What can we learn about police attitudes from four decades of the General Social Survey?

A comment on Roscigno and Preito-Hodge (2021)*

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1. Link to the version submitted on 22 Feb. 2021
2. Link to the editors' invitation to revise and resubmit: 26 Apr. 2021
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   - Post-rejection email correspondence and request for appeal to editorial board
   - Annotated rejection email with my responses to editors
   - Emails from Editor Gullickson (appeal request denied 23 Aug. 2021)

Abstract

Roscigno and Preito-Hodge (2021, RPH) compare police and non-police in the 1984-2018 GSS to support claims that police "uniquely believe they should receive more funding and have the right to use physical force against citizens", and are "distinctly racist". The topics of police racism and use of force involve important questions that deserve careful attention. This note shows RPH's nominal sample is not representative of the target population, and that estimates used to support their core claims are an artifact of selective reporting. For example, RPH report one significant difference on a single item selected from a four-part question about racial inequality, and another on a single item selected from a five-part question about police use of force. Applying RPH's model specifications to the unreported items from these questions produces estimates that do not support their argument. Additional analyses challenge RPH's claims that police are a homogenous group with uniquely/distinctly negative attitudes.

Note: the estimates in this note are presented to illustrate flaws in the claims made by Roscigno and Preito-Hodge (2021, RPH). These estimates, and those reported in RPH, should not be applied to the nominal population of U.S. police.

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Roscigno and Preito-Hodge (2021) recently published a provocative paper that claims police officers uniquely believe they should receive more funding, have the right to use physical force against civilians, and are distinctly racist. To provide evidence for these claims, RPH pool across 22 waves of the General Social Survey (1984-2018) and compare police respondents ($n < 180$) with non-police respondents ($n > 27,000$) using a series of logistic regression models. RPH report statistically significant results on four survey items selected from the GSS core: 1) support for law enforcement spending; 2) whether one can imagine a situation in which they would approve of a policeman striking an adult male; 3) spending on assistance to Blacks; and 4) whether Black-White inequality is mainly due to discrimination.

The paper has received widespread attention, and currently ranks in the top 5% of all research outputs, and first among outputs of similar age published in *Socius*.[1] The paper does not meet the contemporary replication standard for empirical research – there are no publicly available replication materials that would allow a third-party to comprehend, replicate, and evaluate all reported results without communication with the authors (King, 1995). However, the fact that the original analyses were conducted on a publicly available dataset allows for replication of the core findings. The analyses reported in this note – conducted on the same dataset and using the same estimation approach – raise serious concerns about the validity of RPH's analyses and core claims.

First, the sample of police respondents RPH obtain by pooling the 1984-2018 GSS series is a poor approximation to their inferential target of U.S. police officers. For example, more than 20% of the police sample is retired or unemployed, 81% are White, more than half come from areas with a population of under 25,000, and only 25% belong to a union. According to the latest available estimates from the Bureau of Justice Statistics, the officer population is 72% White, 25% are in areas with fewer than 25,000 residents, and over 60% are union members.[2] Further, only 41 police respondents answered all four of RPH's items due to the GSS question rotation scheme. Each of RPH's estimates therefore apply to a different subset of police respondents within their pooled sample.

Second, significant differences on the two GSS items RPH use to support claims that police are "distinctly racist" do not replicate on similar GSS items. Analyses reported here – using RPH's same model specifications – show that the first difference does not replicate on an alternate version of the same question, and the second is distinct to a single measure selected from a four-part question. Differences on other available measures, including behaviorally validated indicators of racial prejudice, are also indistinguishable from zero. Moreover, differences on the two items RPH use to infer police are distinctly racist are not unique to police: these same differences are prevalent across a wide-variety of other occupational sub-groups.

Third, RPH report significant differences on two survey items related to spending on law enforcement and use of force to support claims that police "uniquely believe that they should receive more funding and have the right to use physical force against civilians" (c.f. abstract). Again, differences between occupational sub-groups and other GSS respondents are not unique to police. The differences RPH report to support claims about use of force also apply to a

[1] Altmetric scores as of 31 January 2021. See https://sage.altmetric.com/details/98611505#score

[2] Law Enforcement Management and Administrative Statistics 2016 (Hyland and Davis, 2019)

single item selected from a five-part question. Analysis of the unreported items, using RPH's same model specification, suggest police respondents are less supportive of unjustified uses of force (e.g., striking a suspected murderer), and more supportive of justified uses of force (e.g., striking a person who was physically attacking an officer). Analyses of other GSS spending items show that other groups, like teachers and scientists, also support more occupation-specific funding when compared to all other GSS respondents.

This note is organized as follows. Section 1 offers a critique of RPH's strategy of pooling GSS cross-sections to make inferences about rarely sampled sub-groups, and shows the pooled sample of police respondents is not representative of RPH's target population on observable characteristics. Section 2 offers a critique of RPH's use of "iterative interaction modeling" to select final models for reporting, raises concerns about selective reporting, and replicates RPH's models on a broader universe of similar question items from the GSS core. Section 3 offers a critique of RPH's claims about the uniqueness and distinctness of police as a group, and Section 4 concludes.

1 Pooling cross-sections of rarely sampled sub-groups

RPH use GSS respondents' occupational indicators to construct a sub-sample of "patrol officers, detectives, and those in supervisor policing positions" (p. 5). RPH report that pooling across 22 waves of the 1984-2018 GSS series yields a sample of more than 20,000 non-police and "between 135 and 179 police officers, depending on the particular outcome being examined, and comparative analyses of cops versus the general population" (p. 5). RPH claim that "given the cross-sectional nature of [the pooled sample], we are able to explicitly examine whether police are in fact distinct and the degree to which racist attitudes and 'blue' occupational interests uniquely and jointly exist" (p. 2).

Although the GSS provides a good approximation to the target population of U.S. adults in most years, the survey design does not imply representative coverage of rarely sampled occupational sub-groups.[3] Yet RPH generalize all their claims about the distinct and unique nature of police attitudes to the entire population of U.S. police officers. They offer no caveats about whether their pooled sample is representative of the target population over the 1984-2018 period, or in any particular time period.

Pooling independent cross-sections could, in theory, increase the precision of RPH's estimators if two assumptions hold: 1) each cross-section is a random sample of the target population; 2) the relationships of interest – in this case attitudes among police and non-police – are temporally stable. The validity of RPH's extrapolations from the pooled sample to the population of U.S. police officers rest crucially upon these strong assumptions. If they do not hold, estimators applied to pooled cross-sections are inconsistent and biased in unknown directions (see e.g., Wooldridge, 2006, Chapter 13).

Neither of these assumptions is directly testable, but both have important testable implications. If the first assumption holds, then pooling should yield a representative sample of U.S. police. If the second holds, then police officers' attitudes are homogeneous across time;

[3]The GSS is a complex survey and the design effect is typically 1.5, which implies a 1,500 person GSS sample is roughly equivalent to a random sample of 1,000.

for example, those from the 1985 survey are exchangeable with cops from the 2018 survey.

1.1 Does pooling yield a representative sample of U.S. police?

If each cross-section of police in the GSS is a random sample of police officers, then pooling across the 1984-2018 series combines multiple random samples to produce a single random sample of the same target population. Therefore, the pooled sample should closely match the target population on observed (and unobserved) characteristics.

Although the United States does not conduct a census of police officers, population estimates can be obtained from the Law Enforcement Management and Administrative Statistics (LEMAS) survey. This survey, conducted periodically by the Bureau of Justice Statistics since 1987, collects administrative data on officer demographics from all law enforcement agencies that employ 100 or more full-time sworn officers, and supplements this with a nationally representative sample of smaller agencies.[4]

Table 1 compares demographic characteristics of police respondents[5] in the pooled sample with estimates from the most recent version of the LEMAS survey (see Hyland and Davis, 2019). Table 1 shows meaningful discrepancies between the pooled sample of police respondents and RPH's target population of U.S. police officers. For example, the pooled sample is 81% White, 52% come from areas with a population of under 25,000, and only 25% belong to a union. According to the estimates from LEMAS 2016, the officer population is 72% White, 25% are in areas with fewer than 25,000 residents, and over 60% are union members.

Importantly, however, the LEMAS estimates apply to the population of employed police officers. As Table 1 demonstrates, more than 20% of the pooled GSS sample is either retired or unemployed, which may partly explain why roughly 18% are aged 60+. Overall, the pooled sample of police respondents is disproportionately White, male, rural, and not employed as a police officer at the time of their GSS interview. Given the wide variation in officer characteristics by region (see Hyland and Davis, 2019), and the fact that the majority of U.S. police officers are employed by departments that serve more than 100,000 persons, the pooled sample is a poor approximation to RPH's target population.

Even within this pooled sample, RPH's inferences are further restricted to different subsets of police that provided responses to the four GSS items selected by RPH. Table 2 shows the response distribution for each of RPH's items in the pooled sample, and Figure 1 shows the frequency of observations among police for each question-year cluster. Although pooling

[4] The unit of analysis for the LEMAS survey is the law enforcement agency, rather than the individual officer. Unlike the GSS, the LEMAS survey cannot be used to study individual attitudes. It is, however, the most reliable source of national data on police officer demographics, and serves as the sampling frame for nationally representative surveys of individual officers conducted by polling organizations such as Pew (see https://www.pewresearch.org/social-trends/2017/01/11/behind-the-badge-methodology/). Response rates to LEMAS compare favorably to the GSS response rates of approximately 70%. In the most recent version of LEMAS, response rates were 80%. See https://bjs.ojp.gov/data-collection/law-enforcement-management-and-administrative-statistics-lemas for an overview of LEMAS survey methodology.

[5] Following RPH's coding scheme, police in this note are defined as all individuals in the GSS with the following `occ10` codes: 3710 (First-Line Supervisors Of Police And Detectives), 3850 (Police Officers), and 3820 (Detectives And Criminal Investigators). See Morgan (2017) for mappings between occupations and the GSS codes.

across the 1984-2018 series yields a total of 277 police respondents, only 41 were asked all four of RPH's items: 261 were administered at least one, 181 were asked two or more, and 135 were asked 3 or more.[6]

In sum, this casts serious doubt on the assumption that each GSS wave is a random, albeit small, sample of U.S. police officers. The estimates for each GSS item that RPH report instead correspond to a different sub-group of respondents within a non-representative convenience sample. RPH's extrapolations to the broader population of U.S. police officers are therefore contentious at best.

[6] The distribution of valid observations is not uniform across questions within any given year due to the GSS's rotation scheme and, after 1988, the introduction of split-ballot designs. Under this design, 3 rotations occur across random sub-samples (called "ballots") within each survey rather than across surveys. See `http://gss.norc.org/Lists/gssFAQs/DispForm.aspx?ID=9`, and `http://www.gss.norc.org/documents/codebook/GSS_Codebook_AppendixQ.pdf`. From 1994 onward, the GSS has used a dual sample design with two versions for each wave, which explains the increase in observations from 1994 onward. Both `natcrimy` and `natracey` were asked to the same number of respondents in each wave. Questions `polhitok` and `racdif1`, however, have appeared on different ballots since 1988 (`polhitok`: B,C; `racdif1`: A,B,C in 1990 and A,B for all others).

Table 1: Background characteristics for the pooled GSS sample and LEMAS estimates for the police population

	Target	**RPH Sample**			**Target**	**RPH Sample**	
	LEMAS	Weighted	Unweighted		LEMAS	Weighted	Unweighted
Race				**Region**			
White	0.72	0.81	0.81	*Midwest*	0.21	0.27	0.26
Black	0.11	0.12	0.14	*Northeast*	0.21	0.16	0.16
Other	0.17	0.06	0.05	*South*	0.38	0.38	0.41
Sex				*West*	0.20	0.19	0.17
Male	0.88	0.82	0.81	**Population size (in thousands)**			
Female	0.12	0.18	0.19	*1000 +*	0.22	0.04	0.05
Race x Sex				*500-999*	0.11	0.05	0.04
White Male	0.64	0.68	0.67	*250-499*	0.08	0.04	0.05
Black Male	0.09	0.09	0.10	*100-250*	0.11	0.09	0.10
Other Male	0.14	0.05	0.04	*50-100*	0.11	0.12	0.11
White Female	0.07	0.14	0.14	*25-50*	0.12	0.13	0.12
Black Female	0.03	0.03	0.04	*10-25*	0.13	0.25	0.26
Other Female	0.03	0.01	0.01	*<10*	0.13	0.27	0.29
Education				**Union member**			
Graduate	-	0.06	0.06	*No*	0.31	0.73	0.75
Bachelor's	-	0.26	0.26	*Yes*	0.66	0.27	0.25
Associate's	-	0.17	0.17	*Unknown*	0.03	-	-
High school	-	0.50	0.51	**Age**			
No High School	-	0.02	0.02	*18-23*	-	0.03	0.03
Employment				*24-29*	-	0.14	0.14
Full-time	-	0.74	0.74	*30-39*	-	0.29	0.30
Part-time	-	0.02	0.02	*40-49*	-	0.25	0.22
Retired	-	0.18	0.19	*50-59*	-	0.12	0.12
Unemployed	-	0.05	0.05	60+	-	0.18	0.19

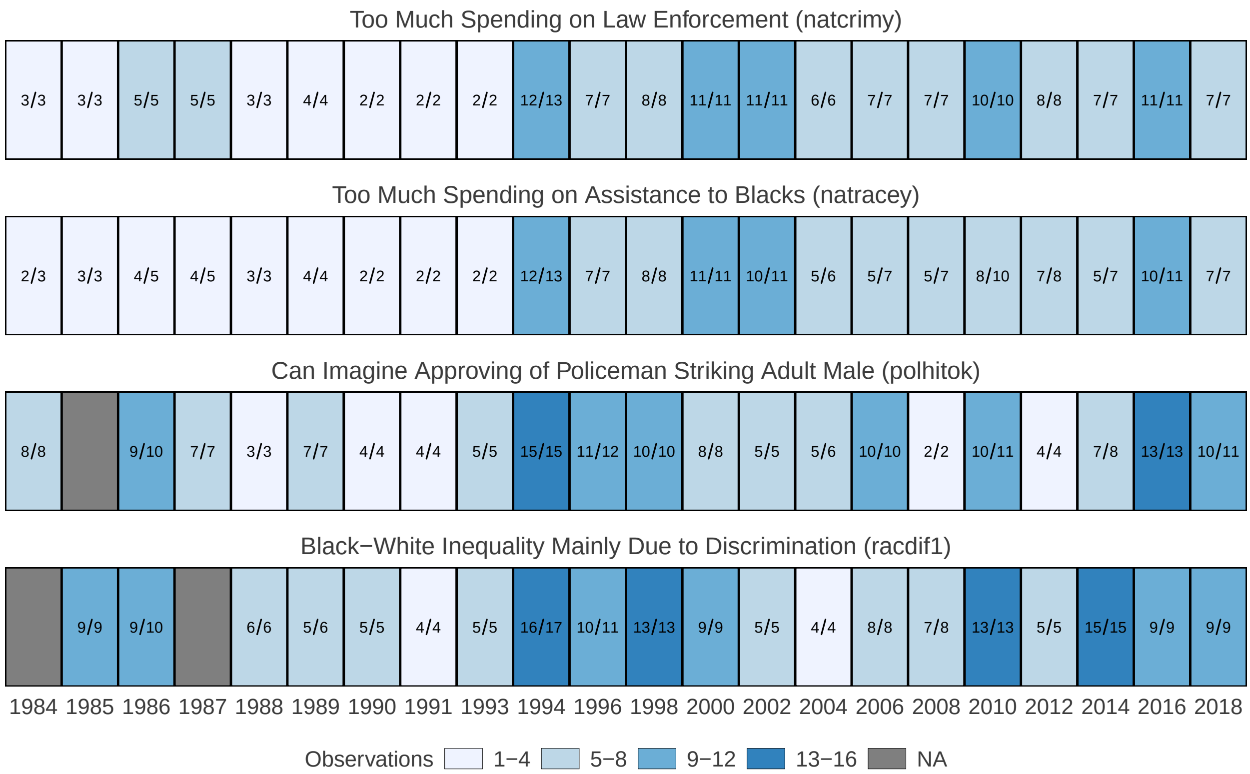
Notes: Estimates for the pooled sample are based on all 277 police in the 1984-2018 GSS. Estimates for the police population come from Law Enforcement Management and Administrative Statistics (LEMAS), 2016 (see Hyland and Davis, 2019). Estimates for union members in LEMAS are based on departments that have collective bargaining agreements and may be an underestimate. Other sources have estimated police unionization rates between 75-80% (see DeLord and York, 2017).

Table 2: Distribution of police responses in the pooled sample for RPH's items

	Too little	*About right*	*Too much*	*Don't know/Refused*	*Not asked*
`natcrimy`	113	22	6	1	135
`natracey`	29	54	43	16	135
		Yes	*No*	*Don't know/Refused*	*Not asked*
`polhitok`		144	13	6	114
`racdif1`		47	119	5	106

Notes: `natcrimy/natracey`: "Are we spending too much, too little, or about the right amount on [Law Enforcement/Assistance to Blacks]?" RPH recode as binary ("Too much" = 1, "About right" = 0, "Too little" = 0). `polhitok`: "Are there any situations you can imagine in which you would approve of a policeman striking an adult male citizen?" `racdif1`: "On the average Blacks have worse jobs, income, and housing than white people. Do you think these differences are mainly due to discrimination?"

Figure 1: Police respondents observed in the 1984-2018 series for RPH's items



Notes: text within each question-year cluster denotes answered/asked. Prior to 1988 the GSS employed a rotation design. Under the GSS "split-ballot" design (1998-2018), 3 rotations occur across random sub-samples ("ballots") within each wave rather than across waves. `natracey` and `natcrimy` have appeared on all three ballots (A,B,C) since 1984. `polhitok` has appeared on (B,C) since since 1988. `racdif1` appeared on (A,B,C) in 1990; and (A,B) for all other waves since 1988. GSS items `racdif1` and `natracey` used the phrase "Negroes/Blacks" until 1993 and "African-Americans/Blacks" thereafter.

1.2 Are attitudes among police respondents stable across time?

As RPH note in their section titled "General Attitudes toward Policing and Race across Time" (pp. 3-5 and Figures 1-4), the attitudes they examine are not stable among the general population across time. They do not, however, report analyses of time trends among police respondents. Figures 2-3 plot agreement with the four GSS items reported in RPH over the 1984-2018 series for both police and non-police respondents. This simple analysis suggests that the attitudes RPH study are not homogeneous across time, and reveals the incredible uncertainty involved in making any comparisons between police and non-police without pooling across years.

RPH acknowledge the presence of time trends among the general population, and decide to add a linear time trend to their model specifications. They justify this decision based on a series of unreported results from tests that are described in a footnote as not yielding statistical significance.[7] When pooling independent cross-sections to make sub-group comparisons, analysts are encouraged to test for temporal stability with a model comparison between 1) an unrestricted model that includes year indicators, sub-group indicators, and their interactions; and 2) a restricted model that does not (see e.g., Wooldridge, 2006, Chapter 13). If the F-statistic from this test is statistically significant, analysts are then encouraged to include these additional factors in their model specifications.

Table 3 replicates the estimates from RPH's primary model specifications for agreement that there is too much spending on law enforcement, and whether respondents can imagine approving of police striking an adult male (RPH Table 1, p. 8). Each set of estimates are compared with those from an "unrestricted" model that simply adds year indicators and their interactions with the police indicator to RPH's model. The F-statistics from model comparisons between RPH's model and the unrestricted version are also reported. All are statistically significant at the conventional threshold. The estimated coefficients on the police indicator – RPH's primary quantity of interest – are about 3 times larger in the unrestricted models when compared to the restricted models used by RPH, and both are statistically significant.

Table 4 presents the same comparisons for RPH's primary model specifications for agreement that there is too much spending on "Assistance to Blacks," and whether they believe Black-White inequality is mainly due to discrimination (RPH Table 2, p. 9). The estimated coefficient on the police indicator for the spending item is roughly the same in the unrestricted model, but the estimated standard error is about 6 times larger and the association is not statistically significant ($P = 0.57$). The estimated coefficient on the police indicator for the Black-White inequality item is about 11 times smaller in the unrestricted model when compared to the restricted model used by RPH, and not statistically significant ($P = 0.92$).

In sum, simply relaxing the temporal stability assumption in RPH's chosen model specifi-

[7] In footnote 9, they write "we also tested year effects with a squared term and a natural log term, but neither proved significant. We also examined temporal trends with distinct decade binary indicators rather than a continuous measure of year. Decade effects, however, revealed the same overall linear pattern, and no interaction with cops were observed. For this reason, we decided to only report the continuous year measure." The results of these analyses are not reported in their paper, and there are no replication materials or code associated with the published manuscript that would permit replication of these tests.

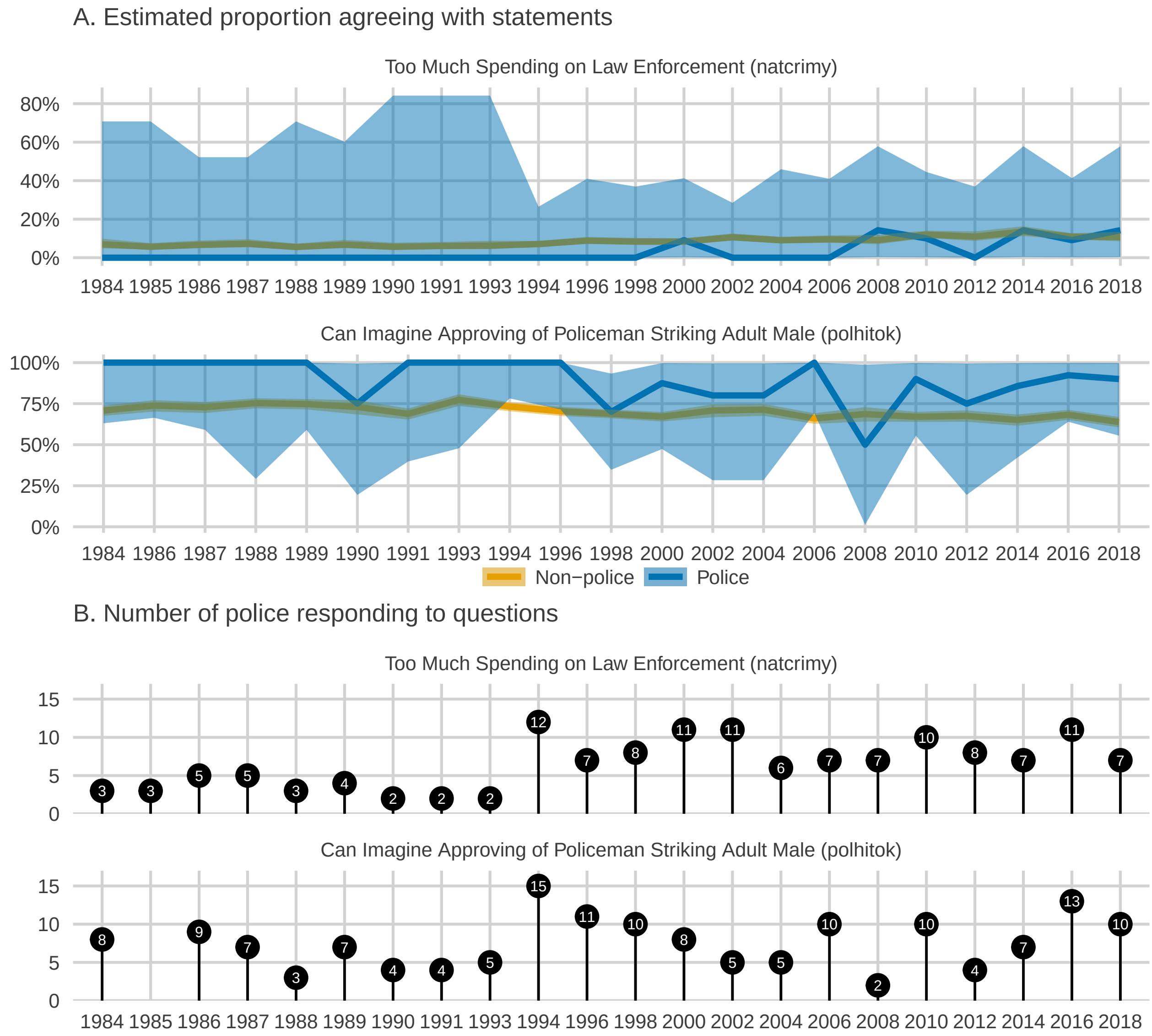
cations has important implications for their primary quantity of interest. First, this increases the strength of the associations between the police indicator and the items RPH use to measure "Vested 'Blue' Occupational Interests". Second, this decreases the strength of the associations between the police indicator and the items RPH use to measure "Racist Attitudes."

Finally, note that there are small numerical differences between the estimated coefficients and standard errors reported in RPH and each set of replication estimates presented in Tables 3-4. For example, the estimated coefficients (standard errors) on the police indicator for the models reported in RPH Table 1 are -4.826 (1.688) and 5.803 (1.297), compared with -4.82 (2.09) and 5.80 (1.35) in the replication presented in Table 3. This is true for all replication results reported here.

Since logistic regression does not have a closed form solution and must be approximately solved numerically, one possibility is that the statistical software RPH use relies on a different method for numerical optimization. Another, not mutually exclusive possibility, is that RPH's standard errors are estimated differently. There are no replication materials or code associated with RPH's paper, and the methods they use are not described in the paper. Though the reason for these numerical differences is unclear, this does not have any substantive implications for inference.

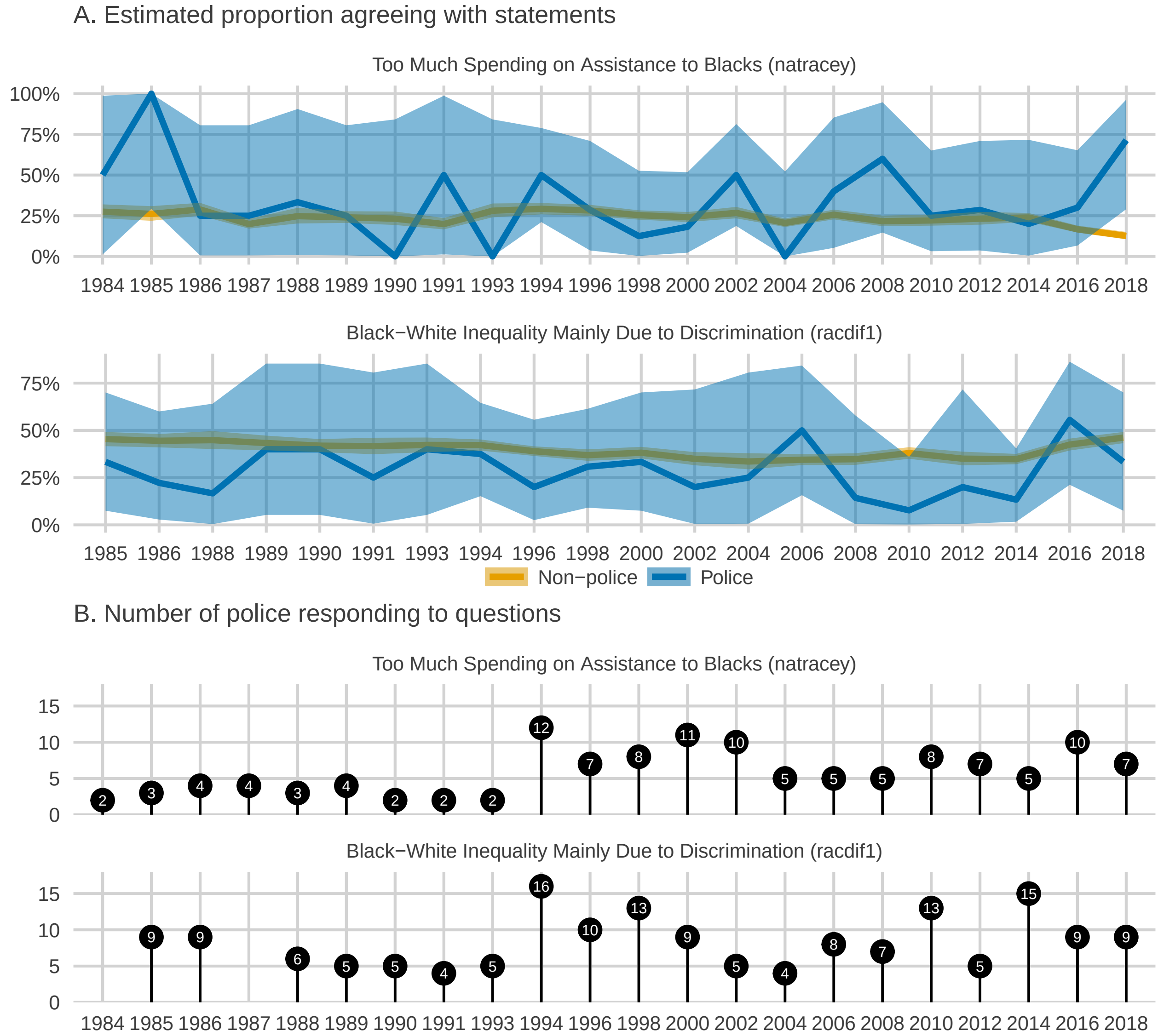
All computations here rely on the `survey` package for `R` (Lumley, 2004), which estimates robust standard errors and uses iteratively reweighted least squares for optimization. Aside from the replications of RPH's model specifications, this note simply uses OLS on a binary indicator (1 = "Police"; 0 = "Non-police") to estimate average differences between the attitudes of police and all other GSS respondents. Replication materials sufficient to reproduce all analyses presented here are publicly available at [BLINDED LINK].

Figure 2: Proportion of police and non-police respondents agreeing with statements from `natcrimy` and `polhitok` in the 1984-2018 GSS series, and police sample sizes



Notes: Lines in Panel A denote group means and shaded regions denote 95% confidence bands. Confidence bands for non-police use the normal approximation method with GSS weights. Confidence bands for police cannot be formed using the normal approximation method due to sample size constraints, and are instead estimated using the exact method for binomial proportions. Among police/non-police respondents, the average widths of the confidence bands are 0.54/0.04 for `natcrimy`, 0.53/0.07 for `polhitok`. Lollipops in Panel B denote the number of police respondents to each question in the series. The GSS has used a dual sample design with two versions since 1994, which explains the increase in observations from 1994 onward. `natcrimy`: "Are we spending too much, too little, or about the right amount on Law Enforcement?" Following RPH's coding, `natcrimy` was recoded as a binary indicator ("Too much" = 1, "About right" = 0, "Too little" = 0). `polhitok`: "Are there any situations you can imagine in which you would approve of a policeman striking an adult male citizen?" (1 = "Yes", 0 = "No").

Figure 3: Proportion of police and non-police respondents agreeing with statements from `natracey` and `racdif1` in 1984-2018 GSS series, and police sample sizes



Notes: Lines in Panel A denote group means and shaded regions denote 95% confidence bands. Confidence bands for non-police use the normal approximation method with GSS weights. Confidence bands for police cannot be formed using the normal approximation method due to sample size constraints, and are instead estimated using the exact method for binomial proportions. Among police/non-police respondents, the average widths of the confidence bands 0.72/0.07/ for `natracey`, and 0.64/0.04 for `racdif1`. Lollipops in Panel B denote the number of police respondents to each question in the series. The GSS has used a dual sample design with two versions since 1994, which explains the increase in observations from 1994 onward. `natracey`: "Are we spending too much, too little, or about the right amount on Assistance to Blacks?" Following RPH's coding, `natracey` was recoded as a binary indicator ("Too much" = 1, "About right" = 0, "Too little" = 0). `racdif1`: "On the average Blacks have worse jobs, income, and housing than white people. Do you think these differences are mainly due to discrimination?" (1 = "Yes", 0 = "No").

TABLE 3: Replication of RPH's estimates for "Vested 'Blue' Occupational Interests", and comparison with estimates from unrestricted models

	Too Much Spending on Law Enforcement		Can Imagine Approving of Police Striking Adult Male	
	Restricted Model	Unrestricted Model	Restricted Model	Unrestricted Model
Police = Yes	-4.82 (2.09)*	-15.92 (1.44)*	5.80 (1.35)*	18.09 (1.61)*
Race = Black	0.45 (0.07)*	0.45 (0.07)*	-1.17 (0.04)*	-1.19 (0.04)*
Race = Other	0.31 (0.09)*	0.32 (0.09)*	-1.31 (0.06)*	-1.31 (0.06)*
Sex = Female	-0.38 (0.05)*	-0.39 (0.05)*	-0.59 (0.03)*	-0.59 (0.03)*
Age	-0.01 (0.00)*	-0.01 (0.00)*	-0.01 (0.00)*	-0.01 (0.00)*
Age x Police	0.07 (0.03)*	0.07 (0.03)*	-0.08 (0.02)*	-0.10 (0.02)*
Year	0.02 (0.00)*	-	-0.01 (0.00)*	-
Constant	-49.76 (5.41)*	-1.84 (0.21)*	18.00 (3.36)*	1.74 (0.09)*
F-statistic	23.37*		42.37*	

Notes: Estimates from logistic regressions fit using GSS weights and the model specification in RPH Table 1 (p. 8). Estimated coefficients for year indicators ($k = 22$) and their interactions with the police indicator ($k = 22$) from the restricted models are omitted. Too Much Spending on Law Enforcement (`natcrimy`): "Are we spending too much, too little, or about the right amount on Law Enforcement?" Following RPH's coding, `natcrimy` was recoded as a binary indicator ("Too much" = 1, "About right" = 0, "Too little" = 0). Can Imagine Approving of Police Striking Adult Male (`polhitok`): "Are there any situations you can imagine in which you would approve of a policeman striking an adult male citizen?" (1 = "Yes", 0 = "No"). $P < 0.05$*.

TABLE 4: Replication of RPH's estimates for "Racist Attitudes", and comparison with estimates from unrestricted models

	Too Much Spending on Assistance to Blacks		Black-White Inequality Mainly Due to Discrimination	
	Restricted Model	Unrestricted Model	Restricted Model	Unrestricted Model
Police = Yes	0.74 (0.22)*	0.75 (1.35)	-0.88 (0.29)*	0.08 (0.81)
Race = Black	-2.43 (0.11)*	-2.42 (0.11)*	1.28 (0.05)*	1.29 (0.05)*
Black x Police	-	-	1.41 (0.55)*	2.15 (0.84)*
Race = Other	-0.50 (0.09)*	-0.51 (0.09)*	0.62 (0.06)*	0.63 (0.06)*
Other x Police	-	-	2.49 (0.94)*	2.26 (0.96)*
Sex = Female	-0.20 (0.04)*	-0.22 (0.04)*	0.21 (0.03)*	0.21 (0.03)*
Female x Police	-	-	1.06 (0.51)*	1.12 (0.57)*
Age	0.01 (0.00)*	0.01 (0.00)*	-0.00 (0.00)	-0.00 (0.00)
Year	-0.01 (0.00)*	-	-0.01 (0.00)*	-
Constant	25.54 (3.84)*	-1.21 (0.12)*	21.05 (3.23)*	-0.42 (0.09)*
F-statistic	33.74*		3.37*	

Notes: Estimates from logistic regressions fit using GSS weights and the model specification in RPH Table 2 (p. 9). Estimated coefficients for year indicators ($k = 22$) and their interactions with the police indicator ($k = 22$) from the restricted models are omitted. Too Much Spending on Assistance to Blacks (`natracey`): "Are we spending too much, too little, or about the right amount on Assistance to Blacks?" Following RPH's coding, `natracey` was recoded as a binary indicator ("Too much" = 1, "About right" = 0, "Too little" = 0). Black-White Inequality Mainly Due to Discrimination (`racdif1`): "On the average Blacks have worse jobs, income, and housing than white people. Do you think these differences are mainly due to discrimination?" (1 = "Yes", 0 = "No"). $P < 0.05^*$.

2 Fishing for significance and selective reporting

In their analyses of the pooled GSS series, RPH fit a series of logistic regressions using a specification search procedure whereby "interactions between [a police indicator,covariates, and a linear time trend] were introduced one at a time to test for variations and significance, and only significant interactions are included in the final trimmed baseline models" (p. 6). RPH infer support for their claims from statistically significant log-odds coefficients on the police indicator and its interaction with other covariates (e.g., race) in their "final trimmed baseline models". Results from non-significant interactions conducted during the model selection process are then reported as "ns". In footnote 11 (p. 7), RPH state that they replicated their results using generalized linear models, but these results are not reported. This note is focused on the "final trimmed baseline models" that were reported in the published manuscript.

RPH call this "iterative interaction modeling," but provide no further description or relevant citation.[8] Decision rules based on the statistical significance of interaction terms in logistic regressions do not, however, appear typical or well-regarded in sociology. For example, a recent editorial report published by the *American Sociological Review* reviewed the methodological literature on this topic and concluded "the case is closed: don't use the coefficient of the interaction term to draw conclusions about statistical interaction in categorical models such as logit, probit, Poisson, and so on" (Mustillo, Lizardo and McVeigh, 2018, p. 1282).

More importantly, the use of any iterative model fitting procedure to select a "final" model involves conditioning on statistical significance during the model selection process. This has long been recognized as a form of "data dredging," or fishing for statistical significance (see e.g., Selvin and Hanan, 1966). The key implication is that the sampling distribution of post-selection estimates is generally unknowable, resulting in biased estimates of regression coefficients and standard errors (see e.g., Berk et al., 2010). In other words, the nominal P-values for the post-selection models reported by RPH do not take into account the model selection process, and are therefore too small.

A broader concern is that RPH's reported analyses are focused on just four items selected from a broader universe of similar items that appear in the GSS core. This section presents results from analyses on this broader universe, using the same estimation approach as RPH where applicable. Table 5 provides a summary of the four items reported by RPH, alongside 11 other items covering the same topics. Differences between police and non-police respondents on 9/11 of these other items did not meet conventional levels of statistical significance, and one significant difference (`polmurdr`) is in the opposite direction of RPH's claims. GSS items `polhitok` and `racdif1` come from sequential question batteries covering police use of force and causal attributions for Black-White inequality.

[8] To the best of my knowledge, the properties of this procedure have not been studied in the statistics literature and it is not widely used in empirical research. A Google scholar search for "iterative interaction modeling" returns a single unpublished working paper from 1999 that describes a formal-theoretic model of "decision and action mechanisms which assist agents during distributed problem solving processes." See https://scholar.google.com/scholar?hl=en&as_sdt=0%2C7&q=%22iterative+interaction+modelling%22&btnG=

Table 5: Summary of reported and unreported comparisons, their statistical significance, and number of police respondents in the 1984-2018 series

GSS Item(s)		Reported	$P < 0.05$	N
Support for police use of force:				
`polhitok`	*"Are there any situations you can imagine in which you would approve of a policeman striking an adult male citizen?"*	Yes	Yes	157
`polabuse`	*"Would you approve of a policeman striking a citizen who had said vulgar and obscene things to the policeman?"*	No	No	166
`polmurdr`	*"... a citizen who was being questioned as a suspect in a murder case?"*	No	Yes*	166
`polescap`	*"... a citizen who was attempting to escape from custody?"*	No	No	164
`polattak`	*"... a citizen who was attacking the policeman with his fists?"*	No	Yes	166
Support for government spending:				
`natcrimy`	*"Are we spending too much, too little, or about the right amount on law enforcement?"*	Yes	Yes	141
`natracey`	*"... on assistance to Blacks?"*	Yes	Yes	126
`natrace`	*"... on improving the conditions of Blacks?"*	No	No	118
Causal attributions for Black-White inequality:				
`racdif1`	*"On the average Blacks have worse jobs, income, and housing than white people. Do you think these differences are mainly due to discrimination?"*	Yes	Yes	166
`racdif2`	*"... because most have less in-born ability to learn"*	No	No	166
`racdif3`	*"... because most don't have the chance for education that it takes to rise out of poverty"*	No	No	164
`racdif4`	*"... because most just don't have the motivation or will power to pull themselves up out of poverty"*	No	No	162
Explicit prejudice:				
`wrkblks`; `wrkwhts`	Difference between Blacks and Whites on 7-pt scales from "hardworking" to "lazy"	No	No	121
`intlblks`; `intlwhts`	Difference between Blacks and Whites on 7-pt scales from "intelligent" to "unintelligent"	No	No	110
Interracial closeness:				
`closeblk`; `closewht`	Difference between Blacks and Whites on 9-pt scales from "Not at all close" to "Very close"	No	No	104

Notes: *denotes a statistically significant difference in the opposite direction of RPH's claims.

2.1 Spending on law enforcement and police use of force

RPH claim that police "uniquely believe that they should receive more funding and have the right to use physical force against citizens" (c.f. abstract). This claim is supported by reference to statistically significant coefficients from logistic regressions of `natcrimy` and `polhitok` on a police indicator (1 = "Police"; 0 = "Non-police"), with additional covariates selected using their iterative interaction modeling procedure. In other words, if police respondents are less likely to say there is "too much" spending on law enforcement when compared to non-police it means the police population uniquely believes it should receive more funding. And if police respondents can more easily imagine a situation in which they would approve of a policeman striking an adult male when compared to non-police, then police believe they have a unique right to use physical force "because of their occupational position" (p. 10).

The item RPH use to support the claim that police uniquely believe they have the right to use physical force against civilians (`polhitok`) was selected from a five-item battery on support for police use of force across different contexts (see Table 5 and GSS codebook p. 501-502). These five items have been used extensively in prior work to study public support for police use of force across different contexts. This work has emphasized the value of using these items to distinguish between support for legally reasonable (`polescap`, `polattak`) versus unreasonable (`polabuse`, `polmurdr`) force, and found public support for the former is stronger than the latter (e.g., Barkan and Cohn, 1998; Silver and Pickett, 2015). More recent work has found a significant increase in opposition to what would be deemed legally reasonable uses-of-force (e.g., striking a citizen who is attacking an officer), and a growing minority of respondents say they cannot imagine any situation (`polhitok`) in which they would approve of a policeman striking an adult male (Mourtgos and Adams, 2020).

According to RPH, a statistically significant coefficient on the police indicator in their model for `polhitok` "suggests quite clearly that [police officers' justifications for] use of physical force squarely align with their occupational identities and interests, in a manner that is distinct from the general U.S. adult population" (p. 7). RPH further claim that "younger officers are more apt to support police spending than older officers and are also more likely than older officers to see police use of physical force as more legitimate" (p. 7). Yet RPH do not report analyses for the GSS items that could plausibly approximate support for legitimate or "legally reasonable" (`polescap`, `polattak`) versus non-legitimate or "legally unreasonable" (`polabuse`, `polmurdr`) uses of force. Table 6 replicates RPH's analysis for `polhitok` (column 1) and applies their same model specification to the other four items in the question battery (columns 2-5 of Table 6).

First, note that RPH's interpretation of the coefficient on the police indicator in their model fit to `polhitok` (Table 6, column 1) is incorrect. Given the interaction between the police indicator and age, the coefficient of 5.80 suggests that, conditional on the model, the difference in log-odds of agreement between police and non-police of age 0 is 5.80. This estimate does not apply to any set of observations contained in the data. The "marginal effect" (partial derivative averaged across all observations in the data) for the police indicator is 2.24, which corresponds to a probability difference of 0.21 on the binary response scale. The youngest police respondent is 20 years old, and the oldest is 89. The partial derivative evaluated at age 20 produces a log-odds coefficient of 4.21, and yields -1.281 when evaluated at age 89.

Second, when RPH's model is fit to the other use of force items (Table 6, columns 2-5), it produces estimates that – under their interpretation of the coefficient on the police indicator – suggest police are not more likely than non-police to support an officer striking a person who was verbally abusive, or trying to escape custody. Moreover, police respondents are significantly *less likely* to support an officer striking a suspected murderer (legally unreasonable), and significantly more likely to support a person who was physically attacking an officer (legally reasonable). While RPH's model suggests that, compared to non-police of age 0, police of age 0 can *more easily imagine a situation* in which they would approve of a policeman striking an adult male, the same model also suggests their belief in whether force is "legitimate" depends on the specific context in ways that align with what prior work has defined to be legally reasonable (e.g., Mourtgos and Adams, 2020).

Table 6: Estimates from logistic regressions for all police use of force items using RPH's model specification

		Legally Unreasonable		Legally Reasonable	
	Any situations? `polhitok`	Verbal abuse? `polabuse`	Murder suspect? `polmurdr`	Escaping? `polescap`	Attacking? `polattak`
Police = Yes	5.80 (1.35)*	-0.42 (0.96)	-2.91 (1.32)*	0.39 (0.67)	3.22 (1.31)*
Race = Black	-1.17 (0.04)*	-0.26 (0.08)*	0.61 (0.06)*	-0.99 (0.04)*	-0.96 (0.06)*
Race = Other	-1.31 (0.06)*	0.43 (0.09)*	1.14 (0.07)*	-0.82 (0.06)*	-1.19 (0.07)*
Sex = Female	-0.59 (0.03)*	-0.29 (0.04)*	-0.09 (0.04)*	-0.44 (0.03)*	-0.44 (0.05)*
Age	-0.01 (0.00)*	0.02 (0.00)*	0.01 (0.00)*	0.00 (0.00)*	-0.00 (0.00)
Age x Police	-0.08 (0.02)*	-0.00 (0.02)	0.04 (0.02)	-0.01 (0.01)	-0.05 (0.02)*
Year	-0.01 (0.00)*	-0.01 (0.00)*	0.02 (0.00)*	-0.01 (0.00)*	-0.03 (0.00)*
Constant	18.00 (3.36)*	20.70 (5.20)*	-49.10 (4.87)*	24.42 (3.24)*	63.19 (4.85)*
Reported in RPH:	Yes	No	No	No	No

Notes: `polhitok`: "Are there any situations you can imagine in which you would approve of a policeman striking an adult male citizen?"; `polabuse`: "Would you approve of a policeman striking a citizen who had said vulgar and obscene things to the policeman?"; `polmurdr`: "Would you approve of a policeman striking a citizen who was being questioned as a suspect in a murder case?"; `polescap`: "Would you approve of a policeman striking a citizen who was attempting to escape from custody?"; `polattak`: "Would you approve of a policeman striking a citizen who was attacking the policeman with his fists?". All items are binary indicators (1 = "Yes"; 0 = "No"). Standard errors in parentheses. $P < 0.05^*$.

2.2 Spending on Assistance to Blacks and causal attributions for Black-White inequality

RPH's claims that police are "distinctly racist" are based on statistically significant coefficients from logistic regressions of `natracey` and `racdif1` on a police indicator (1 = "Police"; 0 = "Non-police"), with additional covariates selected using their iterative interaction modeling procedure. This section show that the inferences RPH make from the estimated coefficients in these models are incorrect, and that differences between police and non-police do not replicate on adjacent GSS items[9] covering the same topics.

Table 7 column 1 replicates RPH's analysis for `natracey` (see "Assistance to Blacks" in RPH Table 2, p. 9). RPH infer that the coefficient of 0.74 on the police indicator means that "cops are about twice as likely as members of the general public to view spending on assistance to African American as being too much." This is misleading. Expressed on the same scale as the binary outcome, the probability of agreement with the statement (conditional on the model) is 0.44 for police and 0.28 for non-police.[10] The probability difference of $0.44 - 0.28 = 0.16$ suggests that, compared with non-police respondents, police respondents are about 16 percentage points more likely to agree that there is too much spending on "Assistance to Blacks".

RPH also infer from this model that differences between police and non-police "hold for all cops, regardless of race, gender, and age"(p. 8). This is incorrect. For example, if this specification is simply fit to non-White GSS respondents, the coefficient on the police indicator is -0.48 (SE = 0.81): a statistically insignificant difference in the opposition direction. To test whether the difference holds for all police respondents "regardless of race , gender, and age" one might fit different models with interactions between the police indicator and these covariates. However, these interactions were reported as "ns" by RPH as they were ruled out by their specification search procedure.

Columns 2-3 in Table 7 report estimates from RPH's final model specification fit to `natrace` ("Improving the Conditions of Blacks"). This item is an alternate question wording for the `natracey` item, and it covers a larger sample of GSS respondents over a longer time period. If `natrace` is used rather than `natracey`, however, the coefficient on the police indicator is about 1/4 the size, and no longer statistically significant. Curiously, RPH do motivate their analyses of time trends using a plot for `natrace` among the general population (see RPH Figure 3, p. 6). In their regression modeling, however, they only report the statistically significant coefficient from the model for `natracey`. RPH do not mention whether `natrace` was included in their specification search procedure and yielded "ns" results, but this seems a plausible explanation for the discrepancy between their regression modeling and graphical analyses.

[9] The term "adjacent" is used to describe GSS items that are part of the same question battery and therefore adjacent, or next to, the item selected by RPH. This is how the questions were asked to survey respondents, and how they are described in the GSS codebook. For example, `racdif2` is adjacent to `racdif1` because it was the second item in a four-part question about respondents' causal attributions for Black-White inequality.

[10] The model somewhat exaggerates the simple difference in sample means: 34% of police agree with the statement versus 24% of non-police.

Table 7: Estimates from logistic regressions for spending on Assistance to Blacks and Improving the Conditions of Blacks using RPH's model specification

	Assistance to Blacks	Improving the Condition of Blacks	
	`natracey` (1984-2018)	`natrace` (1984-2018)	`natrace` (1973-2018)
Police = Yes	0.74 (0.22)*	0.15 (0.25)	0.20 (0.21)
Race = Black	-2.43 (0.11)*	-2.36 (0.13)*	-2.72 (0.12)*
Race = Other	-0.50 (0.09)*	-0.33 (0.10)*	-0.38 (0.09)*
Sex = Female	-0.20 (0.04)*	-0.27 (0.04)*	-0.29 (0.03)*
Age	0.01 (0.00)*	0.01 (0.00)*	0.01 (0.00)*
Year	-0.01 (0.00)*	-0.01 (0.00)*	-0.02 (0.00)*
Constant	25.54 (3.84)*	26.46 (4.41)*	39.35 (2.82)*
Reported in RPH:	Yes	No	No

Notes: Estimates from logistic regressions fit using GSS weights and the model specification in RPH Table 2 (p. 9). `natracey/natrace`: "Are we spending too much, too little, or about the right amount on [Assistance to Blacks/Improving the Conditions of Blacks]?" (RPH coding: "Too much" = 1, "About right" = 0, "Too little" = 0). See GSS codebook p. 270 for `natrace` and p. 277 for `natracey`. Standard errors in parentheses. $P < 0.05^{*}$.

RPH's second measure of racism is `racdif1`. Since 1977, the GSS has asked respondents about four possible causes for Black-White inequality: discrimination (`racdif1`), less in-born ability to learn (`racdif2`), lack of educational opportunity (`racdif3`), and insufficient motivation/willpower (`racdif4`). This question is frequently used in survey research to examine Americans' causal attributions for racial inequality. For example, prior analyses have shown that, among White Americans, lack of motivation/willpower has been the most popular explanation, followed by lack of education, discrimination, and inborn ability (see Bobo et al., 2012, Figure 3.13). Bobo et al. (2012) also showed that "most white Americans do not embrace a single account of black-white economic inequality" (p. 62).

To support their claims that police are distinctly racist, however, RPH select a single account and report statistically significant results from a logistic regression fit to `racdif1`. RPH make no mention of `racdif2-racdif4`. Table 8 replicates RPH's specification fit to `racdif1` (column 1), and applies this same specification to the other three explanations queried by the GSS as part of the same question (columns 2-4). Across all four items, the only statistically significant coefficient on the police indicator is the one reported by RPH. Recall the editorial statement from the *American Sociological Review*: "don't use the coefficient of the interaction term to draw conclusions about statistical interaction in categorical models such as logit".

Unlike the final model that RPH report for `natracey`, their specification search for `racdif1` produced a final model with multiple interactions terms. The estimated coefficient of -0.88 (SE = 0.29) on the police indicator (replicated in Table 8, column 1) therefore does not correspond to a difference between police and all other GSS respondents. Rather, the estimate suggests that, conditional on the model, the difference in log-odds of agreement between White male police respondents and White male non-police respondents is -0.88, or a probability difference of -0.16 on the binary response scale. The partial derivative ("marginal effect") for the police indicator, averaged across all observations in the data, corresponds to a log-odds of 0.06. Differences between other sub-groups (e.g., female police v. non-police) are all signed in the opposite direction.

What if RPH did not condition on the significance of interaction terms in their "iterative interaction modeling" to select a new final model to report for each outcome measure? Importantly, they could have avoided post-selection model inference and the reporting of invalid test statistics – a form of significance fishing that, though common in sociology and criminology, has been widely criticized (see Berk et al., 2010). But even if RPH fit the same model specification used for `natracey`, the estimated coefficient on the police indicator would have been -0.31 (SE = 0.19): roughly 1/3 the size of the reported estimate of -0.88, and no longer statistically significant. Similarly, if this same model is fit to the subset of non-White GSS respondents, the coefficient on the police indicator is 1.01 (SE = 0.43): a statistically significant "effect" in the opposite direction.

In sum, RPH's claims that police are "distinctly racist" are based on incorrectly interpreted logistic regression coefficients from a bespoke specification search procedure that yields significant results only when carefully applied to the two GSS items they selected.

Table 8: Estimates from logistic regressions for all causal attributions for Black-White inequality items using RPH's model specification

	Discrimination	In-born ability	Lack of education	Lack of motivation
	`racdif1`	`racdif2`	`racdif3`	`racdif4`
Police = Yes	-0.88 (0.29)*	0.04 (0.33)	-0.31 (0.21)	0.05 (0.22)
Race = Black	1.28 (0.05)*	0.26 (0.06)*	0.43 (0.04)*	-0.36 (0.05)*
Black x Police	1.41 (0.55)*	-2.06 (1.10)	-0.03 (0.49)	-0.55 (0.50)
Race = Other	0.62 (0.06)*	0.81 (0.08)*	0.18 (0.06)*	0.37 (0.06)*
Other x Police	2.49 (0.94)*	-0.35 (1.04)	1.77 (0.92)	-0.11 (1.00)
Sex = Female	0.21 (0.03)*	-0.10 (0.04)*	0.12 (0.03)*	-0.11 (0.03)*
Female x Police	1.06 (0.51)*	0.37 (0.87)	-0.17 (0.46)	0.85 (0.52)
Age	-0.00 (0.00)	0.03 (0.00)*	0.00 (0.00)*	0.01 (0.00)*
Year	-0.01 (0.00)*	-0.04 (0.00)*	-0.01 (0.00)*	-0.02 (0.00)*
Constant	21.05 (3.23)*	75.82 (4.47)*	19.85 (3.05)*	45.69 (3.11)*
Reported in RPH:	Yes	No	No	No

Notes: Estimates from logistic regressions fit using GSS weights and the model specification in RPH Table 2 (p. 9). GSS Q# 286. "On the average Blacks have worse jobs, income, and housing than white people. Do you think these differences are ...Mainly due to discrimination (`racdif1`); Because most have less in-born ability to learn (`racdif2`); Because most don't have the chance for education that it takes to rise out of poverty (`racdif3`); Because most just don't have the motivation or will power to pull themselves up out of poverty (`racdif4`)" (1 = "Yes"; 0 = "No"). See GSS codebook pp. 524-525. Standard errors in parentheses. $P < 0.05^*$.

2.3 Explicit prejudice and interracial closeness

As shown in the previous section, RPH's claims that police are "distinctly racist" are based on statistically significant coefficients from logistic regressions fit to just two items selected from a broader set of questions covering structural explanations for Black-White inequality and deservingness. RPH "purposely use the term *racist* when discussing [`natracey` and `racdif1`, because these items] capture attitudes about both lack of 'deservingness' of African Americans and acknowledgment (or lack thereof) of racial inequality's roots in systemic bias and discrimination – attitudes that, when taken in tandem, jointly contribute to the legitimation and persistence of racial inequality" (p. 2).

Given the multiple dimensions of racism in the United States, researchers typically leverage multiple measures that include attitudes about structural explanations for Black-White inequality as well as racial prejudice (Kluegel and Smith, 1982; Dovidio and Gaertner, 1986; Bobo and Kluegel, 1993; Bobo, Kluegel and Smith, 1997; Bobo and Charles, 2009). In addition to questions about Black-White inequality, the GSS regularly includes direct questions that tap explicit prejudice and perceived interracial closeness. These items were not included in RPH's analyses, but a growing body of research suggests direct questions are the best way to measure racial attitudes in the survey context (see Axt, 2018).

Explicit prejudice is measured using GSS questions about beliefs in the group-level superiority of Whites over Blacks on work ethic and intelligence. These questions have appeared in the GSS since 1990, are among the most widely used direct measures of anti-Black prejudice in survey research (see e.g., Bobo and Kluegel, 1993; Bobo et al., 2012), and are behaviorally validated indicators of an individual's willingness to engage in discrimination (see Peyton and Huber, forthcoming). Interracial closeness is measured using respondents' reported feelings of closeness to Blacks v. Whites. These questions have appeared in the GSS since 1996, and are widely used indicators of affective prejudice (see e.g., Jackman and Crane, 1986; Tropp and Pettigrew, 2005).

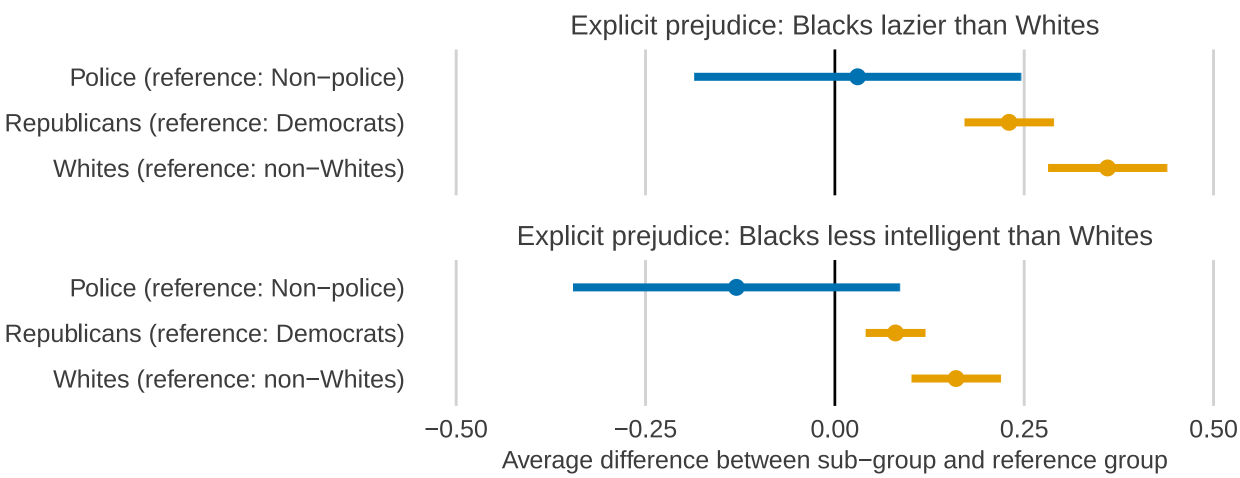
Figure 4 plots average differences between police and non-police on the explicit prejudice measures, alongside the same differences for Republicans v. Democrats, and Whites v. non-Whites. Differences between these other groups are well documented in survey research and provide some context for the size of the differences between police and non-police. Positive estimates indicate a sub-group has higher levels of anti-Black prejudice than the reference group. On average, police respondents in the GSS are approximately 0.03 points more prejudiced than non-police ($P = 0.76$) on the first indicator and 0.13 points less prejudiced than non-police ($P = 0.22$) on the second indicator. By comparison, Republicans are 0.23 points higher than Democrats ($P < 0.001$) on the first, and 0.08 points higher on the second ($P < 0.001$). Finally, Whites are 0.36 points higher than non-Whites ($P < 0.001$) on the first, and 0.16 points higher on the second ($P < 0.001$).

Figure 5 plots the average differences between Police and non-Police on the interracial closeness measure, alongside the same benchmarks for Republicans and Whites. Positive estimates indicate a sub-group has greater feelings of closeness to Blacks (v. Whites), relative to the reference group. On average, police respondents are approximately 0.2 points higher in their feelings of closeness to Blacks (v. Whites) than non-police ($P = 0.38$). By comparison, Republicans are 1.03 points lower than Democrats ($P < 0.001$), and Whites are 2.21 points lower than non-Whites ($P < 0.001$).

Finally, it is worth noting that if the measures from Figures 4-5 (and not those from the previous section) raise concerns about "social desirability bias," such concerns would need to apply uniquely to police respondents. For example, if 28% of police respondents agree with a statement compared with 40% of non-police respondents, then the observed *difference* of 12 percentage points could be explained away by social desirability bias if we assume that police reveal their true agreement, but that non-police exaggerate their agreement by about 12%. Further, given the observed differences between other sub-groups (e.g., Republicans v. Democrats) we would need to assume such biases on these measures do not affect these groups, or at least affect their response patterns in more complex ways.

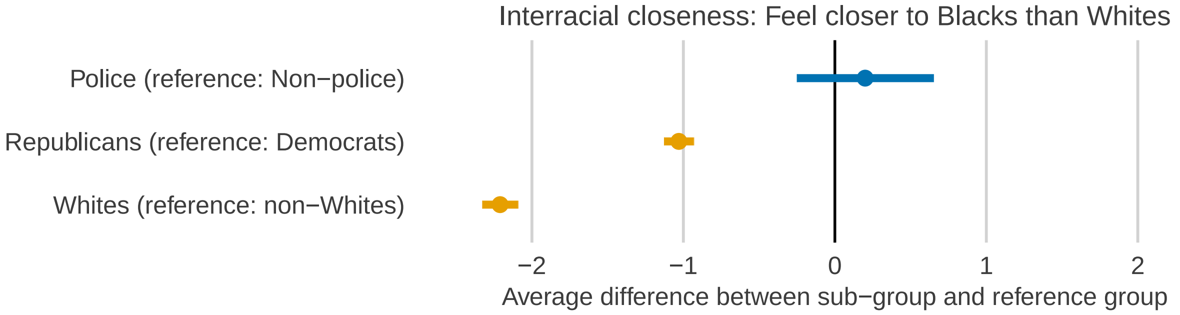
If we instead allow for such biases to exist but simply assume they affect all sub-groups in roughly the same way, then average responses to a question would be inflated by some amount, but the *differences* between sub-groups could not be attributable to social desirability bias. In general, the threat that misreporting survey responses on sensitive topics might pose to survey research seems low. For example, a recent meta-analysis comparing 30 years of list experiments to direct questioning found that, if anything, survey respondents *overreport* racist attitudes by a small but statistically insignificant amount (see Blair et al. 2020).

Figure 4: Average differences between Police and non-Police on measures of explicit prejudice, with benchmarks to differences between Republicans and Democrats, Whites and non-Whites



Notes: point estimates and 95% confidence intervals estimated using GSS weights. Each measure is constructed by subtracting a respondent's rating of Blacks from Whites on 7-point scales from "hardworking" (1) to "lazy" (7), and "intelligent" (1) to "unintelligent" (7). Positive estimates indicate a sub-group has, on average, higher levels of anti-Black prejudice than the reference group. Pooling across the entire series yields 121 police respondents on the first indicator and 110 on the second. More than 18,000 non-police, 6,000 Republicans, 8,000 Democrats, 14,000 Whites, and 4,000 non-Whites responded to these questions. See GSS codebook for question wordings: `wrkwhts` (p. 703), `wrkblks` (p. 704), `intlwhts` (p. 709), and `intlblks` (p. 710).

Figure 5: Average differences between Police and non-Police on measures of interracial closeness, with benchmarks to differences between Republicans and Democrats, Whites and non-Whites



Notes: point estimates and 95% confidence intervals estimated using GSS weights. Interracial closeness is constructed by subtracting a respondent's feelings of closeness to Blacks from Whites on 9-point scales from "Not at all close" (1) to "very close" (9). Positive estimates indicate a sub-group has, on average, greater feelings of closeness to Blacks (v. Whites) than the reference group. Pooling across the 1996-2018 series yields 104 police respondents, 15,635 non-police, 12,055 Whites, 3,684 non-Whites, 7,164 Democrats and 5,267 Republicans. See the GSS codebook for complete question wordings: `closeblk` (p. 387), `closewht` (p.388)

3 Claiming police attitudes are distinct and/or unique

RPH's overarching claims about the distinctness and uniqueness of police seem to rest upon the premise that if police were not a unique/distinct group then there would not be any meaningful differences between the attitudes of police respondents and non-police respondents in the GSS. Further, that differences between the attitudes of police and non-police – at least for the items RPH report – are "distinct" (recognizably different from others) or "unique" (the only group of this kind). It is possible RPH did not intend for the terms "distinct" and "unique" to be understood by their common definition. Taken at face value, however, one testable implication of RPH's uniqueness/distinctness claims is that other occupational groups in the GSS should not differ from the general population in the same way. If, for example, differences between the attitudes of electricians and non-electricians on `natracey` are similar to the differences between police and non-police, it would suggest the classification of "distinctly racist" does not uniquely apply to police.

Restricting attention to those with at least 100 observations in the pooled GSS sample yields 139 other occupational sub-groups besides police. An OLS regression of RPH's racism items on a binary indicator that denotes membership in an occupational sub-group (e.g., 1 = "Police"; 0 = "Non-police") is a simple and transparent estimator for the differences between occupational sub-groups. For example, assuming independent random sampling and temporal stability, the coefficient on the police indicator from a survey regression with the GSS weights has a straightforward interpretation that maps directly onto RPH's inferential target – the average difference between police and non-police.

An important advantage over RPH's "iterative interaction" estimator is that the simple OLS estimator can be applied one time to each outcome, in the same way. RPH's estimator, by contrast, varies across outcomes depending on the statistical significance of interactions between the indicator of interest and other covariates. Another advantage is that, unlike RPH's estimator, the coefficient on the indicator in the OLS estimator maps directly onto the same quantity of interest for each outcome – the average difference between group X and all other GSS respondents.

Applying this simple estimator to RPH's racism items produces significant differences across a wide variety of occupational sub-groups, ranging from electricians to registered nurses. Overall 4% of sub-groups are at least as extreme as police in either direction on `natracey`, and 25% are at least as extreme on `racdif1`. In fact, there are 12 significant differences in the same direction as police for `natracey`, and 23 for `racdif1`. Adjusting P-values for multiple comparisons to control the false discovery rate (Benjamini and Hochberg, 1995) reduces the pool of statistically significant differences from 12 to 2 for `natracey` and from 22 to 14 for `racdif1`. These results, presented in Figures 6-7, demonstrate that significant differences between the attitudes of occupational group members and non-members in the GSS are not, as RPH claim, distinct to police. Square shaped point estimates are statistically significant after adjusting for multiple comparisons.

Likewise, Figures 8-9 demonstrate that differences on `polhitok` and `natcrimy` are not, as RPH claim, unique to the police sub-group. About 4% are at least as extreme as police in either direction on `polhitok`, and about 22% are at least as extreme on `natcrimy`. There are 37 significant differences in the same direction as police for `polhitok`, and 26 for `natcrimy`. Adjusting for multiple comparisons reduces the pool of statistically significant differences from 37 to 32 for `polhitok` and from 26 to 13 for `natcrimy`.

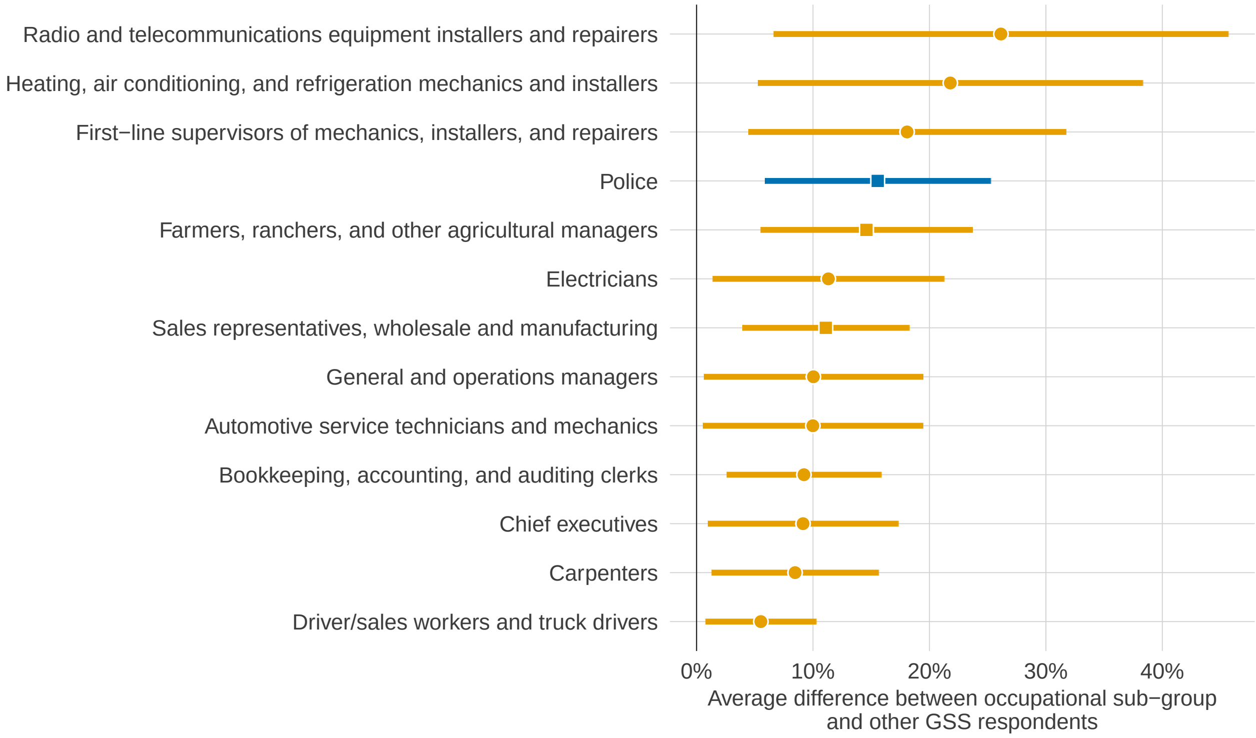
As Figure 8 shows, lawyers, chief executives, firefighters, and a wide variety of other groups can also more easily imagine a situation in which they would approve of a policeman striking an adult male when compared to all other GSS respondents. Such differences do not justify the inference that these groups also uniquely believe police have the right to use physical force against civilians because of their occupational position. Likewise, the significant differences in Figure 9 do not imply that librarians or postal service clerks uniquely believe police should receive more funding.

Finally, RPH argue that since police are less likely to say there is "too much" spending on law enforcement than non-police "officers are occupationally aligned in vested ways when it comes to law enforcement spending" (p. 9). Taken at face-value, RPH seem to be asserting that police as a group are distinct/unique in their support for occupation-specific spending. But are such occupational alignments, as RPH claim, unique to police?

The GSS also asks parallel questions about spending on education, scientific research, and national defense. Given that teachers, scientists, and the military stand to benefit from spending in these areas, one might speculate these groups also have vested interests. Figure 10 demonstrates that, when compared to all other GSS respondents, each group is indeed less likely to agree there is "too much" spending on occupation-specific funding: Teachers -0.02 ($P = 0.02$), Police -0.06 ($P < 0.001$), Scientists -0.11 ($P < 0.001$), Military -0.19 ($P < 0.001$).[11] The fact that police are, like these other groups, more supportive of spending on law enforcement does not necessarily demonstrate anything unique about their occupational position.

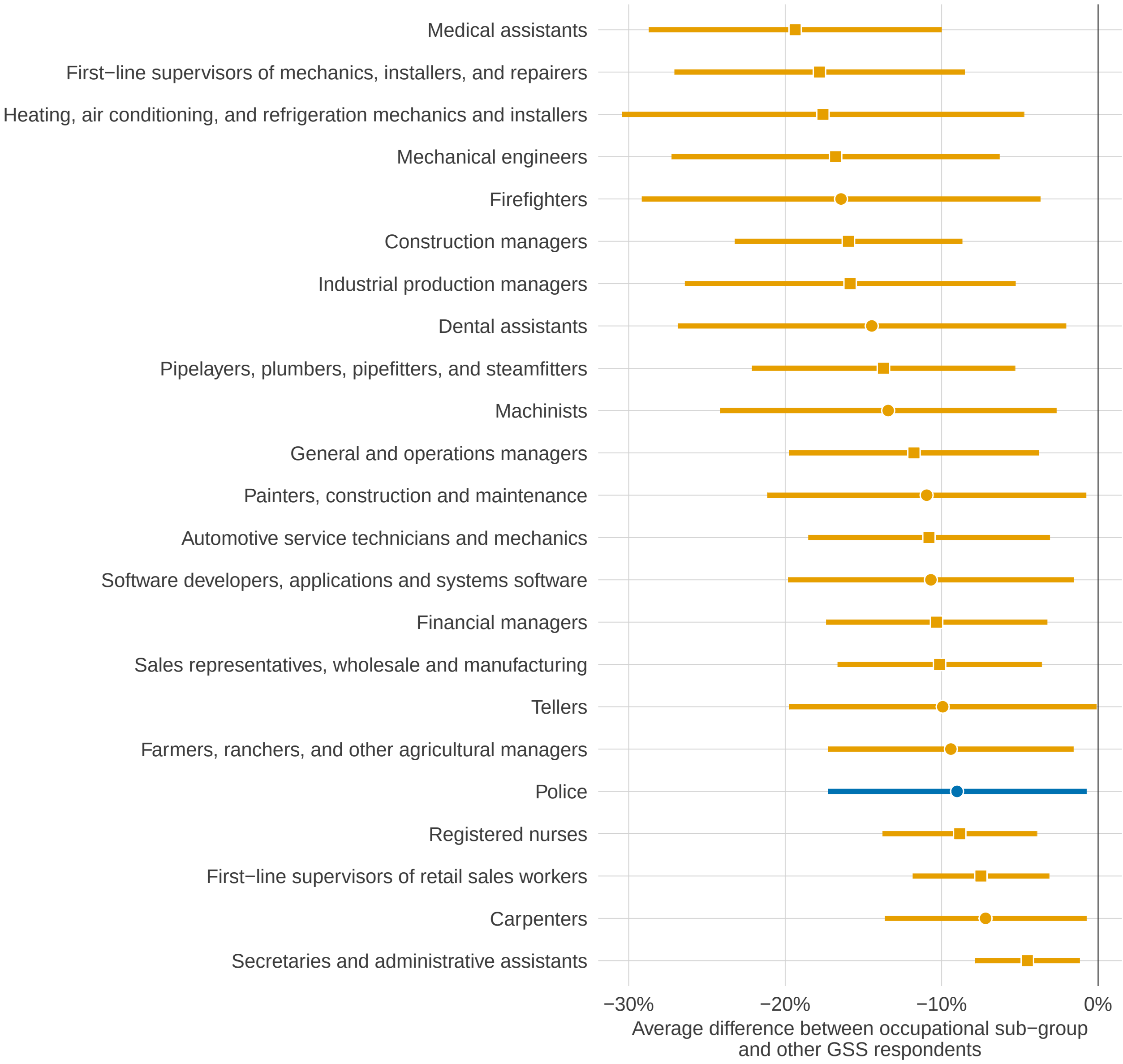
[11] All differences are statistically significant after adjusting for multiple comparisons: Teachers ($P = 0.03$), Police ($P < 0.001$), Scientists ($P < 0.001$), Military ($P < 0.001$). Adding controls for ideology and partisanship does change these inferences: Teachers -0.02 ($P < 0.001$), Police -0.04 ($P < 0.02$), Scientists -0.08 ($P < 0.001$), Military -0.15 ($P < 0.001$)

Figure 6: Significant differences on "Assistance to Blacks" (`natracey`) are not unique to police



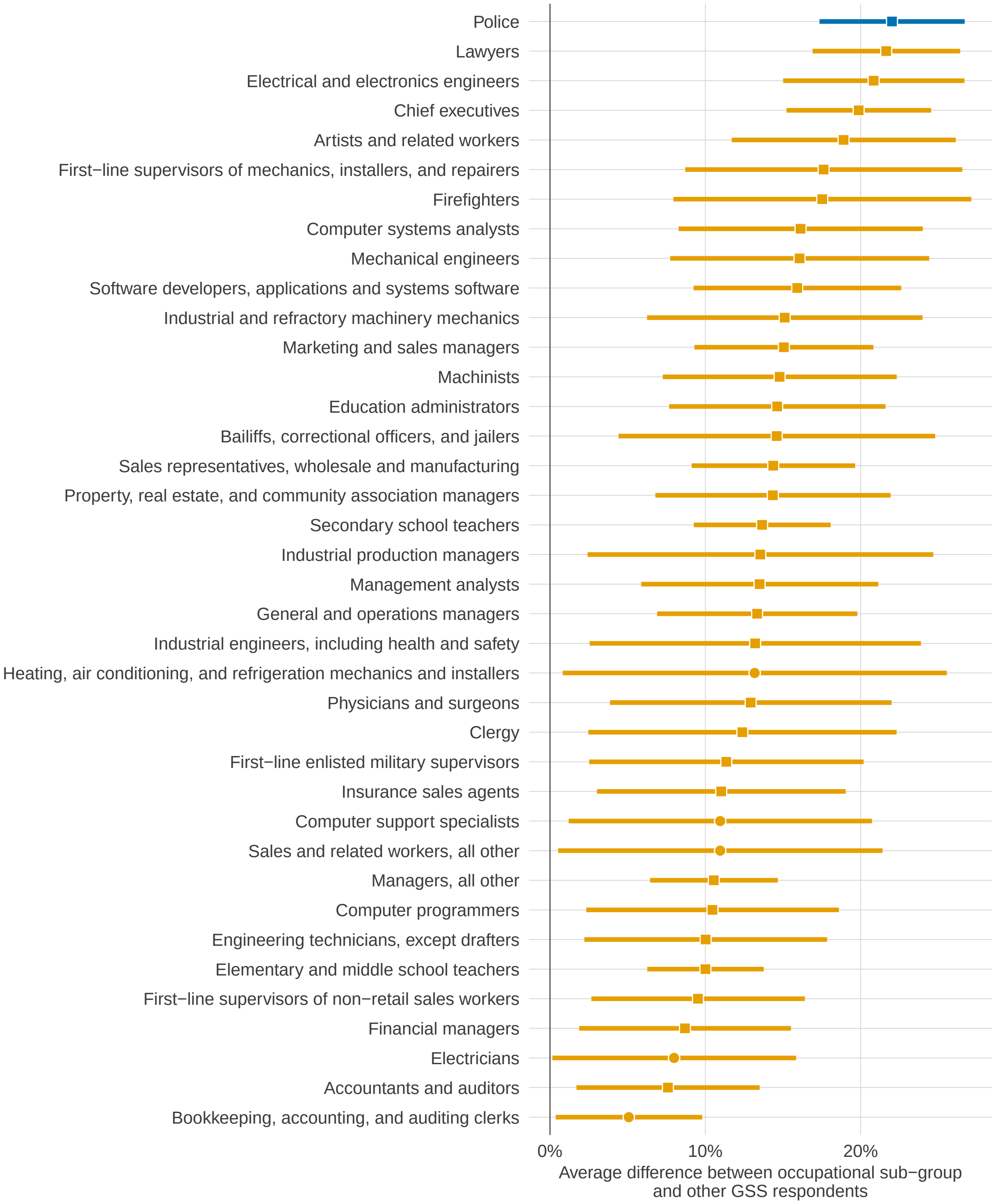
Notes: point estimates and 95% confidence intervals estimated using GSS weights. Square shaped point estimates are statistically significant after adjusting for multiple comparisons. Comparisons restricted to 140 occupational sub-groups with at least 100 observations in the 1984-2018 series. `natracey`: "Are we spending too much, too little, or about the right amount on Assistance to Blacks" (RPH coding: "Too much" = 1, "About right" = 0, "Too little" = 0).

Figure 7: Significant differences on "Racial Inequality Due Mainly to Discrimination" (`racdif1`) are not unique to police



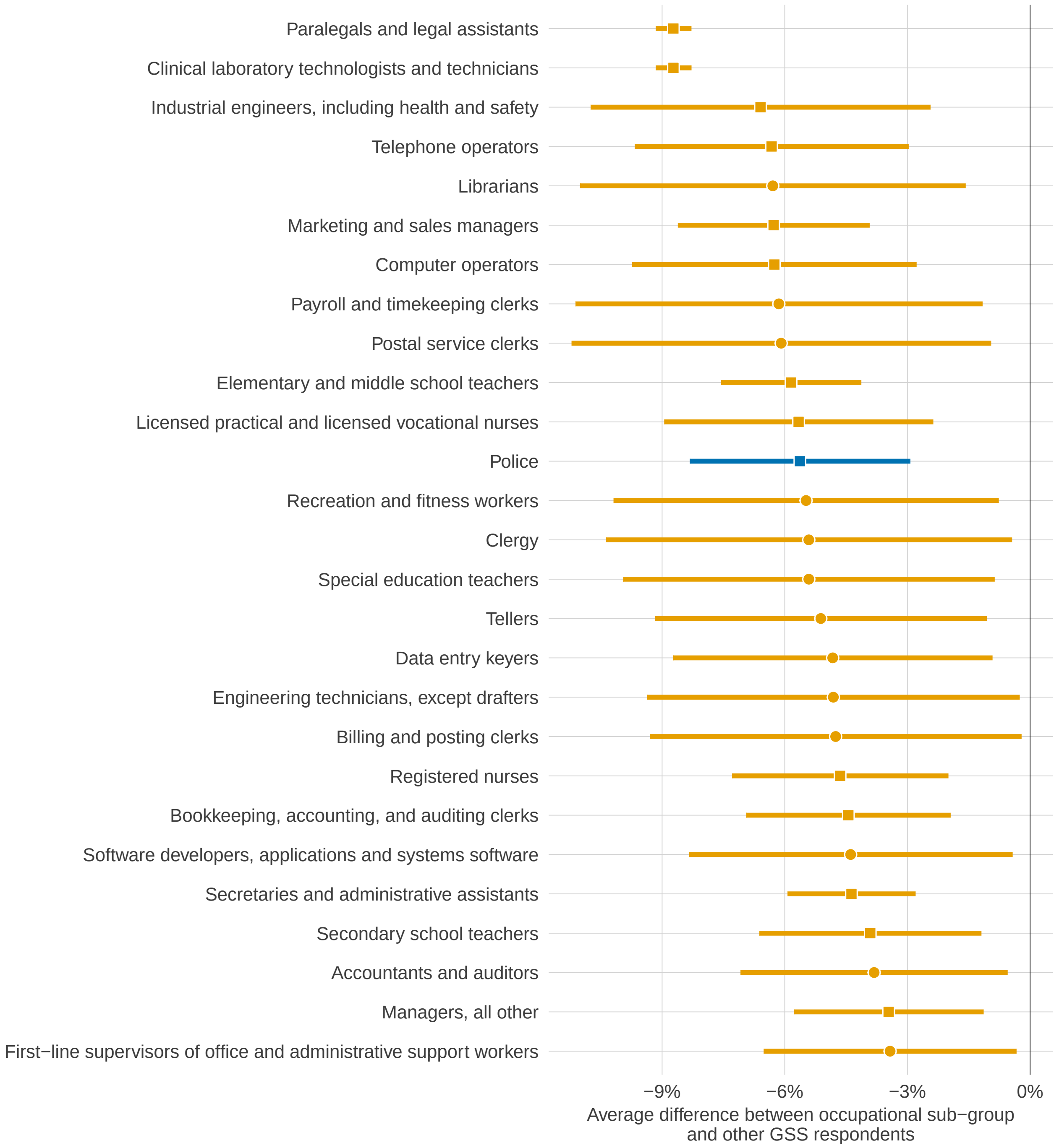
Notes: point estimates and 95% confidence intervals estimated using GSS weights. Square shaped point estimates are statistically significant after adjusting for multiple comparisons. Comparisons restricted to 140 occupational sub-groups with at least 100 observations in the 1984-2018 series. "Mainly due to discrimination" (`racdif1`) is one of four items from GSS Q# 286. "On the average Blacks have worse jobs, income, and housing than white people. Do you think these differences are ..." See GSS codebook pp. 524-525 for question wordings.

Figure 8: Significant differences on "Approve of Police Striking Adult Male" are not unique to police



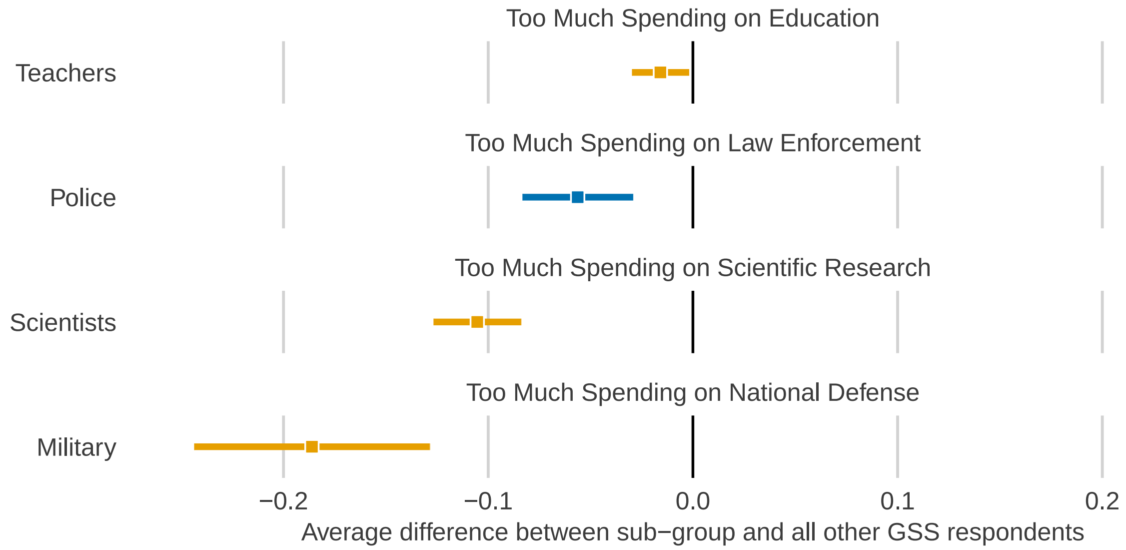
Notes: point estimates and 95% confidence intervals estimated using GSS weights. Square shaped point estimates are statistically significant after adjusting for multiple comparisons. Comparisons restricted to the 140 occupational sub-groups with at least 100 observations in the 1984-2018 series. GSS Q# 252 (`polhitok`). "Are there any situations you can imagine in which you would approve of a policeman striking an adult male citizen?" (1 = Yes, 0 = No).

Figure 9: Significant differences on "Too Much Spending on Law Enforcement" are not unique to police



Notes: point estimates and 95% confidence intervals estimated using GSS weights. Square shaped point estimates are statistically significant after adjusting for multiple comparisons. Comparisons restricted to 140 occupational sub-groups with at least 100 observations in the 1984-2018 series. `natcrimy`: "Are we spending too much, too little, or about the right amount on Law Enforcement" (RPH coding: "Too much" = 1, "About right" = 0, "Too little" = 0).

Figure 10: Average differences between Police and non-Police, Teachers and non-Teachers, Scientists and non-Scientists, Military and non-Military on "too much" spending on occupation-specific funding



Notes: point estimates and 95% confidence intervals for differences between occupational sub-groups and all other GSS respondents are estimated using GSS weights. All estimates are statistically significant after adjusting for multiple comparisons. GSS Q# 81. "We are faced with many problems in this country, none of which can be solved easily or inexpensively. I'm going to name some of these problems, and for each one I'd like you to tell me whether you think we're spending too much money on it, too little money, or about the right amount." Following RPH's coding, "Law Enforcement" (`natcrimy`), "Education" (`nateducy`), "Supporting Scientific Research" (`natsci`) and "National Defense" (`natarmsy`) are recoded as binary ("Too much" = 1, "About right" = 0, "Too little" = 0). Occupational sub-groups identified using GSS occupational codes (`occ10`). Police ($n = 141$): 3850, 3710, 3820. Teachers ($n = 890$): 2300, 2310, 2320, 2330. Scientists ($n = 108$): 1005, 1600, 1610, 1640, 1650, 1710, 1720, 1740, 1760, 1815, 1830, 1860. Military ($n = 155$): 9800, 9810, 9820, 9830

4 Conclusion

Roscigno and Preito-Hodge (2021) have published a provocative paper claiming that police uniquely believe they should receive more funding, have the right to use physical force against civilians, and are distinctly racist. As they note, recent high-profile cases of police violence against unarmed Black civilians have once again "explicitly called into question police use of force, police funding, and whether there is something systemic and fundamentally racist in police attitudes, identities, and conduct" (p. 1). These questions have significant policy implications and deserve thorough analyses. The level of public interest and potential for real-world impact only heightens the need for bold claims like those made by RPH to be supported by careful research.

The analyses reported in this note raise serious concerns about the validity of RPH's claims, even for the less ambitious inferential target of "police officers that appear in the 1984-2018 GSS." Most concerning is the evidence of selective reporting and fishing for significance using "iterative interaction modeling". The significant differences RPH report to support claims that police uniquely believe they "have the right to use physical force against civilians" (c.f. abstract) are unique to a single item, selected from a 5-part question, that simply asks respondents *whether they can imagine a situation in which they would approve of a policeman striking an adult male*. RPH do not report analyses for the four adjacent items that could plausibly approximate support for legitimate v. non-legitimate use of force. Applying their same model specification to these unreported items shows that police respondents are both less likely to support unjustified uses of force (e.g., striking a suspected murderer), and more likely to support justified uses of force (e.g., striking a person who was physically attacking an officer).

Analyses of these adjacent items do not produce statistically significant coefficients that might be used to support a narrative about the homogeneity of police as a group. Likewise, the differences that RPH report on two GSS items to support claims that police are distinctly racist do not replicate on adjacent items using their same modeling specifications. The first item does not replicate on an alternate version of the same question, and the second is distinct to a single measure selected from a four-part question. Differences on other relevant GSS items not reported in RPH's analyses, including behaviorally validated measures of racial prejudice, are likewise indistinguishable from zero.

To be clear: the absence of significant differences on measures that were not reported by RPH should **not** be interpreted as evidence of an absence of racial prejudice among police or police departments. The population of more than 700,000 police officers, spread across more than 15,000 local law enforcement agencies, is not homogeneous across time and space. Attitudinal differences between police and non-police in the GSS do not provide evidence in support of claims that U.S. police are "distinctly racist," nor do they support claims that police are "distinctly not racist".

Questions about racial bias in policing are at the center of contentious policy debates in the United States. These important questions have drawn a flood of recent attention from researchers across a wide range of disciplines. Along with this influx of interest have also come multiple examples of flawed research practices (e.g., recent examples identified by Knox and Mummolo 2020; Knox, Lowe and Mummolo 2020; Nix and Lozada 2021) and retractions (see Johnson et al., 2020; Legewie, 2019). The importance of these questions demands they be examined with care, and this note raises concerns about the validity of claims made in Roscigno and Preito-Hodge (2021)

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