Exhibit A

UNITED STATES DISTRICT COURT
FOR THE DISTRICT OF MAINE

DINNER TABLE ACTION, *et al.*, Plaintiffs, v. SCHNEIDER, *et al.*, Defendants.	Docket No. 1:24-cv-00430-KFW

DECLARATION OF CHRISTOPHER ROBERTSON

I, Christopher Robertson, hereby declare as follows:

1. I am a Professor at Boston University School of Law and was retained in my individual capacity as an expert for Intervenor-Defendants in this case.

2. Attached as Exhibit A-1 is a copy of my surreply report responding to the Declaration of David Primo.

I declare under penalty of perjury that the foregoing is true and correct to the best of my knowledge, information, and belief. Executed on this 13th day of May 2025 in Cambridge, Massachusetts.

/s/ Christopher Robertson
Christopher Robertson

Exhibit A-1

Expert Surreply Report
Christopher T. Robertson, JD, PhD

I have reviewed the relevant part of Plaintiffs' Reply in Support of a Permanent Injunction (ECF #61) and the Declaration of David Primo (ECF # 62-4). I offer the following observations.

Summary

My randomized experiments, with national and Maine samples, showed that higher campaign contributions lead to greater appearances of *quid pro quo* (QPQ) corruption and showed that Maine's $5,000 cap caused a substantial decrease in appearances of corruption. Professor Primo's critiques around particular amounts tested in Experiment 1 are immaterial, and his "external validity" critique of Experiment 2 lacks foundation, as it contradicts the known realities that he acknowledges in other writings.

In contrast, Professor Primo's own work relies on general surveys before and after other reforms were implemented in other states at other times, some nearly 40 years ago. Professor Primo's research suffers from omitted variables, self-selection, and limited sample sizes, all of which prevent a credible causal estimate. Professor Primo's research also uses a crude measure of "trust in government" rather than the precise variable at issue here: appearances of corruption. In short, Professor Primo studies other interventions, fails to isolate their causal effects, and measures the wrong outcomes.

My Expertise

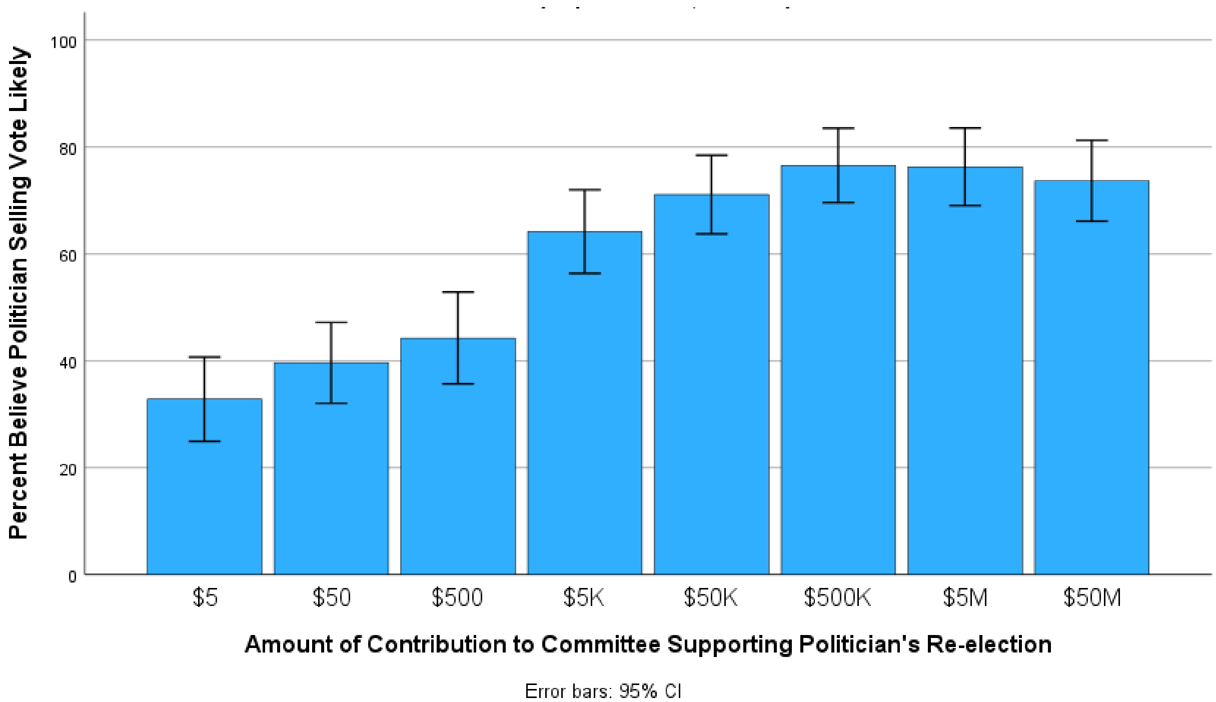
I have developed a coherent theory that explains how money can be corrupting, creating problems in agency relationships in a wide range of domains, including healthcare (bioethics) and politics (see e.g., Robertson 2010; Kesselheim et al., 2012; Robertson, et al., 2012; Spece et al., 2014; Robertson et. al. 2016; Rose et al., 2021). Conflicts of interests, or misaligned incentives, are a fundamental problem that social sciences study as such.

To be sure, Professor Primo also works in a wide range of topics as well. These include analysis of the national budgeting process (Primo 2007) and the causes of airplane crashes (Cobb & Primo 2007).

My Experiment 1 Shows the Clear Correlation Between Levels of Contributions and Appearances of Corruption.

Experiment 1 is a dose-response study, showing that increasing the monetary size of donations generally increases appearances of corruption (p<.001). No statistical model is necessary to see the upward sloping trend, obvious from Figure 1 of my report (reproduced below).

Figure 1. Percent of Respondents Viewing Sale of Policy Outcomes to be Likely by Level of Contribution to Committee (Experiment 1, n=1144, 95% confidence intervals shown).



Professor Primo writes that "restricting campaign contributions will not do much if Americans believe even small contributions are corrupting" (D.E. 62-4, pp. 8-9). This is an interesting hypothesis, subject to empirical testing. In fact, our data allows us to reject that hypothesis. Notwithstanding the minority of people who see even $5 as corrupting, there are many <u>other</u> people who do <u>not</u> see $5 corrupting but who <u>do</u> see payments above $5,000 to be corrupting. That's why we find different appearances of corruption between those levels in Figure 1, above.

Professor Primo also complains that we did not test any levels between $500 and $5,000 (D.E. 62-4, p.14). Prior research (DeBell and Iyengar 2021, 294) shows a lack of variation at these levels. Even if there were variation, it would be uninteresting since *all* those contributions would be *permissible* under the Maine cap at $5,000.

Professor Primo shifts focus to levels above the $5,000 threshold, writing that "the Robertson poll showed no increase in perceptions of corruption between $5,000 contribution and a $50 million contribution – evidencing no benefit to the $5,000 cap at all" (D.E. 62-4, p. 19). To the contrary, I have confirmed there is a positive correlation between amounts and perceived corruption at and above this $5,000 point (p=.004).

Suppose that under Maine's cap, every donor who would have made a larger contribution instead makes a $5,000 contribution. Making a binary comparison between the $5,000 level and the higher levels together, here again we see a significant difference (p=.035).

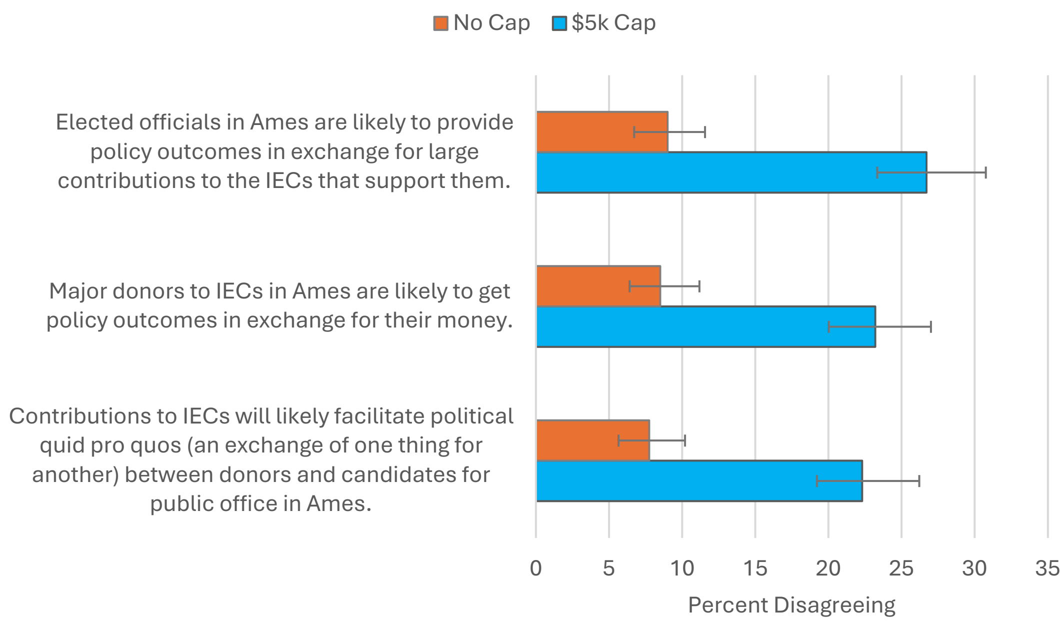
These alternative statistical approaches also address Professor Primo's concern for non-linearity (D.E. 62-4, p.16). The findings are robust.

Accordingly, Experiment 1 supports the proposition that in a world with no SuperPAC contributions above $5,000 we would have lower perceptions of QPQ corruption. This finding is consistent with the prior literature (see e.g., Bowler & Donovan 2016).

On My Experiment 2, Professor Primo's External Validity Critique Lacks Foundation.

In Experiment 2, I manipulate the policy being tested (a $5,000 cap) and measure the attitudes in question (perceptions of QPQ corruption), as shown in Figure 2 reproduced below. This pinpointing of psychological effects is exactly what randomized experiments do well (Thye 2007; Zelditch 1969). In other work, Professor Primo also uses survey experimentation as a valid social science methodology (see e.g., Primo & Milyo 2020, p.62).

Figure 2. Percent of Respondents Who Disagree with Statement by Experimental Condition (Experiment 2, n=1144, with 95% confidence intervals shown)



In my Experiment 2, respondents were asked to respond to a situation where, “contributions to these IECs may come from major donors who want official actions from elected officials, such as having the state government spend money to support a particular industry or de-regulate a particular industry” (D.E. 62-4, p.17, quoting my experimental vignette). Here, Professor Primo argues that this sentence lacks “external validity,” which we agree would be a problem if respondents were asked about a scenario that “does not apply to real-world situations” (D.E. 62-4, p.7). Yet, this vignette statement does apply to real-world situations; interested parties can and do donate to IECs (also known as SuperPACs) (see e.g., Franzen & Giorno 2024).

Professor Primo proposed (but does not test) an alternative framing where donations come from “public-spirited donors”, rather that interest-groups (D.E. 62-4, p.17). Professor Primo’s proposed framing is the utterly unrealistic one. He has chastened scholars that “often ignore the strategic and self-interested components of politics” and instead he demands that we acknowledge that “[p]olitical actors are rational, self-interested, and strategic ... not necessarily benevolent” (Primo 2007 at p.15).

For social scientists, self-interested conduct is the norm. The question here is not whether that problem is real; the question is whether Maine’s reform solves it.

Professor Primo Is Unable to Rule Out Important-Sized Effects Due to Limited Statistical Power.

I now turn to Professor Primo's own research submission in this litigation. Professor Primo's report says that his past research "find(s) no <u>substantively meaningful</u> effects of campaign finance laws" (D.E. 62-4, p.9, emphasis added). Professor Primo's statement must be parsed carefully, as a concession that his statistics cannot rule out all positive effects. It is up to the factfinder to determine whether smaller effects, consistent with his data, are substantively meaningful. On this point, Professor Primo offers only a naked opinion that they are not.

To affirm the null hypothesis, showing that there is <u>no</u> effect of campaign finance reforms, Professor Primo would need a very precise causal estimate, which requires a very large sample of independent observations (Harms & Lakens, 2018). Professor Primo's report says that he has analyzed "nearly 60,000 individual-level observations" (D.E. 62-4, at p.8), but his actual statistical power is much more limited, due to clustering at the state level. As Professor Primo explains in prior work, "less information comes from these data than if the individual-level cases were truly independent of one from another in terms of the policy or other condition by dint of where they live" (Primo et al., 2007, p.449), which is the case for my experiments, where the manipulation is randomly assigned to individuals.

This problem explains why Professor Primo cannot rule out important positive effects in the state policy study offered here. He can at best claim to rule out effects larger than the upper bound of his confidence interval, as he does in his underlying book (see Primo & Milyo 2020, p.144-145, referring to the upper bound of "seven one-hundredths of a standard deviation").

That is a very small effect, but at a population level, small effects can be quite meaningful. For example, in a famous study in the top journal, *Nature*, scientists conducted an experiment with 61 million Facebook users and found that those seeing a social message were significantly more likely to vote, though the effect was only eight one-thousandths of a standard deviation (Bond et al., 2012). The voting intervention increased turnout by 340,000 additional votes. Professor Primo cannot rule out such effects or ones ten times as large in this litigation. A factfinder may well conclude that such an effect would be substantial. As others explain, "researchers should not automatically dismiss 'small' effects" because they can be important at population-level scale, cumulate over time, and interact with other important effects (Funder & Ozer 2019, p.156).

Without a Design for Causal Inference, Professor Primo's Research Suffers from Omitted Variables and Self-Selection Bias.

When trying to evaluate the likely causal effects of a particular policy reform, it is important to hold all other factors equal (Greiner 2008; Rubin 2008; Angrist & Pischke 2010). Otherwise, an analyst may affirm a false positive (finding an effect that was really caused by something else) or, in this case, affirm a false negative (failing to see a real effect because it was masked by some other counter-veiling effect).

This last point bears emphasis. Everybody now knows that (without randomization) correlation does not imply causation. But for the same reason, it is also true that a *lack* of correlation also cannot prove a *lack* of causation, when other variables may be counteracting a real effect (*see* Angrist & Pischke 2010, showing several examples of older social science research rejecting real effects). But that's precisely what Professor Primo proposes here.

I utilize randomized experiments to hold all other things equal, as I assign the manipulated policy (via the vignette) to each respondent individually. In this way, even though I cannot measure everything that affects human behavior, I can be sure that the only difference between the groups I am comparing is the manipulated $5,000 policy. This is why Professor Primo has previously acknowledged randomized experiments as "the gold standard for statistical evaluation studies" (Primo et al., 2007 at p.448).

As renown statistician Don Rubin explains, "observational studies, in contrast, are generally fraught with problems that compromise any claim for objectivity of the resulting causal inferences" (2008, p.808). For this reason, "observational studies for causal effects need to be designed to approximate randomized experiments" (id., p.837). The "credibility revolution" in social sciences demands as much (Angrist & Pischke, 2010).

Professor Primo offers neither a randomized experiment nor an observational study that approximates one. He instead only tries to compare states pre- and post- campaign finance reforms using regression models with a few control variables. The R-squared value measures how much of the variance in outcomes is explained by the variables in his model. Professor Primo's reproduced Table 8.4, shows $R^2 = .20$ (D.E. 62-4, p.9), meaning that it does <u>not</u> explain 80% of the variance (100% minus 20%). His model omits many drivers of trust in government (such as attitudes or experiences), and he just asks us to assume that none of them are relevant to the effect he seeks to study.

Accordingly, I have no confidence that Professor Primo has successfully held all other things equal, to provide a clean identification of the actual causal pathway of interest (Greiner 2008). Professor Primo compares people in one state, say Arizona, that enacted a reform, with people in another state, say Michigan, that did not. There are innumerable unmeasured differences between states. He also compares public opinions before and after reforms, but all sorts of things can happen to change perceptions over those times.

The most important difference between states may be whatever drove some but not others to enact reforms. This problem is called endogeneity, and it implicates a related problem of self-selection.

As an analogy, if we were interested in whether a drug treats depression, we would recruit human subjects and randomize them to the get drug or not, so the only differences between groups will be due to the drug. As Professor Primo has explained: "Random assignment prevents problems from arising from selection bias and endogeneity that might occur if individuals were to self-select into groups" (Primo et al., 2007 at p.448).

In contrast, we would not want to rely on a study where the people who volunteer to take the drug have the most severe forms of depression and thus are most desperate to try an experimental treatment. When the self-selected study ends, we might well see that the people on the drug have worse depression than people not on the drug, even if it is true that the drug helped them get somewhat better than they otherwise would have been without the drug. Such a self-selected study would make it impossible to determine whether the drug actually works.

For Professor Primo's research, the jurisdictions that enact reforms differ fundamentally from jurisdictions that do not enact such reforms. In particular, states that enact reforms may have greater, or growing, or more resilient concerns about political corruption. This problem is the most plausible explanation of Professor Primo's rather peculiar finding "that limits on corporate campaign contributions reduce trust in government, albeit by a small amount" (D.E. 62-4, p.9 emphasis added). Professor Primo has failed to cleanly identify the unique contribution of the campaign finance reform in all the noise of other variations, including some that caused the reform itself.

Just as a good social scientist should do, in his book's endnotes, Professor Primo candidly concedes this endogeneity problem (Primo & Milyo 2020, p. 243). But it remains unsolved. It is hard to overstate the scale of this problem. Without a strategy for causal inference, the analyst's regression coefficients offer no meaningful information about the policy questions of interest (Greiner, 2008; Rubin 2008; Angrist & Pischke, 2010).

Professor Primo Does Not Measure Appearances of QPQ Corruption.

Professor Primo characterizes his work as using "real world" data (D.E. 62-4, pp. 8-12). To be sure, both of us rely solely on surveys of public opinion (see *id.*, at p.8), but we ask respondents different questions.

I measure appearances of *quid pro quo* corruption, while Professor Primo measures other constructs, such as "trust in state government" (see D.E. #62-4 at p.9). Trust in government is multidimensional (Lee & Stoker 2000). One might distrust government if one dislikes the outcomes it produces (e.g., slow economic growth) or the particular laws it passes (e.g., a speed limit), or if one objects to structural factors (e.g., gerrymandering), or if one believes that representatives are incompetent or motivated to protect a different social class, as well as sometimes bribed through *quid pro quo* corruption. A policy reform, such as Maine's cap, may have a large effect on one of those factors while having a very small effect on overall trust in government.

As an analogy, physicians widely recommend mammograms and colonoscopies for early detection of cancer in certain age groups, because rigorous systematic reviews show that early treatment prevents cancer deaths (Nicholson, et al., 2024; Davidson, et al., 2021). However, when one looks at population-level statistics comparing those who do and do not get these cancer screens, there is no proven benefit to overall mortality (Nicholson, et al., 2024; Davidson, et al., 2021). There are just too many other factors that affect overall mortality to detect the benefit of using such a crude measure. And that is especially true if you lack a large, randomized experiment. Professor Primo's "trust in government" is a similarly crude measure.

In social sciences, this problem is known as construct validity or measurement error (Cronbach and Meehl 1955). When proper adjustments are made for measurement error, the effect sizes can be quadruple their unadjusted estimates (see e.g., Boyd et al., 2008).

Professor Primo Does Not Directly Evaluate Maine's Reform.

Professor Primo's underlying research covers many different campaign finance reforms across the country, some dating back 40 years. Professor Primo does not provide any research specifically on the effect of a $5,000 cap on SuperPAC contributions in 2025. Nor does Professor Primo provide any data focused on Mainers in particular. My research does all this.

This point bears emphasis. At best, Professor Primo makes inferences from other research on other campaign finance reforms, some decades in the past, in other contexts.

All other things being equal, direct, timely research is to be preferred over inferences from dated research of other interventions in other contexts.

The Causal Effects Shown by My Experiments Remain the Most Probative Evidence.

Ultimately, Professor Primo cautions that, "Simply asking people if they believe a contribution is corrupt or if politics is corrupt does not tell you if laws restricting contributions will improve attitudes toward government" (D.E. 62-4, p.12). I agree completely. One needs a framework for causal inference focused on the policy change in particular and a measurement of the precise outcome variable.

That is why my research asks about a wide range of contribution levels to determine whether there was a dose-response relationship. That is why my research tests specifically a $5,000 cap, in a randomized experiment that would allow measurement of the effects of the cap against a counterfactual, where there was no cap. My research alone offers a credible causal estimate of the effects of Maine's cap.

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