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Abstract

Election boycotts are over twice as common when international observers are present. Do international observers increase election boycotts as this correlation and past research suggest? This article argues not. Observers tend to go to elections with many problems, and it is primarily these, rather than monitors, that drive boycotts. Furthermore, opposition parties have reasons to hope that observers can improve the quality of the election or that they will increase attention to election fraud, and therefore opposition parties may actually abandon boycott plans. Whether they do, however, depends on their expectations about how the observers will behave. This makes it important to account for the varying reputation of observer organizations. Thus, using matching to address the selection problem, this article shows that international observers can actually deter boycotts, but only if the observers are reputable.

Keywords

elections, boycotts, democracy promotion, international election observation, transnational actors, international election monitoring, international organizations

As transnational organizations grow more active within borders, it becomes important to understand the many ways that these international actors influence domestic politics. The rise of international election monitoring

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(Kelley, 2008a; Santa Cruz, 2005) is particularly interesting because it involves both intergovernmental and nongovernmental organizations and invites numerous questions about how these organizations influence the domestic election process. Although their core mandate is to observe, their many activities on the ground during an election campaign and on election day may have several effects, both intended and unintended. This article considers a specific question recently raised by scholars: Do international election monitors influence the opposition parties' decisions about whether to boycott an election?

This question may seem odd; certainly monitors generally do not meddle with boycott decisions. Indeed, when the question was posed to a long-term practitioner of international election monitoring, it raised mostly puzzlement:

“[A B]oycott” is most often a political decision [by parties, political factions, etc. . . .] which frames an election. We may or may not think it to be a smart move, but there it is. Monitors/observers are not there to redraw the political context and decisions made by actors prior to an election. One also presumes this form of intervention in some way conflicts with electoral observation standards. (G. Fauriol, personal communication, September 12, 2008)

However, even if monitors do not interfere directly in boycott plans, their presence, activities, and anticipated pronouncements could still influence the decisions of opposition parties. Indeed, the data suggest a statistically significant positive correlation: In multiparty elections between 1975 and 2004, the opposition boycotted elections more than twice as often when monitors were present.¹ Furthermore, Hyde and Beaulieu (2009) have argued that monitors cause incumbents to shift to fraud that monitors are less likely to notice or criticize and that this shift raises opposition fears that monitors will overlook the fraud and endorse the election. Therefore, international monitors essentially cause the opposition to boycott to protest the fraud as well as the anticipated false endorsement.

However, this article argues that the positive correlation between monitors and boycotts arises because monitors tend to observe elections that are more prone to boycotts, and furthermore, not only do international observers rarely provoke boycotts, they can actually deter the opposition parties from boycotting. The effect of monitoring on boycotts is not uniform, however. Rather, how opposition parties respond to the presence of international monitors depends on their expectations about how the monitors will behave. This makes it important to account for the fact that organizations have different reputations. If, rather than expecting that monitors will reduce attention to fraud by falsely endorsing elections, opposition parties instead expect that monitors might improve the

quality of the election or increase attention to election fraud, then the presence of international monitors may decrease the opposition parties' incentives to boycott.

The study of boycotts is important because they lessen the quality of elections by limiting choice; some have argued that they may even derail the democratization process (Lindberg, 2006a, pp. 38, 245). The consequences of boycotts for democratization are complex (Schedler, 2002) and outside the scope of this article, but they clearly alter the election landscape in ways that have potentially important consequences for political competition. Furthermore, as the above quote suggests, monitoring organizations would likely be troubled if their presence actually provoked boycotts, as they generally strive to improve competition and participation in elections. As a 1989 memorandum by National Democratic Institute (NDI) notes, "The presence of observers—and particularly advanced knowledge in the country that they will be present—encourages political parties and volunteers to participate in the process" (NDI, 1989). If monitors instead provoke boycotts, then their use may need to be reevaluated. The effect of monitors on boycotts is also important to understand because it illuminates a new area of interaction between international actors and domestic politics.

Given the contradictory expectations about the effect of monitors on boycotts, this article therefore examines the relationship in greater depth. The article first discusses the domestic factors that drive boycotts. After briefly reviewing the argument for why monitors may increase boycotts, the article builds the alternative theoretical argument that depending on the reputation of the monitoring organization, observers may actually decrease boycotts. To examine both of these arguments, the article introduces new data on a greater number of elections covering the years 1975 to 2004 and more detailed data on both the quality of elections and the variety of monitoring organizations. By examining the different implications of the arguments and modeling the inherent selection problem, the article shows that the presence of more credible organizations actually is associated with a lower likelihood of boycotts. From a policy perspective, this has implications for the organization and funding of observation missions. From a theoretical perspective, it stresses the importance of differentiating the characteristics of international actors when theorizing about their influences on domestic politics.

What Domestic Factors Drive Boycotts?

To analyze how international observers may or may not influence boycotts, it is important first to understand why opposition parties boycott in the first place. Existing research is fairly clear that opposition parties primarily boycott

elections to protest fraud and to extract reforms (Beaulieu, 2006; Bratton, 1998; Lindberg, 2006b; Pastor, 1998). Boycotts can serve as protests because they attract attention to fraud.² For example, in Kenya in 1997, the opposition feared that Daniel Moi and the KANU party would fabricate a big victory in the polls and then rewrite the constitution, so they demanded reforms before the election. To pressure the government they threatened with a boycott, mass demonstrations, and civil disobedience (Barkan & Ng'ethe, 1998). Thus, more generally, if opposition parties observe unfairness in the pre-election environment, they may threaten to boycott. Therefore, boycotts should generally occur when the electoral rules and laws are poor, when the incumbent abuses government resources or restricts campaigning by opposition parties, when the media are biased, or when clear technical problems with the voters' registry or other procedural matters with elections become apparent during the pre-election period. In a study of Africa, Lindberg (2006b) finds that the only factor aside from election problems that correlated with boycotts was the degree of violence during the election.

Thus, under conditions of electoral unfairness and violence, parties may use boycotts to protest and to extract electoral reforms from incumbents who need wide participation to ensure the election's legitimacy (Schedler, 2002, p. 114). That boycotts are primarily instruments of protest is also supported by the fact that they tend to occur in countries in the lower to middle range of the democracy scale: Countries where elections are problematic but where reforms may not be attainable through official channels. Yet, as opposed to harsh dictatorships, protest in this middle range of countries is possible (Kelley, 2008b; see Figure 1).

However, it is important to stress that boycotting is costly and opposition parties have incentives to avoid it, if possible. This is why opposition parties often participate despite unfair electoral conditions. First of all, if they boycott, opposition parties must worry about being misunderstood because incumbents often accuse opposition parties of boycotting because they know they cannot win and thus their only hope is to discredit the regime (Pastor, 1998, 1999). Haitian and U.S. officials used this argument in the 1995 election in Haiti. Such boycotts, called "unfair boycotts," are actually not very common,³ but they generally garner international criticism (Monga, 1997, p. 158). Furthermore, opposition parties have incentives to participate in elections to gain experience and visibility and to gain some parliamentary foothold, even if their share of the vote is diminished by fraud. In the long run many opposition parties therefore calculate that they are better off participating in a system of uninterrupted, repetitive elections (Lindberg, 2006a, pp. 58, 75-76). Thus, in 2005 the spokesman for Zimbabwe's Movement for Democratic Change explained that the party's leaders had decided to drop a boycott, although

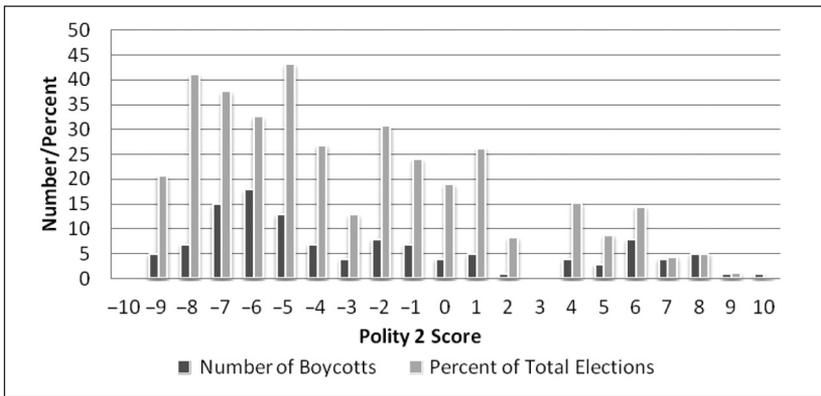


Figure 1. The relationship between boycotts and regime type
Excludes single-party elections.

Mugabe’s government had “failed, and failed dismally” to carry out promised voting changes: “We participate to keep the flames of hope for change alive,” he said.⁴ Thus, unfair electoral conditions do not always engender boycotts, but when boycotts occur, they are usually driven by unfair electoral conditions and are a last resort effort to focus attention on the fraud. Thus, there are both costs and benefits to boycotting and election, and an opposition party will have to weigh these carefully.

Enter Monitors

How does the presence of international monitors alter the calculations of the opposition parties? If, as argued, boycotts are indeed a protest of fraud and an attempt to draw attention to it, then the effect of monitors on boycotts should depend on how the opposition parties expect the monitors to influence the level of actual fraud, or if not, on whether they expect monitors to increase attention to the election fraud, or to overlook fraud either intentionally or unintentionally.

If opposition parties expect, as past research has argued, that monitors cause incumbents to shift to “strategic manipulation,” that is types of fraud that observers are less likely to notice or criticize, then opposition parties will anticipate that monitors will endorse the results despite the fraud. Although opposition parties might have had an incentive to participate in an unmonitored election that it could afterward argue was flawed, endorsements by international monitors will make it even more difficult to convince others that the election was flawed. Thus, opposition parties will be even more likely

to boycott the election altogether to protest not only the fraud but also the anticipated rubber-stamp endorsement. Ergo, the presence of monitors will increase boycotts. This argument is developed in greater depth elsewhere (Beaulieu & Hyde, 2009). The argument is quite plausible because, as Kelley (2009) has recently shown, monitors sometimes do endorse flawed elections. Similarly, Zakaria (1997) criticizes the apparent willingness of Western countries to accept minimal standards.

This argument leads to a set of hypotheses. First, if “blatant” irregularities are defined as those that monitors most likely to criticize, and if incumbents are shifting away from these, then,

Hypothesis 1a (H1a): Blatant forms of cheating should be fairly rare in monitored elections.

The term *rare* is of course imprecise, but if shifting into forms of fraud that monitors are less likely to criticize or notice is quite feasible—an assumption that underlies the argument—then when monitors are present, all incumbents will have strong incentives to do so, and there is little reason to expect widespread blatant fraud to continue since its existence would jeopardize the efforts to deceive the monitors.

Furthermore, strategic manipulation is conceptualized as either covert fraud or types of behavior that monitors are less likely to denounce. By definition, covert fraud cannot be observed, of course, but it is unlikely that incumbents are able to shift all their cheating into covert form. Thus, if “ambiguous” irregularities are defined as those that monitors are more likely to tolerate even if they do call attention to them, then when incumbents shift away from blatant forms of fraud, they should at least partly be shifting into such more ambiguous forms of fraud. Thus,

Hypothesis 1b (H1b): Blatant and ambiguous forms of irregularities should be substitutes, not complements.

Finally, if this shift prompts election boycotts, then,

Hypothesis 1c (H1c): Boycotts should be more likely when international election monitors are present.

An Alternative Argument

Another possibility exists, however. The above argument assumes that opposition parties boycott because they expect monitors to decrease attention to

fraud by providing a false endorsement. But if opposition parties instead expect the presence of monitors to either decrease the actual level of fraud or at least increase attention to fraud, then they should be less, not more, likely to boycott an election.

Several factors may lead the opposition parties to expect that monitors will help, rather than hurt, their cause. Monitors may increase attention to fraud because they make public statements to both the domestic and international media and issue detailed reports about their observations. Furthermore, their organizations have direct links to the international community through their donors or member states. Thus, many of election monitoring organizations have a history of exposing fraud in important elections that have been well publicized, such as the Philippines 1986, Panama 1989, Ukraine 2004, Georgia 2003, Nigeria 1999, Zambia 2001, and others. Indeed, of the monitored elections for which data are available, nearly 18% are condemned by at least one organization. Thus, rather than decrease attention to fraud, opposition parties may expect that there will be greater attention to fraud when monitors are present than when their own boycotts and accusations are the only signals.

Furthermore, opposition parties may also hope that the presence of monitors will actually improve the conduct of the election. If, as argued, international monitors increase the risk of exposure, then the international community will be more likely to notice and therefore also more likely to denounce or hold a fraudulently elected government accountable in various ways. International monitors may also provide domestic actors with an alternative and more credible source of information and increase their confidence in the support of the international community. This may embolden opposition parties. The presence of monitors may therefore lead the incumbent to raise his or her assessment of both the international and domestic cost of cheating and therefore cheat less.

Indeed, recent research has shown that monitors may be associated with less cheating at polling stations visited by monitors (Hyde, 2007), and even with improved overall election quality (Kelley, 2010). Other scholars also suggest that monitoring organizations can increase opposition confidence in the conduct of the election (Anglin, 1998; Chand, 1997; Elklit & Reynolds, 2002; McCoy, Garber, & Pastor, 1991). Monitoring organizations themselves frequently claim that they boost confidence in the election, as has, for example, the EU about Guatemala's 2003 elections (European Commission, 2003, p. 1) and the European Parliament about Congo-Brazzaville's 2002 election (European Parliament, 2002, p. 5).

Monitors could also improve elections by helping opposition parties address their grievances before the election. Organizations often arrive well in advance of the election day and encourage reforms in preparation for the election. For

example, after Russia's 1993 referendum, the International Republican Institute (IRI) reported, "A number of IRI's suggested improvements were adopted by the time of the December 12, 1993 parliamentary elections."⁵ Such changes may deter boycotts. In Azerbaijan's 1998 presidential election, for example, the opposition parties had originally decided to boycott to protest a new electoral law. However, the Organization for Security and Co-operation in Europe mission entered into a long dialogue with the government and the law was changed. This prompted several, though not all, of the opposition parties to abandon their boycott. However, the participation of the Party of National Independence (ANIP) was crucial in giving the election hope of legitimacy. The subsequent manipulation of the election was exposed and actually weakened the popularity of the cheating incumbent, whereas ANIP gained exposure, recognition, and even some legislative foothold (Cornell, 2001, pp. 121-122).

Finally, if opposition parties contemplate an "unfair boycott," monitors may also dampen their resolve. If international monitors are present and willing to testify that the election was fair, then the chance that an opposition boycott is perceived as unfair increases. This in turn raises the cost of such a boycott and therefore makes participation in the election more attractive.

Thus, if opposition parties believe that monitors can improve the quality of the election and help them address their grievances, or if they believe that monitors increase attention to fraud or that monitors will expose them if they boycott unfairly, then they will have less incentive to boycott the election. These arguments thus lead to the following alternative hypothesis:

Hypothesis 2a (H2a): Boycotts are less likely when international election monitors are present.

Importantly, if, as argued, this hypothesis operates because opposition parties believe monitors increase attention to fraud or improve the election conduct, then the quality of the monitoring organization should matter. Kelley (2009) has shown that some types of organizations are more likely to endorse elections and that elections are more likely to be acceptable when credible observers are present (Kelley, 2010). Thus, opposition parties should expect only credible organizations with a record of criticizing fraud to help their cause.

In contrast, opposition parties should not expect that more lenient organizations, those more likely to endorse fraud, will improve the quality of elections, and, if anything, they will expect them to, wittingly or unwittingly, endorse the election despite the fraud. This is particularly harmful for opposition parties

if there are no stricter organizations present to counter their endorsements. Thus, such lenient organizations should not deter boycotts. However, if the opposition parties were not already planning to boycott because they had decided that the benefits of gaining experience and some exposure were worthwhile even in a fraudulent environment, it is not clear whether the opposition parties would decide to boycott merely because one of these lenient organizations was present. If it is obvious that the organization has a dubious record of endorsing elections, then its endorsement may not be credible and therefore less damaging to the opposition's claim of fraud. For example, the fraud in Zimbabwe's elections has been quite clear to the international community and to voters, although Mugabe has been adept at handpicking observer groups that would be favorable to his regime.

Thus, it is important to distinguish among monitoring organizations with different reputations. If monitors are able to deter boycotts, this relationship may hold only for credible organizations. This leads to the following complementary hypotheses:

Hypothesis 2b (H2b): Less credible international monitoring organizations have little or no effect on boycotts.

Hypothesis 2c (H2c): When credible international monitoring organizations are present, boycotts are less likely.

Last, it is important to note that these reputation-based hypotheses emerge because they assume that opposition decides to boycott based on their expectations about the ability of monitors to attract attention to or to reduce fraud. However, if, as the initial argument (H1a, H1b, and H1c) posited, the opposition decision is instead predicated on expectations that monitors will prompt incumbents to change how they cheat to evade criticism, then the quality of the monitoring organization is less important. This is because the idea of changing the method of cheating presumably is to solicit endorsements from credible organizations; lenient organizations need not be manipulated in this way as they can simply be counted on to be favorable regardless. Thus, credible organizations are exactly the ones most expected to increase boycotts, which is of course the opposite of H2c.

Examining the Pattern of Irregularities

To examine H1a and H1b it is necessary to use micro-level data on different types of irregularities and see whether the data patterns fit the expectations. It is important to recall that the term *blatant* is used here not to denote how

observable the irregularities are but to denote those types of cheating that are most likely to lead monitors to denounce the election. The term *ambiguous* is used to denote those irregularities that monitors observe but for various reasons are more likely to tolerate, even if they do complain about them.

To capture blatant cheating, this project draws on the Quality of Elections Dataset (QED),⁶ which codes *pre-election cheating*, and *election-day cheating*. These are conceptualized as the sorts of behaviors that are difficult for international monitors to dismiss as anything other than efforts to manipulate the results. They include things such as abuse of government resources, gross vote tabulation errors, intimidation of voters, and restrictions on media and media biases, which international observers routinely monitor. QED also codes problems in the legal framework such as restrictions on franchise or the scope of the elected offices, which monitoring organizations are less likely to tolerate. Importantly, *pre-election cheating*, *election-day cheating*, and major *legal framework problems* are the same variables that Kelley (2009) found make monitors more likely to denounce elections. That is, they are exactly the kinds of behaviors a savvy government would want to avoid.

Strategic forms of manipulation may include some behaviors that are unobservable, but, by and large, most of the forms of “strategic manipulation” enumerated in earlier work (Beaulieu & Hyde, 2009) are actually among the types of problems commonly scrutinized by international observers. Thus, monitoring organizations increasingly collect information on problems in the voters’ lists, complaints about the conduct of the electoral commission, poor information provision or technical problems during the pre-election period, partisan stacking of the electoral commission, problems with voter information and materials, political accusations of crimes, restrictions on nonpartisan observers, and so on. Thus, to capture these irregularities, this project again draws on the QED, which codes administrative problems. These are conceptualized as the irregularities that could possibly be put down to administrative inexperience or poor capacity or a lag in institutional development. These variables too are recorded separately for the pre-election period and election day. Underscoring that these are indeed the types of behaviors that observers are more ambiguous about, Kelley (2009) found that these variables were not predictors of monitor’s assessments. These variables, *pre-election cheating*, *election-day cheating*, *pre-election administrative problems*, *election-day administrative problems*, and *legal framework problems*, are all described in further detail on the project website (www.duke.edu/web/diem). The variables are available in two different data sets that vary not in the conceptualization of the variables but in the sources on which they draw. The data used by Kelley (2009) are based on reports by international election monitors. The

QED is based on U.S. State Department Reports on Human Rights Practices. The present project uses the latter because it is available for monitored as well as nonmonitored elections and is therefore suited to exploring how the presence of monitors may influence the pattern of irregularities. All the variables are coded from 0 to 3, representing *none*, *minor*, *moderate*, and *major* levels of problems.

Before exploring the pattern of these irregularities, however, it is necessary to first consider the data on boycotts and monitoring. Monitoring is defined as the presence of a formal monitoring delegation from an international agency, not counting small national delegations or local embassy activities, because they are ubiquitous and their observations usually remain internal. Missions of pure technical assistance (such as the Organization of American States (OAS) mission in Argentina in 2003 or CAPEL missions more generally)⁷ are also not considered monitoring events. The *all monitors* variable is a binary indicator. The variable covers the years 1975 to 2004 and is based on documents from the 21 organizations most commonly involved in monitoring either presently or historically. To ensure that this variable was as complete as possible, it was often necessary to communicate directly with each organization for verification because final reports were often unavailable and websites would provide incomplete information (such as when organizations sent only pre-election delegations but listed this as a monitored event). In addition, every election in the data set was searched in the LexisNexis database for additional information. More information can be found at the project website (www.duke.edu/web/diem).

Following Beaulieu and Hyde (2009, p. 396), *boycotts* are defined as decision by a political party not to participate in the elections. The boycott must be carried out, not just threatened. Boycotts were coded based the *Keesing's Record of World Events* and news sources in the LexisNexis database. These sources were supplemented by data from Bratton (1998), the U.S. State Department Reports on Human Rights Practices, Lindberg (2006b), and articles in the *Journal of Democracy*. This research uncovered 123 boycotts in 1,111 national-level multiparty elections between 1975 and 2004 (see the appendix).

It is now possible to examine H1a and H1b. H1a states that blatant cheating should be rare in monitored elections. However, this is not the case. International monitors still discover many rudimentary election irregularities: Ballot stuffing, intimidation, and media restrictions are still rampant forms of “winning” elections. Major problems with the electoral framework occur in only about 5.5% of monitored elections, so these egregious problems are fairly rare, but that cannot be said of other forms of cheating: Of the 419 monitored elections for which there were detailed data on the types of irregularities,

nearly 27% contained major levels of cheating either before or during the election, or both. If it were so simple to cheat subtly, these crude behaviors would desist.

H1b states that *blatant and ambiguous forms of irregularities should be substitutes, not complements*. It is clearly possible to find individual cases that would corroborate such a shift. Surely some cheating parties seek to hide their behaviors (Carothers, 1997); some incumbents try to manipulate even the monitors. In 2005 Kazakhstan's government reportedly went so far as to conduct intelligence operations against the election monitors (Chivers, 2007). However, a more systematic examination finds no evidence that there is a systematic shift from blatant to more ambiguous forms of fraud that can be attributed to monitors. On the contrary, ambiguous and blatant forms of cheating correlate highly: Elections with major administrative irregularities—those where politicians may be using these safer irregularities to evade criticism—also had major blatant irregularities a full two thirds of the time.

Nor is there systematic evidence of a shift within countries from one election to the next or, importantly, that any such shift is more pronounced in monitored elections. If one ignores all the cases where the pattern of cheating remained unchanged from one election to another and focuses on only the elections where the patterns of irregularities changed, it becomes apparent that when countries decreased overt cheating from the level used in previous elections, they most commonly did not increase safer, more ambiguous, administrative irregularities from the level used in the previous election (see Figure 2). On the contrary, most often they committed the same level of administrative irregularities or they improved. Importantly, the pattern differed little between monitored and non-monitored elections. If anything, countries that reduced their overt cheating reduced their administrative problems more often when international monitors were present. A similar pattern occurs if one examines a decrease in overt legal problems (see Figure 3), although here administrative problems are slightly more likely to both decrease and increase with monitors present. But in any case, the percentage of cases that shift from blatant legal problems into administrative irregularities is smaller than those who also decrease their administrative irregularities, and in absolute figures the phenomenon is even less likely to be driving the pattern of boycotts seen.

It is possible to consider other shifts as well depending on how one conceptualizes blatant versus ambiguous irregularities. However, the patterns for shifting from the election day to the pre-election period or from violence to administrative irregularities are similar: They do not suggest that monitors induce a strategic shift.⁸ Rather, when it comes to election fraud, it appears that “bad things go together.” Ambiguous and blatant irregularities appear to complement rather than substitute for each other.

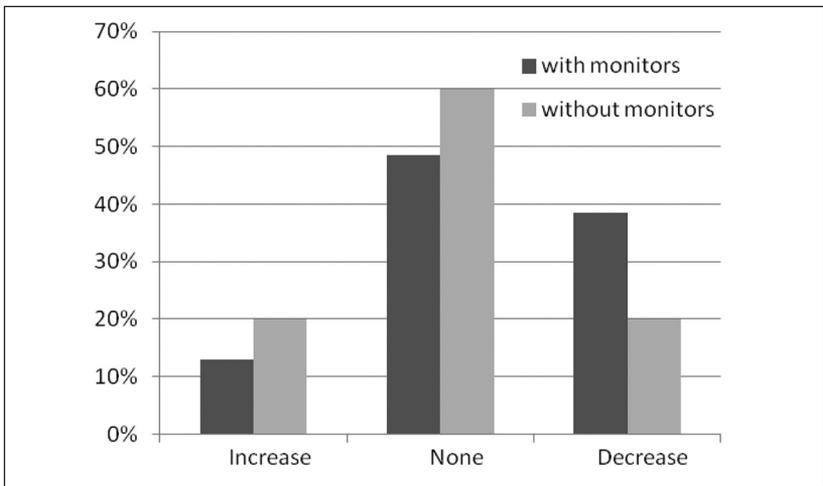


Figure 2. Changes in administrative irregularities when blatant cheating decreases
Source: Quality of Elections Dataset, $N = 100$.

Excludes single-party elections and elections in countries rated “free” by Freedom House.

Data on different types of irregularities are difficult to both collect and code, however, and if indeed a considerable part of the behavior is into truly covert irregularities, then the above examination falls short. Thus, the lack of evidence for a strategic shift in the type of irregularities does not prove that a shift does not occur, nor, for that matter, that monitors do not increase boycotts, as the initial data exploration and H1c suggest. The remainder of this article therefore examines the relationship between monitors and boycotts more directly.

The Relationship Between Monitors and Boycotts

To examine whether the presence of monitors is associated with a greater or lesser likelihood of boycotts, it is vital to address the fact that, as discussed earlier, monitoring organizations tend to go to elections in the middle of the democracy spectrum. Unfortunately, from the perspective of causal analysis, these are also the elections that are most prone to boycotts because the opposition parties often will have cause to protest and because the regime may tolerate some such protest. Thus, earlier work by Beaulieu found that boycotts predicted the presence of monitors in future elections (Beaulieu, 2006, p. 145).

Most traditional methods to correct for selection require a variable that can be used as an instrument—a variable that is correlated with the independent

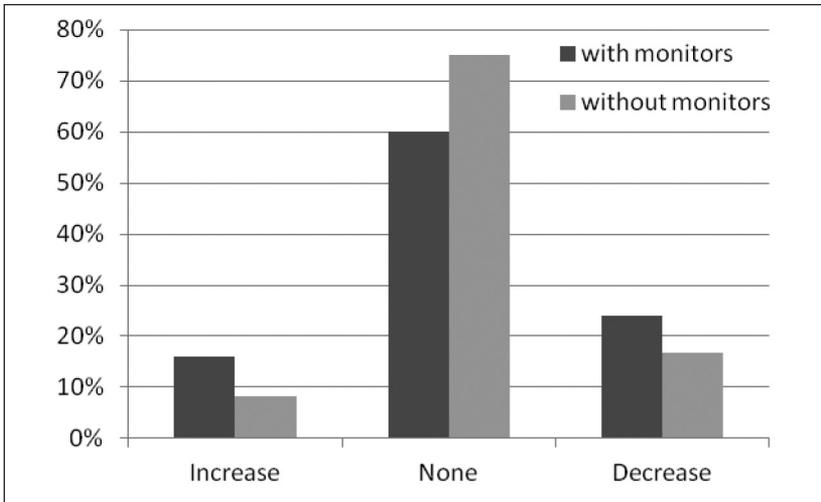


Figure 3. Changes in administrative irregularities when blatant legal problems decrease

Source: Quality of Elections Dataset, $N = 74$.

Excludes single-party elections and elections in countries rated “free” by Freedom House.

variable of interest but is not theorized to have any effect on the dependent variable. Such variables are rare and, in the given context, not available. This analysis therefore uses matching to modify the observational data so that they approximate experimental data, which randomly assign observed units to treatment and control groups.⁹ Thus, matching attempts to produce a data set where the values of control variables do not predict selection into the treatment, or more formally,

$$\rho(X|T = 0) = \rho(X|T = 1),$$

where ρ is the *observed* probability, X is a matrix of control variables, and T is the treatment state. Observations that cannot be matched well are discarded, as they may otherwise bias the analysis. The method to accomplish the matching here is genetic matching. Rather than matching techniques based on propensity scores, genetic matching basically uses a computer program to automatically search through high numbers of matched samples and, through iteration, produce two sets of observations that—with the notable exception that one group was monitored and the other was not—are as alike as possible on a set of other given variables that could possibly influence the likelihood

of a boycott. This preprocessing of the data in turn reduces the probability that the value of any (omitted or observed) control variable is correlated with the presence of monitors in the analyzed sample. The better the data are matched, the lower the probability. In the case of the matching employed for the present analysis, the postmatching balance in the data is good, as shown in the standardized balance scores in Table 1.¹⁰

Importantly, matching is not used to estimate variables' effects but only to preprocess data so that they gain some of the advantages of experimental data. If only a few, discrete variables are used to match, then exact matching may be possible, and then a simple difference of means test is sufficient to estimate the causal effect of the treatment. However, when exact matching is not possible but the balance between the treatment and control group is substantially improved, then completing standard regression analysis on the matched data set can decrease bias by making estimation of the treatment effect conditional on the values of control variables (Rubin & Thomas, 1996). In addition to doing such a regression analysis on the matched data, a simple two-sample test for equality of proportions with continuity correction was also done. This is basically a test that compares the mean occurrence of boycotts in the monitored and nonmonitored samples after matching.

In this analysis, several variables are used for matching and for the post-matching analysis. First, as argued earlier, opposition boycotts are primarily driven by concern about a wide range of fraud. However, it is problematic to use measures that include the conduct of the election day because the boycott decision is made prior (Schedler, 2002, p. 113). Furthermore, final assessments of the election's quality may be tainted by the existence of a boycott. This conundrum has plagued previous studies because separate pre-election environment measures have not been available (Beaulieu, 2006, pp. 18-19). To address it, this study uses the three aforementioned factors from the QED that capture the pre-election environment: legal problems, pre-election administration, pre-election cheating. As also discussed, violence has also been found to predict boycotts. This study therefore uses another measure from the QED called *pre-election violence*, which also is coded on the same scale as the other variables.

Some countries display a long history of boycotts, which may create a political culture, and statistical analysis also shows that the presence of a boycott in a previous election makes a boycott in the next election more likely (Beaulieu, 2006). Thus a variable, *prior boycott*, was included to capture whether any parties had boycotted the previous election. A *year* variable was included because both boycotts and monitoring display definite time trends that also coincide with global trends in democracy. Because single-party states and established democracies are excluded from the analysis, the data are matched simply on

Table 1. Determinants of Boycotts

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8	Model 9
All monitors	-0.659* (0.339)	-0.639 (0.440)	-1.211** (0.424)	-0.609 (0.470)	-0.028 (0.644)	-1.101 (0.677)	-1.290** (0.426)	-1.643** (0.546)	-1.821*** (0.536)
Other monitors									
Quality monitors									
Boycott, prior election	0.861* (0.336)	0.743 (0.433)	0.538 (0.397)	0.644 (0.564)	0.968 (0.785)	0.361 (0.727)	1.040* (0.470)	0.986 (0.565)	0.512 (0.562)
Legal problems	[0.010]	[0.026]	[0.024]	[0.000]	[0.000]	[0.000]	[0.000]	[0.023]	[0.058]
	0.129	0.549*	0.530*	0.041	0.648	0.745	0.089	0.794*	0.450
	(0.178)	(0.228)	(0.213)	(0.278)	(0.396)	(0.395)	(0.245)	(0.326)	(0.298)
Pre-election cheating	[0.021]	[0.063]	[0.022]	[0.000]	[0.000]	[0.129]	[0.096]	[0.035]	[0.025]
	0.443**	0.187	0.516**	0.549*	0.740*	1.241**	0.705***	-0.066	0.624*
	(0.160)	(0.210)	(0.185)	(0.260)	(0.353)	(0.429)	(0.209)	(0.292)	(0.247)
Pre-election administrative problems	[0.097]	[0.082]	[-0.014]	[0.011]	[-0.025]	[-0.183]	[0.068]	[0.064]	[0.000]
	0.493*	0.584*	0.116	1.213**	1.845***	1.815**	0.273	0.042	-0.210
	(0.221)	(0.266)	(0.271)	(0.409)	(0.506)	(0.552)	(0.271)	(0.337)	(0.354)
Pre-election violence	[0.013]	[0.030]	[0.138]	[0.000]	[0.024]	[-0.248]	[0.019]	[0.036]	[0.064]
	0.351**	0.308*	0.101	0.473*	0.038	0.019	0.745***	0.852***	0.407
	(0.124)	(0.153)	(0.143)	(0.199)	(0.295)	(0.290)	(0.185)	(0.226)	(0.209)
Freedom	[0.013]	[0.071]	[0.004]	[-0.010]	[0.036]	[-0.068]	[0.029]	[0.057]	[0.024]
	-0.162	-0.239	-0.335	-0.173	0.488	1.648	-0.099	-0.126	-1.187*
	(0.353)	(0.445)	(0.417)	(0.564)	(0.795)	(1.014)	(0.477)	(0.680)	(0.584)
House, 1-year lag	[-0.063]	[0.000]	[-0.024]	[0.000]	[0.093]	[-0.131]	[-0.058]	[-0.044]	[0.000]
Year (centered)	-0.023 (0.029)	-0.006 (0.039)	-0.002 (0.043)	0.017 (0.048)	-0.066 (0.071)	-0.048 (0.075)	-0.010 (0.037)	0.000 (0.048)	0.022 (0.056)

(continued)

Table 1. (continued)

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8	Model 9
Africa	2.060 (1.055) [-0.008]	1.235* (0.540) [-0.020]		2.636* (1.115) [0.000]	1.391 (1.384) [-0.035]		0.556 (1.166) [-0.015]	0.867 (0.620) [-0.019]	
Latin America	1.302 (1.114) [0.000]	1.510* (0.733) [0.000]		— — [0.000]	— — [0.000]		0.138 (1.192) [-0.028]	1.158 (0.814) [-0.018]	
Asia	0.744 (1.095) [0.000]	— — [0.000]		— — [0.000]	— — [0.000]		-0.547 (1.184) [0.000]	— — [0.037]	
Ln GDP, 1-year lag		-0.369 (0.267) [-0.040]	-0.855*** (0.232) [-0.079]	[0.000]	-0.393 (0.395) [-0.102]	-1.235** (0.433) [-0.114]		-0.953* (0.463) [-0.018]	-0.956** (0.331) [0.011]
Opposition strength, 1-year lag		1.471 (1.114) [0.032]		4.175 (2.199) [0.185]				2.244 (1.519) [0.024]	
Government stability, 1-year lag			-0.130 (0.084) [0.075]			-0.205 (0.144) [0.080]			-0.192 (0.117) [-0.020]
Constant	-3.819*** (1.140) 411	-2.262 (2.078) 334	4.800** (1.736) 284	-4.435** (1.375) 221	-6.091 (3.516) 183	5.325 (3.072) 153	-3.443** (1.208) 326	1.170 (2.919) 264	6.120* (2.554) 220
Unmatched N	352	269	254	140	104	95	245	179	181
Matched N	369.62	256.25	279.54	178.68	120.58	128.84		166.72	179.35
Null deviance	286.13	191.47	206.86	122.84	73.65	78.55		111.31	117.65
Residual deviance									

All models exclude single-party elections and elections in countries rated free by Freedom House the year before the election. Reporting is as follows: coefficient estimate, standard error in parentheses; standardized balance in brackets; (mean_t - mean_{t-1}) / sd.
 a. Dropped because of quasi-perfect prediction.
 p* < .1. *p* < .05. ****p* < .01.

whether Freedom House rated a country was “not free” or “partly free” in the year before the election. The model also includes a measure of GDP, which is a predictor of both monitoring and boycotts in previous studies (Beaulieu & Hyde, 2009; Kelley, 2008b). The variable *Ln GDP lag* is the 1-year lag of the natural log of GDP (UN measure for GDP per capita at current prices in U.S. dollars).

Past analysis has been mixed about finding a relationship between the strength of the political opposition and boycotts (Beaulieu, 2006; Beaulieu & Hyde, 2009). Thus, this analysis includes a measure used also by Beaulieu (2006). This variable, *strength of political opposition*, is equal to 1 minus the *fraction of seats held by the government*, a variable from the World Bank Dataset on Political Institutions (Beck, Clarke, Groff, Keefer, & Walsh, 2001). Because of concerns about missing data, as a robustness check, the country risk group variable of *government stability* was also used (Political Risk Group, n.d.).

Finally, as noted earlier, boycotts are largely an African affair: A full two thirds of all boycotts occur in Africa. In Africa the rate of boycotts in multiparty elections is 30%. The other regions with a high prevalence of boycotts are Asia, with a rate of 8%, and Latin America, with a rate of 6%. In the rest of the world the incidence is less than 2%. The appendix totals the number of boycotts in each region. For this reason the analysis includes regional indicator variables for *Asia*, *Latin America*, and *Africa*. It may also be important to include these regional variables because the different monitoring organizations cover different regions. Some of the more credible organizations operate only in Europe, Asia, or Latin America. Although organizations such as the Carter Center, the IRI, and the NDI do monitor elections in Africa, it is notable that some of the less credible organizations operate only in Africa, where boycotts are so prevalent. Including regional indicators is therefore also important to prevent the variable for quality monitors from capturing simply regional patterns.

Matching on all these variables, especially the pre-election problems and the prior boycotts, reduces the need to match closely on a lot of related variables. Indeed, a model with only these two variables correctly classifies whether or not there was a boycott in more than 80% of all multiparty elections in nonestablished democracies, a fact that reinforces the primacy in domestic factors in driving boycotts. Furthermore, logistic regression on nonmatched data suggested that it was not necessary to match on other election characteristics such as whether an election was a first multiparty election or the first election after a coup or a conflict because these were not statistically significant predictors of boycotts. For the same reason, although Beaulieu (2006) suggests

that aid recipients may be sensitive to boycotts, preanalysis found it unnecessary to match on levels of foreign aid.

Having discussed the variables, it is now possible to move to the modeling. The unit of analysis is all direct legislative and executive elections. Data availability limits the analysis to elections after 1977. Single-party states by definition do not have opposition parties, and boycotts are exceedingly rare in established democracies: Only eight boycotts occurred in the 560 elections in established democracies in the data. Consistent with prior research, and because the causal mechanisms are expected to operate only in these cases, the analysis is therefore limited to multiparty states that are not yet established democracies. To include as many countries as possible, the very comprehensive Freedom House data are used, and established democracies are defined as countries rated “free” in the year before the election (Freedom House, 2010). This leaves 561 elections available for the matching process, 115 of which were boycotted and 335 of which were monitored.

For each of the models, the observations were first matched on the noted variables, and then to estimate the coefficient on the monitoring treatment, a logit model was run including the matched variables plus the year variable (which is too demanding to match on but nevertheless should be controlled for).

Models 1-3 in Table 1 report the results of the logit regression after matching. The models also indicate how many observations were included in the matched data, and the standardized balance scores show how good the matching was. The analysis supports H2a rather than H1c. That is, Models 1-3 suggest that the presence of monitors is associated with fewer, not more, boycotts. The coefficient on monitors is statistically significant at the .05 or .01 level in two of the three models. The simple two-sample test for equality of proportions with continuity correction shows that for Model 1, boycotts occurred in 29.33% of nonmonitored elections, whereas they occurred in only 18.80% of monitored elections. This is a reduction in likelihood of $(29.33 - 18.80) / 29.33 = 35.90\%$. For Model 3 the reduction was 45.77%. These two tests were both significant at the .05 level.

The Quality of Monitoring Organizations

The last step is to consider whether the quality of the monitoring organization matters. To do this, it is necessary to distinguish between more and less credible organizations. This is difficult: A simple examination of how often each organization criticizes elections is misleading because organizations cover elections of varying quality. This study therefore developed a rough, but simple, coding rule based on the assumption that the central trait domestic opposition parties

care about in a monitoring organization is how likely it is that the organization will criticize a fraudulent election. Using overall evaluations of election quality, an election was coded as fraudulent if either the U.S. State Department report or any observation mission present considered it unacceptable.¹¹ Based on this coding, an organization was coded as reputable if it criticized at least half of the fraudulent elections that it monitored.

Thus, the following organizations were coded as reputable: the Carter Center, the NDI, the Asian Network for Free Elections, the IRI, the Organization for Security and Cooperation in Europe, the European Parliament, the European Commission, and the Organization of American States.¹² The variable, *quality monitors*, indicates an election in which at least one of these organizations was present. *Other monitors* indicates when the only presence in an election was by one of the remaining organizations.

Matching is suited to analyzing binary treatments such as the presence of monitors. Therefore, when there are multiple treatment levels such as monitors with different levels of credibility, the best approach is simply to compare elections with each type of monitors to cases where no monitors were present. The analysis therefore first examined H2b by comparing elections with low-quality monitors to nonmonitored elections and then afterward examined H2c by comparing elections with high-quality monitors to nonmonitored elections. In both cases the analysis followed the method described earlier of first using genetic matching and then conducting a logit analysis with the matching variables as well as the year variable.

Consistent with H2b, Models 4-6 show that the presence of monitors other than quality monitors is not statistically associated with the level of boycotts. The two-sample test for equality of proportions showed similar results. In contrast, consistent with H2c, Models 7-9 show that the presence of quality monitors is associated with fewer boycotts. The coefficient on high-quality monitors is statistically significant at the .01 or .001 level in all three models. The simple two-sample test for equality of proportions with continuity correction shows that for Model 4, boycotts occurred in 27.98% of nonmonitored elections, whereas they occurred in 13.58% of monitored elections. This is a reduction of $(27.98 - 13.58) / 27.98 = 51.46\%$. For Model 6 the reduction was 60.87%. These two tests were significant at the .01 and .05 levels, respectively.

However, although the coefficient on high-quality monitors is greater than that on all monitors, one should beware of drawing conclusions from the comparison because the treatment group (the monitored elections) varies and is therefore compared to different control groups. Thus, the counterfactual in the models is different. What one can conclude, however, is that low-quality monitors, when compared to a balanced control group, do not appear to influence

boycotts. In contrast, more credible monitors are associated with significantly lower likelihood of boycotts.¹³

Conclusion

As international actors are increasingly engaged within states, it is important to consider their influence on domestic politics, particularly if, as in this case, the data suggest that they may have unintended effects. This article has examined the relationship between international monitors and boycotts. *Prima facie*, this relationship is positive, leading scholars to suggest that monitors actually lead opposition parties to boycott elections because they cause incumbents to shift to forms of fraud that monitors are less likely to detect or criticize. Expecting that monitors will endorse the elections despite the fraud, opposition parties therefore boycott in protest.

However, this article has found little support for this argument. Using new and more detailed data on election irregularities than have previously been available, the article has shown that ambiguous and blatant forms of fraud tend to be complements, not substitutes. That is, although politicians who cheat prefer to hide their actions, few manage to do so. Most of the countries that use more ambiguous forms of irregularities also cheat quite blatantly. Thus, blatant cheating has persisted, even with the advent of monitors.

A few organizations, such as the African Union (AU), which is rarely critical, may actually be associated with a pattern of endorsement of boycotts that is consistent with the argument that opposition parties boycott elections in anticipation of a rubber stamp by monitors. When the AU monitored an election but other monitors were also present to provide alternative assessments, boycotts occurred in roughly a quarter of cases. This in itself is a high rate, but it is not unusual given that about 60% of all boycotts occur in Africa where the AU operates. However, when the AU was alone, with no other organizations to counter its statements, boycotts occurred in about two thirds of elections. Moreover, the boycotts occurred in elections where the AU endorsed the election despite fraud. In the 10 boycotted elections when the AU operated alone, the U.S. State Department Reports on Human Rights Practices called 6 of these elections fraudulent, but the AU condemned only 1 of these.¹⁴ Still, this pattern accounts for only a small portion of the observed boycotts and applies to only a few monitoring organizations. Furthermore, the opposition may not have boycotted because they anticipated that the AU would endorse the election, but simply because the elections were fraudulent. Certainly, the irregularities in these elections were rather blatant.

Indeed, it is important to stress that boycotts are primarily driven not by external actors but by fraudulent electoral environments. The initial positive correlation between monitoring and boycotts probably is because monitors tend to go to countries where election boycotts more likely to occur. Thus, most of the time it is election fraud and violence—not monitors—that motivates boycotts. This article has been the first to use data on fraud in the pre-election period to model boycotts. Past studies have had to rely on measures of democracy to proxy for election quality, but these measures include events on the election day itself, which the boycott decision precedes. Furthermore, these democracy measures incorporate the boycott event into their coding scheme, making the use of these measures tautological.

When using these pre-election measures, and using genetic matching techniques designed to decrease selection problems in observational data, the analysis finds support for quite a different argument: Not only do monitors not increase boycotts, they appear to decrease them. However, the reaction of opposition parties depends on what they expect the international monitors to do, and this in turn depends on the reputation of the organizations. Opposition parties have reasons to expect that when quality monitors are present, they may improve the election itself or call greater attention to the fraud and also make it less attractive to undertake “unfair” boycotts. Thus, although international monitors generally do not meddle in boycott decisions, the presence and activities of quality organizations can indirectly boost the confidence of opposition parties. The presence of international monitors, especially quality organizations, thus makes it more appealing for opposition parties to participate in election to gain some experience and some visibility and perhaps even some legislative foothold.

This would explain why Carothers (1997, p. 20) argues that the opposition withheld a boycott in 1996 in the Dominican Republic because the president provided the assurance of international observers. It also explains why opposition parties are sometimes strong proponents of international monitoring, as in Guyana and Zambia in the early 1990s. In Guyana, after the fraudulent election in 1985, which was not monitored by international observers, the opposition parties formed the Patriotic Coalition for Democracy (PCD) to demand reforms and international observers for the next election. In the fall of 1990 the PCD threatened a boycott. Eventually, however, several reforms were made, and the commonwealth secretariat was invited to observe the election. However, the opposition parties were uncertain about the impartiality of the commonwealth mission and pressured the government to invite the Council of Freely Elected Head of State (the predecessor to the Carter Center). The council made several pre-election visits and worked to promote electoral

reforms. The boycott was abandoned and the election was considered of good quality (Carter Center, 1992, pp. 17-18).

In Zambia in 1991, president Kaunda gave into opposition demands for elections but initially refused international observers. However, after years of one-party rule, the opposition party, Movement for a Multiparty Democracy (MMD), was so mistrustful of the government that despite the presence of some new domestic monitoring organizations, MMD actually made it a condition for its participation that international observers would be invited (Bjornlund, Bratton, & Gibson, 1992, p. 408). Thus, the party clearly had expectations that international observers would help their cause, and the elections were indeed praised by international observers.

More recent examples can also be found. For example, after the 2008 vote in Zimbabwe, the opposition Movement for Democratic Change insisted it had won the presidency and declared that it would boycott a second round of the election, even if this would for certain hand the victory to Mugabe. However, it said it would take part in a runoff if international observers were allowed to monitor the election ("Crisis? What Crisis?" 2008). Given that no credible observers were invited, its candidate subsequently withdrew. The 2005 Ethiopian election provides a more positive outcome, however. After the government had banned international observers from the 2000 election, opposition parties demanded international observers for the 2005 election. The government averted a possible boycott by giving into their demands (Addis Tribune, 2004). Finally, when Ghana's National Democratic Congress (NDC) was contemplating a boycott in 2004, the party also noted the importance of international observers (Ghanaian Chronicle, 2004). In the event, the election was monitored and the NDC participated.

The analysis in this article has been rigorous. It has analyzed patterns of irregularities that would be expected to hold if monitors caused incumbents to switch into less blatant types of fraud. It has introduced new and more appropriate data about the pre-election period and the types of monitoring organizations. Finally, it has addressed the central selection problem by using matching to preprocess the data to reduce bias. Thus, there is good reason to think that the findings in this article are not spurious. The presence of international monitors, especially quality monitors, is associated with a lower likelihood of boycotts. The evidence does not show this for more lenient organizations, however.

Parties that boycott elections miss out on valuable political experience and forfeit potential legislative participation and a formal voice in domestic debate. Were international election monitors to increase the presence of boycotts, this would be a serious result that was not only unintended but also

antithetical to the mission of the election monitors. Instead, this analysis has suggested monitors can indeed decrease boycotts, which is more in line with their mission. This not only is reassuring for those in the democracy promotion community but also provides an interesting piece of evidence that international actors can also exert positive influences on the domestic politics. Finally, as scholars increasingly study how international actors influence domestic politics, it stresses the importance of differentiating between different types and reputations of international actors.

Appendix

Boycotts in Multiparty States Between 1975 and 2004, by Region

Country	Years and election types	Region total
Africa		74
Algeria	1995 P 1999 P 2002 L 2004 P	
Benin	2001 P	
Burkina Faso	1991 P 1998 P	
Cameroon	1992 P 1997 P	
Central African Republic	1992 P	
Cape Verde Islands	1996 P	
Chad	1996 P 2002 L	
Comoro Islands	1992 L 1996 L 1996 P 2002 P	
Congo	1993 L	
Cote d'Ivoire	1990 L 1990 P 1995 P 2000 L	
Djibouti	1992 S 1993 P 1997 L	
Egypt	1990 L	
Equatorial Guinea	1993 L 1996 P 1999 L 2002 P 2004 L	
Ethiopia	1992 L 1994 L 1995 L 2000 L	
Gabon	1990 L 1996 L 2001 L	
Gambia	1996 P 2002 L	
Ghana	1992 L	
Guinea	1993 P 1995 L 2002 L 2003 P	
Madagascar	2002 L	
Mali	1997 S	
Mauritania	1992 L 1996 L 1997 P	
Mauritius	1976 L	
Morocco	2002 L	
Mozambique	1994 S	
Niger	1996 P 1996 L	
Nigeria	1992 L 1993 P 1999 L	
Senegal	1978 S	
South Africa	1984 L	

(continued)

Appendix (continued)

Country	Years and election types	Region total
Sudan	1996 S 2000 S	
Tanzania	2000 S	
Togo	1993 P 1994 L 1999 L 2002 L 2003 P	
Tunisia	1986 L 2004 S	
Zambia	1996 L	
Zimbabwe	1979 L 1995 L 1996 P	
Asia and Middle East		24
Azerbaijan	1993 P 1998 P 2000 L	
Bahrain	2002 L	
Bangladesh	1986 L 1986 P 1988 L 1996 L	
India	1991 L	
Indonesia	1997 L	
Jordan	1997 L	
Lebanon	1992 L 1996 L	
Nepal	1981 L 1986 L	
Pakistan	1993 L	
Philippines	1981 P 1984 L 1986 P	
Syria	2003 L	
Tajikistan	1994 P 1995 L 1999 P	
Yemen	1997 L	
Latin America		14
El Salvador	1976 L 1978 L 1988 L 1989 P	
Guyana	2001 L	
Haiti	1988 P 1995 L 2000 P	
Jamaica	1983 L	
Nicaragua	1984 S	
Paraguay	1983 S 1988 S	
Peru	1992 L 2000 S	
Other		12
Albania	1996 L	
Belarus	2000 L	
Georgia	2000 P	
Macedonia	1994 S 1999 P	
Serbia	1992 L 1992 L 1997 S 1997 P 2003 L	
Turkey	1987 L	
Grand total		123

P = presidential; L = legislative; S = simultaneous.

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Notes

1. The opposition boycotted 16.09% of the 435 monitored elections, but only 7.84% of the 676 nonmonitored elections.
2. As Pastor (1998), a pioneer of election monitoring, argues, “The opposition boycotts not out of fear of losing but because they believe no one is listening” (p. 161).
3. In Africa between 1990 and 2003 a full boycott of a free and fair election happened only once, in Ghana in 1992, when after contested executive elections the opposition boycotted parliamentary elections, which turned out to be considered free and fair (Lindberg, 2006a). Yet even this instance is disputed (Ofori, 1993).
4. See “Zimbabwe’s Opposition” (2005).
5. See International Republican Institute (1996, p. 2).
6. See www.duke.edu/web/diem. for more information. See also Kelley and Kolev, 2010.
7. El Centro de Asesoría y Promoción Electoral (CAPEL) operates in Latin America.
8. These patterns are examined in greater depth elsewhere (Kelley, 2012, Chapter 5).
9. For an overview of genetic matching see Diamond and Sekhon (2008), Ho, Imai, King, and Stuart (2007), Mebane and Sekhon (1998), and Sekhon and Grieve (2008). The matching for this section was done using a genetic matching search algorithm developed for the statistical computing language *R* in the package *Matching*. For the write-up of the software package, see Sekhon (2007).
10. The weakest matching model is Model 6. The best is Model 4, which achieves near perfect matching. Models 3, 6, and 9, the last in each table, were run both with and without regional indicators, and the coefficient on monitoring did not differ greatly. However, the balance achieved on other variables, when including the area indicators, was less good than that achieved without the regional indicators. Thus, the tables report the models without the regional indicators.
11. The variables were taken from data sets in the Quality of Elections Dataset as well as the Data on International Election Monitoring (DIEM), discussed on the project website (www.duke.edu/web/diem).

12. Although none of these organizations have been immune from criticism, a review of secondary articles as well as interviews with practitioners concurs that these organizations are considered reputable. Nevertheless, there have of course been elections where even these organizations failed to delegitimize the results, such as the elections in Kenya in 1992, in Cambodia in 1998, and in Bosnia-Herzegovina in 1996. A few other organizations also enjoy decent reputations, but—as the United Nations—rarely issue public criticism, or—as the Council of Europe (COE)—often are ambiguous. However, because the COE appears only twice without some other quality organization, the omission of the COE from this list has few practical implications for the analysis.
13. These findings are robust in unmatched logistic analysis and analysis restricted to post-1989.
14. The U.S. State Department criticized the boycotted elections in Ethiopia 1992, Gabon 2001, Mauritania 1996, Sudan 1996 and 2000, and Togo 1993, 1994, and 2001. However, the African Union forthrightly criticized only the election in Gabon in 2001, but remained ambiguous or even encouraging about the others.

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Bio

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