CRIMINOLOGY

THE PRAGMATIC AMERICAN: EMPIRICAL REALITY OR METHODOLOGICAL ARTIFACT?*

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Scholars widely agree that the public is pragmatic about criminal justice. The empirical basis for this conclusion is the failure in several previous studies to find a sizable negative relationship between dispositional and situational crime attributions, or between support for punitive and rehabilitative crime policies. We suggest, however, that public pragmatism may be an artifact of the use of unidirectional question batteries in prior research to measure attribution styles and policy support. When such questions are used, acquiescent responding can introduce systematic error that is positively correlated across items and scales. Drawing on data from an experiment with a national sample ($N = 826$) of Internet panelists, we examine how this methodological approach impacts the bivariate correlations and multivariate relationships between attribution styles and between support for punitive and rehabilitative crime policies. The findings reveal that using unidirectional sets of questions to measure these concepts likely results in 1) inflated alpha reliability coefficients, 2) an underestimation of the magnitude of the negative relationships between attribution styles and between punitiveness and support for rehabilitation, and 3) an underestimation of the extent to which punitiveness and support for rehabilitation are driven by the same factors, working in opposite directions.

A substantial and growing body of research has investigated public opinion about the causes of crime and the appropriate criminal justice response to offending (Applegate, Cullen, and Fisher, 1997; Applegate et al., 2000; Cochran, Boots, and Heide, 2003; Gabbidon and Boisvert, 2012; Grasmick and McGill, 1994; Pickett and Chiricos, 2012; Ramirez, 2013; Thompson and Bobo, 2011; Tyler and Boeckmann, 1997; Unnever and Cullen, 2010). This line of inquiry has been particularly salient because of the documented influence of popular attitudes on criminal justice policy and court decisions (Baumer and Martin, 2013; Brace and Boyea, 2008; Enns, in press;

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Nicholson-Crotty, Peterson, and Ramirez, 2009). An important finding has been that members of the public "are profoundly pragmatic when they consider the issue of crime and its control" (Unnever et al., 2010: 453); they attribute crime to both individual failings (dispositional attributions) and environmental influences (situational attributions) and, as a result, simultaneously support punishment and rehabilitation (McCorkle, 1993; Pickett, Chiricos, and Gertz, 2014). Thus, in their seminal review of the research, Cullen, Fisher, and Applegate (2002: 60) concluded that "the central tendency in public opinion is to be punitive and progressive." Likewise, Mascini and Houtman (2006: 832) have argued that "the habit of conceiving of support for repression and for rehabilitation as diametrically opposed options should have been abandoned long ago."

The existence of public pragmatism has strong policy implications. Political responsiveness to the interests of a pragmatic public would involve bipartisan efforts to develop a multifaceted and balanced approach to crime control that ensures tough punishments for offenders *and* provides them with rehabilitative programming. The theoretical implications of public pragmatism are also significant. Evidence of pragmatism suggests that in the popular view, punitiveness and support for rehabilitation are distinct attitudinal phenomena, rather than opposing sides of a single construct (Unnever et al., 2010). If this is accurate, and punitiveness and support for rehabilitation are not opposites, then they should not simply be related in opposite ways to the same key factors (Mascini and Houtman, 2006; McCorkle, 1993). A central tenet of the pragmatism thesis, then, is that the two types of policy support are best explained by separate theoretical models (Mascini and Houtman, 2006; Pickett, Mancini, and Mears, 2013).

The concern raised in this article, however, is that the pragmatism identified in prior studies may be an artifact of the methodology typically employed in public opinion research in the field of criminology and criminal justice. Despite the overwhelming evidence of acquiescence in survey responding (Krosnick, 1999; Saris et al., 2010; Schuman and Presser, 1981), researchers often have constructed measures of key concepts—crime attributions, punitiveness, and support for rehabilitation—from batteries of questions that are unilaterally positive, where a response of "agree," "support," or "yes" indicates the presence of the concept of interest.[1] In these instances, acquiescence—"the tendency to answer items in a positive way regardless of their content" (Billiet and Davidov, 2008: 543)—can result in systematic measurement error that biases observed scores and relationships by introducing nonsubstantive variation in observations that is positively correlated across items and scales (Baumgartner and Steenkamp, 2001). This finding is important because the strongest evidence of pragmatism has been the absence of a hydraulic relation—a sizeable negative correlation—between either dispositional and situational attributions, or punitiveness and support for rehabilitation (Mascini and Houtman, 2006; Unnever et al., 2010). Indeed, the absence of this hypothesized negative correlation is the criterion for pragmatism that Unnever et al. (2010: 433) identified at the outset of their analysis: "[T]he failure to detect a hydraulic relation would suggest that Americans are more centrist and pragmatic in their views on crime and its control."

In the current study, we evaluate how acquiescence bias may have influenced both the quality of the measures used in prior research and the resultant findings about public

1. The problems associated with response acquiescence are not limited to agree/disagree items; they emerge across many different types of questions when unidirectional sets of statements are used (Blamey, Bennet, and Morrison, 1999; Krosnick, 1999; Schuman and Presser, 1981).

pragmatism. Our analyses draw on data from a recent experiment, in which respondents were randomly assigned to receive questionnaires that either did or did not include controls for acquiescence. Before describing our methodology and findings, we first delve more deeply into the potential problems created by response acquiescence, and we discuss how such stylistic responding may have contributed to the finding of public pragmatism in previous studies.

ACQUIESCENCE AND SURVEY RESPONSE

There is now compelling evidence from "more than one hundred studies using a wide variety of methods" (Saris et al., 2010: 63) that survey respondents often adopt an acquiescence response style in which they "endorse any assertion made … regardless of its content" (Krosnick, 1999: 552). One of the best examples of acquiescent responding is a 1986 *Time*/Yankelovich poll in which 80 percent and 65 percent of respondents, respectively, agreed with the following two contradictory statements: "people should have the right to purchase a sexually explicit book, magazine, or movie, if that's what they want to do"; and "community authorities should be able to prohibit the selling of magazines or movies they consider to be pornographic" (cited in Erikson and Tedin, 2005: 38). Schuman and Presser (1981) presented similar findings for crime attributions. They reported that in multiple surveys, two contradictory statements about the causes of crime—"individuals are more to blame than social conditions for crime and lawlessness in this country" and "social conditions are more to blame than individuals for crime and lawlessness in this country"—were endorsed by the majority of respondents (1981: 207–9).

Theoretically, acquiescent responding is believed to constitute a type of survey satisficing (Krosnick, 1999)—a low-effort response strategy where "instead of generating an optimal answer, respondents settle for generating merely satisfactory answers" (Krosnick, 1991: 215). Here, respondents simply report the first acceptable answer that comes to mind. As Kahneman (2011: 81) has explained, the intuitive and thus easiest approach to answering questions entails "a deliberate search for confirming evidence" and an inclination toward an "uncritical acceptance of suggestions." For this reason, when responding to surveys, it takes less mental effort to identify reasons to endorse affirmative response options (e.g., "agree," "support," or "yes,") than disaffirming ones (e.g., "disagree," "oppose," or "no") (Krosnick, 1999). Adoption of the acquiescence response style among respondents is therefore commonly a function of a low motivation to participate and/or limited cognitive abilities (Krosnick, 1991, 1999). It also can be a consequence of personality traits or social situations that dispose respondents to be especially receptive to suggestion (see, e.g., Carr, 1971; Goldberg, 1990).

Regardless of its etiology, however, when unaccounted for in empirical studies that analyze survey data, acquiescent responding can have substantial effects on research findings. Response acquiescence, by introducing systematic measurement error, can influence observed scores, measures of internal consistency and reliability, and correlations between scales (Baumgartner and Steenkamp, 2001; Billiet and Davidov, 2008; Russell, 1979). In these cases, "even strong underlying associations may turn up weak or incorrectly signed" (Green and Citrin, 1994: 261). Krosnick (1999: 552) described the effects of acquiescence bias. He noted that although there should be strong negative correlations between responses to "statements stating mutually exclusive views (e.g., 'I enjoy socializing' versus 'I don't enjoy socializing'), … across more than 40 studies, the average correlation was

only –.22." In multiple-item scales, such as those used to measure crime attributions and views about criminal sanctions, one of the strongest predictors of acquiescence bias is the directional composition of the component items. Baumgartner and Steenkamp (2001: 146) put it as follows: "[T]he higher the proportion of positively or negatively worded items, the stronger the effect of acquiescent responding on scale scores should be. If the scale is balanced, … adverse effects of acquiescence should be minimized."

PRIOR RESEARCH ON PUBLIC PRAGMATISM

Prior studies that assess whether the public is pragmatic in its views about crime and justice have approached the issue in one of two ways. The first is to examine whether a hydraulic relation exists between dispositional and situational crime attributions (Unnever et al., 2010). According to attribution theory, dispositional attributions—attributing crime to individual failings—should increase perceived offender blameworthiness and incorrigibility and, in turn, foster punitiveness, whereas situational attributions—attributing crime to social or environmental causes—should reduce perceived offender blameworthiness and incorrigibility and, in turn, increase support for rehabilitation (Carroll et al., 1987; Cochran, Boots, and Heide, 2003; Grasmick and McGill, 1994). Theoretically, individuals should tend to endorse *either* a dispositional or a situational attribution style—the hydraulic relation hypothesis (Unnever et al., 2010). In this way, "the punishment-versus-rehabilitation debate is a manifestation of the distinction between dispositional and situational attributions of crime" (Grasmick and McGill, 1994: 40–1).

To our knowledge, five previous investigations have provided tests of the hydraulic relation hypothesis as it relates to crime attributions. Consistent with the hypothesis, two studies found negative correlations between attribution styles, although the correlations are not large ($r = –.09$ and –.26) (Ortet-Fabregat and Pérez, 1992; Templeton and Hartnagel, 2012). One study yielded mixed results across two samples (Carroll et al., 1987). Two studies showed that the two attribution styles are actually *positively* correlated (Mascini and Houtman, 2006; Unnever et al., 2010). Four of these five investigations, however, operationalized attribution styles with scales constructed from agree/disagree items that are *all* positively worded, so that agreeing with the item indicates endorsement of the attribution style. The exception, the study by Ortet-Fabregat and Perez (1992), was the one that provided the strongest support for the hydraulic relation hypothesis. Those authors reversed one item that should likely be in a dispositional attribution scale—"If a person commits a crime it is because he/she wants to"—and included it with 8 positively worded items in a situational attribution scale. The dispositional attribution scale in that study, however, was created from 11 items that are all positively worded. Thus, not a single study to date has assessed the relationship between attribution styles after taking sufficient steps to minimize the influence of acquiescence bias on resultant findings.

The second approach to assessing public pragmatism is to evaluate whether there is a hydraulic relation between support for punitive and rehabilitative crime policies (Mascini and Houtman, 2006). This evaluation is most commonly done indirectly by simply comparing the survey marginals—the percent of the sample saying "agree," "support," or "yes"—for items measuring views about punishment and rehabilitation (see, e.g., Cullen, Fisher, and Applegate, 2002; McCorkle, 1993). For example, Unnever et al. (2010) showed that 69 percent and 58 percent, respectively, of General Social Survey respondents favor the death penalty and support increasing spending on "drug

rehabilitation," broadly defined. This approach to testing the pragmatism thesis is not ideal for two reasons. First, survey marginals can be influenced heavily by acquiescent responding, question wording, and question order (Applegate and Sanborn, 2011; Krosnick, 1999; Stimson, 1999). Second, finding that a majority, or slight majority, of the public generally has at least somewhat favorable attitudes toward punitive and rehabilitative policies does not provide clear evidence of pragmatism. Pragmatism implies that strong support for punitive policies also does not tend to preclude strong support for rehabilitative policies—that is, that there is not a hydraulic relation between such policy views.

A more direct test of the hydraulic relation hypothesis is achieved by examining the correlation between separate scales measuring support for punitive and rehabilitative policies. Mascini and Houtman's (2006) well-known study attempted to do this and has commonly been cited as providing definitive evidence of public pragmatism (see, e.g., Pickett, Mancini, and Mears, 2013; Unnever et al., 2010). In that investigation, punitiveness was measured with a 6-item scale, and support for rehabilitation was measured with a 12-item scale. The findings reveal that a very weak and nonsignificant negative correlation ($r = –.06$) exists between the two types of public views. An earlier study by Mascini and Houtman (2002) actually found a positive correlation ($r = .34$) between punitiveness and support for rehabilitation. Accordingly, in the conclusion of their second study, Mascini and Houtman (2006) argued that they had "established that … support for repression and support for rehabilitation are not the opposites they are typically held to be." And they asserted that "our findings cannot simply be done away with as measurement constructs" (2006: 833). However, the measures of policy support in both of Mascini and Houtman's studies were constructed from agree/disagree items that are *all* positively worded.

Contrasting Mascini and Houtman's (2002, 2006) findings, studies that have measured support for punishment and rehabilitation with scales that include both positively and negatively worded items often have found larger negative correlations between the constructs: $r = –.28$ and $–.39$ (Tam, Au, and Leung, 2008) and $r = –.44$ and $–.48$ (Cullen et al., 1985). However, an important limitation of these studies, and of both investigations by Mascini and Houtman, is that they did not actually analyze policy support, which involves support for specific means to achieve social goals (Stimson, 1999). Focusing specifically on policy support is important because, in the aggregate (as policy mood), it is the form of public opinion that shapes policy making (Enns, in press; Erikson, Mackuen, and Stimson, 2002; Stimson, 2004). Policy support also may be more polarized than other types of policy views because supporting a specific policy necessarily entails making an attitudinal commitment, implicitly or explicitly, to allocate finite public resources toward that policy. The previously mentioned studies, however, used measures of support for punishment and rehabilitation that are based in part on respondents' level of agreement with "utility statements" and "goal statements" and, thus, that conflate policy support with other types of policy views.

Utility statements gauge respondents' judgments about the effectiveness of different policies for reducing crime: "Severe penalties deter potential felons" (Mascini and Houtman, 2006: 827); "[p]unishing criminals is the only way to stop them from engaging in more crimes in the future" (Cullen et al., 1985: 316); and "[c]apital punishment reduces crime in the long run" (Tam, Au, and Leung, 2008: 1076). Utility judgments are not synonymous with policy support, however, because beliefs about whether a policy will prevent recidivism or deter offending are neither the only, nor even necessarily the primary, concerns that drive policy support (Cullen, Fisher, and Applegate, 2000; Payne

et al., 2004; see also Carlsmith, Darley, and Robinson, 2002). That is, even if individuals judge a policy to have no crime-prevention value, they may still support it for other reasons, such as a desire for just deserts. For example, recent national polls reveal that although only 32–35 percent of Americans believe the death penalty deters crime, 60–70 percent of citizens nonetheless support the policy (Gallup Report, 2013). Likewise, Applegate, David, and Cullen (2009: 64–5) found that judgments about whether trying juveniles as adults would lead them to "become productive law-abiding citizens" or to "commit more crime in the future" are only moderately correlated ($r \leq |.21|$) with actual support for transfers (see also Garland, Melton, and Hass, 2012).

Goal statements deal with abstract sanctioning philosophies: "Criminals deserve to be punished because they have harmed society" (Cullen et al., 1985: 316); "[t]he judiciary should convince criminals that they are drifting astray" (Mascini and Houtman, 2006: 828); "[w]e should try to rehabilitate men who have broken the law" (Applegate et al., 2000: 752); and "[c]riminals should be punished to make the criminals pay for their crimes" (Tam, Au, and Leung, 2008: 1076). Using such statements to measure policy support is problematic for two reasons. First, punishment philosophies are weak predictors of policy support (Applegate, Davis, and Cullen, 2009; Garland, Melton, and Hass, 2012; Warr and Stafford, 1984). Second, abstract beliefs in punishment and rehabilitation are largely irrelevant to practical policy debates. As Stimson (1999: 7) explained, "a defining characteristic of the political issue … is that we must be able to imagine reasonable people advocating either side. Thus education spending is an issue; reasonable people want more or less of it. 'Education,' though, is not an issue; virtually no one opposes it." Likewise, the key issues are not "punishment" and "rehabilitation" per se, but whether to increase or decrease the harshness of punishments and whether to expand or reduce rehabilitative programming.

Two recent studies by Pickett and colleagues (2013, 2014) measured punitiveness and support for rehabilitation with scales derived exclusively from questions asking about policy support. However, the policy support questions used in both studies were all positively worded, and neither study took any steps to control for acquiescence bias. Nonetheless, both studies revealed a significant negative correlation ($r = –.23$, and $r = –.24$, respectively) between the scales. In short, then, there is no existing research that has exclusively measured policy support and also done so using both positively and negatively worded questions. The findings from prior studies that have adopted at least one of these two methodological approaches tend to be supportive of the hydraulic relation hypothesis. On the other hand, the investigations that have adopted neither approach generally provide no support for the hypothesis.

CURRENT STUDY

In the remainder of this study, we focus on empirically testing the argument that using agree/disagree or support/oppose scales to test the hydraulic relation hypothesis, either in relation to crime attributions or views about punitive and rehabilitative policies, without first controlling for acquiescent responding is problematic. Importantly, our analyses include measures of punitiveness and support for rehabilitation that focus specifically on policy support rather than on policy goals or utility judgments. To test the preceding argument, we evaluate three specific hypotheses. In developing these hypotheses, we build on prior theoretical and empirical work on survey response that suggests that when

unilaterally positive items are used to measure constructs, acquiescent responding may introduce systematic measurement error that is positively correlated across items and scales.[2]

Hypothesis 1: The use of unilaterally positive items to measure crime attributions and policy support will lead to an overestimation of the reliability of the resulting scales. Testing this hypothesis is important because the quality of measures used to operationalize theoretical constructs constrains the meaningfulness of results and the validity of associated theoretical conclusions. Billiet and Davidov (2008: 545) explained that the positively correlated bias introduced by acquiescent responding may "artificially inflate the alpha reliability coefficients." This observation is notable because even with the use of unilaterally positive items, prior studies have commonly reported relatively low alpha coefficients for crime attribution scales—.34, .57, and .62 (Carroll et al., 1987); .58 and .63 (Cochran, Boots, and Heide, 2003; Unnever et al., 2010); .42 and .65 (Grasmick and McGill, 1994); and .66 and .71 (Templeton and Hartnagel, 2012).[3] It is probable, then, that these scales would have had lower internal consistencies if acquiescence bias were controlled.

Hypothesis 2: The use of unilaterally positive items to measure crime attributions and policy support will lead to an underestimation of the size of the hydraulic relation between dispositional and situational attribution styles, and between support for punitive and rehabilitative policies. Specifically, when such items are used, even if the negative relationships that are predicted by the hydraulic relation hypothesis are present, they may be smaller in magnitude or even positive in sign due to acquiescence bias, which should be positively correlated across scales (Baumgartner and Steenkamp, 2001; Russell, 1979).

Hypothesis 3: Once acquiescence bias is minimized, the effects of attribution styles on both punitiveness and support for rehabilitation will be similar, although in the opposite directions. Examining the correlates of policy support provides for a more complete test of the pragmatism thesis. Recall that the pragmatism thesis suggests that punitiveness and support for rehabilitation are not opposites and, thus, should not simply be related in opposite ways to the same factors (Mascini and Houtman, 2006). However, if policy support is ideological rather than pragmatic, then the two types of policy support should largely be opposite sides of the same coin, and the same factors, working in opposite directions, should drive both. For example, supporting the pragmatism thesis, previous studies often have found that whereas situational attributions are positively related to support for rehabilitation, they are either not correlated with, or are positively correlated with, punitiveness (Carroll et al., 1987; Mascini and Houtman, 2006; Unnever et al., 2010). This finding runs counter

2. It bears emphasizing that the use of unilaterally negative items would pose similar problems.
3. Indeed, to our knowledge, the only crime attribution scales that have achieved relatively high alphas (e.g., alpha = .76 to .80) have been those constructed from a large number (e.g., 7 to 11) of survey items (see, e.g., Mascini and Houtman, 2006; Ortet-Fabregat and Perez, 1992). This observation is noteworthy because, as Cortina (1993: 102) has explained, the "number of items [in a scale] has a profound effect on alpha, especially at low levels of average item intercorrelation."

to the predictions of attribution theory. We believe that it reflects the contaminating influence of acquiescence bias on findings.

METHODS

To test our hypotheses, we conducted an experiment in the summer of 2013 with a national sample of 826 adults (18 years of age and older) who were randomly selected from SurveyMonkey's online Audience panel. At the time of the experiment, more than 400,000 individuals, including residents of all 50 U.S. states, were members of the Audience panel. Membership in the panel simply entails volunteering to complete surveys on a periodic basis in exchange for a small donation to charity and the potential to win $100 in weekly drawings. SurveyMonkey recruits most members of the panel from a pool of more than 30 million persons who participate in surveys every month on the website at the request of individual SurveyMonkey customers. For our experiment, 2,770 randomly selected panelists were invited to participate in the project, of which 867 began the survey and 826 completed the questionnaire. This yielded an overall participation rate of 30 percent and a completion rate of 95 percent.

To be clear, the purpose of this study is not to produce precise estimates of population values. Rather, we focus on testing the effects of alternative methodological approaches experimentally. Such research does not require an analysis of data from a probability sample of all Americans. As the American Association of Public Opinion Research's (AAPOR) recent report on online panels emphasized, "there are times when a nonprobability online panel is an appropriate choice" (Baker et al., 2010: 758). In particular, the report explained that "nonprobability online panels … have proven to be a valuable resource for methodological research of all kinds" (2010: 759). Mounting evidence also suggests that studies using data from volunteer Internet surveys yield experimental findings (see, e.g., Yeager and Krosnick, 2012) and relational inferences (Ansolabehere and Schaffner, in press; Berrens et al., 2003; Bhutta, 2012; Sanders et al., 2007) that parallel those obtained from representative samples of the general population.

In the experiment, one randomly selected group of respondents received unidirectional batteries of questions—all positively worded—about crime attributions and policy support. This method creates the greatest potential for acquiescence bias to affect any resultant findings, but it is nonetheless the typical approach used in studies of public opinion on crime and justice. To examine the consequences of using this method, we had a second randomly selected group of respondents complete bidirectional sets of questions, in which several items were positively worded and several were negatively worded. Varying the direction of questions is a common method for controlling for acquiescence bias that has been implemented in other fields for decades (Block, 1965; Cloud and Vaughan, 1970). Although the method has limits (see Krosnick, 1999), which we discuss in the conclusion, its benefit is that it prevents respondents from obtaining high scores on resultant scales simply by engaging in acquiescent responding (Baumgartner and Steenkamp, 2001). Although ideally the bidirectional sets of questions used to construct scales would be perfectly balanced, including an equal number of positively and negatively worded items, any degree of balancing will help to reduce the contaminating influence of acquiescence bias (Baumgartner and Steenkamp, 2001).

Table 1 displays the descriptive statistics for the two randomized groups of respondents. It is clear that both of these groups differ demographically from the U.S. population. For

Table 1. Descriptive Statistics

Variables	Unidirectional Group			Bidirectional Group					
	Alpha	Mean	SD	Alpha	Mean	SD	Diff.	*t*/*z*	*p*
Variables That Are Measured Differently Across Groups									
Punitiveness	.88	3.84	1.26	.81	3.74	1.13	—	—	—
Rehabilitation	.93	4.59	1.02	.86	4.69	.91	—	—	—
Dispositional attributions	.85	3.42	1.08	.52	3.41	.77	—	—	—
Situational attributions	.80	3.55	.90	.44	3.83	.67	—	—	—
Variables That Are Measured Identically Across Groups									
Racial resentment	.90	3.59	1.29	.88	3.47	1.32	.12	1.24	.21
Perceived risk	.86	2.44	1.09	.87	2.42	1.15	.02	.23	.82
Perceived crime trend	—	3.31	1.11	—	3.35	1.11	.04	.56	.57
News consumption	—	2.32	2.49	—	2.46	2.59	.14	.77	.44
Conservatism	—	2.84	.97	—	2.75	1.00	.09	1.29	.20
Religiosity	—	3.55	1.84	—	3.63	1.87	.08	.62	.54
Family victimization	—	.34	.47	—	.33	.47	.01	.28	.78
Education	—	3.83	.97	—	3.82	.97	.00	.02	.98
Female	—	.52	.50	—	.54	.50	.02	.54	.59
White	—	.80	.40	—	.76	.43	.04	1.38	.17
Age	—	46.30	15.90	—	47.00	16.52	.70	.60	.55
N		379			393				

ABBREVIATIONS: Diff. = difference between means or proportions; *p* = two-tailed *p* value; SD = standard deviation; *t*/*z* = *t* statistic (for test of the equality of means) or *z* statistic (for test of the equality of proportions).

example, in both groups, non-Whites and persons without a college degree are underrepresented, which is normal in Internet surveys (Baker et al., 2010). For our purposes, however, what is most important is that when compared with each other, the demographic and attitudinal characteristics of the respondents in the two groups do not differ beyond what would be expected on the basis of random sampling error. This result indicates that randomization was successful.

MEASURES OF ATTRIBUTION STYLES AND POLICY SUPPORT

Following previous studies, we measured respondents' attribution styles using a series of agree/disagree items (1 = strongly disagree, 6 = strongly agree) that gauged endorsement of dispositional and situational attributions for criminal offending. Five survey items tapped dispositional attributions, and six items assessed situational attributions. Table 2 displays these items. The included items, in their positively worded form, closely parallel those used in previous studies (e.g., Cullen et al., 1985; Templeton and Hartnagel, 2012; Unnever et al., 2010). To generate bidirectional sets of questions, we reversed the direction of two of the dispositional items and four of the situational items. Several considerations needed to be taken into account in developing the reversals of the positive items (see Schuman and Presser, 1981). Most importantly, the new negative items could not be created by simply inserting the word "not" into the positive statements, and they had to be as extreme as the original items (Krosnick and Presser, 2010: 277). We accomplished this by changing a single word in the attribution items; whereas the positive items began with the word "most," the negative items instead began with the word "few" (see table 2).

In the case of respondents who received unidirectional questions, we created the *Dispositional attributions* scale by averaging across responses to the five dispositional items,

Table 2. Questions Measuring Respondents' Attribution Styles

Unidirectional Questions	Bidirectional Questions
Dispositional Attributions	
1. Most offenders commit crimes because they have evil or manipulative personalities and don't care about hurting others.	1. Unchanged.
2. Most offenders commit crimes simply because they have bad moral character.	2. Few offenders commit crimes simply because they have bad moral character.
3. Most offenders commit crimes because they have little or no self-control.	3. Unchanged.
4. Most offenders commit crimes simply because they are selfish people.	4. Few offenders commit crimes simply because they are selfish people.
5. Most offenders commit crimes because they are just too lazy to get a job to earn money.	5. Unchanged.
Situational Attributions	
1. Most offenders commit crimes because of outside influences (e.g., peer pressure, money problems, etc.).	1. Few offenders commit crimes because of outside influences (e.g., peer pressure, money problems, etc.).
2. Most offenders commit crimes because our society offers them little opportunity to get a job and make money.	2. Unchanged.
3. Most offenders commit crimes as a way of coping with poor living conditions (e.g., extreme poverty, violence in the home, marital problems, etc.).	3. Few offenders commit crimes as a way of coping with poor living conditions (e.g., extreme poverty, violence in the home, marital problems, etc.).
4. Most offenders commit crimes in response to inequality or social injustice.	4. Few offenders commit crimes in response to inequality or social injustice.
5. Most offenders commit crimes because their home lives as children were lacking in love, discipline, and supervision.	5. Unchanged.
6. Most offenders commit crimes because they were abused as children.	6. Few offenders commit crimes because they were abused as children.

NOTES: The specific question stem was as follows: "Now thinking about the CAUSES OF CRIME in this country, how much do you disagree or agree with each of the following statements?"

and we created the *Situational attributions* scale by averaging responses to the six situational items. We used the same approach for the respondents who received bidirectional questions, but before doing so, we reverse coded the responses to the negative items. Because our first hypothesis pertains to the effect of acquiescence on alpha coefficients, we wait to discuss the reliability of all of the scales used in this study in the Findings section.

We used 13 questions to measure respondents' support for specific crime policies. Six items gauged support for punitive policies, such as the death penalty, mandatory minimum sentences, and juvenile transfers. Seven items tapped support for rehabilitative policies, such as expanding educational and vocational programs in prison.[4] To increase comparability, the punishment items generally focused on serious offenders and the rehabilitation items pertained to incarcerated offenders. Our focus on serious crime, however, should have no influence on the experimental effect of manipulating the direction of the items. The response options ranged from 1 = strongly oppose to 6 = strongly support.

4. We included prison visitation as a rehabilitative policy because research has shown that inmates who are visited frequently are less likely to reoffend after they are released (Bales and Mears, 2008).

Table 3. Questions Measuring Respondents' Policy Support

Unidirectional Questions	Bidirectional Questions
Punitiveness	
1. Making sentences more severe for all crimes.	1. Unchanged.
2. Increasing the use of the death penalty for murders.	2. Reducing the use of the death penalty for murders.
3. Locking up more juvenile offenders.	3. Unchanged.
4. Increasing the use of mandatory minimum sentencing laws, like "Three Strikes," for repeat offenders.	4. Reducing the use of mandatory minimum sentencing laws, like "Three Strikes," for repeat offenders.
5. Keeping sex offenders who are still considered dangerous locked up past their original sentences.	5. Unchanged.
6. Trying more juvenile offenders as adults in adult courts.	6. Trying fewer juvenile offenders as adults in adult courts.
Rehabilitation	
1. Expanding counseling programs for offenders in prison.	1 Reducing counseling programs for offenders in prison.
2. Expanding job training programs for offenders in prison.	2. Unchanged.
3. Expanding educational programs for offenders in prison.	3. Reducing educational programs for offenders in prison.
4. Making it easier for all inmates to receive family visits.	4. Unchanged.
5. Expanding social skills training for offenders in prison.	5. Unchanged.
6. Expanding drug treatment services for offenders in prison.	6. Reducing drug treatment services for offenders in prison.
7. Helping offenders released from prison find jobs.	7. Unchanged.

NOTES: The specific question stem was as follows: "How much do you oppose or support each of these proposed crime policies?"

All of the items are shown in table 3. Similar to our approach with the attribution questions, we generated a bidirectional set of policy support items by changing a single word in roughly half of the punitive and rehabilitation items. In two punitive items, we changed the word "increasing" to "reducing," and in a third punitive item, we replaced the word "more" with "fewer." In three rehabilitation items, we simply switched the word "expanding" to "reducing." This approach reversed the direction of the statements from positive to negative but kept them as extreme as the originals. For the unidirectional group, we averaged the responses to the punitive items to create a *Punitiveness* scale, and we averaged responses to the rehabilitation items to create a *Rehabilitation* scale. For the bidirectional group, we first reverse coded the negative items and then averaged responses to create the respective scales.

RACIAL RESENTMENT

In the analyses, we include a measure of racial resentment as a potential mediator between attribution styles and policy support. Racial resentment, or symbolic racism, is a modern form of racism centered on a denial of the contemporary existence and effects of racial discrimination (Kinder and Sanders, 1996; Sears and Henry, 2003). It is among the strongest predictors of public views about crime policies (Pickett and Chiricos, 2012; Unnever, Cullen, and Jones, 2008; Unnever et al., 2010). Extant theory and research has

suggested that crime attributions should influence levels of racial resentment, which in turn should impact support for crime policies (Tam, Au, and Leung, 2008). Specifically, in the public mind, criminal offenders often are typified as being Black, and crime is understood to be a racial issue (Pickett et al., 2014; Unnever and Cullen, 2010).[5] It is therefore likely that for many Americans, causal attributions for crime largely amount to causal attributions for *Black* crime and, thus, constitute a form of what Sears and Henry (2003: 272) termed "racialized individualism," which involves the "concern that Blacks do not live up to conventional individualistic values." Sears and Henry (2003) theorized and empirically demonstrated that racialized individualism is a strong predictor of racial resentment, and its effect on policy support is largely mediated by racial resentment (see also Brandt and Reyna, 2012).

In the current study, *Racial resentment* is measured with responses (1 = strongly disagree, 6 = strongly agree) to five items from Henry and Sears's (2002) Symbolic Racism 2000 scale. In the questionnaire, these items were placed several pages after the questions asking about policy support and crime attributions. The exact wording of the items is provided in appendix A. All items have been used in previous studies to measure racial resentment (see, e.g., Pickett and Chiricos, 2012; Unnever and Cullen, 2010). The battery of racial resentment questions was bidirectional in both groups. Thus, before averaging across the items, we recoded responses so that higher values indicated greater racism.

CONTROL VARIABLES

In the multivariate models, we include several controls to account for the slight demographic differences across the two groups and to minimize omitted variable bias. All controls are measured identically in both the unidirectional and bidirectional groups. The control variables are described in detail in appendix A. Here, we note simply that we include controls for the key factors known to influence views about crime and justice (see Applegate et al., 2000; Unnever and Cullen, 2010), such as *Perceived risk* of victimization, *Perceived crime trend*, *News consumption*, *Conservatism*, and *Religiosity*. We also control for respondents' experience with prior *Family victimization, Education*, gender (female = 1), race (White = 1), and *Age* in years.

ANALYTIC STRATEGY

Our analyses unfold in several stages. In the initial stages, we evaluate differences in alpha coefficients and bivariate correlations across the two groups of respondents. In the later stages, we estimate a series of multivariate models predicting attribution styles and policy support. These outcome variables are continuous measures. For this reason, we use ordinary least squares (OLS) regression to estimate the models. Cases with missing values are dropped from the analyses using listwise deletion. Multicollinearity is not an issue in the models—the variance inflation factors do not exceed 2.00. For each model, we conducted White's general test for heteroskedasticity. In several cases, the test was significant. Accordingly, we estimate all models using robust standard errors.

When comparing the multivariate effects of different variables *within* the same group of respondents, we examine the standardized coefficients. However, to compare the

5. As Unnever, Cullen, and Jones (2008: 25) have explained, when many Americans think about crime, "the mental image that they have … is that of a menacing African American male."

multivariate results *between* the groups, we evaluate differences in the unstandardized coefficients. This is the most appropriate approach for two reasons. First, all variables are measured on the same numerical scales in both groups. Second, the standardized coefficients for the two groups are not comparable (Kim and Ferree, 1981). As Allison (1999: 191) has explained, "the well-known problem of comparing standardized coefficients for linear models across groups … [is] that such comparisons are potentially invalidated by differences in the standard deviations across groups."[6] It bears noting, however, that despite this issue, identical conclusions are reached in our experiment regardless of whether one compares the standardized or unstandardized coefficients.

FINDINGS

We open our analyses by investigating whether using unilaterally positive items to measure crime attributions and policy support may artificially inflate the alpha reliability coefficients of resultant scales. Table 1 displays the Cronbach's alpha coefficients for all scales in this study. Observe first that the alpha coefficients for the *Racial resentment* and *Perceived risk* scales, which are measured identically for both groups of respondents, do not vary in a consistent or substantial way. By contrast, and consistent with our first hypothesis, the alpha coefficients for the scales measuring attribution styles and policy support are consistently lower in the bidirectional group, where steps were taken to minimize the effects of acquiescence bias. Indeed, in the bidirectional group, the alpha coefficients for attribution scales are so low (alpha = .52 and .44) as to raise questions about whether these scales have acceptable internal consistencies for use in research. It bears emphasizing that these lower alphas were obtained simply by reversing the direction of a subset of the attribution items, so that respondents had to agree with one item and disagree with another to endorse a particular attribution style. The results conform exactly to what should be observed if the high alphas in the unidirectional group were a function of acquiescence bias.

The next portion of the analyses explores how acquiescence bias may influence conclusions about the hydraulic relation hypothesis. We first examine the bivariate correlations (not shown) between attribution styles and between *Punitiveness* and support for *Rehabilitation*. In the unidirectional group, the correlation between dispositional and situational attributions is –.17, and the correlation between the two types of policy support is –.38. The magnitude of these negative correlations may be deflated by the presence of positively-correlated measurement error introduced by acquiescent responding. Even still, these negative correlations provide some support for the hydraulic relation hypothesis. In the bidirectional group, however, where the effects of acquiescence bias are minimized, support for the hypothesis is considerably stronger. Among the respondents in this group, the respective correlations are –.33 and –.52. In the case of both attribution styles and policy support, the correlations are significantly different across the two groups of respondents ($z = 2.267$, $p = .023$; and $z = 2.502$, $p = .012$, respectively). Thus, operationalizing these constructs with bidirectional rather than unidirectional sets of questions substantially and significantly increases the size of the hydraulic relation between the respective attitudinal phenomena.

6. Williams (2009: 534) put it simply: "In OLS … if variances differ across groups, the standardization will also differ across groups, which makes [standardized] coefficients noncomparable."

Table 4. Multivariate Test of the Hydraulic Relation Hypothesis for Attribution Styles

Variables	DV = Situational Attribution Style					
	Unidirectional Group			Bidirectional Group		
	b	RSE	St. Coef.	*b*	RSE	St. Coef.
Attribution Style						
Dispositional attributions	−.087	(.055)	−.104	−.171**	(.049)	−.196
Control Variables						
Perceived risk	.081	(.046)	.098	−.012	(.032)	−.021
Perceived crime trend	−.070	(.044)	−.086	−.074*	(.032)	−.124
News consumption	−.005	(.021)	−.015	.000	(.014)	.000
Conservatism	−.186**	(.058)	−.200	−.155**	(.036)	−.232
Religiosity	.022	(.027)	.046	−.012	(.016)	−.032
Family victimization	.169	(.096)	.089	−.008	(.067)	−.006
Education	.005	(.049)	.005	.062*	(.031)	.089
Female	.266**	(.092)	.147	.022	(.065)	.016
White	−.062	(.119)	−.027	.086	(.073)	.055
Age	−.003	(.003)	−.055	−.001	(.002)	−.029
R^2		.124			.204	
N		379			393	

ABBREVIATIONS: *b* = unstandardized regression coefficient; DV = dependent variable; RSE = robust standard error; St. Coef. = standardized regression coefficient.
*$p < .05$; **$p < .01$ (two-tailed).

To supplement the bivariate analysis, we assess the multivariate relationships between the two attribution styles and between the two types of policy support. Table 4 displays the results of regressing *Situational attributions* on *Dispositional attributions* and the controls. Table 5 presents the results of regressing *Punitiveness* on *Rehabilitation* and the controls.[7] An inspection of table 4 reveals that the two attribution styles are not significantly related in the unidirectional group. In the bidirectional group, however, the effect of *Dispositional attributions* on *Situational attributions* is highly significant (p = .001), and the coefficient is nearly twice as large as that for the unidirectional group (b = –.171 vs. –.087, respectively). Similar results emerge for the relationship between the two types of policy support (table 5). To be clear, a significant hydraulic relation exists between *Rehabilitation* and *Punitiveness* in both groups, which likely reflects the fact that our measures focus specifically on policy support rather than on policy goals or utility judgments. However, the magnitude of the relationship is more than 50 percent larger in the bidirectional group than in the unidirectional group (b = –.405 vs. –.268, respectively).

It should be noted that when the Paternoster et al. (1998) test is used to evaluate the differences across the experimental groups in the regression coefficients for attribution styles and for policy support, these differences do not reach the

7. For the models shown in tables 4 and 5, it was an arbitrary decision as to which of the attribution scales and which of the policy support scales to treat as the outcome variables. Importantly, the results are unchanged if the *Dispositional attributions* and *Rehabilitation* scales are instead taken as the outcome variables.

Table 5. Multivariate Test of the Hydraulic Relation Hypothesis for Policy Support

Variables	DV = Punitiveness					
	Unidirectional Group			Bidirectional Group		
	b	RSE	St. Coef.	*b*	RSE	St. Coef.
Policy Support						
Rehabilitation	−.268**	(.069)	−.218	−.405**	(.054)	−.326
Control Variables						
Perceived risk	.013	(.057)	.012	.036	(.044)	.037
Perceived crime trend	.231**	(.052)	.204	.216**	(.048)	.213
News consumption	.039	(.024)	.078	.004	(.020)	.008
Conservatism	.413**	(.071)	.319	.290**	(.057)	.257
Religiosity	.020	(.033)	.029	.054	(.028)	.090
Family victimization	−.095	(.118)	−.036	.123	(.098)	.051
Education	−.166**	(.058)	−.128	−.068	(.044)	−.059
Female	.168	(.113)	.067	.056	(.092)	.025
White	−.080	(.136)	−.026	.145	(.109)	.055
Age	.002	(.004)	.028	−.000	(.003)	−.001
R^2		.337			.420	
N		379			393	

ABBREVIATIONS: *b* = unstandardized regression coefficient; DV = dependent variable; RSE = robust standard error; St. Coef. = standardized regression coefficient.
*$p < .05$; **$p < .01$ (two-tailed).

conventional threshold for significance ($z = 1.14$, $p = .254$ and $z = 1.56$, $p = .119$, respectively). Nonetheless, as noted above, the respective bivariate correlations differ significantly across the groups. In addition, the differences in the regression coefficients are both in the hypothesized direction and sizable, and the multivariate hydraulic relation between attribution styles is significant only in the bidirectional group. Taken together, then, we believe the results provide clear support for our second hypothesis.

The evidence presented thus far tells a consistent story. It suggests that when unidirectional batteries of questions are used to measure crime attributions and policy support, acquiescence bias artificially inflates alpha coefficients and artificially deflates the hydraulic relations between attribution styles and between punitiveness and support for rehabilitation. Attention now turns to an important unanswered question: Does the use of such questions also distort conclusions about the theoretical correlates of policy support? The findings presented in tables 6 and 7 suggest that the answer to this question is likely "yes." Table 6 displays the results of regressing *Punitiveness* on attribution styles, racial resentment, and the controls. Table 7 shows the comparable results for support for *Rehabilitation.*

We focus initially on the effects of attributions styles on policy support, net of the controls, and then subsequently we evaluate whether these effects are mediated by *Racial resentment*. Looking first at the results for *Punitiveness* (table 6), an inspection of models 1 and 3 reveals that for both groups of respondents, a large positive relationship exists between *Dispositional attributions* and *Punitiveness*. Notice, however, that among respondents in the unidirectional sample, there is not a significant negative relationship between *Situational attributions* and *Punitiveness*. This finding runs counter to the

Table 6. Regression Models Predicting Punitiveness with Attribution Styles and Racial Resentment

Variables	Unidirectional Group						Bidirectional Group					
	Model 1: Punitiveness			Model 2: Punitiveness			Model 3: Punitiveness			Model 4: Punitiveness		
	b	RSE	St. Coef.	*b*	RSE	St. Coef.	*b*	RSE	St. Coef.	*b*	RSE	St. Coef.
Racial Resentment	—	—	—	.230**	(.052)	.237	—	—	—	.296**	(.042)	.347
Attribution Styles												
Dispositional attributions	.519**	(.058)	.447	.429**	(.062)	.370	.599**	(.061)	.407	.456**	(.065)	.310
Situational attributions	−.085	(.068)	−.061	.012	(.068)	.009	−.234**	(.071)	−.139	−.152*	(.072)	−.090
Control Variables												
Perceived risk	.009	(.047)	.007	.009	(.045)	.008	.033	(.039)	.033	.042	(.035)	.043
Perceived crime trend	.225**	(.048)	.199	.198**	(.048)	.176	.194**	(.042)	.191	.160**	(.041)	.158
News consumption	.017	(.022)	.033	.026	(.022)	.052	.004	(.018)	.009	−.004	(.017)	−.009
Conservatism	.311**	(.062)	.240	.228**	(.063)	.176	.235**	(.052)	.208	.101	(.053)	.090
Religiosity	−.003	(.029)	−.004	−.011	(.028)	−.017	.048	(.026)	.079	.037	(.023)	.061
Family victimization	−.084	(.104)	−.032	−.136	(.100)	−.051	.082	(.090)	.034	.060	(.082)	.025
Education	−.173**	(.051)	−.133	−.164**	(.049)	−.126	−.069	(.041)	−.059	−.048	(.038)	−.042
Female	.141	(.099)	.056	.186	(.096)	.074	.152	(.085)	.067	.234**	(.078)	.104
White	.151	(.117)	.048	.005	(.118)	.002	.154	(.099)	.058	.022	(.096)	.008
Age	−.000	(.004)	−.000	.000	(.003)	.006	−.003	(.003)	−.040	−.001	(.003)	−.020
R^2		.472			.501			.508			.577	
N		379			379			393			393	

ABBREVIATIONS: *b* = unstandardized regression coefficient; RSE = robust standard error; St. Coef. = standardized regression coefficient.
*$p < .05$; **$p < .01$ (two-tailed).

Table 7. Regression Models Predicting Support for Rehabilitation with Attribution Styles and Racial Resentment

Variables	Unidirectional Group						Bidirectional Group					
	Model 1: Rehabilitation			Model 2: Rehabilitation			Model 3: Rehabilitation			Model 4: Rehabilitation		
	b	RSE	St. Coef.	*b*	RSE	St. Coef.	*b*	RSE	St. Coef.	*b*	RSE	St. Coef.
Racial Resentment	—	—	—	−.090	(.050)	−.114	—	—	—	−.236**	(.040)	−.343
Attribution Styles												
Dispositional attributions	−.187**	(.050)	−.198	−.152**	(.056)	−.161	−.238**	(.068)	−.201	−.125	(.066)	−.105
Situational attributions	.270**	(.066)	.239	.232**	(.066)	.205	.273**	(.071)	.201	.207**	(.070)	.153
Control Variables												
Perceived risk	−.083	(.047)	−.089	−.083	(.046)	−.089	−.046	(.041)	−.059	−.054	(.039)	−.068
Perceived crime trend	−.084	(.045)	−.092	−.074	(.044)	−.080	−.104*	(.042)	−.128	−.077	(.040)	−.094
News consumption	.034	(.019)	.081	.030	(.019)	.072	.001	(.017)	.003	.007	(.016)	.021
Conservatism	−.277**	(.063)	−.262	−.244**	(.066)	−.232	−.224**	(.045)	−.246	−.118*	(.045)	−.129
Religiosity	.038	(.027)	.069	.042	(.027)	.075	−.015	(.022)	−.031	−.007	(.021)	−.014
Family victimization	.195*	(.093)	.090	.215*	(.094)	.100	.122	(.083)	.064	.140	(.079)	.073
Education	.117*	(.046)	.111	.114*	(.046)	.107	.022	(.044)	.023	.006	(.042)	.006
Female	.159	(.096)	.078	.141	(.096)	.069	.013	(.082)	.007	−.052	(.076)	−.028
White	−.241*	(.113)	−.095	−.184	(.114)	−.072	−.056	(.101)	−.027	.049	(.098)	.023
Age	.007*	(.003)	.106	.007*	(.003)	.104	.004	(.003)	.072	.003	(.003)	.051
R^2		.313			.320			.313			.379	
N		379			379			393			393	

ABBREVIATIONS: *b* = unstandardized regression coefficient; RSE = robust standard error; St. Coef. = standardized regression coefficient.
*$p < .05$; **$p < .01$ (two-tailed).

predictions of attribution theory but is consistent with the results obtained in several previous studies (Carroll et al., 1987; Mascini and Houtman, 2006; Unnever et al., 2010).[8] By contrast, and although the respective coefficients do not differ significantly across the experimental groups ($z = 1.53$, $p = .126$), the results for the bidirectional sample provide a much different picture. In the bidirectional sample, the effect of *Situational attributions* on *Punitiveness* is highly significant ($p = .001$) and is more than twice as large as that for the unidirectional sample ($b = –.234$ vs. $–.085$, respectively). In addition, the bivariate correlations (not shown) between these two variables differ significantly across the groups ($r = –.411$ vs. $–.203$, respectively, $z = 3.20$, $p = .001$). Thus, the results obtained after taking steps to control acquiescence bias are more supportive of attribution theory.

The differences between the two groups in the effects of attribution styles on support for *Rehabilitation* are less pronounced (models 1 and 3 in table 7). Among both groups, the endorsement of *Dispositional attributions* is negatively associated with support for *Rehabilitation*, and the endorsement of *Situational attributions* is positively associated with support for *Rehabilitation*. The negative relationship between *Dispositional attributions* and *Rehabilitation* is slightly larger for the bidirectional group than for the unidirectional group. This finding is consistent with the idea that uncontrolled acquiescence bias may artificially reduce the magnitude of negative relationships. However, there is another more notable difference between the two groups. In the unidirectional group, only three of the seven factors that predict support for *Rehabilitation—Dispositional attributions*, *Conservatism*, and *Education*—also are associated with *Punitiveness* (see model 1 in tables 6 and 7). By contrast, for the bidirectional group, the same four variables—*Dispositional attributions*, *Situational attributions*, *Perceived crime trend*, and *Conservatism*—are associated, in opposite directions, with support for *Rehabilitation* and *Punitiveness* (see model 3 in tables 6 and 7). Stated differently, when bidirectional questions are used to construct the scales, the two types of policy support have similar, but reversed, relational properties.

The final portion of the analyses evaluates the question of whether acquiescence bias influences the extent to which *Racial resentment* mediates the effects of attribution styles on policy support. As an initial step, we estimate preliminary models (not shown) that examine whether crime attributions are correlated with racial resentment. For both groups of respondents, the two attribution scales strongly predict *Racial resentment*; indeed, their standardized effects are larger than that of any other variable examined except *Conservatism*.[9] The endorsement of *Dispositional attributions* is associated with increased *Racial Resentment*, whereas the endorsement of *Situational attributions* is associated with lower levels of *Racial resentment*.

Having determined that crime attributions are associated with *Racial resentment*, we now investigate whether the effects of the former on policy support are mediated by the

8. Unnever et al. (2010: 446) found a significant positive relationship between situational attributions and punitiveness.

9. Our reliance on cross-sectional data prevents us from empirically establishing the causal order of the relationship between crime attributions and racial resentment. We thus draw on extant theory to make causal-order assumptions.

latter. Models 2 and 4 in tables 6 and 7 display the relevant regression results. The findings are summarized easily. In the bidirectional group, *Racial resentment* is the strongest predictor of both *Punitiveness* and support for *Rehabilitation*, and it mediates a sizeable proportion of the effects of both attribution styles on both outcome variables. Specifically, in the bidirectional group, *Racial resentment* mediates 24 percent (Sobel test: $b = .142$, $z = 4.956$, $p < .001$) of the total effect of *Dispositional attributions* on *Punitiveness* and 35 percent (Sobel test: $b = –.082$, $z = –2.992$, $p = .003$) of the effect of *Situational attributions* on this outcome variable. Additionally, among respondents in this group, *Racial resentment* mediates 48 percent (Sobel test: $b = –.113$, $z = –4.521$, $p < .001$) of the total effect of *Dispositional attributions* on support for *Rehabilitation* and 24 percent (Sobel test: $b = .065$, $z = 2.888$, $p = .004$) of the effect of *Situational attributions* on this outcome variable.[10,11]

This is not the case for the unidirectional group. Indeed, the findings for the respondents in this group are dramatically different than those for the bidirectional group. In this group, *Racial resentment* is positively associated with *Punitiveness* and mediates roughly 17 percent (Sobel test: $b = .089$, $z = 3.961$, $p < .001$) of the effect of *Dispositional attributions* on this outcome variable, but the *Dispositional attributions* scale remains the strongest predictor of punitive attitudes. As before, the *Situational attributions* variable is not associated with *Punitiveness*. What is most striking, however, is that in this group, *Racial resentment* is not related significantly to support for *Rehabilitation*, net of attribution styles and the controls. In the unidirectional group, then, neither *Situational attributions* nor *Racial resentment* is associated in a similar way with both *Punitiveness* and support for *Rehabilitation*. Additionally, the coefficients for the effect of *Racial resentment* on support for *Rehabilitation* differ significantly ($z = 2.280$, $p = .023$) across the experimental groups.

In short, measuring crime attributions and policy support with bidirectional rather than with unidirectional batteries of questions yields a pattern of multivariate relationships that is less supportive of the pragmatism thesis. Specifically, the pragmatism thesis argues that rather than constituting converses of each other, punitiveness and support for rehabilitation are independent attitudinal phenomena with distinct relational properties (Mascini and Houtman, 2006). Contrasting this argument, the results show that in the bidirectional group, racial animus, stemming in part from crime attributions, is the principal determinant of views about *both* punitive policies and rehabilitative programs. It is not in the unidirectional group. Taken together, the multivariate findings provide strong support for our third hypothesis.[12]

10. In supplementary analyses (available in the online supporting information; see footnote 11), we used structural equation models to confirm that attribution styles have significant indirect effects on policy support through racial resentment.
11. Additional supporting information can be found in the listing for this article in the Wiley Online Library at http://onlinelibrary.wiley.com/doi/10.1111/crim.2011.52.issue-2/issuetoc.
12. The effects of several of the control variables, including *Conservatism* and *Education*, on *Punitiveness* in table 6, and on support for *Rehabilitation* in table 7, differ in magnitude and significance across the experimental groups. It is possible that these differences reflect either the effect of a given variable on the tendency to respond acquiescently to questions about crime and justice or the influence of acquiescence bias on patterns of mediation. However, none of these differences are significant, nor were they hypothesized a priori; thus, we suggest caution when interpreting them.

DISCUSSION AND CONCLUSION

Several previous studies have failed to find a hydraulic relation—a sizable negative correlation—between dispositional and situational attributions, or between punitiveness and support for rehabilitation. On the basis of those studies, scholars have largely dismissed the hydraulic relation hypothesis (Cullen et al., 2000; Mascini and Houtman, 2002, 2006; McCorkle, 1993; Pickett, Mancini, and Mears, 2013) and have concluded that "most Americans … are pragmatic in their approach to crime control" (Unnever et al., 2010: 449)—to them, "the idea of punishment *versus* rehabilitation is a false dichotomy" (Bishop, 2006: 657). Our results suggest that it is premature to draw this conclusion. Specifically, the findings indicate that the failure in prior studies to account for acquiescent responding makes it difficult to determine whether extant research findings present an accurate picture of popular sentiments.

To reiterate, most previous studies have used sets of Likert questions that are all positively worded to measure crime attributions and policy support. This measurement approach, however, allows respondents to obtain high values on survey items and resulting scales simply by adopting an acquiescence response style, in which they answer questions affirmatively regardless of content (Krosnick, 1999). Our findings suggest that using this measurement approach results in 1) consistently inflated alpha coefficients for scales measuring crime attributions and policy support; 2) consistently deflated hydraulic relations between attribution styles, and between punitiveness and support for rehabilitation; and 3) inconsistent correlations among attribution styles, racial resentment, and policy support.

Our findings have important theoretical and empirical implications. First, the results show that when scales are measured with bidirectional sets of questions, which is an approach that reduces the influence of acquiescence bias on findings, the hydraulic relation hypothesis is consistently supported, and thus, there is little evidence of pragmatism. This is not the case in the unidirectional group. Among these respondents, we find no relationship between attribution styles, net of the controls, and a weaker hydraulic relation between punitiveness and support for rehabilitation than in the bidirectional group.

In contrast to the pragmatism thesis, the results also show that when bidirectional question batteries are used, punitiveness and support for rehabilitation have similar, but reversed, relational properties. For example, in the bidirectional group, there is consistent support for attribution theory across both types of policy support—dispositional attributions are positively related to punitiveness and negatively related to support for rehabilitation, whereas situational attributions are negatively related to punitiveness and positively related to support for rehabilitation. This does not occur in the unidirectional group, where situational attributions do not have the hypothesized effect on punitiveness. Likewise, in the bidirectional group, we find consistent support for Sears and Henry's (2003) theory of racialized individualism. Those authors theorized that the effects of racialized individualism on policy views should be mediated in part by racial resentment. Assuming, as we do, that crime attributions tap a dimension of racialized individualism, this is exactly what occurs in the bidirectional group for both types of policy support. Specifically, among these respondents, crime attributions strongly predict racial resentment, and racial resentment, in turn, partially mediates the effects of attribution styles on both punitiveness and support for rehabilitation. The results are much different in the

unidirectional group, where racial resentment is not significantly related to support for rehabilitation, net of attribution styles and the controls.

Note that in the bidirectional group, but not in the unidirectional group, we find that racial resentment is the strongest predictor of both punitiveness and support for rehabilitation. This finding is consistent with those emerging in prior studies that use forced-choice questions, which reduce acquiescent responding, to gauge policy support (Unnever and Cullen, 2010; Unnever, Cullen, and Jones, 2008). This increases our confidence that in our experiment, we obtained more valid findings when using bidirectional rather than unidirectional batteries of questions to measure attribution styles and policy support.

A key empirical implication flowing from our findings is that researchers examining attribution styles and policy support should take steps to minimize the effects of acquiescence bias on results. Recall that the strongest predictor of the effect of acquiescent responding on scale scores is the degree to which scales are unbalanced (Baumgartner and Steenkamp, 2001). Thus, a simple way to reduce acquiescence bias is to use bidirectional sets of questions to construct scales, as we do in the current study. One limitation of the method, however, is that it does not remove acquiescent responses; rather, it merely moves them to the middle of scales, where they have less influence on findings (Krosnick and Presser, 2010).[13] An alternative approach can be used when surveys contain several batteries of questions measuring unrelated constructs. Here, researchers can construct an indicator of the extent to which respondents answer affirmatively across heterogeneous items. This measure can then be used to remove (through residualization) the variance in substantive scales that results from acquiescent responding (Baumgartner and Steenkamp, 2001). Also, it is possible, when several balanced sets of questions are available, to use structural equation modeling to identify the latent acquiescence factor underlying responses to survey items (Billiet and McClendon, 2000).

Perhaps the best approach for reducing acquiescence bias, however, is to develop better survey questions for measuring crime attributions and policy support (see Krosnick and Presser, 2010; Saris et al., 2010). This is particularly important in the case of crime attributions because it is not clear that the types of questions that are typically used to measure attribution styles—statements about why "most offenders commit crimes"—are ideal. In previous investigations, the use of such questions has generally resulted in scales with alpha coefficients below the conventional threshold (.70) for acceptable reliability (see, e.g., Carroll et al., 1987; Cochran et al., 2003; Grasmick and McGill, 1994). And our experiment shows that when bidirectional sets of these questions are used, the reliability of the resulting scales is even lower.

A fruitful way forward might be to use sets of "item-specific" survey questions (Krosnick and Presser, 2010; Saris et al., 2010) to measure crime attributions and policy support. Item-specific questions are those that include response options that relate directly to the specific question stem. The evidence suggests that such questions reduce acquiescent responding and increase the overall quality of responses (Saris et al., 2010). For

13. Another potential limitation of using this method is that even when bidirectional question batteries are used, acquiescent responding can deflate the negative correlations between positively and negatively worded items (Green and Citrin, 1994). For this reason, our results likely provide a conservative estimate of the influence of acquiescence bias on findings.

instance, to measure crime attributions with item-specific questions, researchers could replace items asking respondents how much they disagree/agree with statements like "most offenders commit crimes as a way of coping with poor living conditions," with questions such as follows:

1. Thinking about the criminals in this country, how many would you say commit their crimes as a way to cope with poor living conditions: none, a few, a good bit, or most?
2. How often are poor living conditions the primary reason that someone commits a crime in this country: very often, often, sometimes, rarely, or never?

One limitation of our study is that we analyze data from members of an opt-in online panel. Thus, our findings should not be interpreted as demonstrating an absence of pragmatism about criminal justice among the American public. Rather, they only illustrate the problems with the methodology used in previous studies to test the pragmatism thesis. It is the case, however, that we found little evidence of pragmatism among the Internet panelists in our experiment once steps were taken to minimize acquiescence bias. If this finding replicates with a representative sample of Americans, then the implication would be that at the individual level, views about criminal justice generally fall on a continuum from punitiveness to nonpunitiveness, where nonpunitiveness entails support for expanding rehabilitative programming. This would suggest that at any given point in time, most individual citizens have a directional preference—involving *either* a desire to expand punitive policies *or* a desire to expand rehabilitative policies—for how they would like policy makers to deal with the crime problem (see, e.g., Bernard and Kurlychek, 2010). Interestingly, prior research has shown that at the macro level, public mood about criminal justice does fall on a continuum from punitiveness to nonpunitiveness (Nicholson-Crotty, Peterson, and Ramirez, 2009; Stimson, 1999, 2004) and has become less punitive in recent years (Ramirez, 2013).

Also, it is important to note that because the respondents in our experiment were volunteer panelists who had agreed to participate regularly in surveys, they may have been more motivated to respond accurately to survey questions, and they may have had more experience at doing so, than members of the general public (see Chang and Krosnick, 2009). Relative to most types of respondents, then, the participants in our experiment may have been less likely to adopt an acquiescent response style. Thus, our results may underestimate the extent to which such stylistic responding biases findings when unidirectional batteries of questions are used to measure views about crime and justice.

We close by emphasizing that whereas our focus has been restricted to examining the influence of different measurement approaches on conclusions about public pragmatism and the theoretical correlates of policy support, the effects of acquiescence bias are of broader concern. The practice of using unidirectional sets of Likert questions to construct scales is widespread in the criminological literature. For example, this is the typical method used to operationalize concepts such as low self-control (Grasmick et al., 1993), legal cynicism (Kirk and Matsuda, 2011; Sampson and Bartusch, 1998), endorsement of the code of the street (Mears et al., 2013; Stewart and Simons, 2010), and procedural justice (Baker et al., 2014; Mazerolle et al., 2013; Tankebe, 2013). However, researchers using such measures have rarely considered how acquiescent responding may bias findings.

Given the results observed in our study and elsewhere (see Krosnick, 1999), we believe this is an oversight.

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Appendix A: QUESTION WORDING FOR RACIAL RESENTMENT AND CONTROLS

Racial resentment is measured with the following five items:

1. "There is a lot of discrimination against Blacks in the U.S. today, limiting their chances to get ahead."
2. "It's really a matter of some people not trying hard enough; if Blacks would only try harder they could be just as well off as Whites."
3. "Irish, Italians, Jewish, and many other minorities overcame prejudice and worked their way up. Blacks should do the same without any special favors."
4. "Over the past few years, Blacks have gotten less than they deserve."

5. "Generations of slavery and discrimination have created conditions that make it difficult for Blacks to work their way out of the lower class."

Perceived risk of victimization is measured as the average of responses to the five items in the following question:

> On a scale from 1 to 7, where 1 means "not at all likely" and 7 means "very likely," how likely is it that someone will try to commit the following crimes against you or a member of your family in the next five years: 1) Break into your house? 2) Beat you up on the street? 3) Rob or mug you? 4) Rape or sexually assault you? 5) Murder you?

The variable *Perceived crime trend* is measured with responses (1 = decreased greatly, 5 = increased greatly) to the following question: "In your best judgment, how much has the crime rate in the U.S. decreased or increased in the past five years?" The *News consumption* variable gauges the number of days each week the respondent is exposed to national evening news. The specific question was as follows: "In a typical WEEK, on how many days do you do each of the following … watch national evening news like CNN?" *Conservatism* is a measure of respondents' political ideology. It is measured with the question: "How would you describe yourself politically?" The response options were 1 = very liberal, 2 = liberal, 3 = middle of the road, 4 = conservative, and 5 = very conservative. *Religiosity* is a measure of the salience of religion in respondents' lives. It is measured with the following item: "How important would you say religion is in your life?" The responses options were 1 = very unimportant, 2 = unimportant, 3 = somewhat unimportant, 4 = somewhat important, 5 = important, and 6 = very important. We control for prior *Family victimization* using a dichotomous indicator equal to responses (0 = no, 1 = yes) to the following question: "Over the past five years, has anyone in your immediate family been the victim of a crime?" *Education* is measured as 1 = less than high-school degree, 2 = high-school degree, 3 = some college, 4 = college degree, and 5 = graduate degree.

SUPPORTING INFORMATION

Additional Supporting Information may be found in the online version of this article at the publisher's web site:

Supporting Information S1