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# Defector Politicians and Economic Growth: Evidence from India

Karan Makkar

**Abstract**

Political defectors refer to politicians who switch parties. This paper uses a close-election regression discontinuity (RD) model to estimate the impact of electing political defectors to Indian state legislatures on economic growth (proxied by night-light growth) in their electoral constituencies. I estimate that the election of a defector from the governing party causes a 0.13-0.48 percentage point (2-7.5%) increase in annual GDP growth in their constituency. Further, I find strong evidence that this increased growth is not driven by time-invariant ways in which defectors differ from non-defectors. Therefore, defectors improve economic outcomes in their constituencies either because 1) Defection provides politicians with resources and influence (for example in the form of ministerial positions) that they use to benefit their constituents, or because 2) Parties recruit politicians with increasing political influence, status or competence for defection. The evidence on which of these two channels explains the effect is inconclusive.

**JEL Classification:** D72, D73, O12.

**Keywords:** Defector Politicians, Economic Growth, Night-Lights, Close-Elections.

## 1. Introduction

Political defectors refer to politicians who switch their party affiliation. Unlike most western democracies where such defectors are rare, defection is an extremely common occurrence in Indian politics, particularly at the level of state legislative assemblies. For example, just between the years of 2016 and 2020, 405 Members of Legislative Assembly (MLAs)- corresponding to 9.8% of all seats-changed parties (ADR, 2021). In contrast, just 24 American state legislators -0.3 percent of the total- switched parties between 2017 and 2019 (Shor, 2021)[1]. Although there is little academic work investigating the consequences of politician defection in India, there is extensive anecdotal

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[1] I compare 5 years in India to 2 years in the US since these are the respective durations between successive elections in the two countries.

evidence[2] that defector MLAs who defect to the winning party are disproportionately likely to be appointed as Ministers in state governments as compensation for switching parties (compared to their ruling party non-defector counterparts). In the same vein, it is plausible that defectors joining winning parties are rewarded for their defections with increased resources for their constituencies in the form of government jobs, infrastructure projects, electricity lines, national rural employment guarantee (NREGS) jobs, or even through the allocation of competent bureaucrats to their constituencies. Therefore, a combination of increased resources and representatives in positions of power from having defector MLAs may mean that defectors have significant positive effects on economic activity and public goods provision in their constituencies, especially if they belong to the ruling party.

A second reason defectors could affect economic activity is if they differ from non-defectors in ways that are relevant to the effective governance of their constituencies. Evidence from Brazil Desposato (2006) suggests that one of the primary factors driving political defection, particularly in low-development regions, is the desire to access the rents that an office-holder can extract from a position of power. Politicians who shift to parties with which they are less ideologically aligned in order to ensure a seat in the legislative assembly may be especially self-interested, and therefore especially likely to engage in corrupt behavior while in office. If defectors are more corrupt than non-defectors, electing defector MLAs may lead to less efficient construction of infrastructure projects, worse delivery of welfare benefits and public goods, reduced private investment and lower economic activity in their constituencies. On the other hand, if defectors are more ambitious, politically savvy or otherwise effective politicians than non-defectors, they may improve the aforementioned outcomes. These potential differences between defectors and non-defectors are pertinent since Indian state legislators have been shown to exercise significant control over development outcomes in their constituencies (Clots-Figueras, 2012; Bhalotra and Clots-Figueras, 2014; Asher and Novosad, 2017). In particular, because state governments control the transfer of Indian Administrative Service (IAS) officers[3] (Iyer and Mani, 2012), and sitting MLAs influence the executive in the

[2] https://www.newindianexpress.com/nation/2022/mar/29/sawant-sworn-in-defectors-dominate-cabinet-2435370.html,https://www.hindustantimes.com/india-news/karnataka-cabinet-10-mlas-defectors-from-congress-and-jds-to-join-yediyurappa-g story-bTTzh4YEDSXBYNTTmzM58H.html

[3] The chief bureaucrats responsible for the implementation of government policy, including welfare scheme delivery,

transfer of such officers, MLAs can significantly influence state and central government policies in their constituencies[4].

To test the empirical predictions of the aforementioned hypotheses, this paper estimates the impact of electing defectors on economic activity in their constituencies (proxied by night-light growth), using a close-election regression discontinuity (RD) model. Next, I ascertain whether defectors' effects on economic activity are driven by time-invariant characteristics of defector politicians (e.g., higher corruption or competence), time-varying causes of defection (e.g. recruitment of rising political stars as defectors) or by the extra resources and power that defectors may accrue when they are rewarded for switching parties. Finally in order to test a few specific mechanisms by which defectors may affect aggregate economic activity, I combine evidence from an instrumental variables (IV) strategy motivated by the close election design and the aforementioned RD models to test how electing defectors affects their constituents' access to public goods like electricity, tar roads and schools.

I find that the election of a defector increases annual night-light (NL) growth in their constituency by between 1.3 and 4.8 percentage points. Using an elasticity of 0.1 of GDP growth with respect to night-light growth estimated at the district level for India (Bickenbach et al., 2016), this translates to a 0.13-0.48 percentage point increase in GDP growth. Relative to an average annual GDP growth rate of 6.46% over the same time period, this represents a 2-7.5% increase in GDP growth over baseline. This effect is statistically significant and is robust to the use of different bandwidth algorithms, variation in the polynomial order of the running variable, to different sample restrictions, and to the addition of both candidate and constituency controls.

There is strong evidence that defectors' effect on economic growth is not driven by time invariant ways in which they differ from non-defectors. In particular, defectors have no statistically significant impact on night-light growth in the electoral terms prior to and after the election in which they defect. However, the evidence on whether defectors' effect is driven by the defector characteristic itself (e.g. rewards for defection), or because defectors differ in time-varying from

infrastructure construction and business regulation.

[4]https://www.deccanchronicle.com/nation/current-affairs/040118/rift-with-mlas-prove-costly-for-ias-officers.html,https://timesofindia.indiatimes.com/city/jaipur/ias-officer-fines-mlas-driver-transferred/articleshow/75189928.cms

non defectors (e.g. parties recruit politicians with *rising* political influence) is inconclusive. On the one hand, MLAs who defect to the ruling party are 2-3 times more likely than non-defectors from the ruling party to become government ministers post-defection. These minister positions provide a potential mechanism by which defectors my economically benefit their constituencies. Moreover, in elections prior to their defection, defector MLAs from the ruling party are no-more likely to become ministers than non-defector MLAs from the ruling party, indicating that defectors' over-representation as ministers is not the result of a time-invariant selection effect. However, it is possible that defectors being given these positions is reflective of their *rising* political influence and administrative competence- factors which would effect NL growth independent of defection. Furthermore, I find that both governing and opposition defectors increase economic growth in their constituencies, and I cannot reject that their effects on NL growth are equal[5]. This leaves open to further research the question of what traits or characteristics of defector politicians that change around the time of defection cause an increase in NL growth.

In contrast to their clear effects on night-light growth, defectors do not consistently cause an increase in any of the public-goods measures mentioned above. Because the estimated effect of defectors on these public goods are highly imprecise, it is unclear whether the lack of consistent positive effects is due to imprecision or a true-null. Therefore, the specific policy levers that defectors use to boost economic activity are yet undetermined and an additional subject for future research.

In addition to answering the substantive question of the impact of defector politicians on their constituents, these results have three additional implications: two India specific and one more broadly applicable. First, they provide an explanation for why Indian voters don't punish defecting politicians at the ballot box for behavior that appears self-interested or ideologically inconsistent. In particular, defector politicians are more likely than not to win elections in which they are one of the top-two candidates (see Table A1). While alternative accounts of this voter behavior may credit low voter information or a lack of strong ideological preferences, these results suggest that voters may elect defectors in anticipation of benefiting from the power those defector MLAs might

[5]This isn't because of a precisely estimated null difference, but rather effects of similar magnitudes estimated with insufficient precision to be distinguished from each other.

accrue.

Second, it helps explain why voters may care about the personal characteristics of their representatives and display strong in-group preference despite the extremely limited role individual legislators play in shaping legislation in India. In particular, the anti-defection law means that legislators who defy a party whip and vote against their parties are subject to being disqualified from the legislature and losing their seats. Further, state legislatures were in session for an average of 23 days in 2021 and less than 10% of bills were sent to committees for review during that year (Menon et al., 2021). Despite this limited influence, the caste and local ties of politicians are an extremely salient factor in selecting candidates for particular electoral constituencies. This paper helps explain this phenomenon by demonstrating the significant influence of legislators on policy outcomes for their constituents through mechanisms outside of their traditional roles as law-makers. That is, in-group preference and close attention to a politician's personal characteristics may be driven by the influence politicians have over policy implementation in their own constituencies.

Finally, the significant influence of politicians on their constituents outcomes reveals a neglected avenue of improving program implementation in developing countries. Specifically, to the extent that legislators control policy implementation in their constituencies, enlisting their cooperation the execution of programs in their districts may improve their efficacy.[6] RCTs testing interventions in co-operation with state governments could vary whether or not legislators in different districts are enlisted for cooperation to formally test this hypothesis.

The rest of this paper is organized as follows. I first review the prior literature related to this topic and contextualize its importance to my findings. Next, I explain the data and empirical methods used in this paper and discuss the main night light results and causal mechanisms. I conclude with a summary of the findings and policy implications.

## 2. Literature Review

There are three sets of literature relevant to this paper: first, the political science literature studying the causes of politician defection, second, the methodological literature testing the validity of

[6] For example, this could be done by monetarily incentivising legislators for improved outcomes in their districts.

close election RD designs, and third, the economics literature examining the effects of politician characteristics on constituent outcomes in India. A small political science literature examines the factors contributing to high levels of defection in different countries. Heller and Mershon (2005) find in the context of the Italian Chamber of Deputies that the vast majority of defections occur right before the formation of a new government, or the fall of an incumbent government. Similarly, Desposato (2006) finds that in the context of the Brazilian state legislatures, defectors are twice as likely to defect to the party which holds the majority in the national legislature, and more than thrice as likely to defect to the party that holds the state governor's office than to parties which don't hold power[7].

In contrast, O'brien and Shomer (2013) find that whether or not the party in power is only very weakly predictive of probability of defection to or from it. Instead, a party's ideological consistency is highly negatively correlated with defection to and from it. However, given that the study uses data on defections from a set of high-income countries with low rate of defections [8], it is unlikely that these results have external validity to Indian state legislatures. Therefore, both papers from high-defection contexts indicate that access to political power constitute an important motive for party switching. Insofar as similar results hold in Indian state legislatures, increased public resources from increased politician power provides one causal mechanism through which electing defectors affects economic outcomes.

The close election RD design used in this paper has been employed to estimate the effects of election results on a wide range of political and economic outcomes since Lee (2001) first applied it to estimate the electoral advantage to being an incumbent in House of Representatives elections in the US. For example, it has been used to estimate the effects of unionization on worker outcomes (DiNardo and Lee, 2004), political party ideology on government spending and unemployment rates (Ferreira and Gyourko, 2009; Pettersson-Lidbom, 2008), and politician religion on abortion prevalence (Bhalotra et al., 2012) among many other election results and outcomes.

Importantly, the identifying assumption for the close election designs from these papers to

[7]The statistic for the governor's party is true only of low education districts, while the statistic for the central government holds both high and low education districts.

[8]Except for Brazil and Israel

provide unbiased treatment effects is random assignment of the treatment variable at the 0% margin of victory cutoff. Caughey and Sekhon (2011) argue that this assumption is violated in the case of recent US House of Representatives elections, with winners and losers of extremely close elections differing discontinuously on covariates like the amount they spend on their election campaigns and whether they belong to the same party as the incumbent. Further, Grimmer et al. (2011) find that US house candidates from the party holding the governorship or majorities in the state legislatures are disproportionately likely to win close elections. However, Eggers et al. (2015) find that these violations of the RD assumptions are restricted to the particular context of recent US House of Representatives elections. Neither close races to mayoral, statewide or state legislative offices in the US, nor US house races from previous eras, nor elections from the 9 other countries they study display similar discontinuities. Moreover, Hyytinen et al. (2018) find that bias corrected RD models produce similar estimates of incumbency advantage to experimental estimates in the context of Finnish municipal council elections. Finally, the extensive close election RD literature focusing on elections to Indian state legislatures (reviewed below) consistently finds no evidence of manipulation or discontinuous variation of covariates of the cutoff. Consequently, absent specific evidence to the contrary, the close election RD design in this paper likely provides an unbiased estimate of the treatment effect electing of defectors.

In addition to the validating the random assignment of politicians, I heavily draw on the prior methods literature in interpreting my estimand. Specifically, I follow Marshall (2022) in interpreting the treatment effect from close election designs as not the effect of defector status, but instead of defector politicians. That is, the (baseline) treatment effect combines both the effect of defector status, and all other traits on which defectors who contest close elections with non-defectors differ from their opponents on average.

The literature on the effects of politician characteristics on constituent outcomes provides extensive evidence of the importance of state legislatures in the health, educational, and other economic and social outcomes of their constituent. By far the largest body of evidence regarding the effects of politician identity on constituents studies the effect of female politicians. Clots-Figueras (2012) uses an RD design exploiting close elections to state legislatures in India between male and female candidates to find that electing women in urban areas increases the chances that children who grew up in their districts complete primary education. Clots-Figueras (2011), Baskaran et al. (2018) and Priyanka (2020) use similar empirical strategies to show, respectively, that electing women to state legislatures causes increases in primary education spending, higher NL growth in their constituencies, and increased labor force participation for girls who grow up in their district. Chattopadhyay and Duflo (2004), Kumar and Prakash (2017), Iyer et al. (2012) exploit the introduction of gender quotas in state and local government bodies to show that a larger share of female politicians increases provision of public goods favored by women (like access to drinking water), decreases in infant mortality, and increases in reporting of crimes against women. While the literature focusing on developed countries consistently finds no effect of female politicians on the amount and allocations of public spending (e.g. Kuliomina (2021), see Hessami and da Fonseca (2020) for lit-review), electing female legislators is associated with the increased passage of non-budgetary woman-focused policies (Hessami and da Fonseca, 2020).

The literature on politician effects on their constituents extends beyond the study of politician gender. A higher share of Muslim state legislators in a district have shown to be associated with decreased infant mortality (Bhalotra and Clots-Figueras, 2014), a lower probability of religious riots (Bhalotra et al., 2014), and a decreased prevalence of abortion (Bhalotra et al., 2012)in those districts. Pande (2003) uses variation in size of quotas for certain disadvantaged caste groups across state legislatures to show that a higher proportion of legislators from those groups is associated with increased state expenditure on welfare programs and increased caste quotas in public employment. Finally, Chemin (2012), Prakash et al. (2019) and Gehring et al. (2019), Asher and Novosad (2017), and George and Ponattu (2019) demonstrate the importance of politician criminal status, ruling party membership and dynastic status respectively for aggregate economic activity, the use of local development funds, legislative attendance, consumption expenditure, private sector employment and public goods access.

This literature constitutes robust evidence that the characteristics of Indian state legislatures have a measurable impact on the development outcomes of their constituents. Moreover, these impacts can be large in magnitude. For example, a standard deviation increase in the share of female legislators in a district is associated with a 9.8% (1.4 percentage point) decrease in infant mortality, while electing a female MLA is associated with a 25% (1.9 percentage point) increase in GDP growth over the politician's tenure in office. Furthermore, the close election RD studies in particular demonstrate that politicians can affect the outcomes of the specific people in their electoral districts even absent de-jure executive authority over particular state policies. Therefore, while a specific policy mechanism would strengthen the evidence that political defectors increase economic growth in their districts, the absence of such evidence need not imply a lack of an effect.

Finally, in exploring the mechanisms by which defectors effect NL growth, this paper draws on prior work showing evidence of powerful politicians (Indian Chief Ministers) displaying political favoritism and using their influence to prioritise the economic interests of the constituency from which they were elected (Khalil et al. (2021)). While the existing empirical evidence in the Indian context is restricted to chief ministers, the same mechanism, applied to other ministers in state governments (or less-formal positions of influence) provide a lever through which politicians can influence policy and public goods provision to benefit their constituents. Given that defectors are disproportionately likely to hold such ministerial positions (Figure 5), the evidence of political favoritism is a major argument for the rewards for defection explanation of the treatment effect.

## 3. Data

### 3.1. Politician and Election Data

The data on state assembly results and candidate characteristics are drawn from Election Commission of India's Statistical Reports and are cleaned by the Data Development Lab (DDL) (Asher et al.; Prakash et al., 2019) and the Trivedi Centre for Political Data (TCPD) (Jensenius and Verniers, 2017). The datasets are politician-election-constituency level panels tracking electoral results and politician traits in nearly all elections and electoral constituencies in India between 1974 and 2018[9]. I collapse these data to the election-constituency level such that each observation tracks a particular election in a constituency, and includes information on the margin of victory, the political and demographic characteristics of the winner and runner up candidates, as well as information on the defector status of the winner and runner up. This includes 39795 elections from

[9]The excluded constituencies include constituencies that only appear once very early in the election commissions reports and constituencies which had their boundaries change because of the splitting of states. I also exclude elections for which information about either one of the winner or runners up are unavailable.

8358 electoral constituencies[10]. Since the night-light data are only available between 1994 and 2013, the sample for the summary statistics below is limited to the 17544 elections that occurred during this period for which I have both night-light and politician defector status data.

The two different mechanisms through which defectors may affect economic activity require different conceptions of who is a defector. Defectors may affect economic activity because defection is correlated with some time-invariant, inherent characteristic of a politician that is relevant to economic performance like being more corrupt and rent seeking (negative effect) or ambitious, politically astute, and effective (positive effect). This can be modelled as a world with two types of politicians: Loyals (Ls) and Non-Loyals (NLs), where Ls and NLs have different impacts on economic activity and NLs have a higher probability of switching parties in each election than Ls. Therefore, defection provides a signal that causes us to update our prior upwards on the probability that a politician is an NL. If this is the dominant mechanism, then our definition of who is a defector should capture these time invariant characteristics by marking a politician as a defector if they switched parties in any election – past present or future. In contrast, if the dominant mechanism by which defectors affect economic activity is that they're rewarded for defection by positions of power and public resources, then we'd expect the effects of a defector to be apparent exclusively in the elections in which they switch parties.

To distinguish between these mechanisms, I use two definitions of defector. Under the first definition (henceforth definition 1), a politician is classified as a defector if they have, at any point in their political careers, contested elections as members of two or more political parties. That is, a politician is classified as a defector at time t even if they will only defect at time t+n or defected previously at time t-n. This definition is relevant to the time invariant characteristics model where defection is an information revelation mechanism for the politician's type.

This definition may suffer from significant measurement error. For elections near the end of my sample, it is likely that some politicians are coded as "non-defectors", even though they will defect later in their careers. However, since we can't observe defections that haven't yet occurred,

[10]While at any given point in time India has 4121 state legislative constituencies, the boundaries of all of these constituencies change in 2008. Additionally, due to the creation of new states during this time period, there were additional changes in the boundaries of constituencies. As a consequence, the number of constituencies in my panel are more than double the number that exist at any given point of time.

Table 1. Summary Statistics by MLA Defector Status: 1994-2013

| | Total Obs | Non-Defector Mean | Defector Mean | Difference | P-value |
|---|---|---|---|---|---|
| Definition 1 | | | | | |
| Age | 4030 | 50.82 | 50.78 | 0.05 | 0.84 |
| Female | 14956 | 0.05 | 0.05 | -0.00 | 0.50 |
| SC/ST | 4073 | 0.29 | 0.29 | 0.00 | 0.90 |
| Total Assets (Rupees) | 3489 | 36425823.51 | 35142696.01 | 1283127.50 | 0.68 |
| Asset Growth | 1474 | 1.13 | 1.14 | -0.00 | 0.97 |
| Charged With Crime | 3430 | 0.12 | 0.10 | 0.01 | 0.16 |
| Incumbent | 15011 | 0.28 | 0.27 | 0.01 | 0.00 |
| Years of Education | 3440 | 11.95 | 11.88 | 0.07 | 0.28 |
| Governing Coalition Member? | 13008 | 0.43 | 0.42 | 0.01 | 0.25 |
| Number of Elections | 15011 | 2.48 | 2.31 | 0.18 | 0.00 |
| Definition 2 | | | | | |
| Age | 1110 | 51.14 | 50.00 | 1.14 | 0.01 |
| Female | 2797 | 0.05 | 0.04 | 0.01 | 0.13 |
| SC/ST | 1113 | 0.29 | 0.28 | 0.01 | 0.81 |
| Total Assets (Rupees) | 926 | 25267645.80 | 31364689.12 | -6097043.31 | 0.41 |
| Asset Growth | 505 | 1.17 | 1.22 | -0.05 | 0.71 |
| Charged With Crime | 918 | 0.14 | 0.15 | -0.01 | 0.45 |
| Incumbent | 2817 | 0.31 | 0.30 | 0.01 | 0.13 |
| Years of Education | 907 | 11.98 | 11.80 | 0.18 | 0.16 |
| Governing Coalition Member? | 2817 | 0.45 | 0.44 | 0.00 | 0.73 |
| Number of Elections | 2817 | 3.00 | 2.78 | 0.22 | 0.00 |

*Notes*: Differential sample sizes across traits is driven be the fact that politicians were only required to report information about education, assets and criminal status after a supreme court order in 2003.
Definition 1 classifies a politician as a defector if they've ever switched parties in their career.
Definition 2 classifies a politician as a defector for only the election in which they switched parties.

these politicians are classified as non-defectors[11]. This means that absent any corrections, the estimated effect of defector status on any dependent variable will be at biased downward due to attenuation bias. The measurement error can be partially corrected for by two different types of sample restrictions: excluding all elections post 2000 or 2004, and by including only politicians who have been in politics for a minimum number of years (for ex 15 or 20). Both these restrictions restrict my sample to politicians who have spent a long time in politics, and therefore, are very

[11] A second potential source of measurement error comes from the fact that in my dataset I can't observe candidates' party status for more than one election prior to 1974. Therefore, if a politician switched their party in an election prior to 1974, and then stuck with their party post 1974, they are classified as a non-defector. This isn't a large issue given that data for my dependent variables are only available post 1994, when few politicians who had switched parties in the late 1960s/early 70s were still contesting. However, to correct for this, I drop those elections from my sample where one of the top 2 contestants had contested more than 1 election prior to 1974 and that contestant hasn't defected post 1974.

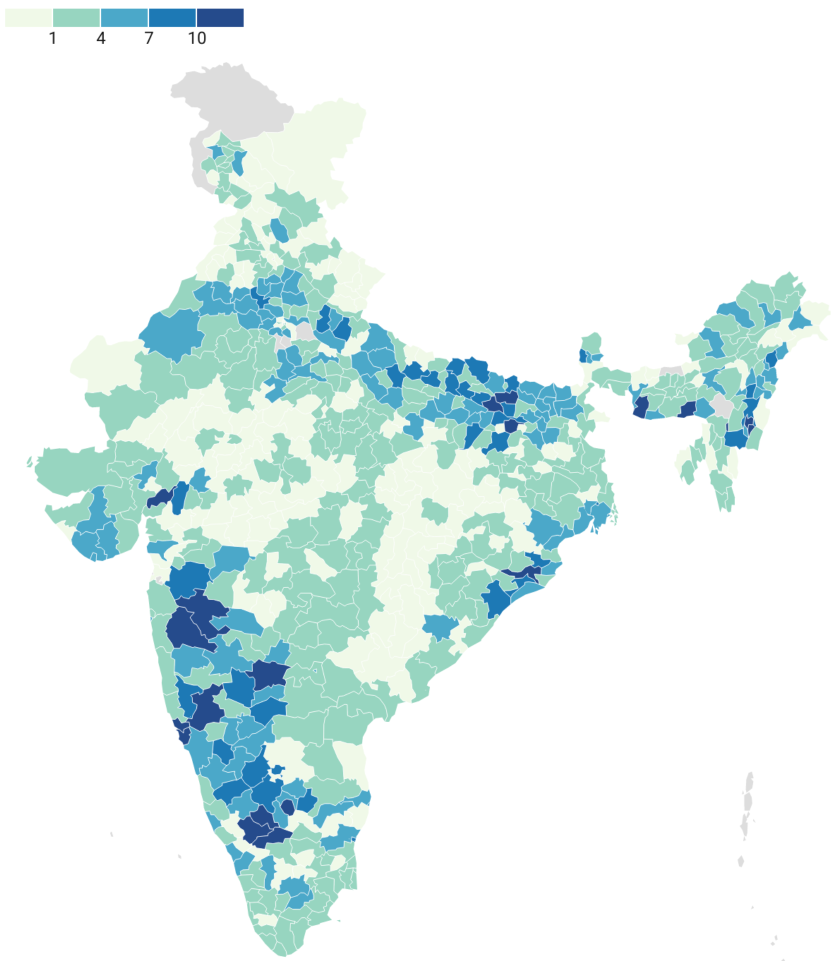
likely have already defected if they were ever going to.

Under the second definition (henceforth definition 2), a politician is classified as a defector in period t only if they changed parties in period t itself. This definition is less relevant to determining whether time invariant characteristics of politicians drive differential outcomes for their constituents. Rather, this definition is applicable to scenarios where the act of switching parties itself affects politician performance, or if defection is caused by changes in time-varying traits that affect NL growth. Therefore, this is the definition of defector most appropriate for testing the hypotheses that defectors increase economic activity in their constituencies because either 1) They're rewarded for defection with ministerial positions, increased state spending and superior public goods provision, or 2)Parties recruit politicians who are increasingly politically, connected, influential and competent, and these traits improve economic performance. Unlike the definition 1, this definition does not suffer from measurement error since whether or not a politician defected in the current period is perfectly observed.

Somewhat surprisingly, defectors and non-defectors have very few statistically significant demographic and political-characteristic differences under either definition (Table 1). The only consistent one across both definitions is that defectors have contested in 0.2 fewer elections than non-defectors on average. The other statistically significant difference using definition 1 is that non-defectors are very slightly less likely to be the incumbent candidate. Strikingly, personal asset growth while in power (a proxy measure of corruption) is nearly identical across both groups. Because of these findings, and because my prior is that the defector-reward mechanism is the most plausible channel through which defectors affect economic activity, my primary regression specifications will use definition 2. However, I will also use definition 1 to check whether defectors affect economic outcomes through time-invariant traits.

Under both definitions, a significant proportion of the elections in the aforementioned sample are relevant to the question of the effects of defectors on economic activity. In particular, of all the elections in the sample, 45% (18.37%) have at least one defector as either the winner or runner up candidate and 30% (10%) have a defector as winner using definition 1 (2). Furthermore, while the proportion of defector winners and contestants has varied significantly across space and time (Figures A1, A2, Table A1), we can see from Figure 1 that the majority of administrative districts

Figure 1. Number of Defector MLAs by District, 1994-2013




*Notes:* The above figure uses definition 2 of defectors. Definition 2 classifies a politician as a defector for only the election in which they switched parties.
The level of aggregation is India's 640 administrative districts using 2011 boundaries.

have had at at least one assembly constituency within it elect a defector MLA between 1994 and 2013 (Median = 1, Mean = 2.9). Moreover, the proportion of elections with a defector (definition 2)[12] in the top 2 remains between 15% and 25% nationally between 1994 and 2013, and there are only 2 states in which the average proportion is below 5% during the same time period. Therefore, despite spatial and temporal heterogeneity, the phenomenon of defection is widespread enough

[12]From now onwards, when I refer to defectors, I refer to definition 2 defectors, unless otherwise specified.

across different states, years, and even districts in India that the effects estimated in this paper are relevant to most regions and all years in the sample.

Moreover, constituencies which have close defector vs non-defector elections between 1994 and 2013 don't appear to differ systematically from those which don't. For example, constituencies which do have such elections have statistically indistinguishable night light intensity[13] growth rates from those that didn't have such elections (Table A2). Furthermore, neither defections nor close elections are exclusive to a select few constituencies within a state. Fully 68% of constituencies for which data is available between 1974 and 2013 have at least one election in which one of the top two candidates is a defector, and more than 84% of constituencies have an election with margin of victory less than 3%. Since RD estimates can be interpreted as weighted average treatment effect with weights equaling the probability that a given unit is close to the cutoff (Lee and Lemieux, 2010), the large number of constituencies with potential defector vs non-defector and close elections implies that this paper's RD estimates are relevant for a large proportion of constituencies. That said, close defector vs non-defector elections between 1994 and 2013 occurred in constituencies which are more rural, lower population and have lower NL intensities than constituencies in which these elections didn't occur. (Table A2). Therefore, insofar as these variables mediate the extent to which defectors affect economic activity, we should be careful while extrapolating the magnitude of this study's estimates to the sample of constituencies which didn't have close defector vs non-defector elections.

### 3.2. Night Light Data

The primary dependent variable is annual growth in night-light intensity- a measure of the luminosity of the sky above a particular geographic area and a proxy for aggregate economic activity. Constituency level night-light data is drawn from a DDL dataset (Asher et al.; Henderson et al., 2012) which maps light data from NASA's DMSP-OLS[14] annual panel dataset to Indian state legislative constituencies. The annual night-light value for a given area measures the average brightness of the night-sky over that area over a year on a scale of 0-63, with 0 least bright and

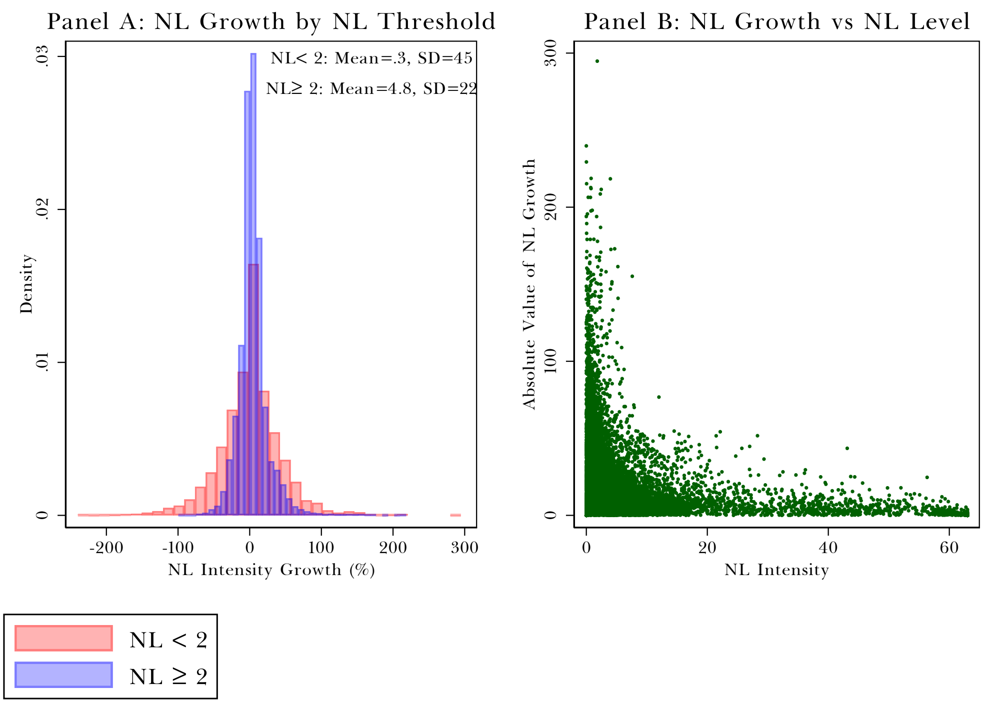
[13]This variable is defined in the next section. It is my proxy for aggregate economic activity.
[14]The acronym stands for Defense Meteorological Satellite Program's Operational Linescan System

63 most bright. NL intensity is computed by dividing the sum of annual night-light values for the pixels within a constituency's boundaries by the number of pixels in the constituency's boundary[15]. I use the uncalibrated DMSP data for my NL intensity measure rather than calibrated data from Elvidge et al. (2014) in order to avoid assuming that reference areas that experience positive GDP and electricity-use growth over time saw no change in night-sky brightness over the same period. Night-light intensity has a correlation of 0.6 with wealth and 0.4 with income at the assembly constituency level in India (Dhillon et al., 2016), and is frequently used as a proxy for aggregate economic activity where measures of the same aren't available at the requisite frequency and geographic level (Doll et al., 2006; Henderson et al., 2011; Pinkovskiy et al., 2013; Michalopoulos and Papaioannou, 2014).

The main sample relevant to the RD specifications are the 11873 observations of from the years after the 3050 elections in which there was one defector and one non-defector in the top-2. The average night-light intensity growth for this sample is 3.4% (slightly lower than the 3.93% for the full 1994-2013 sample). The distribution is extremely long tailed, with a maximum growth of 294% and a minimum of -233% (Figure 2, Panel A). Note, however, that the vast majority of extreme values of NL growth are concentrated at extremely low values of NL intensity (Figure 2, Panel B), and that the standard deviation of NL growth for constituency-years with NL $\geq 2$ is less than half that for constituency years with NL $< 2$ (Figure 2, Panel A). This occurs because small changes in absolute value of NL intensity from very small bases result in very large percent changes in NL intensity. Furthermore, previous work has shown that the elasticity between GDP per capita and NL intensity is lower for low levels of output density and population density (Chen and Nordhaus (2011); Gibson et al. (2021)). I replicate this pattern in the context of Indian assembly constituencies: Both the elasticity and correlation of NL intensity with other measures of development (per-capita consumption, urban population share, non-farm employment/population ratio) is significantly larger for constituencies with higher NL intensities than those with low NL intensities (Table A3, Figure A3). Because of this lower measurement reliability at low levels of NL intensity, and in order to avoid the results being overly influenced by outlier growth numbers, the

[15]Each pixel corresponds roughly (but not exactly) to an area of 1 $km^2$.

Figure 2. Annual Growth in NL Intensity: 1994-2013




*Notes:* Sample is restricted to elections with exactly 1 defector (definition 2). Definition 2 classifies a politician as a defector for only the election in which they switched parties.
Each observation in panel B represents a constituency-year observation.
NL Growth for period t is defined as the log difference in NL intensity between periods t and t-1.

main specifications will restrict the sample to constituencies where NL is likely a better measure of GDP per capita. Specifically, I restrict the sample to either a) Constituency-Years with NL Intensity $\geq 2$ or b) Elections where the minimum NL intensity in the constituency-years associated with that election were $\geq 2$.[16] These restrictions drop 32 and 43% of elections and 37% and 44% of election-years respectively. The mean values of NL growth under these two restrictions are 5% and 2.8% respectively. I use NL intensity as a proxy for output density rather than a more straightforward measure like rural population share or non-agricultural employment because my census measures of these variables have a high degree of missingness and measurement error (see

[16]To guard against picking an NL cutoff to maximize the significance of my results, I present robustness checks using threshholds of NL Intensity = 1 and 3 in the appendix.

Section 3.3).[17]

Table 2. Annual NL Growth by MLA Defector Status: 1994-2013

| | Obs | Mean-Defector Lost | Mean-Defector Won | Difference | P-value |
|---|---|---|---|---|---|
| Definition 1 | | | | | |
| Full Sample | 25327 | 3.77 | 3.88 | -0.12 | 0.77 |
| NL $\geq$2 | 17046 | 5.57 | 5.43 | 0.13 | 0.68 |
| NL$\geq$2 & <5% margin | 5144 | 5.79 | 6.01 | -0.22 | 0.72 |
| Observations | 25327 | | | | |
| Definition 2 | | | | | |
| Full Sample | 11452 | 2.94 | 3.25 | -0.30 | 0.62 |
| NL $\geq$2 | 7189 | 4.67 | 4.89 | -0.23 | 0.66 |
| NL$\geq$2 & <5% margin | 2243 | 3.78 | 5.97 | -2.19 | 0.02 |
| Observations | 11452 | | | | |

*Notes*: Sample restricted to elections with one defector and one non-defector in top 2 vote-winners
Each observation is a constituency-year growth rate.
Definition 1 classifies a politician as a defector if they've ever switched parties in their career.
Definition 2 classifies a politician as a defector for only the election in which they switched parties.

When we use the full sample or use defintion 1 of defection, constituencies which elect defectors grow, on average, no faster or slower than constituencies which elect non-defectors. However, average annual NL growth varies substantially between constituencies which elect defector and non-defector members of legislative assembly (MLAs) when we exclude the low NL intensity constituencies, restrict to close elections and use definition 2 of defector. In particular, constituencies which elected defector MLAs in close elections experience 2.19 pp faster annual NL growth over the politician's tenure compared to constituencies which elect non-defectors (Table 2: row 3) when using definition 2. This difference is significant at $p < 0.05$. While constituencies which elect defectors may, on average, grow faster than those which elect non-defectors this doesn't necessarily imply a causal effect of electing a defector on aggregate economic activity. For example, if there is a non-zero relationship between margin of victory and night-light growth, simply comparing means in growth, even within the 5% margin of victory interval (i.e., for close elections), would result in a biased estimate of the effect of electing defectors because of the "boundary problem" (Lee and Lemieux, 2010)[18]. Therefore, more causally rigorous methods are required to estimate such an

[17]Nevertheless, the appendix contains robustness checks (Table B11) which restrict the sample using rural population share instead of NL intensity and find similar results.

[18]As an example, there could be a negative relationship between NL growth and absolute margin of victory if ruling political parties allocate additional state resources to constituencies which regularly have competitive elections

effect. I discuss these in the next section.

### 3.3. Public Goods Data

One of my primary measures of public goods provision is rural road construction under the central government's Pradhan Mantri Gram Sadak Yojana (PMGSY) scheme. This translates to Prime Minister's Rural Roads Scheme. The policy began in 2000 with the goal of providing transportation connection via all-weather, paved roads to villages which previously didn't have connectivity of this kind. The data is an annual constituency level panel that tracks the length (in km) of roads constructed each year in an electoral constituency between 2004 and 2012. It is drawn from the Ministry of Rural Development's Online Management and Monitoring system and cleaned and mapped to the constituency level by Prakash et al. (2019). Since Prakash et al. (2019). only map the road data to the constituency level between 2004 and 2012, it is available for the duration of only one electoral term for each of the 1398 constituencies in sample , and corresponds to elections that took place between 2004 and 2008[19].

In additional to road construction data, I draw on the data from the decennial Population Censuses of India conducted in 1991, 2001 and 2011 cleaned by Asher et al.. This data tracks the proportion of individuals in an electoral constituency with access to electricity, the proportion whose villages are accessible via a paved road, the number of primary, middle, secondary and senior schools in the constituency, as well as the demographics composition of a the constituency. I use this data to construct measures of change in the number of schools, access to electricity and access to paved roads between 1991 and 2001[20], as well as demographic variables like the constituency's population density, and the rural, literate, Scheduled Caste, Scheduled Tribe shares of the constituencies population. Because this data is originally only available at the village level and is aggregated up to the constituency level by Asher et al., the data suffers from large amounts of missingness where geocoding was imperfect or where too much of the village data was missing. Consequently, the public goods growth data is only available for between 25 and 45% of constituencies depending

with the goal of winning over voters to tip the results of future close elections.

[19] Prakash et al. (2019) doesn't make clear the reason why the road data is missing for 3/4th of constituencies.

[20] I don't use data from the 2011 census because of a change in electoral constituency boundaries in 2008 that makes it impossible to track the number of defectors elected from a constituency between 2001 and 2011.

on the measure in question. Asher et al. recommend caution in the use of the electricity data in particular because of missing data for 60% of villages in 2001. It's also important to note that because of the decennial nature of the data, I'm forced to use census public good estimates for a given constituency for up to 9 years after the census actually takes place. This may introduce substantial measurement error when I condition the sample for a regression on a census variable (e.g. population density).

## 4. Methodology

### 4.1. Night Light RD

I use a regression discontinuity (RD) model to estimate the impact of electing defectors on NL intensity growth and road construction in their constituencies. Specifically, following Hahn et al. (2001), I estimate the local-linear regression below for the subset of elections where the two candidates with the most votes included one defector and one non-defector:

$$\begin{aligned} Y_{ct+1} = \beta_0 + \beta_1 DefectorWon_{ct} + \beta_2 Margin_{ct} + \beta_3 Margin_{ct} * DefectorWon_{ct} + X_{ct} + \epsilon_{ct}, \\ \forall |Margin_{ct}| \leq CCT\% \end{aligned} \tag{1}$$

$Y_{ct}$ is growth in NL intensity for constituency c in year t, where all years from the year of the previous election to the next election are included in the sample. Growth in NL intensity for a given year is calculated as the difference in the log of NL intensity between that year and the previous year. $DefectorWon_{ct}$ is an indicator for whether or not the defector won the previous election in constituency c. $Margin_{ct}$ equals the difference in the vote shares of the winner and runner up in the previous election. The margin is coded as negative for elections in which the defector lost and positive in elections in which the defector won. CCT% is a bandwidth determined by Calonico et al. (2014) and Calonico et al. (2019) optimal bandwidth algorithm. The regression is estimated using a triangular kernel- which places 0 weight on observations outside the bandwidth and increases the weight on elections where the margin was closer to zero. I use this non-parametric, limited bandwidth estimation strategy rather than a traditional RD design with full sample and a high order polynomial running variable in order to avoid the bias associated with using higher order polynomials pointed out by Gelman and Imbens (2019).

$X_{ct}$ represents three sets of controls: first, a set of candidate level controls that includes the winner's gender, caste, incumbency status, the number of elections they've contested in and whether they belong to the governing coalition this election. This set of controls ensures that any effects of defector MLAs on NL intensity growth are not driven by observable candidate characteristics (compensating differentials) correlated with defector status. In addition to the aforementioned candidate controls, the data include a second set of candidate controls including the winner's age, education, assets and criminal status are available for the subset of elections post 2004 ($\sim$ 25% of elections). While not included in the primary regressions to preserve the sample size, these controls are included in additional specifications as robustness checks in the online appendix.

Second, a set of constituency level controls includes the level and growth of NL intensity in the constituency in the year prior to the election, voter turnout, the population of the electoral constituency, and whether or not the constituency was reserved for Scheduled Caste or Scheduled Tribe MLAs. The RD design ought to ensure random assignment of defectors at the cutoff, and therefore an unbiased treatment effect. However, these controls will account for any random variation control variables across control and treatment groups, increase the precision of the estimates for a given sample size, and also thereby improve the bias-variance tradeoff for bandwidth selection. My primary regressions do not include controls for a constituency's rural, literate, SC and ST shares because missing data for $\sim$ 65% of constituencies would greatly *decrease* estimate precision, but I include regressions with these controls as robustness checks in the Online Appendix.

Third, year and state fixed effects. As with the constituency-controls, the RD design should ensure we estimate an unbiased treatment effect even absent these controls. Nevertheless, the inclusion of the FEs in some specifications fulfills two purposes: First, while it would useful be test the identification assumption of random assignment of defector/non-defector MLA to constituencies by state and year, it's impractical to report placebo-results for each year and state. Including Year and State FEs corrects for any worries we may have about defectors being disproportionately likely to win close elections in certain states and years. Second, the inclusion of state and year fixed effects should increase the precision of the estimated treatment effects by partialing out state and year-related variation in growth rates that is orthogonal to defector status.[21]

The main coefficient of interest is on the indicator for whether the winner of the election is a defector. The identification strategy relies on the fact that the regression is run in the sample of elections in which there was one defector and one non-defector politician as the two highest vote winners. As the margins of victory approaches zero, whether a constituency gets a defector or a non-defector MLA becomes quasi-random. Since the winners of close elections are quasi-randomly assigned, we can infer that constituencies that narrowly elect defectors don't systematically differ from those constituencies that narrowly elect non-defectors. Therefore, if the functional form of the running variable is correctly specified such that the model correctly predicts what NL growth would be either side of the cutoff, the estimated difference in growth at the cutoff provides an unbiased estimate of the impact of electing defectors on NL growth.

Since the unbiasedness of the estimate is heavily dependent on the functional form correctly predicting NL growth on either side of the cutoff, I subject my baseline specification to two different types of specification robustness checks. First, I use the two distinct optimal bandwidth algorithms from Calonico et al. (2014), Calonico et al. (2019) to vary the bandwidth over which the local kernel regression is run. Both algorithms trade off the increased bias from a larger bandwidth because of poorer functional form fit with increased precision from the larger sample size that comes with a larger bandwidth. The first, which I will call MSE, minimizes the square of the bias and variance of the RD estimator. The second, which I will call CER minimizes the coverage error of the confidence interval on the RD estimator. The CER bandwidth is always smaller than the MSE bandwidth. Second, I estimate equation 1 with a quadratic rather than a linear running function of margin of victory, as well an additional specification where I impose a zero-slope on the margin of victory variable. Additionally, and unrelated to regression specification, I estimate the model with the full sample in addition to the baseline model with the 5% most extreme values trimmed in order to test my results' robustness to the inclusion of the extremely large left and right tails of the NL growth distribution.

Under the random-assignment for close-elections assumption $\beta_1$ reflects the causal effect of

[21]We can see this in the smaller standard errors for columns 3 compared to 4 and 7 compared to 8 in the regression tables in both the main results section and the appendix.

electing a defector politician as an MLA on NL growth. However, it *does not* reflect the causal effect of defector status on NL growth without additional strong assumptions. That is, defector politicians in close elections may differ from non-defectors in close elections in myriad ways that relate to their effects on NL growth. Therefore, $\beta_1$ from the baseline close-election RD regression (without controls) includes both the effect of the defector politicians' defector status, and of other characteristics on which defectors and non-defectors who contest close elections differ, and which affect politician affects on NL growth (Marshall, 2022))[22]. Unless we assume that defector status is independent of other growth-affecting politician characteristics and that either a) Defector status doesn't affect politician vote share, b) Compensating differentials[23] caused by the effect of defector status on vote share don't affect NL growth or c) Different compensating differentials cancel each other out, $\beta_1$ will reflect a composite of both effects (Marshall, 2022).

I improve on the baseline regression in two ways that loosen the assumptions required to interpret $\beta_1$ as the effect of defector status. First, by continuity tests and co-variate adjustment using the candidate-level variables referenced above. The continuity tests are designed to test for the presence of discontinuous changes in potential compensating differential characteristics which affect NL growth at the 0% margin, while the co-variate adjustments partial-out the effects of these characteristics on NL growth. Importantly, the list of candidate-characteristics above includes gender, defector status and membership of governing/opposition parties- politician characteristics that have previously been shown to affect NL growth. Nevertheless, even if I find all candidate-level variables are continuous at the 0% threshold and the inclusion of controls doesn't substantially affect $\beta_1$, it is still possible that unobserved variables (e.g. politician intelligence) varies discontinuously at the 0% margin and affects NL growth. However, even if this is the case, the inclusion of candidate covariates reduces the proportion of $\beta_1$ that could be explained by such unobservables, and therefore reduces the magnitude of bias in interpreting $\beta_1$ as the causal effect of defector status.

[22] This is true even if defectors and non-defectors who may could *potentially* contest close-elections are statistically identical on other characteristics if defector status effects politician vote share. For example, suppose that being a defector causes a decrease in vote share, and higher competence causes an increase in vote share. Then, defectors who are in close elections will, on average, be more competent than their non-defector counterparts. Insofar as politician competence affects NL growth, this would bias the interpretation of $\beta_1$ as the effect of defector status.

[23] Here, compensating differentials refer to differences in characteristics between defectors and non-defectors in close elections because of the effect of defector status on politician vote share

Second, I compare the effect of defector status using definition 1 (and excluding the election of defection) and definition 2. Suppose we find that that defectors have no effect on NL growth in elections in which they don't defect, but do have an effect in elections in which they do defect. If we're willing to assume changes in compensating differentials are orthogonal to defection, then $\beta_1$ from the definition-2 regression reflects the causal effect of defector status on NL growth. The plausibility of the above orthogonality assumptions varies by type of characteristics. There are three categories of characteristics that it is useful to consider

First, demographic characteristics like candidate gender, caste and age. Absent evidence to the contrary, there aren't strong reasons to believe that the effect of these demographic characteristics on NL growth vary with whether or not a politician defected in a particular election. Second, time varying characteristics like a candidate's political connections or leverage with the party that change *as a consequence* of defection. While these characteristics likely will effect NL growth, they are best understood as part of the defector characteristic I'm trying to measure the effect of. Therefore, their inclusion in the treatment effect is desirable. Finally, changes in time varying-characteristics like political connections or politician performance that both cause defection and are plausibly related to an MLA's effect on NL growth. For example, parties may seek candidates with rising political influence as promising candidates to recruit. Alternatively, candidates who defect may be those who were denied the opportunity to run under their previous party because of poor performance as an MLA. Such time-varying characteristics which also effect NL growth are the most plausible threat to interpreting $\beta_1$ as solely the effect of defector status.

## 5. Results

### 5.1. Identification and Placebo Test

One key assumption of the RD model is that politicians can't precisely manipulate the margin of victory near the threshold. If the density of elections is discontinuous near the 0% margin of victory, it indicates that the defector (or non-defector) has some control over election outcomes even at close margins, and decides to gain just enough votes above the cutoff to win. If defector/non-defector politicians (or their parties) can control election outcomes in this way, the constituencies in which the politicians choose to manipulate votes may not be statistically identical to those in which they don't. Therefore, the assignment of defectors and non-defectors to constituencies is no longer quasi-random at the 0% victory margin.

This is not simply a theoretical concern, since Grimmer et al. (2011) show that US house candidates with advantages like membership of the party holding the governorship or majorities in the state legislature are disproportionately likely to win close elections. Furthermore, in the Indian context, some electoral constituencies in India- particularly pre-1990- suffered from booth capture and ballot stuffing (Guha, 2017; Vaishnav, 2017). This refers to a practice where polling booths are taken over by force, voters likely to vote for opposition parties are prevented from voting and ballot boxes are stuffed with fraudulent votes. Since this practice greatly declined in prevalence post 1990 Vaishnav (2017)'s, it shouldn't be especially relevant for my sample[24]. Nevertheless, I test for the possibility of manipulation using a McCrary (2008) test to check for discontinuity in election density at 0% margin of victory using definition 2 of defector status[25]. The test fails to reject the null of continuity (see Figure 3)

One testable implication of the RD identifying framework is that if the assignment of defectors and non-defectors is random at the cutoff, then none of the other co-variates should vary discontinuously at the cutoff. Therefore, if we estimate RD models akin to equation 1 for different constituency level characteristics, the coefficient of interest ($\beta_1$) should not be statistically distinguishable from 0. Tables A4 and Figures A4 and A5 display this test for NL growth 1 and 2 years prior to an election, the level of NL intensity a year prior to the election, election turnout percentages, constituency size and the reservation status of the constituency[26]. Table A6 and Figure A6 run the same test for the Scheduled Caste, Scheduled Tribe, rural and literate shares of the constituency's population, where the data comes from the last census conducted prior to the election[27].

Of the 12 characteristics for which a placebo test was conducted, only one (literate share)

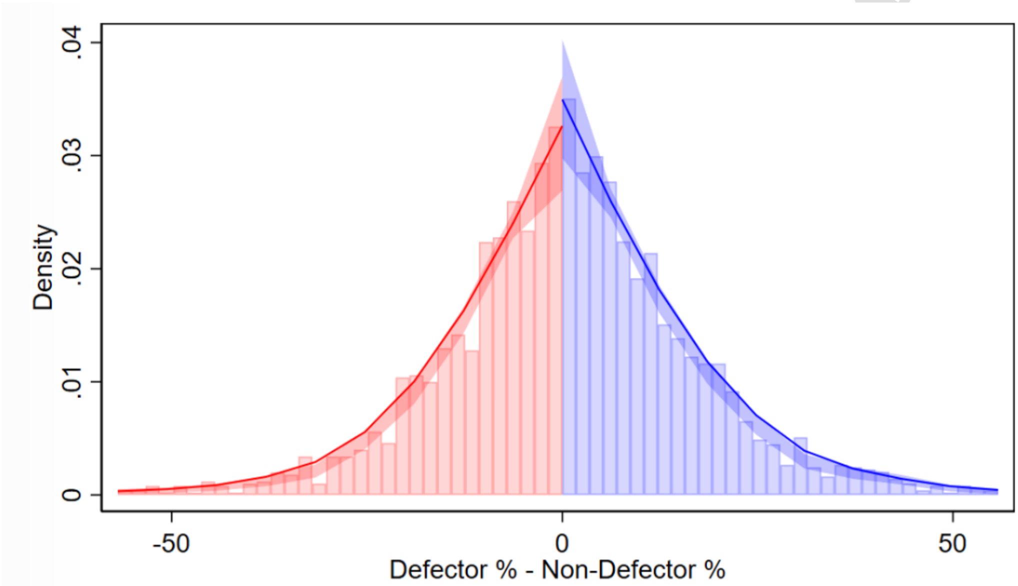
[24] The prevalence of electoral fraud and malpractice diminished significantly after election reforms enacted by T.N. Seshan- India's 10th Chief Election Commissioner (Vaishnav, 2017).

[25] All subsequent results in section 5 also use definition 2, unless explicitly noted otherwise.

[26] The number of bins in the placebo test RD plots are chosen using an algorithm (Calonico et al, 2015) that picks a number of equally spaced bins such that the variability in the binned data mimics the variability in the raw data.

[27] Since census data is available decennially, the estimates in Table A6 can be from up to 10 years before an election.

Figure 3. McCrary Density Test




*Notes:* The x-axis equals the margin of victory for an election in percentage points: the difference in vote shares attained by the top two candidates. Positive values represent defector victories, while negative values represent nondefector victories. The y-axis represents the density of elections for different margin of victory intervals. The curves on either side of the cutoff represent lines of best fit for non-parametric regressions of density on margin of victory. The discontinuity in density at the cutoff is not statistically significantly different from 0 (p = 0.43).

displays a statistically significant (p <.05) discontinuity at the cutoff, with literate share decreasing by 4.5 percentage points at the cutoff. Given that Figure A9 displays a negative association relationship between literate share and NL growth and given that I find a positive effect of defectors on NL growth, the discontinuous decrease in literate share at the cutoff has potential to bias my results upwards. Two factors mitigate the concern. First, even absent any true discontinuities, we would expect one out of every 20 regressions to show a significant result just due to random variation. Given that I run 12 regressions here, finding one statistically significant (p < .05) result is not surprising even absent a true effect. Second, we can see from Figure A6 (Panel D) that while there might be a mean difference in literacy rates at the cutoff, the effect is extremely noisy with no clear visual evidence of a discontinuity.

In addition to testing for continuity of constituency level characteristics, I test for continuity in the candidate-level characteristics of defectors and non-defectors at the 0% cutoff. Rather than verifying random assignment of defectors to different constituencies, this test is measuring whether defectors and non-defectors who contest close elections differ from each other in their demographic and political characteristics. Discontinuities in these characteristics at the cutoff could imply that an effect of defectors on growth rates is driven by these characteristics rather than defector status itself. I run the candidate-level continuity check for 8 characteristics: Gender, SC status, ST status, education, age, assets, criminal status and incumbency status (Table A5, Figures A7, A8). Of these characteristics, only SC status varies discontinuously at the cutoff, with defector winners being 6.3pp more likely to be members of scheduled castes than non-defectors. Moreover, in close elections between defectors and non-defectors (margins of victory $\leq 2$, 3 or 5%) constituencies which elect SC politicians have consistently higher NL growth than constituencies which elect non-SC politicians (Table A7). While the statistical significance of this difference depends on the precise margin of victory we use and how we restrict the sample, the direction of the effect is robust. Despite this, we can be confident that the results in section 5.2 are not driven by defectors' disproportionate SC status for 3 reasons. First, elections with SC winners comprise just under 4% of elections in our sample, and it's unlikely a-priori that such a small subset of the sample drives our results. Second, the main results are robust controlling for having an SC winner (Tables 3, 4: Cols 1 vs 2). Finally, the main results are robust to excluding all elections with SC winners (Online Appendix Table B8). Therefore, it is highly unlikely that any effects of defectors on NL growth are driven by their disproportionate SC status. Although these continuity tests do not rule out unobserved co-variates of defector status driving the result, they do reduce the share of the treatment effect that could be explained by such unobservables.

### 5.2. Night Light Results

When using the NL $\geq 2$ sample, no controls and a CER bandwidth, the RD estimator from the local linear specification in equation 1 yields a statistically significant ($p < 0.01$) estimate of electing a defector increasing a constituency's night light growth by 4.6 percentage points (column 1, Table 3). This estimate is robust to the inclusion of candidate controls, constituency-level controls, NL level and growth lags, state and year fixed effects, and to using the bandwidth from the MSE algorithm. Estimates range from 3.5 to 4.8pp depending on the specification, and remain statistically significant

Table 3. RD Estimates of defector on NL growth, NL $\geq 2$ sample

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| RD Estimate | 4.59*** | 4.31*** | 4.69*** | 3.53** | 4.47*** | 4.01*** | 4.84*** | 3.92*** |
| | (1.52) | (1.36) | (1.74) | (1.38) | (1.31) | (1.16) | (1.53) | (1.23) |
| Bandwidth Algorithm | CER | CER | CER | CER | MSE | MSE | MSE | MSE |
| Bandwidth (%) | 6.30 | 8.02 | 7.08 | 6.91 | 9.23 | 11.7 | 10.1 | 9.90 |
| Obs Left | 1389 | 1667 | 1143 | 1128 | 1804 | 2169 | 1478 | 1437 |
| Obs Right | 1456 | 1703 | 1136 | 1112 | 1897 | 2259 | 1458 | 1443 |
| Robust Standard Error | 1.59 | 1.42 | 1.85 | 1.47 | 1.44 | 1.28 | 1.72 | 1.37 |
| Candidate Controls | N | Y | Y | Y | N | Y | Y | Y |
| Constituency Controls | N | N | Y | Y | N | N | Y | Y |
| NL Level and Growth Lags | N | N | Y | Y | N | N | Y | Y |
| FE | N | N | N | Y | N | N | N | Y |

*Notes*: RD estimate equals coefficient $\beta_3$ from eqn 1. Eqn 1 estimates a local regression discontinuity design with margin of victory running variable, slope allowed to vary on both sides of the cutoff, and bandwidth determined by an optimal bandwidth algorithm.
Obs left & right refer to the number of observations with defector losers and defector winners respectively.
Polynomial Order: 1
Standard Errors in parentheses and clustered at the election level.
* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 4. RD Estimates of defector on NL growth: Min NL per election $\geq 2$

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| RD Estimate | 2.10** | 1.92** | 1.74* | 1.28* | 2.28*** | 2.19*** | 1.76** | 1.66*** |
| | (0.94) | (0.91) | (1.01) | (0.70) | (0.81) | (0.78) | (0.89) | (0.62) |
| Bandwidth Algorithm | CER | CER | CER | CER | MSE | MSE | MSE | MSE |
| Bandwidth (%) | 7.64 | 7.63 | 7.55 | 7.38 | 11.1 | 11.1 | 10.7 | 10.5 |
| Obs Left | 1461 | 1451 | 1091 | 1081 | 1919 | 1910 | 1420 | 1383 |
| Obs Right | 1474 | 1466 | 1039 | 1023 | 1918 | 1902 | 1326 | 1289 |
| Robust Standard Error | 0.98 | 0.95 | 1.08 | 0.76 | 0.89 | 0.86 | 1.01 | 0.72 |
| Candidate Controls | N | Y | Y | Y | N | Y | Y | Y |
| Constituency Controls | N | N | Y | Y | N | N | Y | Y |
| NL Level and Growth Lags | N | N | Y | Y | N | N | Y | Y |
| FE | N | N | N | Y | N | N | N | Y |

*Notes*: RD estimate equals coefficient $\beta_3$ from eqn 1. Eqn 1 estimates a local regression discontinuity design with margin of victory running variable, slope allowed to vary on both sides of the cutoff, and bandwidth determined by an optimal bandwidth algorithm.
Obs left & right refer to the number of observations with defector losers and defector winners respectively.
Polynomial Order: 1
Standard Errors in parentheses and clustered at the election level.
* $p<0.10$, ** $p<0.05$, *** $p<0.01$

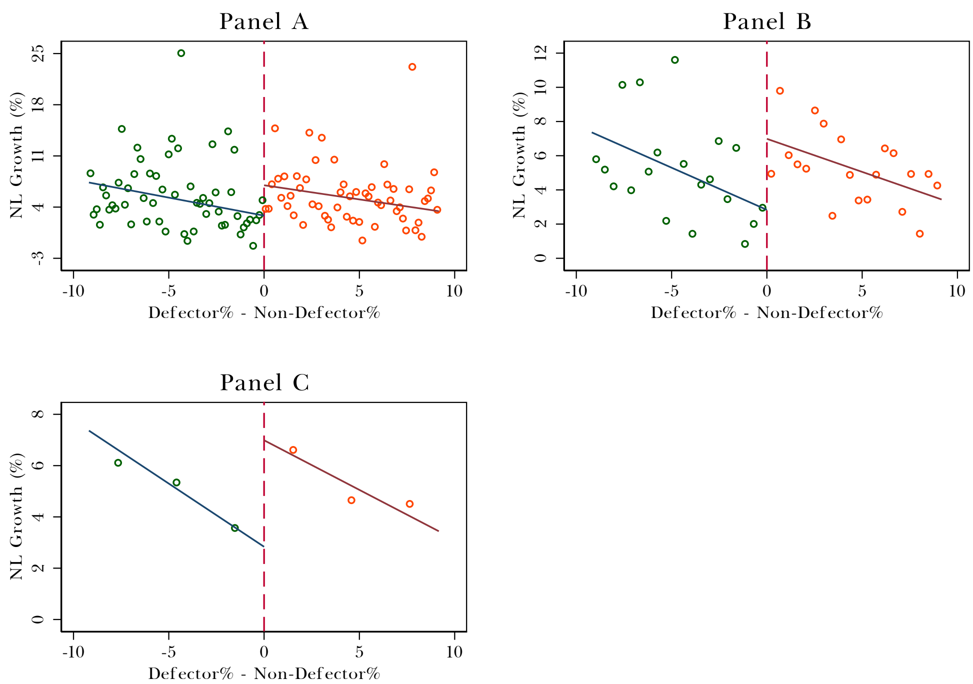
for each of of eight specifications. If, instead, we restrict to election with minimum NL $\geq 2$, we get estimates ranging from 1.3 to 2.3pp. The estimates remain significant at between 0.05 for all but specifications 3 and 4 with the CER bandwidth (where p $<$ 0.1). Using an elasticity of 0.1 of GDP growth with respect to night-light growth estimated at the district level for India (Bickenbach et al., 2016), this translates to a 0.13-0.48 percentage point increase in annual GDP growth associated with electing a defector depending on which sample restriction I use. Relative to an average GDP growth rate of 6.4% over the same time period, this implies a 2-7.5% increase in annual GDP growth over baseline. For context of the magnitude, the effect of defectors on NL growth here is similar to the effect of electing a member of the ruling party (Asher and Novosad, 2017) (∼4pp) and much larger than the effect of electing a dynastic politician (George and Ponattu, 2019) (∼ -0.4 pp) , but significantly smaller than the effects of electing a criminal politician (Prakash et al., 2019))(∼ -25pp) or a female politician (Baskaran et al., 2018)(∼ 15pp).

Figure 4 presents visual evidence of the discontinuity using binned scatterplots with three different bin widths[28]. Plots with different bin widths are included to reflect the tradeoff between representing the variability in the raw data and making the relationship between the running and dependent variables visually observable. Encouragingly, there is clear evidence of a discontinuity at 0 in all three plots, though (as expected) the fewer bins used, the cleaner the relationship looks.

Note that there is a negative relationship between defector vote-share and NL growth on both sides of the cutoff. This indicates that the higher the defector's vote share, the lower the constituency's NL growth, on average. Although this is a plausible direction for the correlation between defector vote share and growth, there isn't a strong a-priori reason to believe that the sign couldn't be 0, uniformly positive or differ on either side of the cutoff. Therefore, I impose a slope of 0 to check the estimate's sensitivity to the negative slope displayed in Figure 4. The coefficients are uniformly statistically significant ($p < .05$) and have a very similar range of magnitudes to the linear polynomial specifications (Online Appendix Table B5). In order to further check the estimates' sensitivity to the functional form of the running variable, I estimate the same specifications from Table 3, but with margin entering as a quadratic rather than a linear term (Online Appendix Table B6). The estimates are very similar, if slightly larger and more significant (p always $< 0.01$), than the linear specification, with the effect of electing defectors on NL growth ranging from 4.2pp to 5.3pp when we restrict NL $\geq 2$ and 1.9-2.6 when we require min NL/Election $\geq 2$.

[28] In order to minimize discretion, I use bandwidths from two different optimal bandwidths from Calonico et al. (2015) and a bin width which has two bins for each percentage point of margin of victory.

Figure 4. NL Growth vs Margin of Victory




*Notes:* The x-axis equals the margin of victory for an election in percentage points: the difference in vote shares attained by the top two candidates. Positive values represent defector victories, while negative values represent non-defector victories. Each marker represents the mean value of NL Growth at the midpoint of a margin of victory bin. Each bin has equal size (in terms of margin of victory, but not number of observations). The number of bins is chosen using the mimicking variance algorithm for panel A, the IMSE algorithm for panel C and 2 bins per percentage point of margin of victory for panel B . The lines either side of the cutoff represent the linear line of best fit for the binned data. The bandwidth for determining which elections are included is determined by the MSE algorithm. Observations are restricted to constituency-years where the minimum NL intensity in the constituency is $\geq 2$.

To ensure that these results are not driven by an idiosyncratic choice of minimum NL intensity, Online Appendix tables B1-B4 report results from the same regression specifications as in Tables 3 and 4, but using minimum NL intensity/min NL Intensity per election sample restrictions of 1 and 3 rather than 2. Note that we should expect marginally lower magnitudes and lower statistical significance for the (min) NL intensity $\geq 1$ restriction because of the lower elasticity of GDP w.r.t NL intensity and the high variance in NL growth caused by the extreme NL growth outliers at low NL intensity levels. The results are largely consistent with both the main results in Tables 3 and 4, and the hypothesis above. Specifically, the treatment effect is similar, if slightly smaller (51-96% of original) in magnitude, and is uniformly highly statistically significant at when using the higher ($\geq$ 3) NL thresholds (Online Appendix Tables B1, B2). In contrast, the treatment effect is significantly smaller in magnitude (20-75% of original) and substantially less likely to be statistically significant at $p < 0.05$ (6/16 specifications) for the lower NL thresholds (Online Appendix Tables B1, B2). In particular, the treatment effect estimates are smaller, but still usually statistically significant when using the NL intensity $\geq 1$ restriction (Online Appendix Table B3), but lose statistical significance when using the minimum NL/election $\geq 1$ restriction (Online Appendix Table B4).

Note that throughout this section, the results reflect the causal effect of electing a defector rather than the causal effect of defector status. That is, the results above could be driven both by a candidate's defector status, or other traits (apart from the candidate controls) that are correlated with defector status. Therefore, the next section seeks to understand the mechanisms by which defectors increase economic growth.

### 5.3. Mechanism Testing

#### 5.3.1. Time Invariant Traits vs Time Varying Traits vs Rewards for Defection

Contingent on there being an effect of defectors on NL growth, it could operate through the three distinct channels discussed in section 1- time invariant defector characteristics, changes to defector traits/capabilities *caused by defection* (rewards for defection), and changes to defector traits that *cause defection* (e.g. recruiting competent defectors). To test the time-invariant characteristics model, I estimate equation 1 using definition 1 of defector and exclude the election in which the politician defected. If we think time invariant-characteristics of defectors are driving the results, then they should persist even in elections before and after the defection. Since the measurement errors mentioned in section 3.1 may bias the results downwards (in magnitude) through attenuation bias, I estimate additional specifications where the measurement error is more limited. In particular, I restrict the sample to include only those non-defectors in my sample who contested their first election 15 or 20 years ago, and therefore are likely to have already defected if they were going to. I also estimate additional specifications where I exclude all elections in years post 2000 or 2005 to ensure that there have been enough years since an election for a politician to defect in the future and hence be counted as a defector.

The results in Table 5 indicate that the time-invariant characteristics hypothesis is unlikely to hold. Across all specifications, there is no statistically significant positive effect of electing defectors on NL growth. Moreover, the full-sample specification has a negative coefficient, significant at $p < 0.1$. Therefore, insofar as there is attenuation bias from mis-measuring defector status, we would expect the unbiased coefficient to be *even more negative* than the results below.

Table 5. RD Estimates of defector on NL growth: Definition 1

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| RD Estimate | -1.39* | 0.038 | 0.57 | 0.15 | -0.16 | -1.48* |
| | (0.74) | (0.75) | (0.75) | (0.83) | (0.75) | (0.77) |
| Bandwidth Algorithm | MSE | MSE | MSE | MSE | MSE | MSE |
| Bandwidth (%) | 12.6 | 14.0 | 8.87 | 10.6 | 11.6 | 11.6 |
| Obs Left | 1949 | 910 | 1185 | 917 | 1440 | 1857 |
| Obs Right | 2479 | 1123 | 1500 | 1055 | 1805 | 2282 |
| Robust Standard Error | 0.87 | 0.87 | 0.87 | 0.98 | 0.88 | 0.90 |
| Max election year | None | 2000 | 2005 | None | None | None |
| Min years in politics | None | None | None | 20 | 15 | 10 |

*Notes*: RD estimate equals coefficient $\beta_3$ from eqn 1. Eqn 1 estimates a local regression discontinuity design with margin of victory running variable, slope allowed to vary on both sides of the cutoff, and bandwidth determined by an optimal bandwidth algorithm.
Obs left & right refer to the number of observations with defector losers and defector winners respectively.
Polynomial Order: 1
Sample Restriction: Min NL per Election $\geq 2$
Definition 1 classifies a politician as a defector if they've ever switched parties in their career.
Controls for candidate characteristics and constituency characteristics included
Standard Errors in parentheses and clustered at the election level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

I test the rewards for defection hypothesis in two ways. First, I provide descriptive evidence regarding an especially important reward - a ministerial position in the state government. In addition to having input into legislation in their domains, ministers are the executives responsible for policy implementation in their areas. Moreover, the ministers' control over policy in their particular domain may give them leverage to demand changes in program implementation for policies officially under ministries that they don't control. Therefore, rewards in the form of ministerial positions not only provide defectors professional advancement and high status, but also a generalised mechanism through which a politician can use state power and influence policy to benefit their constituents. Importantly, the phenomenon of powerful politicians using their political influence to favor their constituents isn't just a theoretical possibility. Khalil et al. (2021) show that shows that Indian state legislative constituencies from which a Chief Minister is elected experience faster NL intensity growth than other constituencies in the state during their tenure in office.

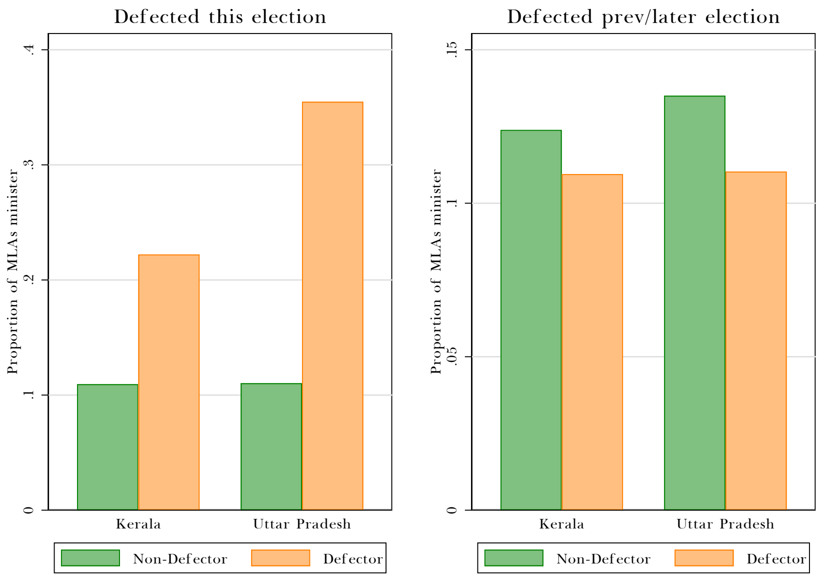
Consequently, the rewards explanation is strengthened by the evidence from Figure 5, which shows the proportion of MLAs from the ruling party who hold ministerial positions in the government in two states- Kerala, 2001-2006 and Uttar Pradesh (UP), 2012-2017[29]-by defector status. Defector MLAs are approximately twice as likely as non-defector MLAs to be given ministerial roles in Kerala, and more than thrice as likely in Uttar Pradesh in the election cycle in which they defect (definition 2) (Figure 5). These differences are highly statistically significant for the pooled sample of both states and for just UP ($p < 0.01$), and even marginally significant for just Kerala ($p = 0.1$). Moreover, this difference is not driven by time-invariant defector characteristics, since defectors who defected in previous or later election cycles (definition 1) are 2.4pp less likely to be ministers than non-defector MLAs ($p$ <0.01, pooled sample).

Note, however, that the above correlation is still theoretically compatible with a non-causal relationship between defecting and holding ministerial positions where parties try to induce defection precisely at the same time that the defectors' previous parties would have offered them ministerial positions had they not change parties and had their prior party won power. This could be the case if parties recruit defectors who are rising in influence within their previous parties or in popularity with the electorate. For example, since parties are disproportionately likely to promote politically popular candidates (Folke et al., 2016; Meriläinen and Tukiainen, 2018), the above pattern could be explained by parties recruiting politicians who they predict will be especially popular in the subsequent election conditional on the previous electoral performance. Importantly, this explanation only fits the data if we assume that these rising political stars lose their positive traits in

[29]There doesn't exist a dataset tracking the members of cabinets of state governments in India. Therefore, I manually assemble the dataset for these two states for the aforementioned election cycles. These two states are chosen so that I have data from states in different regions of the country and with high and low development levels. The time periods are chosen in order to have a large enough number of ruling party defector MLAs so as to avoid making inferences using very small samples (for ex, some elections have just 2 defector MLAs in the ruling party).

the elections post defection since they're no longer more likely to be ministers in elections after the one where they defected. In summary, although substantially increased likelihood of holding a ministerial position provides a *potentially* tangible mechanism through which defectors increase economic activity, further evidence is required on whether or not defectors actually utilise their positions in this way.

Figure 5. Proportion of MLAs who are Minister, by Defector Status




*Notes:* Ministerial positions include both cabinet ministers and ministers of state. Kerala data for 2001-06 assembly. Uttar Pradesh data for 2012-17 assembly. Definition 2 of defector is used here.

For the second test of the rewards hypothesis, I estimate whether electing defectors has a differential effect on NL growth depending on whether or not the defector belongs to the state's governing party. Since governing parties are better able to reward defecting politicians with state resources and positions of power than opposition parties, we should expect returns to electing a defector to be high for defectors who belong to the party in power in the election cycle in which they defected and low (or potentially zero) for defectors who belong to the opposition party. I formally test this hypothesis by estimating equation 1 using definition 2 of defector for two samples: the first contains only those elections in which the winner belonged to the ruling coalition, while the second contains only those elections in which the winner belonged to the opposition coalition.

Table 6 does not provide definitive evidence in favor of the rewards for defection theory. Governning party defectors have a statistically significant (p-val from $< 0.1$ to $< 0.01$) effect on NL growth of a similar magnitude to the main results in each of the four specifications. In contrast, opposition party defectors only have a marginally statistically significant ($p < 0.1$) effect in the no-control specification and a null effect in the others. However, note that the effect sizes for the opposition defectors regression are very similar to those for from those for government defectors, and that the coefficients for both samples fall within each others' 95% confidence intervals. Moreover, the null-effect for opposition defectors isn't robust to using the NL $\geq 2$ restriction (Online Appendix Table B7) rather than the Min NL/Election $\geq 2$ restriction, or to using inter-calibrated night light data. In sum, we can't reject the hypothesis that government and opposition defectors have similar effects on NL growth. Importantly, the failure to find a difference between the effect of government and opposition defectors doesn't reflect a precise estimate of no difference, but rather imprecision and an inability to confidently say which effect (if either) is larger.

In aggregate, these two tests fail to strongly distinguish between the two ways time-varying traits could boost growth, and are compatible with three possible explanations for the positive aggregate effect of defectors. First, that the treatment effect is truly equal across government and opposition defectors, and is entirely driven by time-varying traits of defectors but not by their defector status itself. Second, that rewards for defection explain the entirety of the treatment effect, and our inability to reject a difference in the size of government and opposition effects is because of imprecision. Under this model, opposition parties can reward their defector politicians, to a positive degree, but less effectively than government parties. Third, that the effect of electing defectors is a linear combination of the rewards for defection and other time varying candidate traits, and consequently, that a more precise estimate would show a larger effect for government than opposition defectors. Under this model, the effect for opposition defectors is explained by the the time-varying candidate traits, while the effect for governing defectors is caused by both rewards and time-varying traits.

Explanation 1 has the benefit of fitting well with the fact that opposition defectors have a positive effect of a similar magnitude to government defectors, but has the cost that of the un-intuitive assumption that these time-varying characteristics last for just one election. In contrast, explanations 2 and 3 suffer the obvious cost of having to assume that more precise treatment effect would reveal a difference between government and opposition effects without empirical demonstration. However, both explanations 2 and 3 have the benefit of providing a parsimonious and intuitive explanation of the dual findings of a striking increase in the probability of being a minister on defection and the positive treatment of defection on NL growth.

Table 6. RD Estimates of defector on NL growth, by coalition membership. Min NL/Election $\geq 2$

| | Government Winner | | | | Opposition Winner | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| RD Estimate | 2.40** | 2.73*** | 1.93* | 1.98** | 2.22* | 1.51 | 1.89 | 1.51 |
| | (1.06) | (1.05) | (1.14) | (0.81) | (1.31) | (1.29) | (1.45) | (1.02) |
| Bandwidth Algorithm | MSE | MSE | MSE | MSE | MSE | MSE | MSE | MSE |
| Bandwidth (%) | 14.7 | 14.0 | 13.5 | 10.9 | 8.63 | 8.46 | 7.83 | 5.65 |
| Obs Left | 1347 | 1299 | 1037 | 913 | 669 | 662 | 428 | 342 |
| Obs Right | 1137 | 1087 | 758 | 641 | 825 | 814 | 556 | 478 |
| Robust Standard Error | 1.22 | 1.22 | 1.36 | 0.97 | 1.55 | 1.53 | 1.68 | 1.14 |
| Candidate Controls | N | Y | Y | Y | N | Y | Y | Y |
| Constituency Controls | N | N | Y | Y | N | N | Y | Y |
| NL Level and Growth Lags | N | N | Y | Y | N | N | Y | Y |
| FE | N | N | N | Y | N | N | N | Y |

*Notes*: RD estimate equals coefficient $\beta_3$ from eqn 1. Eqn 1 estimates a local regression discontinuity design with margin of victory running variable, slope allowed to vary on both sides of the cutoff, and bandwidth determined by an optimal bandwidth algorithm.
Obs left & right refer to the number of observations with defector losers and defector winners respectively.
Polynomial Order: 1
Sample Restriction: Min NL per Election $\geq 2$. Government Winner columns only contain elections where the winner ended up in the governing party, while Opposition Winner columns only contain elections where the winner ended up in the opposition party.
Standard Errors in parentheses and clustered at the election level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

#### 5.3.2. Policies and Public Goods

Regardless of whether the causal effect of defector reflects the causal effect of the defector characteristic, or a time-varying selection effect, there still remains the questions of what precise policy levers defectors use to increase economic activity in their constituencies. In order to provide a more granular explanation for the positive causal effect, I estimate the effect of ruling party defectors on a set of public goods- kilometers of roads built under the PMGSY scheme, the proportion of constituents with access to roads and electricity and the number of schools in an electoral constituency. These could be examples of rewards that state governments provide defectors, policies that defectors can influence via their ministerial positions, or simply public goods which are better provided by highly politically connected and influential politicians. I estimate the effects of defectors on these public goods using two empirical strategies (detailed in section Appendix B.2.1 that adapt the close election design to the decennial census data. First, an instrumental variables strategy that uses the number of close elections defectors win (conditional on the number of close elections they contest) as an instrumental variables for the number of years between 2001 and 2011 a constituency had a defector MLA. Second a close election RD design that uses only the first election in each constituency after the 2001 census. Both empirical strategies from section Appendix B.2.1 have potential shortfalls. The IV strategy assumes random assignment at margins where we aren't *certain* that it holds, while the sharp RD strategy requires extrapolation from large to small margins of victory and may suffer from attenuation. Therefore, rather than focusing on any one specification, I test whether both yield results of similar signs and magnitudes.

Unfortunately, the effects of all three public goods are imprecisely estimated. The magnitude and significance, and even the sign of the estimates vary depending on the empirical strategy used, the inclusion or not of controls, and the inclusion or not of outlier observations. For example, while in some specifications defector have a statistically significant positive effect on access to tar-roads or the number of schools in their constituencies, in others the effect has the opposite sign or is positive but not statistically significant (Online Appendix Tables C2-C5).[30] That is, while I don't estimate a precise 0 effect of defectors on any of the goods, nor can I consistently estimate a statistically significant non-zero effect of roughly constant magnitude.

There are two reasons why this result is compatible with the both the overall growth effects. First, the econometric challenges that come from the decennial nature of the census data as well as the extreme levels of missingness for the public goods I tracked mean that I'm much less likely to consistently pick up an effect when compared to the NL regressions. Second, given that I study a limited subset of the many policies and public goods implemented and provided by the Indian state, the lack of an effect of defectors on any one of them should not be surprising. This is especially

[30] See Online Appendix Section C for the regression results from the different specifications.

true since we need not expect all defectors to use their influence to effect the same goods. Other policies could involve boosting private sector economic activity through easing regulatory burdens[31] and increased provision of credit from public sector banks, or through improved access to India's national rural employment guarantee program.

## 6. Discussion & Conclusion

In summary, this paper finds clear evidence that electing defectors boosts economic activity in their electoral constituencies. Moreover, it strongly supports the hypothesis that this effect occurs exclusively in the elections in which defection occurs, and is not driven by time-invariant differences between defectors and non-defectors. Regarding specific mechanisms, I find that defectors are disproportionately likely to become ministers in state government exclusively in the elections in which they defect. Ministerial positions and the power and influence they entail provide defectors with a broadly applicable lever to benefit their constituents and boost economic activity in their electoral constituencies. However, my empirical tests are inconclusive as to whether defectors increase economic growth because of factors caused by defection (e.g. being rewarded for defection) or selection effects related to defection (e.g. recruitment of increasingly politically connected politicians). This inconclusiveness prevents confident conclusions on whether the aforementioned minister positions actually *cause* the increased growth, or are simply a correlate of defection.

Regardless of the specific mechanism by which defectors increase growth, the fact that they do may help explain why defectors aren't punished at the ballot box, and help demonstrate the importance of local legislators to their constituent's welfare. This influence of local politicians provides an explanation for the importance candidate demographic characteristics (conditional on party) in Indian state legislative elections despite legislators' limited roles in actually writing legislation. That is, having an MLA from one's caste or religious in-group can be highly valuable to constituents even if the MLA has little influence over legislation since the MLA has substantial influence over local economic activity and policy implementation. Finally,, the significant influence

[31] This channel is plausible since Asher and Novosad (2017) find that electing a member of the governing party increases economic activity in their constituency through the increased granting of mining licenses.

of state legislators on economic outcomes beyond their roles as lawmakers documented in this paper reveals a neglected avenue for academic research aiming to improving state performance and intervention efficacy in India and other developing countries. In particular, ensuring the cooperation and participation of local politicians may be of crucial importance in the successful implementation of policy reform in their constituencies. Failure to obtain local politician buy-in may severely hamper implementation even if the politician nominally voted to approve the policy, given the highly centralized legislative process. Therefore, seeking local politician cooperation in intervention implementation could be a fruitful addition to future research testing policy changes in concert with state governments.

## Data Availability

The data underlying this article are available in Open ICPSR. The link will be made available after all edits have been made in the review process. The linked material include the raw data from TCPD, DDL and my original data as well as the Stata code to clean the data and produce the results.

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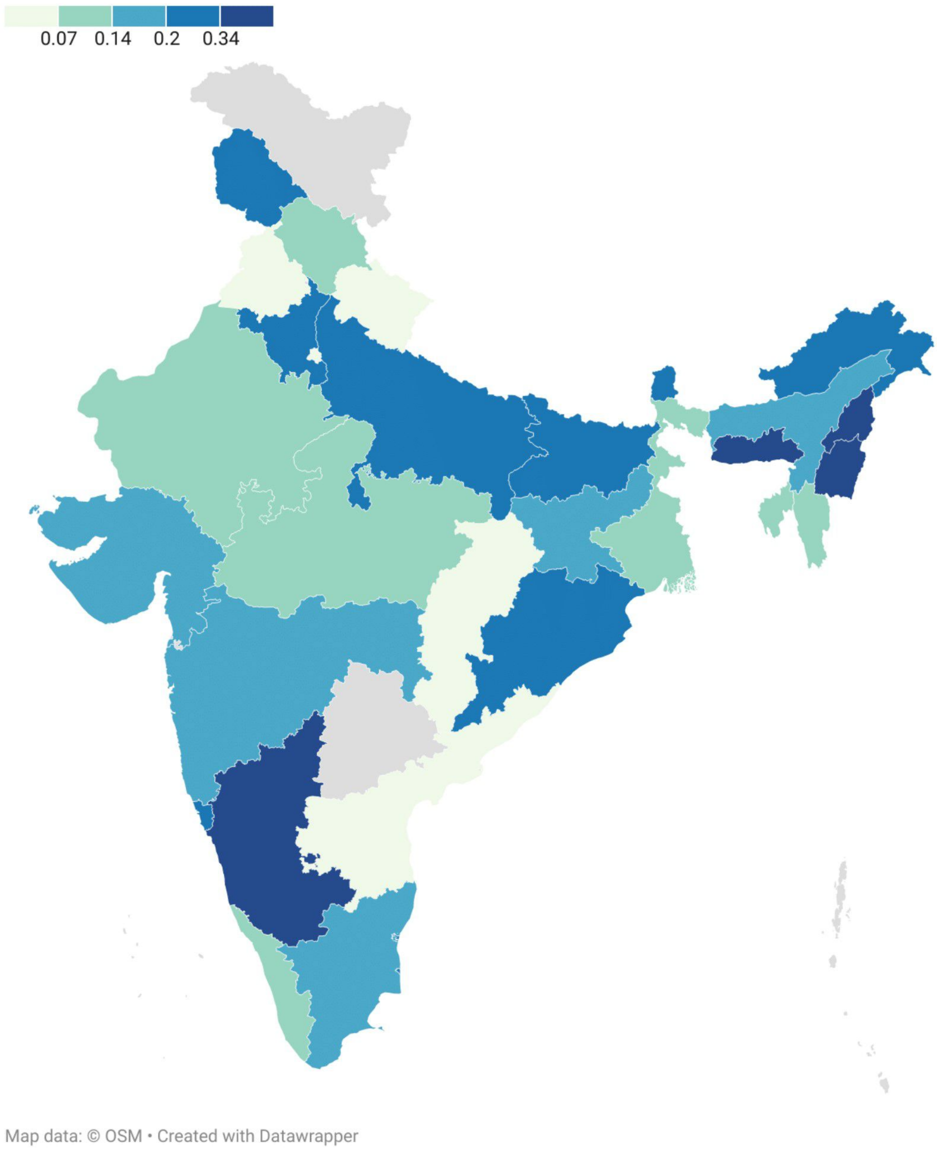
## Appendix A. Appendix

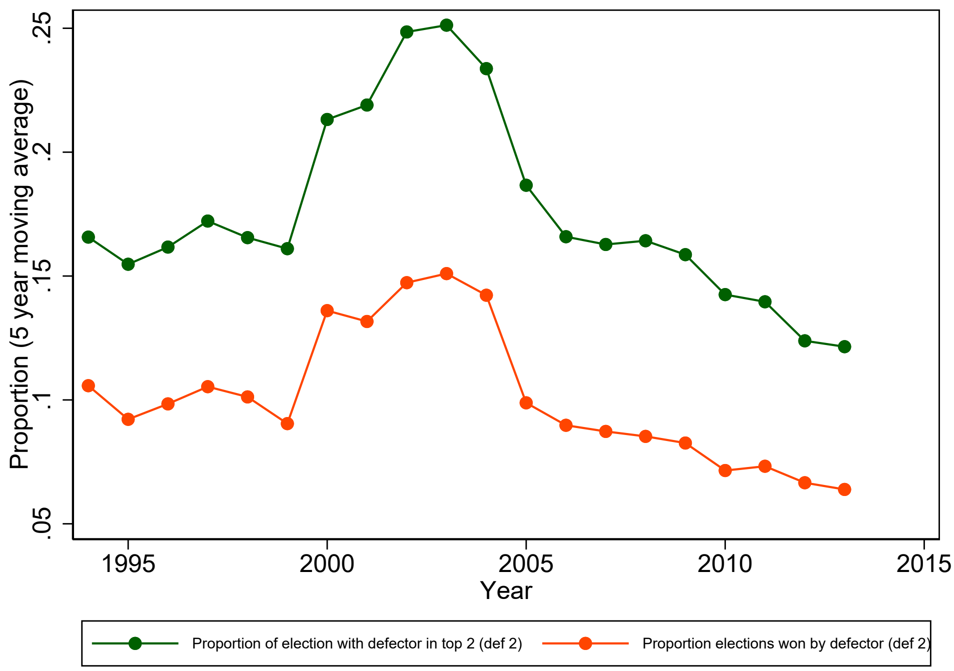
### Appendix A.1. Additional Tables and Figures

Figure A1. Share of elections with defector in top 2 by state: 1994-2013 (definition 2)



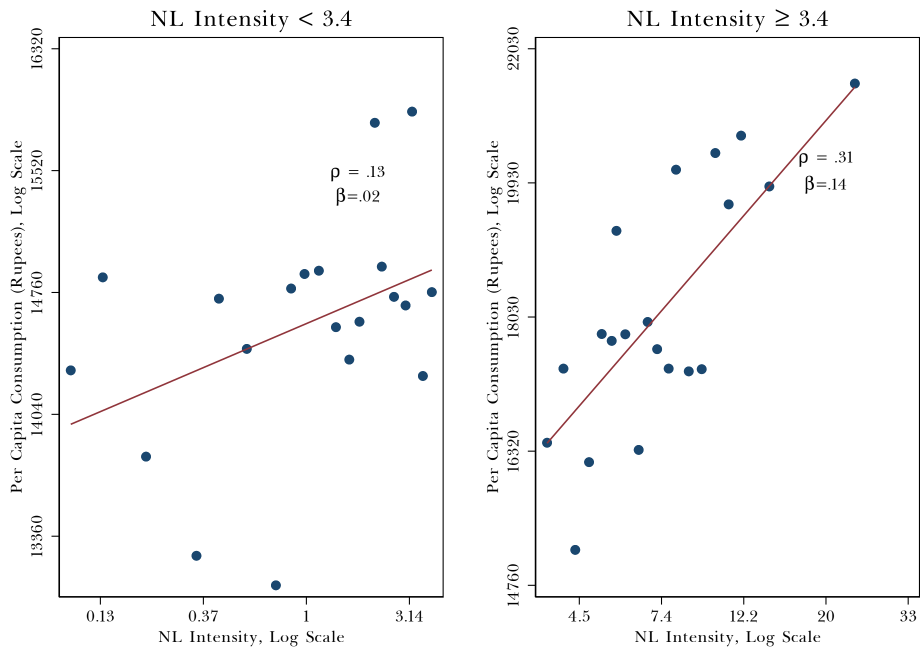

*Notes:* There is no value for Telangana because the state was formed in 2014.
Definition 2 classifies a politician as a defector for only the election in which they switched parties.

Figure A2. Share of elections with defector in top 2/won by defector: 1994-2013 (definition 2)



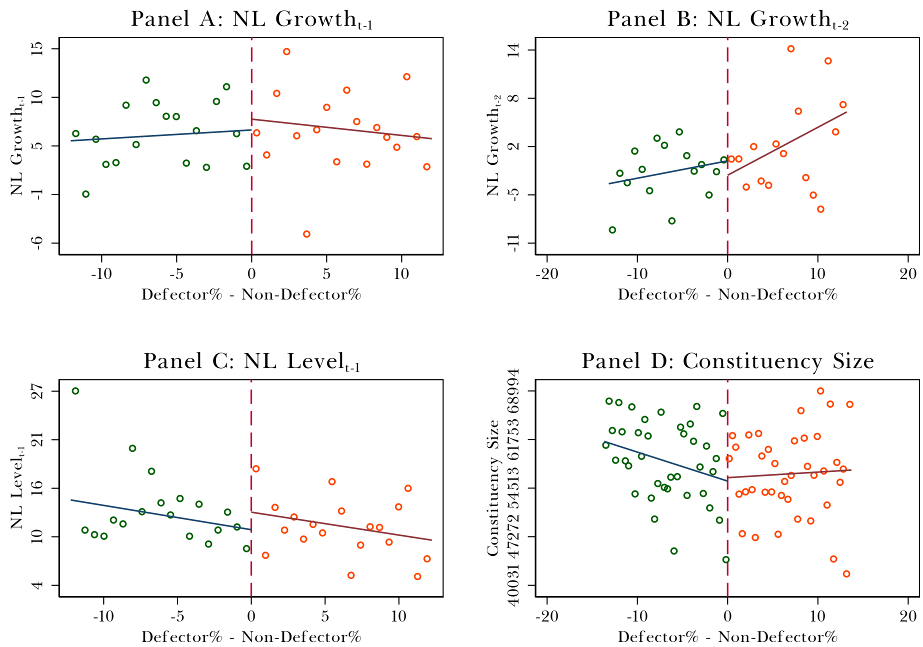

*Notes:* Definition 2 classifies a politician as a defector for only the election in which they switched parties.

Figure A3. Per Capita Consumption vs NL Intensity, by NL Threshold



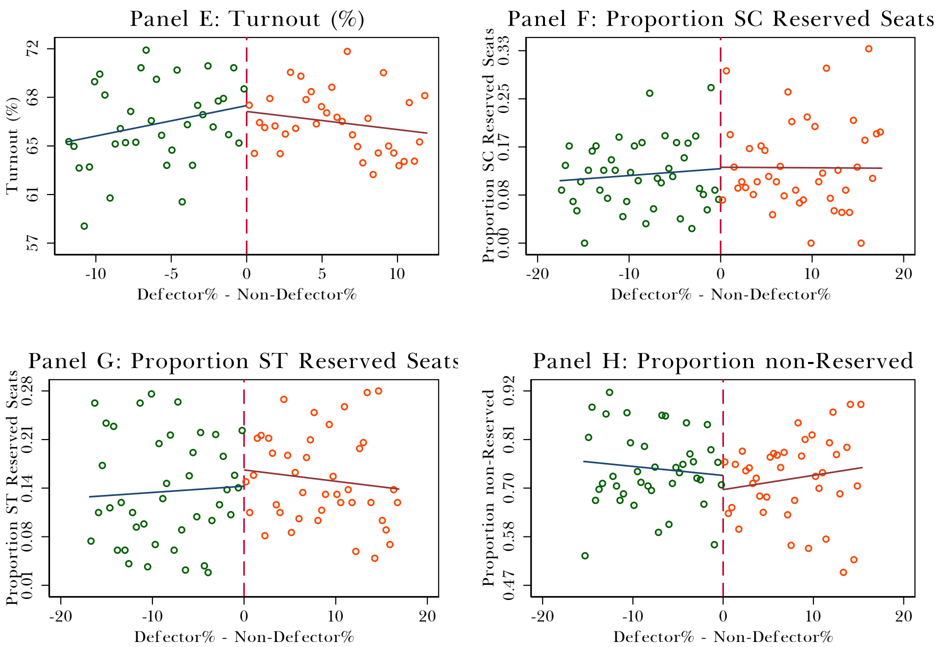

*Notes*: Per-capita consumption data is available only for 2011. Therefore, we also restrict NL data to 2011. Asher et al. construct this data by imputing consumption in the 2011 Socio-Economic and Caste Census from assets and earnings data.
The threshold of 3.4 is picked since it's the mean NL level for the sample of all constituency-year observations in the sample of elections with 1 defector and 1 non-defector.

Figure A4. Constituency Level Placebo Checks 1



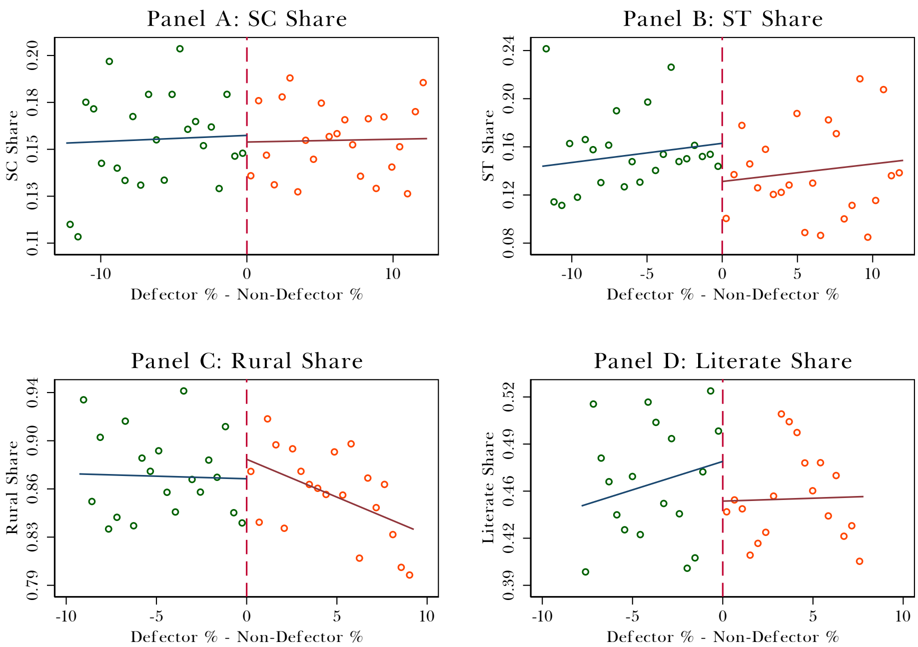

*Notes:* The x-axis equals the margin of victory for an election in percentage points: the difference in vote shares attained by the top two candidates. Positive values represent defector victories, while negative values represent nondefector victories. Each marker represents the mean value of the y-variable at the midpoint of a margin of victory bin. Each bin has equal size (in terms of margin of victory) and the number of bins is chosen using the mimicking variance algorithm. The lines either side of the cutoff represent the linear line of best fit for the binned data. The bandwidth for determining which elections are included is determined by the MSE algorithm.

Figure A5. Constituency Level Placebo Checks 2



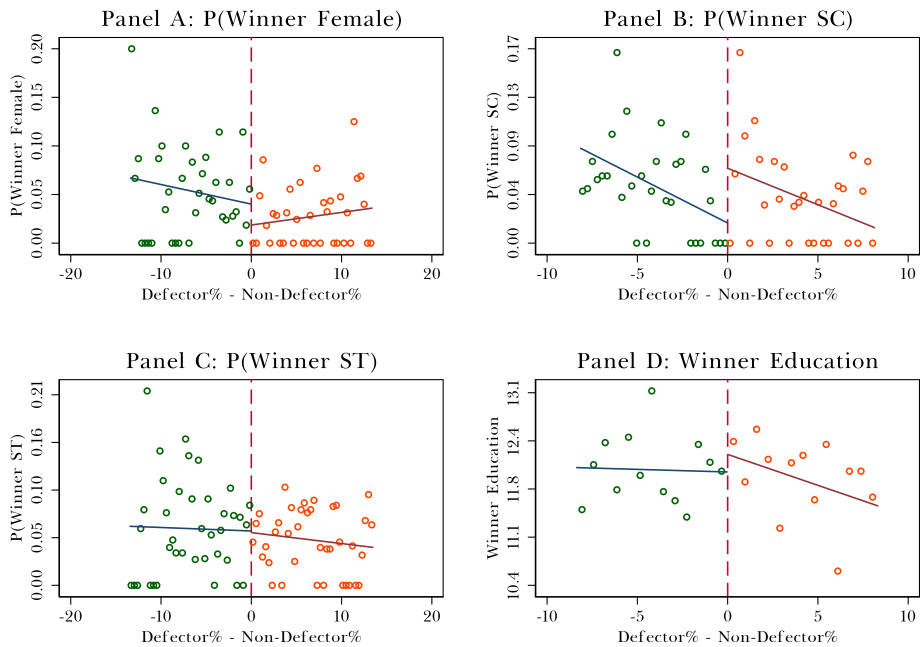

*Notes:* The x-axis equals the margin of victory for an election in percentage points: the difference in vote shares attained by the top two candidates. Positive values represent defector victories, while negative values represent nondefector victories. Each marker represents the mean value of the y-variable at the midpoint of a margin of victory bin. Each bin has equal size (in terms of margin of victory) and the number of bins is chosen using the mimicking variance algorithm. The lines either side of the cutoff represent the linear line of best fit for the binned data. The bandwidth for determining which elections are included is determined by the MSE algorithm.

Figure A6. Constituency Level Placebo Checks 3



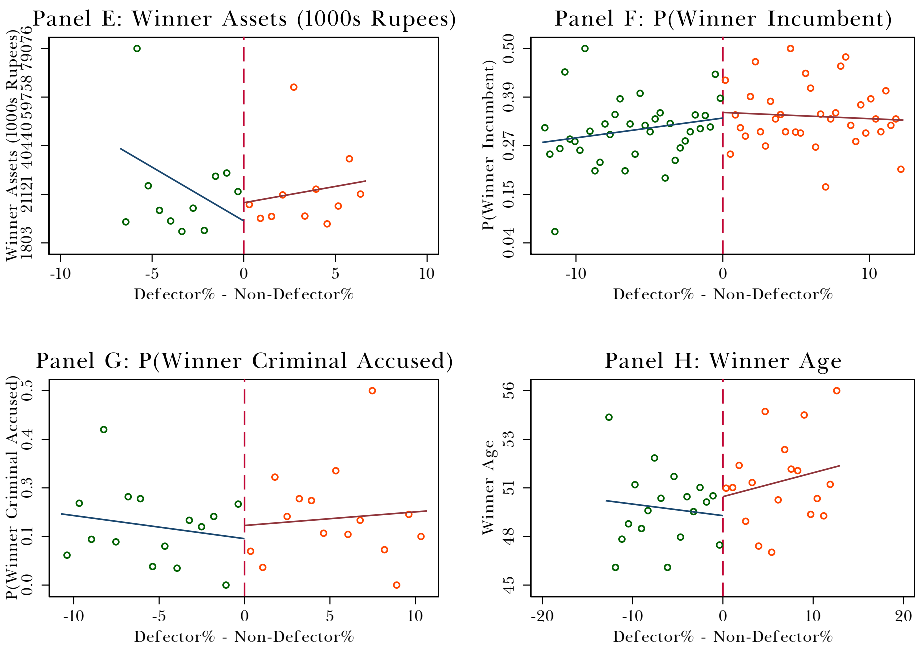

*Notes:* The x-axis equals the margin of victory for an election in percentage points: the difference in vote shares attained by the top two candidates. Positive values represent defector victories, while negative values represent nondefector victories. Each marker represents the mean value of the y-variable at the midpoint of a margin of victory bin. Each bin has equal size (in terms of margin of victory) and the number of bins is chosen using the mimicking variance algorithm. The lines either side of the cutoff represent the linear line of best fit for the binned data. The bandwidth for determining which elections are included is determined by the MSE algorithm.

Figure A7. Person Level Placebo Checks 1



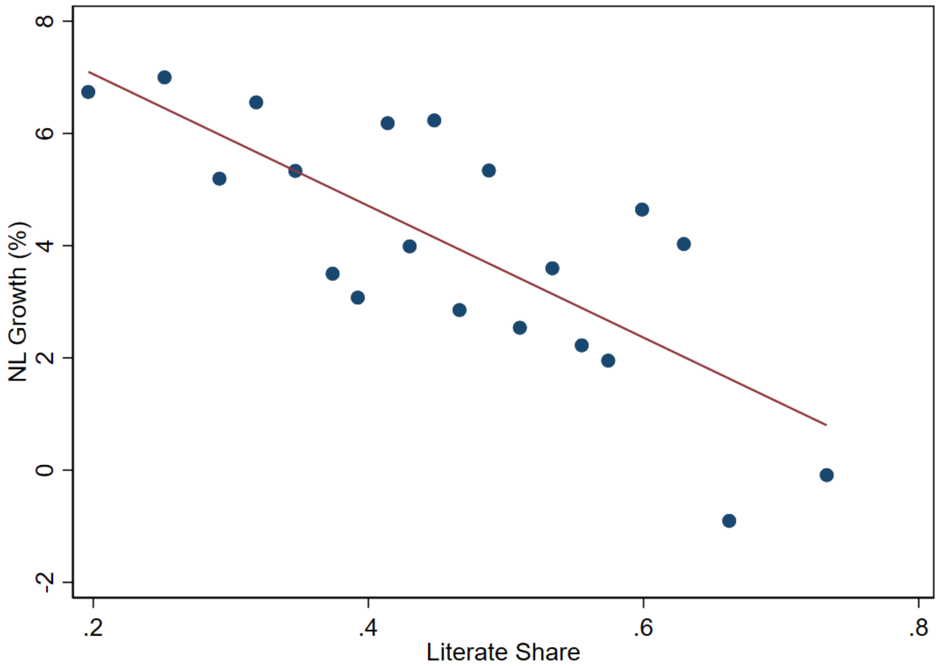

*Notes:* The x-axis equals the margin of victory for an election in percentage points: the difference in vote shares attained by the top two candidates. Positive values represent defector victories, while negative values represent nondefector victories. Each marker represents the mean value of the y-variable at the midpoint of a margin of victory bin. Each bin has equal size (in terms of margin of victory) and the number of bins is chosen using the mimicking variance algorithm. The lines either side of the cutoff represent the linear line of best fit for the binned data. The bandwidth for determining which elections are included is determined by the MSE algorithm.

Figure A8. Person Level Placebo Checks 2




*Notes:* The x-axis equals the margin of victory for an election in percentage points: the difference in vote shares attained by the top two candidates. Positive values represent defector victories, while negative values represent nondefector victories. Each marker represents the mean value of the y-variable at the midpoint of a margin of victory bin. Each bin has equal size (in terms of margin of victory) and the number of bins is chosen using the mimicking variance algorithm. The lines either side of the cutoff represent the linear line of best fit for the binned data. The bandwidth for determining which elections are included is determined by the MSE algorithm.

Figure A9. Binned Scatterplot: NL Intensity Growth vs Literate Share, Close Elections (< 5% Margin)




*Notes:* Literate share data is taken from nearest population census.

Table A1. Defector Shares by State, 1994-2013

| | Proportion of elections with defector in top-2 (Defn 1) | Proportion of elections won by defector (Defn 1) | Proportion of election with defector in top-2 (Defn 2) | Proportion of elections won by defector (Defn 2) |
|---|---|---|---|---|
| Andhra Pradesh | 0.34 | 0.22 | 0.06 | 0.03 |
| Arunachal Pradesh | 0.82 | 0.62 | 0.31 | 0.18 |
| Assam | 0.46 | 0.27 | 0.15 | 0.07 |
| Bihar | 0.64 | 0.47 | 0.27 | 0.17 |
| Chhattisgarh | 0.17 | 0.09 | 0.01 | 0 |
| Delhi | 0.19 | 0.12 | 0.04 | 0.03 |
| Goa | 0.64 | 0.39 | 0.23 | 0.14 |
| Gujarat | 0.42 | 0.26 | 0.16 | 0.08 |
| Haryana | 0.65 | 0.46 | 0.29 | 0.2 |
| Himachal Pradesh | 0.32 | 0.15 | 0.09 | 0.05 |
| Jammu & Kashmir | 0.43 | 0.26 | 0.22 | 0.14 |
| Jharkhand | 0.61 | 0.38 | 0.14 | 0.08 |
| Karnataka | 0.69 | 0.47 | 0.34 | 0.19 |
| Kerala | 0.29 | 0.21 | 0.09 | 0.05 |
| Madhya Pradesh | 0.25 | 0.14 | 0.08 | 0.04 |
| Maharashtra | 0.51 | 0.35 | 0.14 | 0.09 |
| Manipur | 0.83 | 0.61 | 0.46 | 0.26 |
| Meghalaya | 0.72 | 0.53 | 0.35 | 0.21 |
| Mizoram | 0.36 | 0.17 | 0.11 | 0.04 |
| Nagaland | 0.9 | 0.74 | 0.4 | 0.24 |
| Odisha | 0.65 | 0.44 | 0.2 | 0.14 |
| Puducherry | 0.64 | 0.43 | 0.26 | 0.16 |
| Punjab | 0.21 | 0.12 | 0.06 | 0.03 |
| Rajasthan | 0.31 | 0.18 | 0.09 | 0.05 |
| Sikkim | 0.66 | 0.44 | 0.3 | 0.17 |
| Tamil Nadu | 0.29 | 0.17 | 0.14 | 0.07 |
| Tripura | 0.33 | 0.15 | 0.07 | 0.02 |
| Uttar Pradesh | 0.49 | 0.32 | 0.25 | 0.14 |
| Uttarakhand | 0.27 | 0.16 | 0.04 | 0.01 |
| West Bengal | 0.36 | 0.19 | 0.11 | 0.04 |

Table A2. Constituency Characteristics T-Tests

| | Total Obs | External Sample | Internal Sample | Difference | P-value |
|---|---|---|---|---|---|
| Constituency Population | 20838 | 62465.08 | 56846.82 | 5618.25 | 0.00 |
| Non-Reserved | 20852 | 0.73 | 0.71 | 0.02 | 0.19 |
| SC Reserved | 20852 | 0.14 | 0.13 | 0.01 | 0.38 |
| ST Reserved | 20852 | 0.13 | 0.16 | -0.03 | 0.01 |
| NL Growth | 19505 | 4.50 | 3.80 | 0.70 | 0.13 |
| NL Intensity | 20776 | 9.69 | 7.49 | 2.19 | 0.00 |
| Literate Share | 12650 | 0.47 | 0.45 | 0.02 | 0.00 |
| Rural Share | 12793 | 0.86 | 0.87 | -0.02 | 0.04 |
| SC Share | 12650 | 0.17 | 0.16 | 0.00 | 0.25 |
| ST Share | 12650 | 0.15 | 0.15 | 0.00 | 0.69 |

*Notes*: Internal Sample refers to elections with exactly 1 defector and margin of victory $< 5\%$. External sample refers to the complement

Literate share, rural share, SC share and ST share variables are taken from the closest previous census

Table A3. Ln Development Proxys vs Ln NL Intensity, by NL Threshold

| | Above Threshold | | | Below Threshold | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **Ln Per Capita Consumption DV** | | | | | | |
| Ln NL Intensity | 0.17*** | 0.14*** | 0.14*** | 0.022 | 0.018 | 0.027** |
| | (0.0229) | (0.0342) | (0.0393) | (0.0171) | (0.0116) | (0.0113) |
| $R^2$ | .21 | .1 | .08 | .02 | .02 | .04 |
| NL Threshold | 2 | 3.4 | 4 | 2 | 3.4 | 4 |
| Observations | 209 | 172 | 158 | 106 | 143 | 157 |
| **Ln Rural Population Share DV** | | | | | | |
| Ln NL Intensity | -0.30*** | -0.43*** | -0.36*** | -0.018** | -0.017** | -0.017** |
| | (0.0766) | (0.128) | (0.102) | (0.00847) | (0.00755) | (0.00758) |
| $R^2$ | .16 | .18 | .17 | .01 | .01 | .01 |
| NL Threshold | 2 | 3.4 | 2.75 | 2 | 3.4 | 2.75 |
| Observations | 505 | 374 | 436 | 366 | 437 | 435 |
| **Ln (Non-Farm Employment/Pop) DV** | | | | | | |
| Ln NL Intensity | 0.31*** | 0.32*** | 0.31*** | 0.067 | 0.14*** | 0.11*** |
| | (0.0418) | (0.0547) | (0.0505) | (0.0438) | (0.0326) | (0.0358) |
| $R^2$ | .1 | .08 | .08 | .01 | .04 | .02 |
| NL Threshold | 2 | 3.4 | 3 | 2 | 3.4 | 3 |
| Observations | 537 | 403 | 452 | 366 | 500 | 451 |

*Notes*: Per-capita consumption data is available only for 2011. Therefore, we restrict NL data to 2011 for those regressions. Asher et al. construct this data by imputing consumption in the 2011 Socio-Economic and Caste Census from assets and earnings data.
Rural share refers to the share of the constituency's population residing in rural areas. The data is taken from the 2001 and 2011 population censuses. Therefore, we also restrict NL data to 2001 and 2011 for that regression. The non-farm employment data is drawn from the economic censuses of 1998, 2005 and 2013, and the population data uses registered voters as a proxy. NL data is also restricted to 1998, 2005 and 2013 for these regressions. The NL thresholds are chosen as follows: Cols 1, 4 use a threshold of 2- the threshold I used for most regressions. Cols 2, 5 use a threshold of 3.4: the mean NL level for all constituency-years in my sample of elections with exactly 1 defector. Cols 3, 6 use the median NL level for constituency-years with non-missing dependent variable.
Robust standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4. Constituency Level Placebo Checks

| | (1) NL Growth_t-1 | (2) NL Growth_t-2 | (3) NL Level_t-1 | (4) Constituency Size | (5) Turnout | (6) P(SC Reserved) | (7) P(ST Reserved) | (8) P(Not Reserved) |
|---|---|---|---|---|---|---|---|---|
| RD Estimate | 1.97 | -0.92 | 3.09 | 297.3 | -0.87 | 0.018 | 0.017 | -0.036 |
| | (2.36) | (2.61) | (2.02) | (2734.9) | (1.23) | (0.026) | (0.029) | (0.037) |
| Bandwidth (%) | 12.1 | 13.2 | 12.2 | 13.7 | 12.0 | 17.7 | 17.0 | 15.5 |
| Obs Left | 364 | 347 | 388 | 1033 | 954 | 1189 | 1171 | 1105 |
| Obs Right | 365 | 309 | 411 | 1130 | 1028 | 1314 | 1283 | 1219 |
| Robust Standard Error | 2.75 | 3.02 | 2.34 | 3166.3 | 1.41 | 0.030 | 0.032 | 0.043 |

*Notes*: The RD estimate comes from a local regression discontinuity design with margin of victory running variable, slope allowed to vary on both sides of the cutoff, and bandwidth determined by an optimal bandwidth algorithm.
Obs left & right refer to the number of observations with defector losers and defector winners respectively.
Optimal Bandiwdth Algorithm: MSE
Polynomial Order: 1
Standard Errors in parentheses and clustered at the election level.
* $p$ <0.10, ** $p$ <0.05, *** $p$ <0.01

Table A5. Person Placebo Checks

| | (1) P(Winner Female) | (2) P(Winner SC) | (3) P(Winner ST) | (4) Winner Educ | (5) Winner Assets | (6) P(Winner Incumbent) | (7) P(Winner Criminal) | (8) Winner Age |
|---|---|---|---|---|---|---|---|---|
| RD Estimate | -0.024 | 0.063*** | 0.0027 | 0.33 | -4929.8 | -0.0035 | -0.0073 | 1.50 |
| | (0.015) | (0.020) | (0.019) | (0.45) | (9691.0) | (0.042) | (0.060) | (1.47) |
| Bandwidth (%) | 13.4 | 8.17 | 13.5 | 8.39 | 6.84 | 12.3 | 10.8 | 13.0 |
| Obs Left | 1022 | 726 | 1029 | 246 | 212 | 969 | 306 | 421 |
| Obs Right | 1106 | 774 | 1119 | 213 | 190 | 1055 | 253 | 366 |
| Robust Standard Error | 0.018 | 0.022 | 0.023 | 0.52 | 11625.8 | 0.050 | 0.069 | 1.72 |

*Notes*: The RD estimate comes from a local regression discontinuity design with margin of victory running variable, slope allowed to vary on both sides of the cutoff, and bandwidth determined by an optimal bandwidth algorithm.
Obs left & right refer to the number of observations with defector losers and defector winners respectively.
Optimal Bandiwdth Algorithm: MSE
Polynomial Order: 1
Standard Errors in parentheses and clustered at the election level.
* $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table A6. Placebo Check: Demographic Shares

| | (1) SC Share | (2) ST Share | (3) Rural Share | (4) Literate Share |
|---|---|---|---|---|
| RD Estimate | 0.00042 | -0.023 | 0.0083 | -0.045** |
| | (0.011) | (0.020) | (0.024) | (0.021) |
| Bandwidth (%) | 12.4 | 12.1 | 9.28 | 7.85 |
| Obs Left | 502 | 493 | 413 | 364 |
| Obs Right | 573 | 562 | 456 | 394 |
| Robust Standard Error | 0.013 | 0.024 | 0.028 | 0.024 |

*Notes*: The RD estimate comes from a local regression discontinuity design with margin of victory running variable, slope allowed to vary on both sides of the cutoff, and bandwidth determined by an optimal bandwidth algorithm.
Obs left & right refer to the number of observations with defector losers and defector winners respectively.
Optimal Bandwidth Algorithm: MSE
Polynomial Order: 1
Standard Errors in parentheses and clustered at the election level.
* $p$ <0.10, ** $p$ <0.05, *** $p$ <0.01

Table A7. Average Annual NL Growth by Winner SC Status: Close elections

| | Obs | Mean-SC Loser | Mean-SC Winner | Difference | P-value |
|---|---|---|---|---|---|
| Margin $\leq$ 5% | | | | | |
| NL Growth | 910 | 3.63 | 7.49 | -3.86 | 0.13 |
| NL Growth, NL Intensity $\geq$ 2 | 627 | 7.15 | 16.10 | -8.95 | 0.02 |
| NL Growth, Min NL Intensity/election $\geq$ 2 | 535 | 2.89 | 8.27 | -5.39 | 0.01 |
| Margin $\leq$ 3% | | | | | |
| NL Growth | 586 | 3.58 | 7.05 | -3.47 | 0.21 |
| NL Growth, NL Intensity $\geq$ 2 | 402 | 7.57 | 15.19 | -7.62 | 0.11 |
| NL Growth, Min NL Intensity/election $\geq$ 2 | 337 | 2.77 | 7.27 | -4.50 | 0.07 |
| Margin $\leq$ 2% | | | | | |
| NL Growth | 402 | 4.07 | 5.44 | -1.37 | 0.69 |
| NL Growth, NL Intensity $\geq$ 2 | 267 | 6.75 | 19.17 | -12.41 | 0.04 |
| NL Growth, Min NL Intensity/election $\geq$ 2 | 228 | 3.05 | 4.20 | -1.14 | 0.77 |

*Notes*: Sample restricted to elections with exactly one defector in top two.

**Highlights**

**Defector Politicians and Economic Growth: Evidence from India**

- Defector politicians refer to politicians who switch political parties.
- 10-30% of Indian members of legislative assembly (MLAs) are defectors.
- Electing a defector MLA is associated with 0.13-0.48pp higher GDP growth.
- Defectors are 2-3x likelier than non-defectors to become government ministers.
- These effects are not caused by time-invariant characteristics of defectors.

## Conflict of Interest Statement

**Defector Politicians and Economic Growth: Evidence from India**

**Karan Makkar**

The author declares that he has no relevant or material financial interests that relate to the research described in this paper.