The Value of Majority Status: The Effect of Jeffords’s Switch on Asset Prices of Republican and Democratic Firms

Chris Den Hartog  
Department of Political Science  
University of California, San Diego  
9500 Gilman Dr.  
La Jolla, CA 92092-0521  
cdenhart@ucsd.edu

Nathan W. Monroe  
Department of Political Science  
University of California, San Diego  
9500 Gilman Dr.  
La Jolla, CA 92092-0521  
nmonroe@weber.ucsd.edu

Abstract: On May 24, 2001 United States Senator James Jeffords announced that he was switching from Republican to independent and would vote with Democrats on organizational votes, effectively giving majority party control of the Senate to the Democrats. At the time of the switch, there was no change in Senate membership or preferences; rather, the only thing that changed was majority party status. We take advantage of this controlled environment to test competing hypotheses derived from partisan (Cox and McCubbins 2002) and non-partisan (Krehbiel 1998) theories of legislative organization. To conduct our test, we use “event study” methodology to gauge the effect of the Jeffords switch on the asset values of Republican and Democratic “constituent firms.” Our results – which show that the stock prices of Republican firms fell on news of the switch, while Democratic stocks rose – offer compelling evidence of party effects in congress, and cast doubt on non-partisan theories of legislative organization.

*We thank Mat McCubbins, Gary Cox, Gary Jacobson, Jeff Lax, Allan Timmerman, and Cheryl Boudreau for helpful comments.
1. Introduction

On May 24, 2001 United States Senator James Jeffords announced that he was switching from Republican to independent and would vote with Democrats on organizational votes, effectively giving majority party control of the Senate to the Democrats. The announcement, which came just four months after Republicans obtained unified control of the federal government for the first time since 1955, sent shockwaves through the political landscape.\(^1\) Republican Senate Majority leader Trent Lott decried Jeffords’s switch as “the impetuous decision of one man to undermine our democracy…. It was a 'coup of one' that subverted the will of the American voters who elected a Republican majority” (Berke 2001).

Jeffords’s switch also generated intellectual excitement among Congressional scholars, because it created an unprecedented opportunity for testing competing partisan and non-partisan theories of congressional organization. On the one hand, partisan theories (Cooper and Brady 1981; Sinclair 1983; Rohde 1991; Cox and McCubbins 1993, 1994, 1997, 2002; Aldrich and Rohde 1998, 2001; Campbell, Cox, and McCubbins 2002) imply that the switch was a significant event that warranted the strong reaction described above, since partisan control of the Senate changed hands. On the other hand, non-partisan, preference-based theories (Mayhew 1974; Krehbiel 1998) imply that the switch was not a significant event, since they attribute no importance to congressional parties, and since—because there was no change in Senate membership—the preferences of Senate members were constant across the period encompassing the switch.

\(^{1}\) Indeed, the shockwaves were not limited to the United States. Newspapers around the world — for example *The Daily Telegraph* (Australia), *The Guardian* (England), and *The Statesman* (India) — took notice of the Jeffords switch and the impending change in Senate party control.
Previously, testing the two types of models against one another has been beset by an overriding problem: changes in majority status in the U.S. Congress have been accompanied by changes in preferences. In other words, majority status has changed due to elections, but those elections have simultaneously produced changes in the membership of the Senate, and therefore in member preferences. In the Jeffords case, however, there was no change in Senate membership or preferences—rather, the only thing that changed was that Jeffords declared himself an independent and started voting with the Democrats, instead of the Republicans, on organizational votes. In a Senate that was divided 50-50 prior to the switch, this changed partisan control of the Senate. We therefore use the Jeffords switch to test hypotheses derived from partisan theories—and, due to the fortuitous circumstances of the switch, the null hypotheses against which we test them are predicted to be true by non-partisan theories.

There are a number of ways in which majority status is thought to be valuable, and thus a number of ways we might go about trying to determine whether a change in majority party control had significant consequences. For instance, one might look for changes in roll call voting behavior, in committee composition, in resources available to members, or in campaign contributions. Our approach is to examine whether the switch significantly affected the policy benefits that constituents of each party expected to get out of the legislative process. More specifically, using the “event study” methodology that is used routinely in studies of economics, finance, and law, we look at the effect of the Jeffords switch on the asset values of Republican and Democratic “constituent firms.”

---

2 The Republicans were the majority party when the Senate was divided 50-50, since the Republican Vice President, Dick Cheney, held the tie-breaking vote.
An event study looks at the effect of an unanticipated event on the values of relevant market assets, such as stocks, and determines whether the event had a statistically significant impact on the prices of the given assets. For example, previous event studies have examined the effects of Hurricane Andrew (Lamb 1995) on insurance company stock prices, and of unexpectedly positive or negative earnings announcements on share prices (MacKinlay 1997). Event studies rely heavily on the weak efficient markets hypothesis. That is, a basic component of any event study is the assumption that asset prices efficiently incorporate publicly available information, as well as expectations about shares’ future values; rational investors, moreover, react immediately to new information as it becomes available.

The Jeffords switch is therefore a prime candidate for the event study methodology, since it was unforeseen by the press, the public, and even other members of Congress. If majority status does in fact matter, then we should see significant changes in the stock prices of firms whose interests are likely to be better served by one party or the other; if majority status does not matter, then there should be no difference in share prices before and after the switch, since the membership—and therefore the preferences—of the Senate, House, and president remained constant across this period.

We examine the effects of the switch on asset prices of firms in the energy industry. Prior to the switch, and under unified Republican government, Congress was working on an energy bill that was widely expected to bolster the oil industry. We find that the share prices of oil firms fell significantly in response to news of the Jeffords switch, while share prices of firms involved in environmentally-friendly forms of energy production rose significantly. This pattern is consistent with the predictions of partisan
theories of congressional organization, and inconsistent with theories centered on the preferences of individual legislators.

We proceed as follows. In section two, we lay out the core premises and general predictions of the partisan and preference-based theories of congressional organization, and their relationship to the Jeffords switch. In section three, we derive testable hypotheses regarding the effect of the switch on stock prices that is predicted by each of the theories. We then move on to discuss the event study methodology in greater detail in section four, where we also specify the test that we use to evaluate the competing hypotheses, and describe the exact details of our test. In section five, we present and discuss our results.

2. The role of majority status in theories of congressional organization

Much of the congressional organization literature has boiled down to attempts to answer the question, “are parties an analytically useful feature of Congress?” Proponents of partisan theories (Rohde 1991; Kiewiet and McCubbins 1991; Cox and McCubbins 1993; Aldrich and Rohde 2001) argue that the majority party, through some combination of institutional organization and member discipline, biases legislative decision making to benefit its members. Proponents of non-partisan theories argue that parties add nothing to our understanding that cannot be captured theoretically by ignoring parties and simply focusing on the preferences and goals of members of Congress (Mayhew 1974; Krehbiel 1991, 1998).

In order to use the Jeffords switch as a test of partisan vs. non-partisan theories, we must specify carefully exactly how parties are expected to bias decision making under
partisan theories, and what exactly it means for parties to not be analytically useful under non-partisan theories. Once we do so, we can then derive hypotheses from each, and build a test of these hypotheses.

For these purposes, we focus on the variants of partisan and non-partisan theories that most strongly state the cases for and against parties, respectively. On the partisan side, we therefore rely on Cox and McCubbins’s (2002, 2004) procedural cartel model of majority party agenda control; on the non-partisan side, we use Krehbiel’s (1998) pivot model of the legislative process.

Each of these models is a standard unidimensional spatial model of a legislature. Each is characterized, moreover, by an interval in which status quos are “protected”—that is, no bill that proposes to amend a status quo in the protected interval will pass the legislature. However, the protected intervals (as well as the reasons they are protected) differ between the two models.

In the cartel model, the majority party controls which bills do and do not get final passage votes. This gives them negative agenda control—that is, the ability to block consideration of bills. It uses this power to block consideration of bills that would produce new policies that are dispreferred to the status quo by a majority of majority party legislators. More formally, the model assumes that the majority median \( M_j \) (or party leaders acting in the interests of \( M_j \)) decides whether to allow consideration of any bills dealing with the given dimension \( j \). If consideration of a bill dealing with dimension \( j \) is allowed, the bill is considered under an open rule and passes at the ideal point of the floor median \( F_j \). As shown in Figure 1, \( M_j \) therefore blocks consideration of any bills that propose to amend the status quo \( SQ_j \), if \( SQ_j \) is in the “majority blockout zone” between
If $SQ_j$ is outside the majority blockout zone, however, policy on dimension $j$ will be moved to $F_j$.

Figure 1 here

In the pivot model, by contrast, protected status quos are those that fall in the “gridlock zone” between the left pivot $L_j$ and the right pivot $R_j$. These pivots represent two supermajority requirements embodied in the legislative process—the three-fifths cloture vote needed to end a filibuster in the Senate, and the two-thirds vote needed to override a presidential veto—and are determined *solely* by the preferences of members of Congress and the president. If $SQ_j$ falls outside the gridlock zone, a bill will pass that moves policy on dimension $j$ to within the gridlock zone; if $SQ_j$ falls inside the gridlock zone, then no new policy will be adopted on dimension $j$.

The two models therefore make divergent predictions about which status quos are and are not protected. Specifically (and dropping the subscripts for simplicity), there are two regions of disagreement: first, and most importantly for our purposes, the cartel model says that status quos between $2M-F$ and $L$ are protected, whereas the pivot model says they are not protected. Second, the cartel model says that status quos between $F$ and $R$ are protected, whereas the cartel model says they are not.

Now consider the implications of the Jeffords switch. Until now, we have represented the majority party as a generic left-of-center party. In Figure 2, we show the majority blockout zones under Democratic and Republican majorities (where Dem and Rep are the respective party medians), as well as the gridlock zone. Cartel theory implies that status quos in the interval $(2Dem-F)$ were *not* protected by the pre-switch Republican majority, and *became* protected under the post-switch Democratic majority. Pivot theory,

---

3 See Cox and McCubbins 2002 for details.
on the other hand, implies that there was no change in the set of protected status quo policies when Jeffords switched, since there was no change in House or Senate membership and ideal points and therefore no change in the location of the pivots.

3. Asset prices and hypotheses

Returning now to the subject of the value of majority status to constituents, we can recast the preceding discussion in terms of predictions about asset prices. In this and subsequent sections, we assume that markets react rationally to new information. In addition, we assume that the possibility of Jeffords switching parties was unforeseen until a few days before he announced his decision; that is, we assume that the switch was new information that became available in the days leading up to his switch. Finally, we assume that some firms are Republican constituent firms, and that other firms are Democratic constituent firms. By this, we mean that some firms stand to do much better under the policies espoused by one party than they would under the policies espoused by the other party, and therefore are supporters of one party over the other. Note that we are not assuming that all firms fall into one of these categories. Finally, we assume that firms’ business fortunes are sometimes affected by legislative decisions on dimension $j$, and that the expected business success of any such firm is in part a function of the location of policy on dimension $j$. Given these assumptions, we can draw opposing and testable predictions about the effect of Jeffords’s switch on asset prices of Republican and Democratic firms. Note that the pivot hypotheses serve as the null hypotheses for the cartel hypotheses, and vice versa.
**Cartel Hypothesis**\(_\text{Dem}\): Given dimension \(j\) with a status quo favorable to Democratic constituent firms, Jeffords’s switch will have a positive impact on the stock prices of Democratic constituent firms whose success is a function of policy on \(j\), all else constant.

**Cartel Hypothesis**\(_\text{Rep}\): Given dimension \(j\) with a status quo unfavorable to Republican constituent firms, Jeffords’s switch will have a negative impact on the stock prices of Republican constituent firms whose success is a function of policy on \(j\), all else constant.

**Pivot Hypothesis**\(_\text{Dem}\): Given dimension \(j\) with a status quo favorable to Democratic constituent firms, Jeffords’s switch will have no impact on the stock prices of Democratic constituent firms whose success is a function of policy on \(j\), all else constant.

**Pivot Hypothesis**\(_\text{Rep}\): Given dimension \(j\) with a status quo unfavorable to Republican constituent firms, Jeffords’s switch will have no impact on the stock prices of Republican constituent firms whose success is a function of policy on \(j\), all else constant.

In short, the cartel model predicts that any Democrat-friendly status quos that the Republican majorities had not changed as of the Jeffords switch—but which the markets rationally expected to be changed—suddenly became protected when the Democrats
assumed majority status. The rational reaction was therefore to value more highly the stocks of firms that had figured to be hurt by expected Republican policy changes, but that no longer figured to be hurt once the Democrats gained a veto. Similarly, the rational reaction was to value less highly the stocks of firms that had figured to benefit from expected Republican policy changes, but that no longer figured to benefit once the Democrats gained a veto.

4. An event study test of the hypotheses

To test these hypotheses, we use an event study research design. Despite their prominence in finance and economics, there are few examples of event studies in political science. Accordingly, we begin this section by discussing the details of the methodology, including the implicit research design. We then discuss the methods and issues involved in choosing the set of firms whose stock prices we use for our test, and specify how we chose the set of firms that we use.

4.1. Event Study Methodology and research design

The dependent variable in an event study is the rate of return for a given stock or set of stocks. The rate of return is the change in the total value of a security, or portfolio of securities, over some period of time—which, in the case of most event studies, is one day. This rate is expressed as a percentage of the value at the beginning of the time

---

4 See, e.g., Dolley (1933), Myers and Bakay (1948), Barker (1957), Ball and Brown (1968), Fama et al. (1969), Schwert (1981), Heinkel and Kraus (1987), and Mullin et al. (1995).

5 See, e.g., Gilligan and Krehbiel (1988), Gilligan, Marshall, and Weingast (1990), Roberts (1990a), (1990b), and Schnietz (2003). Indeed, with few exceptions, even event studies in law and political science are published in economics or finance journals.
period. So, for example, if a stock is worth $100 on day \( t-1 \), and is worth $101 on day \( t \), then the rate of return for day \( t \) is 1.01. It is important to note that this measure captures only the one-day change, and does not aggregate long-term changes. To fully appreciate what this means for event studies, consider an example in which a stock’s value is constant for five days, increases 100% on the sixth day, and then remains constant for another five days. Although the stock’s value increased by 100% across the time period, the event study dependent variable—the one-day rate of return—is 0 for all days except the sixth day. Once one understands this, the basic idea of an event study is straightforward—the methodology amounts to asking if the rate of return is significantly different during the period when news of the given event becomes public. The actual econometrics used to conduct event studies take different forms in different event studies, due largely to differing ideas about how best to identify “significantly different rates of return.”

A core concept in all event studies is that there is a “normal” rate of return for each day, which is the rate that would obtain on that day in the absence of any significant event effects, and an “abnormal” return, which is the difference between the normal return and the actual return for that day. The critical tests in almost all types of event studies revolve around determining whether the abnormal return is significantly different from zero on the day of the event, or in a range of days centered around the event if one thinks that the dissemination and impact of new information did not occur entirely in the

\[ R_{it} = \frac{p(t) f(t) + d(t)}{p(t-1)} \]  

where \( p(t) \) is the last sale price at the end of day \( t \), \( f(t) \) is the factor to adjust the price at \( t \) (so that the current and previous price are adjusted to account for any “splits,” increases in the number of outstanding shares with proportional equity maintained among shareholders), \( d(t) \) is the dividend amount, \( p(t-1) \) is the sale price of the security at the end of the day before \( t \).
24 hours between the close of trading on day \( t-1 \) and the close of trading on day \( t \). The range of days in which one looks for an event’s impact on stock prices is called the *event window*.

The normal return for each day during the event window is estimated by modeling returns on the given stock (or portfolio) across a range of days that precedes the event window, and then using this model to forecast what the “normal” return would have been for each day in the event window. The range of days that is used to generate this forecast is called the “estimation window,” and typically consists of the 100-200 days leading up to the event window. Modeling and forecasting of normal returns can be done in various ways. The simplest is to calculate the mean price of the stock across the estimation window, and to then forecast this mean return as the normal return for each day in the event window. A less crude method is to control for broad changes in the market when calculating normal returns, thereby accounting for possible confounding effects of events that affect the market as a whole. In its simplest form, this type of “market model” consists of regressing a stock’s daily return rate on the daily return rate of a broad market index across the estimation window, then using the estimated coefficients and the observed market rate of return for each day in the event window to predict the normal return for each event window day. Although such market models are not the most sophisticated option for calculating normal returns, the conclusion found in studies of the various techniques for forecasting normal returns is that market models perform *at least*

---

\(^7\) Obviously, for purposes of hypothesis testing, the effect of an event not only must be significant, but must also be in the hypothesized direction.
as well as more sophisticated models (MacKinlay 1997), and that the market model is “simply the best available model at this time” (McWilliams and Siegel 1997, 629).⁸

Event studies can also differ from each other along another dimension that is orthogonal to the choice of which model to use for forecasting normal returns, though market-model forecasting is usually used (and is what we describe here). This second dimension consists of the exact econometric methodology that is used to determine if the timing of the given event is associated with significant abnormal returns. In practice, one of two methods is usually used.

At this point, we shift gears a bit in our discussion of event study methodology. Until now, we have discussed it largely in terms of a single stock. In practice, however, an event study is almost always conducted upon a set of stocks; that is, the study attempts to identify the effect of an event on a group or class of stocks that is of theoretical interest. This is the case for two reasons—first, we are usually interested in testing general theories, and rarely find the prices of a single stock to be either theoretically interesting or empirically convincing. Second, for econometric reasons, any significant effect of an event is more likely to be detected by the methodology as the size of the sample of stocks increases, all else constant (MacKinlay 1997).

The first method is to calculate abnormal returns for each day in the event window, and to use a test statistic to determine if the abnormal returns are significantly different from zero. So, for a sample of stocks, one takes the sample mean return (i.e., the average of the individual returns for the stocks in the sample) for each day in the

---

⁸ An typical example of a more sophisticated method is the Capital Assent Pricing Model (CAPM), which includes features of each individual firm in the model of that firm’s normal return. As noted, however, these models have proven to be more complicated with little value added, since they do not outperform
estimation window, forecasts the normal sample mean for each day in the event window, and subtracts this forecast from the observed sample mean to generate a sample abnormal return for each day in the event window (this approach was developed by Ball and Brown 1968 and Fama 1969, and refined by Brown and Warner 1985). Then, using the variance of the sample mean return across the estimation window, a test statistic is used to determine whether the sample average abnormal returns during the event window are significant. If so, then the event is said to have had a significant effect.\(^9\)

The utility of this approach declines, however, under a variety of circumstances, such as when the event window grows beyond a single day. For our purposes, the circumstance that is most relevant is when the event under study affects all sample stocks at the same time. To clarify this distinction, it is useful to elaborate on the types of events that are commonly studied. For simplicity, we have discussed events—like Jeffords’s switch—that occur at one point in time and affect all sample stocks at the same time. In practice, many event studies examine the effect of a class of event, such as companies’ earnings announcements, that occurs at different times for different individual stocks in the sample. The tests statistics described above are predicated on the assumption that each sample stock constitutes an independent opportunity for the event to affect stock prices. But this independence assumption is violated when the event in question is contemporaneous (i.e., it occurs at the same time for all sample stocks), and the test statistics therefore yield flawed inferences.

---

\(^9\) We are deliberately vague about the exact way in which estimation-window variance is used to generate a test statistic, because it is done in different ways, depending upon other factors such as how normal returns are forecast. This is not the methodology that we use in this paper. See Ball and Brown 1968; Fama 1969; Brown and Warner 1985; Schwert 1981 for details.
We therefore rely on the second method, which employs time-series-cross-section generalized least squares (GLS) regression in which the cross-sectional unit is an individual firm’s stock and the time series is each day in the estimation and event windows (Schipper and Thompson 1983; see also Malatesta 1986, Angbazo and Narayanan 1996, McWilliams and Siegel 1997). An important advantage of this method is that it explicitly takes into account the non-independence and cross-sectional correlation of abnormal returns that accompany a contemporaneous event. In addition, it makes more efficient use of data, is better suited for smaller sample sizes, and allows for more powerful hypothesis testing (i.e., joint hypotheses and tests about average and cumulative abnormal returns).

The GLS regression model is

$$\tilde{R}_{it} = \beta_0 + \beta_{im} \tilde{R}_{mt} + \sum_{k=1}^{K} \beta_{ik} D_{kt} + \tilde{e}_{it}$$  \(1\)

where \(\tilde{R}_{it}\) is the rate of return for firm \(i\) on day \(t\) \((i = 1, 2, \ldots, N)\); \(\tilde{R}_{mt}\) is the rate of return for the market index on day \(t\); \(D_{kt}\) is a dummy variable equal to one if event \(k\) occurred on day \(t\), and zero otherwise; and \(\tilde{e}_{it}\) is an error term (assumed to be normally distributed and serially independent) for firm \(i\) on day \(t\).

Note that the constant term \(\beta_0\) and the market index term \(\beta_{im} \tilde{R}_{mt}\) jointly constitute the market model, and estimate the normal return. The event dummy term \(\beta_{ik} D_{kt}\) then

---

10 Note that for this GLS approach the “estimation window” consists of day both before and after the event window. In our case, we include all trading days in 2001.

11 For our analysis, we used the CRSP NYSE-AMEX value-weighted index.
captures the abnormal return, which is the impact of the event on each firm’s rate of return.

With this specification, we can test each of our hypotheses—namely, that Republican firms should experience significantly negative abnormal returns, and that Democratic firms should experience significantly positive abnormal returns—in a number of ways (that is, we can use a number of methods to evaluate the hypothesis regarding constituent stocks for each party). First, we can test the prediction (H₁) that the event had a significant effect on each individually (hereafter the “individual impact” test), with a null hypothesis (~H₁) that they are all zero:

\[ \sim H_2: \sum_{i} \delta_{ik} = \sum_{i} b_{ik} = 0 \text{ for each event } k \]

\[ H_{12}: \text{these are not all zero} \]

Because we expect the Jeffords’s switch to affect all of our Republican firms one way, and all of our Democratic firms the other, a second, more appropriate test is to measure the effect on the summed sample-wide abnormal return. Here, for Republican firms, the hypothesis is that the summed effect (hereafter the “aggregate impact” test will be negative (H₂), with a null hypothesis (~H₂) that this sum will be weakly positive (for Democratic firms, the hypothesis is the opposite):

\[ \sim H_3: \sum_{i} \sum_{k} \delta_{ik} \geq 0 \text{ for each event } k \]

\[ H_3: \sum_{i} \sum_{k} \delta_{ik} < 0 \text{ for each event } k \]
To verify that outliers are not driving our results, we can use a sign test (McWilliams and Siegel 1997), which gauges whether the proportion of positive to negative returns (i.e. the proportion of positive event coefficients, $b_i$) exceeds that to be expected from the market model. The z-statistic to perform this test is given by

$$G - \frac{N}{p} \sqrt{Np(1-p)}$$

where $G$ is the number of negative sample coefficients, $N$ is the total number of sample coefficients, and $p$ is the probability of a negative estimate under the null hypothesis (0.50). Note that when the z-statistic takes on a negative and significant value, it indicates that the percentage of positive sample coefficients is significant (the opposite is true when the z-statistic is positive and significant). If the z-statistic is insignificant, or significant but in the wrong direction, then we must be concerned that outliers may be a problem.

A third test, which mimics the intuition behind the aggregate impact test, is to create a portfolio average return by taking the mean of the returns for all Republican (or Democratic) stocks for each day, and to then regress that mean return on the market index and a dummy variable. More formally:

$$\frac{1}{K} \sum_{i=1}^{N} \tilde{R}_t = \tilde{\alpha}_p + \tilde{\beta}_p \tilde{R}_t + \sum_{k=1}^{K} \tilde{\delta}_p D_{kt} + \tilde{\varepsilon}_t \quad (2)$$

where the parameters and values are the same as before, with the exception that they refer to the portfolio $p$ rather than to individual stocks. Note that the form of this equation is
very similar to the GLS method. The key difference is that this method is a simple time series of the average return, rather than a cross-sectional time series. Thus the third hypothesis (hereafter the “portfolio impact” test) (H₃) is that the portfolio’s abnormal return should be negative (positive) and significant for Republican (Democratic), with a null hypothesis (~H₄) that the effect will be weakly positive (negative):

\[ ~H₄: \sum_{k} b_{pk} \geq 0 \text{ for each event } k \]
\[ H₄: \sum_{k} b_{pk} < 0 \text{ for each event } k \]

As mentioned, a key to the utility of this research design is “the fact that, given rationality in the marketplace, the effects of an event will be reflected immediately in security prices” (MacKinlay 1997, p. 13). That is, event studies assume that rational investors, operating in efficient markets, will accurately derive the economic impact of new information on the long-run profitability of firms, and adjust their stock price bids up or down accordingly (Schnietz 2003). Thus, when a surprise occurs, the resulting change in asset prices can be attributed to new, unanticipated information.

As a research design, this is very appealing. A fundamental problem facing much of political science research is generating a valid experimental design, from which causality can be safely inferred, in an uncontrolled setting (i.e. outside of the laboratory). In an event study, the time between pre-test (pre-event stock price), treatment (event), and post-test observation (post-event stock price) is a matter of minutes, hours, or, at most, days.¹² Thus, while event studies do not allow for a controlled laboratory setting,

¹² Note that event studies are textbook examples of regression discontinuity designs, outlined by Trochim (2001).
the short time horizon decreases the potential “history threat” (that is, the possibility that stock price changes were caused by a confounding factor other than the event under study) and thus boosts the internal validity of the research design. In terms of the theory at hand, an event study research design allows us to capture part of the policy impact of the Jeffords switch immediately, despite the fact that the actual policy impact may not have materialized for some time after.

In conducting an event study, a researcher must consider several potential pitfalls, which we address here. First, is it safe to assume that the policy impact expected by market investors is a reliable measure of actual policy impact? We argue that it is. With billions of dollars at stake, market traders have plenty of incentive to be both attentive and well informed. In equilibrium, investors that incorrectly predict or fail to anticipate the financial impact of some class of events, such as political events, will become extinct from the investment pool. Certainly, we would not want to draw an inference from the actions of a single market trader; but in aggregate, investor evaluations of policy impact, measured by changes in stock prices, tend to be very accurate (c.f., Schwert 1981, Gilligan and Krehbiel 1988).

Second, even if we believe that markets are efficient, the event under study needs to have been unanticipated by investors in order for the event study method to capture the event’s impact. As McWilliams and Siegel (1997) warn, failing to find that an event affects stock prices may reflect a lack of surprise, rather than a lack of effect. Put another way, a researcher who finds that an event had the hypothesized effect can be confident both that the event had an effect, and that the event was a surprise (to at least some
extent). A researcher who finds no effect can conclude that one or both of these conditions was unmet.

Because of this potential pitfall, we have taken pains to assure ourselves that Jeffords’s switch was not anticipated prior to the period of a few days before the switch, when speculation about the switch being about to occur became front-page news. We reviewed major newspapers from January 1st to May 20th of 2001, looking for signs that anyone expected Jeffords to switch parties. Though his relationship with fellow Republicans was clearly strained in the months before his switch, his decision was very much a surprise. Our search does not reveal even a hint that Jeffords was about to switch parties.¹³

Third, McWilliams and Siegel (1997:636) suggest that the size of the event window (recall that this is the range of days allotted as the event or “treatment” period) “is possibly the most crucial research design issue.” Indeed, given the assumption of efficient markets, a long event window seems hard to justify theoretically since it implies that information about an event was incorporated slowly into the market.¹⁴ Instead, the event window should begin when news of the event first makes its way into the market, and end when it has been fully incorporated into market prices. In the next subsection, we will detail our event window, and how we determined which days to include.

Finally, even with a relatively short event window, we must be concerned about confounding events; that is, events that are contemporaneous with the “treatment,” and

---

¹³ This is based on a search of Lexis-Nexis. We searched for the word “Jeffords” in the “full text” of the “general news” category, searching only “major newspapers.” This generated 425 hits (many of which were repeat stories published in different newspapers; some were not related to Senator Jeffords, but instead another Jeffords). We then read through each, looking for any mention of the possibility that Senator Jeffords might switch parties, and found no such speculation.
are expected to affect the relevant set of stocks. If such events exist, we cannot draw causal inferences from price changes of the potentially “contaminated” stocks.\textsuperscript{15}

4.2. Operationalizing our hypotheses

In this section, we explain the details of our analysis, beginning with our choice of a policy issue, and moving on to our method of stock selection and our choice of an event window.

4.1.1. Choosing a policy issue

The first step for us in conducting an event study test of our hypotheses is to identify at least one policy dimension on which the ex ante (i.e., pre-switch) status quo was favorable to Democratic constant firms, which is an important condition of our hypotheses. Ideally, we would also like to be able to demonstrate that the Republican majority intended to amend this status quo in a way that would move policy rightward. We began our search for such dimensions by combing through the Republican party platform from the 2000 election, as well as the president’s address to Congress in which he spelled out his policy goals. We identified a number of major policy goals that were repeatedly mentioned, such as a tax cut, increased defense spending, increased oil production, privatized education, and tort reform. Some of these issues, such as the tax cut, were not suitable for our analysis because Republicans had already enacted new legislation (i.e., changed the status quo) by the time of the Jeffords switch.

\textsuperscript{14} Of course, it is possible that information about the “event” of interest is released very slowly. In this case, a longer event window may be theoretically justified, but the increased potential for history threat may then offset the utility of the research design.

\textsuperscript{15} Where relevant events occur outside of the event window, but during the sample period (i.e., the period used to gauge the “normal” stock return), there is no expected bias in the event study results, though standard errors may be inflated, increasing the possibility of Type II error; in other words, it may reduce the chances that we will be able to reject the null hypothesis.
In order for us to be able to conduct an event study, another necessary characteristic of any policy dimension is that firms affected by the policy choice are publicly traded—obviously, without stock prices, we cannot do an event study. This requirement narrowed the field of candidate dimensions sharply, particularly since many of the Democratic constituents likely to be hurt by proposed Republican legislation were labor unions, teachers’ unions, public schools, and trial lawyers—all of which do not offer publicly traded stocks.

Eventually, we identified energy policy as the issue dimension that most clearly fits the necessary conditions. One of the most controversial and well-publicized goals of the unified Republican government that emerged from the 2000 elections was the opening of the Arctic Wildlife Refuge for oil production. In fact, this was the symbolic tip of the iceberg. In the months preceding Jeffords’s switch, a much-ballyhooed Republican energy bill worked its way through Congress; among other things, the bill emphasized expansion of oil and gas production, and did little to bolster conservation or “clean energy.” It was seen as so generous to Republican-friendly oil and gas firms that one journalist was prompted to remark that, “when [oil and gas lobbyists] got to Congress on Tuesday, there was little they were asking for that President Bush hadn't already offered them” (Soraghan 2001).

In addition to the status quo being friendly to Democratic firms, and being expected to change dramatically, this is an issue dimension on which we can identify publicly traded firms that figured to be helped and hurt by the impending energy bill. On the one hand, oil and gas companies were expected to reap a windfall; on the other hand, companies involved in more environment-friendly tasks such as producing clean energy,
or environmental cleanup, were set to see their fortunes decline. These are therefore the kinds of stocks that we use in our analysis.

4.1.2. Stock selection

There are a number of ways that we might choose a sample of oil and gas firms to include in our analysis. At one extreme, we could include every publicly traded firm in these businesses. A problem with this approach, however, is that it is unlikely that all oil and gas firms have the same stakes in the political process, since not all are involved in aspects that would have benefited from the Republican energy bill. Accordingly, we adopt a method that will increase the signal of our test, by using stocks of firms that signaled, via campaign contributions, that they had a stake in the political process. To identify such firms, we first compiled a list of all publicly traded oil and gas firms that, during the 1992-2000 election period, were associated with a corporate PAC and donated money to a candidate for federal office.\footnote{To identify “Oil and Gas” corporate PAC’s, we used the data provided at www.opensecrets.com.} We then excluded firms that had more than two consecutive days of missing data in the Center for Research in Securities Prices (CRSP) database during our time series (the 248 trading days in 2001).\footnote{For stocks with only one or two days of missing data, following common practice, we replaced the missing value with a 0. (Heinkel and Kraus 1988) That is, we pretended as thought the stocks price for the missing days was the same as the price on the previous day.} This left us with 45 stocks, including most of the largest oil companies (see the appendix for a full list).

Ideally, we would have selected our renewable energy stocks in identical fashion. Unfortunately, this would have left us with a sample size of 0, due to the fact that there were no renewable energy corporate PAC’s that contributed money during the 1999-2000 election cycle. We therefore were obliged to make due with the potentially noisy signal.
sent by a sample of all publicly traded renewable energy companies.\textsuperscript{18} After again excluding those firms with more than two consecutive days of missing data during 2001, this gave us 20 firms, including companies that produce wind energy, hydrogen energy, and water power (again, see the appendix for a full list).

4.2.3. Defining the event window

Recall that, when defining our event windows, we want them to begin as soon as news of our event makes its way into the market, and to end as soon as news of the event has been fully incorporated. Based on the following timeline, we define the window as the three-day period from May 22\textsuperscript{nd} to May 24\textsuperscript{th}.

Late on Monday, May 21\textsuperscript{st}, probably after the markets closed (although this is difficult to say for sure) news began to surface that Jeffords might abandon the Republican Party. On the 22\textsuperscript{nd}, this was widely reported in newspapers. On the 23\textsuperscript{rd}, it was initially expected that Jeffords would make a statement. Instead, during the trading day on the 23\textsuperscript{rd}, information surfaced that Jeffords would hold a press conference the following day, and reports increasingly suggested that he had in fact decided to leave the Republican Party. On the 24\textsuperscript{th}, at 2:00p.m. EST (two hours before east coast markets closed), Jeffords announced his plan to switch from Republican to independent, and made it absolutely clear that he would either vote with the Democrats or abstain on organizational matters, thereby giving Democrats effective control of the Senate.

Speculation on the 22\textsuperscript{nd} and 23\textsuperscript{rd} about Jeffords possibly switching likely caused many investors to dramatically increase their estimates of the probability of him switching from near zero, to decidedly more than zero. On the 24\textsuperscript{th}, though many people

\textsuperscript{18} The list of “all” publicly traded renewable energy companies was taken from http://energy.sourceguide.com.
doubtless thought it likely that he would switch, there was still uncertainty until he actually announced his intentions. The actual announcement may therefore have still constituted a sizable “surprise,” in the sense that it may still have conveyed significant new information that affected investor behavior.

Lacking any *a priori* basis for knowing if the impact of the event was felt most strongly on the first, second, or third day of the window, we code the dummy variables that operationalize this window in two different ways. First, we use separate dummies for each of the three days, thereby treating each day as a separate event. This fine-grained approach allows us to detect significant effects on any of the three days in the window. The alternative approach is to use a single dummy that is coded one for all days in the window, thereby treating the three days as a joint event. As already discussed, this approach has the drawback of being more prone to Type II error—that is, rejecting a relationship that does in fact exist. Thus, this is the second event window that we use.

Finally, to account for the possibility of confounding events, we searched online sources of financial news (using the *Lexis-Nexis* “keyword” option), for industry events affecting oil, gas, or renewable energy stocks from May 22\textsuperscript{nd} through May 24\textsuperscript{th}. We found no evidence of contemporaneous events affecting either set of stocks on any of these days.

5. The effects of the switch on stock prices

5.1. Results

Table 1 presents GLS results for oil and gas stocks. The columns, from left to right, show the results of the aggregate impact, individual impact, portfolio impact, and
sign tests. The first three rows show the estimated effect of the event for each individual day in the event window, and the fourth row shows the effect for the joint three-day window. Though we cannot reject the null hypothesis for the individual test on the 22\textsuperscript{nd}, the aggregate impact test, which most closely captures the prediction from our theory, reveals a highly significant and negative abnormal return (p>.03). This implies that investors quickly made the connection between the increase in the probability of a Democratic Senate, based on the rumors that surfaced on the 21\textsuperscript{st} and 22\textsuperscript{nd}, and the long-term valuation of firms in the oil and gas industry.

While we cannot reject the null hypothesis for either of our tests on the 22\textsuperscript{nd} or 23\textsuperscript{rd} (which is unsurprising, given that, on the 22\textsuperscript{nd}, investor behavior seems to reflect a significant increase in the estimated probability of a Jeffords defection), the abnormal returns are negative for both days, and are nearly significant on the 24\textsuperscript{th} (p>.13). This overall negative trend is captured in the three-day event window (May 22\textsuperscript{nd}-24\textsuperscript{th}), for which we can reject the null hypothesis of no effect (p>.01) in favor of the alternative hypothesis that oil and gas stock prices declined significantly over that period. Finally, note that for each day individually, more than 50\% of firms had negative abnormal returns (the percent negative is significant for the 22\textsuperscript{nd}, 23\textsuperscript{rd}, and the three day window combined), which suggests that the effect was felt broadly within our sample of stocks.

Table 2 presents the results for renewable energy stocks over the same period. For both the 22\textsuperscript{nd} and the 23\textsuperscript{rd}, we can reject the null of no effect, as reflected by the portfolio test on both days (p>.09 for the 22\textsuperscript{nd}, p>.01 for the 23\textsuperscript{rd}). Unexpectedly, the abnormal returns are negative for those days. On the 24\textsuperscript{th}, however, we can reject the null hypothesis, with both the aggregate (p>.03) and portfolio (p>.01) tests, in favor of the
alternative hypothesis that the abnormal returns for that day were positive and significant (which seems to have affected the sample broadly, based on the significant sign test). Though we could not find any secondary evidence regarding the negative abnormal returns on the previous two days, it is clear from accounts by market observers that the “rally” in renewable energy was a reaction to Jeffords’ switch.\(^{19}\) For the three-day event window, the positive and negative abnormal returns seem to cancel each other out, as we are unable to reject the null hypothesis.

5.2. Discussion

To believe the conclusions drawn from an event study, the reader must believe that markets are efficient. In the context of the present application, this requires that you believe that investors make good (not perfect, merely unbiased) predictions with regards to the direction and magnitude of policy change. Given the financial incentives of investors, this seems a reasonable assumption. If it is believed, then the market’s reaction to an event represents a change in expected policy outcomes, which must in equilibrium represent an unbiased prediction about actual policy effects.

Thus, the pattern of investor reactions to the Jeffords switch strongly suggests that the change from Republican to Democratic control of the Senate had significant, predictable effects on energy policy. Even controlling for the general trend of the market, which tends to take a hit when Republicans lose office (see Herron et al. 1999 for a review), oil and gas stocks had significant negative abnormal returns in reaction to the Jeffords switch. Conversely, the returns of renewable energy stocks rose well above the level predicted by the market model, yielding significant positive abnormal returns on the day that Jeffords announced his switch.

These findings lend support to partisan theories of congressional organization. Because the membership and preferences of the Senate remained constant over the few days surrounding Jeffords’s switch, we can be confident that the change in majority status is responsible for the observed patterns of change in the stock prices of Democratic and Republican constituent firms.

We cannot, however, reconcile this pattern with theories that ignore the influence of parties in legislative process. According to Krehbiel’s (1998) model, the block out zones should have remained constant during the days surrounding the Jeffords switch. However, we can reject the prediction that follows; that the value of Republican and Democratic constituent firms remained constant over this period.

6. Conclusion

The central conclusion of this paper is that majority party status in Congress is valuable. In this paper, we have provided direct evidence that it is valuable to the controlling party’s “constituent firms.” More impressive, however, are the broader implications of our results for the congressional organization debate. The Jeffords switch is the only instance in the history of the United States where majority status changed but member preferences remained absolutely constant. Thus, it allows us the extremely rare opportunity to compare partisan and non-partisan theories of congressional organization as though Congress were a controlled laboratory setting. Based on the “treatment” offered by Jeffords’s defection – controlling for all other factors – the lab tests have come back positive: parties matter.

This finding is particularly impressive given that they bear directly on partisan influence in the Senate. Indeed, with few exceptions (e.g. Campbell, Cox, and
McCubbins 2002), most evidence of party effects has come from research on the House of Representatives. By all accounts, the Senate is clearly the toughest case for partisan theories of legislative organization. Yet, our results suggest that there is strong partisan influence in Senate decision-making.

Finally, these results are encouraging for scholars that have struggled with the difficulties of measuring public policy impact. That is, they suggest that investors do monitor and react to expected changes in policy, making event studies an excellent option for pursuing both the policy impact of political events and the causal origins of eventual policy change. Indeed, to these ends, the gains in validity, and thus the ability to safely establish causality, make events studies not only adequate, but, in many cases, optimal for studying public policy.
# Appendix A:

## Companies Included in Samples

### Oil and Gas Companies

- Exxon Mobil Corp.
- Peoples Energy Corp.
- Chevron Corp.
- Sunoco Inc.
- Unocal Corp.
- USX Marathon Group
- AGL Resources Inc.
- Enron Corp.
- Synergy Inc.
- Halliburton Company
- Ashland Inc.
- Equitable Resources Inc.
- TXU Corp
- Oneok Inc.
- National Fuel Gas Co.
- Kerr Megee Corp.
- Energen Corp.
- Questar Corp.
- Murphy Oil Corp.
- BP Plc.
- Occidental Petroleum Corp.
- Tesoro Petroleum Corp.
- Williams Cos
- Nisource Inc.
- Apache Corp.
- Nicor Inc.
- Piedmont Natural Gas Inc.
- Dominion Resources Inc.
- Anadarko Petroleum Corp.
- Burlington Resources Inc.
- Cabot Oil & Gas Corp.
- Cross Timbers Royalty Trust
- Ultramar Diamond Shamrock Corp.
- Torch Energy Royalty Trust
- McMorran Exploration Co.
- Valero Energy Corp.
- El Paso Energy Partners LP
- Pennzoil Quaker State Co.
- Devon Energy Corp.
- Tidewater Inc.
- Mitchell Energy & Dev Corp.
- Frontier Oil Corp.
- Northwest Natural Gas Co.
- Southwest Gas Corp.
- Southwestern Energy Co.

### Renewable Energy Companies

- Energy Conversion Devices Inc.
- Thomas & Betts Corp.
- Honda Motor Ltd.
- Spire Corp.
- UQM Technologies Inc.
- Valence Technology Inc
- Fuelcell Energy Inc.
- Exide Corp.
- Electric Fuel Corp.
- Astropower Inc.
- Evercel Inc.
- Plug Power Inc.
- Energizer Holdings Inc.
- Capstone Turbine Corp
- DCH Technology Inc.
- Active Power Inc.
- H Power Corp.
- Millennium Cell Inc.
- Proton Energy Systems Inc.
- Evergreen Solar Inc.
References


Table 1: Oil and Gas Stocks

<table>
<thead>
<tr>
<th></th>
<th>Aggregate Impact Test t-statistic (p-value)</th>
<th>Individual Impact Test $\hat{\omega}$-value (p-value)</th>
<th>Portfolio Test Coefficient (p-value)</th>
<th>Sign Test Percent Negative (z-stat)</th>
</tr>
</thead>
<tbody>
<tr>
<td>May 22\textsuperscript{nd}</td>
<td>-1.87** (.03)</td>
<td>24.06 (.49)</td>
<td>-.0084 (.25)</td>
<td>73%*** (3.13)</td>
</tr>
<tr>
<td>May 23\textsuperscript{rd}</td>
<td>-.84 (.20)</td>
<td>24.37 (.49)</td>
<td>-.036 (.39)</td>
<td>64%** (1.94)</td>
</tr>
<tr>
<td>May 24\textsuperscript{th}</td>
<td>-1.11 (.13)</td>
<td>22.62 (.49)</td>
<td>-.0052 (.34)</td>
<td>58% (1.04)</td>
</tr>
<tr>
<td>May 22\textsuperscript{nd}-24\textsuperscript{th}</td>
<td>-2.22*** (.01)</td>
<td>31.43 (.46)</td>
<td>-.0057 (.22)</td>
<td>69%*** (2.53)</td>
</tr>
</tbody>
</table>

* significant at 10% level, one-tail test  
** significant at 5% level, one-tail test  
***significant at 1% level, one-tail test
Table 2: Renewable Energy Stocks

<table>
<thead>
<tr>
<th></th>
<th>Aggregate Impact Test</th>
<th>Individual Impact Test</th>
<th>Portfolio Test</th>
<th>Sign Test</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>t-statistic (p-value)</td>
<td>2-value (p-value)</td>
<td>Coefficient (p-value)</td>
<td>Percent Negative (z-stat)</td>
</tr>
<tr>
<td>May 22\textsuperscript{nd}</td>
<td>-.49 (.31)</td>
<td>10.38 (.48)</td>
<td>-.0068* (.09)</td>
<td>65% (1.34)</td>
</tr>
<tr>
<td>May 23\textsuperscript{rd}</td>
<td>-1.12 (.13)</td>
<td>8.53 (.45)</td>
<td>-.0155*** (.01)</td>
<td>70%** (1.79)</td>
</tr>
<tr>
<td>May 24\textsuperscript{th}</td>
<td>1.86** (.03)</td>
<td>16.47 (.34)</td>
<td>.0255*** (.01)</td>
<td>30%** (-1.79)</td>
</tr>
<tr>
<td>May 22\textsuperscript{nd} - 24\textsuperscript{th}</td>
<td>.14 (.44)</td>
<td>4.21 (.50)</td>
<td>.0011 (.35)</td>
<td>50% (.00)</td>
</tr>
</tbody>
</table>

* significant at 10% level, one-tail test
** significant at 5% level, one-tail test
*** significant at 1% level, one-tail test
Figure 1. Protected intervals: the majority party blockout zone and the gridlock zone
Figure 2. Protected intervals under Democratic and Republican majorities