the second wave fail to match, but they would account for only 0.08% of the first wave and so can be ignored.) Multilevel logistic regression is used to estimate the probability of response in the second wave. A student who does not appear in the second wave *either* was not sampled for the second wave *or* was sampled and failed to respond. I therefore weight these students by the probability that they were sampled, which is estimated for each institution as:

(min(299, number of freshmen in first wave) – number of respondents in second wave) /

(number of freshmen in first wave – number of respondents in second wave)

The total number of students is 126,437 (weighted, 43,977). Missing values reduce this number to 115,153.

Model S1 in Table S1 confirms the Commission's finding that "marginal students in lower quality institutions are underrepresented in the achieved sample" (Carnegie 1972: 75). Thus the probability of response increases with the student's grade in high school, and with the institution's academic quality. At the institutional level, there is an inverted-U relationship with the proportion of other freshmen who anticipated some chance of protest. Figure S1 shows how this affected the predicted probability for the typical freshman.[32] Students were slightly more likely to respond in the middle of the range than at either extreme. Most importantly for our purpose, response does not vary with the student's expectations of protest—which of course is the most powerful predictor of subsequent protest. Model S2 shows that there is also no
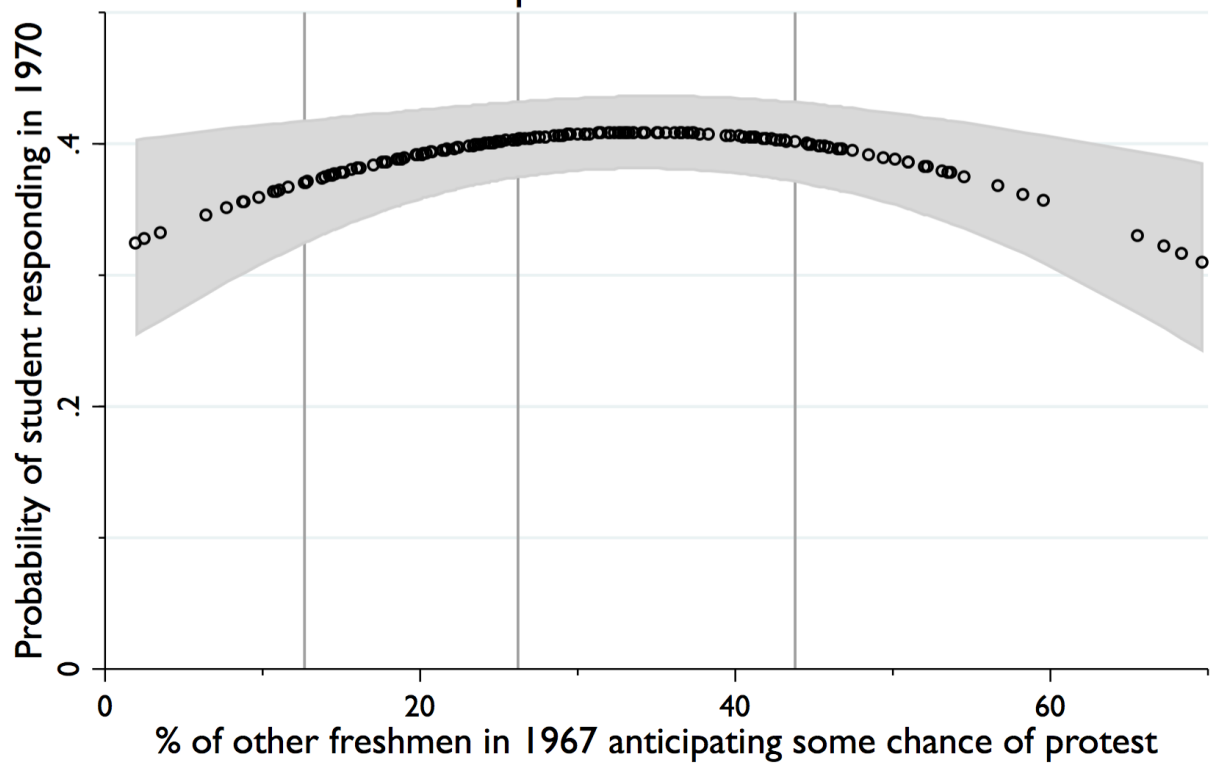
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[32] Variables are set to the median, which is necessarily derived from the unweighted distribution.

Table S1. Predicting response to second wave

	Model S1			Model S2		
	odds	se	p	odds	se	p
Individual						
Male	.87	.03	.00 ***			
Race: black	.46	.04	.00 ***			
Race: Asian	.72	.07	.00 **			
Race: native	.73	.17	.17			
Race: other	.73	.05	.00 ***			
Parents' income (logged)	.93	.02	.01 **			
Parents' political liberalism						
Parents' highest education: some high school	1.03	.08	.69			
Parents' highest education: high school graduate	1.00	.07	.95			
Parents' highest education: some college	1.03	.07	.65			
Parents' highest education: college degree	1.09	.08	.26			
Parents' highest education: postgraduate degree	1.06	.08	.45			
Home in the South	.96	.04	.43			
School type: private non-religious						
School type: private religious						
Average grade in secondary school	2.74	.16	.00 ***			
Number of students known by name (logged)	.97	.02	.13			
Number of students considered close friends (logged)	.97	.02	.18			
Can use Robert's *Rules of Order*	1.10	.03	.00 ***			
Can describe freedoms in Bill of Rights	.98	.02	.33			
Participated in organized demonstrations in last year of school	.93	.02	.00 **			
Present religion: Catholic	.88	.02	.00 ***			
Present religion: Jewish	.87	.03	.00 ***			
Present religion: none	.81	.03	.00 ***			
Present religion: other	.78	.03	.00 ***			
Importance of keeping up to date with political affairs	1.00	.04	.98			
Whether an individual can affect social change	1.01	.03	.67			
Whether college officials have right to ban extremist speakers	1.04	.03	.26			
Intends to obtain graduate degree	1.12	.03	.00 ***			
Probable major: social science	1.11	.04	.00 **			
Probable major: education	1.15	.05	.00 **			
Probable major: biological and physical sciences	1.18	.04	.00 ***			
Probable major: business	.88	.04	.01 **			
Probable major: engineering	1.10	.05	.04 *			
Probable major: professional	.98	.03	.63			
Probable major: other	1.01	.04	.89			
Concern over financing college	1.06	.04	.15			
Dropped out for at least one term						
College in home state	1.07	.03	.01 **			
Chance of protest: little	1.02	.02	.38	1.03	.02	.13
Chance of protest: some	1.03	.03	.28	.99	.02	.59
Chance of protest: very good	1.03	.05	.50	.94	.04	.14
Institution						
Number of full-time undergraduates (logged)	.91	.07	.21			
Freshmen choosing college for academic reputation	1.57	.24	.00 **			
Undergraduates in dormitories	1.19	.10	.03 *			
Doctoral program	1.04	.05	.48			
Control: private Catholic	1.17	.07	.01 **			
Control: private Protestant	1.13	.06	.03 *			
Control: public	1.20	.08	.01 **			
Institution for blacks	.94	.13	.66			
Institution for armed services	1.06	.24	.80			
Other freshmen expecting some chance of protest	1.30	.39	.38			
Other freshmen expecting some chance of protest (squared orthogonal)	.86	.05	.01 **			
Southern state	.84	.06	.01 *			
Proportion of state's students demonstrating at school	.49	.28	.22			
Random intercept: college (variance)	.04	.01		.14	.02	
N	115,153			115,153		

Multilevel logistic regression with 7 integration points; weighted by the estimated probability that the student was sampled in the second wave

odds: odds ratio; se: standard error; p: p-value (two-tailed), *** p < .001, ** p < .01, * p < .05

Figure S1. Effect of student composition on response to second wave

Probability of student responding in 1970

% of other freshmen in 1967 anticipating some chance of protest

substantive bivariate relationship. In sum, freshmen who envisaged a very good chance of protesting were not significantly more or less likely to respond in 1970 than those who expected no chance. This gives us confidence that the results presented in the paper are not affected by attrition between the first and second waves.

Appendix S3. Sample weights

The respondents in the second wave did not form a representative sample of the national cohort of freshmen for three reasons. Firstly, the institutions surveyed in the first wave were not strictly representative; there were too many elite universities, for example. Secondly, the freshmen in the first wave did not have an equal chance of being sampled for the second wave because larger institutions contributed only about 300 students (from the 1967 freshman cohort); larger institutions were thus underrepresented. Finally, the response rate in the second wave varied across institutions. The Carnegie data include an institutional weight to rejig the sample so that it is more representative of the distribution of freshmen across institutional types (defined by college versus university, and by expenditure per student). Applying these weights, the proportion of protesters falls from 45% to 35% (the 95% confidence interval is 31 … 41%). The latter is the right estimate of the proportion of protesters amongst American students who had started as freshmen in four-year institutions in 1967 and who remained in the same institution by 1970. For the purpose of analysis, however, weighting is not used because it gives such disproportionate weight to a few institutions. The maximum weight is 38 times larger than the median weight. The four institutions with the highest weights contribute 24% of the weighted sample, but their 293 students represent only 2.3% of respondents in the second wave.