University of California
Los Angeles

Essays in Finance and Economics

A dissertation submitted in partial satisfaction
of the requirements for the degree
Doctor of Philosophy in Management

by

Florian Schulz

2014

Abstract of the Dissertation

Essays in Finance and Economics

by

Florian Schulz

Doctor of Philosophy in Management

University of California, Los Angeles, 2014

Professor Mark J. Garmaise, Chair

The first chapter of this dissertation revisits discount rates. In a recent article *On the Timing and Pricing of Dividends*, van Binsbergen, Brandt, and Koijen (2012) empirically investigate term structure properties of the equity premium by recovering prices of the short-term component of the market index. Their finding of a downward-sloping term structure of the equity premium contradicts predictions of many leading consumption-based asset pricing theories. In this paper, I provide an alternative explanation and show that the higher average rate of return on the short-term component of the index, as extracted by BBK, is fully explainable by the fact that investors are compensated for the extra tax burden of dividends on the ex-date and does *not* represent compensation for risk.

The second chapter of this dissertation is co-authored with Raymond Fisman and Vikrant Vig and is titled "The Private Returns to Public Office". In this paper, we study the wealth accumulation of Indian state politicians using public disclosures required of all candidates. The annual asset growth of winners is 3-5 percent higher than runners-up, a difference that holds also in a set of close elections. The relative asset growth of winners is greater in more corrupt states and for those holding ministerial positions. These results are consistent with a rent-seeking explanation for the relatively high rate of growth in winners' assets.

The dissertation of Florian Schulz is approved.

Ivo I. Welch

Pierre-Olivier Weill

Richard W. Roll

Mark J. Garmaise, Committee Chair

University of California, Los Angeles

2014

To my mother and my wife

for always being there for me.

Table of Contents

List of Figures

List of Tables

Acknowledgments

I am indebted to my dissertation committee, and particularly to Ivo Welch and Mark Garmaise, for their tremendous advice and continual support during my graduate studies at UCLA. I thank Vikrant Vig, an early mentor in my career, for his support and for challenging me intellectually throughout these years. The second chapter of this dissertation would not exist without my co-authors Vikrant and Ray.

Vita

2003–2005	Diploma program in Business and Economics (Vordiplom in 2004), Johann Wolfgang Goethe-University, Frankfurt, Germany.
2005–2006	MBA, University of Iowa, Iowa City, IA, USA.
2007–2008	MSc in Finance, London Business School, London, UK.
2010-2014	Teaching Assistant, UCLA Anderson, Los Angeles, California.

Chapter 1

On the Timing and Pricing of Dividends: Revisiting the Term Structure of the Equity Risk Premium

1.1 Introduction

Many leading theoretical models in consumption-based asset pricing predict a non-decreasing *term structure of the equity risk premium*: discount rates of risky cash flows should increase with maturity (e.g., Campbell and Cochrane (1999), Bansal and Yaron (2004)) or be flat (e.g., rare disaster models as in Barro (2006) or Gabaix (2012)).

In their recent article "On the Timing and Pricing of Dividends", van Binsbergen, Brandt, and Koijen (2012) (BBK hereafter) *empirically* analyze the term structure properties of the equity premium by recovering prices of the short-term component of the market index. This *short-term asset*, or dividend strip, pays the dividends on the index over the near horizon only, and hence is short-term relative to the index. Their clever empirical approach presents many novel results, which are inconsistent with many leading theories in asset pricing. The authors find that expected returns, Sharpe ratios, and volatilities on the short-term asset are higher than on the index, despite betas of less than one. While most leading theories predict an upward-sloping term structure of the equity premium, BBK find that discount rates of risky cash flows are highest at short-horizons. This presents a novel empirical challenge for economists' understanding of risk and return relationships in equity markets and has motivated new theoretical work in asset pricing to reconcile this finding (e.g., Belo, Collin-

Dufresne, and Goldstein (2012); Ai, Croce, Diercks, and Li (2012)).

My paper examines the robustness of the downward-sloping term structure of the equity premium and provides a simple alternative *non-risk-based* explanation for BBK's finding. BBK derive their approach from simple no-arbitrage arguments assuming friction-less markets. However, the empirical application is in an environment that incorporates significant market imperfections, most importantly, taxation differences between dividends and capital gains that affect the measurement of returns. Motivated by the stylized fact of significant pre-tax excess returns on ex-dividend days, I replicate the return series of the *short-term asset* but adjust returns to account for estimated tax rate disadvantages of dividends. I show that the higher average rate of return on the short-term component of the index, as extracted by BBK, is fully explainable by the fact that investors are compensated for the extra tax burden of dividends on the ex-date. After adjusting returns to make them comparable, one no longer observes a downward-sloping term structure: if anything, the average return on the *short-term asset* appears to be lower than that on the overall index, consistent with the aforementioned asset pricing theory predictions.

My measurement-based as opposed to risk-based explanation of the result in BBK is further supported by an analysis of subsamples with different US tax regimes that help explain the time variation patterns shown in BBK. Rather than time variation in the term structure of the equity risk premium, substantial tax cuts for dividends in 2003 seem to be the main reason for subsequently lower returns of the *short-term asset*, another result reported in BBK. Finally, I replicate BBK and extend the analysis to include alternate maturities and segments of the term structure. By varying maturities, I can *mechanically* manipulate the dividend yield of the *short-term asset*. The strong empirical relation that exists between dividend yields and measured returns – and which is *independent of risk* – is also observed for these synthetic dividend assets. This highlights the conclusion that the downward-sloping term structure of the equity premium documented in BBK is largely a result of differential tax implications of capital gains and dividends and does not represent a term structure of risk.

The rest of this article is organized as follows: Section 1.2 reviews and summarizes the approach used in BBK. Using a simple stylized model, section 1.3 motivates the need for an adjustment of the return series in BBK. I analyze the impact of market imperfections, specifically tax rate differences of dividends and capital gains, on measured returns. Section 1.4

describes data and section 1.5 presents empirical results for the more recent history of US stock market data. Section 1.6 reviews implications for BBK and adjust returns of the *short-term asset* using my estimates of implied tax rate measures. In section 1.7, I exploit variation in US tax regimes to show robustness of my results and further replicate and extend the BBK analysis to demonstrate that dividend yields and *not* risk drive the return patterns observed in BBK. Section 1.8 concludes.

1.2 Dividend Strips and the Term Structure of the Equity Premium

Using notation similar to van Binsbergen, Brandt, and Koijen (2012), this section briefly summarizes the main framework used to estimate the term structure of the equity premium. Assuming absence of arbitrage opportunities, there exists a stochastic discount factor M_{t+1} that can be used to discount cash flows. Using the discount factor, the value of any asset, S_t (here, S&P 500 index), can thus be represented as sum of discounted future cash flows, $(D_{t+i})_{i=1}^{\infty}$ (here, dividends). Further, one can decompose this sum of discounted future cash flows into short-term and long-term components:[1]

$$S_t = \sum_{i=1}^{\infty} E_t(M_{t:t+i} D_{t+i}) = \underbrace{\sum_{i=1}^{T} E_t(M_{t:t+i} D_{t+i})}_{\text{Price of the } \textit{short-term asset}} + \underbrace{\sum_{i=T+1}^{\infty} E_t(M_{t:t+i} D_{t+i})}_{\text{Price of the long-term asset}} \tag{1.1}$$

where $M_{t:t+i} = \prod_{j=1}^{i} M_{t+j}$. No-arbitrage also implies that put-call parity for European options (Stoll (1969)) holds on the index S_t:

$$c_{t,T} + Xe^{-r_{t,T}(T-t)} = p_{t,T} + S_t - \mathcal{P}_{t,T} \tag{1.2}$$

where $c_{t,T}$ and $p_{t,T}$ are prices at t of European-style call and put options on S_t, respectively, that expire in T years and have a strike of X, and $r_{t,T}$ is the interest rate between t and T. $\mathcal{P}_{t,T}$ is the present value of dividends between t and T and hence represents the price of the

[1]It is important to note that for (1.1) to hold, cash flows should technically include *all* equity payouts, including net share repurchases. It has been shown in the literature that the definition of cash flow can affect research conclusions. For example, Ackert and Smith (1993) show that the results of variance-bound tests depend on how distributions to shareholders are measured. The authors find evidence of excess volatility when cash flow is defined narrowly (dividends only) but cannot reject the hypothesis of market efficiency when the payout measure also includes share repurchases and takeover distributions. Over the past two decades, equity repurchases have become a significant source of shareholder distributions. In the following I abstract from share repurchases to maintain consistency with the approach of BBK.

short-term asset defined in (1.1):

$$\mathcal{P}_{t,T} \equiv \sum_{i=1}^{T} E_t(M_{t:t+i} D_{t+i}) = p_{t,T} - c_{t,T} + S_t - X e^{-r_{t,T}(T-t)} \tag{1.3}$$

Because it includes the index S_t, the short-term asset receives all the actual dividends paid by the constituents. Hence its return has two components, the change in price $\mathcal{P}$ and the dividend component D. BBK then measure the monthly return as:

$$R_{t+1} = \frac{\mathcal{P}_{t+1,T-1} + D_{t+1}}{\mathcal{P}_{t,T}} - 1 \tag{1.4}$$

Equation (1.4) corresponds to (6) in BBK and the average maturity of the *short-term asset* is 1.6 years. (In the appendix, I briefly outline implementation details of the trading strategy that is used to generate dividend returns (see also, van Binsbergen, Brandt, and Koijen (2012)).) Using the return definition (1.4), BBK then report an average monthly excess return, $R_{t+1} - r_{f,t+1}$, for the *short-term asset* of 0.88% during 1996-2010, much higher than the overall market risk premium of 0.27% during that period, despite having a CAPM beta of less than one. BBK also implement a second trading strategy, a so-called "steepener", that focuses on the latter part of the short end. That is, the *steepener* goes long the *short-term asset* but then shorts the short-end of it, and hence is entitled to only those dividends that accrue *after* an initial period. Excess return on this asset average 0.84% per month. These results thus present evidence of a downward-sloping term structure of the equity risk premium.

Although the authors' approach is derived from simple no-arbitrage arguments assuming friction-less markets, it was subsequently estimated in an environment that incorporates market frictions. In the next section, I motivate the need for an adjustment of the return series (1.4) by analyzing the impact of market imperfections, specifically the tax rate differential of dividends and capital gains, on measured returns.

1.3 Market imperfections: tax effects and measured returns

Stock prices tend to decline by *less* than the amount of the dividend payment on ex-dividend days. While there has been a large literature on the ex-dividend day price drop effect, for simplicity and ease of exposition, I will assume that it is entirely a result of differential taxes on dividends and capital gains.[2] (It is important to note, however, that for my subsequent

[2] Abnormal stock price behavior around ex-dividend days has long been documented in the literature. Early work by Campbell and Beranek (1955) documents an average stock price drop-off on the ex-dividend date of

results to hold, I do not need to identify the exact sources for this stylized fact.)

To motivate the empirical analysis, I consider an economy with two companies $i \in \{N, P\}$ of identical risk but with different (exogenously determined and perfectly known) payout policies. Company P pays a dividend whereas company N does not (for example, it only distributes cash via share repurchases).[3] For simplicity assume that returns are riskless and that there is only one investor with tax rates τ_c for capital gains and τ_d for dividends. BBK measure company i's return over t and $t+1$ according to (1.5) as:

$$r^m_{i,t+1} = \frac{P_{i,t+1} - P_{i,t} + D_{i,t+1}}{P_{i,t}} \tag{1.5}$$

where $D_{i,t+1}$ is the dividend paid by i between t and $t+1$. The investor cares about expected post-tax returns (for example, see Brennan (1970) for market equilibrium conditions that take into account investor marginal tax rates). Given the assumption of identical risk, both stocks need to yield identical expected returns *after* taxes, i.e.:[4]

$$\begin{aligned} \mathbb{E}[r^r_{N,t+1}] &= \frac{(1-\tau_c)(P_{N,t+1} - P_{N,t})}{P_{N,t}} \\ &= \frac{(1-\tau_c)(P_{P,t+1} - P_{P,t}) + (1-\tau_d)D_{P,t+1}}{P_{P,t}} = \mathbb{E}[r^r_{P,t+1}] \end{aligned} \tag{1.6}$$

Now suppose that over a very short period, the *ex-dividend day*, expected returns after taxes are normalized to zero, i.e., $\mathbb{E}[r^r_{i,t+1}] = 0$. Further if I normalize $\tau_c = 0$, then I can rewrite (1.6) to interpret τ_d as relative tax disadvantage of dividends (relative to capital gains), or simply as the marginal investor's tax rate τ:[5]

$$\tau = \frac{P_{P,t+1} - P_{P,t} + D_{P,t+1}}{D_{P,t+1}} = \frac{P_{ex} - P_{cum} + D}{D} \tag{1.7}$$

about 90% of the amount of the dividend. Subsequently, Elton and Gruber (1970) find that prices drop by only about three-fourth of the dividend on ex-dividend days. The authors interpret the ex-dividend day behavior of common stocks as evidence that differential tax rates cause investors to discount the value of taxable cash dividends relative to long-term capital gains. Barclay (1987) examines ex-dividend day behavior of common stock prices *before* the enactment of the federal income tax. During that pre-tax period (1900-1910), he finds that stock prices fell on average by the full amount of the dividend providing support for the differential taxation hypothesis. Many other papers have subsequently analyzed the abnormal stock price behavior, with no definite consensus as to why stocks drop on ex-dividend dates by an average less than the dividend amount. Among the many papers, see for example, Kalay (1982), Eades, Hess, and Kim (1984), Poterba and Summers (1984). See also Green and Rydqvist (1999) for an example of Swedish lottery bonds where cash distributions are tax-advantaged relative to capital gains.

[3]The assumption of exogenously determined dividend policies can be motivated by recent survey evidence by Brav, Graham, Harvey, and Michaely (2008). The authors survey 328 financial executives and find that personal income tax consequences do not appear to be the primary concern in setting payout policies.

[4]For simplicity, I assume that capital gains are taxed at $t+1$. In practice, capital gains are taxed in the period in which the stock is sold, and capital losses can be used to offset capital gains (but not dividends).

[5]More generally, $\tau = \frac{\tau_d - \tau_c}{1-\tau_d}$.

There are two simple insights from this simple model: (1) In the absence of dividend taxes, or if *effective* marginal capital and dividend tax rates are identical (no tax-disadvantage of dividends, i.e., $\tau_d = \tau_c$), then one can evaluate *relative performance* of stocks using returns as measured by (1.5). However, if the effective marginal tax rate on dividends exceeds that on capital gains ($\tau > 0$), then – even when properly adjusted for risk – returns measured according to (1.5) are higher for dividend-paying stocks. (2) The discrepancy between measured and realized return increases in the dividend yield, D_{t+1}/P_t. *I will show in Table 1-5 that the dividend yield of the short-term asset in BBK averages about 5.2% per month whereas the dividend yield of the overall S&P 500 index it is compared with averages just 0.15% per month.* The extent to which these different payout properties can *mechanically* affect conclusions about the term structure of the equity premium presented in BBK thus depends on the marginal investor's effective tax rate τ during the sample period. In the next section I will review empirical evidence on stock price reactions around ex-dividend days for the overall US stock market historically as well as for the sample of BBK.

This model has abstracted from several considerations. For example, I have limited attention to only consider one taxable investor, ignoring trading by tax-exempt institutions. It is well known however that *tax capture* trading by tax-exempt institutions plays a role in financial markets which should reduce the tax penalty on dividends. However, there is only a limited number of tax-exempt investors who may want to remain diversified. Further, the IRS rules limit this kind of trading rendering tax-exempt institutions unable to *fully* exploit tax arbitrage.

In the next sections, I describe data and present empirical results. I first present evidence of the market imperfection for the more recent US stock market history and for the sample of BBK, and then adjust returns of the *short-term asset* using my estimates of implied tax rate measures.

1.4 Data

The main data used in this study come from the CRSP Daily Stock files. Some of the primary variables are *Return without Dividends* (CRSP variable RETX), *Holding Period Return* (RET), and variables related to distribution information, such as *Distribution Code* (DISTCD), *Dividend Cash Amount* (DIVAMT), *Declaration Date* (DCLRDT), *Record Date*

(RCRDDT), or *Payment Date* (PAYDT).[6] My focus is on ordinary dividend distributions that are paid in cash on ordinary common shares. In a first step, I identify dividend incidents as those observations for which *Return without Dividends* and *Holding Period Return* exist and *differ* on a given day. For the period July 1963 to 2011, this leaves 488,079 distribution day incidents (about 0.75% of total observation-days). Second, I remove all distributions that are not specifically indicated as cash payments.[7] Finally I consolidate incidents by company and date (for example, on November 15, 2004 Microsoft Corp has two dividends on record, a quarterly dividend ($0.08) and a special dividend ($3)) and discard observations that have both cash and non-cash distribution incidents on the same date. This leaves a total sample of 463,333 observations.

Daily industry returns using the 49 Fama-French value-weighted industry portfolios are sourced from Kenneth R. French's data library and daily total return data on the S&P 500 index come from Bloomberg (index returns without dividends are from CRSP). To examine the robustness of the empirical findings in BKK, in the second part of the empirical study, I utilize a dataset of monthly returns and prices of synthetic short-term dividend strips of the S&P 500 index that was made available online by van Binsbergen, Brandt, and Koijen (2012).

Table 1-1 shows some descriptive statistics for the period July 1963 to 2011, as well as for the subperiod 1996 to 2009 on which BBK's study is based.[8] *Declaration Date to Ex-Div Date* measures the time period, in calendar days, between the ex-dividend date and the declaration date. The median time between declaration and ex-dividend dates is 13 calendar days. The payment date follows the ex-dividend date after on average 22 days. Table 1-1 further shows dividend yields, measured as the ex-day cash dividend as fraction of the previous day closing price of the stock. The mean (median) dividend yield is 0.91% (0.74%).

Finally, I obtain option quotes and yield curve data from OptionMetrics that are used to replicate and extend the BBK analysis.

[6] See the Data Descriptions Guide for the CRSP US Stock & US Index Databases, June 2011, for detailed information on definitions of CRSP variables.

[7] The objective of this article is to highlight biases in return computations that can result from including cash dividends at nominal value; hence the focus on cash dividends. For differential analysis of dividend types see, for example, Eades, Hess, and Kim (1984).

[8] All variables are winsorized at the 1 and 99 percentiles.

1.5 Empirical Analysis of Returns around ex-dividend dates

I now document again the well-known regularity that daily pre-tax returns around ex-dividend dates are much higher than on non-dividend days. Table 1-2 reports daily excess and abnormal returns of a portfolio composed of ex-dividend companies around ex-dividend dates, where returns are defined according to the *standard* definition of holding period returns (1.5). Specifically, at the end of each day, the portfolio invests equally in ordinary common stocks that trade ex (cash) dividend the following day.[9] The portfolio is rebalanced every day to then again only include ex-dividend stocks and, on average, consists of 38.41 companies. Returns are winsorized at the 1 and 99 percentile to mitigate the impact of potential outlier observations.[10] This sample covers 12,070 trading days from July 1963 to 2011.

Portfolio excess returns, defined as return in excess of the risk-free return (one-month T-Bill), average 0.304% per day on ex-dividend days (i.e., 76.61% if simply annualized assuming an average of 252 trading days per year) and are highly significant. Further there seems to be modest positive drift leading up to the ex-dividend day, but no particular pattern in the days after the stocks trade ex-dividend.[11] Next, I explore whether the high excess returns on ex-dividend dates can be attributed to factors commonly known to explain returns. When a market model is used, abnormal returns – defined as regression constant from a regression of daily portfolio excess returns on the excess return of the market portfolio – continue to be large (0.293% per day or 73.8% annualized) and significant. Further, using the three-factor model (Fama and French (1993)) only slightly lowers the average abnormal return on ex-dividend days (0.284% per day).[12]

Figure 1-1 shows the yearly time series of portfolio abnormal returns on ex-dividend dates. For each year, abnormal returns are estimated as regression constants from a regression of daily portfolio excess returns on the excess return on the market portfolio (market model). The average abnormal return over the 49 years is 73.45% per year, almost identical to the estimate over the whole sample. Returns are somewhat lower in the second half of the sample,

[9] Note that the objective of this article is to highlight a possible average bias in standard return definitions, not to uncover a possibly profitable trading strategy.

[10] Results are qualitatively and qantitatively very similar when using non-winsorized returns.

[11] The modest positive drift leading up to the ex-dividend day may possibly result from tax arbitrage trading where demand from tax advantaged institutions drives up the price prior to the ex-dividend date.

[12] Adding a fourth factor, momentum (Carhart (1997)), does not change the abnormal return to any meaningful degree (not reported).

but in each year, the point estimate is positive and never falls below 16%. Overall, this apparent anomaly – high excess returns despite the absence of any systematic news[13] – strongly suggests that returns measured according to (1.5) incorporate an implicit bias that overstates the "true", or investor-relevant, return: On average, investors seem to price *nominal* cash dividends at a discount, consistent with the the first prediction of the simple model when the marginal tax rate is positive.

The average returns in Table 1-2 mask considerable heterogeneity among dividend-paying companies. In Panel A of Table 1-3, I repeat the analysis of excess returns on ex-dividend days but condition on dividend yields, or relative sizes of dividends. Dividend yields are the (known) ex-day cash dividends as fraction of the previous day closing price of the stocks. For the full sample, the median dividend yield of dividend payers was about 0.83%. There is a strongly monotonic relationship between pretax returns measured according to (1.5) and dividend yields, consistent with the second prediction of the stylized model. For the restricted sample of companies with dividend yields of at least 1%, the average excess return increases to 0.418% per day (105.34% annualized), about 37.5% higher than the unconditional average on ex-days. For companies with dividend yields at least 2%, this further increases to 0.568% per day (143.14% annualized). Finally, for the subset of companies with dividend yields of 5% or larger, the excess return on ex-dividend days averages 2.651%, or more than 668% per year. Clearly, excess returns of such magnitude are unrealistically high and strongly indicative of mis-measurement of investor-relevant returns when using (1.5) to measure *and* compare returns. In Panel B of Table 1-3, I sort observations into dividend yield quartiles by year and report excess returns. Similarly to the observation of Panel A, companies with larger dividend yields average higher excess returns on ex-days.

Another way in the literature (e.g., Campbell and Beranek (1955); Elton and Gruber (1970); Kalay (1982); Michaely (1990)) to analyze the stylized fact of excess returns around ex-dividend days that is unaffected by variation in dividend yields is to use "dividend drop ratios" (Δ). I define (unadjusted) dividend drop ratios as the change in the closing price of stock i on the ex-dividend day relative to the *nominal* cash dividend. Using (1.7), these

[13]Remember that dividend declaration dates precede the ex-dividend dates by some days; see Table 1-1.

dividend drop ratios can easily be translated into implicit marginal tax rate measures,

$$\Delta^i_{unadj} = \frac{P_{i,t} - P_{i,t+1}}{D_{i,t+1}} = 1 - \tau^i_{unadj}. \tag{1.8}$$

Dividend drop ratios represent estimates of valuation for dividends, i.e., how much the marginal investor is willing to pay for the claim to the known dividend. Stated differently, they provide measures of tax rate disadvantages of dividends. A drop ratio of one indicates that the marginal investor's capital and dividend tax rates are identical. A drop ratio below one (marginal tax rate larger than 0) measures a market imperfection. I will focus on the interpretation of dividend drop ratios as marginal tax rate measures, i.e., $\tau = 1 - \Delta$.

Panel A of Figure 1-2 plots the median as well as the weighted average implied marginal tax rate for 1963-2011 by year.[14] For the entire sample, the mean and median marginal tax rate are 38%. The average (median) of the 49 yearly weighted-average implied tax rates is 19% (18%). Not reported, the estimated tax rates are almost orthogonal to dividend yields (correlation of –0.0068) for this sample.

Expression (1.8) for the implied marginal tax rate assumes that expected returns over a very short period are zero. One possible refinement of equation (1.8) takes into account general market movements on ex-dividend dates and adjusts stock prices by some benchmark return ($R_{benchmark}$). Thus I define a benchmark-*adjusted* implied marginal tax rate for stock i as follows:

$$\tau^i_{adj} = \frac{P_{i,t+1} - (1 + R_{benchmark,t+1})P_{i,t} + D_{i,t+1}}{D_{i,t+1}} = \tau^i_{unadj} - \frac{R_{benchmark,t+1}}{D_{i,t+1}/P_{i,t}} \tag{1.9}$$

Specifically, I define a market-adjusted (τ^i_{madj}) as well as a industry-adjusted ($\tau^{i,j}_{iadj}$) implied marginal tax rate, where the respective benchmark returns are, respectively, the return incl. dividends of the CRSP value-weighed index ($R_{market,t+1}$) on the ex-dividend date and the return of one of the 49 Fama-French value-weighted industry portfolios ($R^j_{industry,t+1}$) to which the company is assigned based on its 4-digit SIC code.[15] A benefit of these refined tax rates is

[14] Weighted average implied tax rates are computed using total dollar dividend amounts as weights. Formally, using all observations i during year t, for year t it is defined as $\sum_i \frac{Div_i \cdot Shares_i}{\sum_i Div_i \cdot Shares_i} \cdot \tau^i$ where Div_i denotes dividend per share of company i and $Shares_i$ denotes number of shares of company i. The definition is analogous to the market-value weighted portfolio approach that is commonly used in the asset pricing literature. To mitigate the impact of potential outlier observations, τ^i is winsorized at the 1 and 99 percentile in each year.

[15] A different way to express (1.9) is as $1 + \frac{RETX - R_{benchmark}}{RET - RETX}$ where RET and RETX are the stock returns with and without dividend, respectively (see Section 1.4). The denominator is simply the dividend yield and the numerator the return without dividends in excess of the benchmark return. If the benchmark trends upward, this will decrease the ratio, thus decreasing the implied tax rate estimate.

that they allow to more closely isolate the ex-day effect of dividends on price changes, that is, noise is reduced. Panels B and C of Figure 1-2 plot the median as well as weighted average implicit marginal tax rates according to (1.9) for each year. The mean (median) *market-adjusted* implied tax rate is 29% (25%). This is lower than the unadjusted rate of 38% because of the upward drift in daily asset prices, yet remains indicative of a significant tax rate disadvantage of dividends. The correlation of estimated tax rates with dividend yield remains close to zero at –0.0013. The average (median) of the 49 yearly weighted average *market-adjusted* tax rates is 14% (10%). The mean (median) *industry-adjusted* implicit tax rate is 28% (24%). Arguably, industry-adjusted implied tax rates may yield the cleanest estimates because they adjust for industry-specific return trends on ex-dividend days. With a correlation of merely –0.0017, implied tax rates are also orthogonal to dividend yields. The average (median) of the 49 yearly weighted average *industry-adjusted* tax rates estimates is 12% (10%).

The results presented so far suggest that an adjustment method to (1.5) is desirable in order to align measured returns with after-tax realized returns. Only upon such correction are returns of assets with different dividend rates made comparable in economic terms. Without adjustments, the stylized facts presented in this section indicate that dividend-paying assets will measure higher returns simply because tax consequences are not accounted for, and further, that this measurement bias is larger when dividends are higher.[16] I will consider two adjustments for longer period horizons:

1. One can replace the ex-day return component with the contemporaneous market- or industry-specific return. On average, and in the absence of any major news, this may eliminate the measurement bias and make returns more comparable.

2. One can measure returns according to an *adjusted* holding period return formula in which the estimated marginal tax rate disadvantage of dividends, $\hat{\tau}$, is accounted for as

[16] In fixed income markets it has long been recognized that expected returns, or *yields*, of municipal bonds and those of comparable treasury or corporate bonds – even when properly adjusted for risk – cannot simply be compared on an unadjusted basis. For example, interest payments on municipal bonds enjoy certain tax benefits that other bonds do not, and hence yields can only be meaningfully compared *after* adjusting for the differential tax treatment. Equity return as well have components, price changes and distributions such as dividends, with differential tax implications for investors.

follows:

$$r^{adj}_{i,t+1} = \frac{P_{i,t+1} + (1-\hat{\tau}) \cdot D_{i,t+1}}{P_{i,t}} - 1 \tag{1.10}$$

In the following section, I show how such adjustment to the returns of dividend strips on the S&P 500 index used in BBK – an asset heavily loaded on dividends – can alter conclusions about the term structure of the equity premium.

1.6 Implications for BBK

1.6.1 Dividend yields and implicit marginal tax rates on the S&P 500 index 1996-2009

As a first step to see whether the returns on the *short-term asset* in BBK are indeed subject to the aforementioned measurement bias, it is important to analyze implied marginal tax rate measures of S&P 500 index constituents over the relevant sample period. For the entire period from February 1996 to October 2009, about 75% of S&P 500 index constituents have cash dividend distributions each year (the fraction of payers is somewhat higher, about 80%, in the earlier sample years 1996 to 1999, and tends to decrease slightly during recessions). The total sample consists of 19,368 ex-dividend observations, an average of 117 per month. The average (median) dividend yield, defined as Div_{t+1}/P_t, is 0.58% (0.48%) for dividend-paying companies. While this may seem low at first glance, it is important to keep in mind that the dividend yield on the *short-term asset* is a leveraged multiple of this average as it represents a claim to *nominal* dividends over the short horizon only. For the overall S&P 500 index, the monthly dividend yield averages 0.15%, or 27% of its total average monthly return. The monthly dividend yield on the *short-term asset* averages 5.2%, or 449% of its total average monthly return (see also Table 1-5).

Using this sample, I first estimate unadjusted and adjusted implicit tax rates measuses $\hat{\tau}$ according to (1.8) and (1.9), respectively. Table 1-4 reports median and dividend-weighted average *implicit marginal tax rates* for the S&P 500 index constituents during February 1996 and October 2009. Implied marginal tax rates are measured on ex-dividend days as well as over the four (ten) days surrounding the ex-dividend date.[17] For the entire sample, the median

[17]For example, the unadjusted *t(-2,2)* implied marginal tax rate is measured as 1 - $\frac{P_{t-2}-P_{t+2}}{Div_{t+1}}$. Market- and industry-adjusted tax rates are defined similarly but correct for contemporaneous returns on the market and industry. Note that simple means are generally higher than medians (not reported). Since the short-term asset is the claim to total dividends, arguably the relevant average is the total dividend-weighted average.

(dividend-weighted average) unadjusted implied tax rate is 19.4% (18.4%) on ex-dividend days. This rate increases as one expands the measurement window, because of a general upward drift in asset prices.[18] Adjusting for contemporaneous market movements lowers the median and weighted average implied tax rate estimates to 10.8% and 16.1%, respectively, on ex-days.[19] Further, using industry adjustments rather than market adjustments yields similar results. In addition to the values reported in Table 1-4, I also estimate a time series of *monthly* median and dividend-weighted average implicit marginal tax rates for the S&P 500 sample.

Having established that dividends of S&P 500 index constituents were significantly tax-disadvantaged (relative to capital gains) during the sample period, in a next step I show how returns of the *short-term asset* reported in BBK change when correcting for the estimated implied tax disadvantage of dividends. It is only after such adjustment that returns of the *short-term asset* and the *long-term asset*, or S&P 500 index, can be meaningfully compared and inferences about the term structure of the equity premium be drawn.

1.6.2 Adjusting Returns of the *Short-term Asset*

The stylized empirical fact of economically significant positive implied marginal tax rates τ suggests that dividend components in returns be adjusted to reflect their after-tax valuation and allow for meaningful comparisons of returns across assets. As was shown earlier, this adjustment becomes more important the higher the dividend yield of the asset. The dividend yield on the *short-term asset* averages about 5.2% *per month*, making its return particulary susceptible to this measurement concern. I reestimate (1.4) with an adjusted return formula as follows:

$$R_{t+1}^{adj} = \frac{\mathcal{P}_{t+1,T-1} + (1-\hat{\tau}) \cdot D_{t+1}}{\mathcal{P}_{t,T}} - 1 \tag{1.11}$$

where $\mathcal{P}$ is the price of the *short-term asset*, D_{t+1} the dividends paid by S&P 500 constituents during t and $t+1$ (one month), and $\hat{\tau}$ reflects a measure of ex-day implied marginal tax rates for the S&P 500 sample.

In Table 1-5, I report *adjusted* monthly returns on the *short-term asset*, as well as the standard deviation and Sharpe ratio of the adjusted return series. In columns (1) and (3), I

[18] Note that expanding the measurement window runs the risk of adding noise through price-relevant news, particularly the announcement of dividends that can shock prices.

[19] Conforming with the sample, I use the S&P 500 total return as market benchmark. Similar results are obtained when using the CRSP value-weighted return instead.

adjust the dividend series using the median and dividend-weighted average ex-day implied tax rate of S&P 500 index constituents estimated over the entire *sample period* (see Table 1-4). In columns (2) and (4), each monthly return is adjusted using the respective ex-day implied tax rate estimated over the same *month*. Results are reported for unadjusted ex-day implied tax rates as well as for rates corrected for market or industry movements. For reference, I also show the unadjusted monthly mean return (1.16%), standard deviation (7.8%), and Sharpe ratio (0.1124) for the *short-term asset* as reported in van Binsbergen, Brandt, and Koijen (2012). Using unadjusted implied tax rate estimates eliminates the positive average return on the *short-term asset* almost entirely, however, as mentioned before unadjusted tax rate estimates may be too large of an adjustment as they ignore general upward drifts in asset prices. Thus, more plausible adjustments may use the market- or industry-adjusted implicit tax rate measures. When adjusting dividends with these median implied tax rates, estimated over the whole sample, the return on the *short-term asset* decreases to between 0.6%-0.63% (0.42%-0.49% when adjusting with the series of monthly implied rates) – much lower than the unadjusted results reported by the authors. Since D_{t+1} in (1.11) is a value-weighted portfolio of total constituent dividends itself, arguably the most representative estimate $\hat{\tau}$ will reflect this fact and use a total dividend-weighted average implied marginal tax rate. When reestimating (1.11) with such dividend-weighted average tax rates, adjusted for market or industry effects, the average return of the short-term asset lowers to about 0.32%-0.47% per month. Noting that the volatility of the adjusted return series is of similar magnitude to that of the unadjusted return, this further implies that adjusted Sharpe ratios are sharply lower. Overall, once adjusted, the *short-term asset* appears to have a return similar to that of the S&P 500 index, which averaged 0.56% per month over the sample period, but at a higher volatility (index volatility averaged only 4.68%).[20]

Returns so adjusted are thus much more comparable to those of the overall index and do not support BBK's result of an downward-sloping term structure of the equity premium.[21]

[20] The risk-free rate over the sample period averaged 0.28% per month.

[21] Adjusting the return of the index in a similar way is negligible given its low yield, and does not affect the qualitative nature of our results.

1.7 Robustness

Given the empirical estimates of dividend tax disadvantages, the high returns of the *short-term asset* could not be realized by a representative investor that is subject to taxation. Adjustments for taxes suggest that such an investor would in fact be facing an upward-sloping term structure of risk. However, tax-exempt investors do not face this additional burden.[22] If investors in the dividend strip are indeed tax-exempt, then one should expect these returns to be fairly invariant to changes in tax regimes. Exploiting variation in tax regimes in the following section will help me to address this question.

1.7.1 Exploiting Variation in US Tax Regimes

To further strengthen my conclusion that the average returns of the *short-term asset* reported in BBK are mechanically inflated by ignoring tax implications of dividends, in this section, I explore the robustness of my adjustment for the two subsample periods analyzed in BBK, 1996-2002 and 2003-2009. Here, the "Bush tax cuts" of 2003 lend themselves to a natural experiment. Before the 2003 tax reform, dividend income was taxed to individuals as ordinary income at their marginal tax rates whereas the tax rate on long-term capital gains was capped at 20%, significantly lower than the top marginal tax rate on ordinary income. As a result of the tax cuts, the tax rate on dividends was essentially lowered to the level of that on capital gains.[23] If tax rate disadvantages of dividends explain the equity premium term structure result of BBK, then one should expect changes in the tax regime to be reflected in return patterns over time.

In Table 1-6, I repeat analysis and show median and dividend-weighted average *implicit marginal tax rates* for S&P 500 index constituents as well as returns of the *short-term asset* and the S&P 500 index as reported in BBK for the two subsamples (1996:2-2002:12 and 2003:1-2009:10). As before, implied tax rates are measured on ex-dividend days and measure the tax disadvantage of dividends relative to capital gains and are reported unadjusted as well as adjusted for market or industry movements. Adjusted median (dividend-weighted average)

[22]Note, however, that a comparison of short-term discount rates of tax-exempt investors with long-term discount rates of the marginal (taxable) investor does not generate a very meaningful term structure of the equity risk premium.

[23]There still exist notable differences, in particular, dividends are taxed at realization and cannot be used to offset capital losses. Thus, while the effective tax rates are lowered, dividends can persist to be slightly tax-disadvantaged.

implied marginal tax rates are estimated at 17.4%-18.7% (27%-38.8%) for the period before 2003 and 4.6%-5.1% (2.7%-5.3%) for the period subsequent to 2003. This is strong evidence that the ex-day return effects I analyzed before indeed capture tax effects. In column (6), I compute a slope measure of the term structure of the equity premium as difference of average returns of the *short-term asset* and the S&P 500 index for each subsample (as reported in BBK). For the first half sample, BBK report a return on the *short-term asset* of 0.94% per month in excess of the overall S&P 500 index return, whereas the second half sample shows a significant decline in that premium to 0.26% per month.[24] The fact that these return patterns strongly align with the predictions from different tax regimes provides additional evidence against the risk-based conclusion of BBK.[25] Before 2003, dividends were significantly tax-disadvantaged causing assets with large dividend yields such as the *short-term asset* to have returns that are measured too high. Upon elimination of some major dividend tax disadvanages in 2003, *unadjusted* returns of assets became much more comparable. Overall, it appears that the relatively higher measured returns of the *short-term asset* can simply be attributed to taxation effects of dividends.

1.7.2 Replication and Analysis of alternate Term Structure Segments

BBK analyze two segments of the term structure of the equity premium using their synthetic dividend claims. With an average maturity of 1.6 years, BBK trading strategy 1, the *short-term asset*, is designed to capture the average risk premium of the short end of the term structure of the equity risk premium. BBK trading strategy 2, the dividend *steepener*, analyzes the latter half of that short end. These strategies produce assets with very different dividend yields – about 5.2% per month for the *short-term asset* and 0% for the *steepener* – but very similar returns (1.16% and 1.12% per month, respectively).

In this section I replicate BBK and extend the analysis to include alternate segments of the term structure. By varying T of $\mathcal{P}_{t,T}$ in equation (1.3), I can adjust the average maturity of the *short-term asset*. This allows me to obtain a more granular picture of the risk premium

[24]Note that – likely due to the small sample size – the economic significance does not translate into statistical significance, which is similar to what BBK document for the entire sample as well.

[25]The monthly dividend yield of the *short-term asset* is approximately 5.5% in the first half sample and only somewhat *smaller* (4.9%) in the second half sample, supporting in the identification. Further, the overall lower premium of the *short-term asset* in the second-half sample is not driven by the months of September to November 2008, a period of significant market decline and limitation to short selling. The average return of the short-term asset *exceeded* that of the S&P 500 index by 2.6% per month during that period.

in the short end of the term structure. At the same time, variation in T will *mechanically* manipulate the dividend yield of these assets – as T decreases $D_{t+1}/\mathcal{P}_{t,T}$ increases – which, as was shown in Section 1.5 and Table 1-3, has a strong empirical relation to measured returns that is *independent of risk*.

There is one notable difference between the options data used in this analysis and the data used in BBK. While BBK use intra-day option quotes to compute prices of synthetic dividend claims, my replication analysis relies on end-of-day quotes from OptionMetrics. Using end-of-day quotes causes some synchronization issues and likely introduces additional noise. Indeed, in footnote 6 of van Binsbergen, Brandt, and Koijen (2012), the authors state that they "reproduced [their] results using OptionMetrics data, and find similar results for average returns, but the volatility of prices and returns is substantially higher." Thus, using OptionMetrics should yield qualitatively similar conclusions to those of BBK. The replication procedure is detailed in Appendix 1.9.2.

In Figure 1-3, I plot average monthly returns, in excess of the S&P 500, and dividend yields of eight synthetic dividend claims[26] with average maturities ranging from 0.14 to 1.68 years (additional statistics are shown in Table 1-7). Asset $s23\_6$, which replicates the *short-term asset* of BBK, has an average monthly return of 1.34%, similar to the 1.16% reported in BBK and 0.78% higher than the overall S&P 500 index. Its return volatility is 11.64% which is higher than the 7.8% reported in BBK (likely a result of the noise induced from using OptionMetrics data). As anticipated, the average dividend yield is stongly monotonically decreasing in the assets' average maturity, ranging from 5.2% per month for the longest-maturity asset (maturity of 1.68 years) to 60.8% per month for the shortest-maturity asset (maturity of 0.14 years). With the exception of asset $s11\_6$ (maturity of 0.68 years), there is also a negative monotonic relationship between average maturities and average excess returns of assets.[27] Thus, by mechanically inflating dividend yields of these synthetic assets, I subsequently observe returns that are increasing in the dividend yield. This is consistent with the tax-based or measurement-based explanation provided in this article but seems strongly at odds with any risk-based explanations. (Further, it seems implausible that dividend claims obtained over the

[26]These short-term assets, or trading strategies, are labeled sT_d where T refers to the initial maturity (in months) and d indicates the number of months the claim is held before it is reset to its initial maturity. This follows the approach used by BBK. See Appendix 1.9.2 for more details.

[27]For reference, Figure 1-3 also shows geometric average returns (in excess of geometric average returns of the S&P 500).

next 0.14 years command average excess returns of 29.31% per month (or 4.76% per month when considering geometric average returns)). This reiterates the need for an adjustment of returns when measuring and comparing returns of assets with different dividend yields.

An important caveat of this analysis relates to the use of non-synchronously measured data, particularly for the shorter-term synthetic assets. In a related important article by Boguth, Carlson, Fisher, and Simutin (2013), the authors show that small pricing frictions in underlying markets can cause large biases in synthetic dividend strip returns and that these pricing frictions can help to reproduce a downward-sloping term structure of equity premia. Here, the major friction that explains BBK's empirical finding is related to mis-measurement, not taxation. In order to disentangle these effects, in Table 1-8, I combine my robustness tests and present effects of tax regime changes on excess returns of replicated *short term assets* of various maturities. The dependent variable $R_{ST,t} - R_{SP500,t}$ is the return of the short term asset in excess of that of the S&P 500 index and thus represents a measure of slope of the term structure of equity risk. The dummy *Low Tax Regime* captures the "Bush tax cuts" in 2003 that removed major tax disadvantages of dividends and is defined as 1 for the period 2003-2009. Throughout these assets, excess returns are much larger in the period 1996-2002 and significantly diminish afterwards. Unless one believes that pricing frictions also differ significantly during these two sample periods, these results suggest that the mechanical effect observed in Figure 1-3 cannot merely be explained by noise. Instead, dividend taxation disadvantages seem to have a first-order role in explaining the return patterns.[28]

Finally, Figure 1-4 shows average monthly returns of the original BBK *steepener*, replicated using OptionMetrics data, as well as an alternative steepener. For reference, I also show returns for the original BBK *steepener* and the S&P 500 index. The monthly return of the replicated steepener, $s23\_6\_m\_11\_6$,[29] averages 0.80%. While this is somewhat lower than the return measured by BBK (1.12%), it is still higher than the average return of the S&P 500

[28] Using data from 1962-1994, Bali and Hite (1998) argue that ex-day price changes will not equal dividends since prices are constrained to discrete tick multiples while dividends are continuous. However, Graham, Michaely, and Roberts (2003) study how price discreteness and transaction costs affect stock returns by comparing ex-dividend pricing before and after decimalization of stock price quotations. They find that abnormal ex-dividend day returns *increase* following decimalization, which is inconsistent with microstructure explanations of ex-day price movements. Using the sample of S&P 500 constituents, I verify that during 2001-02 – the period *after* decimalization but *before* the dividend tax cuts of 2003 – median and dividend-weighted average implicit marginal tax rates are large, ranging from 15% to 47.2% depending on the adjustment made.

[29] The *steepener* is computed as the difference in prices of short-term assets with initial maturities of 23 months and 11 months.

during that period (0.56%). Thus, despite zero dividend yield, this asset produces returns in excess of the overall index. To analyze the extent to which this result is driven by the particular choice of the short end component, I compute returns of an alternative steepener strategy. The alternative steepener, $s23\_6\_m\_17\_6$, is similar to the original *steepener* in that it has zero dividend yield but differs in that it focuses on the latter quartile of the *short-term asset*. The mean monthly return of the alternative steepener is only marginally higher than that of the S&P 500, 0.59% vs. 0.56%. (Note, however, that the geometric average return of the alternative steepener is close to zero). This result too indicates that the original BBK *steepener* lacks certain robustness in demonstrating a downward-sloping term structure of the equity risk premium.

1.8 Conclusion

In a recent article "On the Timing and Pricing of Dividends", van Binsbergen, Brandt, and Koijen (2012) empirically investigate term structure properties of the equity premium by recovering prices of the short-term component of the market index. By documenting that discount rates of risky cash flows are highest at short horizons, the authors present novel empirical insights for the understanding of risk and return relationships in equity markets. The result of a downward-sloping term structure of risk premia is at odds with the predictions of many leading theoretical models, which predict that discount rates are increasing with maturity (e.g., Campbell and Cochrane (1999), Bansal and Yaron (2004)) or flat (e.g., Gabaix (2012)). BBK's empirical finding has also motivated a new strand of theoretical models that aim to reconcile this discrepancy (see, for example, Belo, Collin-Dufresne, and Goldstein (2012)).

In this paper, I examined the robustness of the BBK result and provide an alternative explanation. I argue that market imperfections, notably a taxation differential between dividends and capital gains, cause the returns on the *short-term asset* to be measured too high, relative to the market index. Thus, the higher returns that BBK measure for the short-term asset are merely a result of taxation and do not represent compensation for risk. After adjusting returns to allow for meaningful (after-tax) comparisons, one no longer observes the downward-sloping term structure pattern reported in BBK. If anything, average short-term discount rates are below long-term rates, a prediction that existing theoretical models seem quite capable of delivering. Further, exploiting variation in US dividend tax regimes and analyzing returns of dividend claims with other maturities provides additional support for my conclusion.[30]

[30] In a subsequent related note, BBK also report comparable excess returns using *dividend futures* for the period 2002-2009 (see also van Binsbergen, Hueskes, Koijen, and Vrugt (2013)). Dividend futures are fairly novel (index listed futures have only been introduced in 2008) contacts that pay off *based on* the total dividends paid by index constituents, however, contrary to the *short-term asset* they do not provide a way to actually collect the ordinary cash dividends paid by companies in the S&P 500. The futures thus does not reflect the value of dividends to an investor but rather their *nominal* pre-tax values (for example, tax treatment may differ since there is no company that actually pays *dividends* to the buyer of the futures contact). In earlier years, some practitioners have described dividend derivative assets as "one-sided" markets, as structured products issuers looked for a way to recycle their by-products (thus receiving cash and paying the outcome of the dividend calculation). The structured products issuance boom in the 2000s may thus have led to an oversupply of dividends in the market, making it hard to say whether the reported returns indeed present compensation for risk. To the contrary, such "buyer's market" is generally not a feature of liquid index options markets– on which the *short-term asset* analyzed above is based.

Bibliography

Ackert, L. F., and B. F. Smith (1993): "Stock Price Volatility, Ordinary Dividends, and Other Cash Flows to Shareholders," *Journal of Finance*, 48, 1147–1160.

Ai, H., M. M. Croce, A. M. Diercks, and K. Li (2012): "Production-Based Term Structure of Equity Returns," Working paper.

Bali, R., and G. L. Hite (1998): "Ex dividend day stock price behavior: discreteness or tax-induced clienteles?," *Journal of Financial Economics*, 47, 127–159.

Bansal, R., and A. Yaron (2004): "Risks for the Long Run: A Potential Resolution of Asset Pricing Puzzles," *Journal of Finance*, 59, 1481–1509.

Barclay, M. J. (1987): "Dividends, taxes, and common stock prices: The ex-dividend day behavior of common stock prices before the income tax," *Journal of Financial Economics*, 19, 31–44.

Barro, R. J. (2006): "Rare disasters and asset markets in the twentieth century," *Quarterly Journal of Economics*, 121, 823–866.

Belo, F., P. Collin-Dufresne, and R. S. Goldstein (2012): "Endogenous Dividend Dynamics and the Term Structure of Dividend Strips," Working paper.

Boguth, O., M. Carlson, A. Fisher, and M. Simutin (2013): "Leverage and the Limits to Arbitrage Pricing: Implications for Dividend Strips and the Term Structure of Equity Risk Premia," Working paper.

Brav, A., J. R. Graham, C. R. Harvey, and R. Michaely (2008): "Managerial Response to the May 2003 Dividend Tax Cut," *Financial Management*, 37, 611–624.

BRENNAN, M. J. (1970): "Taxes, Market Valuation and Corporate Financial Policy," *National Tax Journal*, 23, 417–427.

CAMPBELL, J. A., AND W. BERANEK (1955): "Stock Price Behavior on Ex-Dividend Dates," *Journal of Finance*, 10, 425–429.

CAMPBELL, J. Y., AND J. H. COCHRANE (1999): "By Force of Habit: A Consumption-Based Explanation of Aggregate Stock Market Behavior," *Journal of Political Economy*, 107, 205–251.

CARHART, M. M. (1997): "On Persistence in Mutual Fund Performance," *Journal of Finance*, 52, 57–82.

EADES, K. M., P. J. HESS, AND E. H. KIM (1984): "On interpreting security returns during the ex-dividend period," *Journal of Financial Economics*, 13, 3–34.

ELTON, E. J., AND M. J. GRUBER (1970): "Marginal Stockholder Tax Rates and the Clientele Effect," *Review of Economics and Statistics*, 52, 68–74.

FAMA, E. F., AND K. R. FRENCH (1993): "Common risk factors in the returns on stocks and bonds," *Journal of Financial Economics*, 33, 3–56.

GABAIX, X. (2012): "Variable Rare Disasters: An Exactly Solved Framework for Ten Puzzles in Macro-Finance," *Quarterly Journal of Economics*, 127, 645–700.

GRAHAM, J. R., R. MICHAELY, AND M. R. ROBERTS (2003): "Do Price Discreteness and Transaction Costs Affect Stock Returns? Comparing Ex-Dividend Pricing Before and After Decimalization," *Journal of Finance*, 58, 2611–2635.

GREEN, R. C., AND K. RYDQVIST (1999): "Ex-day behavior with dividend preference and limitations to short-term arbitrage: the case of Swedish lottery bonds," *Journal of Financial Economics*, 53, 145–187.

KALAY, A. (1982): "The Ex-Dividend Day Behavior of Stock Prices: A Re-Examination of the Clientele Effect," *Journal of Finance*, 37, 1059–1070.

MICHAELY, R. (1990): "Ex-Dividend Day Stock Price Behavior: The Case of the 1986 Tax Reform Act," *Journal of Finance*, 46, 845–859.

Poterba, J. M., and L. H. Summers (1984): "New Evidence that Taxes Affect the Valuation of Dividends," *Journal of Finance*, 39, 1397–1415.

Stoll, H. R. (1969): "The Relationship Between Put and Call Option Prices," *Journal of Finance*, 24, 801–824.

van Binsbergen, J., M. Brandt, and R. Koijen (2012): "On the Timing and Pricing of Dividends," *American Economic Review*, 104, 1596–1618.

van Binsbergen, J., W. Hueskes, R. Koijen, and E. Vrugt (2013): "Equity yields," *Journal of Financial Economics*, 110, 503–519.

1.9 Appendix

1.9.1 Implementation Details of the *short-term asset*

In this section, I briefly outline implementation details of trading strategy 1 that generates the return series of the *short-term asset* as described in van Binsbergen, Brandt, and Koijen (2012) (for reference, see van Binsbergen, Brandt, and Koijen (2012)). The authors describe the strategy as follows (quote):

> The specific maturities we follow for trading strategy 1 vary between 1.9 years and 1.3 years. To be precise, for trading strategy 1, we go long in the 1.874 year dividend claim on January 31st 1996, collect the dividend during February and sell the claim on February 29th 1996 to compute the return. The claim then has a remaining maturity of 1.797 years. We buy back the claim (or alternatively, we never sold it), go long in the 1.797 year claim, collect the dividend, and sell it on March 29th 1996. We follow this strategy until July 31st 1996 at which time the remaining maturity is 1.381 years. On this date a new 1.881 year contract is available so we restart the investment cycle at this time. We continue this procedure until October of 2009, which is the end of our sample.
>
> For trading strategy 2, we follow the same maturities, apart from the fact that we go long in the 1.874 year dividend claim and short in the 0.874 dividend claim on January 31st 1996. On July 31st 1996 the remaining maturities are 1.381 years and 0.381 years at which point we restart the investment cycle in the 1.881 year contract and the 0.881 year contract available at that time.

In order to analyze and *adjust* the return series, I need monthly prices of the short-term asset ($\mathcal{P}$) and monthly dividends (D). While I have returns and dividends provided by BBK, I do not have actual prices of the *short-term asset*. However, I can approximate the price of the *short-term asset* at the start of each investment cycle (i.e., every six months) and then iterate forward using actual returns and dividends to obtain subsequent prices. Specifically, every six months, I construct the price of the *short-term asset* by interpolating between prices of standardized maturities (18 months and 24 months) provided by the authors. Then using information on returns and dividends, I back out subsequent prices according to equation (1.4). This allows me to decompose the reported return of the *short-term asset* into two components, capital appreciation and dividend yield, and adjust according to equation (1.11). Note the approximation error of the initial price of the *short-term asset* is not a first-order concern in this analysis. What is important is that the dividend yield component in (1.4) is large and this fact is not very sensitive to perturbations in $\mathcal{P}$.

1.9.2 Replication Details using OptionMetrics

Similarly to BBK, I measure dividend prices using the put-call parity in equation (1.2) during 1996-2009. At the end of each month, I obtain end-of-day bid and ask option quotes and S&P 500 index values from OptionMetrics and match call options of a given strike X and maturity T with corresponding put options. I discard observations with missing implied volatility which is an OptionMetrics indicator of non-standard settlement (including these observations does not affect results of the analysis). Interest rates are obtained from the OptionMetrics yield curve and interpolated to match the maturity of the option pair, as was done in BBK. I then use option bid-ask midquotes, interpolated interest rates, and the S&P 500 index value to compute a synthetic dividend price for each maturity and strike pair. Next, I remove observations with *negative* dividend prices as negative prices clearly constitute a violation of no arbitrage and cannot be used to compute returns.[31] For a given maturity, I then take the median across all prices and use this as final price of the dividend claim in my analysis analysis.[32] Overall, this approach resembles BBK with the difference that BBK match data using intra-daily quotes.

Using these prices, I devise dividend strategies to measure returns of *short-term assets* with *eight* different average maturities. The approach mirrors BBK (which is quoted in Appendix 1.9.1) but captures more segments of the term structure. Specifically, I label short-term assets as sT_d where T refers to the initial maturity (in months) and d indicates the number of months the claim is held before it is reset to its initial maturity.[33] For example, the short-term asset $s11\_3$ initially invests in a dividend claim that matures in 11 months and holds this claim for 3 months (during which it also collects dividends) before it is sold. After 3 months, the trading strategy is reset and a new 11-month claim is purchased. The average maturities of the short-term assets so created range from 0.14 years to 1.68 years.

Further, I devise two dividend steepener strategies following the approach used in BBK. The first strategy, labeled $s23\_6\_m\_11\_6$, replicates the dividend *steepener* of BBK. The price of this asset is computed as difference of the prices of short-term assets with initial maturities

[31] Negative synthetic dividend prices most likely result from non-synchronous data recording.

[32] For robustness, I also redo analysis using mean instead of median prices as final price. This has almost no effect on results. Further, including other filters such as requiring minimum option bid and ask quotes (e.g., \$1 or \$2) or non-zero open interest does not materially affect any results. All these results can be obtained from the author upon request.

[33] Using trading strategies to measure dividend returns is necessitated by the non-monthly issuance cycle of options.

of 23 and 11 months. Notably, because the price is computed as difference of two short-term assets, the return has zero dividend yield and is completely composed of capital gain. The asset is held for 6 months before it is reset to its initial maturity. A second alternative steepener, labeled $s23\_6\_m\_17\_6$, is constructed similarly but uses prices of short-term assets with initial maturities of 23 and 17 months.

Figure 1-1: **Abnormal holding period returns on ex-dividend dates**

Notes: The graph shows the time series of average annualized abnormal returns on ex-dividend dates of a portfolio that consists of ex-dividend day companies only. Specifically, at the end of each day, the portfolio invests equally in ordinary common stocks that go ex (cash) dividend the following day. Returns are measured according to the standard definition of holding period returns, that is, as the ratio of (nominal) dividends and price changes over previous closing price. The portfolio is rebalanced every day and, on average, consists of 38.41 companies. For each year, abnormal returns are estimated as regression constants from a regression of daily portfolio excess returns on the excess return on the market portfolio. The 95% confidence interval is constructed using Newey-West standard errors. Returns are winsorized at the 1 and 99 percentiles. The sample covers 12,070 trading days from 1963-2011.

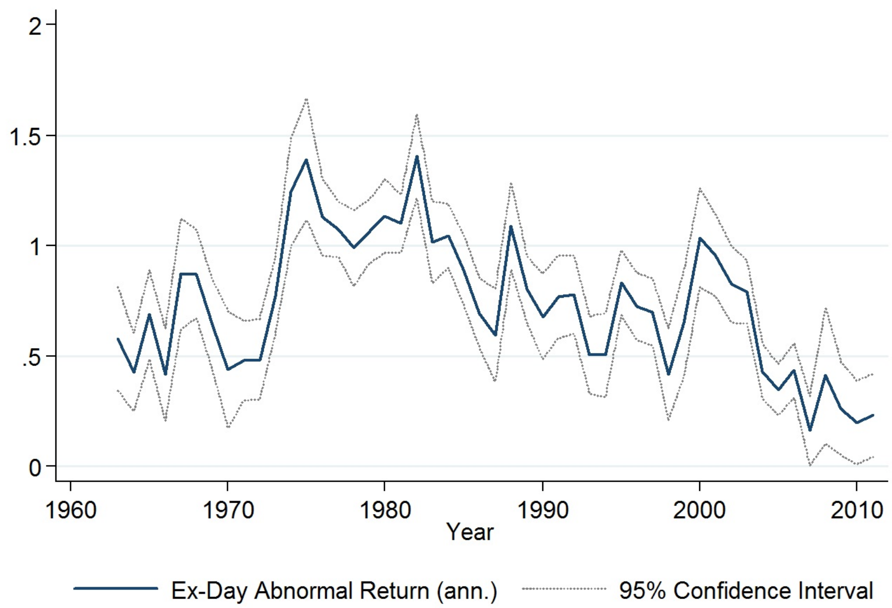
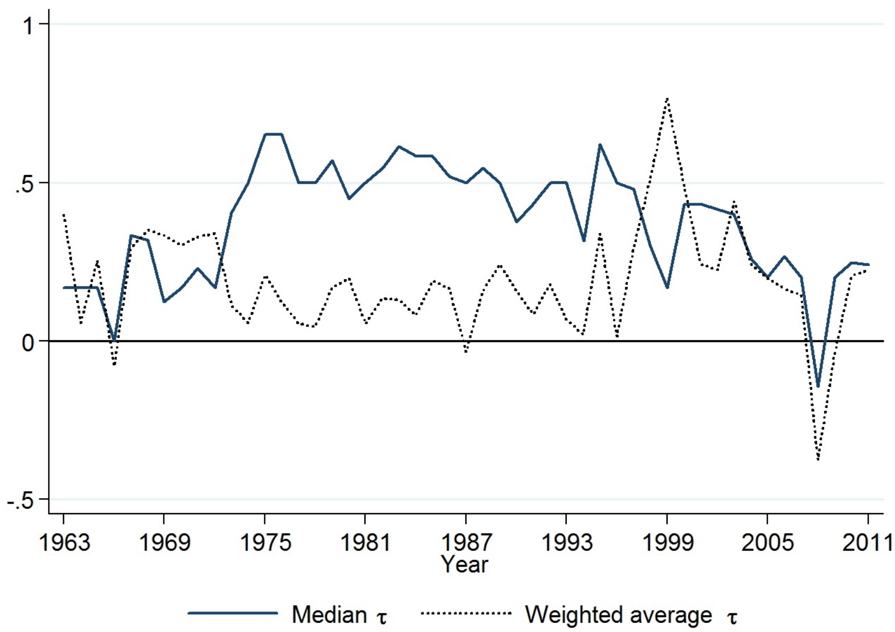


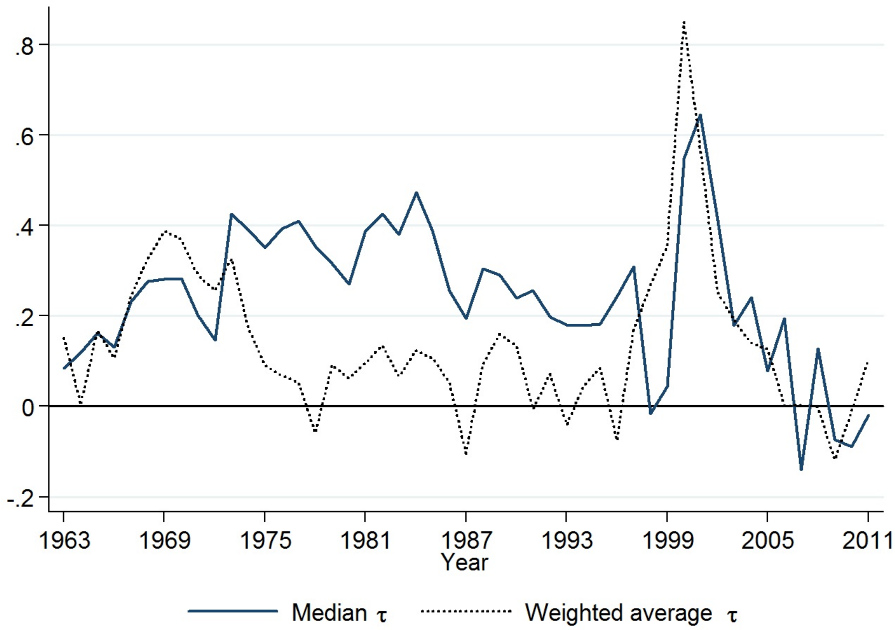
Figure 1-2: **Implicit marginal tax rates over time**

Notes: The following graphs show time series of median and weighted average implied marginal tax rates (τ) *by year*. Implicit marginal tax rates are computed as one minus the price change relative to dividends on ex-dividend days. Panel A plots unadjusted tax rate measures. To account for market-wide movements on ex dividend dates, Panel B shows implied marginal tax rates adjusted for contemporaneous returns on the CRSP value-weighted index (incl. dividends). In Panel C, adjustment to implied tax rates is made by controlling for industry-specific returns using the 49 value-weighted Fama-French industry portfolios. Implied tax rates are winsorized at the 1 and 99 percentiles and weighted averages use the total market value of dividend distributions (dividend yield times market capitalization) to compute weights.

Panel A: Implicit marginal tax rate (Unadjusted)



Panel B: Implicit marginal tax rate (Market-adjusted)



Panel C: Implicit marginal tax rate (Industry-adjusted)

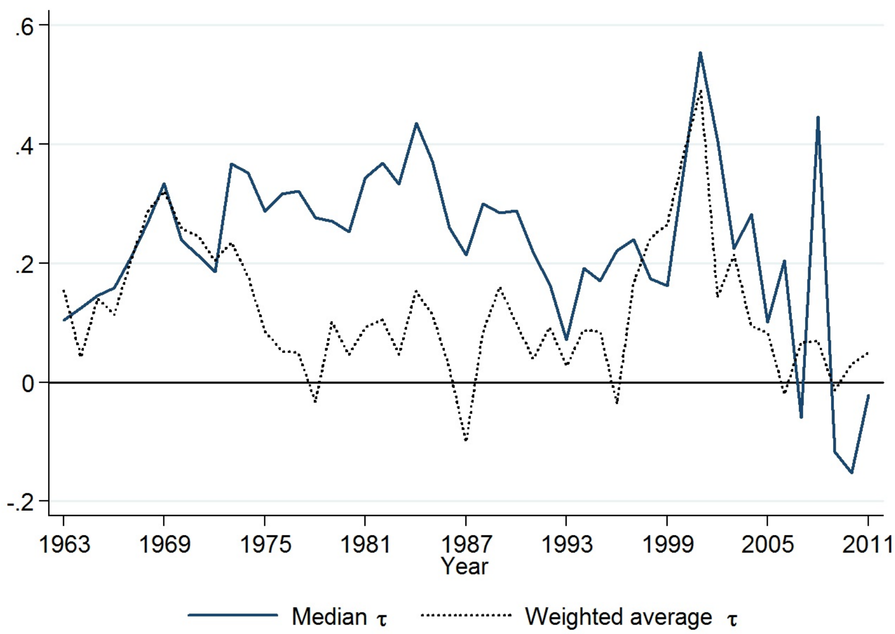


Figure 1-3: **Analysis of synthetic dividend claims of various maturities**

Notes: The Figure shows average monthly returns (in excess of the S&P 500) and dividend yields of eight synthetic dividend claims with maturities ranging from 0.14 years (asset *s*2_1) to 1.68 years (asset *s*23_6). Additional statistics are shown in Table 1-7. Asset *s*23_6 replicates the *short-term asset* of BBK. Decreasing the maturity of dividends claims increases the dividend yield of these assets. Since investors must be compensated for the extra tax burden of dividends, they command higher average rates of returns.

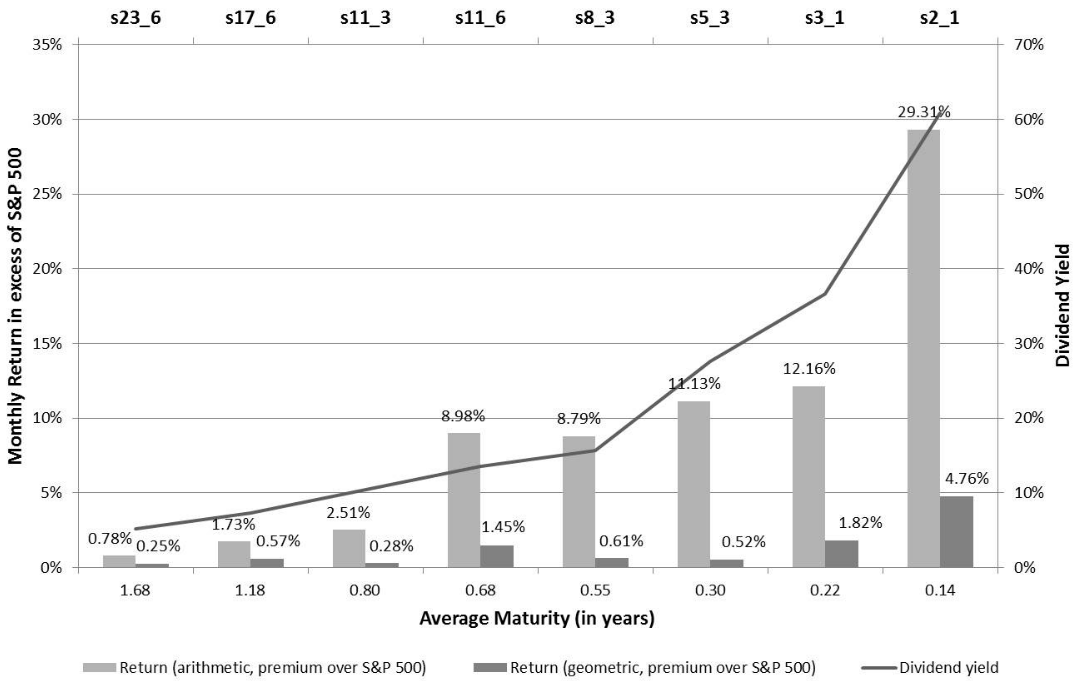


Figure 1-4: **(Alternative) Steepener Analysis 1996-2009**

Notes: The Figure shows average monthly returns of the original BBK *steepener*, replicated using OptionMetrics data and labeled $s23\_6\_m\_11\_6$, as well as an alternative steepener ($s23\_6\_m\_17\_6$). For reference, I also show returns for the original BBK *steepener* and the S&P 500 index. Additional statistics are shown in Table 1-7.

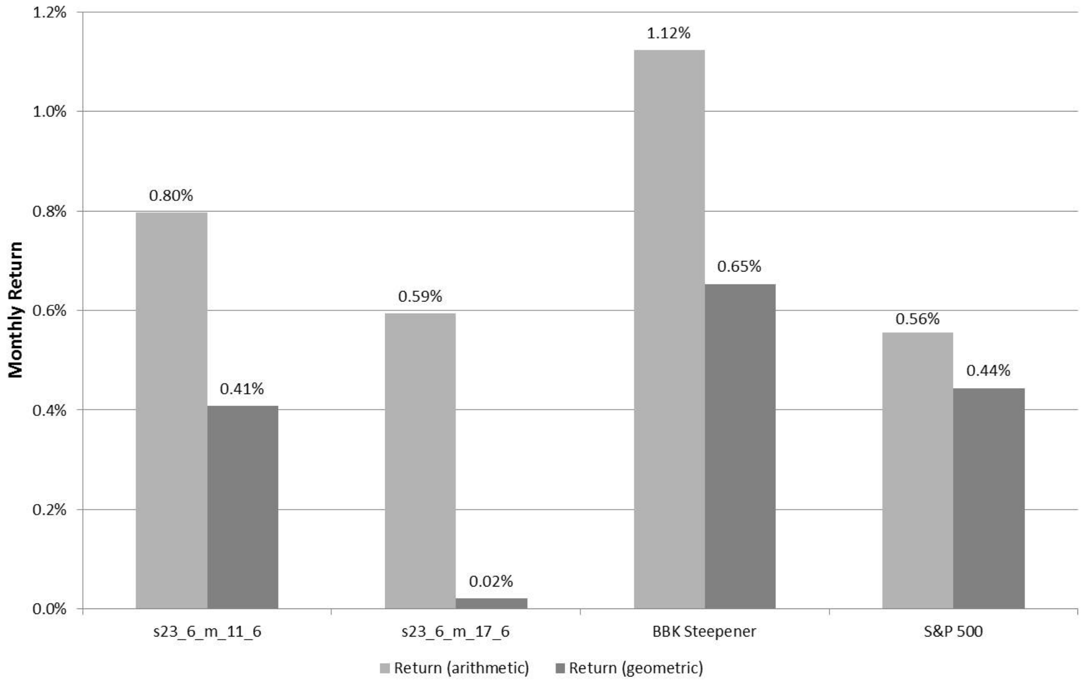


Table 1-1: **Descriptive Statistics**

Notes: The Table presents descriptive statistics for the observations of our sample. *Declaration Date to Ex-Div Date* measures the time period, in calendar days, between the ex-dividend date and the date on which the board of directors declares a distribution (declaration date). *Ex-Div Date to Payment Date* measures the time period, in calendar days, between the date upon which dividend checks are mailed (payment date) and the ex-dividend date. *Dividend yield* measures the ex-day cash dividend as fraction of the previous day closing price of the stock. The descriptive statistics are shown for the sample period July 1963 to 2011, and for the subperiod 1996 to 2009 on which the BBK study is based. All variables are winsorized at the 1 and 99 percentiles.

			Percentiles					
	Mean	Std. Dev	5%	25%	50%	75%	95%	NObs
1963.7-2011								
Declaration Date to Ex-Div Date	19.21	16.60	5	8	13	25	55	457,233
Ex-Div Date to Payment Date	22.30	7.21	12	17	21	26	36	463,544
Dividend Yield	0.91%	0.64%	0.19%	0.48%	0.74%	1.19%	2.20%	463,633
1996-2009								
Declaration Date to Ex-Div Date	20.15	17.45	6	9	13	26	57	148,120
Ex-Div Date to Payment Date	19.23	6.33	9	16	18	22	33	152,941
Dividend Yield	0.73%	0.60%	0.14%	0.40%	0.56%	0.86%	1.95%	152,961

Table 1-2: **Daily Returns around Ex-Dividend Dates**

Notes: The Table presents daily excess returns, standard deviations and abnormal returns (in basis points), of a portfolio composed of ex-dividend companies only around ex-dividend dates. Specifically, at the end of each day, the portfolio invests equally in ordinary common stocks that trade ex (cash) dividend the following day. Returns are measured according to the standard definition of holding period returns, that is, as the ratio of dividends and price changes over previous closing price. The portfolio is rebalanced every day and, on average, consists of 38.41 companies. *Excess returns* are defined as portfolio returns in excess of the risk-free return (one-month T-Bill). *Market model* shows the abnormal return (regression constant) from a regression of daily portfolio excess returns on the excess return of the market portfolio. *3-Factor Model* reports the abnormal return when the Fama-French three-factor model is used. Newey-West standard errors of mean estimates are shown in parentheses. Returns are winsorized at the 1 and 99 percentiles. The sample covers 12,070 trading days from 1963.7-2011.

Date	t(-5,-4)	t(-4,-3)	t(-3,-2)	t(-2,-1)	t(-1,0)	Ex-Div Day	t(1,2)	t(2,3)	t(3,4)	t(4,5)	t(5,6)
Excess Return	6.86	7.05	7.45	6.90	13.02	30.35	1.15	0.77	-0.11	1.27	0.89
s.d	83.47	83.95	84.06	83.03	82.26	84.44	83.54	83.20	82.88	82.30	82.70
s.e. mean	(0.87)	(0.86)	(0.87)	(0.85)	(0.86)	(0.88)	(0.85)	(0.85)	(0.84)	(0.85)	(0.86)
Market Model	5.72	5.91	6.18	5.75	11.88	29.30	-0.08	-0.49	-1.34	0.00	-0.45
	(0.58)	(0.58)	(0.58)	(0.57)	(0.58)	(0.63)	(0.56)	(0.55)	(0.55)	(0.57)	(0.57)
3-Factor Model	4.72	4.97	5.24	4.82	11.00	28.36	-1.01	-1.37	-2.28	-0.92	-1.45
	(0.53)	(0.53)	(0.53)	(0.52)	(0.53)	(0.58)	(0.51)	(0.52)	(0.52)	(0.52)	(0.53)
N(days)	12,070	12,070	12,070	12,070	12,070	12,070	12,069	12,067	12,067	12,066	12,064
N(obs)	463,304	463,331	463,356	463,406	463,497	463,633	463,477	463,400	463,111	463,010	462,905

Table 1-3: **Ex-Dividend Day Returns by Dividend Yield**

Notes: Panel A presents daily excess returns and standard deviations (in basis points) of a portfolio composed of ex-dividend day companies for various dividend yield subgroups. Specifically, at the end of each day, the portfolio invests equally in ordinary common stocks that go ex (cash) dividend the following day. The portfolio is rebalanced every day. Dividend yield measures the ex-day cash dividend as fraction of the previous day closing price of the stock. Excess returns are defined as portfolio returns in excess of the risk-free return (one-month T-Bill). For each dividend yield subgroup, I further report the median yield and the number of portfolio trading days. In Panel B, each year ex-day observations are sorted into dividend yield quartiles and average excess returns for observations in each quartile are presented. Heteroskedasticity-adjusted standard errors of means are reported in parentheses. Returns are winsorized at the 1 and 99 percentiles. The sample covers 1963.7-2011.

Panel A: Portfolios conditioned on dividend yields

Dividend Yield	All	$\geq 1\%$	$\geq 2\%$	$\geq 3\%$	$\geq 4\%$	$\geq 5\%$
Excess Return	30.35	41.78	56.83	125.02	214.52	265.09
s.d.	84.44	114.97	207.50	354.10	488.39	575.13
s.e. mean	(0.77)	(1.06)	(2.34)	(5.65)	(10.45)	(14.73)
Cond. Median D/P	0.83%	1.47%	2.47%	3.75%	5.36%	6.90%
Portfolio Days	12,070	11,675	7,856	3,928	2,183	1,525

Panel B: Observations sorted into D/P-Quartiles by year

D/P Quartiles	All	BotQ	Q2	Q3	Q4
Excess Return	29.21	14.95	22.74	35.77	43.40
s.d.	203.67	218.91	188.67	194.87	209.64
s.e. mean	(0.30)	(0.64)	(0.55)	(0.57)	(0.62)
Mean D/P	0.96%	0.34%	0.66%	0.95%	1.90%
s.d.	1.41%	0.17%	0.21%	0.31%	2.55%
Observations	463,633	115,927	115,913	115,901	115,892

Table 1-4: **Implicit marginal tax rates for S&P 500 Sample**

Notes: This Table shows median and weighted average *implicit marginal tax rates* for S&P 500 index constituents during February 1996 and October 2009. Implied marginal tax rates measure the tax disadvantage of dividends relative to capital gains and are reported unadjusted as well as adjusted for market or industry movements over respective measurement periods (see Section 1.5 for detailed variable definitions). *Ex-day* measures the implied tax rate over one day, *t(-2,2)* measures the implied tax rate over the four days surrounding the ex-dividend date, and *t(-5,5)* estimates the tax rate over the ten days surrounding the ex-dividend date. The sample only includes companies distributing cash dividends and consists of 19,368 observations. Weighted averages use the total market value of dividend distributions (dividend yield times market capitalization) to compute weights.

		Median tax rate	Div-weighted Avg.
Unadjusted	**Ex-day**	**0.194**	**0.184**
	t(-2,2)	0.393	0.292
	t(-5,5)	0.949	0.563
S&P 500-adjusted	**Ex-day**	**0.108**	**0.161**
	t(-2,2)	0.148	0.236
	t(-5,5)	0.267	0.224
Industry-adjusted	**Ex-day**	**0.101**	**0.133**
	t(-2,2)	0.056	0.109
	t(-5,5)	0.073	0.076

Table 1-5: **Adjusted Returns of the Short-term Asset**

Notes: This Table reports *adjusted* monthly returns of the *short-term asset*, where the holding period return definition is adjusted to account for implied tax rate disadvantages of dividends. Specifically, using the unadjusted return series reported in van Binsbergen, Brandt, and Koijen (2012), we decompose reported monthly holding period returns into two parts, a capital appreciation component and a dividend component. Monthly dividend components are then adjusted using various estimates of implicit tax rate measures. Finally, a series of monthly *adjusted* holding period returns is constructed and mean return, standard deviation, and Sharpe ratio for the *short-term asset* are analyzed. In columns (1) and (3), I adjust the dividend series using the median and dividend-weighted average ex-day implied tax rate of S&P 500 index constituents estimated over the whole sample period (see Table 1-4). In columns (2) and (4), each monthly return is adjusted using the respective ex-day implied tax rate estimated over the contemporaneous month. Results are reported for unadjusted ex-day implied tax rates as well as for rates corrected for market or industry movements (see Section 1.5 for detailed variable definitions). For comparison, I also show the unadjusted monthly mean return, standard deviation, and Sharpe ratio for the *short-term asset* reported in van Binsbergen, Brandt, and Koijen (2012). The average monthly return of the S&P 500 index is 0.0056 and the risk-free rate averages 0.0028 per month. The sample period spans 165 months from February 1996 to October 2009.

Adjustment Method (Estimation)		Median Tax Rate		Div-weighted Tax Rate	
		of sample	per month	of sample	per month
		(1)	(2)	(3)	(4)
Unadjusted Tax Rate	**Mean**	**0.0015**	**0.0014**	**0.0020**	**-0.0019**
	Std. Dev.	(0.0777)	(0.0829)	(0.0777)	(0.0892)
	Sharpe ratio	-0.0166	-0.0174	-0.0100	-0.0524
S&P 500-adjusted Tax Rate	**Mean**	**0.0060**	**0.0042**	**0.0032**	**0.0001**
	Std. Dev.	(0.0778)	(0.0794)	(0.0777)	(0.0860)
	Sharpe ratio	0.0409	0.0175	0.0052	-0.0315
Industry-adjusted Tax Rate	**Mean**	**0.0063**	**0.0049**	**0.0047**	**0.0032**
	Std. Dev.	(0.0778)	(0.0798)	(0.0778)	(0.0916)
	Sharpe ratio	0.0453	0.0258	0.0237	0.0040
As Reported (BBK)	**Mean**	**0.0116**			
	Std. Dev.	(0.0780)			
	Sharpe ratio	0.1124			
Monthly Dividend Yields	*Short-term Asset*	**0.0520**			
(% of total monthly return)		(449%)			
	S&P 500 Index	**0.0015**			
		(27%)			

Table 1-6: **Implicit marginal tax rates and returns for subsamples**

Notes: This Table shows median and dividend-weighted average *implicit marginal tax rates* for S&P 500 index constituents as well as returns of the *short-term asset* and the S&P 500 index as reported in BBK for two subsamples: 1996:2 - 2002:12 and 2003:1 - 2009:10. Implied tax rates are measured on ex-dividend days and measure the tax disadvantage of dividends relative to capital gains and are reported unadjusted as well as adjusted for market or industry movements. In column (6), I compute a slope measure of the term structure of the equity premium as difference of average returns of the *short-term asset* and the S&P 500 index for each subsample. The monthly dividend yield of the *short-term asset* is approximately 5.5% in the first half sample and 4.9% in the second half sample.

	Median (Div-weighted avg.) implicit tax rate estimates			**Reported mean returns and TS slope (BBK)**		
	Unadj.	S&P 500-adj.	Ind.-adj	$R_{ST,t}$	$R_{SP500,t}$	$R_{ST,t} - R_{SP500,t}$
	(1)	(2)	(3)	(4)	(5)	(6)
First half sample	0.219	0.187	0.174	0.0159	0.0065	**0.0094**
1996:2 - 2002:12	(0.414)	(0.388)	(0.270)			[t=0.99]
Second half sample	0.163	0.046	0.051	0.0072	0.0046	**0.0026**
2003:1 - 2009:10	(0.048)	(0.027)	(0.053)			[t=0.57]

Table 1-7: **Replication and Analysis of alternate Term Structure Segments**

Notes: The Table presents return summary statistics of synthetic dividend assets with varying maturities (trading strategies devised to generate these returns are described in Appendix 1.9.2). Asset *s23_6* replicates the *short-term asset* of BBK and asset *s23_6_m_11_6* replicates the *steepener* of BBK.

	Short-term Assets								*Steepeners*	
Trading Strategy	s23_6	s17_6	s11_3	s11_6	s8_3	s5_3	s3_1	s2_1	s23_6_m_11_6	s23_6_m_17_6
Average Maturity (years)	**1.68**	**1.18**	**0.80**	**0.68**	**0.55**	**0.30**	**0.22**	**0.14**		
Mean Dividend Yield	0.052	0.073	0.104	0.135	0.157	0.276	0.366	0.608	0	0
Median Dividend Yield	0.049	0.068	0.097	0.116	0.140	0.245	0.329	0.543	0	0
Mean Return (arithmetic)	**0.0134**	**0.0229**	**0.0307**	**0.0953**	**0.0935**	**0.1170**	**0.1262**	**0.2993**	**0.008**	**0.0059**
s.d.	0.1164	0.1742	0.2536	0.6886	0.6958	0.9665	0.6269	1.8281	0.088	0.1091
s.e. mean	0.0091	0.0136	0.0197	0.0536	0.0542	0.0771	0.0505	0.1503	0.0069	0.0085
Excess over S&P 500 (arithm.)	0.0078	0.0173	0.0251	0.0898	0.0879	0.1113	0.1216	0.2931	0.0024	0.0004
Corr(BBK short-term asset)	0.43	0.33	0.24	0.07	0.07	0.07	0.25	0.10	0.45	0.36
Corr(BBK steepener)	0.44	0.33	0.26	0.11	0.11	0.08	0.20	0.08	0.45	0.40
Mean Return (geometric)	**0.0069**	**0.0102**	**0.0072**	**0.0189**	**0.0105**	**0.0097**	**0.0218**	**0.0528**	**0.004**	**0.0002**
Excess over S&P 500 (geom.)	0.0025	0.0057	0.0028	0.0145	0.0061	0.0052	0.0182	0.0476	-0.0004	-0.0042
Number of months	165	165	165	165	165	157	154	148	165	165
Mean Observations/month (exist)	22.3	27.5	29.2	35.2	33.6	44.4	36.4	53.6	28.8	24.9
Mean Observations/month (used)	16.3	19.9	21.4	24.5	23.7	30.6	26.6	37.4	20.4	18.1

Table 1-8: **Tax Regime Effects on Returns of Short-term Assets of various maturities**

Notes: The Table presents effects of tax regime changes on excess returns of replicated *short term assets* of various maturities (the replicated short-term assets are described in Appendix 1.9.2). The dependent variable $R_{ST,t} - R_{SP500,t}$ is the return of the short term asset in excess of that of the S&P 500 index and thus represents a measure of slope of the term structure of equity risk. The dummy *Low Tax Regime* captures the "Bush tax cuts" in 2003 that removed major tax disadvantages of dividends and is defined as 0 for the period prior to 2003 and 1 for the period 2003-2009. Dividend yields of these short-term assets are monotonically decreasing in the maturity (e.g., see Figure 1-3), which predicts a differential impact of changes in the dividend tax regime on measured (i.e., pre-tax) returns of these assets. Newey-West standard errors are shown in parentheses. Coefficients with ***, **, and * are statistically significant at the 1%, 5%, and 10% levels, respectively.

Variables:	$R_{ST,t} - R_{SP500,t}$							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constant	0.0145	0.0312*	0.0464	0.172*	0.170*	0.2270	0.211***	0.537*
	(0.0120)	(0.0186)	(0.0286)	(0.0927)	(0.0933)	(0.1490)	(0.0663)	(0.3000)
Low Tax Regime	-0.0133	-0.0279	-0.0428	-0.166*	-0.164*	-0.2210	-0.170**	-0.4560
	(0.0128)	(0.0195)	(0.0295)	(0.0931)	(0.0938)	(0.1500)	(0.0739)	(0.3050)
Short-term asset	23_6	17_6	11_3	11_6	8_3	5_3	3_1	2_1
Average Maturity (years)	1.68	1.18	0.80	0.68	0.55	0.30	0.22	0.14
Observations	165	165	165	165	165	157	154	148
R2	0.003	0.006	0.007	0.015	0.014	0.013	0.019	0.016

Chapter 2

The Private Returns to Public Office

2.1 Introduction

Understanding the motivations of politicians is a central question in economics and political science. It is crucial for modeling the pool of candidates that will seek office, and also important for designing policies to constrain politicians' behavior while in office. Individuals may stand for election because of the non-pecuniary benefits of public service, or because of the financial returns that come with political office. The latter may include official salaries, private sector opportunities after leaving office, and also non-salary earnings while in office, legal or otherwise. There is relatively limited evidence on the returns to public office in large part because, at least until recently, unofficial earnings have seldom been reported publicly.

In this paper, we examine the net financial returns for public officeholders in India, taking advantage of data gathered via India's Right to Information (RTI) Act. Since 2003, the RTI has required all candidates standing for public office at all levels to disclose the value and composition of their assets. Disclosure was mandatory, with punitive consequences for misreporting. We calculate the asset growth of politicians using the disclosures of those competing in consecutive state assembly elections and use these figures to compare the asset growth of election winners versus election runners-up.

A common challenge in estimating the value of public office is to account properly for the unobserved skills or resources available to politicians regardless of whether they are elected. To provide a plausible group of 'control' politicians, we focus on the subset of elections where both winner and runner-up from the same constituency run in the subsequent election, allowing us to compare the asset growth of plausibly similar political candidates. When we further limit the sample to very close elections, we argue that any difference in asset growth is unlikely to be driven by unobserved ability differences between winners and runners-up.

In our baseline specifications, we find that winning politicians' assets grow at 3 to 4 percent per year faster than the assets of runners-up; the estimated "winner's premium" is slightly higher for politicians winning in close elections (we consider winning margins of 10, 5, and 3 percentage points). When we use a regression discontinuity (RD) design, we estimate a winner's premium of 4.5 percent.

To understand the mechanism underlying the high returns of election winners, we examine the geographic and candidate-level heterogeneity in the winner's premium. First, we examine whether the winner's premium is higher in more corrupt constituencies, as one would predict if it were the result of bribery and other forms of rent-extraction. We proxy for corruption by focusing on constituencies in the so-called BIMARU states (Bihar, Madhya Pradesh, Rajasthan, and Uttar Pradesh) that have been singled out for corruption (see, for example, Bose (2007)).[1] Our estimates indicate that for BIMARU politicians, the winner's premium is more than twice that of lawmakers in other states. Employing an RD design, we observe even starker differences: we estimate a winner's premium of more than 10 percent per year for BIMARU politicians, whereas we observe no discontinuity at the winning margin in non-BIMARU states. We find similar results using alternative corruption proxies, including BIMAROU designation (which augments the BIMARU list with the state of Orissa), as well as Transparency International's state-level corruption index from 2005.

We also assess how the extent of political power - and the resultant funds at a politician's disposal - affect the returns to office. We find that despite *similar official salaries*, the winner's premium for state ministers is more than 10 percent higher than for non-minister winners. Interpretation of this estimate can be confounded by the fact that assignment to minister posts is non-random. To deal with concerns of unobserved ability correlated with minister assignment, we compare the asset returns of candidates who obtain minister positions in the period we study, to politicians who were ministers in the past, won in this election, but do not hold ministerial posts during our sample period simply because of a shift in a state's ruling party. For this sample of 'minister-quality' politicians, we still find a large and significant

[1] http://www.livemint.com/Companies/p1lgqFU3hlQjPlM955E4CO/Ashish-Bose–The-man-who-coined-the-term-8216Bimaru821.html

asset growth premium for holding ministerial positions, of more than 6 percent per year.

As a separate measure of political advancement, we examine the winner's premium of incumbents versus candidates that had not recently held office. We find relatively low financial returns to winning for "freshman" politicians. Indeed, the point estimates imply a negative return to public office for non-incumbents, suggesting that their returns from private sector outside options are comparable to or even higher than the returns obtained through public office. By contrast, for incumbents our estimate of the winner's premium is more than 12 percent.

Finally, we consider a pair of further analyses that are less subject to selection concerns. First, we examine the returns to political office of "seasoned candidates." Specifically, we focus on contests between pairs of politicians where both had competed and been winner or runner-up in the two elections prior to 2003. We argue that these "seasoned" politicians are very likely to have similar abilities and outside options, and we obtain similar (though larger) estimates for the winner's premium using this subsample. Second, we look at a quasi-experiment in the state of Bihar where a hung assembly in February 2005 resulted in a follow-up election in October of the same year. By looking at candidates that won in February but lost in October, and vice-versa, we argue that we come as close as possible to providing a causal estimate of the returns to public office. The Bihar quasi-experiment yields similar estimates to our main analysis of BIMARU states.

Overall, our main empirical findings are best explained by a model of rent-seeking in political office where the scope for rent extraction increases as politicians rise in the legislative hierarchy: 'freshman' returns are negative relative to outside options, incumbents and seasoned candidates benefit from a substantial winner's premium in asset growth, and ministers benefit from a further asset growth premium over and above that of incumbents.

Our study contributes to the literature on politicians' motivations for seeking public office. There exist numerous theoretical models describing politician motivation and behavior. These include the seminal contributions of Barro (1973), Ferejohn (1986) and Buchanan (1989), as well as more recent work by Besley (2004), Caselli and Morelli (2004), and Matozzi and Merlo (2008). A number of recent papers examine empirically the role of official wages in motivating politician labor supply, including Ferraz and Finan (2011) and Gagliarducci and Nannicini (forthcoming) for Brazilian and Italian mayors respectively; Kotakorpi and Poutvaara (2011) for Finnish parliamentarians; and Fisman et al. (2013) for Members of the European Parliament. Diermeier et al. (2005) further consider the role of career concerns for Members of Congress in the United States.

Our work connects most directly to prior studies that examine the wealth accumulation of politicians, which have focused primarily on U.S. and British lawmakers. Lenz and Lim (2009) compare the wealth accumulation of U.S. politicians to a matched sample of non-politicians from the Panel Study on Income Dynamics. Their results suggest little benefit from public office. Using a regression discontinuity design, Eggers and Hainmueller (2009) find that British Conservative party MPs benefit financially from public office while Labour MPs do not. Finally, Querubin and Snyder (2009) examine the wealth accumulation of U.S. politicians during 1850-1880 using a regression discontinuity design and find that election winners out-earn losers only during 1870-1880.

We view our work as complementary to these studies in several ways. First, India differs from the US and the UK in having far greater corruption (Transparency International 2000). This is critical in considering the mechanism through which politicians benefit from office. For example, in Eggers and Hainmueller, which bases their measure of wealth accumulation on assets at time of death, the primary source of financial benefit appears to be legitimate post-office employment as company directors. By contrast, both our focus on India, combined with our finding of a higher winner's premium in high corruption states, points to rent-seeking while in office as the source of asset growth. Second, since our data afford the opportunity to examine the asset growth of politicians of differing degrees of experience and influence—in particular ministers versus rank-and-file MLAs—our findings are better able to shed light on the nature of political rent-seeking in a political hierarchy.[2]

Closest to our study is the concurrent work of Bhavnani (2012), which also examines politicians' wealth accumulation in India based on mandatory asset disclosures. Given the similarities, it is important to note the distinguishing features of our work. Bhavnani's data include information on elections in 11 states, while we have a much more comprehensive

[2]Our work also relates to several studies that attempt to infer the non-salary financial benefits of public office. Two recent papers examine the stock-picking abilities of U.S. legislators over different time periods, and with widely disparate results - Ziobrowski et al. (2011) reports high positive abnormal returns for Senators and members of the House of Representatives, while Eggers and Hainmueller (2011) reports that Congress members' portfolios *underperform* the market. Braguinsky et al. (2010) estimate the hidden earnings of public servants in Moscow by cross-referencing officials' salary data with their vehicle registrations.

database covering elections in 24 states. This affords a number of crucial advantages. Most importantly, this allows us to examine how the asset returns premium of winners varies across states as a function of proxies for corruption. The fact that the winner's premium is significantly higher in corrupt states helps to rule out many alternative explanations and connect our findings more credibly to rent-seeking. Second, our approach of exploiting past ministers—who are elected to the assembly but lose their minister portfolios as a result of their parties no longer forming the government—presents a credible counterfactual to benchmark our measure of the minister asset growth premium, helping to rule out explanations based on parties selecting "higher-quality" politicians as ministers.[3]

Finally, we note that while our study focuses on India, comparable asset disclosure laws now exist for politicians in many countries. It is in theory possible to employ a similar approach in other countries where candidates for public office are required to disclose their assets, and where these disclosures are subject to legal sanction and/or media scrutiny. This presents a promising avenue for future research.[4]

In the next section, we provide a detailed description of the data followed, in Section 3, by institutional background on Indian politics and disclosure laws. We follow in Section 4 with a simple model that will help to organize our results and motivate the empirical strategy. Section 5 presents our results, where we estimate the winner's premium and its correlates using both a regression approach and also a regression discontinuity design. In Section 6, we provide a discussion of selection concerns and also consider several alternative explanations for the winner's premium, and argue that it is difficult to reconcile these explanations with our full set of findings. We provide our conclusions in Section 7.

2.2 Data

National Election Watch, in collaboration with ADR, provides digitized records based on affidavits from candidates in state assembly elections (Vidhan Sabha). These records serve as

[3]Our specifications also differ in a number of ways - for example, we focus on assets net of liabilities, a standard measure of wealth, while Bhavnani focuses only on assets. This distinction is potentially important in the presence of, for example, preferential loan access for politicians which would mechanically inflate asset measures. Our larger sample also means that we can, in most cases, include analyses that allow for constituency fixed-effects, which helps to rule out many explanations for the winner's premium based on unobserved differences across candidates. Finally, our sample is formed by matching disclosures across elections by hand rather than via a matching algorithm, which may lead to fewer errors based on variability in the spelling of names.

[4]The comprehensive overview of politician disclosure laws in Djankov et al. (2010) provides an indication of the widespread adoption of such laws.

the basis for most of our dataset. For the nine elections in our sample prior to October 2004, however, digitized records were unavailable, so we collected data directly from scanned pictures or PDFs of candidate affidavits. The affidavits were gathered from either the GENESYS Archives of the Election Commission of India (ECI)[5] or the various websites of the Office of the Chief Electoral Officer in each state. A sample affidavit is shown in Online Appendix A.

Since reporting requirements are limited to those standing for election, asset growth can only be measured for re-contesting candidates, i.e., those that contest and hence file affidavits, in two elections. Therefore, our study is limited to elections in the 24 states which had at least two elections between November 2003 and May 2012, covering about 94 percent of India's total electorate. Table 1 lists the 24 states in our sample along with descriptive information corresponding to the first of the two elections.

In a first step, among all the candidates that contest in the first election in each state, we filter out the winners and the runners-up using the Statistical Reports of Assembly Elections provided by the Election Commission of India (ECI).[6] We then match the names of these winners and runners-up with candidates that contest in the subsequent election in that state (that is, we can account for politicians that choose to rerun but switch constituencies within a state across elections). Due to the many commonalities among Indian names as well as different spellings of names across elections, matching was done manually. Overall, we are able to manually match a total of 3,715 re-contesting candidates (2,347 winners and 1,368 runners-up from the first elections) based on variables such as name, gender, age, education, address, and constituency, as well as family members' names (usually the name of the father or spouse).[7]

Of these initial 3,715 candidates that competed in consecutive elections, we were unable to locate affidavits for both elections for 53 candidates because of broken web links and hence discard them from our sample. Further, we filter out candidates with affidavits that are poorly scanned, have missing pages, or handwriting that is too unclear or ambiguous to get a clear picture of a candidate's reported financial situation. This eliminates a total of 573 candidates,

[5]http://eci.gov.in/archive/

[6]http://eci.gov.in/eci_main1/ElectionStatistics.aspx

[7]A probabilistic matching algorithm, based on variables such as name and age, proved to be inefficient. To provide an example, in the Tamil Nadu Election of 2006, there are 2 candidates with identical names (RAJENDRAN.S), Age (56), and education (10th Pass) despite being identifiably distinct politicians. We also commonly encountered differential spellings of names between elections, for instance, Shakeel Ahmad Khan (Bihar, 2005) and Shakil Ahmad Khan (Bihar, 2010).

or about 15.6 percent of the remaining sample.[8] Next, we verify suspicious values and, since our main focus is on growth in wealth, remove candidates that list significant assets without corresponding market value information, leaving a sample of 3,021 matched candidates (1,911 winners and 1,110 runners-up). Of these 3,021 candidates, we have 658 constituencies in which both the winner and the runner-up re-contest in the following election.

From the affidavits, we compute each candidate's *Net Wealth* at the time of filing, just prior to each election. In each case, we define net wealth as the sum of movable assets (such as cash, deposits in bank accounts, and bonds or shares in companies) and immovable assets (such as land and buildings) less liabilities (such as loans from banks), aggregated over all dependent family members listed on the affidavit. Finally, we remove candidates with negative or extremely low net asset bases using a cutoff of beginning net worth of Rs 100,000.[9] This yields a final sample of 2,810 matched candidates (1,791 winners and 1,019 runners-up) of which 1,140 are constituency-matched pairs, i.e., we have 570 constituencies in which both the winner and runner-up recontest. The numbers in parentheses in the last column of Table 2-1 provide a state-level breakdown of these 570 constituencies. We define *Final Net Wealth* as net wealth at the end of the electoral cycle under consideration, and *Initial Net Wealth* as net wealth at the beginning of the cycle.

We also generate a number of control variables for our regressions. We define a *Criminal Record* dummy as equal to one if the candidate has pending or past criminal cases at the time of the first election, and measure education based on years of schooling (*Years of Education*). We collect data on election victory margins and incumbency from ECI's Statistical Reports of Assembly Elections. The reports also allow us to classify constituencies as Scheduled Caste (SC), Scheduled Tribe (ST), or "general" constituencies. SC and ST constituencies are reserved for candidates classified as SC or ST, in accordance with a policy introduced to promote the representation of historically under-represented groups; general candidates cannot compete in these constituencies. We also distinguish among winning candidates based on whether they went on to hold significant positions in the state government, using an indicator

[8] Affidavit availability and quality differs somewhat across states and tends to be slightly worse in the earlier years. For example, out of 54 matched candidates in Delhi (2003), 27 percent of affidavits are unavailable or of very poor quality.

[9] None of these adjustments materially changes the quantitative nature of our results. Our findings are very robust to using different cutoff values (e.g., Rs 500,000) or no adjustment at all. (This is shown in the Appendix, Tables B.8 - B.28.)

variable *Minister* to denote membership in the Council of Ministers, the state legislature's cabinet. To identify former ministers, we developed a list of all state-level ministers for the electoral cycle that preceded the 2003-2012 elections that we study here.[10] We then matched these names with our sample of re-contesting candidates, resulting in a total of 268 matches.

We use several state-level measures to proxy for opportunities for political rent extraction. First, we define an indicator variable, *BIMARU*, to denote constituencies located in the states of Bihar, Madhya Pradesh, Rajasthan, and Uttar Pradesh which, as noted in the introduction, have been singled out for corruption and dysfunction ("bimar" means sick in Hindi). The neighboring state of Orissa is often added to the group, leading to the acronym *BIMAROU*; we generate an additional indicator variable denoting constituencies located in one of these five states. We also use a perception-based corruption measure provided in a 2005 study on corruption by Transparency International India. This report constructs an index for 20 Indian states based on perceived corruption in public services using comprehensive survey results from over 10,000 respondents. The index takes on a low value of 240 for the state of Kerala and a high of 695 for Bihar. Our sample covers 17 of the 20 states for which the index is available; for ease of interpretation, we rescale the original measure such that it has a mean of zero and standard deviation of one, for the 17 states in our sample. There is a high degree of concordance between the Transparency measure, *TICorruption*, and the BIMARU classification. Three BIMARU states—Bihar, Madhya Pradesh, and Rajasthan—fill three of the five highest-corruption positions in the Transparency index, while Uttar Pradesh is ranked ninth out of 20.

Finally, we collected a cross-section of state legislature salaries during 2003-2008, and use the *Base Salary* of politicians to examine more formally whether official salaries are an important determinant of wealth accumulation.

Table 2-2 lists definitions of the main variables used in the analysis, and Table 2-3 provides descriptive statistics for our constituency-matched sample of 1,140 candidates (Panel A) as well as for candidates from the subsample of elections decided by margins of 5 percent or less (Panel B). The median of log(*Initial Net Assets*) is identical for winners versus runners-up, at 15.15. This corresponds to about Rs 3.8 million ($76,000 at an exchange rate of Rs 50 per dollar) for winners and for runners-up. The median of log(*Final Net Assets*) is 16.09 for

[10] Most information was sourced from archives of state government websites as well as an extensive review of newspaper articles.

winners, versus 15.93 for runners-up, a difference of 15.5 percent, given the log scale, and significant at the 10 percent level. Since there is an average of 4.9 years between the two snapshots of net assets, the difference between initial and final net assets implies a different rate of asset growth of 3.2 percent (15.7/4.9).

Winners and runners-up also differ based on incumbency. Incumbents are less likely to win than non-incumbents in our sample, consistent with Linden's (2004) finding of an incumbency disadvantage for Indian politicians. About 14 percent of winners are members of the state Councils of Ministers (by definition, zero percent among runners-up), and 18 percent of the elections in our sample are from SC/ST-designated constituencies. Runners-up in the subsample of close elections tend to be slightly more educated than winners on average (14.35 years of education vs. 13.69 for winners), though the median years of education is identical. Overall, based on these observables, runners-up seem to constitute a reasonably comparable control group.

2.3 Background

2.3.1 State-level electoral politics in India

As measured by revenues, there is a near-equal balance of power between the central and state governments in India (Global Financial Statistics, 2012). Among other responsibilities, state governments play a role in industrial development, the assignment of mineral rights, education, and health policy. These areas often fall under the ultimate control of members of the state Council of Ministers.

Members of the Legislative Assembly (MLAs) are elected to 5 year terms. In most cases an MLA's duties are only part-time work so they may continue to work in the private sector, albeit on a more limited basis.[11] This right to continue private sector pursuits while in office was reinforced by 2012 legislation affirming the right of lawyers to continue their practice while in office (Supreme Court Ruling under Advocates Act and Bar Council Rules, 2012). Six hundred of the 1,140 candidates in our main sample list a primary profession in public disclosures, as shown in Appendix Table B.1. The most common self-identified profession is agriculture (29.8 percent) followed by business (23.5 percent).

[11] In recent years, opportunities for elected MLAs to work concurrently in other *public* posts have opened up. Article 102 and 191 of the Indian Constitution barred legislators from collecting salaries from other public posts, but this was amended in 2006 to exempt 45 government posts from this disqualification.

The days in session for state assemblies is listed in Appendix Table B.2, ranging from a low of just over eight (Arunachal Pradesh) to 50.2 (Kerala) with a median of 28.2.[12] This is clearly a lower bound on the time required by holding office, which involves committee obligations, constituency meetings, and managing disbursement of local development funds.[13] Attendance rates, which are available only for a small set of states, are also included, and range from 23 to 95 percent with most states falling in the 70 to 90 percent range. These figures indicate that the average MLA could expect to lose about a month's worth of work days that might otherwise be devoted to private sector pursuits.

MLA salaries range from 2,000 to 70,000 rupees per month (though they also receive substantial expense allowances), with a median of 8,000 rupees (or 160 dollars at an exchange rate of 50 rupees to the dollar). This is potentially a material sum relative to the median annual asset accumulation of the MLAs in our sample of just over 900,000 rupees, if one thinks about the job of an MLA as comprised solely as time spent in the legislature. We consider below whether official salaries can explain the greater asset growth of winners.

Ministers face a much more stringent set of constraints on outside employment than rank and file MLAs. The same ruling that affirmed MLAs' right to continue legal practices stated explicitly that this did not extend to ministers, who are forbade from practicing law while in office. Also important is the fact that ministers receive official salaries that are only modestly higher than rank and file MLAs.

Neither regular MLAs nor ministers are required to divest themselves of commercial interests and many maintain active involvement in running businesses. A number of prominent and wealthy business owners have served as MLAs or Members of Parliament, most prominently Rahul Bajaj, chairman of the Bajaj group of companies with a personal net worth estimated as USD3.4 billion in 2012.

In addition to the personal asset disclosures that we exploit in this paper, campaign spending information is available for a subset of MLA candidates.[14] There is a cap on cam-

[12] Notably, these figures are uncorrelated with our measures of state-level corruption. We have insufficient overlap between data on attendance and our measures of corruption to examine whether these are related in their impact on asset accumulation.

[13] In the United States, for example, both the Michigan and Pennsylvania legislatures are considered full-time, though neither meets for more than 80 or so days per year. The bulk of state representatives' time is taken up with other duties.

[14] This set is limited by the data available on the myneta.info website, which had campaign finance information on only a subset of politicians for the 2011/2012 elections.

paign expenditures of 1.6 million rupees in 16 of the 25 states in our sample (in the remainder—mostly small states—the limit ranges from 800,000 to 1.4 million). We have collected campaign finance data on 359 of the MLAs in our sample who report, on average, expenditures of about 664,000 rupees. Importantly, campaign expenditures are uncorrelated with a candidate's status as winner versus runner-up (average expenditures of 651,000 rupees versus 681,000 respectively). Of course, there may also be differences in the time that standing politicians versus runners-up devote to election campaigns. There is little anecdotal evidence on this point in either direction, and we consider a number of tests below to examine this issue empirically.

To summarize, MLAs receive modest direct financial benefits for holding office, but are also required to devote a significant amount of time to participating in legislative activities.

2.3.2 Anecdotal evidence on the returns to public office

Journalists and investigators have uncovered a number of cases that directly implicate politicians in exploiting public office for financial benefit, often leading to criminal proceedings. These involve the trading of favors for bribes as well as direct theft of government funds. Documented bribery cases include payments in exchange for government contracts, such as the awarding of a Commonwealth Games contract to a Swiss firm in 2010 that reportedly cost the government over USD20 million. The mining industry is also thought to be afflicted with widespread corruption, with accusations of politicians and other public officials accepting bribes to facilitate illegal mining, the acquisition of concessions, and underpayment of royalties. In one high-profile case, for example, two state ministers from Karnataka, Janardhana Reddy and Karunakara Reddy, were arrested for illegal mining. The most infamous of recent bribe scandals, the 2G scam, involved the allocation of India's spectrum rights on the basis of bribes, costing the government billions of dollars in foregone revenues (see Sukhtankar (2013) for details).

High-profile embezzlement cases include the Fodder Scam, which involved the siphoning off of over USD200 million from Bihar's treasury and implicating, among others, the state's chief minister at the time. Uttar Pradesh's health minister was similarly implicated in the National Rural Health Mission Scam of 2011, which involved fraudulent claims that ran into the billions of dollars. A number of cases involve the embezzlement of assets instead of cash: in 2011, Karnataka's former chief minister, B.S. Yeddyurappa, was charged with acquiring government land parcels at extremely favorable prices before selling them off to mining companies.[15] Uttar Pradesh chief minister Mayawati Kumari was charged in connection with the Taj Corridor Scam, which involved the embezzlement of funds earmarked for upgrading tourist facilities near the Taj Mahal. (Separately, Kumari drew criticism for openly accepting gifts from the public on her birthday.)

Criminal cases need not be based on a specific, documented instance of bribery or embezzlement. Under the 1988 Prevention of Corruption Act, politicians with "resources or property disproportionate to his known sources of income" may be charged. This has resulted in proceedings against several Jharkhand ministers (including chief minister Madhu Koda) in 2009, who, investigators found, owned assets far greater in value than those declared in their election disclosures. Mayawati Kumari was charged under the same law based on evidence that she paid taxes on income far in excess of her modest chief minister's salary.

There are several notable attributes of the examples described above, which are largely representative of corruption cases highlighted in the Indian media.[16] First, they tend to focus almost exclusively on higher-level officeholders—ministers and even chief ministers—and involve diverse channels of rent extraction.[17] Additionally, many of the most flagrant abuses appear to come up in BIMARU states long noted for their lack of effective governance.

There is, to our knowledge, no systematic repository of corruption cases in India. Several sources provide some indication, however, that the above examples may be generalized beyond individual anecdote. First, a survey of public perceptions of corruption, conducted by *India Today* in 1997, asked Indian voters which part of government was most rife with corruption.[18] "Ministers" was ranked first, ahead of police. Respondents were also asked to name the country's most corrupt individual; all top responses, aside from a single prime minister,

[15] "Ministers stole millions in Karnataka mining scam," *BBC South Asia*, July 21, 2011

[16] Further details on several of these and other cases may be found in "Indian politicians in jail: Rendezvous of a different kind," *Gulf News*, November 5, 2011. This compendium is limited to examples of politicians serving jail time as a result of their behaviors in office.

[17] While the potential for rent extraction may be greatest for ministers, every MLA has control over a Local Area Development (LAD) fund of up to several million rupees, providing even lower-level politicians with opportunities to obtain rents. MLAs from some larger states have access to as much as 20 to 40 million rupees to spend at their discretion on the development of their constituencies. (The LAD program has been criticized in the Indian media as being a conduit for corruption. See, for example, "The lad fails to deliver," *Business Today*, March 6, 2011.)

[18] "India's Sleaze Sheet," *India Today*, November 24, 1997. Available at http://www.india-today.com/itoday/24111997/sleaze.html, last accessed December 14, 2013.

were state-level ministers. The high visibility of ministers could of course account for this result, which underscores the value of our empirical exercise.[19] Second, India's Association for Democratic Reforms (ADR) has compiled a list of MLAs that, in 2013, had declared cases against them for "Offences and Corrupt Practices in Connection with Elections."[20] While these cases reflect both corruption and effective enforcement, and election-related corruption is arguably different from that which we explore below, it is nonetheless noteworthy that the fraction of politicians with declared cases against them is more than twice as high in BIMARU versus non-BIMARU states (5.5 versus 2.3 percent).

2.3.3 Asset Disclosure Laws

Prompted by a general desire to increase transparency in the public sector, a movement for freedom of information began during the 1990s in India. These efforts eventually resulted in the enactment of the Right to Information Act (2005), which allows any citizen to request information from a "public authority," among other types of organizations. During this period, the Association for Democratic Reforms (ADR) successfully filed public interest litigation with the Delhi High Court requesting disclosure of the criminal, financial, and educational backgrounds of candidates contesting state elections.[21] Disclosure requirements of politicians' wealth, education and criminal records were de facto introduced across all states beginning with the November 2003 assembly elections in the states of Chhattisgarh, Delhi, Madhya Pradesh, Mizoram, and Rajasthan.

Candidate affidavits provide a snapshot of the market value of a contestant's assets and liabilities at a point in time, just prior to the election when candidacy is filed. In addition to reporting her own assets and liabilities, a candidate must disclose the wealth and liabilities of her spouse and dependent family members. This requirement prevents simple concealment of assets by putting them under the names of immediate family members. Further, criminal records (past and pending cases) and education must be disclosed.

Punishment for inaccurate disclosures may include financial penalties, imprisonment for up to six months, and disqualification from political office. While there have been a hand-

[19] We have found several partial listings of corruption cases pending in India (such as those referenced above), in which state-level ministers comprise a very high fraction of total cases relative to their total numbers. But this could also be a result of the high visibility of minister-level politicians.

[20] Further details, along with the list of cases, may be obtained at http://adrindia.org/content/sitting-mps-mlas-declared-cases-related-electoral-offences. Last accessed December 14, 2013.

[21] http://adrindia.org/about-adr/

ful of revelations of politicians' asset misstatements[22] and at least one prosecution, against Jharkhand minister Harinarayan Rai, for failing to disclose assets, for the most part popular accounts focus instead on the very high level of asset accumulation implied by these disclosures.[23]

2.4 Empirical Strategy

We present a simple model of electoral incentives based on the costs of running for office and the financial returns of private versus political employment.

We model a politician's career as lasting for two periods; candidates who contest elections in period 0 may recontest in period 1. Initially, we assume that periods are independent and that candidate i's probability of winning an election is given by p_i. The cost of running a political campaign is fixed as M in each period, which must be covered by the candidates themselves. We assume an initial wealth level of W^0 after payment of M (This is consistent with what we measure in the data). We denote returns for candidates by R_j where $j \in \{\mathcal{W}, \mathcal{L}\}$ denotes whether a politician won or lost the election. Our goal is to estimate $R_{\mathcal{W}} - R_{\mathcal{L}}$, i.e. the difference in rates of asset growth as a result of being in office. Differential return opportunities across constituencies c are captured by a_c, and candidate i's wealth growth can further be affected by her characteristics $\mathbf{x}_i$ such as, for example, level of education. We model candidate i's wealth dynamics as:

$$\frac{dW_{ic}^t}{W_{ic}^t} = (R_{\mathcal{L}} + (R_{\mathcal{W}} - R_{\mathcal{L}}) \cdot D_i + b'\mathbf{x}_i + a_c)dt + d\epsilon_i^t \tag{2.1}$$

where D_i indicates whether the candidate has been in office during the period and $d\epsilon_i^t \sim N(0, dt)$ captures idiosyncratic shocks to wealth. The log of final wealth accumulated by a candidate at $t = 1$ is given by:

$$logW_{ic}^1 = logW_{ic}^0 + (R_{\mathcal{W}} - R_{\mathcal{L}}) \cdot D_i + b'\mathbf{x}_i + \alpha_c + \epsilon_i^1, \tag{2.2}$$

where $\alpha_c = R_{\mathcal{L}} + a_c - 0.5$. Our objective is to measure final assets for the typical individual elected to public office, relative to the counterfactual where he was not elected:

$$\mathbb{E}(logW_{ic}^1 | D_i = 1) - \mathbb{E}(logW_{ic}^1 | D_i = 0) = R_{\mathcal{W}} - R_{\mathcal{L}} \tag{2.3}$$

[22] For example, Firstpost India reported that Himachal Pradesh MLA Anil Kumar failed to declare ownership of a pair of properties in his 2007 disclosure.

[23] See, for example, "How the political class has looted India," *The Hindu*, July 30, 2012, [http://www.thehindu.com/opinion/lead/how-the-political-class-has-looted-india/article3700211.ece].

Initially, we will assume that all candidates (winners and losers) recontest, which in the model corresponds to the case with $M = 0$. We then discuss the implications of relaxing this assumption. The equation (2.2) gives us an unbiased estimate of $R_{\mathcal{W}} - R_{\mathcal{L}}$ based on ordinary least squares, provided that ϵ_i^1 is uncorrelated with the regressors. However, given that winning candidates may differ from those that lose, the presence of omitted variables may generate a bias in the estimated coefficients. To confront this issue, our identification strategy focuses on close elections which, we argue, results in a comparison of the returns of very similar candidates (i.e., those that barely win versus those that barely lose).

Of course, in equation (2.3) we cannot measure winner versus loser wealth for politicians who are winners and so instead will make a comparison across observed winners i and losers j. That is, we observe:

$$\begin{aligned}\mathbb{E}(logW_{ic}^1|D_i = 1) - \mathbb{E}(logW_{jc}^1|D_j = 0) &= \mathbb{E}(logW_{ic}^1|D_i = 1) - \mathbb{E}(logW_{ic}^1|D_i = 0) \\ &+ \underbrace{\mathbb{E}(logW_{ic}^1|D_i = 0) - \mathbb{E}(logW_{jc}^1|D_j = 0)}_{\text{Selection term}} \qquad (2.4)\end{aligned}$$

Comparison of similar candidates ensures that the selection term underlined above is small.

While looking at close elections allows us to compare similar candidates, another mechanical selection arises when we allow $M > 0$ even in the case where ϵ_i^1 is independent of D_i, α_c, and $\mathbf{x}_i$. In the context of our simple model, selection arises from the fact that some candidates will be hit by negative wealth shocks that prevent them from recontesting at $t = 1$. Specifically, in order for a candidate to be observed in the sample, he must have sufficient funds to cover the election expense, $W_{ic}^1 \geq M$.[24] Given that the wealth of winners is larger than that of runners-up as a result of higher earnings in office, there is a natural discontinuity in the recontesting probabilities – winners are more likely to recontest elections than losers.[25] To understand how this affects our estimates, suppose that all candidates with sufficient funds

[24] Consistent with the model, we find that the runners-up that exit the sample have lower initial wealth (and are thus relatively more affected by wealth shocks).

[25] In this model, one can identify four cases in which wealth shocks may affect electoral participation for a given pair of candidates: (1) positive wealth shocks leading both candidates, winner and runner-up, to recontest, (2) large negative wealth shocks such that both candidates exit the sample, (3) negative wealth shocks such that only runners-up exit the sample, and (4) wealth shocks such that only the winner exits the sample. If one assumes that shocks to wealth are idiosyncratic and follow the same distribution for runners-up and winners, then it follows that case (3) is more likely than case (4) since it requires a relatively larger negative shock for winners to exit the sample.

wish to contest, and consider the selection equation capturing the recontesting decision z_i:

$$z_i = \left\{ \begin{array}{l} 1 \text{ if } logW^0_{ic} + R_j + b'\mathbf{x}_i + \alpha_c + \epsilon^1_i \geq logM \\ 0 \text{ if } logW^0_{ic} + R_j + b'\mathbf{x}_i + \alpha_c + \epsilon^1_i < logM \end{array} \right\}$$

We do not observe final wealth of candidates for which $z_i = 0$, and must therefore compare only candidates who recontest. Our estimate of the returns to office, denoted by $\hat{\beta}$, corresponds (in population) to the *difference* in expected values when D_i switches from 0 to 1. This can be written as:[26]

$$\begin{aligned} \hat{\beta} &= \mathbb{E}[logW^1_{ic}|\mathbf{x}_i, D_i = 1, z_i = 1] - \mathbb{E}[logW^1_{ic}|\mathbf{x}_i, D_i = 0, z_i = 1] \\ &= R_{\mathcal{W}} - R_{\mathcal{L}} \\ &+ \Big[\mathbb{E}[logW^1_{ic}|\mathbf{x}_i, D_i = 1, z_i = 1] - \mathbb{E}[logW^1_{ic}|\mathbf{x}_i, D_i = 1, z_i = 0]\Big] \cdot P(z_i = 0|\mathbf{x}_i, D_i = 1) \\ &- \Big[\mathbb{E}[logW^1_{ic}|\mathbf{x}_i, D_i = 0, z_i = 1] - \mathbb{E}[logW^1_{ic}|\mathbf{x}_i, D_i = 0, z_i = 0]\Big] \cdot P(z_i = 0|\mathbf{x}_i, D_i = 0) \end{aligned} \tag{2.5}$$

Thus, $\hat{\beta}$ is the true β plus a selection term generated by differential exit rates between winners and runners-up. The direction of possible bias in our estimate of the winner's premium will depend on the sign of the selection term.

Proposition 1 *If wealth shocks are i.i.d. across candidates and independent of D_i and $\boldsymbol{x}_i$, and $R_{\mathcal{W}} > R_{\mathcal{L}}$, then $\hat{\beta} < (R_{\mathcal{W}} - R_{\mathcal{L}})$. That is, our estimate of the private returns to public office is biased downward.*

Please refer to the appendix for the proof. The intuition for this bias is as follows: Since a greater proportion of runners-up will exit due to negative wealth shocks, had we observed these exiting candidates as well, our estimate of the average returns to office would have been larger. So, the selection bias driven by differential exit rates is negative.

Our parsimonious model ignores alternative sources of exit. For example, in addition to wealth shocks one could augment the model to allow for variation in candidates' outside options at the reelection date $t = 1$, so that $R_{ij,t=1} = R_{ij,t=0} + \eta_i$. Thus, a sufficiently large positive shock to outside opportunities would convince any candidate - winner or loser - to opt out of standing for election. If these shocks affect both winners and runners-up

[26] For ease of exposition, in the following expressions $\mathbf{x}_i$ includes all variables other than the election winner indicator, D_i.

symmetrically, they will not generate any differential exit and hence will not create bias. An upward bias in our estimate results only if such shocks have a disproportionately positive impact on runners-up.

2.5 Results

We present our results using three separate approaches. First, we provide a graphical depiction of candidates' net asset growth. We then present estimates of the winner's premium and its correlates using regression analyses, followed by a presentation of the results using a regression discontinuity design. After presenting our main results, we turn to a pair of alternative approaches to estimating the winner's premium based on "seasoned candidates" and a quasi-experiment resulting from Bihar's hung assembly in 2005.

2.5.1 Graphical presentation of results

We begin by presenting a series of figures that provide a visual description of our results. In Figure 2-1 we plot the Epanechnikov kernel densities of the residuals obtained from regressing log(*Final Net Assets*) on candidate observables, including log(*Initial Net Assets*). Panel A uses the entire sample of constituency-matched candidates while Panel B only uses candidates that were within a margin of 5 percentage points. In both cases, the Kolmogorov-Smirnov test for equality of the distribution function of winner and runner-up residuals is rejected at the 1 percent level. These figures thus suggest a differential effect of election outcomes on net asset growth between the treatment and control groups. Panel C limits the sample to candidates with constituencies in *BIMARU* states, while Panel D shows only candidates from non-*BIMARU* constituencies. Panel C shows a clear rightward shift for winners relative to runners-up, and we reject the equality of the distribution functions at the 1 percent level. By contrast, in Panel D, only a very small shift appears, and we cannot reject the test for equality of distributions (p-value of 0.215). Thus, the existence of a winner's premium is driven largely by candidates in high corruption states.

In Panel E, we disaggregate winners into ministers and non-ministers and plot kernel densities of these two groups alongside the full sample of runners-up. The kernel density plots indicate a higher rate of asset growth for ministers, and also suggest a long right tail for ministers, implying that a relatively small number of these high-level politicians generate very high asset growth.

Finally, Panels F and G disaggregate the sample based on whether an incumbent is standing for reelection in the constituency. Panel F shows winner and runner-up densities for the sample of constituencies where an incumbent was standing for reelection. The winner distribution is clearly shifted to the right, implying a greater winner's premium in races involving incumbents (a test for equality of the distribution functions is rejected at the 1 percent level). Panel G shows densities for the subsample of non-incumbent constituencies - the winner distribution is now slightly shifted to the left but a test for equality of the distribution functions cannot be rejected (p-value of 0.622).

2.5.2 Regression Analyses

We now turn to analyze the patterns illustrated in Figure 2-1 based on the regression framework we developed in Section 2.4. The basic estimating equation is given by:[27]

$$\begin{aligned} log(FinalNetAssets_{ic}) = \alpha_c + \beta Winner_{ic} + \delta_1 log(InitialNetAssets_{ic}) \\ + \delta_2' Controls_{ic} + \epsilon_{ic} \end{aligned} \tag{2.6}$$

These within-constituency estimates of the winner's premium are presented in Table 2-4. In the first column, we show the binary within-constituency correlation between the indicator variable *Winner* and log(*Final Net Assets*), including log(*Initial Net Assets*) as a control. The coefficient of 0.167 (significant at the 1 percent level) implies that, after accounting for initial net assets, winners finish a five year electoral cycle with 16.7 percent higher assets than runners-up. This is equivalent to an annual asset growth premium of 3.4 percent.[28] Column (2) adds controls for gender, incumbency, having a criminal record, the logarithm of years of education, as well as quadratic controls for age; the point estimate is virtually unchanged, at 0.164 (significant at the 1 percent level). In columns (3) - (5) we examine the winner' s premium in close elections, defined by those where the vote share gap between winner and runner-up was less than 10, 5, and 3 percentage points. In each case, we find that winners' assets are 16 - 21 percent higher than runners-up at the end of an electoral cycle, representing a 3 - 4 percent annual growth premium (significant at least at the 5 percent level). In

[27] Results are essentially unchanged when using *net asset growth* as the dependent variable. (This is shown in the Appendix, Tables B.30 - B.36.)

[28] 16.7/4.9 years; the average legislature period in our sample is 4.9 years.

Appendix Table B.29, we show that the interaction of $Winner_{ic}$ and log($InitialNetAssets_{ic}$) is negative but not significant, consistent with public office generating rents that are unrelated to politicians' initial wealth.

We next explore heterogeneity in the winner's premium, motivated by the background discussion in Section 2.3. Specifically, we will examine heterogeneity by state-level measures of corruption and level of position in government, both of which are associated, at least anecdotally, with scope for rent extraction.

If the higher asset accumulation of winners versus runners-up may be attributed to rent-seeking behavior, then we expect to see a greater impact of electoral success on asset growth in high corruption constituencies. We present in Table 2-5 results based on several measures of state-level corruption. Given that our variation in corruption is at the state-level, standard errors are clustered by state throughout the table. To account for the small number of clusters, we use the wild cluster bootstrap-t method as suggested by Cameron, Gelbach, and Miller (2008). We begin, in columns (1) and (2), with the sample split based on whether a constituency is located in a *BIMARU* state. The coefficient on *Winner* is twice as large for *BIMARU* relative to non-*BIMARU* states. In Column (3) we use the full sample, and include the interaction term *Winner***BIMARU*. The coefficient implies a winner's premium that is 0.136 higher in *BIMARU*-based constituencies, and the interaction term is significant at the 5 percent level.

In Columns (4) and (5) we present results employing two alternative state-level measures of corruption, *BIMAROU* and *TICorruption*. The point estimate for *Winner***BIMAROU* is 0.156 and significant at the 1 percent level.[29] The direct effect of *Winner* is reduced to 0.104. In column (5), we find that the interaction term *Winner***TICorruption* is positive, and significant at the 5 percent level; its magnitude implies that a one standard deviation increase in corruption is associated with an incremental 1.3 percent (0.063/4.9) higher annual asset growth rate for election winners. In Appendix Table B.3 we include state per-capita income interacted with *Winner* as a control in all regressions. This has a minimal effect on the magnitudes of the corruption-*Winner* interactions. The estimates remain significant at conventional levels apart from the *TICorruption***Winner* term, where the standard error increases three-fold.

[29] Given the larger point estimate using *BIMAROU*, it is not surprising that when we estimate (2.6) for Orissa alone, we obtain a relatively high estimate of the winner's premium of 0.28.

To the extent that the higher asset growth of election winners is the result of the advantage of office-holding itself - rather than unobserved differences between candidates that are correlated with holding office - there are two further predictions that suggest themselves. First, elected officials that are members of the ruling party or coalition should be better placed to benefit from holding office. Second, higher-level offices, where the potential for rent-seeking is greatest, should also be associated with particularly high asset growth. It is of particular note, in considering these two further hypotheses, that state-level legislators' official salaries do not depend at all on affiliation with the ruling coalition, and that ministers' official salaries are only slightly higher than those of rank and file politicians, while the time commitment required of minister positions is much greater.

We begin in Table 2-6 by comparing the returns of ruling party politicians to those who were elected but not part of the majority party or coalition. We denote ruling party or coalition members by the indicator variable, *Government*, and include it as well as the interaction term *Government**Winner* as covariates in Equation (2.6). The coefficient on the interaction term is 0.606, significant at the 10 percent level, while the direct effect of *Government* is negative and large in magnitude (-0.217), though not significant (p-value=0.207). The direct effect of *Winner* is negative, though not significant. Overall, our estimates indicate that the benefits of winning public office, relative to outside options, accrue exclusively to those who are part of the ruling government.

We next explore the effect of membership in the Council of Ministers (COM) on asset accumulation. Column (2) presents the results of our basic specification in Equation (2.6), augmented by the inclusion of *Minister*, an indicator variable denoting COM membership. The coefficient on *Minister* is 0.602, significant at the 1 percent level, implying a more than 12 percent higher asset growth rate, relative to non-minister winners.[30] The winner's premium is reduced to 0.083, implying that a significant fraction of the overall winner's premium is the result of very high asset growth rates for high-level politicians.[31] In column (3), we include both *Minister* and *Government**Winner* as covariates. The coefficient on *Minister*

[30] Note that, since all ministers are also election winners, it is not appropriate to include a *Winner**Minister* term.

[31] When we limit the sample to close elections, decided by margins of 10, 5, and 3 percent respectively, the point estimates for *Minister* - particularly for the 5 percent threshold - are marginally smaller than for the full sample. However, in all cases, they are significant at least at the 5 percent level. (This is shown in Appendix Table B.37.)

falls modestly, to 0.534, while the coefficient on *Government*Winner* falls by about a third, and is no longer significant at conventional levels (p-value=0.172). This indicates that a large fraction of the benefits to being a member of the governing party are the result of control over the Council of Ministers.

The primary concern in interpreting our results on the asset growth of ministers is that it could reflect the higher outside earnings of those with the skills and experience to obtain ministerial positions. To account for the unobserved attributes of "minister quality" candidates, we compare the returns of politicians who served as ministers during 2003-2012 to those of elected politicians who did not hold ministerial posts during 2003-2012 but had served as minister in a prior period. We argue that these former ministers - who were no longer in the cabinet primarily because their party was thrown out of office - serve as a plausible comparison group to control for the unobserved abilities of sitting ministers.

Since only a small subset of politicians ever hold ministerial posts, we cannot perform this analysis for our constituency-matched sample. We therefore return to our original set of 2,810 re-contesting candidates with usable affidavits and wealth greater than Rs 100,000 (see the Background and Data section), and utilize all candidates who held a ministerial post during 2003-2012, or the preceding legislative period. For this sample of present and past ministers, we show the results of a modified version of Equation (2.6), including *Minister* as the main covariate of interest, in Table 2-7. We include state fixed effects to account for unobserved differences in earnings opportunities across states. In our baseline results in column (1), the coefficient of 0.312 (significant at the 1 percent level) indicates that current ministers generate asset growth that is 6.4 percent (0.312/4.9) higher than politicians who previously served as ministers, but do not in the 2003-2012 electoral cycle. In column (2) we include *Incumbent* as a control, to account for the possibility that current minister status is simply picking up the effects of multiple terms in office, and find that our point estimate increases marginally to 0.343. In column (3), we include fixed effects for India's districts, representing a much finer set of controls for unobserved differences across candidates. Our point estimate on *Minister* increases to 0.439. Finally, in column (4), we further refine the sample to only include (i) current ministers and (ii) past ministers who won the current election but whose party was not a member of the ruling state government. This subsample allows us to tease out another "government effect": politicians of both groups won the current election *and* held a ministerial post at least once, but differ in that only one group's party was part of the government. Put differently, while the groups are very comparable in many dimensions, only the current ministers exercise control over large budgets during the period we study. The point estimate of *Minister* for this subsample is 0.236, significant at the 1 percent level. While not dispositive, this evidence strongly suggests that at least some component of the high asset growth for state ministers is likely the result of minister status itself, rather than unobserved characteristics correlated with holding that office.

In the remaining two columns in Table 2-7, we disaggregate assets into *Movable Assets*, holdings such as cash, bank deposits, and jewelry, and *Immovable Assets*, such as land and buildings (see the full definition in the Data section). As noted in Section 2.3, land acquisition is one channel through which politicians have been caught misusing their public offices. We find that, based on public asset disclosures, this is likely to be more prevalent among high-level politicians. The coefficient on *Winner* is a highly significant predictor of growth in movable assets, implying a winner's premium of 6.22 percent annually. The magnitude of the coefficient on *Minister* in (5) implies a further premium in movable asset growth of 6.35 percent annually, significant at the 10 percent level. For immovable assets, the minister growth premium is 7.59 percent and significant at the 5 percent level, while the winner's premium is small in magnitude and statistically insignificant. Note that immovable assets constitute, on average, about three quarters of a candidate's total assets, so this difference in form of asset accumulation helps to accentuate the differential rate of asset growth for ministers versus rank-and-file MLAs.

We next turn to examine the effect of incumbency, and more generally the impact of having more prior experience in government on asset accumulation. In Table 2-8 we include the interaction term *Incumbent*Winner* as a covariate. In column (1), we observe that its coefficient is very large in magnitude, 0.75, and significant at the 1 percent level. The point estimate on the direct effect of *Incumbent* is -0.29, implying that at least part of the reason for the larger winner's premium among incumbents is the low earnings of incumbents who fail to be reelected. This indicates that incumbent politicians may have weak private sector employment opportunities after spending a term in office. In column (2) we include *Minister* as a control, since attainment of high-level positions is correlated with tenure in state politics (the correlation between *Minister* and *Incumbent* for members of the ruling party is 0.21).

The inclusion of this control reduces the coefficient on *Incumbent*Winner* marginally, to 0.65 (significant at the 1 percent level), and has little effect on other coefficients. Finally, in column (3) we control for whether a candidate was elected to the assembly at t=-2 (i.e., the election that precedes the pair of elections we study here by two periods), denoted by the indicator variable *PriorMember*. The inclusion of *PriorMember* and its interaction with *Winner* has no effect on the measured effects of incumbency.

To recap our results thus far: We observe higher asset growth for politicians relative to runners-up, particularly in more corrupt states and for politicians that are incumbents or ministers.[32] These results fit with a model of politician rent-seeking where opportunities for rents grow with experience and progression through the political hierarchy.

We conclude this section by looking at the effect of a number of other personal and constituency attributes on candidates' asset growth. A measure of market-earnings potential often employed in the labor literature is education. In column (1) of Table 2-9, we include log(*Years of Education*) as a control, and also its interaction with *Winner*. In keeping with prior evidence on the returns to education, the coefficient on the direct effect of log(*Years of Education*)—reflecting earnings for non-winners—is positive, though not significant at conventional levels (p-value=0.11). Its interaction with *Winner* is negative, and its coefficient, -0.585, indicates a relatively modest return to public office for politicians with higher levels of education, who are likely to have relatively lucrative options in the private labor market.

In column (2) we include a measure of per capita income, approximated by the average state-level per capita net domestic product between 2004 and 2009, log(*Income per Capita*), taken from the Reserve Bank of India (RBI). The coefficient on the interaction of income and *Winner* is negative, though small in magnitude and not statistically significant.[33]

In column (3) we consider the set of constituencies reserved for members of disadvantaged groups, so-called Scheduled Tribes and Castes (SC/ST). The interaction term *SC/ST_Quota*Winner* is significant at the 5 percent level (p-value=0.016), and implies a winner's premium in as-

[32] While we report these results in separate tables for ease of exposition, when we include interactions of *Winner* interacted with both *BIMARU* and *Incumbent* as well as the direct effect of *Minister* in the same specification, our results are virtually unchanged. We have limited ability to examine heterogeneity in the effect of minister status, given the small number of ministers in our sample. Neither *Incumbent*Minister* nor *Minister*BIMARU* approaches significance in analyses that include these interaction terms. (This is shown in Appendix Table B.38.)

[33] Results are nearly identical when using a district-level measure of household income for 2008 instead. (This is shown in Appendix Table B.39.)

set growth of about 6 to 7 percent for constituencies reserved for SC/ST candidates. There are two primary explanations for the relatively high winner's premium for SC/ST-designated constituencies. First, since these seats are reserved for a limited set of potential candidates, it may slacken electoral competition, allowing candidates to extract greater rents without fear of losing their positions. Alternatively, SC/ST politicians may have less lucrative private sector options as a result of discrimination, lower unobserved skill levels, or weaker labor market opportunities in SC/ST-dominated areas. While we cannot include both the direct effect of *SC/ST_Quota* and constituency fixed effects in a single specification, column (4) shows the direct effect of SC/ST quotas with a coarser set of fixed effects, at the district level. There are approximately half as many districts as constituencies in our main sample. We find a very similar coefficient on the interaction term *SC/ST_Quota*Winner* in this specification - approximately 0.33 - while the direct effect of *SC/ST_Quota* is -0.31. These estimates suggest that among runners-up, SC/ST politicians fare significantly worse than other candidates, consistent with the high winner's premium in SC/ST constituencies resulting in large part from different private sector opportunities.

We show the interaction of *Female* and *Winner* in column (5). The coefficient is positive and marginally significant. Finally, in column (6) we interact *Winner* with log(*Base Salary*). We find no evidence that the winner's premium is higher in states with more generous official salaries for legislators, implying that it is unlikely that official salaries play a major role in the differential asset accumulation of elected officials.

2.5.3 Regression Discontinuity Design

An alternative identification strategy is based on a regression discontinuity design, with the winner's premium identified from the winner-loser differential in close elections. In this section, we explicitly model the value of winning using regression discontinuity methods. We show a series of figures that depict our tests for discontinuities around the winning threshold, followed by estimates of winner-loser discontinuities.

We calculate the discontinuity using a local linear regression approach as suggested by Imbens and Lemieux (2008), and employed by Querubin and Snyder (2011) in a similar context to our own. Specifically, we augment (2.6) by the variable $Margin_{ic}$ and use the subsample of elections that were decided by margins of 5% or less.[34] As shown in Table 2-3,

[34] This is comparable to the regression analyses above, limiting the sample to elections decided by margins of

covariates for winners and runners-up are fairly balanced for this set of close elections.[35]

The scatterplots and lines of best fit we show alongside our estimates of the winner's discontinuity are produced using common methods developed in the regression discontinuity literature (e.g., DiNardo and Lee (2004), Imbens and Lemieux (2008) and Angrist and Pischke (2009)). First, we generate residuals by regressing log(*Final Net Assets*) on candidate observables, including log(*Initial Net Assets*), gender, incumbency, and age, but excluding *Winner* and *Margin*. We next collapse and plot the residuals on margin intervals of size 1 percentage point (margins ranging from -25 to +25) and also plot estimates of the following specification:

$$\bar{R}_i = \alpha + \tau \cdot D_i + \beta \cdot f(Margin(i)) + \eta \cdot D_i \cdot f(Margin(i)) + \epsilon_i \tag{2.7}$$

where $\bar{R}_i$ is the average residual value within each margin bin i, $Margin(i))$ is the midpoint of margin bin i, D_i is an indicator that takes a value of one if the midpoint of margin bin i is positive and a value of zero if it is negative, and ϵ_i is the error term.[36] $f(Margin(i))$ and $D_i \cdot f(Margin(i))$ are flexible fourth-order polynomials.

In columns (1) - (7) of Table 2-10, Panel A, we show discontinuity estimates of (2.6) using local linear regressions as described above, while in Figure 2-2, Panels A - G, we present accompanying graphs to illustrate visually our discontinuity estimates.[37] We additionally present our discontinuity estimates based on the procedure employed in our graphs in Panel B of Table 2-10, to allow for a comparison of discontinuity estimates illustrated in the graphs and those obtained from local linear regressions.[38]

5% or less. In Appendix Tables B.40 - B.44, we repeat all of our regression analyses using this subsample, and find that the results are almost always very similar both in terms of magnitudes and statistical significance. There are two differences worth noting. First, the minister premium based on the set of close races is lower than for the full sample, and is closer to the figure we obtain with our analysis that exploits ex-ministers as the counterfactual. Interestingly, when the minister premium is measured based on the ex-minister counterfactual, our estimates are almost the same whether we use the full sample or the subsample of close elections. Second, the difference in movable versus immovable asset growth for rank-and-file MLAs versus ministers is even sharper for the close election subsample, where we find *no* effect of minister status on movable assets (and no effect of winner status on immovable assets).

[35]For robustness, we also repeat the analysis for different subsamples and including higher-order polynomials in *Margin*.

[36]To address heterogeneity in the number of candidates and residual variance within each bin, we weight observations by the number of candidates, and alternatively by the inverse of within-bin variance. Results are similar in both specifications.

[37]Note that the symmetries in the RD plots are the result of constituency fixed effects. Including constituency fixed effects allows us to control for observable and unobservable constituency-level heterogeneity such as differences in local labor markets or *SC/ST_Quota*.

[38]Note that while the scatterplots we show are generated via collapsed data, the results reported in Panel B

For the full constituency-matched sample, the discontinuity estimate indicates a jump in the residual values around the threshold. The point estimate of τ is 0.236, and statistically significant at the 10 percent level, as shown in column (1) of Table 2-10 Panel A. (In Appendix Figure B.1 we show an analogous figure for log(Initial New Assets); for initial wealth, we observe no discontinuity at the victory threshold.) The estimate employing residual data generates a similar though slightly smaller discontinuity, 0.207. Next, in columns (2) and (3) we partition the sample into *BIMARU* and *Non-BIMARU* constituencies (the corresponding graphs are shown in Figure 2-2, Panels B and C). We observe a winner's premium of 0.493 in *BIMARU* constituencies, significant at the 1 percent level (the residual data used to generate the figures produce a coefficient of 0.624). Our estimates for *Non-BIMARU* constituencies indicate no difference in returns for winners versus runners-up. Overall, these results are in line with those obtained from standard regression analyses.

Column (4) includes only ministers with corresponding runners-up. The point estimate of the discontinuity is 0.773, significant at the 1 percent level, a result qualitatively similar to that obtained through the regression analysis in the previous section. The premium is somewhat smaller in magnitude, 0.627, when estimated using the residual data, as indicated in Figure 2-2, Panel D. On the other hand, the subsample of non-minister winners and their corresponding runners-up does not indicate a statistically distinguishable jump - the estimate of the discontinuity is 0.168 with a standard error of 0.155 (see also Figure 2-2, Panel E). In columns (6) and (7), we disaggregate the sample based on whether an incumbent is standing for reelection in the constituency (see also Figure 2-2, Panels F and G). The coefficient estimate of the discontinuity for the incumbent subsample is 0.310, significant at the 10 percent level (0.286 and significant at the 5 percent level for the residual data). By contrast, for the sample of non-incumbent constituencies, we observe no jump at the threshold (the point estimate is -0.168 with a standard error of 0.259).[39]

of Table 2-10 use raw (i.e., uncollapsed) residuals. As can be seen, the estimates of discontinuities using this two-step approach are quantitatively and qualitatively very similar to those of the local linear regressions that we employ as the benchmark specification.

[39] As an alternative approach to generating this table, we employed the procedure of Imbens and Kalyanaraman (2012) to calculate optimal bandwidths for each of our main analyses. These bandwidths average 4.94%, ranging from 4.23% to 6.36% depending on the subsample under consideration. The results using this approach, shown in Appendix Table B.4, are broadly consistent with the findings we report in the tables below. One notable exception is that our basic result on the winner's premium is not statistically significant, though its point estimate is similar to that obtained with a 5% bandwidth. Despite this, the estimates using this approach *do* show a sharp difference in the winner's premium for *BIMARU* versus *non-BIMARU*, with the *BIMARU* winner's premium significant at the 1% level.

Finally, in Figure 2-3 we plot kernel densities of log(*Initial Net Assets*) and *Age* for the sample of constituency-matched candidates that were within a *Margin* of 5 percentage points ("close elections"). Panel A plots log(*Initial Net Assets*) densities for winners and runners-up and Panel B plots densities for *Age*. For both initial wealth and age, the Kolmogorov-Smirnov test for equality of the distribution function of winners and runners-up cannot be rejected at the 5 percent level (p-values of 0.979 and 0.099, respectively), providing some validation of our regression discontinuity design.[40]

Based on these discontinuities, we can perform a simple back-of-the-envelope calculation to approximate the winner's premium in monetary terms. We do this by first calculating how winners' average wealth would have grown had they not won the election using the net asset growth rate of all constituency-matched runners-up, and then comparing this average to the level of wealth accumulation using the discontinuity estimates from the RD design. Overall, for *Winners* as a group, the estimated annual premium is approximately Rs 1,000,000 (USD 20,000). However, for *Ministers* the winner premium is significantly larger, about Rs 3,700,000 per year (USD 74,000). These estimated premiums are much larger than the average salary of a state-level legislator, which is under 100,000 rupees, with very little variation as a function of seniority. They are also substantial as a fraction of candidates' initial assets which are, on average, only about Rs 10,000,000 (USD 200,000)—implying a large impact in percentage terms.

2.5.4 Evidence from Seasoned Candidates

We analyze a restricted sample of constituencies where both winner and runner-up are seasoned politicians, in the sense of both competing in at least two elections *prior* to the elections we consider in our analysis, and where both were either winner or runner-up in these earlier elections. Repeated contests of this sort between seasoned politicians is surprisingly common

[40] As a final note, our regression discontinuity approach assumes that winners in close elections are broadly representative of the politicians that obtain public office, and further that a close election is not a treatment in itself. For example, an MLA who wins by a narrow margin may be more focused on private sector activities during his term in the expectation that he will soon be out of office. We cannot rule out such concerns. However, there are several patterns in the data that limit the extent to which this is likely to explain our results. First, election margin at t=0 does not predict whether a winner chooses to stand for reelection; this is true for the full sample and also the subset of reasonably close ($Margin \leq 10$) elections. For winners from t=0 that do choose to stand for reelection, margin at t=0 *does* strongly predict victory at t=1; however, when we limit the sample to those candidates that won by a margin of 10 percentage points or less, this relationship disappears. The limited relationship between election margins and future electoral prospects suggest that the treatment effect of close elections themselves is likely to be relatively modest.

in our data—our restricted sample contains 100 candidates from 50 constituencies. We provide one illustrative example below for the Biswanath Assembly Constituency in the state of Assam. In this case, both candidates, Prabin Hazarika and Nurjamal Sarkar, have contested all elections since 1991 and have been either a winner or a runner-up in each instance. We argue that such career politicians are less likely to exit because of party decisions or a reevaluation of prospects for future electoral success since, by construction, we include only politicians who have performed well as candidates in the recent past. This subset of active seasoned politicians arguably represent more comparable treatment and control candidates than the full sample of re-contesting politicians.[41]

Biswanath Assembly Constituency (Assam)

Year	Winner	%age	Party	Runner-up	%age	Party
2011	Prabin Hazarika	45.51	AGP	Nurjamal Sarkar	44.09	INC
2006	Nurjamal Sarkar	41.76	INC	Prabin Hazarika	39.46	AGP
2001	Nurjamal Sarkar	48.55	INC	Prabin Hazarika	44.3	AGP
1996	Prabin Hazarika	42.62	AGP	Nurjamal Sarkar	31.76	INC
1991	Nurjamal Sarkar	46.49	INC	Prabin Hazarika	17.39	AGP

We focus our analysis on this set of active seasoned candidates in Figure 2-4, employing the same regression discontinuity design as in Section 4.3 above. Figure 2-4 shows residuals from the regression of log(*Final Net Assets*) on candidate observables, excluding *Winner* and *Margin*, as well as the RDD estimated polynomial (see Section 4.3 for details), and indicates a clear discontinuity around the winning threshold. The point estimate of the discontinuity is 0.52—somewhat larger than the RDD-estimated winner's premium using the full sample—and significant at the 10 percent level.

2.5.5 Evidence from Bihar's Hung Assembly

We conclude this section by presenting some results from a quasi-experiment. In Bihar's legislative assembly election in February 2005, no individual party gained a majority of seats, and attempts at forming a coalition reached an impasse. As a result of this hung assembly, new elections were held in October/November of the same year.[42] In a significant fraction of

[41] At the same time, it is important to note that these politician-pairs are those who may have relatively limited outside options (hence their repeated election bids). So while we argue that our seasoned politician comparison represents a legitimate causal estimate, it is one that may have limited external validity.

[42] Bihar was under the direct rule of India's federal government during this period.

these contests, repeated within less than a year of one another, the initial winner was defeated in the follow-up election. For these constituencies, we come as close as possible to observing the counterfactual of winners reassigned to runner-up, and vice-versa.

From the 243 constituencies contested in the February election, we sample those where both the winner and runner-up competed again in the October election of the same year and emerged as winner/runner-up or runner-up/winner in this later election. This leaves a sample of 260 candidates (130 constituencies) for which we analyze the probabilities of winning the October election as a function of the winning margin in the February election. Overall, winners in the February 2005 election won in the later contest only 66.2 percent of the time. Further, as one narrows the February 2005 margin, this advantage decreases monotonically, as shown in Table 2-11, Panel A. At the 5 percent threshold, the probability that the initial winner also won the second election is only 52.2 percent, and is statistically indistinguishable from 50 percent. This suggests a significant element of randomness for close elections in this sample of 46 constituencies.[43]

We compare the net asset growth of a *treatment* group and a *control* group. The *treatment* group consists of candidates that were runners-up in the February 2005 election but won in the October 2005 contest, while the *control* group is comprised of candidates that were winners in February 2005 but runners-up in the October election. These cases where winners and losers were switched owing to the hung assembly provide a measure of the returns to public office with a straightforward causal interpretation. We look at all such candidates whose winner status shifted between these two 2005 elections, and *also* chose to run again in 2010, so we can calculate their asset growth rates. The resulting set of candidates is relatively small - 25 winners and 26 runners-up - which limits our statistical power.

We present this comparison in Table 2-11, Panel B, where we observe that the annual net asset growth of the *treatment* group is on average 12.76 percent higher than that of the control group (28.88 versus 16.12 percent), a difference that is significant at the 5 percent level. In Column (2) we limit the sample to the constituency matched samples where winner

[43]Recent papers by Snyder (2005), Caughey and Sekhon (2010), Carpenter et al. (2011), and Folke et al. (2011) critically assess regression discontinuity studies that rely on close elections. There remains an active debate on whether close elections can really be considered a matter of random assignment. If sorting around the winning threshold is not random, but close winners have systematic advantages, then the RD design may fail to provide valid estimates of the returns to office. The Bihar example provides at least suggestive evidence that close elections are relatively random in the context we consider in this paper.

and runner-up status switched and *both* candidates ran in the 2010 election. This reduces the sample to 11 constituencies - 22 candidates - and we find a difference in the net asset growth between winners and runners-up of approximately 6 percent, roughly similar to the magnitudes we observe with the full sample. Given the small sample size, the difference in asset growth for the sample of 22 candidates is not statistically significant.

2.6 Discussion of Results

The results documented above show a significant return to public office, which increases as legislators progress through the the political hierarchy. Our focus on constituency-matched candidates where the election was decided by a narrow margin ensures that these returns are benchmarked to similar "quality" individuals; yet the issue naturally arises of whether these results generalize to the broader set of state assembly candidates. We assess this question, and consider possible alternative explanations for our results, in the discussion that follows. We conclude this section with a brief discussion of the implications of electoral accountability for our winner's premium estimates.

2.6.1 Selection Concerns

As we observed in our descriptive statistics, there exist several modest differences in predetermined characteristics between winners and runners-up. To examine the possible bias that could result from these different attributes of winners versus runners-up, we construct a predicted value of log(*Final Net Assets*) based on the observable characteristics log(*Initial Net Assets*), log(*Years of Education*), *Criminal Record*, *Age* and *Age*^{2}, *Female*, and *Incumbent*. We then examine whether *Winner* status is correlated with this predicted value. We find that *Winner* is negatively related to predicted log(*Final Net Assets*), though with a large standard error. This is shown in Table 2-12. This suggests that, if anything, selection based on these observables creates a downward bias in our estimates.

To assess empirically whether winners and runners-up select differently into the decision to rerun for office, we include in Appendix Table B.5 a probit analysis of whether predetermined attributes affect the probability of standing for reelection and, more importantly, whether they differentially affect the probability of running again for winners versus runners-up. While many attributes affect the probability of standing for rerunning, there is little evidence of any differential selection of winners versus runners-up: For the full sample, only *Age*Winner* is significant (at the 10 percent level) in predicting the decision to rerun.

Finally, we note that a number of observed patterns in candidate attributes further reinforce the view that our estimates of β are, if anything, biased toward zero. First, consistent with the model, we observe a significantly higher exit rate among candidates, particularly runners-up, with low initial wealth. While these candidates were able to finance an initial campaign, they are most affected by negative shocks to wealth. Second, the data do not support the view that runners-up who choose *not* to run again for office have higher outside earnings options than those runners-up who stand for reelection (and hence remain in the sample). Indeed we find the opposite to be true: Taking years of education as a proxy for outside earnings opportunities, we find that runners-up who opt to run for election again have 13.76 years of education on average, compared with 13.09 for those who do not stand for election the following time. This runs counter to the spare model outlined above, but also suggests an additional selection on runners-up that may bias our results *towards* zero, assuming that education is positively correlated with private labor market outcomes. (While beyond the focus of this paper, the high education of candidates who choose to run despite an initial loss could plausibly result from more educated candidates placing a higher value on the non-pecuniary benefits of holding public office. If the ego benefits of public office are correlated with human capital - as suggested by, for example, Besley (2004) - then high education runners-up (who value the office for its own sake) will be more likely to run for office than low education runners-up, all else equal.)

2.6.2 Alternate explanations for the *Winner's premium*

Our estimates of asset growth are based on *disclosed* wealth. If standing politicians face higher disclosure standards, this could plausibly generate a pure reporting-based winner's premium in observed asset growth. We note, however, that the most straightforward versions of this hypothesis would generate the opposite pattern for incumbents versus non-incumbents than what we observe: Non-incumbents at t=0 would disclose few assets, and conditional on winning would provide fuller disclosure at t=1. Incumbents, by contrast, would provide relatively full disclosure for both elections conditional on winning, and hence observed asset *growth* of incumbents would be lower. Further, to the extent that standing politicians are better monitored in low-corruption states, the disclosure bias would predict a *higher* winner's premium in low corruption states, again the opposite of the patterns observed in the data.

These arguments are not dispositive - more complicated models of disclosure bias can generate at least some of our findings - but the most straightforward cases of asset underreporting are biased against our findings on the cross-sectional correlates of the winner's premium.

Other alternate explanations for the winner's premium may relate to the differential consumption of winners and runners-up. For example, if winners substitute government perquisites for consumption while in office or shy away from conspicuous consumption that might offend voters, differential spending patterns between the two groups of candidates may generate higher asset growth among winners. We investigate this concern using data on durable goods consumption, such as motor vehicles and jewelry, and find that it is in fact higher for winners than for runners-up, particularly among those appointed to the Council of Ministers, which is at odds with the differential consumption hypothesis. Further, to the extent that conspicuous consumption would elicit greater voter backlash in low corruption states, the differential consumption hypothesis would predict a greater winner's premium in low corruption states, the opposite of the pattern we observe in the data.

Finally, we consider the possibility that election costs may be lower for incumbent politicians owing, for example, to their greater visibility and support of their parties.[44] As noted in our background discussion, there is no evidence of this based on formal campaign finance disclosures. It is still possible that politicians may underreport their true campaign expenditures, or that differences in required campaign effort may lead to differences in winner and runner-up earnings. To examine this possibility, we look again at Bihar's hung parliament in 2005. As we describe above, Bihar held two elections in 2005. The first, in February, did not lead to the formation of a government, and a second election was held in October/November. This generates an interesting scenario for assessing the role of campaign finance in generating the winner's premium. First, since no government was formed, opportunities for rent extraction by winners was likely limited. Second, since this was a time of relatively intense

[44]Note that we include these campaign costs in our asset growth calculations for both winners and runners-up. This is partly borne of necessity, as we only observe candidates that re-run. However, we argue that if our interest is in estimating the benefits that politicians extract while in office, this is an appropriate control, as standing politicians must themselves incur the time and financial cost of running. Hence, if we wish to estimate the increment to assets that result from holding office we should subtract these campaign expenses from both winners and their comparison group.

electioneering, if the higher rate of asset accumulation of winners were the result of differential campaigning costs, we would expect any difference in asset growth due to campaign finance or effort to be particularly large during this period. We thus look at the asset disclosures of winner and runner-up candidates from the February election, who chose to stand for election again in October/November (disclosures were required immediately prior to *both* elections).

65 out of Bihar's 243 constituencies were decided within a margin of 5 percentage points during the February 2005 elections. After omitting candidates with missing affidavits and poor scans, we were able to analyze the asset growth of 82 candidates in these close constituencies, examining their asset accumulation between the two election dates. In Appendix Table B.6, we show that there is no winner's premium during this period, as might have been expected if campaign costs were a primary contributor to the higher asset growth of public officeholders.

2.6.3 Electoral accountability and the *Winner's premium*

The extent to which legislators extract financial returns from their positions may be limited by pressure from the electorate. We emphasize that the asset growth calculations we perform in this paper are based on data easily accessible via the internet, and their availability has been widely reported in the Indian media. This is of particular concern if politicians rationally anticipate that high rent extraction will drastically reduce their reelection prospects and thus self-select out of re-running. Then we will be selecting out the politicians with the highest asset growth, thus biasing our results downward. To evaluate the plausibility of this theory, in Appendix Table B.7 we examine whether there is any effect of high asset growth on election outcomes. While neither *Winner* nor Net_Asset_Growth significantly predict election outcomes, the results point, if anything, in the opposite direction - the coefficient on Net_Asset_Growth is positive in Column (1), and its interaction with *Winner*, capturing the effect of asset growth among election winners, is positive (Column (2)). We also note that the negative coefficient on *Winner* is consistent with a negative incumbency effect in India that was already observed in Table 2-3.[45]

[45] In results not reported, we also find that legislators who win by large margins do not earn a higher winner's premium. Such a specification is, however, subject to extreme problems of unobserved heterogeneity - the large margin may be because of a candidate's effort or political skill, confusing the interpretation of the *Winner*Margin* interaction.

2.7 Conclusion

In this paper, we utilize the asset disclosures of candidates for Indian state legislatures, taken at two points across a five year election cycle, and accessed through the country's Right to Information Act. We use these data to compare the asset growth of election winners versus runners-up to estimate the financial returns from holding public office relative to private sector opportunities available to political candidates.

Our main findings suggest that the annual growth rate of winners' assets is 3-5 percent higher than that of runners-up. This effect is more pronounced among legislators in more corrupt regions of India, implying that the higher returns are likely associated with political rent extraction. We also find that the winner's premium is much higher for senior politicians - ministers and incumbents. This pattern is best explained by a model of rent-seeking where the financial benefits of office increase with experience and progression through the political hierarchy.

These findings have a number of implications for theories of politicians' behavior and the political process. First, to the extent that the winner's premium is driven by private agents "purchasing" influence, our results suggest that the influence of senior legislators is much more valuable than that of rank-and-file state assembly members. They may also imply that, from a financial perspective, the potential for long-term rewards from more senior positions may be more of a motivating factor to run for election than the short-run returns of a rank-and-file position. This is broadly consistent with a tournament model of politics in the spirit of Lazear and Rosen (1981), where participants compete for the high returns that only a small fraction of entry-level politicians will attain.

A few comments and caveats are in order in interpreting our findings. First, our results necessarily account only for publicly disclosed assets, and hence may serve as a lower bound on any effect (though we note that non-politicians may also engage in hiding assets for tax purposes). This makes it all the more surprising that the data reveal such high returns for state ministers and those holding office in high-corruption regions. Additionally, we measure the returns to holding public office only while a politician is in power. To the extent that politicians profit from activities like lobbying and consulting after leaving office, our estimates represent a lower bound on the full value of holding public office (Diermeier et al., 2005). Further, even if we assume transparent financial disclosure, the relatively modest returns from winning public office for lower-level or first-time politicians do not imply the near-absence of corruption. Given the low salaries of legislators, they may be required to extract extra-legal payments merely to keep up with their private sector counterparts. Finally, our research design does not allow us to distinguish between explanations of the winner's premium based on adverse selection (i.e., the selection of more corrupt politicians in high corruption regions) versus moral hazard (weaker constraints on rent-seeking in high corruption regions).

Our work also presents several possible directions for future work. Given the high returns we observe among ministers, it may be fruitful, with the benefit of additional data, to examine whether particular positions within the Council of Ministers are associated with high rents. One may also assess whether electoral accountability is affected by voter exposure to asset data, in the spirit of Banerjee et al. (2011). It may be interesting to explore the impact of the Right to Information Act itself: disclosure requirements may induce exit by winners that have extracted high rents, in order to avoid possible corruption-related inquiries. Finally, we are unable in this work to uncover the mechanism through which asset accumulation takes place. We leave these and other extensions for future work, which might be enabled either by experimental intervention or the accumulation of new data via the Right to Information Act.

Bibliography

Angrist, Joshua and Jorn-Steffen Pischke, 2009, *Mostly Harmless Econometrics*, Princeton University Press, Princeton, NJ.

Banerjee, Abhijit V., Selvan Kumar, Rohini Pande and Felix Su, 2011, *Do Informed Voters Make Better Choices? Experimental Evidence from Urban India*, Working Paper.

Barro, Robert, 1973, *The Control of Politicians: An Economic Model*, Public Choice 14:19-42.

Besley, Timothy, 2004, *Paying Politicians: Theory and Evidence*, Journal of the European Economic Association, 2, 193-215.

Bhavnani, Rikhil R., 2012, *Using Asset Disclosures to Study Politicians' Rents: An Application to India*, Working Paper.

Braguinsky, Serguey , Sergey Mityakov, and Andrey Liscovich, 2010, *Direct Estimation of Hidden Earnings: Evidence From Administrative Data*, Working Paper.

Buchanan, James, 1989, *The Public Choice Perspective*, in Essays on the Political Economy, Honolulu: University of Hawaii Press.

Cameron, A. Colin, Jonah B. Gelbach and Douglas L. Miller, 2008, *Bootstrap-Based Improvements for Inference with Clustered Errors*, The Review of Economics and Statistics 90(3), 414-427.

Carpenter, David, Brian Feinstein, Justin Grimmer and Eitan Hersh, 2011, *Are Close Elections Random?*, Working Paper.

Caselli, Francesco and Massimo Morelli, 2004, *Bad Politicians*, Journal of Public Economics 88, 759-782.

Caughey, Devin M. and Jasjeet S. Sekhon, 2010, *Regression-Discontinuity Designs and Popular Elections: Implications of Pro-Incumbent Bias in Close U.S. House Races*, Working Paper.

Diermeier, Daniel, Michael Keane and Antonio Merlo, 2005, *A Political Economy Model of Congressional Careers*, American Economic Review 95(1): 347-373.

DiNardo, John and David S. Lee, 2004, *Economic Impacts of New Unionization on Private Sector Employees: 1984-2001*, Quarterly Journal of Economics 119, 1383-1441.

Djankov, Simon, Rafael La Porta, Florencio Lopez-de-Silanes, and Andrei Shleifer, 2010, *Disclosure by Politicians*, American Economic Journal: Applied Economics 2, 179-209.

Eggers, Andrew and Jens Hainmueller, 2009, *MPs for Sale? Estimating Returns to Office in Post-War British Politics*, American Political Science Review, 103(4): 513-533.

Eggers, Andrew and Jens Hainmueller, 2011, *Political Investing: The Common Stock Investments of Members of Congress 2004-2008*, Working Paper.

Ferejohn, John, 1986, *Incumbent Performance and Electoral Control*, Public Choice 50: 5-25.

Ferraz, Claudio and Frederico Finan, 2011, *Electoral Accountability and Corruption: Evidence from the Audits of Local Governments*, American Economic Review 101, 1274-1311.

Fisman, Raymond J., Nikolaj Harmon, Emir Kamenica and Inger Munk, 2013, *Labor Supply of Politicians*, Working Paper.

Folke, Olle, Shigeo Hirano and James M. Snyder, Jr, 2011, *A Simple Explanation for Bias at the 50-50 Threshold in RDD Studies Based on Close Elections*, Working Paper.

Gagliarducci, Stefano and Tommaso Nannicini, forthcoming, *Do Better Paid Politicians Perform Better? Disentangling Incentives from Selection*, Journal of the European Economic Association.

Imbens, Guido and Karthik Kalyanaraman, 2012, *Optimal Bandwidth Choice for the Regression Discontinuity Estimator*, Review of Economic Studies 79(3) 933-959.

Imbens, Guido and Thomas Lemieux, 2008, *Regression Discontinuity Designs: A Guide to Practice*, Journal of Econometrics 142(2) 615-635.

Kotakorpi, Kaisa and Panu Poutvaara, 2011, *Pay for Politicians and Candidate Selection: An Empirical Analysis*, Journal of Public Economics 95 (7-8): 877-885.

Lazear, Edward P. and Sherwin Rosen, 1981, *Rank-Order Tournaments as Optimum Labor Contracts*, Journal of Political Economy 89(5): 841-864.

Linden, Leigh, 2004, *Are Incumbents Always Advantaged? The Preference for Non-Incumbents in India*, Working Paper.

Lenz, Gabriel, and Kevin Lim, 2009, *Getting Rich(er) in Office? Corruption and Wealth Accumulation in Congress*, Working Paper.

Matozzi, Andrea and Antonio Merlo, 2008, *Political Careers or Career Politicians?*, Journal of Public Economics 92(3-4), pages 597-608.

Olken, Benjamin and Rohini Pande, 2012, *Corruption in Developing Countries*, Working Paper.

Querubin, Pablo and James Snyder, *Returns to U.S. Congressional Seats in the Mid-19th Century*, in The Political Economy of Democracy, Enriqueta Aragones, Carmen Bevia, Humberto Llavador, and Norman Schofield (eds.), 2009.

Querubin, Pablo and James Snyder, 2011, *The control of politicians in normal times and times of crisis: Wealth accumulation by U.S. congressmen, 1850-1880*, Working Paper.

Snyder, Jason, 2005, *Detecting Manipulation in U.S. House Elections*, Working Paper.

Sukhtandar, Sandip, 2013, *Much Ado about Nothing? Corruption in the Allocation of Wireless Spectrum in India*, unpublished manuscript.

Ziobrowski, Alan J., James W. Boyd, Ping Cheng and Brigitte J.Ziobrowski, 2011, *Abnormal Returns From the Common Stock Investments of Members of the U.S. House of Representatives*, Business and Politics: Vol. 13: Iss. 1, Article 4.

2.8 Appendix: Proof of Proposition 1

Proposition 1 *If wealth shocks are i.i.d. across candidates and independent of D_i and $\boldsymbol{x}_i$, and $R_{\mathcal{W}} > R_{\mathcal{L}}$, then $\hat{\beta} < (R_{\mathcal{W}} - R_{\mathcal{L}})$. That is, our estimate of the private returns to public office is biased downwards.*

Proof: We begin by restating the expression for $\hat{\beta}$:

$$\begin{aligned}
\hat{\beta} &= \mathbb{E}[logW_{ic}^1|\mathbf{x}_i, D_i = 1, z_i = 1] - \mathbb{E}[logW_{ic}^1|\mathbf{x}_i, D_i = 0, z_i = 1] \\
&= R_{\mathcal{W}} - R_{\mathcal{L}} \\
&+ \Bigg[\mathbb{E}[logW_{ic}^1|\mathbf{x}_i, D_i = 1, z_i = 1] - \mathbb{E}[logW_{ic}^1|\mathbf{x}_i, D_i = 1, z_i = 0]\Bigg] \cdot P(z_i = 0|\mathbf{x}_i, D_i = 1) \\
&- \Bigg[\mathbb{E}[logW_{ic}^1|\mathbf{x}_i, D_i = 0, z_i = 1] - \mathbb{E}[logW_{ic}^1|\mathbf{x}_i, D_i = 0, z_i = 0]\Bigg] \cdot P(z_i = 0|\mathbf{x}_i, D_i = 0)
\end{aligned}$$

Using $logW_{ic}^1 = logW_{ic}^0 + (R_{\mathcal{W}} - R_{\mathcal{L}}) \cdot D_i + b'\mathbf{x}_i + \alpha_c + \epsilon_i^1$ and canceling out non-stochastic components this can be written as:

$$\begin{aligned}
\hat{\beta} &= R_{\mathcal{W}} - R_{\mathcal{L}} \\
&+ \Bigg[\mathbb{E}[\epsilon_i^1|\mathbf{x}_i, D_i = 1, z_i = 1] - \mathbb{E}[\epsilon_i^1|\mathbf{x}_i, D_i = 1, z_i = 0]\Bigg] \cdot P(z_i = 0|\mathbf{x}_i, D_i = 1) \\
&- \Bigg[\mathbb{E}[\epsilon_i^1|\mathbf{x}_i, D_i = 0, z_i = 1] - \mathbb{E}[\epsilon_i^1|\mathbf{x}_i, D_i = 0, z_i = 0]\Bigg] \cdot P(z_i = 0|\mathbf{x}_i, D_i = 0)
\end{aligned} \tag{2.8}$$

Next, note that:

$$\begin{aligned}
\mathbb{E}[\epsilon_i^1|\mathbf{x}_i, D_i] &= \mathbb{E}[\epsilon_i^1|\mathbf{x}_i, D_i, \epsilon_i^1 \leq c] \cdot P(\epsilon_i^1 \leq c|\mathbf{x}_i, D_i) + \mathbb{E}[\epsilon_i^1|\mathbf{x}_i, D_i, \epsilon_i^1 > c] \cdot P(\epsilon_i^1 > c|\mathbf{x}_i, D_i) \\
&= \Bigg[\mathbb{E}[\epsilon_i^1|\mathbf{x}_i, D_i, \epsilon_i^1 \leq c] - \mathbb{E}[\epsilon_i^1|\mathbf{x}_i, D_i, \epsilon_i^1 > c]\Bigg] \cdot P(\epsilon_i^1 \leq c|\mathbf{x}_i, D_i) + \mathbb{E}[\epsilon_i^1|\mathbf{x}_i, D_i, \epsilon_i^1 > c]
\end{aligned}$$

where c is some constant. Rearranging, this gives:

$$\mathbb{E}[\epsilon_i^1|\mathbf{x}_i, D_i, \epsilon_i^1 > c] - \mathbb{E}[\epsilon_i^1|\mathbf{x}_i, D_i] = \Bigg[\mathbb{E}[\epsilon_i^1|\mathbf{x}_i, D_i, \epsilon_i^1 > c] - \mathbb{E}[\epsilon_i^1|\mathbf{x}_i, D_i, \epsilon_i^1 \leq c]\Bigg] \cdot P(\epsilon_i^1 \leq c|\mathbf{x}_i, D_i) \tag{2.9}$$

Lemma 1 $\mathbb{E}[\epsilon_i^1|\epsilon_i^1 > c] - \mathbb{E}[\epsilon_i^1]$ *is increasing in c.*

The proof of Lemma 1 is straightforward. $\mathbb{E}[\epsilon_i^1]$ is constant and the conditional expectation, $\mathbb{E}[\epsilon_i^1|\epsilon_i^1 > c]$, increases in the value of the lower bound, c.

Let $\bar{\epsilon}_{ij} = logM - logW^0_{ic} - R_j - b'\mathbf{x}_i - \alpha_c$ be the cutoff value for re-contesting. For shocks $\epsilon^1_{ij} < \bar{\epsilon}_{ij}$, a candidate i drops out of the sample since he is unable to afford the cost of running an election campaign (i.e., $z_i = 0$). Since $R_{\mathcal{W}} > R_{\mathcal{L}}$,[46] we have $\bar{\epsilon}_{i\mathcal{W}} < \bar{\epsilon}_{i\mathcal{L}}$. Under the assumption of i.i.d. wealth shocks that are independent of $\mathbf{x}_i$ and D_i, we get for winners ($D_i = 1$) and losers ($D_i = 0$), respectively;

$$\begin{aligned}\mathbb{E}[\epsilon^1_i|\mathbf{x}_i, D_i = 1, z_i = 1] - \mathbb{E}[\epsilon^1_i|\mathbf{x}_i, D_i = 1] &= \mathbb{E}[\epsilon^1_i|\mathbf{x}_i, D_i = 1, \epsilon^1_i \geq \bar{\epsilon}_{i\mathcal{W}}] - \mathbb{E}[\epsilon^1_i|\mathbf{x}_i, D_i = 1] \\ &= \mathbb{E}[\epsilon^1_i|\epsilon^1_i \geq \bar{\epsilon}_{i\mathcal{W}}] - \mathbb{E}[\epsilon^1_i]\end{aligned} \tag{2.10}$$

$$\begin{aligned}\mathbb{E}[\epsilon^1_i|\mathbf{x}_i, D_i = 0, z_i = 1] - \mathbb{E}[\epsilon^1_i|\mathbf{x}_i, D_i = 0] &= \mathbb{E}[\epsilon^1_i|\mathbf{x}_i, D_i = 0, \epsilon^1_i \geq \bar{\epsilon}_{i\mathcal{L}}] - \mathbb{E}[\epsilon^1_i|\mathbf{x}_i, D_i = 0] \\ &= \mathbb{E}[\epsilon^1_i|\epsilon^1_i \geq \bar{\epsilon}_{i\mathcal{L}}] - \mathbb{E}[\epsilon^1_i]\end{aligned} \tag{2.11}$$

Using (2.9) and substituting (2.10) and (2.11) into (2.8) yields the following expression for $\hat{\beta}$:

$$\begin{aligned}\hat{\beta} &= R_{\mathcal{W}} - R_{\mathcal{L}} \\ &+ \Big[\mathbb{E}[\epsilon^1_i|\mathbf{x}_i, D_i = 1, z_i = 1] - \mathbb{E}[\epsilon^1_i|\mathbf{x}_i, D_i = 1, z_i = 0]\Big] \cdot P(z_i = 0|\mathbf{x}_i, D_i = 1) \\ &- \Big[\mathbb{E}[\epsilon^1_i|\mathbf{x}_i, D_i = 0, z_i = 1] - \mathbb{E}[\epsilon^1_i|\mathbf{x}_i, D_i = 0, z_i = 0]\Big] \cdot P(z_i = 0|\mathbf{x}_i, D_i = 0) \\ &= R_{\mathcal{W}} - R_{\mathcal{L}} \\ &+ \Big[\mathbb{E}[\epsilon^1_i|\epsilon^1_i \geq \bar{\epsilon}_{i\mathcal{W}}] - \mathbb{E}[\epsilon^1_i]\Big] - \Big[\mathbb{E}[\epsilon^1_i|\epsilon^1_i \geq \bar{\epsilon}_{i\mathcal{L}}] - \mathbb{E}[\epsilon_i]\Big]\end{aligned} \tag{2.12}$$

By Lemma 1 and $\bar{\epsilon}_{i\mathcal{W}} < \bar{\epsilon}_{i\mathcal{L}}$, we have $(\mathbb{E}[\epsilon^1_i|\epsilon^1_i \geq \bar{\epsilon}_{i\mathcal{W}}] - \mathbb{E}[\epsilon^1_i]) \leq (\mathbb{E}[\epsilon^1_i|\epsilon^1_i \geq \bar{\epsilon}_{i\mathcal{L}}] - \mathbb{E}[\epsilon^1_i])$ and thus $\hat{\beta} \leq R_{\mathcal{W}} - R_{\mathcal{L}}$. Further, under the assumption of normally distributed wealth shocks, the inequality is strict, $\hat{\beta} < R_{\mathcal{W}} - R_{\mathcal{L}}$, implying that our estimate of the private returns to office is biased downwards.

[46] Note that the discontinuity in re-contesting that is observed in the data requires $R_{\mathcal{W}} > R_{\mathcal{L}}$.

Table 2-1: **Overview of Sample States**

Notes: This Table provides an overview of the states in our sample along with some state characteristics at the time of the first elections. The columns labeled *Winners* and *Runners-up* show the number of candidates which we were able to manually match across elections and in parentheses we show the number of matches that were potentially usable (i.e., good quality affidavits and minimum candidate net wealth of Rs 100,000). *Pairs* refers to the number of constituencies in which Winners and Runners-up both recontested. Sources: Statistical Reports on General Elections, Election Commission of India, New Delhi (various years); India Corruption Study 2005, Transparency International India (June 30, 2005).

								Matched Candidates		
State	**Year 1**	**Year 2**	**Corruption Index**	**Electorate**	**Turnout**	**Constit-uencies**	**Total Contestants**	**Winners**	**Runners-up**	**Pairs**
								(Parentheses: usable affidavits)		
BIMARU										
Bihar*	2005	2010	695	51,385,891	45.85%	243	2,135	180 (131)	114 (72)	84 (34)
Madhya Pradesh	2003	2008	584	37,936,518	67.25%	230	2,171	127 (104)	51 (38)	30 (17)
Rajasthan	2003	2008	543	33,928,675	67.18%	200	1,541	105 (72)	72 (52)	41 (18)
Uttar Pradesh	2007	2012	491	113,549,350	45.96%	403	6,086	300 (267)	221 (179)	172 (124)
Non-BIMARU										
Andhra Pradesh	2004	2009	421	51,146,498	69.96%	294	1,896	152 (112)	94 (76)	57 (35)
Arunachal Pradesh	2004	2009	-	683,512	64.02%	60	168	55 (38)	22 (11)	19 (7)
Assam	2006	2011	542	17,434,019	75.77%	126	997	109 (95)	69 (48)	62 (37)
Chhattisgarh	2003	2008	445	13,543,656	71.30%	90	819	56 (23)	31 (14)	15 (2)
Delhi	2003	2008	496	8,448,324	53.42%	70	817	46 (27)	8 (3)	7 (2)
Goa	2007	2012	-	1,010,246	70.51%	40	202	36 (34)	19 (18)	18 (17)
Haryana	2005	2009	516	12,735,888	71.96%	90	983	59 (48)	44 (38)	29 (15)
Jharkhand	2005	2009	520	17,766,202	57.03%	81	1,390	63 (41)	51 (33)	43 (19)
Karnataka	2004	2008	576	38,586,754	65.17%	224	1,715	85 (49)	35 (22)	3 (2)
Kerala	2006	2011	240	21,483,937	72.38%	140	931	105 (62)	31 (23)	25 (15)
Maharashtra	2004	2009	433	65,965,792	63.44%	288	2,678	214 (183)	112 (96)	85 (61)
Manipur	2007	2012	-	1,707,204	86.73%	60	308	47 (33)	33 (24)	28 (14)
Mizoram	2003	2008	-	532,028	78.65%	40	192	31 (13)	17 (9)	15 (5)
Orissa	2004	2009	475	25,651,989	66.05%	147	802	108 (81)	78 (56)	60 (37)
Puducherry	2006	2011	-	659,420	86.00%	30	218	25 (22)	17 (12)	14 (9)
Punjab	2007	2012	459	16,775,702	75.45%	116	1,043	89 (75)	61 (48)	46 (29)
Sikkim	2004	2009	-	281,937	79.23%	32	91	12 (11)	14 (11)	2 (2)
Tamil Nadu	2006	2011	509	46,603,352	70.82%	234	2,586	127 (97)	43 (32)	23 (13)
Uttarakhand	2007	2012	-	5,985,302	59.45%	69	785	57 (47)	30 (27)	23 (17)
West Bengal	2006	2011	461	48,165,201	81.97%	294	1,654	159 (126)	101 (77)	60 (39)
TOTALS				**631,967,397**		**3,601**	**32,208**	**2,347 (1,791)**	**1368 (1,019)**	**961 (570)**
Lok Sabha	2004	2009		671,487,930	58.07%	543	5,435			

*October 2005 re-election

Table 2-2: **Variable Definitions**

Variable	Description
Movable Assets (1)	Sum of (i) Cash, (ii) Deposits in Banks, Financial Institutions and Non-Banking Financial Companies, (iii) Bonds, Debentures and Shares in companies, (iv) NSS, Postal Savings etc., (v) Personal loans/advance given, (vi) Motor vehicles, (vii) Jewelry, and (viii) Other assets such as values of claims/interests as reported on the candidate affidavit. This item excludes the value of life or other insurance policies (which are usually reported at payoff values).
Immovable Assets (2)	Sum of (i) Agricultural Land, (ii) Non-Agricultural Land, (iii) Commercial Buildings and (vi) Residential Buildings ("Buildings and Houses"), and (v) Others as reported on the candidate affidavit.
Total Assets	Defined as the sum of (1) and (2).
Total Liabilities (3)	Sum of (i) Loans from Banks and Financial Institutions, (ii) Loans from Individuals/Entities and (iii) any other liability, as well as (iv) any dues reported on the candidate affidavit.
Net Assets	"Net Worth" of the Candidate. Defined as the sum of (1) and (2) minus (3) and computed at the beginning (*Initial Net Assets*) and at the end (*Final Net Assets*) of the electoral cycle under consideration. We remove candidates with extremely low net asset bases (Net assets below Rs 100,000 as of election 1).
Net Asset Growth	Annualized Growth in Net Assets over an election cycle. Winsorized at the 1 and 99 percentiles.
Winner	Dummy variable taking on a value of 1 if the contestant won election at t=0.
Minister	Dummy variable indicating whether the constituency winner was appointed to the state's Council of Ministers.
Margin	Vote share difference between winner and runner-up (negative for runners-up).
Incumbent	Dummy variable taking on a value of 1 if the contesting candidate won the preceding constituency election (t=-1).
Prior Member	Dummy variable taking on a value of 1 if the contesting candidate won the constituency election at t=-2.
Education	Ordinary scale variable ranging from 1 to 9. We assign values based on the following education bands: 1 = Illiterate, 2 = Literate, 3 = 5th Pass, 4 = 8th Pass, 5 = 10th Pass, 6 = 12th Pass, 7 = Graduate or Graduate Professional, 8 = Post Graduate, 9 = Doctorate. This variable is missing if education information was not given.
Years of Education	Number of years of education the candidate has received. When using log specification, one is added to the number of years of education.
Criminal Record	Dummy variable indicating whether the candidate has past or pending criminal cases.
Government	Dummy variable indicating whether the candidate's party is part of the ruling state government.
SC/ST Quota	Dummy variable indicating whether the constituency of the candidate is that of disadvantaged groups, so-called Scheduled Castes and Tribes (SC/ST).
TI Corruption	Survey-based state corruption index (based on perceived corruption in public services) as reported in the 2005 Corruption Study by Transparency International India. The index takes on a low value of 240 for the state of Kerala (perceived as "least corrupt") and a high value of 695 for Bihar (perceived as "most corrupt"). We rescale the original measure such that it has a mean of zero and standard deviation of one, for the 17 states in our sample.
Female	Dummy indicating the gender of the candidate (1 = Female).
Age	The age of the candidate at the first election.
Base Salary	Monthly base salaries of MLAs. Collected from states' Salaries and Allowances and Pension of Members of the Legislative Assembly (Amendment) Acts, official websites, and newspaper articles.
BIMARU	Dummy variable indicating whether the constituency is located in one of the states Bihar, Madhya Pradesh, Rajasthan or Uttar Pradesh.
BIMAROU	Dummy variable indicating whether the constituency is located in one of the states Bihar, Madhya Pradesh, Rajasthan, Orissa or Uttar Pradesh.
Income per Capita	Average state-level per capita net domestic product at factor cost between 2004 and 2009 (Source: RBI).

Table 2-3: **Descriptive Statistics of Constituency-Matched Pairs (1140 Candidates)**

Notes: Panel A shows descriptive statistics for the 1,140 constituency-paired candidates that constitute our main sample (570 winners and 570 runners-up). In Panel B, we only include candidates of those constituencies that are decided by a winning margin of five or less percent ("close elections"). Except for *Net Wealth*, which is shown both elections, all variables are as of the first of the two elections. Variables are defined in detail in Table 2-2. The last two columns show average differences and t-statistics of difference-in-means tests.

	Winner and Runner-up			**Winner**			**Runner-up**			**Diff. in Means**	
Variable	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.	Diff.	T-stat
Panel A: All Constituencies											
log(Initial Net Assets)	15.15	15.15	1.42	15.13	15.15	1.40	15.16	15.15	1.44	-0.04	-0.42
log(Final Net Assets)	16.04	16.02	1.43	16.11	16.09	1.36	15.97	15.93	1.50	0.14	1.67
Female	0.06	0	0.23	0.06	0	0.24	0.06	0	0.23	0.00	0.25
Age	48.42	48	9.89	47.83	48	9.80	49.02	49	9.94	-1.19	-2.03
Years of education	13.90	15	3.15	13.74	15	3.38	14.05	15	2.90	-0.32	-1.67
Incumbent	0.37	0	0.48	0.34	0	0.47	0.40	0	0.49	-0.06	-2.15
Criminal Record	0.30	0	0.46	0.30	0	0.46	0.31	0	0.46	-0.00	-0.11
Minister	0.07	0	0.25	0.14	0	0.35					
Margin	8.39	6.29	7.43								
SC/ST Quota	0.18	0	0.39								
MLA Base Salary	16,671	8,000	21,391								
Panel B: Constituencies decided by Margin $\leq$ 5%											
log(Initial Net Assets)	15.08	15.18	1.38	15.04	15.12	1.34	15.13	15.19	1.42	-0.08	-0.65
log(Final Net Assets)	15.97	15.99	1.36	16.02	16.02	1.26	15.92	15.95	1.46	0.10	0.79
Female	0.06	0	0.24	0.05	0	0.23	0.07	0	0.26	-0.02	-0.78
Age	48.44	48	9.83	47.63	47	9.53	49.26	49	10.09	-1.63	-1.76
Years of education	14.02	15	3.15	13.69	15	3.49	14.35	15	2.73	-0.66	-2.21
Incumbent	0.37	0	0.48	0.34	0	0.48	0.40	0	0.49	-0.05	-1.17
Criminal Record	0.32	0	0.47	0.30	0	0.46	0.35	0	0.48	-0.04	-1.01
Minister	0.06	0	0.23	0.12	0	0.32					
Margin	2.42	2.51	1.46								
SC/ST Quota	0.13	0	0.34								

Table 2-4: **Within-Constituency Effects of Winning the Election**

Notes: The regression equation estimated is: $log(FinalNetAssets_{ic}) = \alpha_c + \beta Winner_{ic} + \delta_1 log(InitialNetAssets_{ic}) + \delta_2' Controls_{ic} + \epsilon_{ic}$. The dependent variable, $log(FinalNetAssets_{ic})$, is the logarithm of net wealth at the end of the legislative period. α_c is a constituency fixed-effect. $Winner_{ic}$ is the dummy for winning the initial election (t=0). $log(InitialNetAssets_{ic})$ is the logarithm of the initial net assets of the politician. $Controls_{ic}$ include the logarithm of years of education, criminal record (dummy if a criminal record were present as of the first election), gender, age, and incumbency. The regression is also run for close elections (Columns 3-5), where the vote share gap between the winner and the incumbent was less than 10, 5, and 3 percentage points. Robust standard errors are given in parentheses. The reported constant is the average value of the fixed effects. Lastly, we convert our point estimates into annual asset growth premiums (point estimate divided by 4.9; the average legislative period in our sample is 4.9 years.) Coefficients with ***, **, and * are statistically significant at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
Variables	log(Final Net Assets)				
Winner	0.167***	0.164***	0.187***	0.160**	0.209**
	(0.049)	(0.052)	(0.056)	(0.067)	(0.085)
log(Initial Net Assets)	0.722***	0.710***	0.715***	0.693***	0.674***
	(0.031)	(0.034)	(0.038)	(0.047)	(0.058)
log(Years of Education)		-0.057			
		(0.117)			
Criminal Record		0.061			
		(0.089)			
Female		-0.293			
		(0.181)			
Age		-0.012			
		(0.028)			
Age^2		1.07E-04			
		(0.000)			
Incumbent		0.081			
		(0.062)			
Constant	5.021***	5.651***	5.108***	5.432***	5.704***
	(0.469)	(0.894)	(0.569)	(0.704)	(0.873)
Close Elections:			$\lvert Margin \rvert \leq 10$	$\lvert Margin \rvert \leq 5$	$\lvert Margin \rvert \leq 3$
Observations	1,140	1,099	768	450	274
R-squared	0.833	0.841	0.848	0.861	0.868
Annual growth premium:					
Winner	*3.40%*	*3.35%*	*3.81%*	*3.27%*	*4.26%*

Table 2-5: **Winner Premium and State-level Corruption**

Notes: This table presents results based on several measures of state-level corruption. In columns (1) and (2), the sample is split based on whether a constituency is located in a *BIMARU* state and the regression equation estimated is: $log(FinalNetAssets_{ic}) = \alpha_c + \beta * Winner_{ic} + \delta * log(InitialNetAssets_{ic}) + \epsilon_{ic}$. The dependent variable, $log(FinalNetAssets_{ic})$, is the logarithm of net wealth at the end of the legislative period. α_c is a constituency fixed-effect. $Winner_{ic}$ is the dummy for winning the initial election (t=0) and $log(InitialNetAssets_{ic})$ is the logarithm of the initial net assets of the politician. In column (3), we use the full sample and include an interaction term *Winner*BIMARU*. In columns (4) and (5), we present results employing two alternative state-level measures of corruption, *BIMAROU* and *TICorruption*. Bootstrapped standard errors clustered at the state-level are given in parentheses (Cameron, Gelbach, and Miller (2008)). The reported constant is the average value of the fixed effects. Lastly, we convert our point estimates into annual asset growth premiums (point estimate divided by 4.9; the average legislative period in our sample is 4.9 years.) Coefficients with ***, **, and * are statistically significant at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
Variables			log(Final Net Assets)		
Winner	0.257***	0.122**	0.121**	0.104*	0.188***
	(0.026)	(0.051)	(0.051)	(0.054)	(0.045)
log(Initial Net Assets)	0.681***	0.743***	0.721***	0.720***	0.718***
	(0.022)	(0.040)	(0.029)	(0.030)	(0.031)
Winner*BIMARU			0.136**		
			(0.058)		
Winner*BIMAROU				0.156***	
				(0.059)	
Winner*TI Corruption					0.063**
					(0.027)
Constant	5.697***	4.672***	5.033***	5.051***	5.080***
	(0.324)	(0.612)	(0.450)	(0.454)	(0.471)
Sub-Sample:	*BIMARU*	*Non-BIMARU*			
Observations	386	754	1,140	1,140	998
R-squared	0.842	0.83	0.833	0.834	0.833
Annual growth premium:					
Winner	*5.24%*	*2.49%*	*2.47%*	*2.12%*	*3.82%*
*Winner*BIMARU*			*2.77%*		
*Winner*BIMAROU*				*3.17%*	
*Winner*TI Corruption*					*1.28%*

Table 2-6: **The Effect of Potential Influence in Government on the Returns to Office**

Notes: This table compares the returns of ruling party politicians to those who were elected but not part of the majority party or coalition. We denote ruling party or coalition members by the indicator variable, *Government*, and include it as well as the interaction term $Government{*}Winner$ in Equation (2.6). $Minister$ denotes whether the constituency winner was appointed to the state's Council of Ministers. Robust standard errors are given in parentheses. The reported constant is the average value of the fixed effects. Lastly, we convert our point estimates into annual asset growth premiums (point estimate divided by 4.9; the average legislative period in our sample is 4.9 years.) Coefficients with ***, **, and * are statistically significant at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)
Variables	log(Final Net Assets)		
Winner	-0.121	0.083	-0.096
	(0.142)	(0.051)	(0.139)
log(Initial Net Assets)	0.729***	0.715***	0.721***
	(0.031)	(0.031)	(0.031)
Government	-0.217		-0.181
	(0.172)		(0.167)
Government*Winner	0.606*		0.416
	(0.316)		(0.304)
Minister		0.602***	0.534***
		(0.152)	(0.159)
Constant	4.986***	5.125***	5.097***
	(0.469)	(0.467)	(0.468)
Observations	1,140	1,140	1,140
R-squared	0.835	0.838	0.839
Annual growth premium:			
Winner	*-2.47%*	*1.70%*	*-1.96%*
Government	*-4.43%*		*-3.69%*
*Winner*Government*	*12.36%*		*8.48%*
Minister		*12.27%*	*10.88%*

Table 2-7: **Returns of Past and Present Ministers & Asset Growth Decomposition**

Notes: The dependent variable in columns (1)-(4) is the log of the politician's final net worth. The sample in columns (1)-(3) consists of all re-contesting candidates who either held a ministerial post during the current or preceding legislative period, or both. In column (4), the sample is further refined to only include current ministers as well as past ministers who won the current election but whose party was not a member of the ruling state government ("minister quality" sample). In columns (5) and (6), the dependent variable is the log of the politician's final movable and immovable assets, respectively, and the sample consists of the constituency-matched pairs. Robust standard errors are given in parentheses. The reported constant is the average value of the fixed effects. Lastly, we convert our point estimates into annual asset growth premiums (point estimate divided by 4.9; the average legislative period in our sample is 4.9 years.) Coefficients with ***, **, and * are statistically significant at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
					log(Final	log(Final
Variables		log(Final Net Assets)			Mov. Assets)	Immov. Assets)
Winner	0.057	0.060	-0.117		0.305***	0.070
	(0.099)	(0.099)	(0.172)		(0.063)	(0.065)
Minister	0.312***	0.343***	0.439**	0.236***	0.311*	0.372**
	(0.083)	(0.088)	(0.176)	(0.090)	(0.165)	(0.162)
Incumbent		0.085	0.058	0.068		
		(0.079)	(0.151)	(0.075)		
log(Initial Net Assets)	0.694***	0.692***	0.736***	0.659***		
	(0.027)	(0.027)	(0.051)	(0.030)		
log(Initial Movable Assets)					0.629***	
					(0.034)	
log(Initial Immovable Assets)						0.645***
						(0.039)
Constant	5.461***	5.407***	4.818***	6.057***	5.929***	6.127***
	(0.429)	(0.436)	(0.804)	(0.497)	(0.452)	(0.576)
Sub-Sample:				*Minister quality*		
Observations	514	514	514	378	1,114	1,070
Fixed Effects	State	State	Dist.	State	Const.	Const.
R-squared	0.731	0.732	0.887	0.785	0.799	0.792
Annual growth premium:						
Winner	*1.16%*	*1.22%*	*-2.38%*		*6.21%*	*1.42%*
Minister	*6.36%*	*6.99%*	*8.96%*	*4.82%*	*6.34%*	*7.59%*
Incumbent		*1.73%*	*1.19%*	*1.39%*		

Table 2-8: **Incumbency**

Notes: The table shows results for the constituency fixed-effects regression model and investigates the effects of incumbency. The log of politicians' final net assets is the dependent variable. *Winner* is 1 if the politician won the initial election (t=0) and 0 if the politician did not win. *Incumbent* is the dummy for incumbency. We also include an interaction term between *Incumbent* and *Winner*. *Minister* indicates whether the constituency winner was appointed to the state's Council of Ministers. In column (3), we also include a dummy variable, *PriorMember*, which indicates whether the candidate won the constituency election at t=-2, as well as its interaction with *Winner*. Robust standard errors are given in parentheses. The reported constant is the average value of the fixed effects. Lastly, we convert our point estimates into annual asset growth premiums (point estimate divided by 4.9; the average legislative period in our sample is 4.9 years.) Coefficients with ***, **, and * are statistically significant at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)
Variables	log(Final Net Assets)		
Winner	-0.106	-0.145	-0.136
	(0.105)	(0.104)	(0.106)
log(Initial Net Assets)	0.709***	0.707***	0.711***
	(0.032)	(0.031)	(0.032)
Incumbent	-0.288**	-0.276**	-0.275**
	(0.127)	(0.126)	(0.131)
Incumbent*Winner	0.751***	0.651***	0.685***
	(0.238)	(0.236)	(0.247)
Minister		0.537***	0.564***
		(0.156)	(0.158)
PriorMember			-0.036
			(0.115)
PriorMember*Winner			-0.126
			(0.204)
Constant	5.340***	5.356***	5.314***
	(0.477)	(0.474)	(0.477)
Observations	1,140	1,140	1,140
R-squared	0.837	0.841	0.841
Annual growth premium:			
Winner	*-2.15%*	*-2.97%*	*-2.77%*
Incumbent	*-5.88%*	*-5.62%*	*-5.60%*
*Incumbent*Winner*	*15.31%*	*13.28%*	*13.97%*
Minister		*10.94%*	*11.49%*

Table 2-9: **Other Candidate Characteristics**

Notes: Other characteristics analyzed include education, average income per capita, constituencies reserved for SC/ST candidates, gender, MLA base salaries and their interactions with *Winner*. *log(Years of Education)* is the logarithm of one plus years of education the candidate has received. *Income per Capita* measures average state-level per capita net domestic product between 2004 and 2009. *SC/ST_Quota* is a dummy for whether or not the constituency of the candidate is that of a disadvantaged group, so-called Scheduled Tribes and Castes (SC/ST). *Female* is the dummy for the gender of the candidate. Robust standard errors are given in parentheses. The reported constant is the average value of the fixed effects. Coefficients with ***, **, and * are statistically significant at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Variables			log(Final Net Assets)			
Winner	1.722**	0.852	0.108**	0.110**	0.135***	-0.175
	(0.677)	(0.922)	(0.053)	(0.052)	(0.051)	(0.508)
log(Initial Net Assets)	0.714***	0.720***	0.723***	0.725***	0.726***	0.714***
	(0.033)	(0.032)	(0.031)	(0.024)	(0.030)	(0.034)
log(Years of Education)	0.291					
	(0.184)					
log(Years of Education)*Winner	-0.585**					
	(0.254)					
Winner*log(Income per Capita)		-0.067				
		(0.091)				
SC/ST_Quota*Winner			0.321**	0.330***		
			(0.132)	(0.127)		
SC/ST_Quota				-0.311**		
				(0.128)		
Female					-0.549**	
					(0.225)	
Winner*Female					0.566*	
					(0.307)	
Winner*log(Base Salary)						0.034
						(0.055)
Constant	4.359***	5.054***	5.001***	5.024***	4.998***	5.146***
	(0.657)	(0.475)	(0.460)	(0.363)	(0.458)	(0.502)
Observations	1,100	1,140	1,140	1,140	1,140	1,035
R-squared	0.84	0.833	0.835	0.766	0.835	0.841

Table 2-10: **Regression Discontinuity Design**

Notes: In this table, we report results from regression discontinuity specifications. In Panel A, we present discontinuity estimates using local linear regressions for the subsample of elections that were decided by margins of 5% or less. In column (1), we report results using the entire sample of constituency matched winners and runners-up. In columns (2) and (3) we partition the sample into *BIMARU* and *Non-BIMARU* constituencies. Column (4) only includes *Ministers* with corresponding runners-up, and (5) only includes winners not appointed to the Council of Ministers and corresponding runners-up. Finally, in columns (6)-(7), we disaggregate the sample based on whether an incumbent is standing for reelection in the constituency. Column (6) shows results for the sample of constituencies where an incumbent was standing for reelection; column (7) uses the sample of non-incumbent constituencies. In Panel B, we present discontinuity estimates in residuals at the winning threshold according to equation (2.7) and corresponding to the plots shown in Figure 2-2. Specifically, in a first step we generate residuals by regressing log(*Final Net Assets*) on candidate observables, including log(*Initial Net Assets*), gender, incumbency, and age, and a constituency-fixed effect but excluding the winner dummy and margin. In a second step we run the following regression: $res_{ic} = \alpha + \tau \cdot D_{ic} + \beta \cdot f(Margin_{ic}) + \eta \cdot D_{ic} \cdot f(Margin_{ic}) + \epsilon_{ic}$, where res_{ic} is the residual obtained in the first-step regression, D_{ic} is the dummy for winning, and $f(Margin_{ic})$ are flexible fourth-order polynomials. The goal of these functions is to fit smoothed curves on either side of the suspected discontinuity. The magnitude of the discontinuity, τ, is estimated by the difference in the values of the two smoothed functions evaluated at 0. Coefficients with ***, **, and * are statistically significant at the 1%, 5%, and 10% levels, respectively. Robust standard errors are given in parentheses. P-values for tests of differences between estimated coefficients are reported for all contrasts.

Panel A: Estimation using Local Linear Regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Constituency Sample	All Winners	BIMARU	Non-BIMARU	Ministers	Non-Ministers	Incumbent	Non-Incumbent
Winner	0.236*	0.493***	0.115	0.773***	0.168	0.310*	-0.168
	(0.138)	(0.180)	(0.188)	(0.252)	(0.155)	(0.160)	(0.259)
p-values:		0.0297		0.0005		0.0167	
Observations	440	162	278	50	390	325	115
R-squared	0.871	0.889	0.869	0.932	0.869	0.87	0.91

Panel B: RDD using Residuals

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Constituency Sample	All Winners	BIMARU	Non-BIMARU	Ministers	Non-Ministers	Incumbent	Non-Incumbent
Winner	0.207*	0.624***	-0.034	0.627***	0.125	0.286**	-0.056
	(0.115)	(0.149)	(0.154)	(0.184)	(0.127)	(0.131)	(0.231)
p-values:		0.002		0.0213		0.1921	
Observations	1,102	380	722	150	952	818	284
R-squared	0.021	0.09	0.015	0.229	0.01	0.05	0.041

Table 2-11: **Evidence from Bihar's Hung Assembly**

Notes: We present results from a quasi-experiment using Bihar's hung legislative assembly. From the 243 constituencies contested in the February election, we sample those where both the winner and runner-up competed again in the October election of the same year and emerged as winner/runner-up or runner-up/winner in this later election. In Panel A, we list the probabilities of winning the October election as a function of the winning margin in the February election. In Panel B, we show the annual net asset growth of candidates whose status as winner/runner-up switched as a result of the hung assembly (*Winner* indicates election winners in the October election). In Column (1), we include all such candidates whose winner status shifted between these two 2005 elections and in Column (2) we limit our analysis to the constituency matched sample. Standard errors of differences are reported in parentheses.

Panel A: Randomness of Close Elections

Bihar February 2005	**Probability of Winning October 2005 Election**					
Winner	66.2%	63.2%	60.9%	58.6%	52.2%	50.0%
Runner-Up	33.8%	36.8%	39.1%	41.4%	47.8%	50.0%
Margin (February 2005)		$< 20\%$	$< 15\%$	$< 10\%$	$< 5\%$	$< 1\%$
Elections	130	117	110	87	46	10

Panel B: Annual Net Asset Growth of "Switchers"

	(1)	(2)
Winner	0.289	0.195
Runner-up	0.161	0.137
Difference	0.128**	0.058
	(0.064)	(0.073)

Table 2-12: **Predicted values of final wealth and Winner**

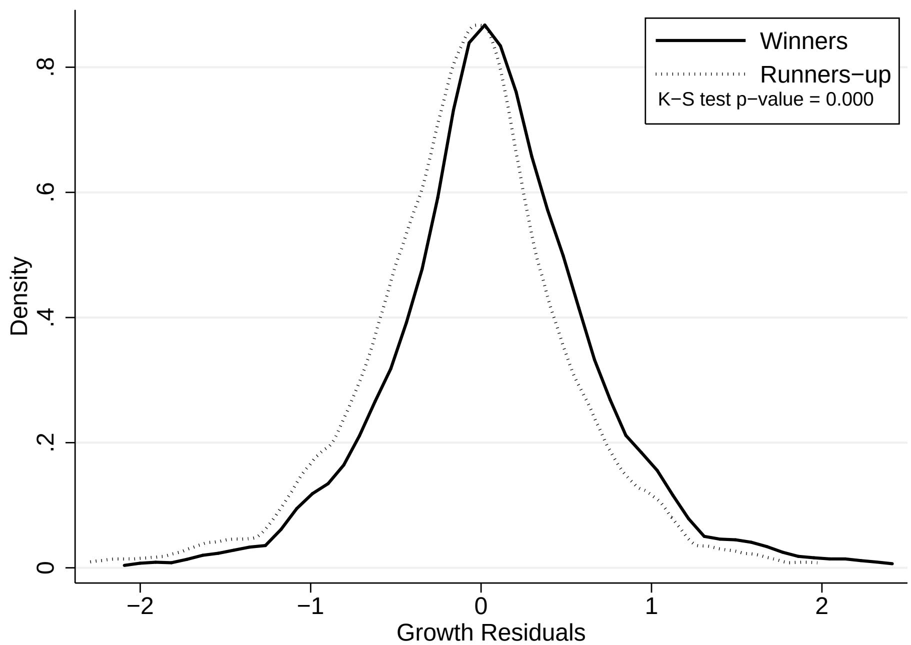
Notes: In a first stage, we regress log(*Final Net Assets*) on candidate characteristics. In a second stage, we regress the predicted values of log(*Final Net Assets*) on the dummy for winning the election, *Winner* (Column 2). Column 3 further includes *Minister* and *BIMARU* interactions. P-values of a test of equality of the "placebo" coefficient (with predicted final wealth) and the "real" coefficient (with actual final wealth) are 0.000 for *Winner*, 0.0701 for *Winner*BIMARU*, and 0.003 for *Minister*. The regression is also run for close elections (Columns 4-6), where the vote share gap between the winner and runner-up was less than five percentage points (corresponding p-values are 0.0004, 0.1357, and 0.1244). Robust standard errors are given in parentheses. The reported constant is the average value of the fixed effects in columns (1) and (4). Coefficients with ***, **, and * are statistically significant at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	log(Final Net Assets)			log(Final Net Assets)		
Variables	*observed*	*predicted*	*predicted*	*observed*	*predicted*	*predicted*
Winner		-0.021	-0.078		-0.069	-0.159
		(0.079)	(0.105)		(0.121)	(0.163)
Winner*BIMARU			0.0802			0.182
			(0.158)			(0.241)
Minister			0.231			0.169
			(0.163)			(0.228)
log(Initial Net Assets)	0.712***			0.665***		
	(0.034)			(0.054)		
log(Years of Education)	-0.097			-0.217		
	(0.121)			(0.199)		
Criminal Record	0.049			0.108		
	(0.090)			(0.119)		
Female	-0.288			-0.13		
	(0.188)			(0.293)		
Age	-0.012			0.004		
	(0.028)			(0.042)		
Age$\hat{2}$	9.18E-05			-6.8E-05		
	(0.000)			(0.000)		
Incumbent	0.068			0.198**		
	(0.063)			(0.091)		
Constant	6.400***	16.06***	16.00***	6.045***	16.00***	15.88***
	(0.858)	(0.057)	(0.074)	(1.140)	(0.088)	(0.115)
BIMARU			0.178			0.344*
			(0.111)			(0.178)
Close Elections:					$\lvert\text{Margin}\rvert \leq 5$	
Observations	1,099	1,099	1,099	440	440	440
R-squared	0.837	0	0.008	0.867	0.001	0.03

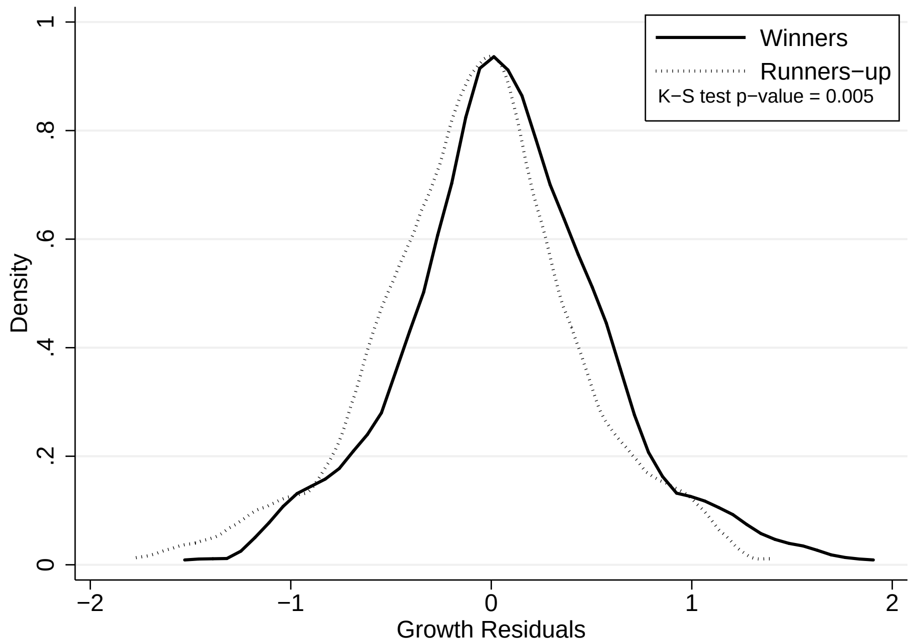
Figure 2-1: Kernel Densities of Asset Growth Residuals

Notes: This figure plots Epanechnikov kernel densities of residuals obtained from regressing log(*Final Net Assets*) on log(*Initial Net Assets*) and candidate observables (characteristics such as net assets, gender, and age but excluding winner dummy and margin) for the sample of constituency-matched candidates. Panel A uses the entire sample of constituency-matched candidates while Panel B only uses candidates that were within a margin of 5 percentage points ("close elections"). In Panels C and D, we divide the sample based on whether their constituencies are located in *BIMARU* states. In Panel E, we further disaggregate winners into ministers and non-ministers and plot kernel densities of these two groups as well as the runners-up. Finally, in Panels F and G, we disaggregate the sample based on whether an incumbent is standing for reelection in the constituency. Panel F shows winner and runner-up densities for the sample of constituencies where an incumbent was standing for reelection. Panel G shows densities for the subsample of non-incumbent constituencies. K-S = Kolmogorov-Smirnov test for equality of distributions. The chosen bandwidth is the width that would minimize the mean integrated squared error if the data were Gaussian and a Gaussian kernel were used.

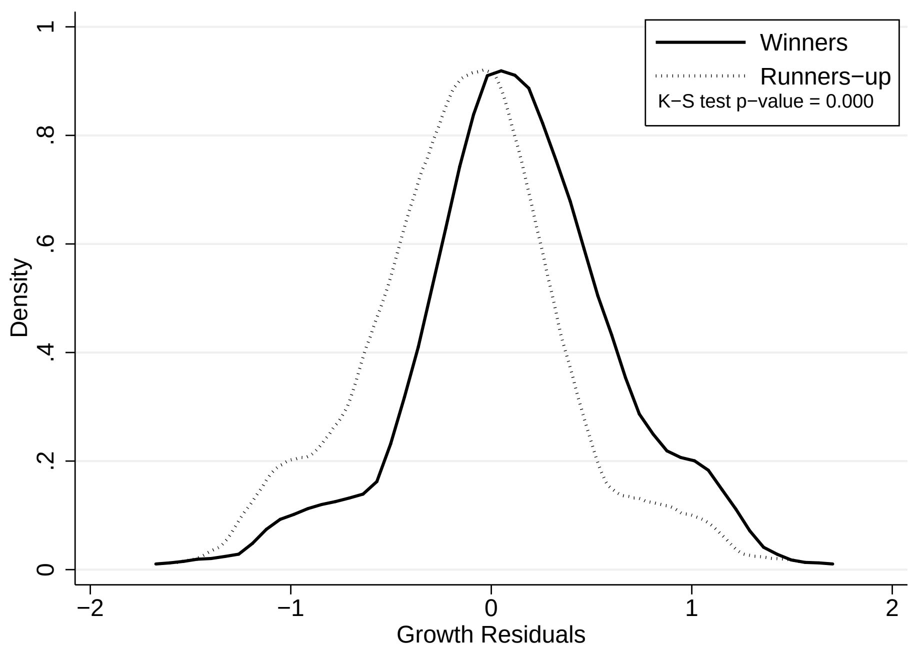
Panel A: Winners and Runners-up



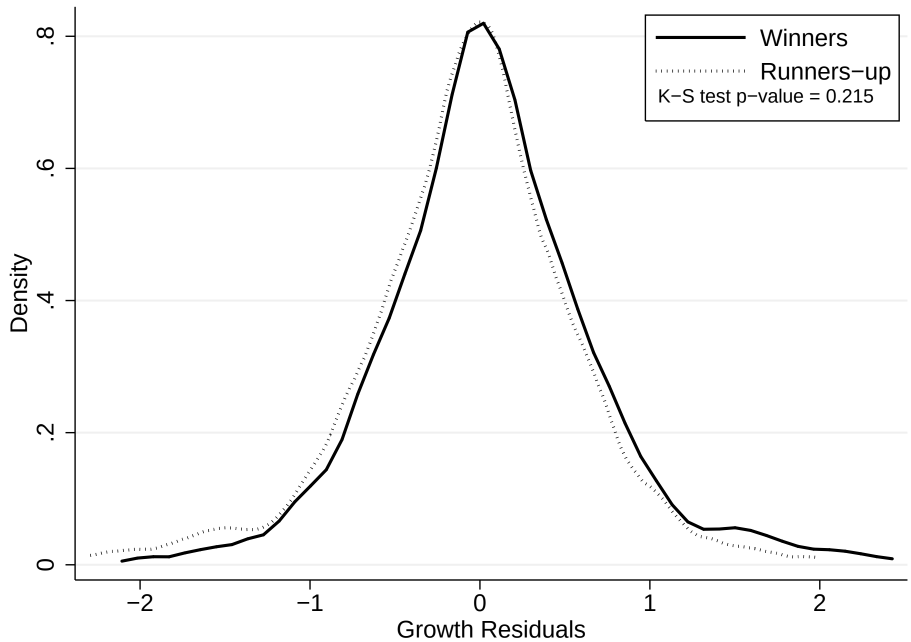
Panel B: Winners and Runners-up in Close Elections (Margin $\leq$ 5%)



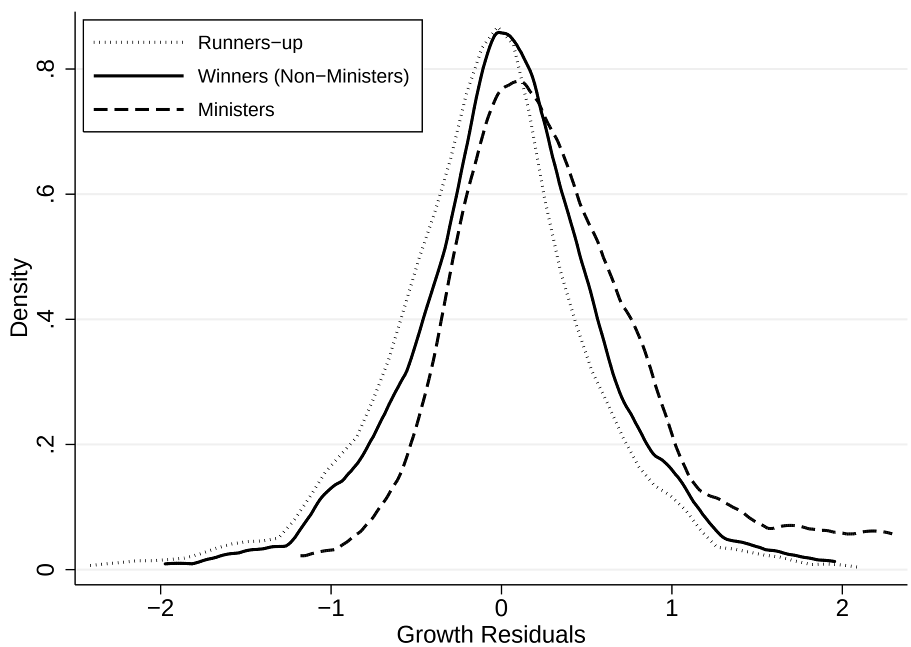
Panel C: Winners and Runners-up in BIMARU Constituencies



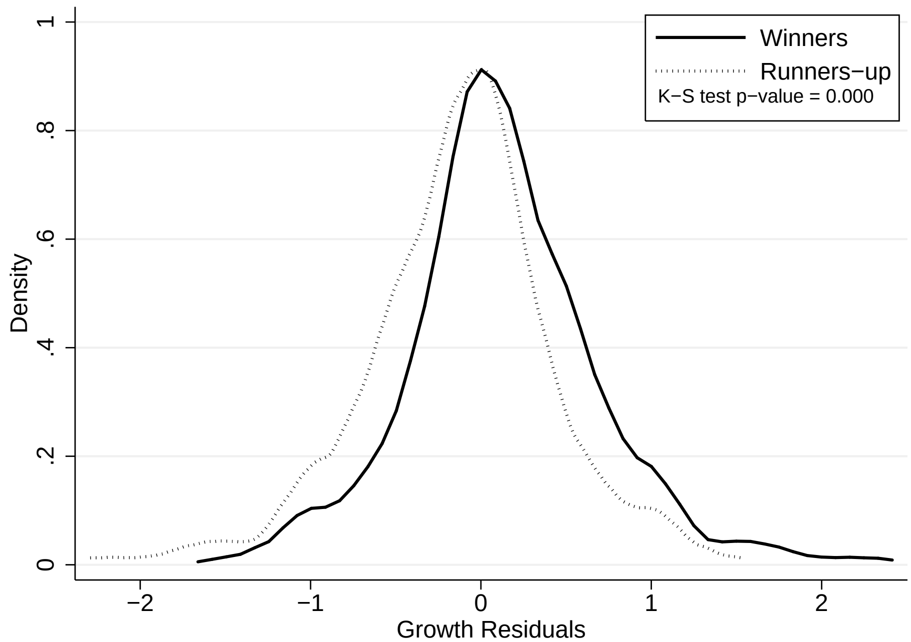
Panel D: Winners and Runners-up in Non-BIMARU Constituencies



Panel E: Ministers, Non-Minister Winners, and Runners-up



Panel F: Winners and Runners-up in Constituencies with Incumbent standing for reelection



Panel G: Winners and Runners-up in Constituencies without Incumbent standing for reelection

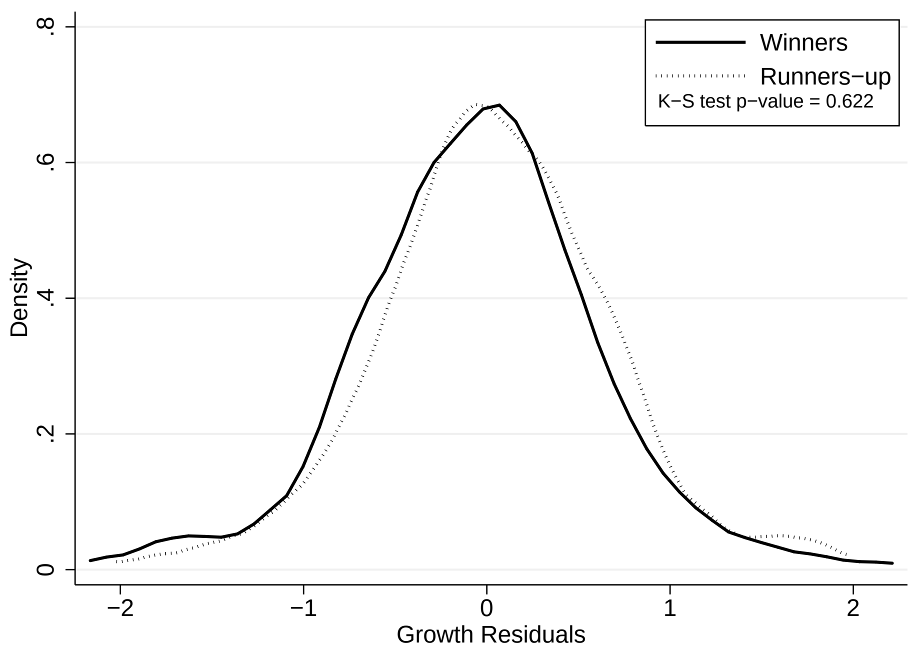
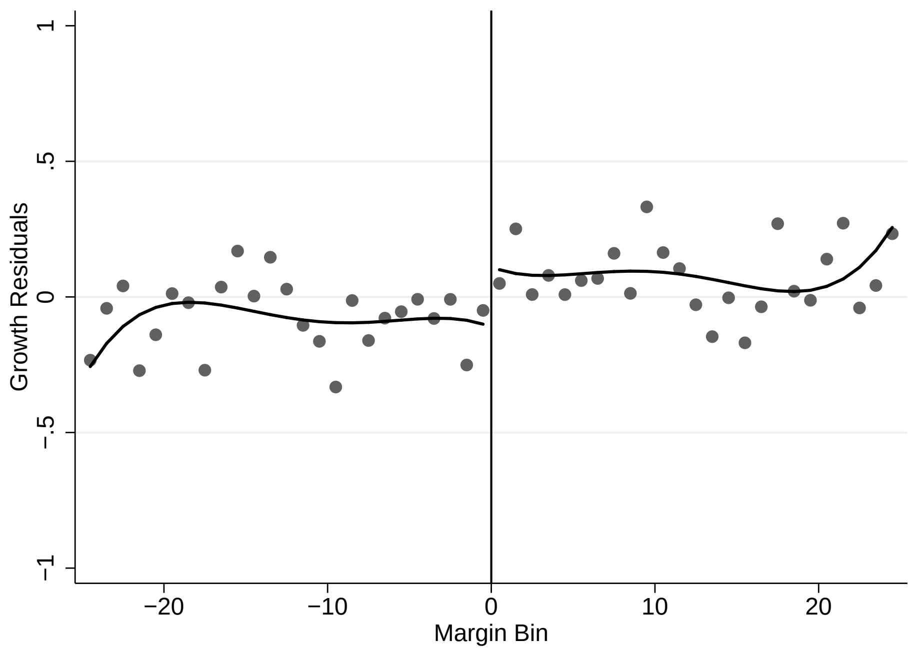


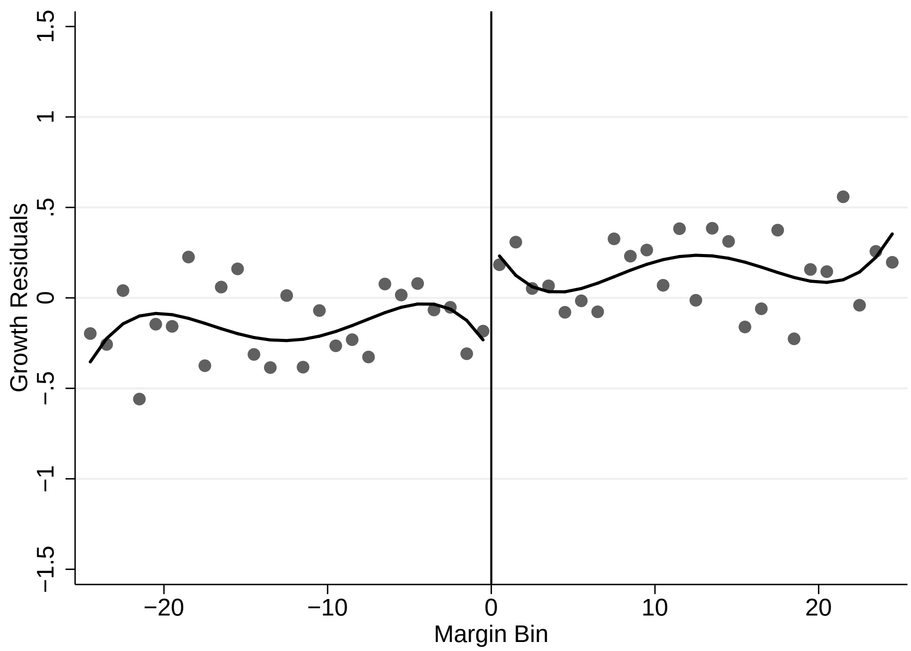
Figure 2-2: Regression Discontinuity Design

Notes: This figure investigates residuals obtained by regressing *log(Final Net Assets)* on candidate observables, including *log(Initial Net Assets)*, gender, incumbency, and age, but excluding winner dummy and margin as a function of winning margin for the sample of constituency-matched candidates. We first collapse residuals on margin intervals of size 1 percentage point (margins ranging from -25 to +25) and then estimate the following equation: $\bar{R}_i = \alpha + \tau \cdot D_i + \beta \cdot f(Margin(i)) + \eta \cdot D_i \cdot f(Margin(i)) + \epsilon_i$ where $\bar{R}_i$ is the average residual value within each margin bin i, $Margin(i))$ is the midpoint of the margin bin i, D_i is an indicator that takes a value of 1 if the midpoint of margin bin i is positive and a value of 0 if it is negative, and ϵ_i is the error term. $f(Margin(i))$ and $D_i \cdot f(Margin(i))$ are flexible fourth-order polynomials. Panel A shows results using the sample of all winners sand runners-up. In Panels B and C we partition the sample based on whether a constituency was located in a *BIMARU* state. Panel D only includes Ministers with corresponding Runners-up; Panel E only includes winners that were not appointed to the Council of Ministers with corresponding Runners-up. Finally, in Panels F and G, we disaggregate the sample based on whether an incumbent is standing for reelection in the constituency. Panel F shows results for the sample of constituencies where an incumbent was standing for reelection; Panel G shows the subsample of non-incumbent constituencies.

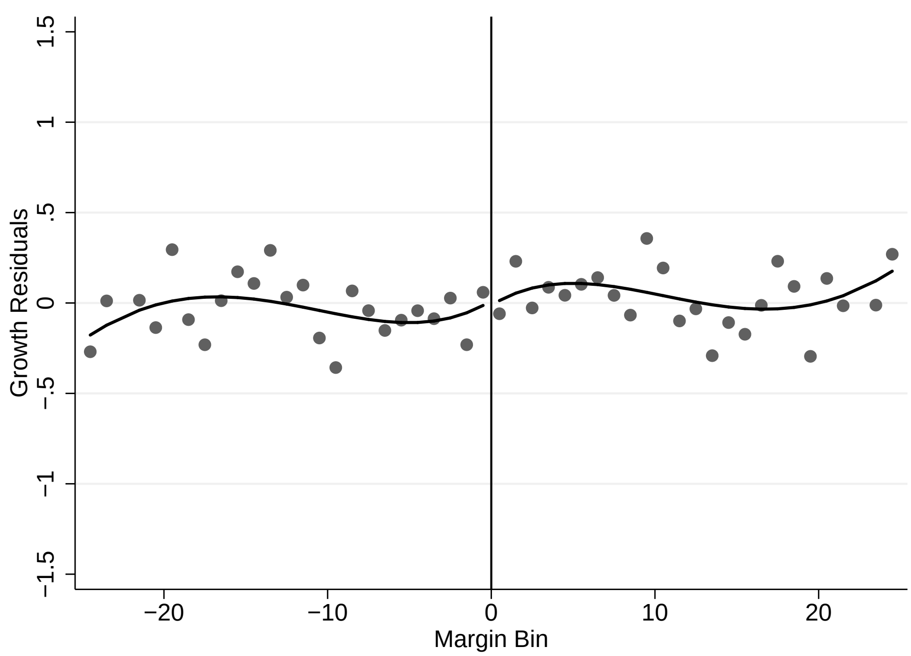
Panel A: Winners and Runners-up



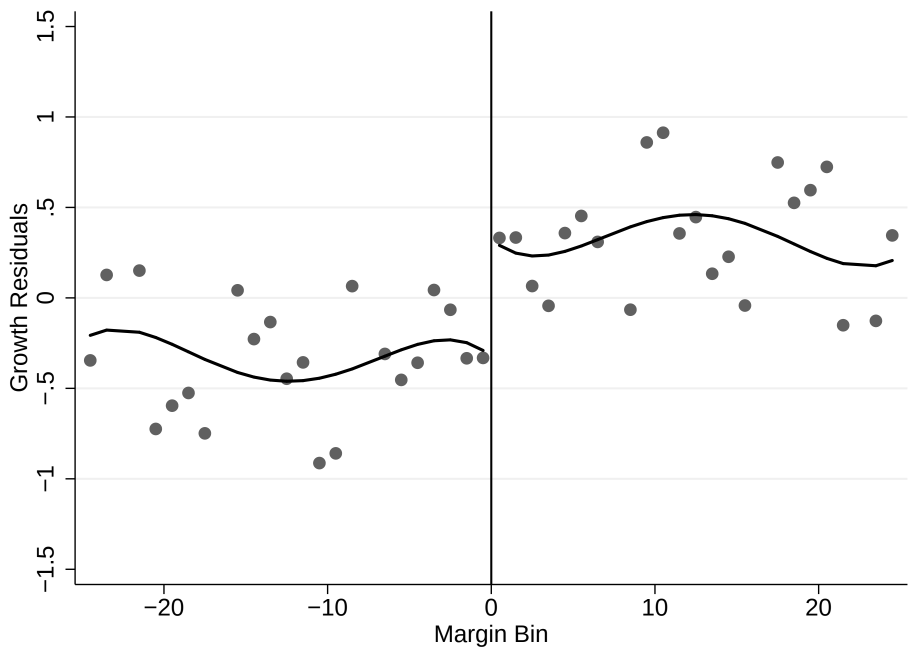
Panel B: Winners and Runners-up in BIMARU States



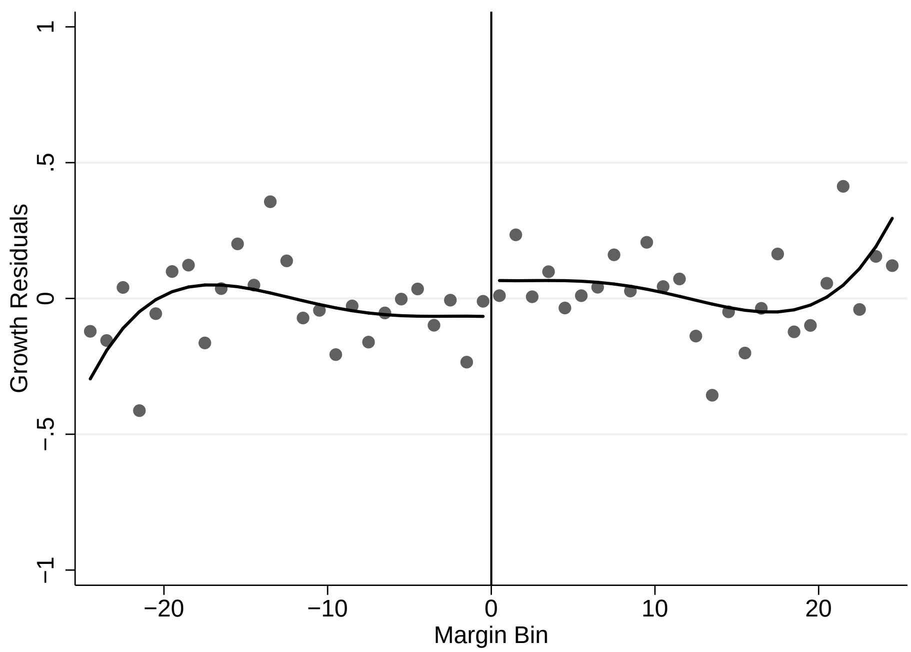
Panel C: Winners and Runners-up in Non-BIMARU States



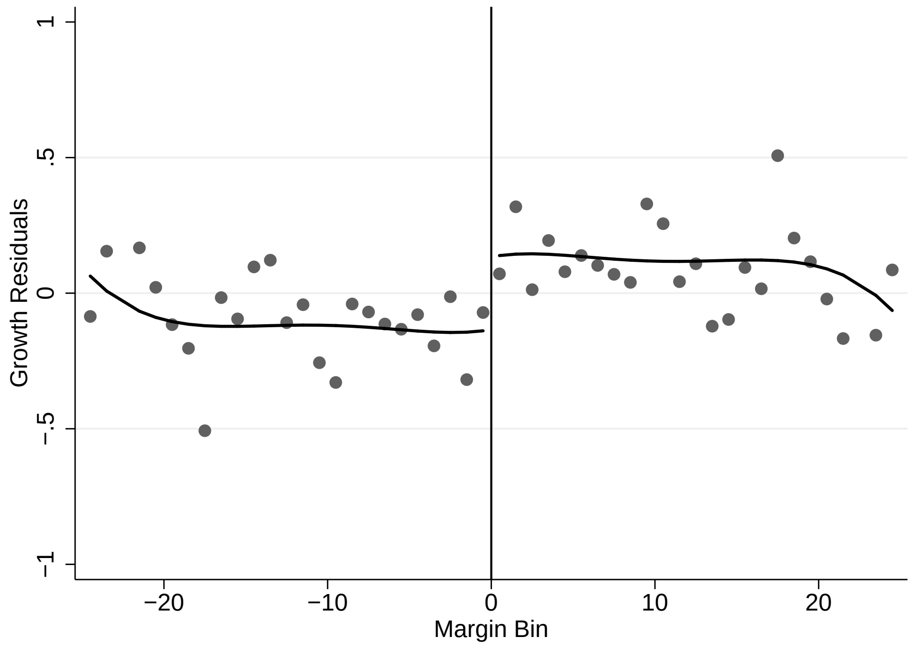
Panel D: Ministers and Runners-up



Panel E: Non-Ministers and Runners-up



Panel F: Winners and Runners-up in Constituencies with Incumbent standing for reelection



Panel G: Winners and Runners-up in Constituencies without Incumbent standing for reelection

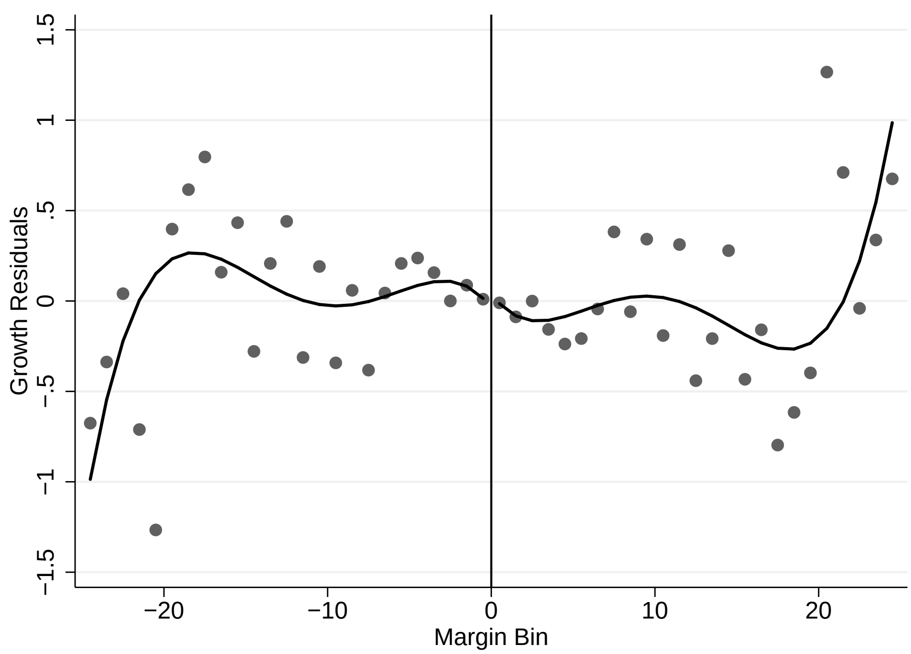
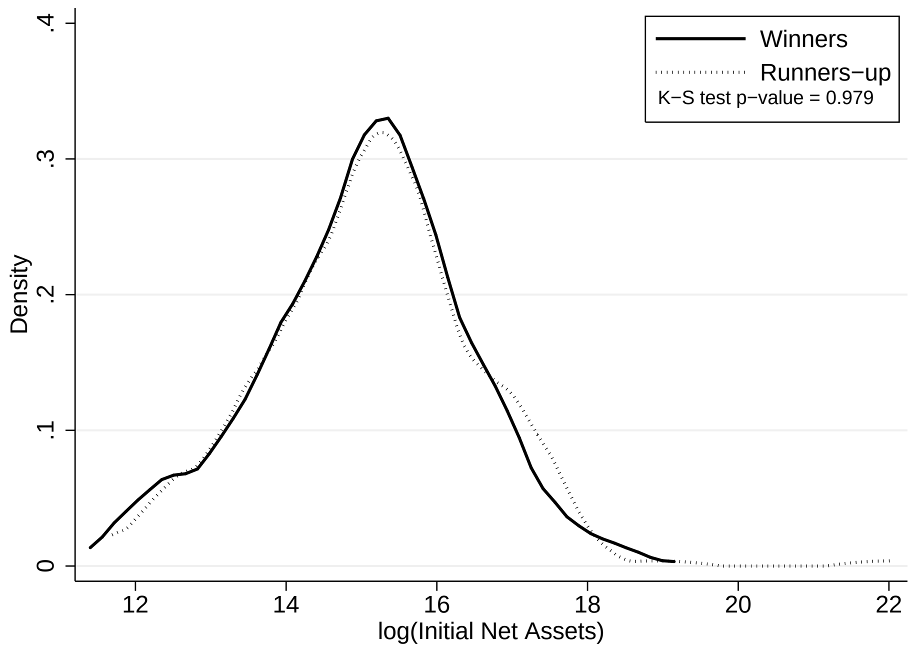


Figure 2-3: Kernel Densities of Observables Characteristics in Close Elections

Notes: This figure plots Epanechnikov kernel densities of log(*Initial Net Assets*) and *Age* for the sample of constituency-matched candidates that were within a *Margin* of 5 percentage points ("close elections"). Panel A plots log(*Initial Net Assets*) densities for winners and runners-up and Panel B plots densities for *Age*. K-S = Kolmogorov-Smirnov test for equality of distributions. The chosen bandwidth is the width that would minimize the mean integrated squared error if the data were Gaussian and a Gaussian kernel were used.

Panel A: Initial Net Wealth in Close Elections (Margin $\leq 5\%$)



Panel B: Age in Close Elections (Margin $\leq$ 5%)

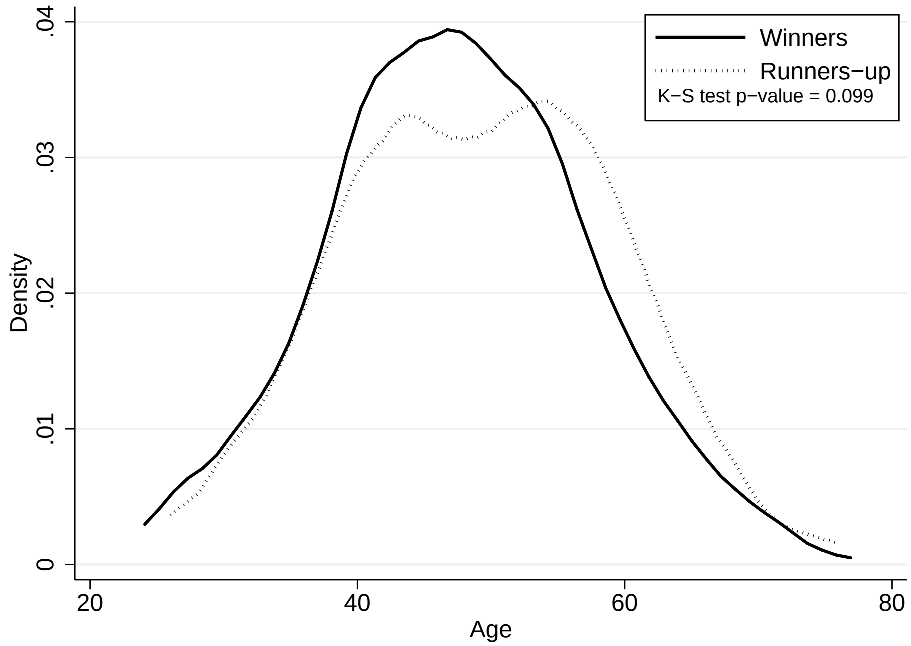


Figure 2-4: Seasoned Candidates

Notes: We investigate the winner's premium for the subsample of seasoned politicians. The point estimate of the discontinuity is 0.521 and significant at the 10% level (t-statistic of 1.84).