Causal analysis of public support

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October 15, 2024

PROPOSAL

"And I've noticed, by the way, that since it's become popular
they don't call it Obamacare no more."

Barack Obama, Chicago, August 20, 2024

Often, policies that experts would consider socially desirable take time to become widespread because of their (ex-ante) unpopularity. Examples from environmental economics include Pigouvian taxes aiming at curbing carbon emissions, congestion, or household garbage (Carattini, Carvalho, and Fankhauser 2018). Well-known examples from other domains include smoking bans, seat belt laws, and the expansion of healthcare coverage. These three policies tended to be unpopular among an important portion of the population, but are much less unpopular these days, as documented by various opinion polls. Anecdotal evidence for congestion charges (Schuitema, Steg, and Forward 2010; Andersson and Nässén 2016) and carbon pricing (Murray and Rivers 2015) also shows increases in public support over time after those policies were implemented.

While opinion polls can show general trends in a policy's popularity among the population and some subsets thereof, there is a dearth of knowledge showing causally when and how voters revise their beliefs about a given policy after experiencing it. Do voters underestimate, ex ante, the ability of congestion charges to reduce traffic and local air pollution and revise their beliefs after seeing the policy in effect? Can voters exposed to carbon pricing observe a reduction in carbon emissions compared to the counterfactual? Do voters believe ex ante that carbon pricing would lead to major troubles for their economy, which implies belief revision when such a very pessimistic scenario is not realized? Did voters who revised their beliefs about the Affordable Care Act benefitted from its healthcare provision directly? Or people around them did? Would voters in and around New York City have perceived its congestion charge more positively had the policy been implemented on June 30, 2024, rather than being paused indefinitely? How many of them would have revised their beliefs and become supportive of the policy by Election Day?

To address all these questions, we need a new stream of research. This paper aims to complement a recent piece by Carattini, Dur, and List (2024) that introduces the concept of "causal analysis of public support" and shares a call to arms for more research combining policy evaluation and the causal analysis of public support.

The causal analysis of public support consists in measuring beliefs over and support for a policy both before and after its implementation in a treatment group, in comparison with a proper control group. By combining policy evaluation and the causal analysis of public support, researchers would provide evidence on a policy's effects and how voters learn about it. Policymakers are likely to care about both items. Leveraging the experience of frontrunning jurisdictions, a body of knowledge would emerge showing when, how, and why voters may support some policies ex post more than they do ex ante, potentially decreasing policymakers' reticence to implement ex-ante unpopular yet ex-post popular socially desirable policies.

To date only one paper combines policy evaluation and the causal analysis of public support (Carattini, Baranzini, and Lalive 2018). It does so by examining the adoption of pricing garbage by the bag in Swiss municipalities following a decision by the federal supreme court of Switzerland and in presence of an already-treated control group. Before experiencing the policy, people in the treatment group were much more hostile towards the policy than those in the already-treated control group. After experiencing the policy, people in the treatment group revised their beliefs about the policy's effectiveness and fairness and public support

increased. No tangible change in public support was observed in the control group, which already knew the policy. Findings in Carattini, Baranzini, and Lalive (2018) are consistent with evidence from the lab, where participants would be more likely to support Pigouvian instruments after a trial period (Cherry, Kallbekken, and Kroll 2014; Dal Bó, Dal Bó, and Eyster 2018; Janusch et al. 2021).

Because of space constraints, Carattini, Dur, and List (2024) only discuss a limited set of methodological aspects in this new research area. With the present paper we aim to fill this gap by providing a set of methodological best practices for the causal analysis of public support. These best practices are also informed by lessons learned during our experience collaborating with the Abdul Latif Jameel Poverty Action Lab in guiding researchers in including the causal analysis of public support in their experimental design. Including the causal analysis of public support in experimental designs is aligned with "option C thinking," the idea that researchers should think about both how policies may work in a given setting but also how they can be scaled up (List 2024). It is reasonable to expect scaling up of many programs to require some degree of political buy-in, which may depend on how such programs are perceived by the general public, ex ante as well as ex post.

This paper aims to cover the following aspects. First, it provides guidelines on how to measure belief revision causally under ideal conditions, where the researcher is implementing a field experiment and can run a baseline survey to measure prior beliefs, to be followed by at least one follow-up survey (and more to measure medium- to long-run effects). In this context, the main challenge that the researcher faces is experimenter demand effects. Individuals interviewed twice about the same intervention, even with some time in between, may feel compelled to warm up towards the policy. This effect may be stronger in the treatment group. In the paper, we discuss how to tackle experimenter demand effects at the design stage. A field experiment provides the cleanest context for the causal analysis of public support, as it usually does for policy evaluation.

There are numerous examples of randomized interventions for which it might have been particularly interesting to examine how public support would have evolved with experience. For instance, in Duflo, Hanna, and Ryan (2012), teachers' attendance was monitored through cameras and their salaries adjusted for attendance. In the Minneapolis domestic violence experiment, police officers were encouraged to arrest by default the alleged perpetrator of domestic violence (Sherman and Berk 1984; Angrist 2006). In Finkelstein et al. (2012), enrollment in the Medicaid expansion occurred at random through a lottery. The paper provides a additional guidelines, besides the need to tackle experimenter demand effects, for researchers involved in field experiments interested in examining causally how perceptions about an intervention vary with exposure.

Second, we extend the discussion to the case when the policy is implemented by a government agency and the researcher, while not controlling the policy's timing, has the ability to collect baseline data on public support. In this case, the main challenge for the researcher consists in identifying a suitable control group that allows for credible causal inference in a quasi-experimental setting. The paper discusses how to orient this choice. Data on public support for a given policy going back in time may exist only for some policies and only for some geographical areas. Their most likely source would be opinion polls, i.e. cross-sectional snapshots of public opinion. Inferring from various opinion polls may be challenging, given the different wording that questions may have across surveys and the potential compositional effects, even as most surveys aim to be representative of the same underlying population. The paper discusses how to use opinion polls, when available, to guide the choice of control units, and how to use alternative data when opinion polls are not available or too scarce to be useful. It also discusses how control groups may be "contaminated" if indirectly exposed to the treatment group's experience via media coverage or word of mouth. For instance, a

voter not in expansion states might have "experienced" the benefits of Medicaid expansion by reading about it in the news or by having a friend or relative benefitting directly. Already-treated control groups can help, but dynamic treatment effects may complicate matters.

Third, we consider the case in which the policy was already implemented and the researcher aims to "travel back in time" to recover baseline data. This context is especially challenging. Still, we discuss how a researcher could use opinion polls not only to guide the choice of control units, but also as a replacement to the baseline survey or to obtain a time series, which may be necessary in presence of a single treated cluster.

Fourth, we focus on mechanisms, which is a key component of the causal analysis of public support. Whether public support increases or decreases tends to depend on prior beliefs and how they are affected when experiencing the policy. The abovementioned examples refer to situations where voters were overly pessimistic about a given policy's prospects. With experience, public support improved.

But public support may also decrease over time, which is equally important to document. Drawing from our own research, an interesting setting is Washington state, where carbon pricing was introduced with a revenue-positive policy design. In 2023, following the defeat of two carbon tax proposals at the polls in 2016 and 2018, Washington state introduced through the legislature a policy named "cap and invest," a cap-and-trade system whose revenues are used mostly for environmental projects and to support disadvantaged communities. A fraction is rebated through utilities to low-income households. In November 2024, voters in Washington state will be voting on Initiative 2117, which aims to repeal cap and invest. With only about one year from its implementation, voters are unlikely to have experienced any tangible benefits from cap and invest. Direct benefits from carbon pricing are not especially visible and indirect effects take time to materialize, as the projects funded by cap and invest's auction revenues need time to deliver. But voters have experienced an immediate increase in gas prices, with Washington state currently having the highest gas prices in the country along with California. Media outlets in Washington state tend to agree, regardless of their leaning, that such price increases are in the order of at least 25 cents per gallon. When the policy was announced, Governor Inslee mentioned "pennies" when referring to expected price increases. Hence, it is plausible that in this case priors about the cost of the policy were overly optimistic.

When it comes to mechanisms, narratives and mental models matter too. In the case of Washington state, for instance, the camp opposing Initiative 2117, and thus supporting cap and invest, blames price gouging on the gas market for the unexpected increase in gas prices. California, another state pricing carbon, just passed legislation that gives the state the power to oversee gas companies' storage decisions, with the stated goal of avoiding shortages and preventing price spikes in the market for gas. In Canada, conservatives blame the federal carbon tax for much of the inflation that has affected the country over the last few years. Thus, also building on our own research, the paper discusses ways to combine mental models and the causal analysis of public support.

Going back to the situation where public support increases over time, belief revision may only be one driver. Some voters may simply be averse to change or uncertainty. Even in presence of generally correct beliefs, uncertainty around expected effects, versus certainty about the current conditions, may favor the status quo. As the policy is implemented, the status quo changes. The paper discusses how to disentangle various potential mechanisms driving changes in public support and attribute them the relevant weight.

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