SUPPORTING INFORMATION FOR
Does Private Regulation Preempt Public Regulation?
American Political Science Review

Neil Malhotra (neilm@stanford.edu)
Edith M. Cornell Professor of Political Economy
Stanford Graduate School of Business

Benoît Monin (monin@stanford.edu)
Bowen H. and Janice Arthur McCoy Professor of Ethics, Psychology, and Leadership
Stanford Graduate School of Business

Michael Tomz (tomz@stanford.edu)
Professor of Political Science, Stanford University
Senior Fellow, Stanford Institute for Economic Policy Research

Version: September 9, 2018

Table of Contents

Bluefin Tuna:

- Baseline (no VEP mentioned): Some people think the U.S. government should ban the sale of bluefin tuna in the United States, because bluefin tuna populations are at very low levels. Other people think the government should not ban the sale of bluefin tuna in the United States, because a ban would cause many people in the fishing and restaurant industries to lose their jobs. Do you think the government should or should not ban the sale of bluefin tuna in the United States? {Should ban, Should not ban, Don't know}

- Broad VEP (with deep versus shallow VEP in square brackets): Companies sometimes take voluntary steps to combat overfishing; they do more than what the government requires. Suppose that all tuna fishing companies voluntarily agree not to fish in [40 OR 20] percent of the waters where bluefin tuna live. If all tuna fishing companies take these steps without being required by the government, do you think the government should or should not ban the sale of bluefin tuna in the United States? {Should ban, Should not ban, Don't know}

- Narrow VEP (with deep versus shallow VEP in square brackets): Here is a different scenario. Suppose that half of the tuna fishing companies voluntarily agree not to fish in [40 OR 20] percent of the waters where bluefin tuna live. The other companies take no voluntary action to reduce their bluefin tuna fishing. In this scenario, do you think the government should or should not ban the sale of bluefin tuna in the United States? {Should ban, Should not ban, Don't know}

Car Fuel Efficiency:

- Baseline (no VEP mentioned): Some people think the U.S. government should require all new cars to get at least 60 miles per gallon by the year 2020. They say that emissions from cars contribute to climate change. Other people say the government should not require all new cars to get at least 60 miles per gallon by the year 2020. They say these requirements would make cars much more expensive, hurting consumers and businesses. Do you think the government should or should not require all new cars to get at least 60 miles per gallon by 2020? {Should require, should not require, Don't know}

- Broad VEP (with deep versus shallow VEP in square brackets): Companies sometimes take voluntary steps to combat climate change; they do more than what the government requires. Suppose that all car companies voluntarily agree that every new car they produce will get at least [50 OR 40] miles per gallon by the year 2020. If all car companies take these steps without being required by the government, do you think the government should or should not require all new cars to get at least 60 miles per gallon by 2020? {Should require, should not require, Don't know}

- Narrow VEP (with deep versus shallow VEP in square brackets): Here is a different scenario. Suppose that half of the car companies voluntarily agree that every new car they produce will get at least [50 OR 40] miles per gallon by 2020. The other car companies do not agree to make those improvements in fuel efficiency. In this scenario, do you think the government should or should not require all new cars to get at least 60 miles per gallon by 2020? {Should require, should not require, Don't know}

Genetically Modified Foods

- Baseline (no VEP mentioned): Scientists can change the genes in some food crops and farm animals to make them grow faster and resist pests, drought, and disease. Foods that rely on this process are called "genetically modified foods." Some people think the U.S. government should ban genetically modified foods. They say that genetically modified foods are unsafe for humans and could disrupt the environment by introducing species that did not arise naturally. Other people think the government should not ban genetically modified foods. They say that genetically modified foods are safe for humans, and they help the environment by significantly reducing the use of water, pesticides, and fertilizers. Do you think the government should or should not ban genetically modified foods? {Should ban, Should not ban, Don't know}

- Broad VEP (with deep versus shallow VEP in square brackets): Companies sometimes take voluntary steps to protect humans and the environment; they do more than what the government requires. Suppose that all food companies voluntarily agree to label all genetically modified foods beginning in the year [2014 OR 2020], so consumers can make informed decisions. If all food companies take these steps without being required by the government, do you think the government should or should not ban genetically modified foods? {Should ban, Should not ban, Don't know}

- Narrow VEP (with deep versus shallow VEP in square brackets): Here is a different scenario. Suppose that half of the food companies voluntarily agree to label all their genetically modified foods beginning in the year [2014 OR 2020]. The other food companies do not agree to label their genetically modified foods. In this scenario, do you think the government should or should not ban genetically modified foods? {Should ban, Should not ban, Don't know}

Neonic Insecticides:

- Baseline (no VEP mentioned): In the 1990s, agrochemical companies developed a new generation of insecticides called neonicotinoids, also known as neonics. These chemicals protect crops against damage by aphids, beetles, and other insects. Some people say the U.S. government should ban neonics. They say that neonics are killing the bees that pollinate crops, and are poisoning birds and other wildlife. Other people say the government should not ban neonics. They say that neonics are safe for bees, birds, and other wildlife when properly used. They also warn that banning neonics would cause crop

yields to fall by around 20 percent, hurting farmers and raising food prices. Do you think the U.S. government should or should not ban neonics? {Should ban, Should not ban, Don't know}

- Broad VEP (with deep VEP in square brackets): Companies sometimes take voluntary steps to protect the environment; they do more than what the government requires. Suppose that all neonics manufacturers voluntarily take the following steps: They offer free training for all farmers, to teach safe application methods that will not hurt bees, birds, and other wildlife [; and they voluntarily agree not to sell neonics to farmers who grow the kinds of crops that attract bees]. If all neonics manufacturers take these steps without being required by the government, do you think the government should or should not ban neonics? {Should ban, Should not ban, Don't know}

- Narrow VEP (with deep VEP in square brackets): Here is a different scenario. Suppose that half of the neonics manufacturers voluntarily start offering free training for farmers, to teach safe application methods that will not hurt bees, birds, and other wildlife [; and they voluntarily agree not to sell neonics to farmers who grow the kinds of crops that attract bees]. The other neonics manufacturers do not take these voluntary steps to train farmers [and avoid selling to farmers who grow crops that attract bees]. In this scenario, do you think the government should or should not ban neonics? {Should ban, Should not ban, Don't know}

Plastic Packaging:

- Baseline (no VEP mentioned): Some people think the U.S. government should ban plastic containers for prepackaged foods and drinks. They say the production and disposal of plastic containers hurts the environment. Other people think the government should not ban plastic containers for prepackaged foods and drinks. They say a ban would impose high costs on businesses and consumers by significantly increasing the price of food. Do you think the government should or should not ban plastic containers for prepackaged foods and drinks? {Should ban, Should not ban, Don't know}

- Broad VEP (with deep versus shallow VEP in square brackets): Companies sometimes take voluntary steps to protect the environment; they do more than what the government requires. Suppose that all food and beverage manufacturers voluntarily increase their efforts to recycle plastic, by making sure their plastic containers have at least [70 OR 30] percent recycled content. If all food and beverage manufacturers make this change without being required by the government, do you think the government should or should not ban plastic containers for prepackaged foods and drinks? {Should ban, Should not ban, Don't know}

- Narrow VEP (with deep versus shallow VEP in square brackets): Here is a different scenario. Suppose that half of the food and beverage manufacturers voluntarily increase their efforts to recycle plastic, by making sure their plastic containers have at least [70 OR 30] percent recycled content. The other food and beverage manufacturers do not

increase their efforts to recycle plastics. In this scenario, do you think the government should or should not ban plastic containers for prepackaged foods and drinks? {Should ban, Should not ban, Don't know}

Wind Turbines:

- Baseline (no VEP mentioned): Some people think the U.S. government should ban wind turbines in areas where birds might be at risk. They say that wind turbines kill hundreds of thousands of birds each year.   Other people say the government should not ban wind turbines in areas where birds might be at risk. They say such restrictions would greatly reduce the ability to generate wind power, which is cleaner than fossil fuels and does not contribute to climate change. Do you think the government should or should not ban wind turbines in areas where birds might be at risk? {Should ban, Should not ban, Don't know}

- Broad VEP (with deep versus shallow VEP in square brackets): Companies sometimes take voluntary steps to protect wildlife; they do more than what the government requires. Suppose that all wind power companies voluntarily change where they locate and how they operate turbines, and these changes reduce bird deaths from wind turbines by [60 OR 25] percent.   If all wind power companies take these steps without being required by the government, do you think the government should or should not ban wind turbines in areas where birds might be at risk? {Should ban, Should not ban, Don't know}

- Narrow VEP (with deep versus shallow VEP in square brackets): Here is a different scenario. Suppose that half of the wind power companies voluntarily make changes that reduce bird deaths by [60 OR 25] percent on their wind farms. The other companies take no voluntary action to reduce bird mortality on their wind farms.   In this scenario, do you think the government should or should not ban wind turbines in areas where birds might be at risk? {Should ban, Should not ban, Don't know}

This appendix details how we selected the levels of the deep and shallow treatments for the six different issues. We first determined what the status quo regulations were and what companies were already doing with respect to self-regulation. We then researched the extent of self-regulation that would be plausible based on current industry practices.

Plastic Packaging. Most current consumer and packaged goods (CPG) companies use very little recycled materials in their packaging (substantially less than 10%) (Sustainable Packaging Coalition 2010). There are some exceptions such as miscellaneous household care and laundry bottles, blister packs for electronics, and shampoo/conditioner bottles, with recycled content levels ranging from 25%-50%. Although some companies have pledged to have packaging be fully recycled by a certain date, many of the major CPG companies such as Coca-Cola use about 10% recycled materials (Howard 2017). Other companies have made pledges between 25%-50% (Glover 2015; Ellen Macarthur Foundation 2014). Hence, we viewed 30% recycled content as a shallow VEP given current commitments by companies and the status quo level of recycled material. Conversely, 70% recycled content (our treatment representing a deep VEP) is extremely large compared to both the status quo and even robust industry self-regulation practices.

Genetically Modified Foods. Although Europe has long required GMO labeling, at the time of our survey there were no similar requirements in the United States. Given that a few states such as California and Vermont began exploring similar legislation in 2012 at the time of our experiments, we viewed 2014 to be an aggressive date to implement labeling and therefore interpreted this deadline as a "deep VEP" (Hopkinson 2016). On the other hand, 2020 would be viewed as a late date by respondents and therefore represents a "shallow VEP." There are some voluntary labeling efforts in the U.S. For example, the "certified organic" and "non-GMO verified" labels are reserved for non-GMO foods. Some companies such as Frito-Lay and McDonalds do not source products made from GMO seeds (Lynch and Vogel 2001). Consequently, we believed that immediate action would indicate deep self-regulation.

Neonic Insecticides. Between 2009-2012, the EPA began reviewing the registration of neonics to make sure they met federal regulations on pesticides (7 U.S.C. § 136a). The EPA already requires neonics to have labels instructing proper use to avoid harm to bees (Environmental Protection Agency 2013). Therefore, requiring producers to teach safe application methods goes only slightly beyond the status quo, and can therefore be classified as a shallow VEP. Not selling to farmers who grow certain types of crops that attract bees would be a much more substantial form of self-regulation—a deep rather than a shallow VEP.

Wind Turbines. Current federal regulations penalize energy companies for bird deaths. However, there are only voluntary recommendations on siting and operation. Although there is little peer-reviewed research on how much bird deaths could be avoided via optimal siting and operation, some studies have suggested that stringent efforts could reduce bird deaths by 53-87% (Arnett et al. 2009). Consequently, we set the deep VEP treatment at 60%, and the shallow VEP treatment at a much lower level distinct from optimal siting and operation (25%).

Bluefin Tuna. Currently, there are catch limits on Bluefin tuna. Only 2-3% of the world's oceans are currently protected from tuna fishing, and this is mainly concentrated in sensitive spawning grounds such as the Gulf of Mexico and the Mediterranean (International Commission for the Conservation of Atlantic Tunas 2015). Consequently, we chose a substantially high level of protected waters (40%) for the deep VEP. Although the shallow level of self-regulation (20%)

is still large compared to the status quo protection levels, it is substantially different from the protected amount of sea represented by the deep VEP treatment.

       Car Fuel Efficiency. The CAFÉ standard implemented in 2011 requires car manufacturers to have fleets of passenger cars with 44-45 miles per gallon (MPG) by 2020. The requirements for light trucks are approximately 31 miles per gallon. The combined requirements are between 38-39 miles per gallon (477 Fed. Reg. 62623, 62641). Note that these requirements are averages whereas our self-regulation treatments stated that this would be minimum MPG standards for every produced car. We considered 40 mpg to be a "shallow VEP" because it was similar to the current CAFÉ standard whereas 50 mpg was a much more substantial increase and therefore represented a "deep VEP."

References

Arnett, Edward B., Michael Schirmacher, Manuela M.P. Huso, and John P. Hayes. 2009. "Effectiveness of Changing Wind Turbine Cut-in Speeds to Reduce Bat Fatalities at Wind Facilities." Annual Report Prepared for the Bats and Wind Energy Cooperative and the Pennsylvania Game Commission. http://www.batsandwind.org/pdf/ Curtailment_2008_Final_Report.pdf (accessed March 15, 2017)

Ellen Macarthur Foundation. 2014. *The New Plastics Economy*. https:// www.ellenmacarthurfoundation.org/assets/downloads/EllenMacArthurFoundation_NewP lasticsEconomy_21-1-2016.pdf (accessed March 15, 2017)

Environmental Protection Agency. 2013. "New Labeling for Neonicotinoid Pesticides." http://www.epa.gov/pollinator-protection/new-labeling-neonicotinoid-pesticides (accessed March 15, 2017)

Glover, Mark. 2015. "Nestle Waters Announces California-Specific Plan to Boost Recycling Content in Bottles." *Sacramento Bee*. http://www.sacbee.com/news/business/ article38091852.html (accessed March 15, 2017)

Hopkinson, Jenny. 2016. "How Vermont Beat Big Food." *Politico*. http://www.politico.com/ agenda/story/2016/03/vermont-gmo-labeling-law-national-standard-000067 (accessed March 15, 2017)

Howard, Theresa. 2017. "Coca-Cola Steps Up Plastic-Bottle Recycling." *USA Today*. http://abcnews.go.com/Business/story?id=3562575 (accessed March 15, 2017)

International Commission for the Conservation of Atlantic Tunas. 2015. *ICCAT Biennial Report*. http://www.iccat.int/Documents/BienRep/REP_EN_14-15_I-1.pdf (accessed March 15, 2017)

Lynch, Diahanna, and David Vogel. 2001. "The Regulation of GMOs in Europe and the United States: A Case-Study of Contemporary European Regulatory Politics." Council of Foreign Relations. http://www.cfr.org/agricultural-policy/ regulation-gmos-europe-united-states-case-study-contemporary-european-regulatory-politics/p8688 (accessed March 15, 2017)

Sustainable Packaging Coalition. 2010. "Guidelines for Post Consumer Recycled Content in Plastic Packaging." http://www.sustainablepackaging.org/uploads/resources/ spc_pcr_plastic_packaging.pdf (accessed March 15, 2017)

We obtained the first activist sample in collaboration with The Audubon Society. In October 2013, Audubon sent email invitations to a random sample of people who satisfied at least one of the following criteria: they were dues-paying members of Audubon and subscribers to Audubon magazine; they had donated money to Audubon in the past; or they had signed up to receive emails alerting them to take political action—such as signing petitions and contacting politicians—on environmental issues.

The invitation explained, "As a member of Audubon's community, your thoughts and opinions are tremendously valuable to us. In order to better engage supporters like you, we invite you to take a special community survey that we are conducting in collaboration with researchers at [REDACTED] University." The email contained a link to a web-based survey that we designed to study the effects of voluntary corporate action on political activity. We offered to donate $1 to Audubon if the respondent completed the survey. A total of 2,374 Audubon affiliates completed the survey between October and December 2013.

The main article summarizes the demographic and political attributes of the Audubon affiliates. Figure 1, below, maps the approximate geographic locations of respondents, based on the IP addresses used to complete the survey. (To save space the map only shows locations in the lower 48 states.) The size of each circle is proportional to the number of respondents from that geographic area. The map illustrates the geographic diversity of respondents.
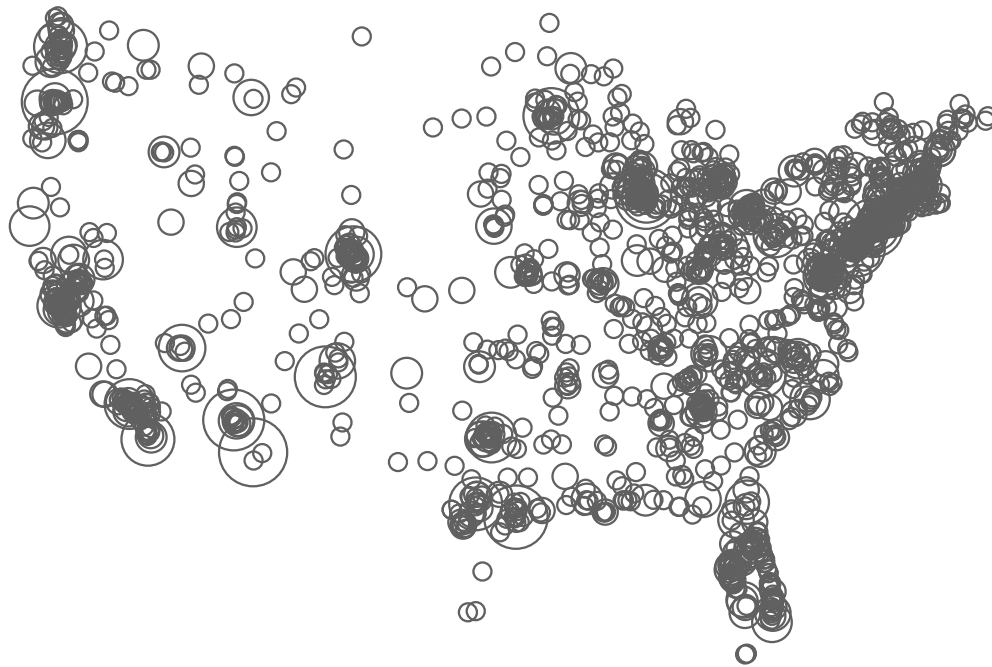
Figure 1: Geographic Locations of Audubon Affiliates

To obtain a sample of petition signatories, we cosponsored an environmental petition on Care2, a social networking website. The petition, which appeared on the Care2 website in July 2013, urged members of Congress to take strong and swift action on climate change. Figure 2 shows the advertisement for the petition, and Figure 3 gives the text of the petition itself. Respondents were told that, after signing, they would receive regular communications from Audubon and "may also receive email from [REDACTED] University about an environmental research project."

Tell Congress to Act on Climate Change

overview | letter

Target: U.S. Congress
Sponsored by: National Audubon Society and researchers from Stanford University

Twelve of the hottest 13 years on record have occurred since 2000. This May, global temperatures were the third hottest on record. Glaciers are melting across the planet. Alaska is experiencing a heat wave with temperatures in the 80s and 90s -- almost unheard of for June. Extreme weather events are increasing -- in 2012, the United States endured 11 extreme climate- and weather-related events that each caused more than $1 billion in damage.

We can do something about climate change. We can demand action from Members of Congress. Now is the time to let our lawmakers know loud and clear that we want action.

signatures: 13,001

deadline: ongoing

signature goal: 20,000

Figure 2: Overview of the Environmental Petition

9

I strongly support efforts to reduce industrial carbon pollution from power plants, while at the same time pursuing renewable energy and energy efficiency. We must take these critical steps to reduce greenhouse gas emissions that are causing dangerous climate change.

Already, climate change is producing extreme weather, which is destroying life and property across the country. Last year, Hurricane Sandy left 131 dead, demolished 380,000 homes, and created a storm surge that broke the all-time record in New York Harbor. Wildfires in the U.S. were the largest on record; they destroyed hundreds of homes and millions of acres of land. In 2012, more than 40,000 daily heat records were broken around the country, causing heat-related deaths to rise dramatically. Disease transmitted by food, water and insects are becoming more widespread. Inaction is costing lives and billions of dollars. These problems will only get worse if nothing is done.

Leading climate scientists agree that we need to reduce carbon dioxide, methane and other carbon pollution by 80% by 2050 in order to avoid the worst impacts of climate change. We must act so that we have a planet that supports life for generations to come. Poll after poll shows that a majority of Americans want climate action now.

I urge Congress to take strong and swift steps to significantly reduce carbon pollution.

Figure 3: Text of the Environmental Petition

After harvesting contact information from each signatory, we emailed them in February 2014. Our email explained: "In July, you signed an online petition encouraging Congress to act on climate change. Today, we are contacting you about another issue: fracking." We then displayed two petitions: one arguing that the U.S. government should ban fracking; the other arguing that the U.S. government should not. We asked whether they preferred the first petition, the second petition, or neither. Viewers who registered an opinion were redirected to an online survey, which invited them to express opinions about other environmental issues. That survey contained our embedded experiments. Of the 10,710 people we emailed, 1,722 (16%) clicked on our email and completed our questionnaire.

The main article summarizes the demographic and political attributes of the petition signatories. Figure 4, below, maps the approximate geographic locations of respondents, based on the IP addresses used to complete the survey. (To save space, the map only shows locations in the lower 48 states.) The size of each circle is proportional to the number of respondents from that geographic area. The map illustrates the geographic diversity of respondents.
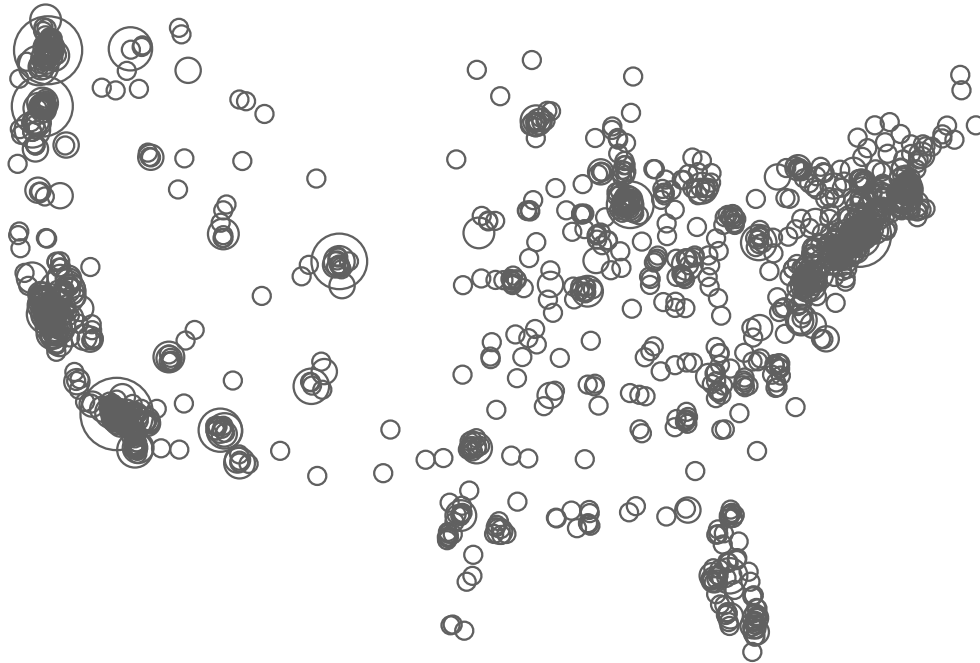
Figure 4: Geographic Locations of Petition Signatories

Survey Sampling International (SSI) provided a sample of 1,708 U.S. adults, who were interviewed in April 2014. When drawing respondents for this study, SSI used quota sampling to ensure that the gender, age, education, and race of respondents closely approximated the distributions in the U.S. Census.

To further enhance the representativeness of the sample, we constructed poststratification weights to match U.S. Census benchmarks for gender, age, education, and race, and to match the Pew Research Center's benchmarks for political party affiliation. We obtained Census benchmarks from the U.S. Census Bureau's *Current Population Survey: 2014 Annual Social and Economic Supplemen*t, which provides demographic data for U.S. adults 18 years or older.[1] Party affiliation benchmarks were based on 25,010 interviews conducted in 2014 by the Pew Research Center, which asked "In politics today, do you consider yourself a Republican, Democrat, or independent?"[2]

We constructed weights via entropy balancing, a procedure assigns a weight to each respondent in the dataset, such that the distributions of the weighted dataset match the target distributions.[3] We weighted our sample to match the marginal distribution for each of the five variables, rather than the joint distributions of the five variables.

Table 1 presents the demographic characteristics of the unweighted and weighted samples, and compares them to the attributes of the U.S. population. Before weighting, the sample was highly representative with respect to gender and age, but differed somewhat from the U.S. population on other characteristics. For example, the raw sample overrepresented people who attended college, as well as people who affiliated with the Democratic Party. The raw sample also differed from the population on certain racial categories (percentages for race sum to more than 100% because respondents could check more than one racial category). After weighting the raw sample, the percentage in each demographic category matched the U.S. population to within 0.1 percentage points. Nevertheless, as shown in Figure 25 and Figure 26, below, our conclusions do not depend on whether we weight the sample or not.

---

[1] Downloaded from http://www.census.gov/hhes/socdemo/education/data/cps/2014/tables.html on April 28, 2015.

[2] Downloaded from http://assets.pewresearch.org/wp-content/uploads/sites/5/2015/04/4-7-2015-Party-ID-Combined-Detailed-Tables.pdf on June 8, 2018

[3] Jens Hainmueller, "Entropy Balancing for Causal Effects: A Multivariate Reweighting Method to Produce Balanced Samples in Observational Studies," *Political Analysis* 20 (2012):25–46. To construct weights, we used the entropy balancing package for Stata written by Hainmueller and Yu (2013) and available at https://web.stanford.edu/~jhain/ebalancepage.html. Accessed June 8, 2018.

Table 1: Demographic Characteristics of Unweighted and Weighted Public Samples, Compared to the U.S. Population

		Sample		U.S.
		Unweighted	Weighted	Population
Gender				
	Female	52 %	52 %	52 %
	Male	48	48	48
	Not reported	0	0	
Age				
	18-44 years	48 %	47 %	47 %
	45-64 years	37	35	35
	65 and over	14	19	19
	Not reported	0	0	
Education				
	High school or less	22 %	42 %	42 %
	Some college	34	29	29
	College degree	32	19	19
	Graduate degree	12	10	10
	Not reported	0	0	
Race				
	White	71 %	79 %	79 %
	Black	17	12	12
	Latino	10	15	15
	Asian	4	6	6
	Other	2	1	1
	Not reported	0	0	
Political Party				
	Democrat	42 %	32 %	32 %
	Independent	32	39	39
	Republican	23	23	23
	Other	4	6	6
	Not reported	0	0	

Note: Sample size was 1,708

Figure 5, below, maps the approximate geographic locations of respondents, based on the IP addresses used to complete the survey. (To save space the map only shows locations in the lower 48 states.) The size of each circle is proportional to the number of respondents from that geographic area. The map illustrates the geographic diversity of respondents.
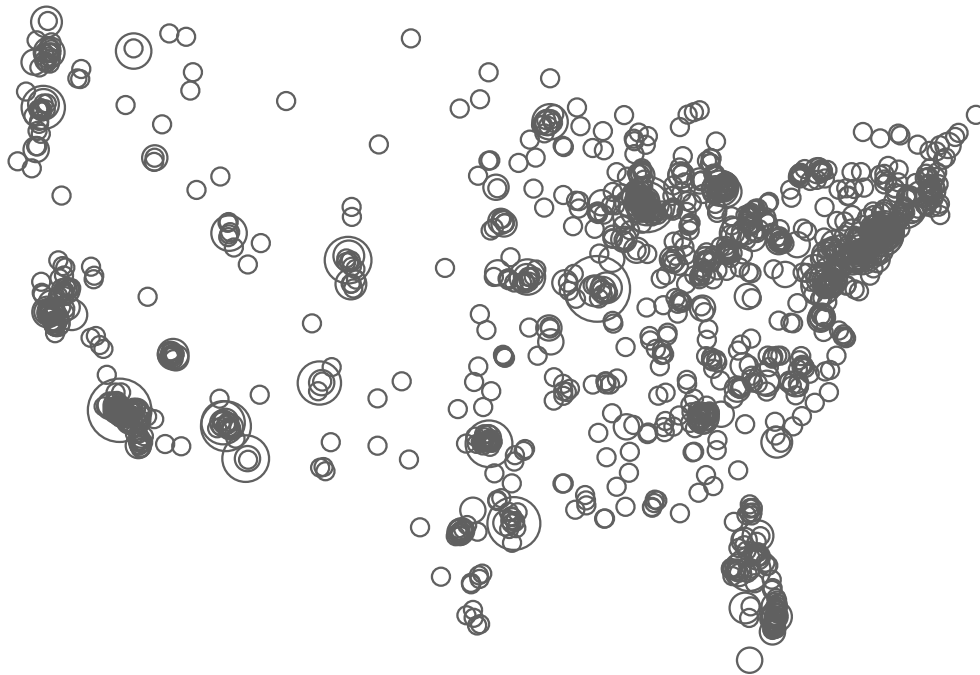
Figure 5: Geographic Locations of U.S. Public Respondents

We aimed to survey political elites who held positions at all levels of government. We obtained contact information for government officials from the firm KnowWho. The officials included a wide range of executives, legislators, bureaucrats, and staffers at the federal, state, and local level.

Our first list of government officials, constructed in 2015, contained 62,158 individuals. From this large list we drew a random sample of 10,200 officials, whom we emailed in August-October 2015 to request participation in our study. The study was not advertised as a survey about environmental issues, so we do not anticipate selection bias with respect to interest in the environment. A total of 923 completed this first study.

Table 2 shows the percentage of federal, state, and local officials in the sampling frame, the random sample of invited officials, and the people who ultimately completed the survey. The response rate was highest among local officials, but as shown in the article, the effects of VEPs on this first sample of government officials did not vary substantially by level of government.

Table 2: First Sample of Government Officials, by Level of Government

Level of Government	Frame	Invited	Completed
Federal	23 %	23 %	11 %
State	21	21	13
Local	56	56	76
N	62,158	10,200	923

The 25 most common job titles among respondents were: council member, commissioner, mayor, supervisor, board chairman, representative, board member, trustee, council president, legislative assistant, board vice chairman, director, senator, chief of staff, legislator, constituency services representative, district director, staff assistant, counsel, district representative, assembly member, council vice president, vice mayor, legislative director, and policy advisor.

Figure 6, below, maps the approximate geographic locations of respondents, based on the IP addresses used to complete the survey. (To save space the map only shows locations in the lower 48 states.) The size of each circle is proportional to the number of respondents from that geographic area. The map illustrates the geographic diversity of respondents.
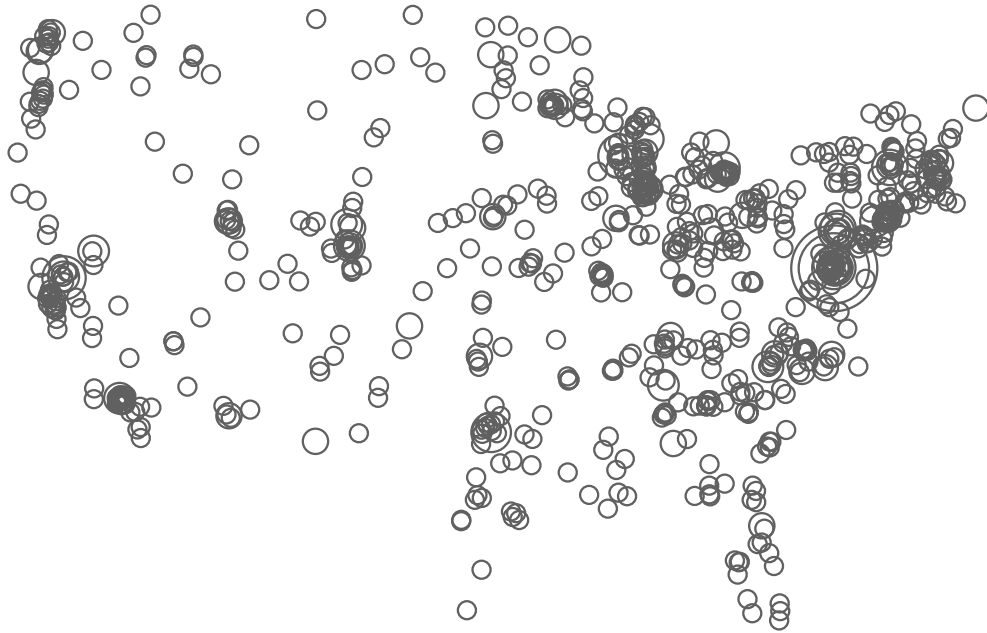
Figure 6: Geographic Locations of First Sample of Government Officials

Our second list of government officials, constructed in 2016, contained 55,854 individuals. From this large list we drew a random sample of 10,397 officials, whom we emailed in October 2016 to request participation in our study. The study was not advertised as a survey about environmental issues, so we do not anticipate selection bias with respect to interest in the environment. A total of 608 completed this second study.

Table 3 shows the percentage of federal, state, and local officials in the sampling frame, the random sample of invited officials, and the people who ultimately completed the survey. In total, 80% of respondents were from the local level, because the second list started with a higher proportion of local officials, and because the response was highest among local officials.

Table 3: Second Sample of Government Officials, by Level of Government

Level of Government	Frame	Invited	Completed
Federal	13 %	13 %	6 %
State	21	21	14
Local	66	66	80
N	55,854	10,397	608

Figure 7, below, maps the approximate geographic locations of respondents, based on the IP addresses used to complete the survey. (To save space the map only shows locations in the lower 48 states.) The size of each circle is proportional to the number of respondents from that geographic area. The map illustrates the geographic diversity of respondents.
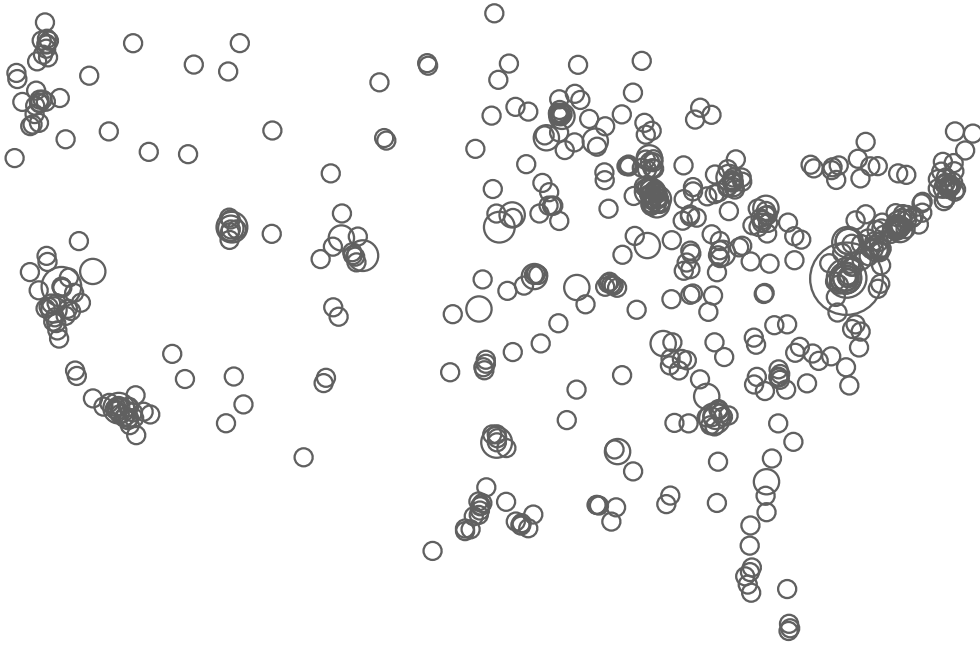
Figure 7: Geographic Locations of Second Sample of Government Officials

Effects of VEPs on Audubon affiliates, when outcome is percent support for regulation: In the article, we measured support for government regulations on a seven-point scale that was recoded to range from 0 to 100, and estimated how VEPs affected the mean value of this scale. Figure 8, below, shows that our conclusions would have remained the same if we had constructed a binary variable (support regulations or not) and estimated how VEPs affected the percentage of respondents who supported regulations. The Bonferroni method was used to adjust confidence intervals for multiple comparisons.
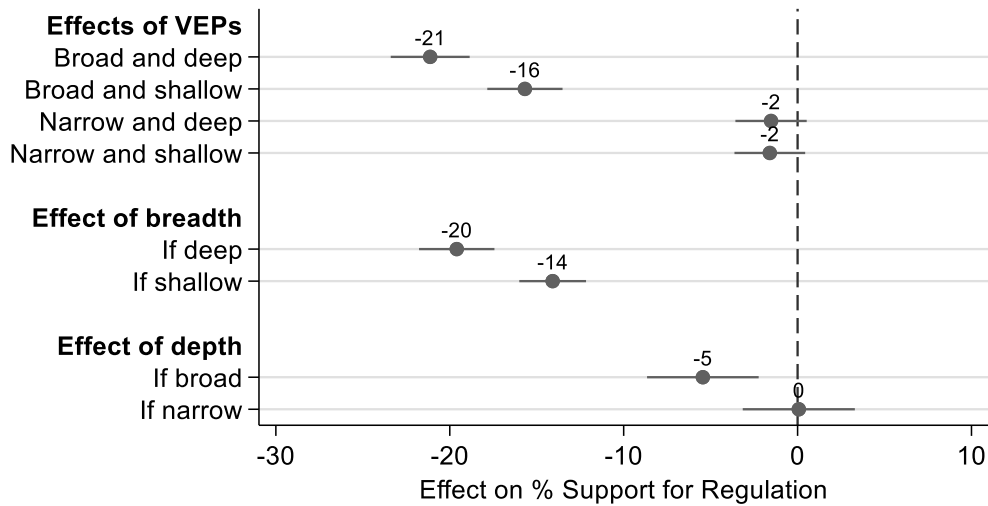
Figure 8: Effects of VEPs on Audubon Affiliates, When Outcome is Percent Support for Regulation

Effects of VEPs on Audubon affiliates, by issue number: Each Audubon respondent was asked about three issues, selected at random from a set of six. In the article we pooled the responses to all three issues. To assess the robustness of findings, we reanalyzed the data, keeping only responses from the first issue, the second issue, or the third issue. Figure 9 shows that the average effects of VEPs were similar, regardless of whether the estimates were based on the first, second, or third issue we presented.
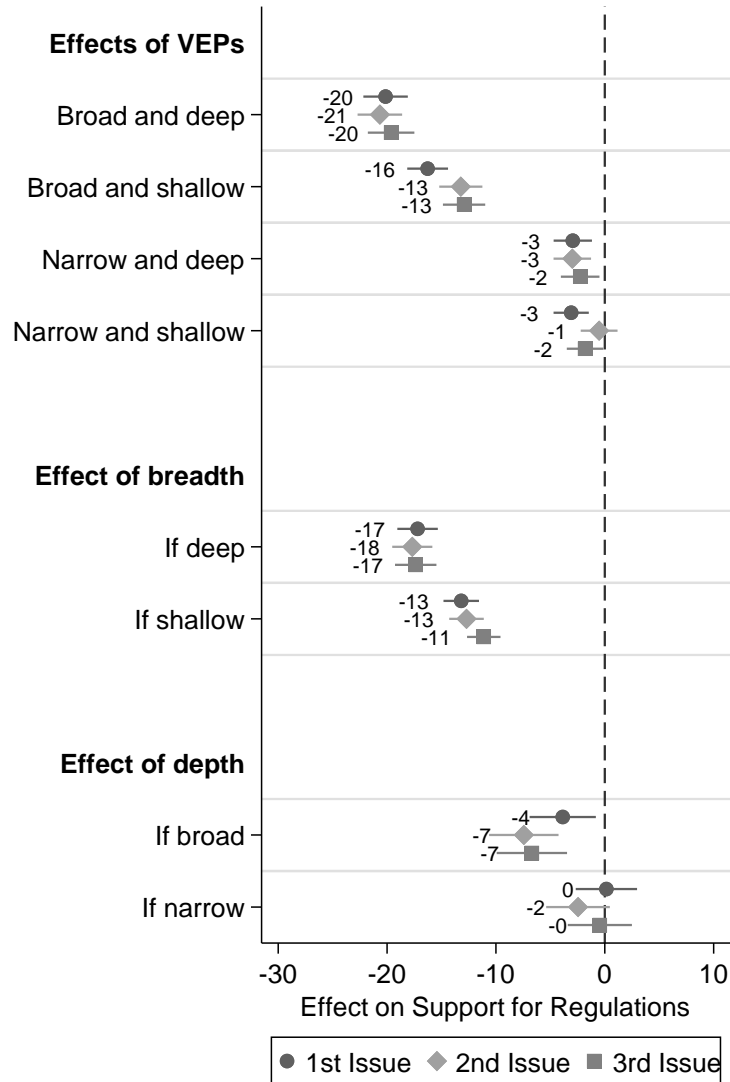
Figure 9: Effects of VEPs on Audubon Affiliates, by Issue Number

Effects of VEPs on Audubon affiliates, after dropping the third measurement: For each issue, we took three measurements of the respondent's attitudes toward government regulations (see Figure 1 of the main article). First, we measured the respondent's opinion when no VEPs were mentioned. Second, we randomly described one of four VEP scenarios—broad and deep, broad and shallow, narrow and deep, or narrow and shallow—and measured the respondent's opinion given that scenario. Third, we presented a follow-up scenario in which we changed the breadth of VEPs while holding depth constant, and measured the respondent's opinions given that scenario. In the article we use all three measurements to estimate the effects of VEPs. Figure 10 below shows that our conclusions would not change if we dropped the third measurement from the dataset.
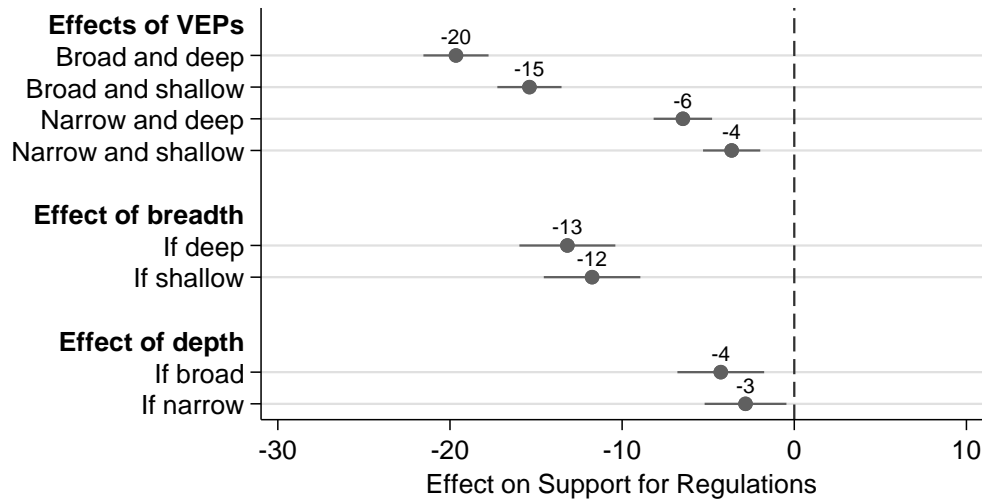
Figure 10: Effects of VEPs on Audubon Affiliates, After Dropping the Third Measurement

<u>Effects of VEPs on Audubon affiliates, using only the first issue and dropping the third measurement</u>: Figure 11 presents the estimated effects of VEPs when we restricted the analysis to the first issue and eliminated the third measurement.
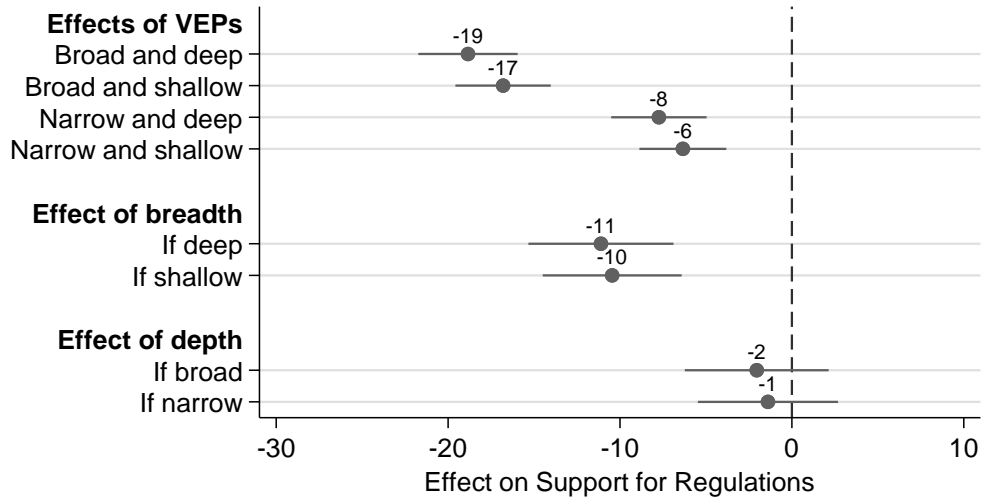
Figure 11: Effects of VEPs on Audubon Affiliates, Using Only the First Issue and Dropping the Third Measurement

Effects of VEPs on Audubon affiliates, by order in which scenarios were presented: Half of the respondents were randomly assigned to react to broad VEPs before reacting to narrow VEPs. The other half were given the opposite ordering. Figure 12 presents the effects of VEPs on these two groups. The figure shows that sequencing sometimes made a difference. In the article we average over these differences, thereby counterbalancing any variation introduced by the order in which the scenarios were presented.
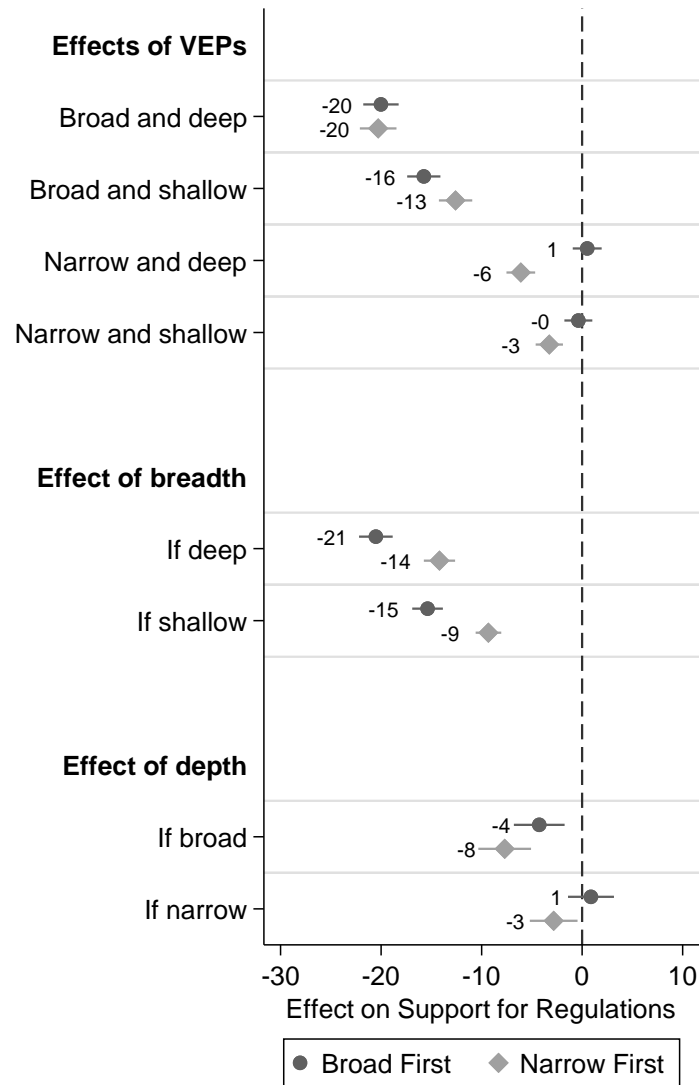
Figure 12: Effects of VEPs on Audubon Affiliates, By Order in Which Scenarios Were Presented

<u>Effects of VEPs on petition signatories, when outcome is percent support for regulation</u>: In the article, we measured support for government regulations on a seven-point scale that was recoded to range from 0 to 100, and estimated how VEPs affected the mean value of this scale. Figure 13, below, shows that our conclusions would have remained the same if we had constructed a binary variable (support regulations or not) and estimated how VEPs affected the percentage of respondents who supported regulations. The Bonferroni method was used to adjust confidence intervals for multiple comparisons.
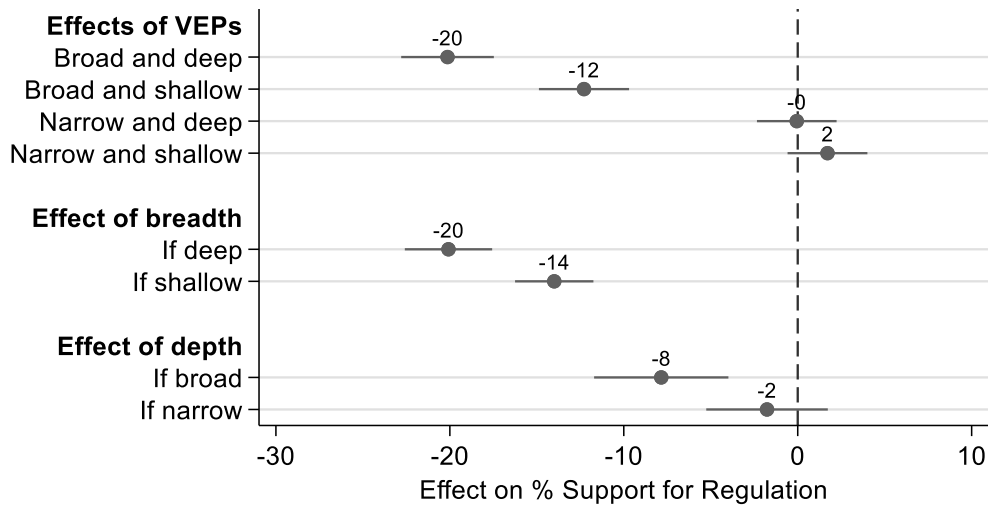
Figure 13: Effects of VEPs on Petition Signatories, When Outcome is Percent Support for Regulation

Effects of VEPs on petition signatories, by issue number: Each petition signatory was asked about three issues, selected at random from a set of six. In the article we pooled the responses to all three issues. To assess the robustness of findings, we reanalyzed the data, keeping only responses from the first issue, the second issue, or the third issue. Figure 14 shows that the average effects of VEPs were similar, regardless of whether the estimates were based on the first, second, or third issue we presented.
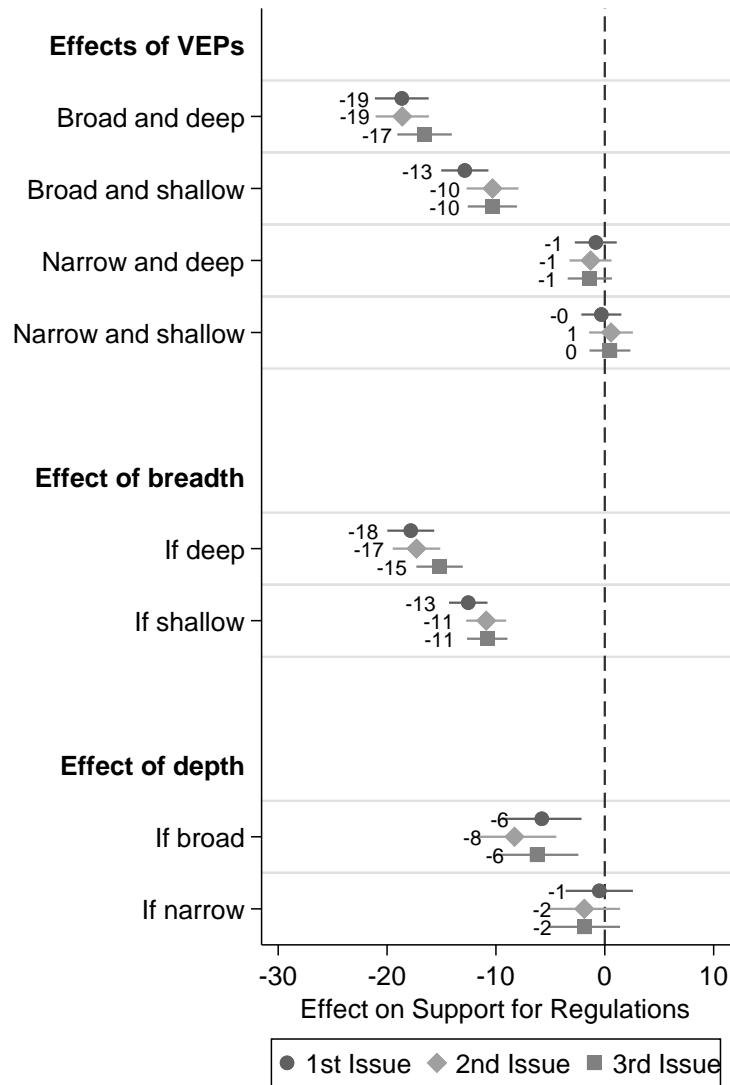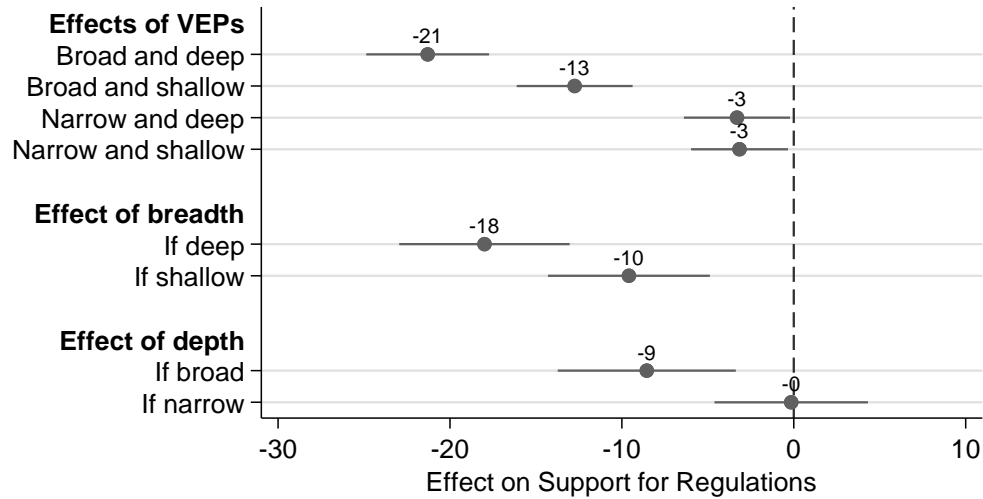

Figure 14: Effects of VEPs on Petition Signatories, by Issue Number

<u>Effects of VEPs on petition signatories, after dropping the third measurement</u>: For each issue, we took three measurements of the respondent's attitudes toward government regulations (see Figure 1 of the main article). First, we measured the respondent's opinion when no VEPs were mentioned. Second, we randomly described one of four VEP scenarios—broad and deep, broad and shallow, narrow and deep, or narrow and shallow—and measured the respondent's opinion given that scenario. Third, we presented a follow-up scenario in which we changed the breadth of VEPs while holding depth constant, and measured the respondent's opinions given that scenario. In the article we use all three measurements to estimate the effects of VEPs. Figure 15 below shows that our conclusions would not change if we dropped the third measurement from the dataset.
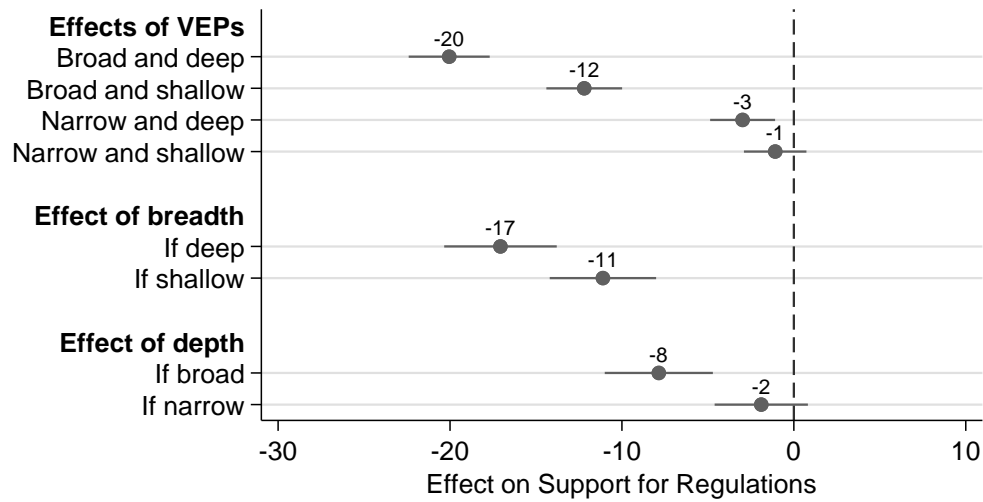
Figure 15: Effects of VEPs on Petition Signatories, After Dropping the Third Measurement

<u>Effects of VEPs on petition signatories, using only the first issue and dropping the third measurement</u>: Figure 16 presents the estimated effects of VEPs when we restricted the analysis to the first issue and eliminated the third measurement.
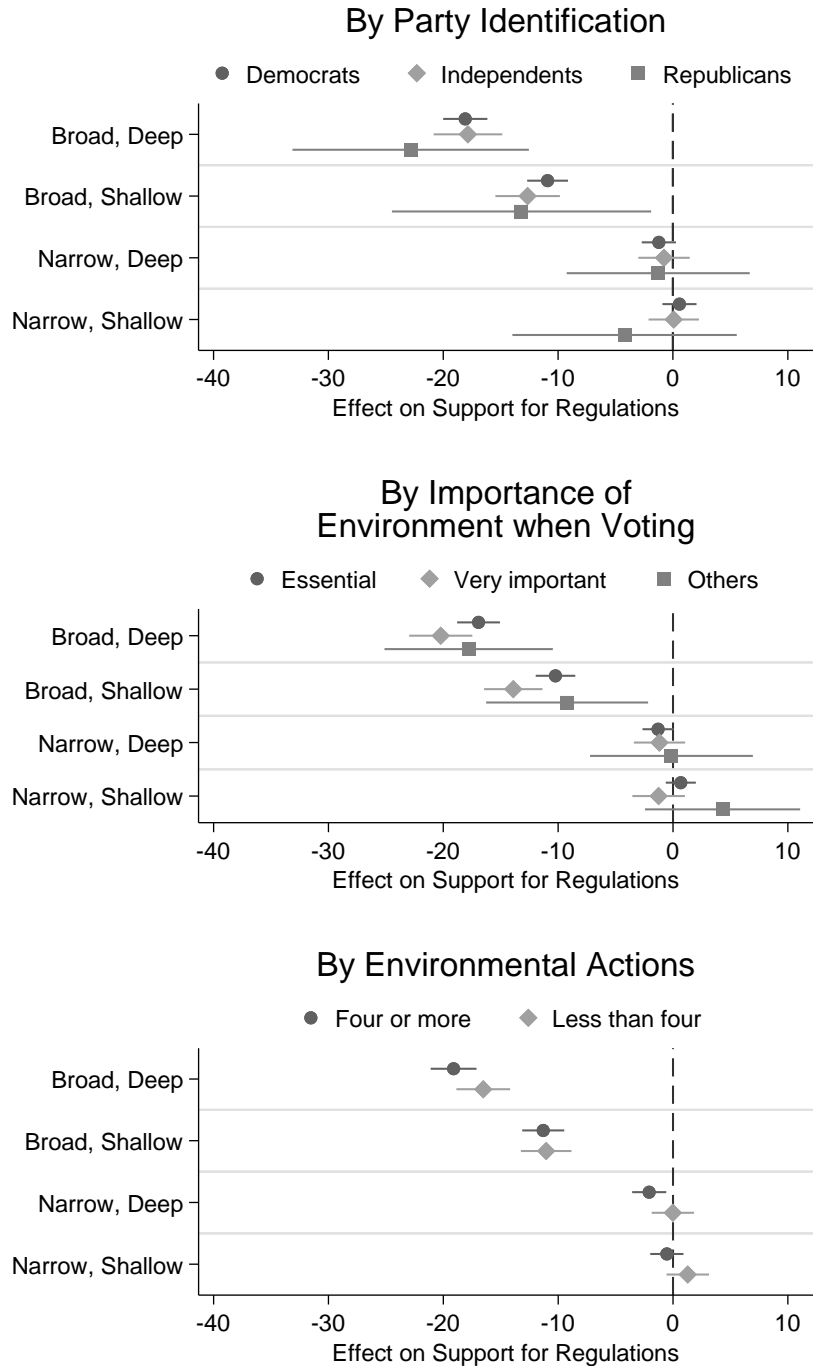

Figure 16: Effects of VEPs on Petition Signatories, Using Only the First Issue and Dropping the Third Measurement

Effects of VEPs on petition signatories, by order in which scenarios were presented: Half of the respondents were randomly assigned to react to broad VEPs before reacting to narrow VEPs. The other half were given the opposite ordering. Figure 17 presents the effects of VEPs on these two groups. The figure shows that sequencing sometimes made a difference. In the article we average over these differences, thereby counterbalancing any variation introduced by the order in which the scenarios were presented.
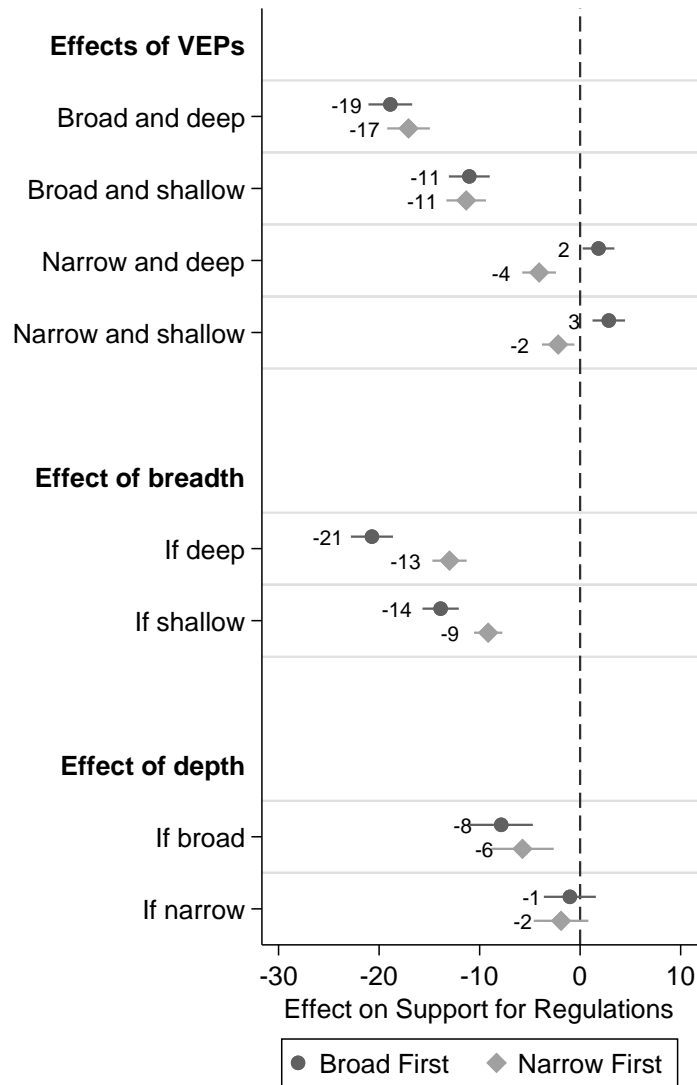
Figure 17: Effects of VEPs on Petition Signatories, By Order in Which Scenarios Were Presented

Effects of VEPs on petition signatories, by political subgroup: The article presents the effects of VEPs on Audubon affiliates, by political subgroup. Below, we present analogous estimates for the effects of VEPs on petition signatories, by political subgroup.

Figure 18: Effects of VEPs on Petition Signatories, By Political Subgroup
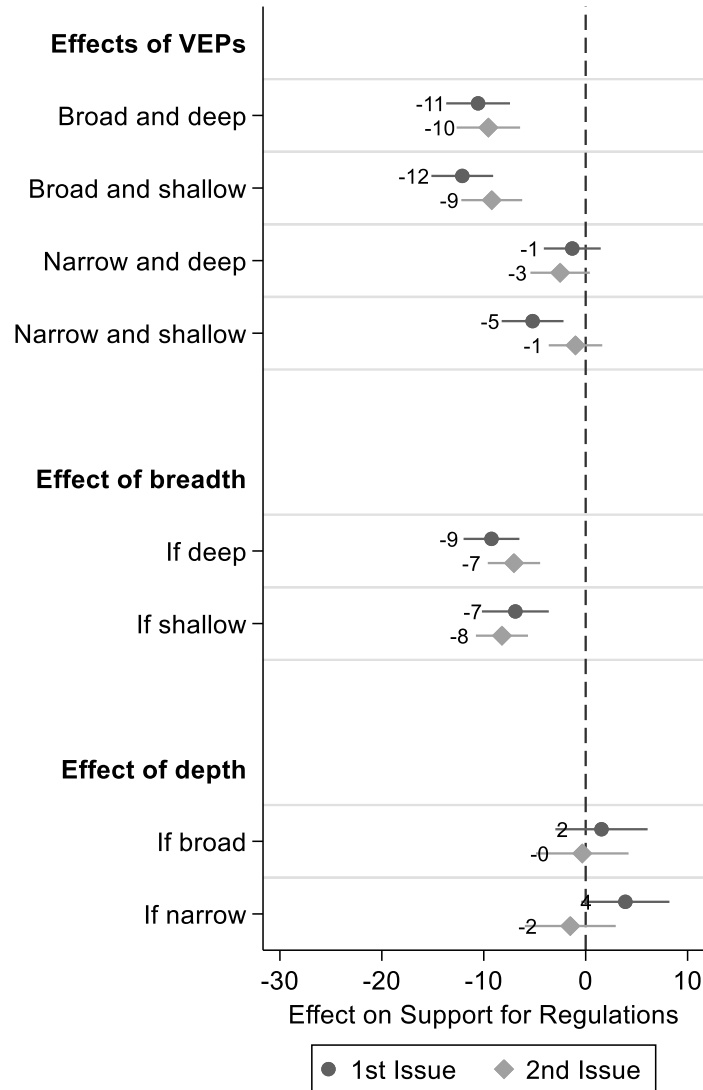
Effects of VEPs on the mass public, when outcome is percent support for regulation: In the article, we measured support for government regulations on a seven-point scale that was recoded to range from 0 to 100, and estimated how VEPs affected the mean value of this scale. Figure 19, below, shows that our conclusions would have remained the same if we had constructed a binary variable (support regulations or not) and estimated how VEPs affected the percentage of respondents who supported regulations. The Bonferroni method was used to adjust confidence intervals for multiple comparisons.
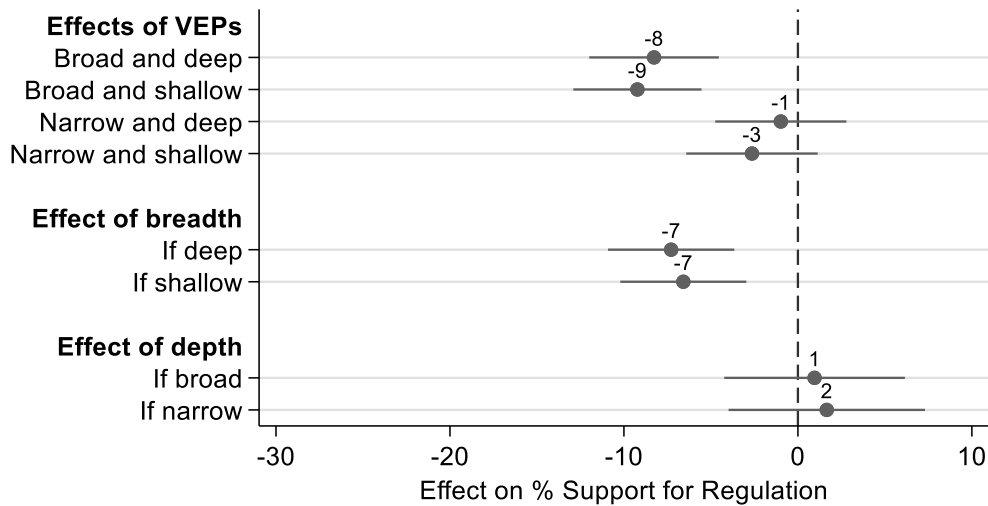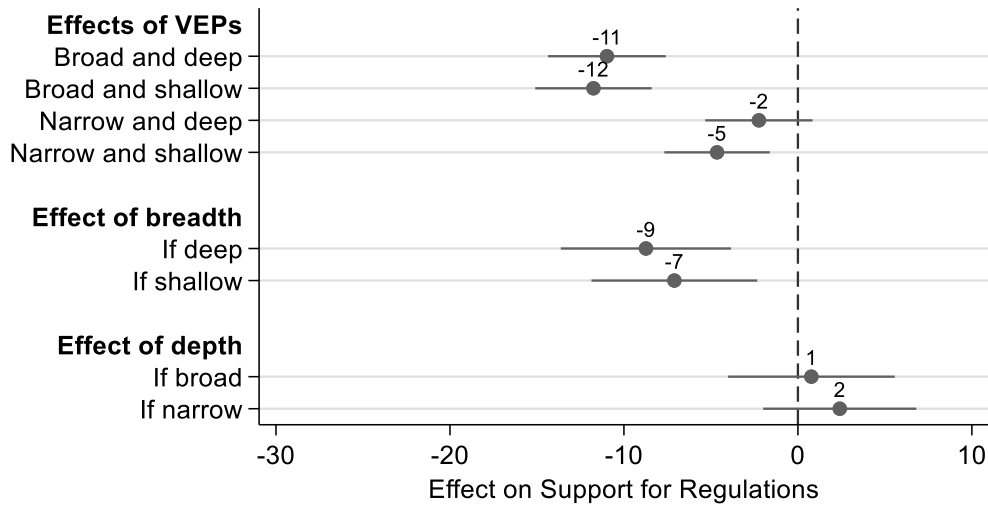

Figure 19: Effects of VEPs on the Mass Public, When Outcome is Percent Support for Regulation

<u>Effects of VEPs on the mass public, by issue number</u>: Each respondent from the mass public was asked about two issues, selected at random from a set of six. In the article we pooled the responses from both issues. To assess the robustness of findings, we reanalyzed the data, keeping only responses from the first issue or the second issue. Figure 20 shows that the average effects of VEPs were similar, regardless of whether the estimates were based on the first or the second issue we presented.

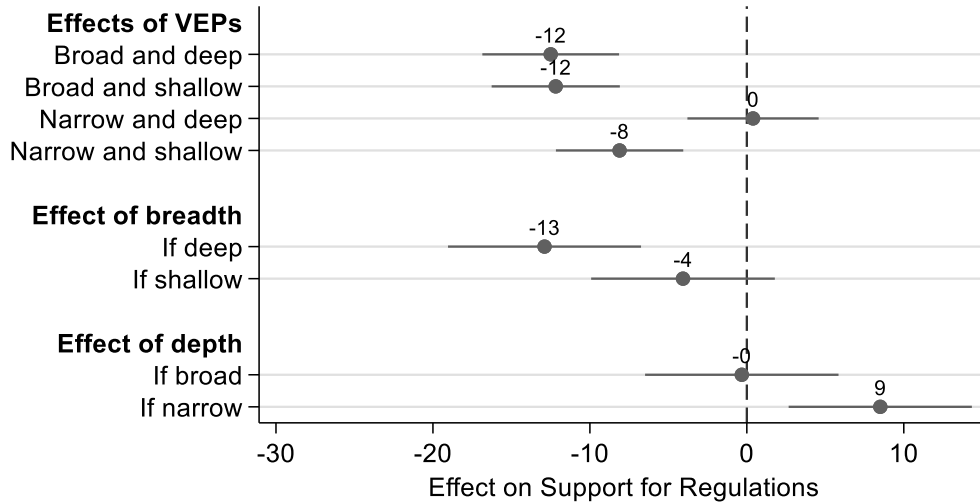**Figure 20: Effects of VEPs on the Mass Public, by Issue Number**

31

Effects of VEPs on the mass public, after dropping the third measurement: For each issue, we took three measurements of the respondent's attitudes toward government regulations (see Figure 1 of the main article). First, we measured the respondent's opinion when no VEPs were mentioned. Second, we randomly described one of four VEP scenarios—broad and deep, broad and shallow, narrow and deep, or narrow and shallow—and measured the respondent's opinion given that scenario. Third, we presented a follow-up scenario in which we changed the breadth of VEPs while holding depth constant, and measured the respondent's opinions given that scenario. In the article we use all three measurements to estimate the effects of VEPs. Figure 21 below shows that our conclusions would not change if we dropped the third measurement from the dataset.

Figure 21: Effects of VEPs on the Mass Public, After Dropping the Third Measurement
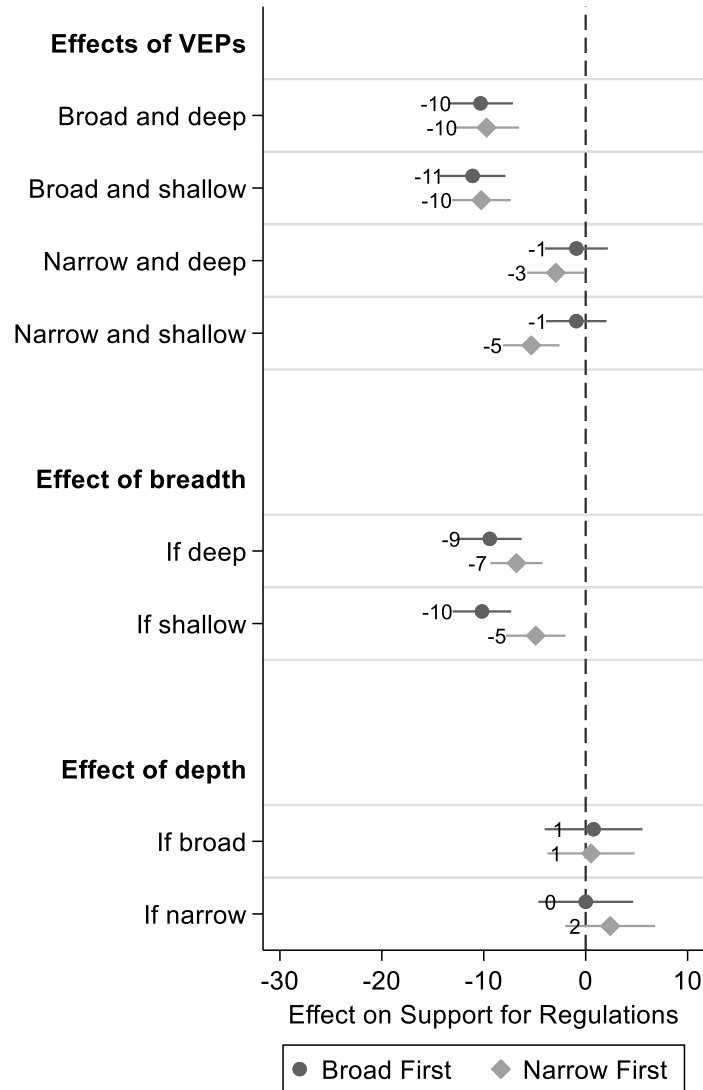
<u>Effects of VEPs on the mass public, using only the first issue and dropping the third</u>
<u>measurement</u>: Figure 22 presents the estimated effects of VEPs when we restricted the analysis
to the first issue and eliminated the third measurement.

Figure 22: Effects of VEPs on the Mass Public, Using Only the First Issue and Dropping

the Third Measurement

: Half of the respondents were randomly assigned to react to broad VEPs before reacting to narrow VEPs. The other half were given the opposite ordering. Figure 23 presents the effects of VEPs on these two groups. The figure shows that sequencing sometimes made a difference. In the article we average over these differences, thereby counterbalancing any variation introduced by the order in which the scenarios were presented.
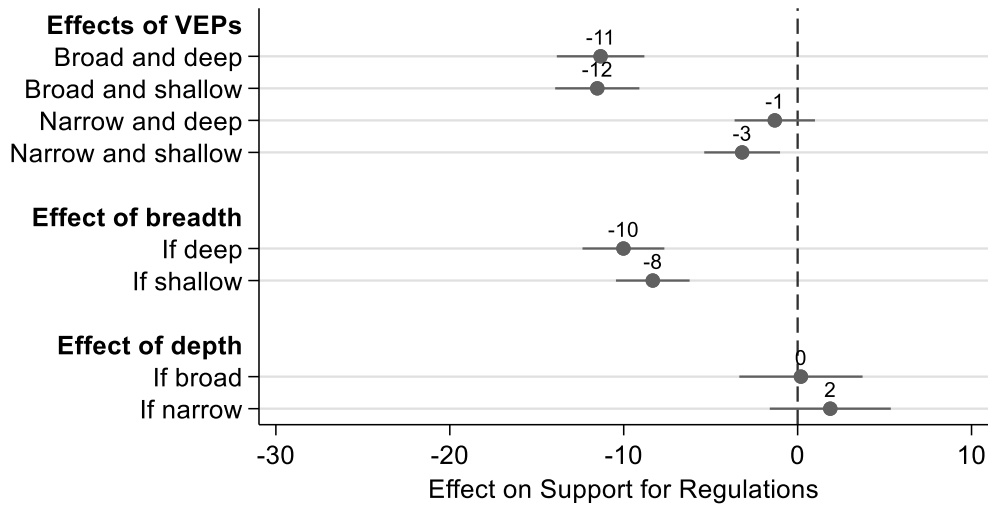

Figure 23: Effects of VEPs on the Mass Public, By Order in Which Scenarios Were Presented

Effects of VEPs on the mass public, by political subgroup: The article presents the effects of VEPs on Audubon affiliates, by political subgroup. Below, we present analogous estimates for the effects of VEPs on the mass public, by political subgroup.
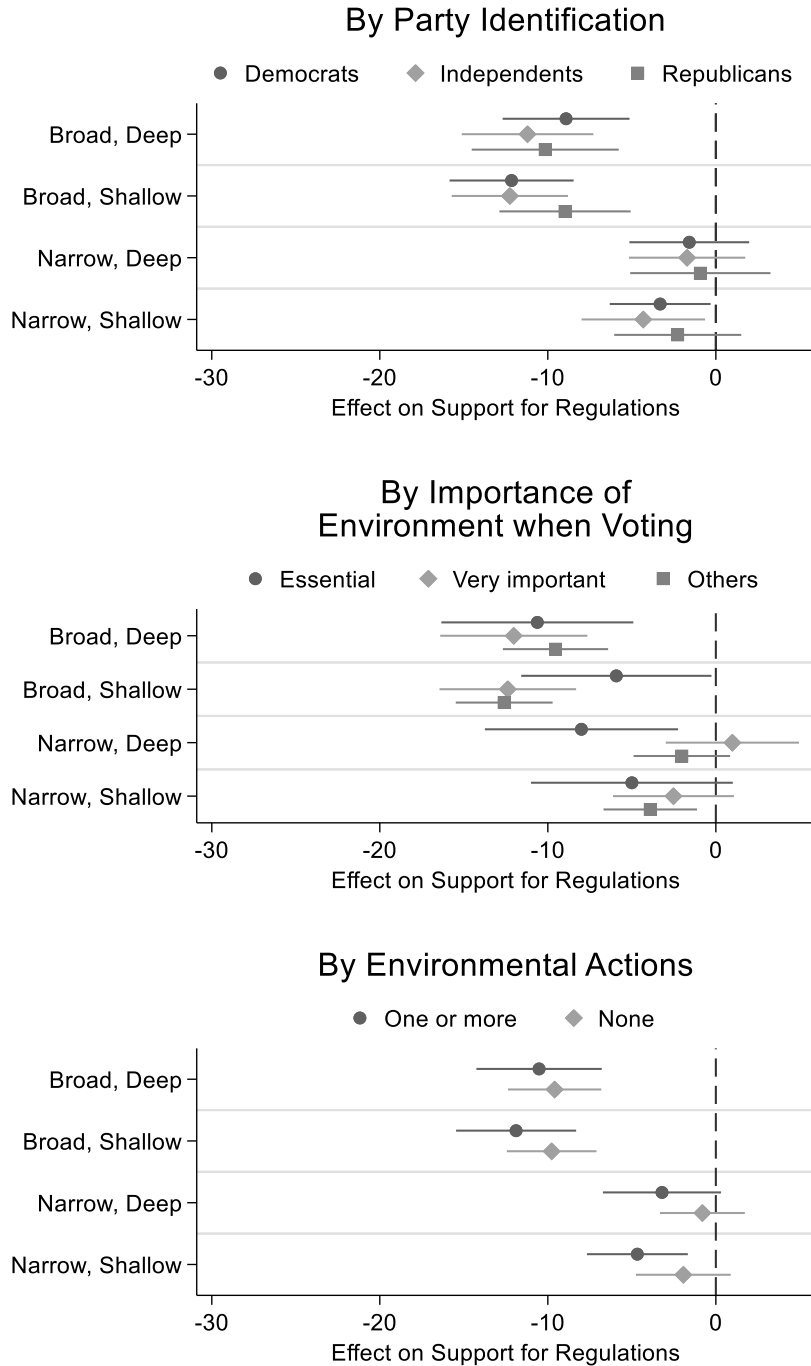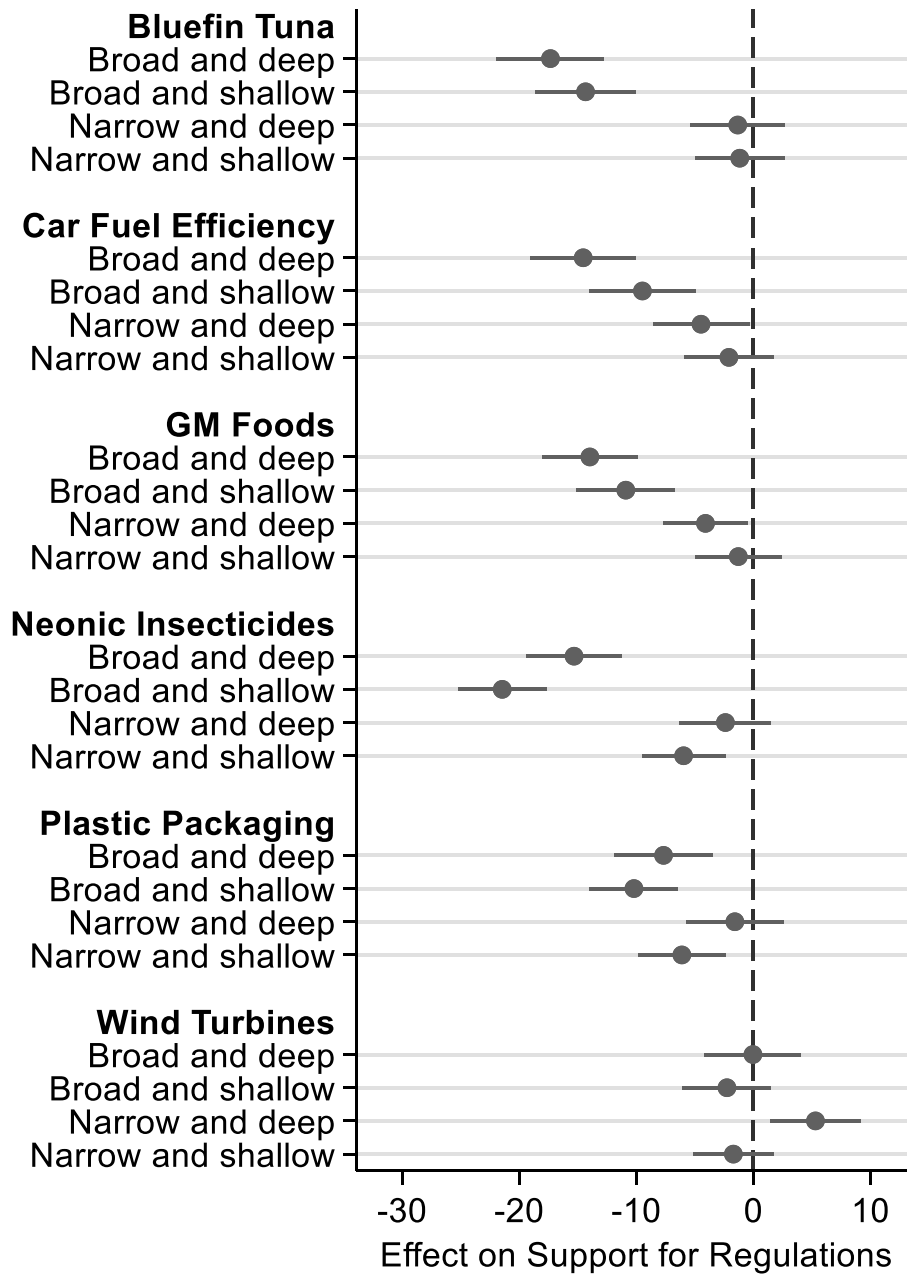

Figure 24: Effects of VEPs on the Mass Public, By Political Subgroup

Effects of VEPs on the mass public, unweighted: In the article, the mass public sample is weighted to match U.S. Census benchmarks for gender, age, education, and race, and to match the Pew Research Center's benchmarks for political party affiliation. Figure 25, below, shows that our conclusions would not change if we presented unweighted analyses instead of weighted ones. The Bonferroni method was used to adjust confidence intervals for multiple comparisons.

**Figure 25: Effects of VEPs on the Mass Public, Unweighted**

<u>Effects of VEPs on the mass public, by issue, unweighted:</u> Figure 26, below, presents the unweighted effects of VEPs on each issue.

Figure 26: Effects of VEPs on the Mass Public, By Issue, Unweighted

Effects of VEPs on the first sample of government officials, when outcome is percent support for regulation: In the article, we measured support for government regulations on a seven-point scale that was recoded to range from 0 to 100, and estimated how VEPs affected the mean value of this scale. Figure 27, below, shows that our conclusions would have remained the same if we had constructed a binary variable (support regulations or not) and estimated how VEPs affected the percentage of respondents who supported regulations. The Bonferroni method was used to adjust confidence intervals for multiple comparisons.
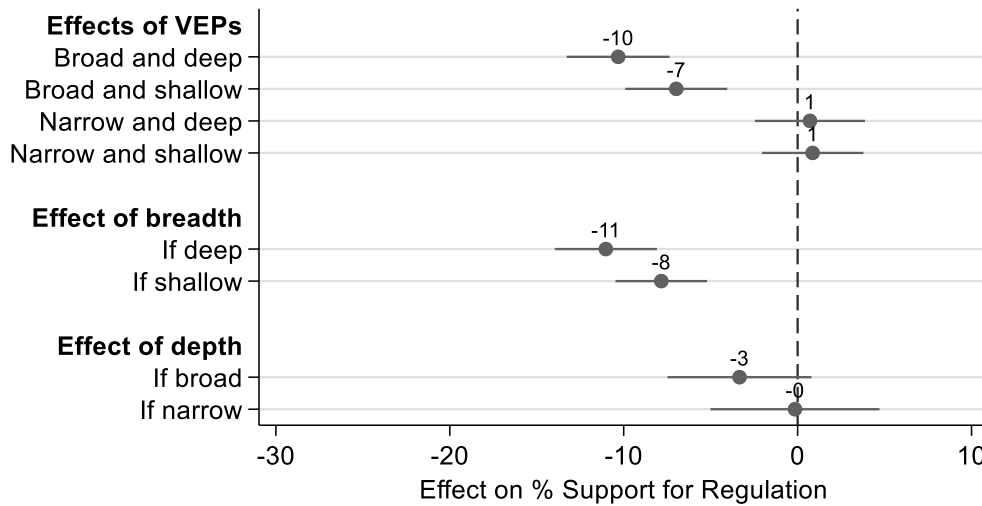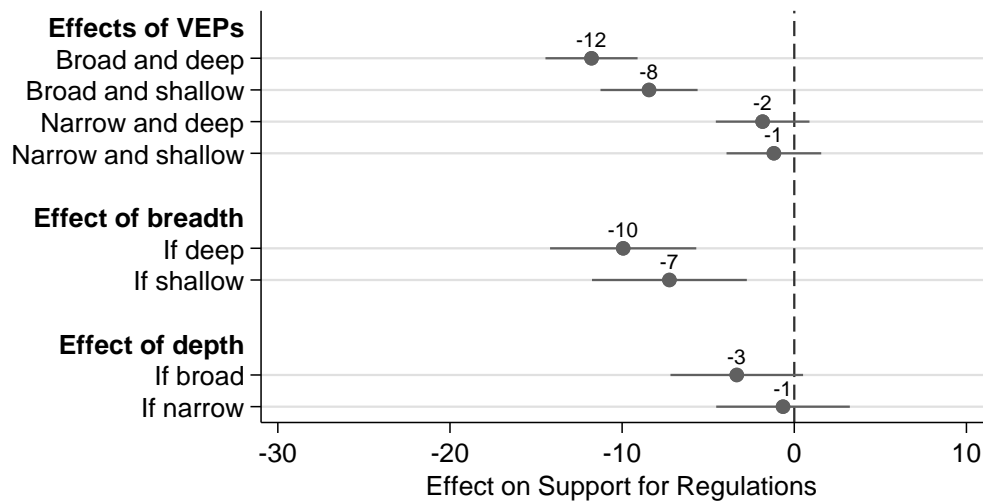

Figure 27: Effects of VEPs on the First Sample of Government Officials, When Outcome is Percent Support for Regulation

<u>Effects of VEPs on the first sample of government officials, by issue number</u>: Each respondent was asked about three issues, selected at random from a set of six. In the article we pooled the responses to all three issues. To assess the robustness of findings, we reanalyzed the data, keeping only responses from the first issue, the second issue, or the third issue. Figure 28 shows that the average effects of VEPs were similar, regardless of whether the estimates were based on the first, second, or third issue we presented.
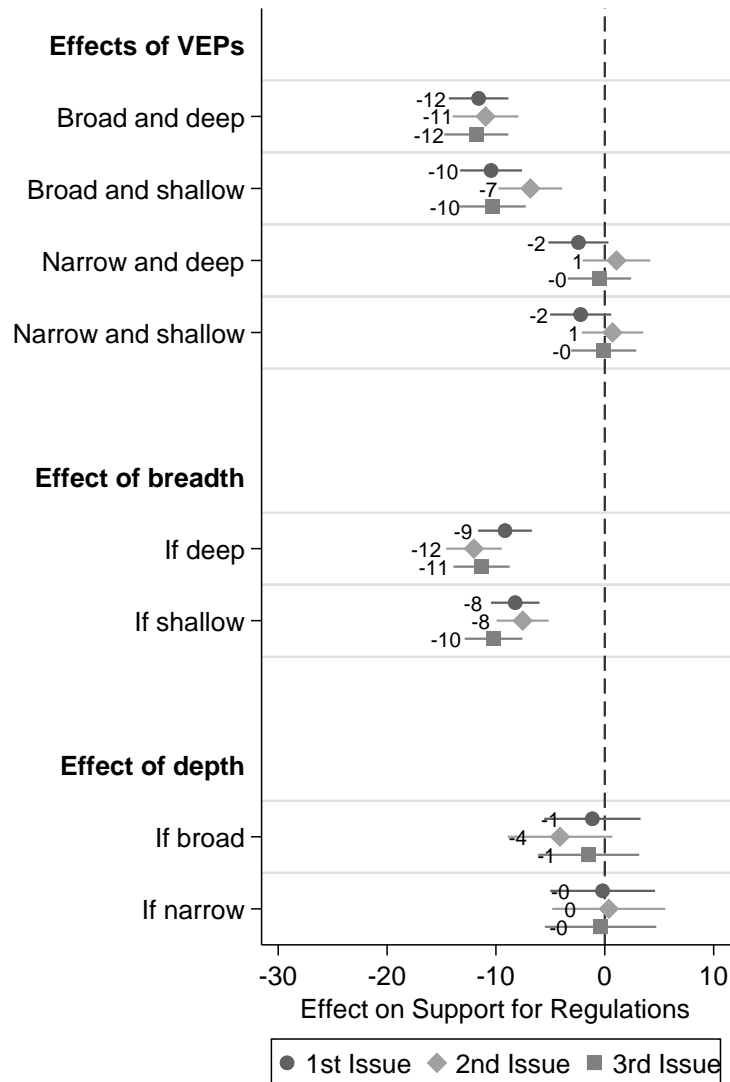
Figure 28: Effects of VEPs on the First Sample of Government Officials, by Issue Number
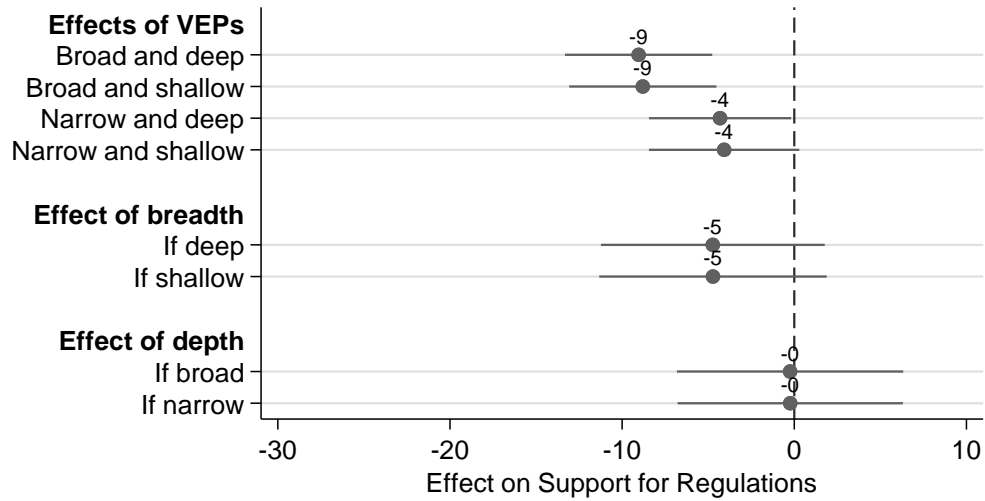
<u>Effects of VEPs on the first sample of government officials, after dropping the third measurement</u>: For each issue, we took three measurements of the respondent's attitudes toward government regulations (see Figure 1 of the main article). First, we measured the respondent's opinion when no VEPs were mentioned. Second, we randomly described one of four VEP scenarios—broad and deep, broad and shallow, narrow and deep, or narrow and shallow—and measured the respondent's opinion given that scenario. Third, we presented a follow-up scenario in which we changed the breadth of VEPs while holding depth constant, and measured the respondent's opinions given that scenario. In the article we use all three measurements to estimate the effects of VEPs. Figure 29 below shows that our conclusions would not change if we dropped the third measurement from the dataset.

Figure 29: Effects of VEPs on the First Sample of Government Officials, After Dropping the Third Measurement

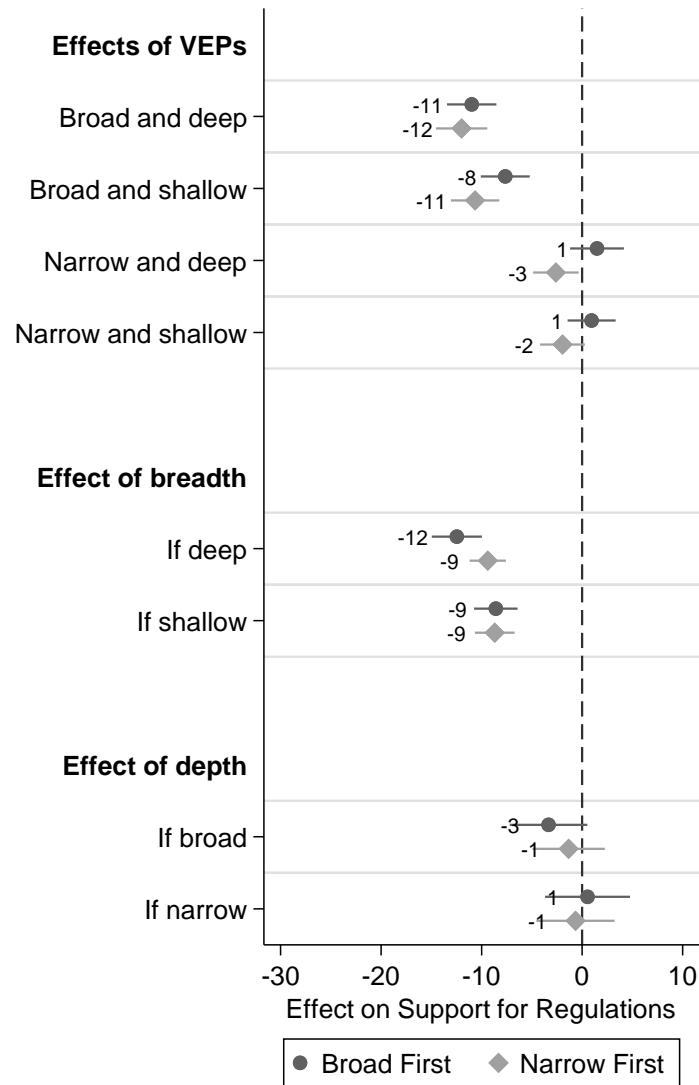<u>Effects of VEPs on the first sample of government officials, using only the first issue and dropping the third measurement</u>: Figure 30 presents the estimated effects of VEPs when we restricted the analysis to the first issue and eliminated the third measurement.

Figure 30: Effects of VEPs on the First Sample of Government Officials, Using Only the First Issue and Dropping the Third Measurement

: Half of the respondents were randomly assigned to react to broad VEPs before reacting to narrow VEPs. The other half were given the opposite ordering. Figure 31 presents the effects of VEPs on these two groups. The figure shows that sequencing sometimes made a difference. In the article we average over these differences, thereby counterbalancing any variation introduced by the order in which the scenarios were presented.

Figure 31: Effects of VEPs on the First Sample of Government Officials, By Order in Which Scenarios Were Presented

Effects of VEPs on the first sample of government officials, by issue: Figure 32, below, presents the effects of VEPs on each issue.
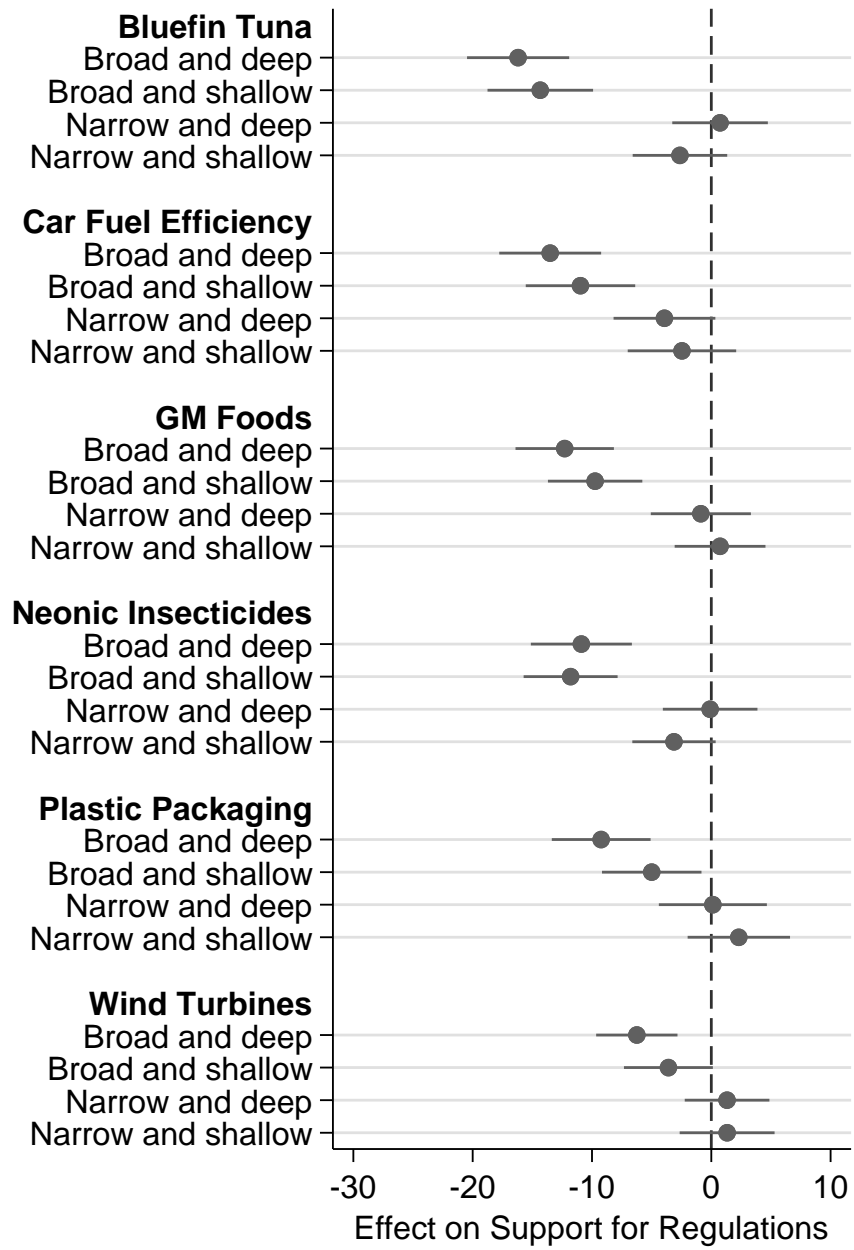
Figure 32: Effects of VEPs on the First Sample of Government Officials, By Issue

Effects of VEPs on the second sample of government officials, when outcome is percent support for regulation: In the article, we measured support for government regulations on a seven-point scale that was recoded to range from 0 to 100, and estimated how VEPs affected the mean value of this scale. Figure 33, below, shows that our conclusions would have remained the same if we had constructed a binary variable (support regulations or not) and estimated how VEPs affected the percentage of respondents who supported regulations.
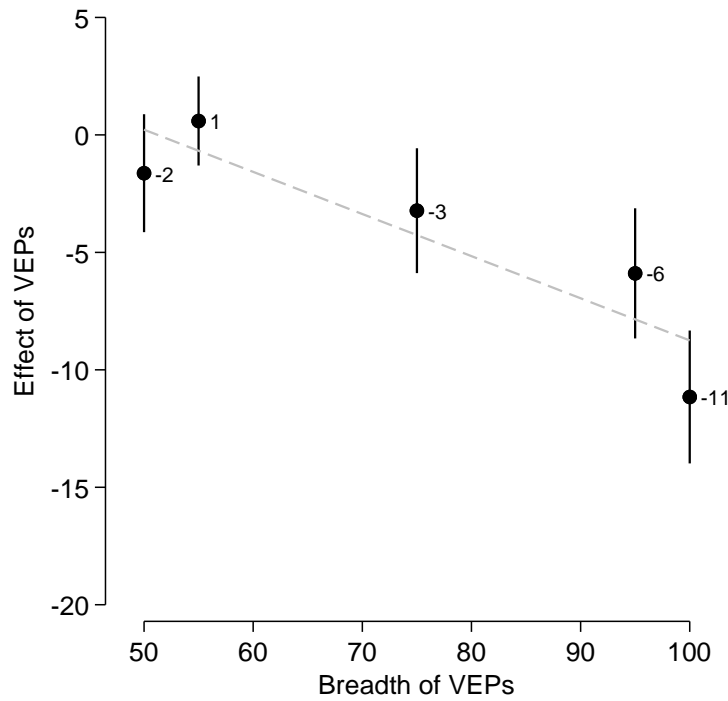
Figure 33: Effects of VEPs on the Second Sample of Government Officials, When Outcome is Percent Support for Regulation

Effects of VEPs on the second sample of government officials, by issue number: Each respondent was asked about four issues, presented in random order. In the article we pooled the responses to all three issues. To assess the robustness of findings, we reanalyzed the data, keeping only responses from the first issue, the second issue, the third issue, or the fourth issue. Figure 34 shows that the average effects of VEPs were similar, regardless of whether the estimates were based on the first, second, third, or fourth issue we presented.
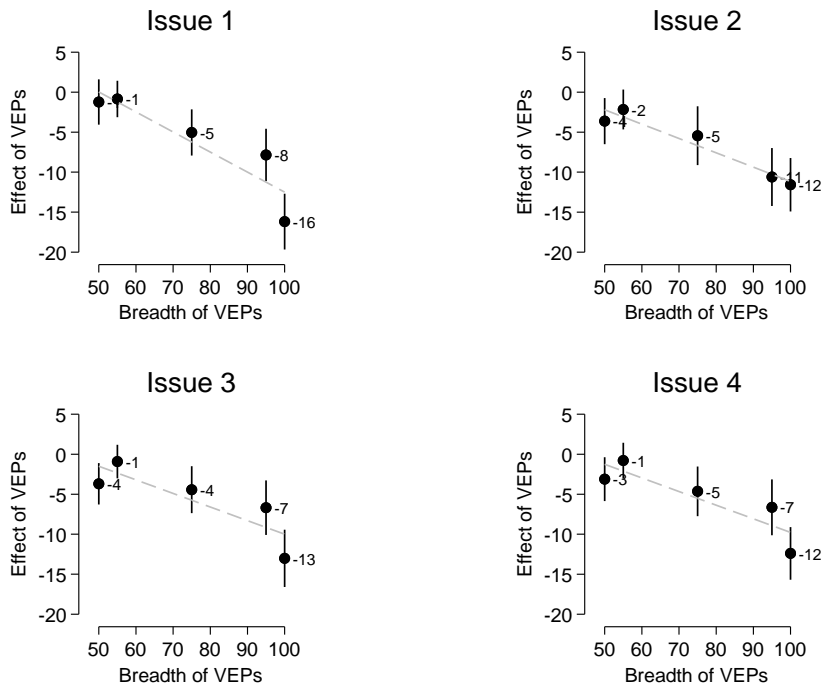
Figure 34: Effects of VEPs on the Second Sample of Government Officials, by Issue Number

<u>Effects of VEPs on the second sample of government officials, after dropping the third measurement</u>: For each issue, we took three measurements of the respondent's attitudes toward government regulations. First, we measured the respondent's opinion when no VEPs were mentioned. Second, we randomly described one of five VEP scenarios (50%, 55%, 75%, 95%, or 100% participation) and measured the respondent's opinion given that scenario. Third, we presented a follow-up scenario in which we changed the breadth of VEPs while holding depth constant, and measured the respondent's opinions given that scenario. In the article we use all three measurements to estimate the effects of VEPs. Figure 35 below shows that our conclusions would not change if we dropped the third measurement from the dataset.
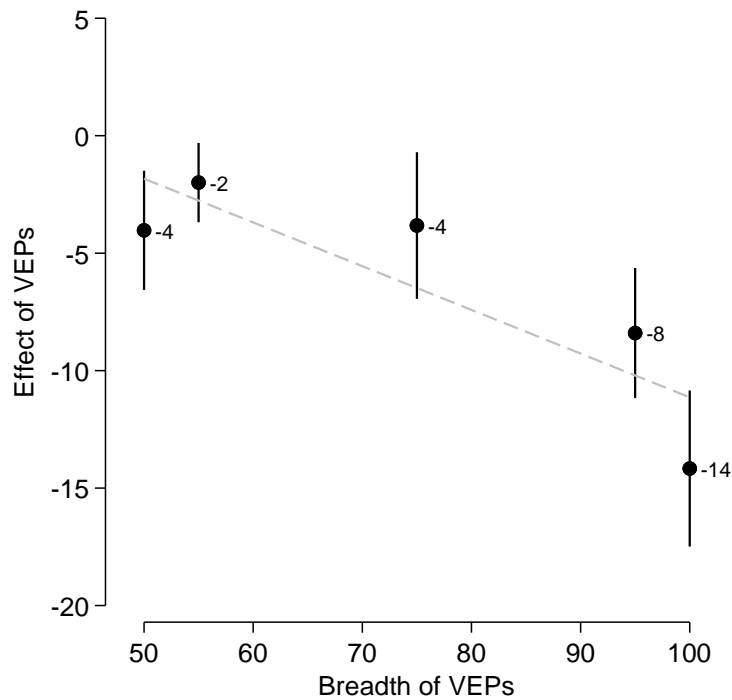
Figure 35: Effects of VEPs on the Second Sample of Government Officials, After Dropping the Third Measurement

Effects of VEPs on the second sample of government officials, using only the first issue and dropping the third measurement: Figure 36 presents the estimated effects of VEPs when we restricted the analysis to the first issue and eliminated the third measurement.
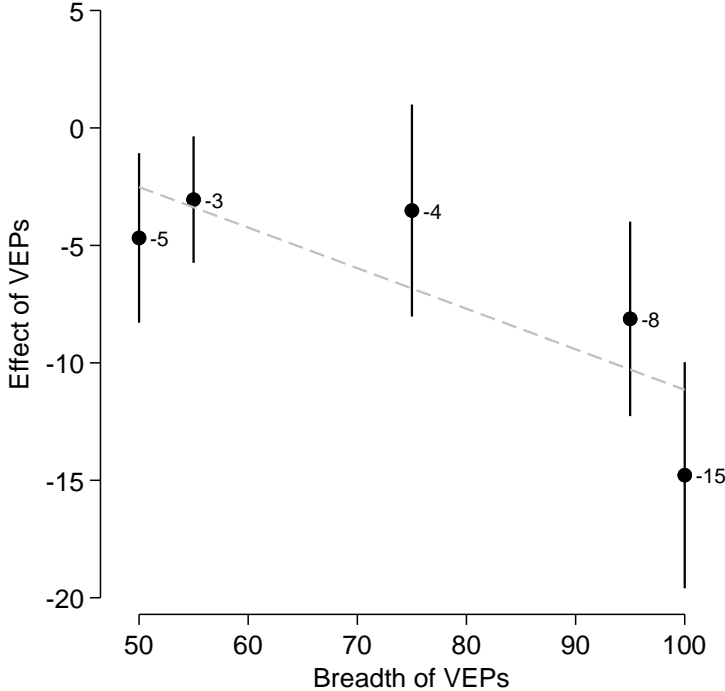
Figure 36: Effects of VEPs on the Second Sample of Government Officials, Using Only the First Issue and Dropping the Third Measurement

<u>Effects of VEPs on the first sample of government officials, by order in which scenarios were presented</u>: Half of the respondents were randomly assigned to react to broad VEPs before reacting to narrow VEPs. The other half were given the opposite ordering. Figure 37 presents the effects of VEPs on these two groups. The figure shows that sequencing sometimes made a difference. In the article we average over these differences, thereby counterbalancing any variation introduced by the order in which the scenarios were presented.
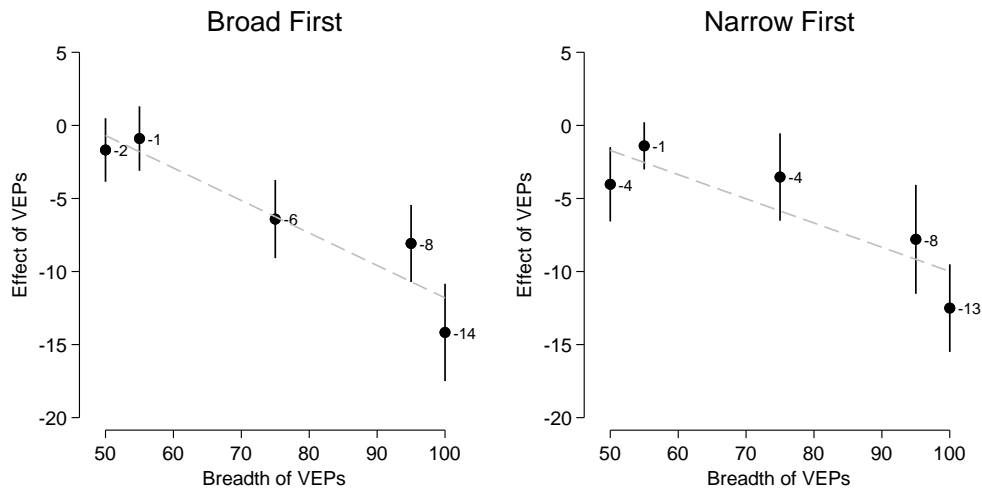
Figure 37: Effects of VEPs on the Second Sample of Government Officials, By Order in Which Scenarios Were Presented

Effects of VEPs on the second sample of government officials, by issue: Figure 38, below, presents the effects of VEPs on each issue.
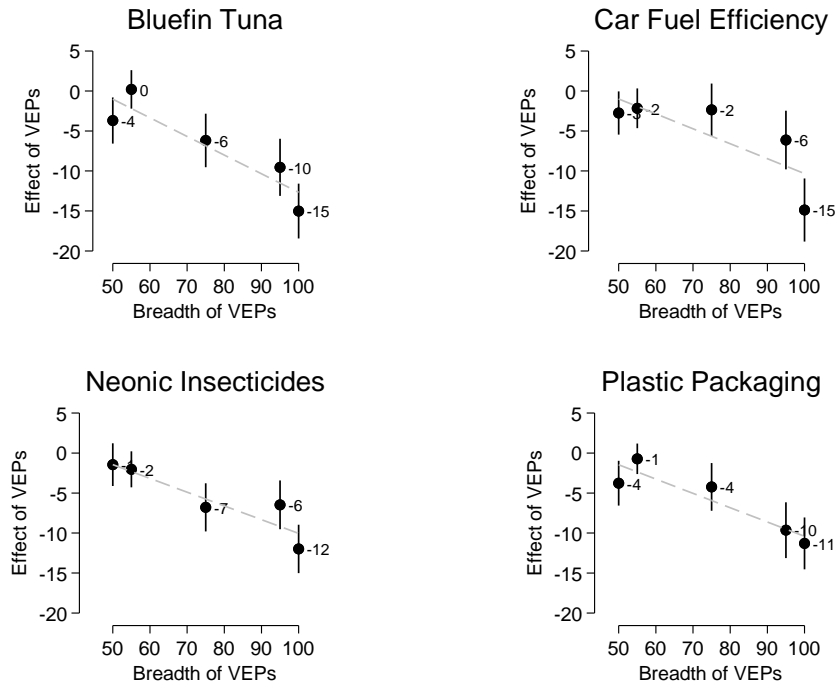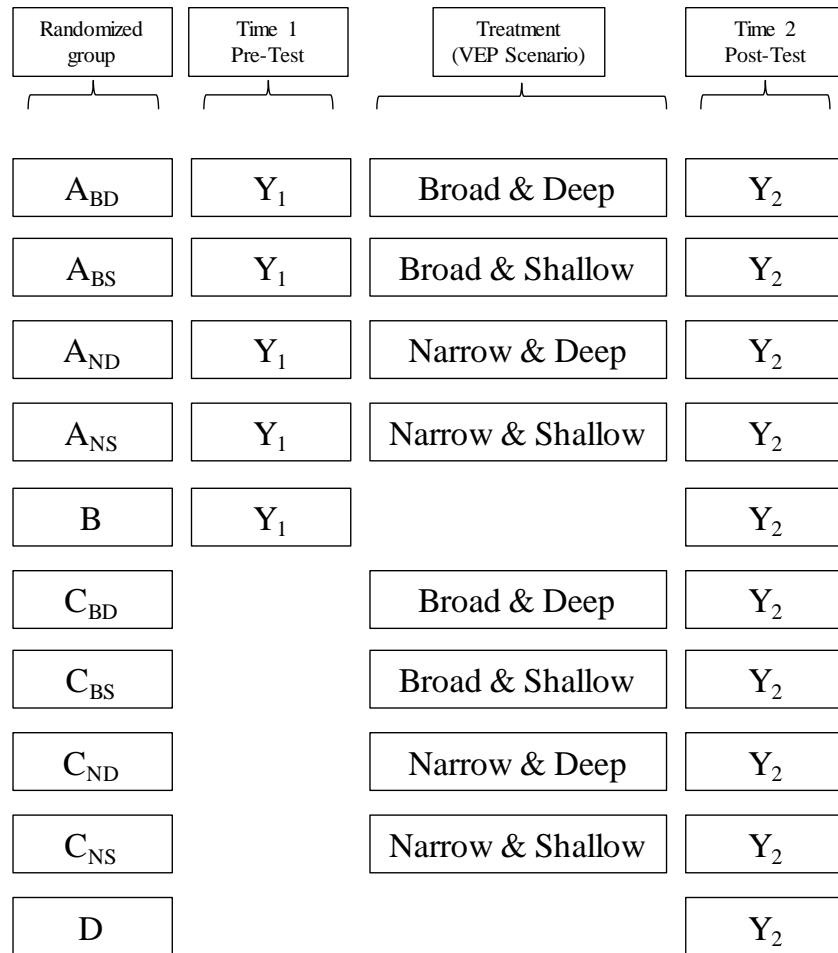

Figure 38: Effects of VEPs on the Second Sample of Government Officials, By Issue

The experiments in the paper employed a repeated-measures design: we measured each respondent's baseline support for regulation, presented two scenarios involving VEPs, and re-measured the respondent's support for regulation in light of those two VEPs. One might wonder whether the use of a pretest affected the way people responded to treatment, a problem known as "instrument reactivity."

To test for instrument reactivity, we ran two additional experiments, each with a Solomon-type design. The standard Solomon experiment requires only four groups (de Vaus 2001, 63-65), but we needed ten to accommodate the full array of VEPs we were studying.

In the new experiments, each participant was randomly assigned to one of ten experimental groups listed on the left side of the Figure 39. Members of groups A_{BD}, A_{BS}, A_{ND}, and A_{NS} followed a repeated-measures protocol: we first measured their baseline support for regulation, then described one of four types of VEPs, and finally measured their support for regulation given the VEPs. In contrast, members of groups C_{BD}, C_{BS}, C_{ND}, and C_{NS} did not receive a pretest question. They simply read about one type of VEP and registered their opinion about regulations given the VEPs we had presented. We also included two control groups, B and D, which did not receive any information about VEPs. Members of group B provided their baseline preferences twice (pretest and posttest), whereas members of group D provided their baseline preferences once (posttest only).

Randomized group	Time 1 Pre-Test	Treatment (VEP Scenario)	Time 2 Post-Test
A_{BD}	Y_1	Broad & Deep	Y_2
A_{BS}	Y_1	Broad & Shallow	Y_2
A_{ND}	Y_1	Narrow & Deep	Y_2
A_{NS}	Y_1	Narrow & Shallow	Y_2
B	Y_1		Y_2
C_{BD}		Broad & Deep	Y_2
C_{BS}		Broad & Shallow	Y_2
C_{ND}		Narrow & Deep	Y_2
C_{NS}		Narrow & Shallow	Y_2
D			Y_2

Figure 39: Solomon Ten-Group Design

Although the cost of applying the design to all six issues would have been prohibitive, we applied this Solomon design to two issues: neonic insecticides and Bluefin tuna. We selected neonics and tuna because they varied in their mode of presentation. Voluntary action regarding neonics was portrayed qualitatively, whereas voluntary action regarding tuna was described quantitatively. By studying both issues, we could not only test for instrument reactivity, but also see if reactivity varied across these two modes of presentation.

Both of these Solomon experiments took place in March 2018. We recruited a diverse sample of U.S. adults via Amazon Mechanical Turk, an online service that is widely used for academic research.[4] The final sample sizes were 2,016 for neonics and 2,012 for tuna.

---

[4] Numerous studies have found that experiments conducted via MTurk produce experimental treatment effects similar to what one would obtain from nationally representative samples (Mullinix et al. 2015; Berinsky et al. 2012; Buhrmester et al. 2011).

Fortunately, we found little evidence of instrument reactivity, and the estimated effects of VEPs remained the approximately same, regardless of whether there were repeated measures or not. We now present these findings in detail.

To analyze the neonics experiment, we estimated the following OLS regression model:

$$Y_{2i} = \beta_0 + \beta_1 BD_i + \beta_2 BS_i + \beta_3 ND_i + \beta_4 NS_i + \beta_5 P_i +$$

$$\beta_6(BD_i \times P_i) + \beta_7(BS_i \times P_i) + \beta_8(ND_i \times P_i) + \beta_9(NS_i \times P_i) + \varepsilon_i$$

where i indexes the respondent; Y_{2i} represents the respondent's Time 2 posttest measure of support for banning neonics on a scale from 0 to 100; BD_i, BS_i, ND_i, and NS_i are dummy variables representing the four types of VEPs (broad and deep, broad and shallow, narrow and deep, and narrow and shallow); P_i is a dummy variable indicating whether the respondent answered a Time 1 pretest item; the interactions indicate whether the VEPs were preceded by a pretest; and ε_i is a normally distributed stochastic error.

Figure 40 presents the regression coefficients and associated confidence intervals. The estimated intercept (β_0) was 59, meaning that average support for banning neonics was 59 out of 100 when respondents did not receive a VEP or answer a pretest. The next four coefficients (β_1, β_2, β_3, β_4) show how much VEPs reduced support for regulations among respondents who did not receive a pretest. As in our previous studies about the mass public, VEPs substantially reduced support for regulations relative to the baseline in which no VEPs were mentioned; broad VEPs were more consequential than narrow VEPs; and the public did not distinguish between deep and shallow VEPs.

The remaining coefficients speak to the question of instrument reactivity. The coefficient on "had pretest" (β_5) was small—only 4 points on a 100-point scale—and statistically indistinguishable from zero, implying that the postest scores of group B (which took a pretest) were not different from the posttest scores of group D (which did not take a pretest). Moreover, the coefficients on the interaction terms were all close to zero and statistically insignificant, implying that the effects of VEPs on support for regulation were nearly the same, regardless of whether respondents answered a pretest or not. Overall, we found very little evidence of instrument reactivity.
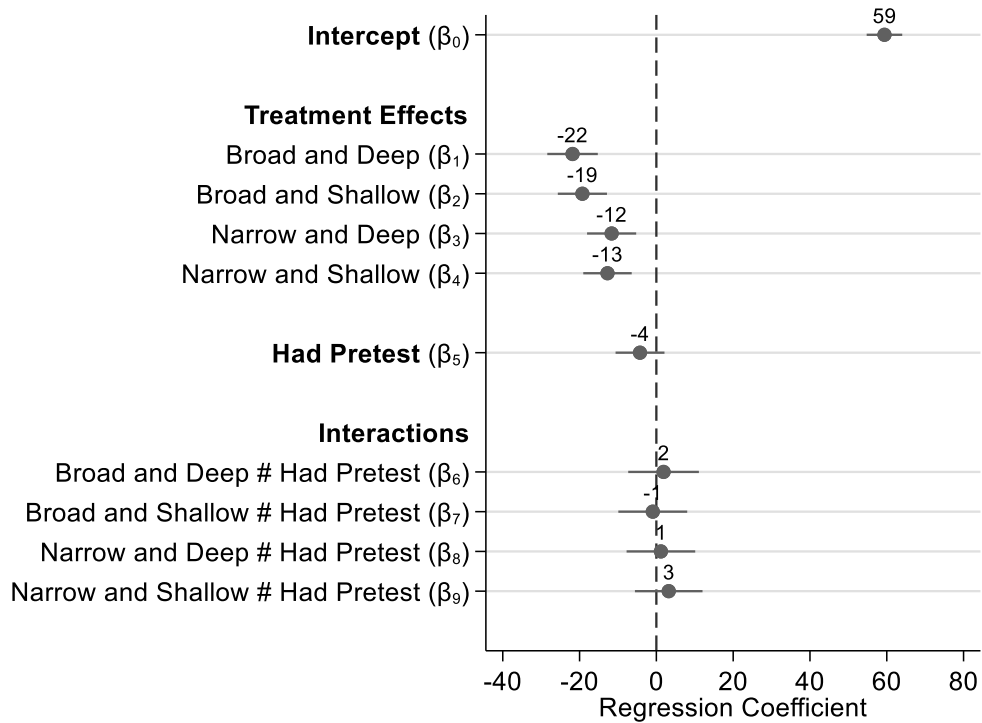
Figure 40: Tests for Instrument Reactivity (Neonics)

Note: The figure gives estimated coefficients and confidence intervals for a linear regression in which the dependent variable was posttest support for banning neonics, measured on a scale from 0 to 100. The sample size was 2,016. The Bonferroni method was used to adjust confidence intervals for multiple testing.
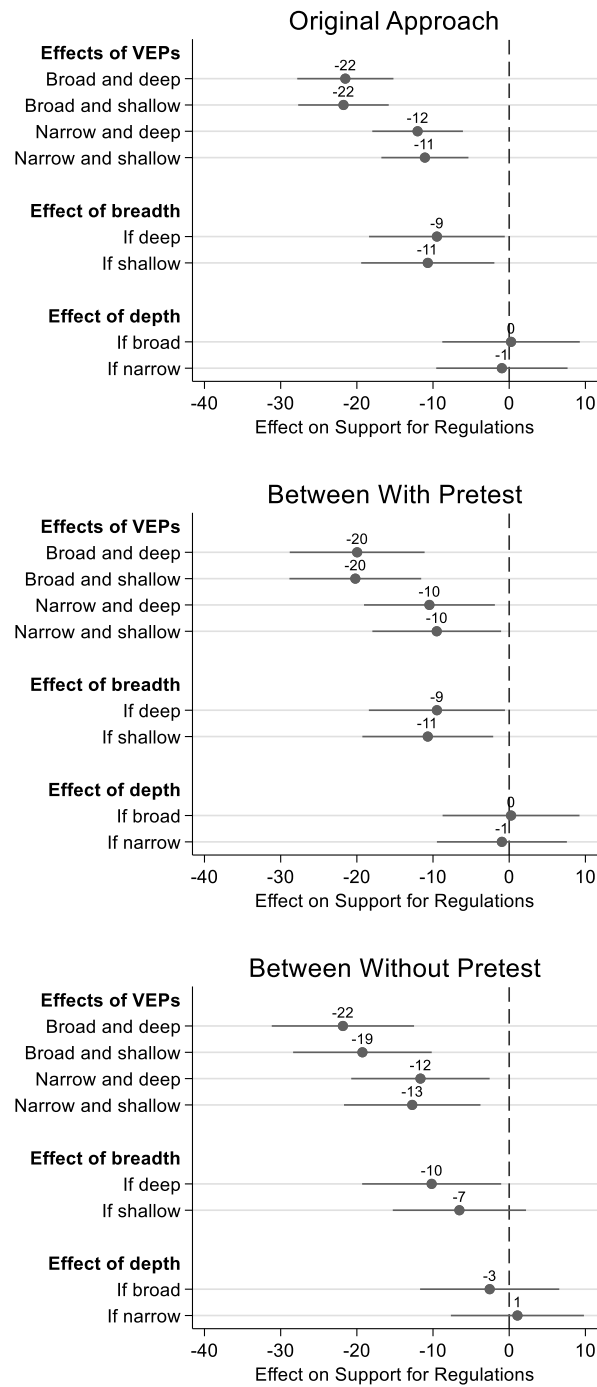
To further verify that the presence of a pretest did not affect our conclusions, we compared three approaches to estimating the effect of VEPs on support for banning neonics.

The first approach took advantage of the pretest data. This approach, employed in the paper, used the pretest measures from groups A_{BD}, A_{BS}, A_{ND}, and A_{NS} to establish baseline support for regulation in the absence of VEPs, and used the posttest measures from those same groups to estimate support for regulation in the presence of VEPs. The top portion of Figure 41 presents this "original approach."

The second approach compared the posttest data of respondents who had completed a pretest. We focused on respondents who took a pretest (A_{BD}, A_{BS}, A_{ND}, A_{NS}, and B), but discarded their pretest measures and estimated the effects of VEPs by conducting a between-subjects comparison of their posttest values. The middle portion of Figure 41 presents this "between with pretest" approach.

The third approach compared the posttest data of respondents who did not receive a pretest. We focused on respondents who did not take a pretest (C_{BD}, C_{BS}, C_{ND}, C_{NS}, and D), and estimated the effects of VEPs by conducting a between-subjects comparison of their posttest values. The bottom portion of Figure 41 presents this "between without pretest" approach.

As Figure 41 shows, all three approaches gave similar results. Thus our conclusions about the effects of VEPs do not seem sensitive to the fact that our original experimental design included a pretest.

Original Approach

Effects of VEPs

Broad and deep −22
Broad and shallow −22
Narrow and deep −12
Narrow and shallow −11

Effect of breadth

If deep −9
If shallow −11

Effect of depth

If broad 0
If narrow −1

Effect on Support for Regulations

Between With Pretest

Effects of VEPs

Broad and deep −20
Broad and shallow −20
Narrow and deep −10
Narrow and shallow −10

Effect of breadth

If deep −9
If shallow −11

Effect of depth

If broad 0
If narrow −1

Effect on Support for Regulations

Between Without Pretest

Effects of VEPs

Broad and deep −22
Broad and shallow −19
Narrow and deep −12
Narrow and shallow −13

Effect of breadth

If deep −10
If shallow −7

Effect of depth

If broad −3
If narrow 1

Effect on Support for Regulations

Figure 41: Three Approaches to Estimating the Effects of VEPs (Neonics)

Note: "Original approach" involved comparing pretest measures to posttest measures. "Between with pretest" involved a between-subject analysis of the posttest measures of respondents who took a pretest. "Between without pretest" involved a between-subject analysis of the posttest measures of respondents who did not take a pretest.

55

To check the robustness of these conclusions, we conducted a second Solomon experiment involving a different issue, Bluefin tuna. Figure 42 presents the estimated coefficients and confidence intervals from a linear regression in which the dependent variable was posttest support for banning the sale of Bluefin tuna. The figure reveals several familiar patterns: VEPs reduced support for regulations relative to the baseline in which no VEPs were mentioned; broad VEPs were more consequential than narrow VEPs; and the public did not distinguish between deep and shallow VEPs.

Figure 42 also shows little evidence of instrument reactivity. The coefficient on "had pretest" was only 4 points on a 100-point scale and statistically indistinguishable from zero, implying that the postest scores of group B (which took a pretest) were not different from the posttest scores of group D (which did not take a pretest). Moreover, the coefficients on the interaction terms were all close to zero and statistically insignificant, implying that the effects of VEPs on support for regulation were nearly the same, regardless of whether respondents answered a pretest or not.
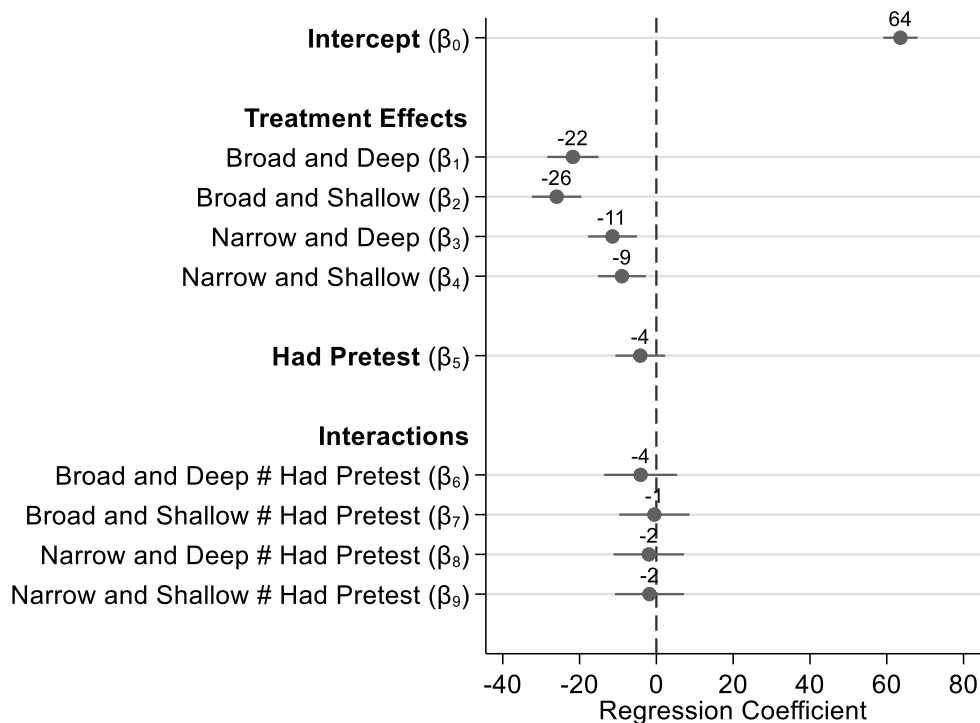

Figure 42: Tests for Instrument Reactivity (Tuna)

Note: The plot gives estimated coefficients and confidence intervals for a linear regression in which the dependent variable was posttest support for banning the sale of Bluefin tuna, measured on a scale from 0 to 100. The sample size was 2,012. The Bonferroni method was used to adjust confidence intervals for multiple testing.

Finally, we compared three approaches to estimating the effects of VEPs on support for banning the sale of Bluefin tuna. The "original approach" involved comparing pretest measures to posttest measures; "between with pretest" involved a between-subject analysis of the posttest measures of respondents who took a pretest; and "between without pretest" involved a between-subject analysis of the posttest measures of respondents who did not take a pretest. As Figure 43 shows, all three methods gave similar results, reinforcing the conclusion that the estimated effects of VEPs did not depend on the fact that our original experiments included pretests.
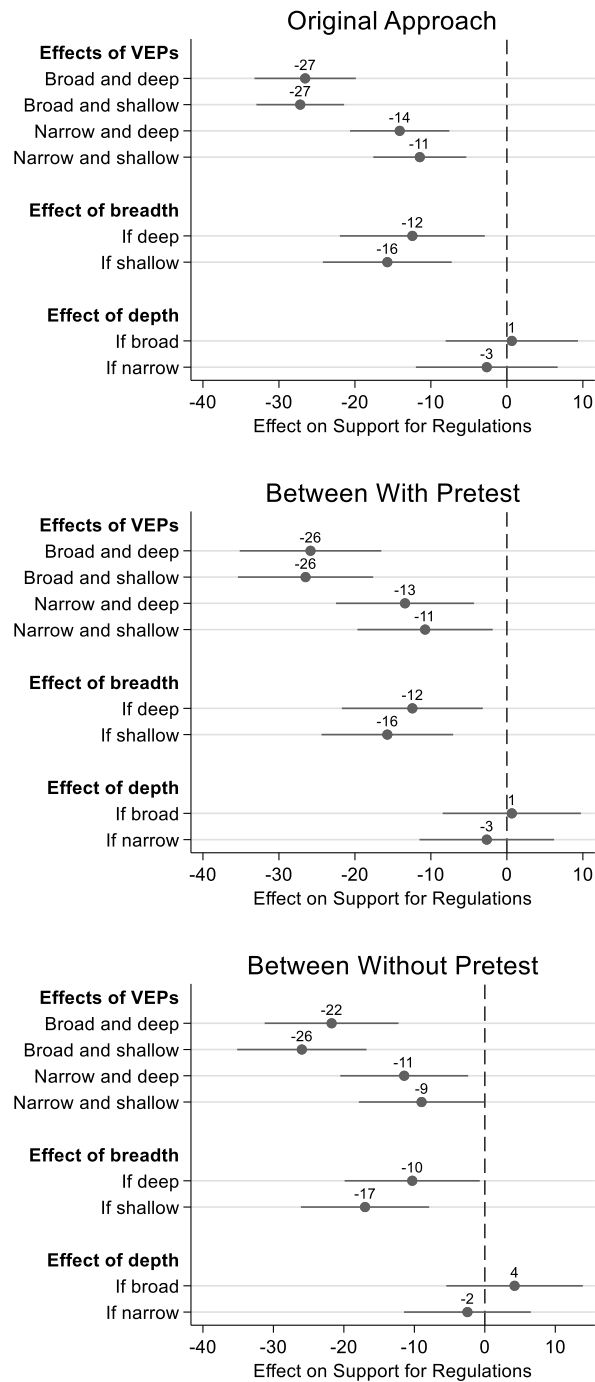
Figure 43: Three Approaches to Estimating the Effects of VEPs (Tuna)

Note: "Original approach" involved comparing pretest measures to posttest measures. "Between with pretest" involved a between-subject analysis of the posttest measures of respondents who took a pretest. "Between without pretest" involved a between-subject analysis of the posttest measures of respondents who did not take a pretest.

References

Berinsky, Adam J., Gregory A. Huber, and Gabriel Lenz. 2012. "Evaluating Online Labor Markets for Experimental Research: Amazon.com's Mechanical Turk." *Political Analysis* 20(3):351–68.

Buhrmester, Michael, Tracy Kwang, and Samuel D. Gosling. 2011. "Amazon's Mechanical Turk: A New Source of Inexpensive, Yet High-Quality, Data?" *Perspectives on Psychological Science* 6(1):3–5.

de Vaus, David A. 2001. *Research Design in Social Research*. London: Sage.

Mullinix, Kevin J., Thomas J. Leeper, James N. Druckman, and Jeremy Freese. 2015. "The Generalizability of Survey Experiments." *Journal of Experimental Political Science* 2:109–38.