

**Randomizing International Election Observation:  
The 2004 Presidential Elections in Indonesia\***

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**DRAFT**

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**Abstract**

Democracy promotion activities represent an overt attempt by international actors to change the course of domestic politics, yet the effects of democracy promotion efforts remain poorly understood. Using field experimental methods, I test whether international election monitors influence election quality. By randomly assigning international observers to locations for election day monitoring, all other variables are held constant, and any difference between observed and unobserved groups can be attributed causally to international observers. I present the results from the first study in which international observers were randomly assigned, and examine whether observers from the Carter Center had a measurable influence of voting behavior in the 2004 presidential elections in Indonesia.

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**Note to Readers: This paper is mid-revision, and comments are particularly welcome pertaining to the methods of analysis.**

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During the 1990s, international democracy promotion grew into a booming industry. Aid spent directly on democracy promotion activities (not including indirect activities like aid conditionality) represented more than \$500 million annually in the US alone (Carothers 1999).<sup>1</sup> Democratization—usually thought of as a purely domestic political process—since the end of the Cold War has become permeated with international actors (Burnell 2000; Gleditsch 2000; Pevehouse 2003; Whitehead 1996). This development represents an overt attempt by international actors to influence the course of domestic politics, yet, like many “second-image reversed” relationships, it remains understudied (Gourevitch 1978). Non-governmental organizations (NGOs), intergovernmental organizations (IGOs), private development contractors, and powerful Western states sponsor and carry out hundreds of programmatic activities intended to facilitate various aspects of democratization (Carothers 1999). Democracy promotion may include programs ranging from voter registration audits to political party training, and purchasing transparent ballot boxes to encouraging constitutional reforms. One of the most well-known and potentially consequential forms of democracy promotion is international election monitoring (Bjornlund 2004).<sup>2</sup> Since the late 1990s, states have been expected to invite delegations of foreign election monitors who observe and report on the quality of elections. States, organizations, and scholars who support election observation argue that it increases voter and political party confidence in the electoral process, deters fraud directly, and generates a third-party evaluation of election quality for international and domestic audiences, thus

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<sup>1</sup> Democracy promotion has existed for some time, but has increased enormously since the end of the Cold War. Woodrow Wilson is often credited as the modern initiator of democracy promotion, but since this time, encouraging democracy in other countries has been advocated by a number of state leaders, buoyed into a formal position in foreign policy by the democratic peace theory, and now advocated by nearly all of the major inter-governmental organizations (IGOs).

<sup>2</sup> Election monitoring and election observation are used interchangeably in this article.

making negative consequences for a leader who holds fraudulent elections more likely (Bjornlund, Bratton and Gibson 1992, Middlebrook 1998). As Kofi Annan stated while Secretary General of the United Nations:

The presence of international election observers, fielded always at the invitation of sovereign states, can make a big difference in ensuring that elections genuinely move the democratic process forward. Their mere presence can dissuade misconduct, ensure transparency, and inspire confidence in the process. (quoted in OSCE 2005, 2)

Skepticism of this view of observers is prevalent. The most critical argue that international election observers are simply glorified tourists (Carothers 1997, Soremekun 1999), or that they are biased representatives of their host governments, out only to promote their country's narrow economic interests (Brown 2001, Geisler 1993). Others argue that observers fail on other grounds. Robert Pastor implies that observers do not succeed in their mission when they actually observe electoral fraud because documenting fraud demonstrates that they have failed to prevent it (1998, 155). Even supporters of election observation suggest that observers should be more professionalized and receive better training, increase consistency in their evaluations across countries, improve coordination with local actors, and generally increase accuracy of their evaluations of election quality (Abbink 2000; Carothers 1997, Pastor 1998; van Cranenburgh 2000).

An assortment of case studies examine the role of international observers in the democratization process, and show mixed support for the claims made by proponents of election observation. For example, Bjornlund, Bratton and Gibson (1992) suggest that the presence of observers (both domestic and international) contributed to the successful transfer of power during the 1991 Zambian elections, but that observers struggled with their own political legitimacy vis-à-vis domestic audiences. In El Salvador,

Montgomery (1998) argues that the OAS's hasty judgment of the 1991 elections and their failure to criticize blatant attempts to manipulate the election contributed to the government's failure to reform obvious problems prior to the 1994 elections.<sup>3</sup> These and other similar studies offer a wealth of primarily qualitative information on elections in dozens of countries throughout the developing world, and set the foundation for more systematic tests of the effects of observers.

The central weakness of existing cross-national and case-study research on election monitoring is that these studies cannot attribute causal effects to observers. They show that the presence of observers is often correlated with a variety of positive and negative outcomes following elections, but they cannot compare these outcomes to the (hypothetical) counterfactual world in which observers were not present. As Thomas Carothers has rightly argued, the most significant potential effect of international observers is nearly impossible to measure:

Out of fear of being caught by foreign observers, political authorities may abandon plans to rig elections. Of course, few foreign officials would readily acknowledge having had such plans, making it hard to measure precisely the deterrent effect of electoral observation. Yet that effect should not be underestimated. (Carothers 1997, 18)

Knowledge that international observers will be present at an election may prevent fraud from being attempted by political parties and candidates, although the nature of the decisions to invite international observers and commit fraud prevents any meaningful test of this hypothesis. Do election monitors actually reduce election fraud, or have other meaningful effects on election day behavior?

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<sup>3</sup> Bjornlund 2004, Beigbeder 1994, Booth 1998, Bratton 1998, Geisler 1993, Kumar 1998, Laakso 2002, Matlosa 2002, McCoy 1998, Middlebrook 1998, Nwankwo 1999, Oquaye 1995, Orozco 2002, Pastor 1998.

The pre-election prevention of fraud that Carothers highlights is the ideal outcome for organizations interested in promoting clean elections, but Carothers neglects another possible means by which international observers can deter fraud. It is also a realistic hypothesis that international observers reduce election day fraud directly. Because individuals committing fraud, intimidation, or other electoral improprieties may not wish to carry out their intended actions in the physical presence of international observers, the fact that observers are present in a number of polling stations on election day may reduce the level of vote manipulation in those polling stations.

Using field experimental methods, I focus on a subset of potential effects of election observers: whether international election observers have any direct effects on election day behavior. By randomly assigning international observers during their election day observation, all other variables are held constant, and any difference between the group of observed and unobserved areas can be causally attributed to international observers. I present the results from the first study in which observers were randomly assigned, and show evidence that suggests that observers from the Carter Center had measurable effects on election day behavior during the final round of the 2004 presidential election in Indonesia.

As this is a new application of field experimental methods, I discuss many of the challenges presented by this study with the intention of facilitating future field experimental research on election observation and on other related areas. This study is ideal for replication. Ideally, randomization of short-term observers could be adopted as a common practice of election monitoring missions.

## **Random Assignment and the Effects of International Election Observers**

For scholars interested in understanding international influences on domestic politics, random assignment of international observers carries a number of advantages in terms of testing causality in a field where causal inference is difficult. Even though random assignment carries other advantages for election monitoring missions (it can provide the observation mission with a random sample from which to draw their conclusions about the quality of the election, and can provide another method of voting fraud detection), it is not standard practice in the field of international election observation. Current practice varies by election and by organization, but the most common scenario is to allow individual observer teams to choose the polling stations that they visit within a given region after they have been deployed throughout the country.

During the 2004 presidential elections in Indonesia, I was given the opportunity to apply random assignment of international observers to The Carter Center's election day deployment plan. To my knowledge, this was the first attempt of this type within the field of international election observation.<sup>4</sup> This randomization allows a field experimental test of whether observers influence election day behavior.

If voting fraud or other forms of electoral manipulation occur on election day, and if international observers reduce that manipulation via their presence in and around polling stations, then on average, the cheating candidate(s) should perform worse in areas that were observed. This general effect is phrased as a hypothesis.

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<sup>4</sup> Since that time, randomized assignment of international observers has been conducted by The Carter Center in Nicaragua (2006), by a Yale University student delegation participating in a US Embassy mission in Mauritania (2007), and by NDI in the 2006 Palestinian elections.

**H1: If international observers reduce election day manipulation, average vote share for cheating candidates should be higher in unmonitored areas than in monitored areas.**

“Vote share” can be measured as a percentage of registered voters (absolute vote share) or as a percentage of votes cast (relative vote share). The vote share comparison is most straightforward if only one candidate in the area is attempting to commit electoral fraud. It is possible that more than one party will commit manipulation (perhaps varying by region), and observers may influence the process in a non-uniform manner across the country. Because of the possibility that observers will have different effects on different populations, comparisons also account for potential heterogeneous treatment effects.

Vote manipulation may also influence turnout, but the direction of the potential effect of observers on turnout depends on the form of electoral manipulation and whether vote suppression occurs. Cheating parties or candidates may attempt to gain an advantage through fraud by artificially increasing their own support or by artificially suppressing support for their opponents(s). Ballot box stuffing, for example, should artificially increase turnout reported in election results. Voter intimidation and other forms of vote suppression should artificially decrease turnout. If observers have a negative effect on election day manipulation, then their physical presence should be associated with significant differences in voter turnout or vote share relative to areas which were not visited by observers.

It is also likely that any parties intent on manipulating the election may do so in advance of election day. I focus exclusively on election day fraud here, but I do not deny the possibility that pre-voting manipulation takes place, and may take place instead of election day fraud.

Thus far the argument has presented in general form. The next section turns to describing the application of random assignment to the 2004 elections in Indonesia.

### **Experimental Design in Indonesia**

There were two rounds of the 2004 presidential election; this paper focuses on the second round runoff between the incumbent candidate Megawati Sukarnoputri and the leading challenger, Susilo Bambang Yudhoyono. Indonesia is one of the largest and most geographically diverse election-holding countries in the world. With approximately 155 million eligible voters, 17,508 islands, and nearly 580,000 polling stations, Indonesian elections are no small affair. The 2004 presidential election was the first direct presidential election in Indonesian history, giving little historical precedent on which to base predictions of vote patterns for the 2004 elections. There are five levels of administrative divisions in Indonesia pertaining to elections. There are 33 provinces (*propinsi*) which are divided into 414 regencies (*kabupaten or kota*). These regencies, in turn, are divided into 4,987 sub-districts (*kecamatan*), the districts are divided into approximately 60,000 villages or neighborhoods (*kelurahan or desa*), and the villages and neighborhoods are divided into 574,945 polling stations (*TPS*).

In cooperation with The Carter Center's Democracy Program, I helped design the deployment plan for Carter Center short term observers in the July and September presidential elections. The case of Indonesia was selected because the opportunity to

attempt random assignment of international observers was made available. The introduction of randomly assigned international observers had been met with some skepticism by other practitioners. Although international election observation missions regularly use randomization to assign international observers to vote-counting centers at the end of election day as part of a parallel vote tabulation,<sup>6</sup> random assignment of international observers during polling had not been attempted because it was thought unnecessary, logistically too difficult, or contrary to some of the other goals of election observation.<sup>7</sup>

There were five logistical challenges to the randomization of observers in Indonesia, all of which were possible to mediate. The first logistical challenge was that many areas of the country were not accessible to international observers on election day, and therefore random assignment could not be attempted across the entire population of polling stations even if more observers had been available. Rather than randomize across the entire population, randomization was conducted within regencies, as described below.

Second, there was no complete list of polling stations available from the central government. This problem was addressed by randomizing at the village level, and training observers to select polling stations randomly within villages.

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<sup>6</sup> The parallel vote tabulation, or quick count, provides an independent measure of the election results, within a margin of error, and is traditionally more reliable than exit polling. Observers (domestic or international) are assigned to a random sample of polling stations to directly observe the counting process. They call in the tallies from the vote count, and because the sample is random, quick counts typically provide very accurate estimations of the election results, and thus guard against manipulation during the counting process (Estok, Nevitte, and Cowan 2002).

<sup>7</sup> For example, one strategy for election monitoring is to send observers to the areas that are expected to have problems, or to send observers to areas that would 'benefit from seeing an international presence'. These strategies create clear bias in the content of election day observations, but are perceived as politically important. (Personal conversations between the author and international election observation professionals from NDI, the EU, the OSCE/ODIHR, and The Carter Center.) Of course, it would be possible to randomize within regions that are expected to have problems in order to alleviate this concern.

Third, it was unclear prior to the election whether any disaggregated election results would be made available, the lack of which would have thrown a significant barrier in front of any attempts to better understand the effect of observers on election day behavior. This problem was difficult to address in advance, but because the government had promised to release these results before the election, and ultimately did so, it did not become a problem.

Fourth, because election law mandated that each polling station have a maximum of 300 voters, a full-length election day was determined unnecessary, and polling stations were only open from 7 am to 1 pm for the presidential election, significantly reducing the number of polling stations that an observer team could visit on election day. This challenge did not prevent the experiment from taking place, but reduced the number of villages visited by observers.

Finally, because it was the first time that random assignment of international election observers had been attempted, many of the challenges in applying this methodology to election observation had yet to be worked out and agreed upon by the interested parties. Although this explains some of the decision-making timeline, the cooperation and flexibility of the Carter Center staff and delegation made the project feasible.

The Carter Center's mission for the second round of the election consisted of 50 observers (24 observer teams). The long-term election observers and the Jakarta-based staff of The Carter Center selected 24 areas of Indonesia (primarily *kabupaten* and *kota*, or regencies and cities) where The Carter Center would send election observers. In order for an area to be selected, it had to be accessible by car or aircraft within one day's travel

time, and had to have basic accommodations for the observer team that was judged as sufficiently safe.<sup>8</sup> There was also some effort made to avoid extensive overlap with the European Union election observation mission, as well as consideration for whether access was granted to areas where foreigners are usually prohibited from traveling such as Banda Aceh, Ambon, and parts of Papua. Thus, the selection of regencies to be visited by the Carter Center was not random. For 21 of the 24 teams, random assignment was instead applied within each regency or pair of regencies where Carter Center observers were sent.

The information on where the Carter Center short term election observers would be deployed was not released ahead of time to the public. International observers typically rove from polling station to polling station during election day. Ideally observers would have been assigned randomly to polling stations within each regency where Carter Center observers were sent. However, even if there had been a complete list of polling stations, observers would have had a difficult time locating them. Many were set up outdoors at locations without physical addresses such as community badminton courts, in the middle of streets, on sidewalks, in empty lots, etc. The best alternative was a complete list of the next largest administrative divisions above polling stations: *kelurahan* and *desa*. These administrative divisions equate to villages in non-urban areas or neighborhoods within cities. They can be as small as a few hundred voters, and as large as 60,000 voters. Most villages/neighborhoods are identifiable on a local map, making it possible for the observer teams to find them. This made random assignment across villages logistically possible, both because a complete list existed, and because

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<sup>8</sup> Security concerns are relatively standard on election observation missions, but were heightened in Indonesia because of recent Western-targeted bombings of hotels and the Australian embassy.

international observer teams had a reasonable chance of being able to identify and locate the villages on election day.

### ***Randomized Observation Procedure***

Each observer team was assigned a list of randomly selected villages to visit on election day. The method used to randomly select a list of villages for each team was simple. Using a complete list of villages in each *kabupaten*, or regency villages were sorted by an ID number that roughly identified them regionally. Every *n*th village was marked and included in the sample, but the first village chosen was selected arbitrarily.<sup>9</sup> The intention behind this method was to achieve some geographic stratification in the absence of maps or other characteristics of villages.<sup>10</sup>

Prior to being deployed to the regencies, observers were given instructions on how to select polling stations once they arrived at the village using a method that approximates randomization. Observers were necessarily limited to those polling stations that they could find. Most teams were able to spend the day before election day scouting the area and looking for signs that polling stations were being set up for the election. They were trained not to choose polling stations based on any substantive characteristics such as the number of voters, complaints about the polling station, known popularity of one candidate, or recommendations from local officials or police. Rather, if they went to

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<sup>9</sup> The arbitrary selection was made by the author. It was not random (say, by roll of dice) but the first selected village to start the skipping pattern was selected without attention to location.

<sup>10</sup> A better method for most cases is to use a computer program to generate a list of random numbers that correspond with each polling station, sort by the random numbers, and select the appropriate number of polling stations from the top of the list.

more than one polling station in a given village, they were instructed to go to every third or fifth polling station that they could locate.<sup>11</sup>

Because observers were assigned randomly across villages (rather than across polling stations), and the small 300-voter polling stations were often adjoining or within sight of each other, the hypothesis is tested at the village level.<sup>12</sup>

As mentioned above, a further challenge in randomly assigning international observers in Indonesia is that it was difficult to know ahead of time how many villages each team would be able to travel to during the election day period. Polling stations were originally scheduled to be open from 7am until 1pm. For the second round in September, the election commission issued a late ruling that polling stations could close at 11:30 am if all voters registered at the polling station had voted. In practice, many polling stations closed when the local officials felt that everyone who was going to vote had voted, creating wide variation in the length of election day. The terrain and population density also varied considerably, and travel time between villages was unknown when the lists of randomly assigned villages were generated.

Because of these uncertainties, each team was given a list of villages that intentionally contained more villages than they should have been able to visit on election day. This ensured that they would not run out of randomly selected villages during election day, and in the event that a listed village was inaccessible, they could continue going to other villages on their list.

## **Data**

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<sup>11</sup> If this was not practical, they were instructed to draw a line or a “V” through the village on a local map and go to polling stations along this line. Both of these methods were intended to encourage observers to select polling stations in areas of a given village of varying demographic characteristics.

<sup>12</sup> I have also gathered polling station level election results.

This experiment requires disaggregated election results. Data on the vote share for each candidate and voter turnout were collected for all villages within the regencies visited by Carter Center election observers in the second round of the 2004 presidential election.<sup>13</sup> Polling station level data for the same regions were also collected.<sup>14</sup> The unofficial results were made available by the Indonesian *KPU* (roughly translated as the General Elections Commission) for most of the country, but data was incomplete for three of the regencies visited by Carter Center observers in the second round: Mimika, Kupang and Manokwari. Unofficial results were uploaded regionally by election officials, and are subject to the usual disclaimers about unofficial election results. The village level unofficial election results were downloaded, copied, and aggregated by the author. Excluding the regions with missing data, this leaves 2,165 village level observations. Table 1 presents aggregate summary statistics. Out of all villages in the visited regions, Carter Center observers were assigned to visit 482 villages, 95 of which were actually visited. Within these 95 assigned and visited villages, 147 individual polling stations were visited.

## **Results**

In the second round of the 2004 presidential elections, Susilo Bambang Yudhoyono (commonly referred to as SBY) and his running mate Jusuf Kalla were the leading candidates, having won 34% of the votes cast in the first round out of the five candidate field. The incumbent president, Megawati Sukarnoputri (commonly referred to

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<sup>13</sup> Data reported here are for the second round of the presidential elections, and are reported at the village level. The village level is appropriate for substantive reasons, but the polling station level will also be tested when data collection is complete. Additionally, if the first round election results are again made available by the General Elections Commission at the village and polling station levels, these results will also be reported in future iterations of this project.

<sup>14</sup> Due to the sheer magnitude of the data, polling station level unofficial election results were downloaded and compiled using a program written by Jon Immel. Data were downloaded from <http://tnp.kpu.go.id/>.

as Megawati), garnered 27% in the first round. The runoff was held on September 20, 2004. SBY won the presidency with 60.6% of the vote. The average vote share for SBY across all of the regencies included in the sample in this study is 63.6%, underscoring that the selection of regencies was not representative of the entire country. All comparisons of vote share only include regencies where Carter Center observers were deployed and where they participated in the randomization.

Because the blocks (within which the randomization was conducted) and the villages (the unit of analysis) vary in size, and the treatment rates are relatively low and vary considerably, the best way to analyze the data is not obvious. Table 2 summarizes the areas observed by Carter Center observers at the village level. Note that within each village, observers visited one to four individual polling stations. Also note that some villages in the control group were visited.<sup>15</sup> The control group contamination is likely to result in underestimation of the effect of observers, but does not invalidate the experiment.

I present several different sets of results. First, I pool the regions included in the experiment and use dummy variables for each (non-overlapping) region to which an observer team was assigned. Second, I present data for each region separately, as individual experiments, in order to explore potential heterogeneous treatment effects. The effects within each experimental block are then aggregated using meta-analysis in which the average effect is estimated from the coefficients and standard errors from the twenty individual experiments.

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<sup>15</sup> For some teams, visiting control group villages or neighborhoods was accidental and resulted from visiting polling stations near the border between urban neighborhoods. Other teams encountered logistical (usually transportation related) problems that caused them to choose to visit villages outside of their assigned list.

The null hypothesis for H1 is that there is no difference in either candidate's vote share between monitored polling stations and unmonitored polling stations. The effect of observers is examined on four dependent variables, all of which are measured at the village level: (1) overall turnout, measured as ballots cast as a percentage of registered voters (2) votes cast for Megawati as a percent of registered voters, (3) votes cast for SBY as a percent of registered voters, (4) votes for Megawati as a percent of total votes cast.

Table 3 presents the first set of tests using an OLS model. The primary independent variable in Models 1-4 is whether a given village was assigned to the treatment group (the list of villages assigned to international observers). Note that in these models, *treatment group* is a measure of the intent to treat the village, not the actual presence of observers on election day. This measure, also called an "intent-to-treat", or ITT effect, should dampen any observable difference between groups, but it has the distinct advantage of eliminating any possibility of bias in the actual treatment (see Gerber and Green 2000; Green, Gerber and Nickerson 2003). Because inclusion in the treatment group was random, there is no reason that anything other than the presence of observers could have caused a difference between groups. For each model, I also include a covariate for village size measured as the natural log of the total number of registered voters. Although few covariates are available at the village level, including them in the regression helps to reduce unexplained variance in candidate performance and turnout.

For three of the four dependent variables, being included in the treatment group appears to have no statistically significant effect. However, Model 2 (Table 3) shows a

statistically significant result: being assigned to the treatment group is associated with an average of a 1.3 % higher absolute vote share for Megawati. Note that the significant effect of observers on Megawati's vote share in Model 2 appears despite the fact that the average treatment rate was only 20%. If the treatment rate was higher, the ITT effect would likely be stronger.

What is the effect of actual treatment rather than the intent to treat? Following Alan Gerber and Don Green's (2000) voter mobilization experiments, I use an instrumental variables regression. In their experiments, some voters are easier to reach than others, and therefore are more likely to receive the experimental treatment. This becomes a problem for analysis when the probability of receiving the treatment is correlated with the dependent variable. In their study, voters who are much harder to reach are also less likely to vote, thus creating bias in the actual treatment. Similarly, in this experiment, the difference between villages that were assigned to the treatment group and villages that were actually "treated" or visited by international observers could be related to candidate vote share. Some areas are more difficult to find than others, so the likelihood of being visited by observers may be correlated with any of the four dependent variables. For example, it is possible that smaller villages are systematically more difficult to find, and that voter turnout or support for one of the candidates is always higher in smaller villages (and in fact, this appears to be the case in Indonesia).

Following Gerber and Green (2000), the solution to this problem is to find an instrument that is correlated with the actual treatment but that is not correlated with the dependent variable. Assignment to the treatment group of villages within a region is random, and therefore is not correlated with any village-level variables. Being visited by

observers is a function of a village being assigned to the treatment group. Thus, when using the actual visit by observers to a village as an explanatory variable, assignment to the treatment group satisfies the conditions for a valid instrument. Table 4 presents results from the instrumental variables regression on the same four dependent variables, with and without a control variable for village size.

In Models 6a and 6b, the presence of observers in a village continues to have a significant effect on the absolute vote share for Megawati. Predictably, the size of the effect in the 2SLS regression is larger than in the OLS regression. Accounting for actual treatment rates, international observers cause Megawati to gain approximately 7% more votes cast as a percentage of registered voters.

From this point forward, the analysis focuses on only one dependent variable for reasons of space: absolute vote share for Megawati. Table 5 presents Megawati's vote share as a percentage of registered voters by region.

Before examining potential explanations for this effect of international observers, I explore whether the effect is uniform across all regions included in the study. As mentioned above, it is possible that the effects of international observers on election day behavior are heterogeneous. Even though Tables 3 and 4 show a significant effect of observers on election day behavior in models that include regional dummies, it is still useful to examine the effects of observers within each region. Because observers were assigned to villages from a pool of all villages in a given territory, each geographic area can be treated as a separate mini-experiment. Table 6 presents the results, by region, of the two-stage least squares regression. For regions in which the treatment rate is very low (or observers did not visit a high percentage of villages assigned to them) the

standard errors in the 2SLS regression become large. Note that in each region or mini-experiment, standard statistical significance is unlikely because of the small treatment group and population sizes. Of potentially greater interest is the consistency of the sign on each coefficient. Note that out of the 20 studies; only five of the coefficients on the presence of observers are negative.

### *Meta-Analysis*

Meta-analysis is typically used to compile the results of different studies on the same topic. In medical research, studies examining the same question are often repeated across different populations and across time. Meta-analysis allows these results to be combined in order to evaluate the overall effect of the treatment variable and to judge the relative influence of each study on the results. Additionally, because the treatment rates vary considerably between each region, it is useful to see which regions are most influential in evaluating the overall effect of observers on voting behavior.

For Megawati's absolute vote share, the initial meta-analysis confirms that there is not significant heterogeneity between the results of the studies (regions) using the 2SLS regression ( $p = 0.251$ ). Table 7 presents the pooled estimates of the effect of observers on Megawati's absolute vote share (the same dependent variable presented in Table 6). I present the results of fixed and random effects models from both OLS and the 2SLS models.

When the 20 regions are treated as separate experiments and then aggregated in this fashion, the coefficients are smaller and are not statistically distinguishable from zero. This decrease in the size and significance of the effect is not surprising given the extremely small sample size in the treatment groups within regions. Most importantly, the

meta-analysis shows that there is not significant heterogeneity in the effect across regions.

The estimated effect of observers on Megawati's vote share using meta-analysis is somewhat smaller than in the estimates created using regional dummies. The pooled estimate of the effect of assignment to the treatment group on Megawati's absolute vote share is less than 1%. The effect of actual treatment using a random effects model is close to 4%, but this estimate does not achieve traditional levels of statistical significance in a two-tailed test.

*Alternative Analysis: Effect of Treatment on Treated*

The above analyses do not account for the study's "double cross over": the fact that not all villages assigned to the treatment group were visited, and some villages in the treatment group were visited. Freedman (2006) offers an alternative IV estimator that accounts for this form of double crossover in experiments.

Table 8 presents alternative results for Megawati's absolute vote share, following Freedman, with two important differences. First, in Table 5, the vote shares are estimated at the village level, and the hypothesized effect of observers is a percentage effect. In Table 8, all estimates, including vote shares, are calculated at the treatment/control group level and yield somewhat different results. For example, for a given region, the absolute vote share within the treatment group is calculated as the sum of all votes cast for Megawati in the treatment group as a percentage of the sum of all registered voters in the treatment group. In other words, the vote share is calculated at the regional rather than village level.

The ETT (effect of treatment on treated) estimator is calculated by region, following Freedman (2006), as,

$$\frac{Y^T - Y^C}{X^T - X^C},$$

where  $Y^T$  is the average response in the treatment group,  $Y^C$  is the average response in the control group,  $X^T$  is the treatment rate in the treatment group, and  $X^C$  is the treatment rate in the control group.

[Still need to calculate standard errors and decide how to calculate aggregate effect across the 20 regions].

## **Conclusion**

Do international election observers deter fraud or increase voter confidence in the electoral process? Does the presence of foreigners in polling stations change election day behavior? This article has outlined how random assignment of international election observers can be used to answer these empirical questions. In the case of the second round of the 2004 presidential election in Indonesia, international observers were randomly assigned to the villages that they visited, and instructed to randomly select polling stations within these villages. On most variables, there is no significant difference between observed and unobserved groups. However, the data show that the presence of observers caused some changes in the vote share for the incumbent, Megawati Sukarnoputri, who went on to lose the election and peacefully transfer power to her competitor. Similar field experimental methods should be applied to advance our understanding of the effects of election observation and of other democracy promotion activities.

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**Table 1: Summary Statistics for All Available Village-Level Variables**

<b>Variable</b>	<b>Obs</b>	<b>Mean</b>	<b>Std. Dev.</b>	<b>Min</b>	<b>Max</b>
Observed	2165	0.0554	0.2289	0	1
Sample	2165	0.2222	0.4158	0	1
SBY Relative Vote Share	2165	0.6355	0.1956	0.002430	0.9929
MEGA Relative Vote Share	2165	0.3645	0.1956	0.006778	0.9976
SBY Absolute Vote Share	2165	0.4537	0.1461	0.00819	0.9537
MEGA Absolute Vote Share	2165	0.2703	0.1690	0.006778	0.9651
Overall Turnout	2165	0.7242	0.1108	0.1	1.007
Ballots Received	2165	5387	5453	35	59567
Valid Ballots	2165	3749	3605	6	42112
Invalid Ballots	2165	86.38	128.2	0	1582
Extra Ballots	2165	5.316	23.64	0	345
Damaged Ballots	2165	52.76	273.1	0	5923
Ballots Not Used	2165	1504	1945	0	16612
Total Registered Voters	2165	6525	6664	35	63272

**Table 2: Carter Center Observation Coverage in Round 2<sup>17</sup>**

Study or Region	Province	Regency or City	Number of Villages in Treatment Group	Number of Villages in Control Group	Number Villages Observed	Villages Visited in Control Group	Total Voters
1	Nanggroe Aceh Darussalam	Kota Banda Aceh	19	71	3	1	173,265
2	East Java	Kota Surabaya	34	129	3	0	2,078,486
3	Nusa Tenggara Barat	Kota Mataram	11	12	4	0	241,483
4	East Java	Sampang	41	145	5	0	569,216
5	Bali	Tabanan	27	90	6	0	325,701
6	East Java	Situbondo	32	104	10	0	488,633
7	DI Yogyakarta	Kota Yogyakarta	20	25	4	0	327,873
8	East Java	Kota Kediri	15	31	11	0	200,137
9	North Sumatra	Kota Medan	30	126	5	2	1,525,526
10	Riau	Kampar, Kota Pekanbaru	53	190	5	0	740,924
11	East Kalimantan	Kota Samarinda	8	34	4	2	453,693
12	West Java	Cianjur	68	275	1	3	1,378,863
13	Central Kalimantan	Kota Palangka Raya	6	25	1	4	123,596
14	West Kalimantan	Kota Pontianak	9	15	4	3	371,780
15	West Sumatra	Kota Padang	20	83	4	1	525,422
16	South Sumatra	Palembang	20	83	11	1	906,169
17	North Sulawesi	Kota Bitung	12	48	3	4	120,637
18	North Maluku	Kota Ternate	11	41	5	1	95,771
19	Maluku	Kota Ambon	18	38	4	1	192,097
20	South Sulawesi	Kota Makassar	28	118	2	2	812,977
<b>TOTAL</b>			<b>482</b>	<b>1683</b>	<b>95</b>	<b>23</b>	<b>11652249</b>

<sup>17</sup> Note that the additional Carter Center teams were not included in this study, as they did not participate in the randomization.

**Table 3: Effects of Assignment to Treatment Group on Vote Share and Turnout**

	(1)	(2)	(3)	(4)
	<b>Overall Voter Turnout</b>	<b>Votes for Megawati/ Registered Voters</b>	<b>Votes for SBY/ Registered Voters</b>	<b>Votes for Megawati/ Total Votes Cast</b>
<b>Treatment Group</b>	0.007 (0.005)	0.013* (0.006)	-0.006 (0.006)	0.013 (0.007)
<b>ln (Tot. Reg. Voters)</b>	-0.013** (0.002)	-0.020** (0.003)	0.007* (0.003)	-0.022** (0.004)
<b>Constant</b>	0.783** (0.020)	0.248** (0.025)	0.536** (0.026)	0.307** (0.030)
<b>Observations</b>	2165	2165	2165	2165
<b>R-squared</b>	0.35	0.57	0.36	0.52

**Notes: Models in tables include dummy variables for each region within which observers were assigned (not reported). Standard errors in parentheses.**

**\* significant at 5%; \*\* significant at 1%**

**Table 4. IV Regression: Instrumented Variable: Village Visited by International Observers.  
Endogenous Variable: Village in Treatment Group.**

	(5a)	(5b)	(6a)	(6b)	(7a)	(7b)	(8a)	(8b)
	Overall Turnout	Overall Turnout	Votes for Megawati/ Registered Voters	Votes for Megawati/ Registered Voters	Votes for SBY/ Registered Voters	Votes for SBY/ Registered Voters	Votes for Megawati/ Total Votes Cast	Votes for Megawati/ Total Votes Cast
<b>Village Observed</b>	.0330 (0.0268)	0.0363 (0.0267)	0.0682* (0.0337)	0.0717* (0.0335)	-0.0329 (0.0345)	-0.0341 (0.0346)	0.0795 (0.0408)	0.0718 (0.0406)
<b>ln (Tot. Reg. Voters)</b>		-0.0144 ** (0.0025)		-0.0221 ** (0.0031)		0.0075* (0.0032)		-0.0237 ** (0.0038)
<b>Constant</b>	0.683 (0.00965)	0.790 ** (0.0204)	0.0989 (0.122)	0.2618** (0.0257)	0.5845 (0.0125)	0.5290** (0.0265)	0.1457 (0.0147)	0.3207** (0.0311)
<b>Obs.</b>	2165	2165	2165	2165	2165	2165	2165	2165
<b>R-squared</b>	0.334	0.34	0.545	0.55	0.3621	0.36	0.5022	0.51
<b>Root MSE</b>	0.091	0.090	0.115	0.113	0.117	0.117	0.139	0.137

\* significant at 5%; \*\* significant at 1%

Note: Instrumental variable regressions computed using a single-stage equation (ivreg in Stata). Models in tables include dummy variables for each region within which observers were assigned (not reported).

**Table 5: Megawati Absolute Vote Share by Treatment and Control Groups**

	<b>Not Visited by Observers</b>	<b>Visited by Observers</b>	<b>Assigned to the Control Group</b>	<b>Assigned to Treatment Group</b>
1. Nanggroe Aceh Darussalam	17.2	15.6	17.3	11.5
2. East Java	22.1	26.8	21.8	29.8
3. Nusa Tenggara Barat	19.3	16.1	19.3	18.2
4. East Java	27.7	27.7	26.2	33.0
5. Bali	64.0	69.4	63.9	65.4
6. East Java	29.4	41.0	29.0	34.5
7. DI Yogyakarta	28.7	28.2	28.6	28.6
8. East Java	37.1	39.8	36.3	40.7
9. North Sumatera	21.4	20.4	21.6	20.1
10. Riau	17.4	12.2	17.1	17.8
11. Kalimantan Timur	19.4	19.0	19.1	20.3
12. West Java	31.9	23.2	32.0	30.8
13. Central Kalimantan	33.6	22.0	30.7	36.1
14. West Kalimantan	19.0	18.4	19.7	17.3
15. West Sumatra	8.9	6.7	8.8	8.5
16. South Sumatra	35.6	36.3	36.0	34.4
17. Sulawesi Utara	40.0	28.1	39.4	35.1
18. North Maluku	22.1	15.1	20.5	24.0
19. Maluku	44.4	29.2	42.9	43.5
20. South Sulawesi	7.6	7.1	7.3	10.1
Average Vote Share	27.0	27.8	26.6	28.4

**Table 6: Instrumental Variable Regression by Region/Study**

Region:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	MEGA	MEGA	MEGA	MEGA	MEGA	MEGA	MEGA
	Absolute	Absolute	Absolute	Absolute	Absolute	Absolute	Absolute
	Vote Share	Vote Share	Vote Share	Vote Share	Vote Share	Vote Share	Vote Share
Villages Visited	0.1035	0.0787	0.0485	0.5926*	0.0699	0.1786*	0.1475
	(0.0922)	(0.1574)	(0.0799)	(0.2646)	(0.1461)	(0.0722)	(0.1029)
ln (Total Voters in Village)	-0.0056	0.0111	-0.0376	0.0611**	-0.0863**	-0.0109	-0.0261
	(0.0068)	(0.0072)	(0.0449)	(0.0217)	(0.0247)	(0.0135)	(0.0326)
Constant	0.1388*	0.1896**	0.5189	-0.2234	1.3204**	0.3790**	0.4960
	(0.0527)	(0.0669)	(0.4053)	(0.1735)	(0.1954)	(0.1112)	(0.2897)
Obs	90	163	23	186	117	136	45

  

Region:	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	MEGA	MEGA	MEGA	MEGA	MEGA	MEGA	MEGA
	Absolute	Absolute	Absolute	Absolute	Absolute	Absolute	Absolute
	Vote Share	Vote Share	Vote Share	Vote Share	Vote Share	Vote Share	Vote Share
Villages Visited	0.0639	-0.1031	0.0695	0.0298	-2.8458	4.9410	-0.0399
	(0.0341)	(0.1066)	(0.1771)	(0.0466)	(22.5260)	(52.1034)	(0.0853)
ln (Total Voters in Village)	-0.0382	0.0012	-0.0154*	-0.0284**	-0.0065	-0.9359	0.0331
	(0.0201)	(0.0075)	(0.0065)	(0.0101)	(0.3374)	(9.4528)	(0.0233)
Constant	0.6784**	0.2070**	0.2893**	0.4465**	0.4071	6.1386	-0.1121
	(0.1638)	(0.0667)	(0.0494)	(0.0909)	(2.6635)	(58.4394)	(0.2059)
Obs	46	156	243	42	343	31	24

  

Region:	(15)	(16)	(17)	(18)	(19)	(20)
	MEGA	MEGA	MEGA	MEGA	MEGA	MEGA
	Absolute	Absolute	Absolute	Absolute	Absolute	Absolute
	Vote Share	Vote Share	Vote Share	Vote Share	Vote Share	Vote Share
Villages Visited	0.0274	-0.0314	-0.2877	0.1436	0.0225	0.1186
	(0.0693)	(0.0277)	(0.1895)	(0.1377)	(0.2749)	(0.2908)
ln (Total Voters in Village)	-0.0275**	-0.0380**	-0.0222	-0.1237**	-0.0073	-0.0098
	(0.0081)	(0.0098)	(0.0322)	(0.0268)	(0.0282)	(0.0071)
Constant	0.3185**	0.7008**	0.5807**	1.0970**	0.4853*	0.1857**
	(0.0666)	(0.0887)	(0.2160)	(0.1930)	(0.2132)	(0.0594)
Obs	103	103	60	52	56	146

**Table 7: Meta-Analysis (Need to run on total votes for Megawati)**

<b>Model and Variable</b>		<b>Pooled Coefficient Estimate (95% CI)</b>	<b>Asymptotic z_value</b>	<b>Asymptotic p_value</b>	<b># Studies</b>	<b>Test for Heterogeneity</b>
<b>Intent-to-Treat on Megawati Absolute Vote Share (OLS)*</b>	Fixed	0.008 (-0.000 0.017)	1.939	0.052	20	$Q = 27.672$ $df = 19$ $p = 0.090$
	Random	0.010 (-0.001 0.020)	1.762	0.078		
<b>Actual Treatment on Megawati Absolute Vote Share (2SLS)*</b>	Fixed	0.027 (-0.004 0.059)	1.695	0.090	20	$Q = 22.685$ $df = 19$ $p = 0.251$
	Random	0.038 (-0.003 0.078)	1.808	0.071		

\*Coefficients and standard errors from models run by region which includes village size. The OLS models available from the author. The 2SLS models are presented in Table 6 above.

**Table 8: Estimates of ETT accounting for double crossover.**

	Total Voters	Y_t (percent)	Y_c (percent)	X_t (percent)	X_c (percent)	ITT (percent)	<b>ETT (percent)</b>
1	173265	9.92	9.18	10.66	0.01	0.74	<b>7.61</b>
2	2078486	30.45	29.52	5.81	0.00	0.93	<b>16.05</b>
3	241483	18.80	16.80	49.49	0.00	2.00	<b>4.04</b>
4	569216	35.07	28.62	10.66	0.00	6.45	<b>60.52</b>
5	325701	62.49	61.56	22.47	0.00	0.93	<b>4.14</b>
6	488633	35.65	28.25	41.18	0.00	7.41	<b>17.98</b>
7	327873	29.02	26.38	19.97	0.00	2.64	<b>13.22</b>
8	200137	40.93	35.05	87.71	0.00	5.88	<b>6.70</b>
9	1525526	20.06	20.86	22.66	0.04	-0.79	<b>-4.07</b>
10	740924	15.99	14.73	10.60	0.00	1.25	<b>11.80</b>
11	453693	18.84	18.06	50.19	0.49	0.78	<b>1.97</b>
12	1378863	27.52	28.78	2.21	0.01	-1.26	<b>-151.74</b>
13	123596	25.20	22.48	83.84	9.54	2.72	<b>27.51</b>
14	371780	18.78	20.03	59.46	3.08	-1.25	<b>-3.59</b>
15	525422	7.55	8.06	16.80	0.02	-0.51	<b>-3.46</b>
16	906169	33.09	34.76	46.44	0.02	-1.67	<b>-3.74</b>
17	120637	32.28	34.81	47.11	1.22	-2.53	<b>-8.79</b>
18	95771	13.98	14.88	61.65	0.05	-0.89	<b>-1.51</b>
19	192097	37.32	42.29	31.55	0.09	-4.97	<b>-18.61</b>
20	812977	9.98	9.45	7.44	0.03	0.53	<b>9.48</b>