

Campaign Contributions, Independent Expenditures, and the Appearance of Corruption: Public Opinion vs. the Supreme Court's Assumptions

Matthew DeBell and Shanto Iyengar

ABSTRACT

We use survey experiments to test the validity of judicial assumptions underlying campaign finance regulation. Our evidence supports the key assumption that “appearance of corruption” is directly related to the monetary value of campaign contributions. Contrary to the Court’s reasoning in *Buckley v. Valeo* and *Citizens United v. FEC*, independent expenditures are more likely to elicit the appearance of corruption than direct contributions, and direct contributions well below the legal limit also create the appearance of corruption. Our findings therefore call into question key legal tenets underlying campaign finance regulation and suggest that the amounts raised by virtually every federal election campaign exceed the threshold required to elicit widespread public perceptions of corruption.

Keywords: campaign finance; corruption; Citizens United

INTRODUCTION

DEMOCRACY LIVES OR DIES BY THE LEGITIMACY of its institutions. Large sums of money donated to politicians or spent on their campaigns can create an appearance of corruption that undermines political legitimacy. This premise is the basis for laws challenged in the U.S. Supreme Court’s two most important campaign finance decisions, *Buckley v. Valeo* (1976; hereafter *Buckley*) and *Citizens United v. Federal Election Commission* (2010; hereafter *Citizens*). The empirical predicate of the *Buckley* decision is that large direct contributions to political candidates create an appearance

of corruption. The empirical predicate of the *Citizens* decision is that unlike direct contributions, independent campaign expenditures by groups such as corporations and unions do not create any appearance of corruption.

The *Buckley* and *Citizens* decisions have had far-ranging consequences for American campaigns and elections, and the Court’s assumptions about the effects of campaign finance and corruption on democracy have been closely analyzed and empirically challenged (e.g., Primo and Milyo 2020; Shaw et al. 2021). Yet no one has subjected the Court’s assumptions to a carefully tailored and methodologically robust test of the effects of campaign contributions or expenditures on appearances of “corruption” as the Court defines it. In this article we use survey experiments to identify the financial threshold at which contributions are deemed corrupt. Our investigation covers the two key circumstances addressed by the *Buckley* and *Citizens* cases: (1) when members of Congress receive direct campaign contributions, and (2) when

Mathew DeBell is a senior research scholar at the Institute for Research in the Social Sciences at Stanford University in California, USA. Shanto Iyengar is a professor of political science at Stanford University in California, USA. This research was supported by grant no. SES-1444910 to Stanford University (Shanto Iyengar, Principal Investigator).

organizations make independent expenditures to support candidates for Congress.¹

We begin by using the *Buckley* and *Citizens* decisions to derive a set of testable hypotheses about what appears corrupt. Specifically, our hypotheses test the Supreme Court's assumptions about how the degree of apparent corruption is affected by the type and the monetary value of campaign contributions and independent expenditures. Next, we review the literature on public opinion concerning campaign finance and describe its limitations with respect to tests of the Court's assumptions. Third, we describe our experiments designed to overcome the key limitations of the existing literature, and we present our findings. We find that the dollar threshold at which a substantial proportion of the citizenry believes contributions corrupt the political process is very low, and much lower than the legal ceiling. Moreover, contrary to the Court's assertion, independent expenditures are especially tainted in the sense that they are more likely to be associated with corruption. We conclude with a discussion of the implications of these empirical results for campaign finance law and democratic legitimacy.

HYPOTHESES FROM BUCKLEY AND CITIZENS

The Supreme Court upheld limits on campaign contributions in *Buckley* because of the government's interest in "the prevention of corruption and the appearance of corruption spawned by the real or imagined coercive influence of large financial contributions on candidates' positions and on their actions if elected to office" (*Buckley* 1976, 25). The Court defined corruption as "large contributions ... given to secure a political *quid pro quo* from current and potential office holders" (*Buckley* 1976, 26).² "Corruption," in this context of constitutionally acceptable justifications for campaign finance regulation, is only *quid pro quo* exchange, and the Court reasoned that the public interest in minimizing the *appearance* of corruption is substantial because of its potential to undermine the system of government.

In *Buckley* the Supreme Court held that the government may limit individual donors to a maximum of \$1,000 per candidate (now \$2,800 and indexed for inflation). The Court found these lim-

its permissible under the First Amendment because contribution limits impose relatively little direct restraint on the freedoms of speech or association. In contrast, limits on independent expenditures "impose direct and substantial restraints on the quantity of political speech" (*Buckley* 1976, 39) and therefore "heavily [burden] core First Amendment expression" (48). It was in the context of this First Amendment burden that the Court struck down an independent expenditure limit, reasoning that "the independent expenditure ceiling ... fails to serve any substantial government interest stemming the reality or appearance of corruption in the electoral process" (*Buckley* 1976, 47–48). The Court asserted that independent expenditures were "untainted" because "[t]he absence of prearrangement and coordination ... alleviates the danger that expenditures will be given as a *quid pro quo* for improper commitments from the candidate" (47).³

Later, the Court extended this line of reasoning in *Citizens* and held that the government may not ban independent expenditures by corporations (whether for-profit or non-profit), labor unions, or other associations. In essence, the Court exempted independent expenditures from most regulation on the grounds they do not give rise to the appearance of corruption.

The opinions and reasoning of the Supreme Court in *Buckley* and *Citizens* reflect three core assumptions about the effects of campaign finance laws and practices that form the basis for our hypotheses: (1) The public exercises reasonable judgment when considering the appearance of corruption, (2) individual contributions create an

¹Independent expenditures are defined as money spent "for a communication expressly advocating the election or defeat of a clearly identified candidate that is not made in cooperation, consultation, or concert with, or at the request or suggestion of, a candidate, a candidate's authorized committee, or their agents, or a political party committee or its agents" (11 CFR 100.16(a)).

²Many have criticized the narrowness of this definition, e.g., Lessig (2014), and the public's view of corruption is more expansive (e.g., Bowler and Donovan 2016; Primo and Milyo 2020; Spencer and Theodoridis 2020).

³The opinion also held that it is unconstitutional to limit candidates' personal expenditures on their campaigns, struck down the system by which FEC commissioners were appointed, and upheld other aspects of federal law. We do not address these aspects of the case in this article.

appearance of corruption, and (3) independent expenditures do not create such an appearance.⁴ In examining these assumptions, we posit that the considered opinions of the general public are an appropriate metric for defining the appearance of corruption in a democratic polity.

To regard the public as exercising reasonable judgment about the appearance of corruption, the court has to find or assume that public opinion is based on rational considerations about the campaign finance system and not on animus toward politicians or contributors.⁵ From this “reasonableness” assumption, we derive the testable hypothesis that perceived corruption increases monotonically with the amount of money contributed or spent. That is, additional money may increase but never decreases the amount of apparent corruption.

As a second indicator of reasonableness, we look for evidence of the additive effects of corrupt practices. If a single donor gives money to a single politician, the contribution has some probability, p , of inducing corrupt behavior on the part of the politician. If many donors give the same amount of money to many politicians, the probability of corrupt behavior occurring is logically $> p$. Therefore we expect perceptions of corruption to increase when multiple donors give to multiple members of Congress, compared to when an individual donor gives to an individual member.

The hypotheses based on the assumption of reasonable public judgment can now be summarized as follows:

Hypothesis 1A: Perceived corruption increases monotonically with the amount of money contributed or spent.

Hypothesis 1B: Perceived corruption is greater when many donors give to multiple members of Congress than when an individual donor gives to an individual member.

The second major assumption by the Court is that large direct contributions create an appearance of corruption while small contributions do not. The threshold between large and small is the legal contribution limit; if small, lawful contributions appeared corrupt (or caused actual corruption), the law would not reduce the appearance (or occurrence) of corruption, and the contribution limits would not be effective or justifiable. The majority wrote that “[t]he contribution provisions [prohib-

iting campaign contributions exceeding \$1,000 per election] … are appropriate legislative weapons against the reality or appearance of improper influence stemming from the dependence of candidates on large campaign contributions …” (*Buckley* 1976, 3). The Court considered and rejected the claim “that the \$1,000 restriction is unrealistically low because much more than that amount would still not be enough to enable an unscrupulous contributor to exercise improper influence …” (*Buckley* 1976, 30).

Our second set of hypotheses about the relationship between direct contributions and apparent corruption may now be stated as follows:

Hypothesis 2A: Direct contributions below the legal limit do not appear corrupt.

Hypothesis 2B: Direct contributions exceeding the legal limit appear corrupt.

The third and final assumption is that independent expenditures do not create an appreciable appearance of corruption. The *Buckley* Court’s opinion was that independent expenditures had little potential for improper influence. “The absence of prearrangement and coordination of an expenditure with the candidate or his agent not only undermines the value of the expenditure to the candidate, but also alleviates the danger that expenditures will be given as a quid pro quo for improper commitments from the candidate” (*Buckley* 1976, 47). Therefore, the Court found that “the independent expenditure ceiling thus fails to serve any substantial governmental interest in stemming the reality or appearance of corruption in the electoral process …”

⁴The Court also has concluded that campaign finance law can influence public opinion. Specifically, campaign finance regulations can limit the appearance of corruption in the political system and changing these appearances will improve the public’s political engagement, participation, and support for the political system. These propositions are beyond the scope of our present empirical tests. For consideration of such issues, see, e.g., Shaw et al. (2021).

⁵Laws that curtail liberties based on mere hostility or animus are constitutionally suspect. In *City of Cleburne v. Cleburne Living Center* (1985), the Court declared that a law faces greater scrutiny if it appears to be based on “a bare … desire to harm a politically unpopular group” (580, quoting *U.S. Department of Agriculture v. Moreno*, 413 U.S. 528, 534 (1973)). Also see *U.S. v. Windsor* (2013) and *Palmore v. Sidoti* (1984), disallowing curtailment of liberties based on animus. A law should not be based on private biases such as hostility toward politicians or contributors.

(*Buckley* 1976, 47–48). The *Citizens* Court similarly concluded that corporate independent expenditures do not create an appearance of corruption.

A clear testable implication of the special treatment afforded independent expenditures is that any appearance of corruption associated with independent expenditures will be less apparent than corruption associated with direct expenditures that exceed the federal limits. The *Buckley* Court assumed this was the case, writing that “... independent advocacy ... does not presently appear to pose dangers of real or apparent corruption comparable to those identified with large campaign contributions” (*Buckley* 1976, 46). If this was true in 1976, it might not be true today due to changing circumstances (Katyay 2011), but it remains questionable whether independent expenditures presently appear less corrupt than large direct (now illegal) contributions.

Our third set of hypotheses concerning independent expenditures can be stated as follows:

Hypothesis 3A: Independent expenditures do not appear corrupt, regardless of the amount.

Hypothesis 3B: Lawful independent expenditures appear less corrupt than unlawful direct contributions exceeding federal limits.

APPEARANCES AND PUBLIC OPINION: THE LITERATURE

There is a significant literature on the relationship between campaign finance and public opinion about corruption. Yet the Supreme Court’s assumptions elaborated in our preceding hypotheses have not been well tested. There is also a literature attacking the premise that campaign finance law affects public opinion at all. In this section we review what prior studies have to say about our hypotheses and we address the claim that finance practices are irrelevant to public opinion.

Bowler and Donovan (2016) used survey experiments to examine the relationship between campaign financing and public cynicism about Congress. They found that the sources of funding, amounts spent, and what they are spent on all affect perceptions of corruption: 62% of participants found a \$1,000,000 independent expenditure by a corporation to be at least somewhat corrupt, 49% found an independent expenditure of the same amount by an individual at least somewhat corrupt, and 51% found a direct contribution of \$5,000 by

an individual to be at least somewhat corrupt. In a separate small convenience sample, a majority of participants found contributions of just \$50 corrupt. Bowler and Donovan further found that independent expenditures by corporations were seen as more “corrupt” than independent expenditures by individuals, and they found spending for “negative” TV advertising was seen as more corrupt than spending for other forms of advertising.

These findings directly challenge the Court’s claim in *Citizens* that independent expenditures by corporations do not create an appearance of corruption. But a critical limitation of the study is that it presented scenarios that are not necessarily corrupt, which guarantees that study participants considered factors other than quid pro quo corruption. For example, participants were asked how corrupt or honest it was for a candidate for Congress to receive a direct contribution of \$500 from an individual and then use the money to buy TV advertising. Under U.S. law this behavior is not “corrupt.” Measuring opinion about whether contributions or expenditures are “corrupt” is informative about public opinion, but is not directly applicable to the particular form of corruption recognized by the Supreme Court. Indeed, Bowler and Donovan’s finding that spending on negative ads is especially likely to elicit perceptions of corruption shows the importance of factors other than the perception of quid pro quo. To match empirical findings to the Court’s rationale, we need surveys that use scenarios more compatible with the Court’s narrow definition of corruption.

Brown and Martin (2015) tested several of the Court’s empirical assumptions in *Citizens* about campaign spending and the public’s faith in democracy. Using survey vignettes that experimentally varied the details of campaign contribution and expenditure scenarios, they found that participants’ “faith in democracy” was reduced as the amount of organizations’ campaign contributions increased. However, these scholars’ purpose was to demonstrate “a public value in campaign spending that is separate ... from quid pro quo corruption” (1089), so they did not report effects on the perceptions of quid pro quo corruption specifically.

Ayres (2003) found that “large” or \$25,000 contributions to political parties were viewed by a majority of Americans as likely to result in members of Congress giving “special consideration” to the donor. Blass et al. (2012) found that 70% of survey participants believed politicians vote to

please their contributors “often” or “all the time.” A related study after the *Citizens* decision found that 69% of participants agreed that “the new rules that let corporations, unions and people give unlimited money to Super PACs will lead to corruption;” 73% agreed that “there would be less corruption if there were limits on how much could be given to Super PACs;” and 68% agreed that “a company that spent \$100,000 to help elect a member of Congress could successfully pressure him or her to change a vote on a proposed law” (Brennan Center for Justice 2012). These findings all tend to support the case for a link between campaign finance and public opinion, but Ayers’ (2003) finding, the voting described by Blass et al. (2012), and the “corruption” described in two of the three Brennan Center questions do not necessarily appear to be quid pro quo. So these findings almost certainly overstate the public’s perception of *quid pro quo* corruption.⁶

Several studies have challenged the idea that campaign finance practices affect perceived corruption, cynicism, turnout, or related variables. Persily and Lammie (2004, 158) argued that perceptions of corruption are driven primarily by people’s political predispositions, not by the campaign finance system, and that “attitudes about incumbent job performance and about government corruption are one and the same.” Blass et al. (2012) found little evidence that beliefs about the influence of money in politics affect political participation (such as voter turnout). Coleman and Manna (2000) found that while campaign spending boosts public knowledge, it had little or no effect on trust, efficacy, attention, or interest. Primo (2002) observed a near-zero correlation between trust in government and campaign spending over time. Milyo (2016) found that state campaign finance regulations have almost no impact on trust in state government, and Primo and Milyo (2006; 2020) similarly found no substantial effects of campaign finance laws on political efficacy or attitudes toward government.

The collective tenor of several of these studies is to suggest, first, that campaign finance laws do not affect public perceptions of corruption (e.g., Persily and Lammie 2004), and second, that finance laws and public perceptions of corruption do not affect general support for the political system (Persily and Lammie, and work by Primo and Milyo). Our results will challenge the first claim, as have studies already cited (e.g., Bowler and Donovan 2016;

Brown and Martin, 2015). We do not empirically address the second claim, but for the sake of contextualizing our own study we note a basis for expecting that perceptions of corruption can affect system support, and we will note some doubts regarding the literature denying that it does.

The expectation that perceptions of corruption affect system support is based on a well-developed theoretical literature about democratic political legitimacy. Popular legitimacy rests on the belief that government authority is right and proper and worthy of compliance (e.g., Easton 1975; Gurr 1970). The appearance of fair and lawful conduct by public officials is an important element of legitimacy (Tyler 2001). The appearance of corruption is thought to harm democracies by eroding public confidence in government institutions. Apparent corruption encourages people to believe their elected representatives care more for private moneyed interests than the public interest, that government institutions operate in secret instead of in the open, and that policy debates by leaders are disingenuous (Warren 2006), all of which are expected to undermine the public’s trust in government, their sense of political efficacy, and their motivation to participate in politics. Evidence of corruption spurs increased participation in some circumstances, such as when voters who still believe in the electoral system are motivated to replace corrupt officials (e.g., Escaleres et al. 2012; Kostadinova 2009). This literature is not without controversy (Seligson, 2002, reviews controversy about corruption’s effects), but the evidence also suggests that people who believe corruption is relatively commonplace feel less efficacious and less trusting of government (Agerberg 2019; Mishler and Rose 2001; Wang 2016), and that under some conditions corruption or its appearance makes people less likely to vote (Dahlberg and Solevid 2016; Kostadinova 2009; Stockemer et al. 2013; Warren 2004; in contrast see Shaw et al. 2021), which undermines legitimacy. As Anderson and Tverdova (2003, 91) put it, “political corruption is a powerful determinant of political support across widely varying political, cultural, and economic contexts.”

⁶In the case of the Brennan Center study, all three survey questions unfortunately used an agree-disagree question format that has been known for generations to be biased in favor of agreement (e.g., Cronbach 1946; Lentz 1938; Saris et al. 2010).

The empirical literature denying that finance laws can affect public opinion has a number of limitations. Milyo (2016) and Primo and Milyo (2006; 2020) looked for the effects of state laws, which may be less salient to survey participants than national laws and national politics, and where numerous confounds make effects difficult to detect. Persily and Lammie (2004) argued that the campaign finance system does not affect perceptions of corruption, but their empirical analysis had three major flaws: (1) their measures of perceptions of “corruption” did not include the only definition now accepted by the Supreme Court: quid pro quo exchange; (2) their sole campaign finance variable was the aggregate “soft money” spending by political parties at seven points in time from 1992 to 2004, yet many other finance variables, such as the amounts and sources of direct contributions and independent expenditures, are of equal or greater concern; and (3) their multivariate data analysis found that factors such as demographics and political opinions correlated moderately with their problematic “corruption” indicators, but never actually modeled the relationship between perceived corruption and campaign finance characteristics.

The other cited studies finding null effects on public opinion from campaign finance laws show that the relationship between campaign finance law and cynicism is not linear and monocausal. If large contributions were the only cause of political cynicism, and the relationship between cynicism and contributions was linear, then we would not observe the results from these studies, so the studies have falsified that narrow hypothesis. But a more complex causal system seems likely, or at least possible, involving both nonlinearity and multicausal elements. Concerning nonlinearity, concluding that campaign finance regulations do not affect public opinion may be like observing that half an aspirin does not help a headache and then concluding that aspirin is an ineffective treatment. If policies have not attained what in medicine is called the minimum effective dose, it comes as no surprise if regulations have no measurable effect on public cynicism.

Concerning multi-causality, campaign finance laws may cause cynicism, but if other factors also cause cynicism (such as polarization, negative advertising, dishonest politicians, media biases, partisan gerrymandering, and legislative gridlock), then it is also not surprising to find null effects of finance laws on cynicism. In causal analysis, this is the

problem of pre-emption (Brady 2011), which can lead to failures to observe real causal effects. An example of pre-emption illustrates the point. If glare and hangovers both cause headaches, removing glare from a patient’s environment may not have a measurable effect if the hangover is still present. When other factors strongly promote cynicism, it may be that alterations to campaign finance are a necessary but not sufficient condition for changes in opinion.

Question wording experiments in surveys are a powerful tool to overcome this problem of multi-causality and pre-emption. The advantage of experimentation over observational studies of changes in opinion in response to real-world changes in regulation is that survey experiments can manipulate specific elements of the regulatory scenario while holding all other factors constant. By contrast, observational studies are badly confounded by myriad endogenous considerations that vary across time and place. With survey experiments we can isolate variables of interest and measure their effects on opinion. We can also tailor the questions to the defining issues raised by the court decisions, overcoming the other primary limitation of the existing literature. Persily and Lammie (2004) lamented that available studies “do not precisely answer the more relevant constitutional questions concerning the perception of corruption arising from large campaign contributions.” We present the results of a study that does precisely that.

METHODS AND DATA

Data to test our hypotheses come from two scientific surveys conducted by the American National Election Studies (ANES): the ANES 2016 Time Series Study and the ANES 2018 Pilot Study. Both studies were funded by the National Science Foundation and designed and conducted jointly by the University of Michigan and Stanford University.

The ANES Time Series has been widely considered a “gold standard” survey representing the highest quality of public opinion research (e.g., Aldrich and McGraw 2012; National Science Foundation 2018). The 2016 study used two nationally representative probability samples to interview 4,270 people using either face-to-face interviews or questionnaires administered on the Internet. The response rate (using AAPOR response rate

formula 1; AAPOR 2016) was at least 50% in face-to-face interviews and at least 44% on the Internet. Interviews were in English or Spanish with U.S. citizens age 18 or older. The data use complex samples and must be statistically weighted for analysis that generalizes to the population, with sampling errors that account for the complex design; our results use these methods. Further methodological details of the study are given in DeBell et al. (2018).

The 2018 study used a non-probability, opt-in sample and was administered online. The sample is weighted to closely match the U.S. adult citizen population in terms of demographic characteristics, but unlike the 2016 study's top-quality probability sample, this study is not as well suited to making population inferences because "this method produces a sample that looks similar to a probability sample on the matched characteristics, but may still differ in unknown ways on unmatched characteristics" (ANES 2019, 1). The sample is intended to support statistical analysis that examines the relationships between variables and to provide valid tests of differences between experimental groups. Further details of this study are given in ANES (2019).

The ANES 2016 Time Series Study asked two questions about the appearance of corruption in campaign finance, as follows:

When organizations spend money on advertising to support candidates for Congress, how much does Congress respond by passing laws to benefit those organizations?

When people give [\$AMOUNT] each to the election campaigns of Members of Congress, how much does Congress respond by passing laws to benefit people who gave them money?

These items are, respectively, the questionnaire items "CAMPFIN_ORG" and "CAMPFIN_DIRECT," and correspond to the data variables V162235 and V162236. The first question measured the appearance of corruption from independent expenditures. The second question measured the appearance of corruption from direct contributions. The second question randomly varied the amount of money stated in the question as either \$50, \$250, \$1,000, \$2,000, or \$5,000. Note that at the time of the interview, only the \$5,000 contribution to a single campaign would have been illegal.

These questionnaire items provide strong analytical leverage over key questions of campaign finance because, uniquely in the survey research literature, they are tailored to the appearance of corruption from a quid pro quo exchange. Instead of asking about "corruption" that is not defined, they describe an apparently corrupt act by Congress—to "respond" to campaign donations or to independent advertising by voting to benefit the donors or advertisers—and ask how often this occurs. Survey participants may or may not consider such behavior corrupt, but they are assessing the frequency of behavior that meets the legal standard of the appearance of corruption. (It is possible for a legislator to "respond" to a donation for reasons other than a quid pro quo exchange, but the response does have this *appearance*.)

The ANES 2018 Pilot Study asked the following questions about the appearance of corruption in campaign finance:

Direct1: When people give \$[AMOUNT: 25 or 2,500] each to the election campaigns of Members of Congress, how much does Congress respond by passing laws to benefit people who gave them money?

Direct2: When a person gives \$[AMOUNT: 25 or 2,500] to the election campaign of a Member of Congress, how much does that Member of Congress respond by supporting laws to benefit the person who gave them money?

Indirect1: When organizations spend money on advertising to support candidates for Congress, how much does Congress respond by passing laws to benefit those organizations?

Indirect2: When an organization spends \$[AMOUNT: 2,500 or 250,000] on advertising to support a candidate for Congress, how much does that candidate respond by supporting laws to benefit that organization?

Participants were randomly assigned to receive either Direct1 or Direct2, but not both, and were separately randomly assigned to receive either Indirect1 or Indirect2, but not both; these are "between subjects" wording experiments in which each participant was asked to respond to just one question about direct contributions and just one question about indirect expenditures. Direct1 and Indirect1 duplicate the ANES 2016 items, but Direct1 used different dollar amounts than were

used in 2016. Direct2 changed the direct contribution scenario from the plural to the singular. Indirect2 changed the indirect expenditure scenario from the plural to the singular and randomized specific dollar amounts.

RESULTS

Before testing our hypotheses, we begin with a description of the results for the full population and for subgroups, based on ANES 2016 population estimates. Table 1 shows the appearance of cor-

TABLE 1. APPEARANCE OF CORRUPTION
VIA INDEPENDENT EXPENDITURES

Question: "When organizations spend money on advertising to support candidates for Congress, how much does Congress respond by passing laws to benefit those organizations? Not at all, a little, a moderate amount, a lot, or a great deal?"

Characteristic	Percent	
	A little or more	A moderate amount or more
Total	92.3	74.9
Voter turnout (2016)		
Voted	92.8	75.0
Did not vote	91.3	74.9
Partisanship		
Democrat or leaner	92.8	74.5
Independent or other	91.6	78.2
Republican or leaner	92.2	74.3
Trust in government		
Middle or higher	92.6	74.3
Low	92.3	75.5
Efficacy		
Median or higher	93.2	75.2
Low	91.5	74.7
Political knowledge		
Median or higher	93.6	76.0
Low	90.5	73.3
Age		
18–29	93.0	76.7
30–49	93.1	75.5
50–64	92.2	74.1
65 and older	91.2	74.0
Sex		
Female	92.4	73.6
Male	92.4	76.7
Educational attainment		
High school or less	89.3	73.8
Some college	94.1	76.2
Bachelor's or higher	94.7	75.2
Race/ethnicity		
White non-Hispanic	93.0	75.7
Black non-Hispanic	88.5	71.5
Hispanic	91.9	72.3
Other non-Hispanic	91.9	76.5

Source: American National Election Studies 2016 Time Series Study.

ruption via independent expenditures. A mere eight percent of the electorate believe that Congress does not respond to independent expenditures by passing laws to benefit the organizations that make independent expenditures. There is overwhelming agreement, with 92 percent of Americans agreeing, that Congress responds at least "a little" to independent expenditures by passing laws to benefit the organizations that make the expenditures. A large majority, 75 percent, say there is "a moderate amount" or more of quid pro quo responsiveness.

The size of this large majority is remarkably consistent across diverse population groups. It remains approximately three-quarters of the population, plus or minus about four percentage points, among voters and non-voters, Democrats, Republicans, and political independents, different demographic groups, and people who are relatively high or low in their trust in government, sense of political efficacy, or level of political knowledge. Three out of four Americans believe that Congress responds at least a moderate amount of the time (or more) to independent expenditures by passing laws to benefit the spending organizations, and this consensus is not meaningfully influenced by voting status, partisanship and other political predispositions, or demographics.

Table 2 shows the mean corruption ratings for direct expenditures, by contribution amount and for the same participant characteristics shown in Table 1. Corruption ratings are scaled from 0 to 1 where 0 means "not at all," 0.25 means "a little," 0.5 means "a moderate amount," 0.75 means "a lot," and 1 means "a great deal." Again, although there is some variation, the take-away here is the consistency across groups. Voters and non-voters, Democrats and independents and Republicans, and people in different categories of efficacy, political knowledge, age, sex, educational attainment, and race/ethnicity all perceive meaningful degrees of corruption at all dollar amounts—always more than "a little," on average, and usually closer to "a moderate amount" of corruption.

Hypothesis 1A predicted that perceptions of corruption increase monotonically with the amount of money contributed or spent. Table 3 shows the mean corruption ratings by contribution or expenditure amount and type. In the ANES 2016 study, the mean rating increased monotonically from .34 for \$50 contributions (substantively, somewhere between a little and a moderate amount) to

TABLE 2. MEAN CORRUPTION RATING BY CONTRIBUTION AMOUNT AND RESPONDENT CHARACTERISTIC

Characteristic	Contribution dollar amount (experimental group)				
	\$50	\$250	\$1,000	\$2,000	\$5,000
All respondents	.34	.38	.44	.45	.48
Voters (2016)					
Voted	.34	.37	.44	.46	.48
Did not vote	.36	.41	.47	.43	.46
Partisanship					
Democrat or leaner	.37	.41	.45	.44	.50
Independent or other	.29	.37	.47	.50	.49
Republican or leaner	.32	.35	.43	.46	.45
Trust in government					
Middle or higher	.37	.42	.44	.44	.50
Low	.32	.35	.45	.47	.46
Efficacy					
Median or higher	.34	.34	.45	.47	.46
Low	.34	.42	.44	.43	.50
Political knowledge					
Median or higher	.31	.34	.43	.46	.48
Low	.41	.44	.47	.44	.47
Age					
18–29	.36	.45	.43	.49	.47
30–49	.37	.39	.43	.46	.48
50–64	.31	.35	.45	.46	.43
65 and older	.32	.36	.48	.39	.56
Sex					
Male	.34	.38	.44	.47	.46
Female	.34	.38	.44	.44	.49
Educational attainment					
High school or less	.35	.43	.45	.46	.50
Some college	.33	.39	.47	.47	.49
Bachelor's or higher	.34	.32	.42	.43	.43
Race/ethnicity					
White non-Hispanic	.32	.36	.43	.44	.48
Black non-Hispanic	.42	.42	.48	.45	.52
Hispanic	.37	.50	.52	.53	.42
Other non-Hispanic	.36	.36	.42	.42	.47

.48 (substantively the same as a moderate amount) for \$5,000. The mean rose sharply from .34 to .44 as contributions increased from \$50 to \$1,000, but then increased only slightly from .44 to .48 as contributions increased from \$1,000 to \$5,000. This indicates that perceptions of corruption are relatively inelastic within the \$1,000 to \$5,000 range. In the 2018 study, the singular question wording (referring to “a person” giving to “a Member of Congress”) yielded average ratings of .22 and .41, respectively, for \$25 and \$2,500 contributions; the plural question wording (referring to “people” giving to “Members of Congress”) gave correspond-

TABLE 3. MEAN CORRUPTION RATINGS BY CONTRIBUTION AMOUNT AND TYPE

Contribution	Mean	s.e.	n
Direct contributions, 2016 study			
\$50	.34	0.014	708
\$250	.38	0.015	729
\$1,000	.44	0.014	684
\$2,000	.45	0.014	705
\$5,000	.48	0.014	716
Direct contributions, 2018 study			
Singular question wording			
\$25	.22	0.011	614
\$2,500	.41	0.013	629
Plural question wording			
\$25	.26	0.011	624
\$2,500	.45	0.013	627
Indirect expenditures			
2016 study: general	.55	0.006	3561
2018 study: general	.52	0.013	1206
2018 study: \$2,500	.45	0.012	635
2018 study: \$250,000	.61	0.013	655

ing ratings of .26 and .45. Indirect expenditures of \$2,500 and \$250,000 were rated with means of .45 and .61, respectively. In every case, larger contributions or expenditures were rated as more corrupt than smaller amounts. This confirms Hypothesis 1A.

Hypothesis 1B was that the public perceives more corruption in relations among multiple donors and recipients than in a single donor-recipient relationship. To test this hypothesis we compare the mean corruption ratings in the singular and plural question wordings. For a \$25 contribution, the means were .22 for a single donor-recipient relationship and .26 for groups (a difference of .04, *s.e.* = 0.016, *t* = 2.30, *p* < .05). For a \$2,500 contribution, the means were .41 for a single donor-recipient relationship and .45 for groups (a difference of .04, *s.e.* = .018, *t* = 1.99, *p* < .05). This confirms Hypothesis 1B.

Hypothesis 2A was that direct contributions below the legal limit do not appear corrupt, while 2B was that direct contributions over the limit appear corrupt. The legal limit was \$2,700 at the time of these surveys in 2016 and 2018 (Federal Election Commission 2015, 2018). Table 3 shows that the mean corruption rating for legal contributions, such as \$1,000 or \$2,000, was .44 or .45, and the mean corruption rating for an illegal contribution, \$5,000, was .48, which is a substantively trivial difference. Table 4 shows the percentage distribution of responses to the direct contribution item in the ANES 2016 study. The bottom row of the table shows the percentage of participants—the estimated percentage of the U.S. adult citizen

TABLE 4. APPEARANCE OF CORRUPTION VIA DIRECT CONTRIBUTIONS: PERCENTAGE DISTRIBUTION OF RESPONSES BY CONTRIBUTION DOLLAR AMOUNT

Response	Contribution dollar amount (experimental group)				
	\$50	\$250	\$1,000	\$2,000	\$5,000
A great deal	6.9	8.4	8.3	8.9	12.0
A lot	12.1	12.7	17.0	17.9	17.3
A moderate amount	21.8	27.5	35.0	33.2	32.8
A little	29.0	26.2	23.6	25.3	25.5
Not at all	30.2	25.3	16.0	14.7	12.4
Moderate or more	40.8	48.6	60.3	60.0	62.1

population—who indicated that a given contribution amount caused “a moderate amount” or more corruption. Small contributions of \$50 or \$250 were rated as moderately or more corrupt by 41 and 49 percent of participants, respectively. Larger legal contributions of \$1,000 or \$2,000 were rated as moderately or more corrupt by 60 percent of participants. Contributions of \$5,000 were rated as moderately or more corrupt by 62 percent of participants, again reflecting no substantively large difference between contributions in the \$1,000 to \$5,000 range.

Given that we observe a monotonic relationship between contributions and perceived corruption, and that 49 percent of the electorate see “a moderate amount” or more corruption when the direct contribution amount to members of Congress is \$250 (Table 4), it follows that a majority of the American electorate believes direct contributions cause “a moderate amount” or more of corruption when the direct contributions are slightly greater than \$250. To estimate the contribution amount that crosses the majority threshold for the perception of at least moderate corruption, we model the five observed values for “moderate or more” in Table 4 with a logarithmic function: $y = \beta_0 + \beta_1(\ln(x))$, where y is the percentage of the electorate perceiving moderate or greater corruption and x is the dollar amount of the campaign contribution. This model fits the data very well ($R^2 = .95$, $\beta_0 = 22.167$, $\beta_1 = 4.959$) and estimates the critical threshold—the contribution amount at which a majority sees at least moderate corruption—as \$274. This curve is shown in Figure 1. We can also estimate the percentage of the electorate that perceives moderate or more corruption from a contribution at the then-legal limit of \$2,700: it is 61 percent.

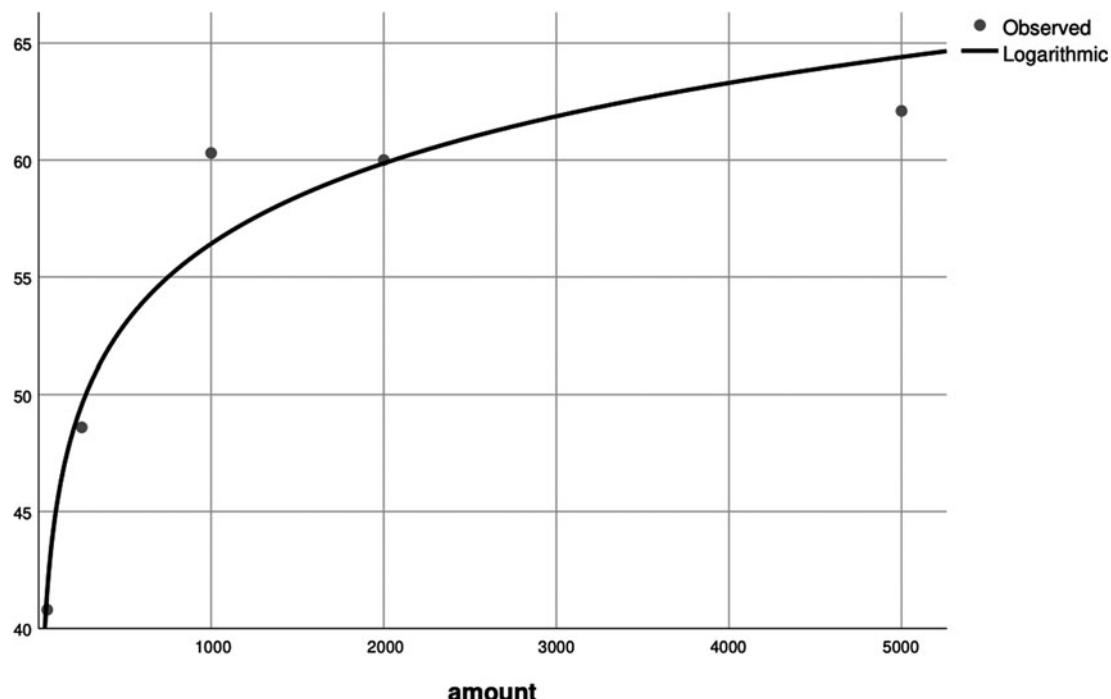


FIG. 1. Logarithmic curve fitted to observed percentages of electorate perceiving moderate or greater corruption, by contribution amount (ANES 2016 data). Horizontal axis is contribution amount (dollars); vertical axis is percentage of electorate.

TABLE 5. APPEARANCE OF CORRUPTION VIA INDEPENDENT EXPENDITURES

Characteristic	Percent					Percent sums		
	Not at all	A little	A moderate amount	A lot	A great deal	A little or more	A moderate amount or more	A lot or more
2016 estimate	7.6	17.5	37.5	23.8	13.6	92.4	74.9	37.4
2018 estimates								
2016 question wording	16.3	13.7	30.6	23.0	16.3	83.6	69.9	39.3
Single amount: \$2,500	17.2	24.5	30.6	18.3	9.5	82.9	58.4	27.8
Single amount: \$250,000	11.0	9.8	29.4	22.4	27.4	89.0	79.2	49.8

These results confirm Hypothesis 2B, that contributions above the \$2,700 legal limit appear corrupt, but reject Hypothesis 2A, that smaller contributions do not appear corrupt. Contributions well below the legal threshold appear nearly as corrupt as larger contributions, and contributions as low as \$274 are seen by a majority of Americans as giving rise to at least a moderate amount of corruption.

Hypothesis 3A was the key empirical predicate of the *Citizens United* decision, namely, that independent expenditures do not appear corrupt. Table 5 summarizes public opinion concerning the appearance of quid pro quo corruption from independent expenditures. The table includes the results from the single question on independent expenditures in the ANES 2016 study and from the three versions of the question in the 2018 study. The results under the “Percent” heading show the percentage of participants choosing each response option. For convenience, the results under “Percent sums” show the sums of selected combinations of the answers.

Table 5 shows that, when asked about independent expenditures that do not refer to a specific dollar amount, 75 percent of ANES 2016 participants perceived “a moderate amount” or more of quid pro quo corruption. In the 2018 study, 70 percent perceived the same degree of quid pro quo corruption. When a low dollar amount of \$2,500 was specified, this figure fell to 58 percent; with the higher dollar amount of \$250,000, it rose to 79 percent.

“A moderate amount” is not an objective or necessary threshold, but we think it is a reasonable one. For a matter as serious as quid pro quo corruption on the part of elected officials, any occurrence is meaningful, and it probably should not have to happen “a lot” or “a great deal” to constitute a meaningful societal problem or to justify a response. A reader who is more tolerant of corruption may prefer the “a lot or more” threshold, while a reader who is

more intolerant of corruption may prefer “a little or more.” Using the moderate threshold, a large majority of the electorate believes independent expenditures give rise to corruption, and this result has been replicated in different studies conducted two years apart using different methods, so Hypothesis 3A can be rejected.

The final hypothesis (3B) was that lawful independent expenditures appear less corrupt than unlawful direct contributions exceeding the federal limits. We compare these directly using the means reported in Table 3. The mean corruption rating for lawful independent expenditures, .55, is greater than the mean for the highest lawful direct contribution, .48 (difference .07, $s.e.=0.015$, $t=4.44$, $p < .001$).⁷ The hypothesis is rejected. Lawful independent expenditures with no amount specified actually appear more corrupt than direct contributions of \$5,000. The magnitude of this effect is non-trivial (Cohen’s $d=0.24$).

DISCUSSION AND CONCLUSION

We have found that public perceptions of *quid pro quo* corruption in congressional campaign finance are reasonably related to campaign finance practices. Perceptions of corruption increase consistently (monotonically) with the amount of money contributed or spent (Hypothesis 1A), and the public perceives more corruption in relations among multiple donors and recipients than in a single donor-recipient relationship (Hypothesis 1B). The

⁷If we compare a lawful direct contribution of \$2,000 to a lawful independent expenditure (no specified amount), the hypothesis is also rejected. The difference is larger (difference .10, $s.e.=0.015$, $t=6.31$, $p < .001$).

public perceives a substantial degree of apparent quid pro quo corruption resulting from both campaign contributions and independent expenditures. The relative invariance in the relationship between perceived corruption and political characteristics such as voter turnout, efficacy, partisanship, and knowledge indicates that the perceived corruption of standard campaign practices is by no means limited to political cynics, experts, partisans, or any other narrow grouping. It is a super-majority judgment of the American citizenry.

Direct contributions appear corrupt at very low dollar amounts. While this result supported the hypothesis that direct contributions exceeding the limit appear corrupt (Hypothesis 2B), it rejects the converse hypothesis that contributions under the limit do not appear corrupt (Hypothesis 2A).

In the Supreme Court's majority opinion in *Citizens*, Justice Kennedy echoed *Buckley* when he wrote that "independent expenditures do not lead to, or create the appearance of, *quid pro quo* corruption" (Hypothesis 3A). Our results falsify this claim. Not only do independent expenditures give rise to the appearance of corruption, they do so to a greater extent than direct contributions that exceed federal limits (rejecting Hypothesis 3B).

A striking result is that the perceived corruption from direct contributions, for which limits have been upheld as constitutional because of the compelling state interest in their regulation, is *less* than the perceived corruption from judicially protected independent expenditures. In other words, even though unlimited independent expenditures appear more corrupt to the American public than do direct contributions of \$2,000 or \$5,000, the courts have upheld the direct contribution limits while striking down the independent expenditure limits.

Our empirical results corroborate prior findings that independent expenditures appear corrupt (e.g., Blass et al. 2012; Bowler and Donovan 2016; Brennan Center for Justice 2012), but strengthen the connection by using questions tailored to the Court's definition of quid pro quo corruption. To the extent that it applied, prior research was likely to have overstated the extent of quid pro quo corruption connected to campaign finance practice due to factors noted above such as the use of survey questions subject to acquiescence bias and the invitation to use expansive definitions of corruption. The new ANES questions that remove these exaggerating influences show that perceived corruption is substantial.

Contrary to Persily and Lammie (2004), our experimental manipulations establish that the dollar amounts given to candidates, or spent in advocacy of candidates, significantly impact perception of the extent of corrupt quid pro quo exchanges. Moreover, people's personal characteristics have little to do with their belief that members of Congress engage in quid pro quo behavior in response to either direct or independent expenditures.

In the context of the literature that has found null effects of campaign finance laws on political participation, trust, or efficacy (e.g., Blass et al. 2012; Coleman and Manna 2000; Primo and Milyo 2006), this study provides some reason, albeit only indirect, to question such conclusions. We have shown that legally allowable and commonplace contributions are seen by a majority of the electorate as contributing to quid pro quo corruption, and legal and commonplace independent expenditures are seen as being even worse than direct contributions. In view of previously cited literature linking perceived corruption to political system support, this suggests that current campaign finance laws may contribute to reduced trust in government and lower voter turnout.

Though we have falsified important empirical assumptions of the *Buckley* and *Citizens* decisions (i.e., hypotheses 2A, 3A, and 3B), we do not argue that these cases were wrongly (or rightly) decided. Whether the appearance of corruption and its consequences justify limiting First Amendment rights is beyond the scope of this article. What we have shown is that empirical assumptions integral to the legal reasoning of these decisions are unsound. If the decisions are nevertheless sound in their holdings, it is not by virtue of the validity of these assumptions.

Our findings do not unequivocally advantage one side at the expense of the other in the ongoing campaign finance debate. Opponents of campaign finance regulation can argue that if the apparent corruption elicited by \$5,000 contributions is barely any worse than legal contributions of \$2,800, and if the appearance of corruption curve is as flat as the logarithmic model in Figure 1 indicates, then existing limits on direct contributions accomplish little, and the contribution ceiling could be raised with little or no effect on citizen engagement, relative to the status quo. On the other hand, the public's willingness to see corruption in very small contributions, as low as \$25 or \$50, means that the rationale

underlying the current ceiling would apply just as well to a ceiling that is much closer to \$0 than \$2,800. This study has indicated that the average American would regard a \$2,800 contribution, or even a \$1,000 contribution, as sufficient to produce something very close to “a moderate amount” of congressional corruption. If Congress wishes to establish a contribution limit that produces something closer to “a little” than “a moderate amount” of apparent corruption, it would need to set contribution limits of less than \$250. For the average corruption rating to be no more than “a little,” the contribution limit would have to be about \$25.

Limits this low would make it impossible to fund federal campaigns at recent spending levels unless the donor base was radically expanded,⁸ and may be unconstitutional for that reason. In *Randall v. Sorrell* (2006), the Supreme Court struck down a Vermont law that imposed contribution limits as low as \$200 for state legislators. The Court rejected these limits because they were considered too low for candidates to run effective campaigns, and particularly too low for challengers to run effective campaigns in the most competitive races. Federal limits in the range of several hundred dollars or less surely would be challenged on this basis, given that the lowest limit the court has upheld was \$1,075 (in *Nixon v. Shrink Missouri Government PAC*, 2000; over \$1,600 in 2021 dollars) and that one of the “danger signs” the Court looks for in evaluating limits is a limit “substantially lower than … the limits we have previously upheld” (*Randall v. Sorrell* 2006, 253).

This creates a legal tension: these precedents contemplate a range of legal contribution limits that are low enough to avoid the appearance of corruption but high enough to allow conventional campaigning in the current environment, yet our results suggest a range satisfying both criteria does not exist. A majority of Americans views modest payments as yielding “a moderate amount” of corrupt exchange, and a significant minority sees corrupt exchanges from amounts as low as \$25 or \$50. This indicates that a significant minority of Americans regards any financial exchange between individuals and candidates for office as inherently suspect. It is not difficult to see why they might think so. Campaign contributions of any amount to politicians create a potential conflict of interest by giving officials a personal incentive to use their official powers to benefit private contributors at the

expense of the public interest. This incentive constitutes an appearance of corruption.

These results for direct contributions and independent expenditures have implications for governmental legitimacy in the United States. Existing campaign finance practices create conflicts of interest for Congress that appear meaningful to most Americans. These conflicts pit the public’s interest in legislation promoting the general welfare against members’ self-interest in raising money (or protecting their power) by serving their financial benefactors. Our results show that the public has little faith that members of Congress can resist even small incentives to serve contributors. As a result, policy discussions in which legislators offer public-interest rationales for their votes appear more likely to be disingenuous post-hoc rationalizations for decisions that were dictated by their own interest in raising money, giving the public reason to doubt that legislators follow established norms and rules of public service by acting in the public interest (see Warren 2006). As a result, members of the public have reason to trust the government less, to be less likely to vote, and to be less compliant with the law.

We close with a thought on what has been termed “sociological gobbledegook.” Many important court cases have relied on social science evidence, famously the *Muller v. Oregon* (1908) decision on labor hours with its citation of the Brandeis Brief, and *Brown v. Board of Education*’s (1954) citation of the harms caused by racial segregation in schools. Judges are still sometimes wary of social science. Chief Justice Roberts famously (at least among political scientists) called political science research on gerrymandering “sociological gobbledegook” (oral argument in *Gill v. Whitford* 2017). Roberts’ objection was not that the science is wrong, but that it is esoteric and relying on it might be seen as partisan. His stated concern was whether the public can accept and trust the decisions of the court if they are based on highly technical arguments. Of course, this does not stop all manner of scientific evidence from being presented in court, such as the routine use of forensic evidence in criminal cases and of survey evidence in trademark cases. Public opinion on campaign finance should

⁸In the 2020 presidential campaign, both party nominees raised a majority of their contributions in amounts over \$200 (Center for Responsive Politics 2021).

raise only limited concern over judicial legitimacy because it is less technical than most forensic evidence and less partisan than redrawing congressional districts. The data analysis presented here is relatively non-technical and, perhaps more importantly, its purpose and effect is simply to verify and quantify the nonpartisan “common sense of the American people” (Justice Stevens, concurring in part and dissenting in part, *Citizens* 2010, 486) that giving money to political candidates and spending money on behalf of political candidates more often than not looks corrupt.

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Address correspondence to:

Matthew DeBell

Institute for Research in the Social Sciences

Stanford University

30 Alta Road

Stanford, CA 94305

USA

E-mail: debell@stanford.edu

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