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Sunlight Disinfects? Free Media in Weak Democracies*

Leopoldo Fergusson[†] Juan F. Vargas[‡] Mauricio A. Vela[§]

February 18, 2013

Abstract

Free media may not favor political accountability when other democratic institutions are weak, and may even bring undesirable unintended consequences. We propose a simple model in which politicians running for office may engage in coercion to obtain votes. A media scandal that exposes these candidates increases their coercion effort to offset the negative popularity shock. This may result in the tainted politicians actually *increasing* their vote share. We provide empirical evidence from one recent episode in the political history of Colombia, the ‘parapolitics’ scandal featuring politicians colluding with illegal armed paramilitary groups to obtain votes. We show that colluding candidates not only get more votes than their clean competitors, but also concentrate them in areas where coercion is more likely (namely, areas with more paramilitary presence, less state presence, and more judicial inefficiency). Harder to reconcile with other explanations and as a direct test of the effects of media exposure, we compare tainted candidates exposed *before* elections to those exposed *after*. We find that those exposed before elections get as many votes as those exposed once elected, but their electoral support is more strongly concentrated in places where coercion is more likely. Our results highlight the complementarity between different dimensions of democratic institutions.

Keywords: Media, Democracy, Elections, Colombia, Civil Conflict, Coercion.

JEL: D72, D74, L82, P16.

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El Sol Desinfecta?

Prensa Libre en Democracias Débiles

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18 de febrero de 2013

Resumen

La libertad de prensa puede no favorecer la rendición de cuentas políticas cuando otras instituciones son débiles, y puede incluso tener consecuencias negativas no intencionadas. En este artículo, proponemos un modelo simple en el que los políticos que participan en elecciones pueden ejercer coerción para obtener votos. Un escándalo mediático que expone a estos candidatos incrementa sus esfuerzos por compensar el resultante choque negativo de popularidad con mayor coerción electoral. Esto puede implicar que el porcentaje de votación de los políticos expuestos se *incrementa*. En el artículo, presentamos evidencia empírica de un episodio reciente en la historia política de Colombia, el escándalo de la ‘parapolítica’ en el que políticos se aliaron con grupos armados paramilitares para obtener votos. Mostramos que los candidatos aliados con los paramilitares que fueron expuestos por la prensa no sólo reciben más votos que sus competidores, sino que los concentran en áreas en las que la coerción es más probable (en particular, áreas con más presencia paramilitar, menor presencia del estado, y menor eficiencia judicial). En un ejercicio más difícil de conciliar con hipótesis alternativas y como una prueba directa del efecto del escándalo, comparamos políticos afectados por el escándalo que fueron expuestos *antes* de las elecciones con los que fueron expuestos *después*. Encontramos que los expuestos antes de elecciones reciben tantos votos como los expuestos una vez elegidos, pero que su apoyo electoral está más concentrado en lugares donde la coerción es más probable. Nuestros resultados destacan la complementariedad entre diferentes dimensiones de las instituciones democráticas.

Palabras clave: Medios, Democracia, Elecciones, Colombia, Conflicto Civil, Coerción.

JEL: D72, D74, L82, P16.

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1 Introduction

A free and active media is recognized as essential for political accountability. By providing information, mass media can help voters make better decisions and hold politicians accountable. Often, journalists also help uncover corruption scandals and undue influence of special interests groups. A famous example from US history is the Progressive Era, when many argue that an active, informative press reduced corruption and mobilized the population against the power and abuses of Robber Barons and their trusts.¹ It is precisely around this time that Louis Brandeis, who championed the Progressive ideals, famously remarked: “Sunlight is said to be the best of disinfectants”.² Earlier, Founding Father Thomas Jefferson went so far as to say that free media is a sufficient condition for political accountability: “The functionaries of every government have propensities to command at will the liberty and property of their constituents. There is no safe deposit for these but with the people themselves, nor can they be safe with them without information. *Where the press is free, and every man able to read, all is safe.*” (Thomas Jefferson to Charles Yancey, 1816. ME 14:384, quoted in Snyder and Strömberg (2010), emphasis added).

In this paper, we argue that Jefferson was wrong: free media *per se* is no guarantee of political accountability. Unless free media operate in a sufficiently strong institutional environment, provision of information about politicians may not increase political accountability and may even have unintended negative consequences. In particular, exposure of misdeeds can lead politicians to double-down on coercion where they have that option to make up for a decline in electoral support elsewhere.

To explore these ideas, we propose a simple political economy model where two types of politicians compete in an election. One of the politicians may coerce a fraction of voters into voting for him. The specific form of coercion we highlight is one where the politician colludes with illegal armed groups (paramilitaries, in our empirical application) that force people into voting in a particular way. The ‘parapolitician’ (who has colluded with paramilitaries) must decide both the policy platform to offer to voters and the level of coercion to exert.

In this context, we study the implications of media exposure. We assume that media exposure of the parapolitician decreases his popularity relative to his opponent. While this effect reduces electoral support for the parapolitician across all municipalities, there is now an increased incentive to invest in coercion. Hence, the media scandal may *increase* the parapolitician vote share in municipalities where, by increasing coercion, he fully compensates for the decrease in popularity due to exposure. We show that this is more likely to occur where the media scandal is intense and

¹Glaeser, Gentzkow, and Goldin (2004) argue that the expansion of the US newspaper industry between 1870 and 1920 partly caused the decline of political corruption in that period. Gentzkow, Shapiro, and Sinkinson (2009) show that the entry of US daily newspapers from 1869-1928 produced a more active electorate, increasing voter turnout. Acemoglu and Robinson (2012) highlight the role of a free press to expose the excesses of Robber Barons as well as corruption in local and federal politics.

²See <http://www.law.louisville.edu/library/collections/brandeis/node/196>.

where voters are less ideologically biased towards either party. Each of these parameters increase parapoliticians' incentives to coerce following the media scandal: a more intense scandal and less ideological voters implies more votes are lost as a result of the scandal. Finally, the model suggests that it is less likely that increased coercion more than compensates the fall in popularity if local institutions are strong, as this increases the marginal cost of coercion.

An important implication of the analysis is that, since votes may increase in some municipalities where coercion is strong enough, the media scandal may not hurt the overall electoral success of the parapolitician. An additional important implication is that, when selecting policies, both politicians take more into account the preferences of citizens in municipalities where there is less coercion, because only non-coerced voters respond to policy proposals when deciding whom to vote for. Also, all else equal, the parapolitician gets more votes than his opponent because paramilitaries provide him with coerced votes.

The model highlights that there are three ways in which the media scandal may have negative unintended consequences. First, it increases coercion. Second, as a result, politicians pay less attention to voters' needs. Third, if this endogenous response of coercion is sufficiently strong, the media scandal may not even reduce the overall votes for politicians that are shown to have links with paramilitaries, an arguably desirable objective³. These simple results highlight the complementarity between the different dimensions of democratic institutions. Having a free, active media may not be enough and may even bring undesirable unintended consequences if other dimensions of institutional quality, like the existence of free and fair elections, are not guaranteed. Indeed, the negative consequences of the media scandal would disappear with sufficiently strong institutions (that is, if coercion is prohibitively costly).

We test some of the implications of our model in the context of Colombia's legislative elections from 2002 to 2010, using one salient episode: the 'Parapolitics' scandal. The timing and scope of the media scandal is revealed in Figure 1, which shows a pronounced increase in the share of 'parapolitics' stories to all recorded stories about our set candidates around 2006. While a few stories appeared around the 2002 elections, especially during the 2006 elections the national media denounced politicians' deals with illegal armed paramilitary groups to obtain votes by exerting coercion in areas of little state presence, in exchange of a lenient legislation toward these criminals. As we will discuss in more detail below, coercion included, in addition to the intimidation of voters through terror, massacres and political assassinations, electorally fraudulent practices like vote buying, ballot stuffing and the use of the voting IDs of deceased people (Valencia, 2007). While the media scandal had positive effects in that it led to judicial investigations of (and jail sentences to) many of the tainted politicians, we show that it also had unintended negative consequences.⁴

We first document, in line with what has been shown before by other scholars, that parapoliti-

³Parapolitician wellbeing, on the other hand, is worse after media exposure because increasing coercion is costly.

⁴For a description of how the media helped avoid impunity in parapolitics cases, see Restrepo (2008).

cians have a different vote distribution than competitors and that this concentration is in line with the predictions from our theoretical model: Senate candidates involved in the scandal get significantly more votes in areas where there is paramilitary presence, less presence of (judiciary) institutions, and the available judicial institutions are inefficient. These results are robust to using different measures of the individual involvement with the scandal for each politician as well as to using elections fixed effects and candidate-specific controls.

Most importantly, we present an additional exercise that directly tests the effects of the scandal and is harder to reconcile with potential alternative accounts. Indeed, the aforementioned patterns of vote distribution hold when comparing, among parapoliticians, candidates exposed by the media before they were elected with those exposed once elected. Consistent with the model, parapoliticians exposed by the press before the elections shift their distribution of votes to areas in which coercion is easier to exert (areas with more paramilitary presence and less presence and efficiency of state institutions). These results are also robust to including candidate controls and election fixed effects.

Finally, we provide evidence that parapoliticians are relatively more successful than their clean competitors in terms of their vote share. Even those involved in the scandal before elections were able to counteract the media scandal with increased support in areas where coercion is more likely, and their vote share does not significantly differ from that of candidates exposed once elected.

To complement our main findings, we discuss anecdotal evidence suggesting that coercion (which we cannot directly observe) is the likely underlying mechanism explaining these voting patterns. We also find that parapoliticians are more likely than non-parapoliticians to get support from municipalities exhibiting unusually high levels of turnout. Similarly, among parapoliticians, those exposed by the media before elected get a higher share of their votes from places with unusually high levels of turnout than those exposed once elected. These results are also in line with coercion being the underlying mechanism of our main results, as most forms of coercion by paramilitaries documented in the media and judicial investigations inflate vote totals.

Though the role of media in democracy has long been studied, our paper relates most closely to the relatively recent economics research on the behavior and political effects of mass media (surveyed by Prat and Strömberg (2011)). According to this line of research, access to media and its content influences citizens' electoral behavior and attitudes, increasing turnout (Gentzkow et al., 2009), improving information about representatives (Snyder & Strömberg, 2010), and influencing vote shares for certain candidates (DellaVigna & Kaplan, 2007; Prat & Strömberg, 2005). Moreover, the influence of mass media extends to policy by empowering citizens (Besley & Burgess, 2002; Strömberg, 2004a, 2004b), and by making some issues more salient than others (Eisensee & Strömberg, 2007).

A key message from the literature is that, by improving information and electoral response, mass media may improve political accountability (see, for example, Ferraz and Finan (2008), Snyder and Strömberg (2010) and Brunetti and Weder (2003)). Nevertheless, the power of the media is

a double-edged sword. For instance, mass media can present biased information, its bias may persist over time, and it may influence electoral behavior (Baron, 2006; Durante & Knight, 2009; Gentzkow & Shapiro, 2004, 2006, 2008, 2010b; Groseclose & Milyo, 2005; Puglisi, 2006). Along the same lines, while access to free media may be key for democratic consolidation (Andriantsoa et al., 2005; Glaeser et al., 2004) and even development (Djankov & McLiesh, 2002), with media “capture” by the government or other powerful actors, checks and balances may be dismantled rather than strengthened (McMillan & Zoido, 2002; Besley & Prat, 2006; Petrova, 2008).

Our paper relates to this literature by emphasizing the limits of mass media in achieving political accountability. However, it proposes a novel mechanism. In particular, we do not study the potentially negative effects of mass media bias due to political capture, the profit motive, or other reasons. We also do not examine the many instances where an active media is absent because of political oppression or other causes. Instead, we argue that even if mass media is independent and provides valuable information to voters thus increasing transparency, it may not promote political accountability in a weakly-institutionalized environment.

A few recent papers also emphasize unintended consequences of information and media exposure, but the mechanisms highlighted differ from ours. In particular, DellaVigna, Enikolopov, Mironova, Petrova, and Zhuravskaya (2011) show that in a conflict environment the media may have an unintended effect of increasing ethnic animosity. They present evidence suggesting that Croats listening to Serbian radio (intended for Serbian listeners across the border) are more likely to vote for extreme nationalist parties, even after several years of peace time. Chong, De La O, Karlan, and Wantchekon (2011) provide experimental evidence that information about corruption in Mexico decreases incumbent support in local elections, but may be insufficient to improve political accountability, as voters also respond to information by withdrawing from the political process.

Our paper also relates to the idea that political elites often exercise *de facto* power to maintain their political power and shape institutions (Acemoglu & Robinson, 2008). Notice also that our model predicts that the media may help create deeper electoral polarization, a result that has recently been studied for the internet, though via other mechanisms (see, for example, Gentzkow and Shapiro (2010a)).

Finally, the ‘parapolitics’ phenomenon in Colombia has been amply studied (for a review, see Barrera and Nieto Matiz (2010)), though the effects of the media scandal specifically, which is the focus of our paper, have been largely overlooked.

We next present our model in section 2. Section 3 discusses our empirical strategy to examine some of the implications of the theory in the context of Colombia, and section 4 presents our main data. In section 5 we discuss our main results and robustness checks, and section 6 concludes. We relegate a few details on data construction and an extension to the baseline model to the appendix.

2 A simple model

To guide our empirical analysis, in this section we present a model of the electoral consequences of media exposure of parapoliticians. We consider an environment with $m = 1, \dots, N$ municipalities, each with a mass of voters of size 1. Two politicians, A and B, run for office, but A (the parapolitician) is able to collude with illegal armed groups (paramilitaries) that force voters to vote in a particular way. We model this as in Acemoglu, Robinson, and Santos (forthcoming), assuming that when coercion occurs, the population votes for politician A, the candidate supported by the armed group. However, we assume that only a share λ_m of the population is coerced and votes for A (in Acemoglu et al. (forthcoming), for most of the analysis λ_m is set to 1 in a subset of municipalities with paramilitary control).

The remaining fraction $1 - \lambda_m$ of voters freely decide whom to vote for and we model this decision as in the standard probabilistic voting model (Lindbeck & Weibull, 1987). More specifically, voter i in municipality m votes for A so long as:

$$u^m(\mathbf{q}^A) + \sigma^{im} > u^m(\mathbf{q}^B),$$

where \mathbf{q}^x is the policy vector offered by politician $x \in \{A, B\}$, $u^m(\cdot)$ is the indirect utility over policies for citizens in municipality m , and σ^{im} is the ideological distribution in favor of politician A in municipality m . We assume σ^{im} is distributed uniformly with mean $\sigma_m - \mu_m$ and density ϕ_m (hence, over the support $[-\frac{1}{2\phi_m} + \sigma_m - \mu_m, \frac{1}{2\phi_m} + \sigma_m - \mu_m]$). The mean of σ^{im} in each municipality has two components. First, σ_m is the average ideological leaning toward party A absent a media scandal in municipality m . Second, if a media scandal breaks out and newspapers publish the collusion of illegal armed groups with party A, the ideological bias in favor of A falls across all municipalities. This is captured by μ_m , measuring a potentially different impact of the media scandal in each municipality m . We also allow ϕ_m to vary across municipalities, with a larger ϕ_m meaning less ideological dispersion and therefore stronger response of voters to policy proposals.

Finally, the cost of coercion is given by

$$C(\{\lambda_m\}_{m=1}^N) = \sum_{m=1}^N \psi_m c(\lambda_m),$$

with increasing and convex costs, $c'_\lambda > 0$, $c''_{\lambda\lambda} > 0$. To keep our model as simple as possible, we assume a quadratic cost, $c(\lambda_m) = \lambda_m^2/2$. We interpret the parameter ψ_m as the overall quality of institutions, especially the enforcement of electoral law, which increases the cost of coercion.

To solve this simple game, we find the policy positions \mathbf{q}^A and \mathbf{q}^B that politicians A and B simultaneously offer to maximize their total electoral support⁵. Policy proposals are credible (politicians

⁵The implications of the model when assuming that politicians maximize their probability of winning are similar. However, in the context of legislative elections it seems more natural to model each politician as maximizing his votes,

fulfill their promises). Politician A, who has colluded with paramilitaries, also chooses the level of coercion effort λ_m to exert in each municipality. A more realistic formulation would incorporate an exchange between politician A and paramilitaries, who in turn select coercion. However, since we are not directly interested in the bargaining game between politicians and paramilitaries, we simplify by assuming that A selects λ_m directly.

2.1 Equilibrium policies and coercion

To find the Nash Equilibrium of the game, we calculate the share of votes for each politician as a function of policy proposals. First we find the share of individuals voting freely that support A in each municipality m using the distribution of σ^{im} . More specifically, we find the fraction of free voters for whom $\sigma^{im} > u^m(\mathbf{q}^B) - u^m(\mathbf{q}^A)$:

$$\frac{1}{2} + \phi_m (\sigma_m - \mu_m + u^m(\mathbf{q}^A) - u^m(\mathbf{q}^B)).$$

Since in each municipality a share λ_m vote for A, total support for A in each municipality, π_m^A , is

$$\begin{aligned} \pi_m^A &= \lambda_m + (1 - \lambda_m) \left[\frac{1}{2} + \phi_m (\sigma_m - \mu_m + u^m(\mathbf{q}^A) - u^m(\mathbf{q}^B)) \right] \\ &= \frac{1 + \lambda_m}{2} + (1 - \lambda_m) \phi_m (\sigma_m - \mu_m + u^m(\mathbf{q}^A) - u^m(\mathbf{q}^B)). \end{aligned} \quad (1)$$

Adding over all municipalities, total electoral support for A, Π^A , is:

$$\Pi^A = \sum_m \pi_m^A = \frac{N + \sum_m \lambda_m}{2} + \sum_m \phi_m (1 - \lambda_m) (\sigma_m - \mu_m + u^m(\mathbf{q}^A) - u^m(\mathbf{q}^B)).$$

Politician A chooses policies and coercion to maximize $\Pi^A - C(\{\lambda_m\}_{m=1}^N)$. In turn, politician B chooses policies to maximize $\Pi^B = N - \Pi^A$. The solution to this problem is policy convergence with \mathbf{q}^* that satisfies:

$$\sum_{m=1}^N (1 - \lambda_m^*) \phi_m \nabla u^m(\mathbf{q}^*) = \mathbf{0}. \quad (2)$$

From the first order conditions of politician A, equilibrium coercion λ_m^* can be written as:

$$\lambda_m^* = \max \left\{ 0, \frac{1}{\psi_m} \left(\frac{1}{2} + \phi_m (\mu_m - \sigma_m) \right) \right\}, \quad (3)$$

for each municipality m . These results and their comparative statics implications are summarized in the following proposition.

and our assumption of two candidates running for office is just for analytical simplicity.

Proposition 1 *Equilibrium policies and coercion*

In the equilibrium of the electoral game:

1. Parties A and B converge to the vector of policy proposals \mathbf{q}^* in (2). The resulting policy attention to voters in municipality m is decreasing in coercion λ_m^* and increasing in the density of ideological biases ϕ_m .
2. Party A exerts coercion λ_m^* in each municipality as given by (3). λ_m^* satisfies the following comparative static properties:

$$\begin{aligned} \frac{\partial \lambda_m^*}{\partial \psi_m} &\leq 0, \quad \frac{\partial \lambda_m^*}{\partial \sigma_m} \leq 0, \\ \frac{\partial \lambda_m^*}{\partial \mu_m} &\geq 0, \\ \frac{\partial \lambda_m^*}{\partial \phi_m} &\begin{cases} \geq 0 \\ < 0 \end{cases} \text{ if } \sigma_m - \mu_m \begin{cases} \leq 0 \\ \geq 0 \end{cases}. \end{aligned}$$

Proof. Follows directly from (2) and (3), derived in the main text. ■

Proposition 1 emphasizes that, as in any probabilistic voting model, municipalities where the ideological biases σ^{im} of the population are less densely distributed (that is, where ϕ_m is small) receive less policy attention. Indeed, with small ϕ_m there are more voters with strong biases towards party A or party B. Such voters will pay little attention to policy proposals in their voting decision. Thus, politicians have little incentive to cater to the policy needs of such voters. More important for our analysis, the utility of voters in each municipality receives a weight that is decreasing in the level of coercion. Obviously, policy proposals have no influence in the voting decision of coerced voters. With coercion, only a share $1 - \lambda_m^*$ of citizens react to policy. Thus, both politicians take more into account the preferences of citizens in municipalities where there is less coercion⁶. A useful way of thinking about the solution in (2) is to notice that it is equivalent to the solution of maximizing a social welfare \mathcal{W} function that gives a weight to citizens of municipally m that is proportional to $1 - \lambda_m^*$,

$$\mathcal{W} = \sum_{m=1}^N (1 - \lambda_m^*) \phi_m u^m(q).$$

Hence, even ignoring differences in voters' ideological biases (if $\phi_m = \phi$ for all m) only in municipalities without coercion, $\lambda^* = 0$, citizens' utility is fully taken into account in the political

⁶This is similar to the Acemoglu et al. (forthcoming) case of a solution with passive paramilitaries, only that they assume that no voters are able to respond to policy in the subset of paramilitary controlled areas where coercion occurs. In our model, such set of municipalities correspond to those for which $\frac{1}{2} + \phi_m(\mu_m - \sigma_m) < 0$. One can directly verify that given the distribution of ideological biases, in such municipalities all voters would freely vote for A ($\pi_m^A = 1$) and there is no incentive to exert coercion. In other words, given our simple formulation where the marginal cost of exerting minimal coercion approaches zero, parapoliticians coerce every municipality where at least some voters support the opponent.

equilibrium. In this sense, coercion introduces a distortion relative to the choice of a social planner maximizing the sum of citizens' utilities.

Turning to equilibrium coercion, λ_m^* is decreasing in the quality of local institutions ψ_m , as better institutions make coercion more costly. On the other hand, coercion increases with the impact of the media scandal (μ_m): when politician A's popularity is adversely affected by the media, he tries to counteract the effect on electoral support by coercing more voters to vote for him. Finally, notice that where the population is more ideologically biased in favor of A, there is less equilibrium coercion. Indeed, in such case coercion is not as 'necessary' as most voters will vote for A driven solely by their ideological sympathy to this politician. This observation is also important to understand why the impact on coercion of the density ϕ_m of ideological biases depends on the sign of $\sigma_m - \mu_m$. Since μ_m measures the fall in average popularity of party A following the media scandal and σ_m is the average ideological bias in favor of A absent any media scandal, $\sigma_m - \mu_m$ measures the average *net* sympathy towards A in the population, after the scandal. When this is negative, given policy convergence the majority of voters is inclined to vote against party A. Moreover, the mass of voters that will favor B instead is proportional to ϕ_m . For this reason, when $\sigma_m - \mu_m < 0$ and ϕ_m increases, to avoid losing this electoral support party A responds by increasing coercion.

2.2 Support for the Parapolitician

With policies and coercion described in Proposition 1, we can find the equilibrium share of votes received by party A in each municipality m :

$$\pi_m^A(\mu_m, \lambda_m^*) = \frac{1 + \lambda_m^*}{2} + (1 - \lambda_m^*)\phi_m(\sigma_m - \mu_m). \quad (4)$$

Adding over all municipalities, total equilibrium support for A is

$$\Pi^A = \frac{N + \sum_m \lambda_m^*}{2} + \sum_m \phi_m(1 - \lambda_m^*)(\sigma_m - \mu_m). \quad (5)$$

The expression highlights that party A, all else equal, has more votes than party B because parapoliticians give him coerced votes ($\sum \lambda_m^*/2 > 0$). More importantly, the impact of the media scandal on the electoral support for the parapolitician combines two effects that appear in the following expression for the derivative of $\pi_m^A(\mu_m, \lambda_m^*)$ with respect to μ_m :

$$\begin{aligned} \frac{d\pi_m^A}{d\mu_m} &= \frac{\partial \pi_m^A}{\partial \mu_m} + \frac{\partial \pi_m^A}{\partial \lambda_m^*} \frac{\partial \lambda_m^*}{\partial \mu_m} \\ &= -(1 - \lambda_m^*)\phi_m + \left(\frac{1}{2} + \phi_m(\mu_m - \sigma_m) \right) \frac{\partial \lambda_m^*}{\partial \mu_m} \geq 0. \end{aligned} \quad (6)$$

On the one hand, holding coercion fixed, support for A falls because the media scandal makes him less popular. This is the first term in (6), capturing the *direct* effect of the media scandal on parapolitician support. On the other hand, as the second term shows, as long as there is positive coercion in municipality m , there is an *indirect* effect whereby party A at least partially compensates for the fall in free votes by endogenously increasing coerced votes⁷.

Let's examine more closely how the media scandal affects each of the two terms in the indirect effect, namely: the rise in coercion following the media scandal ($\partial\lambda_m^*/\partial\mu_m$), and the electoral return of such increase in coercion ($\partial\pi_m^A/\partial\lambda_m$). The key observation is that both of these terms are increasing functions of the media scandal. Hence, the indirect effect which is the product of these two may dominate the direct effect under some parameter values (and, specifically, when the media scandal is large enough).

Starting with the electoral returns of an increase in coercion, note that this is simply

$$\partial\pi_m^A/\partial\lambda_m = 1 - (1/2 + \phi_m(\sigma_m - \mu_m)).$$

The first term in this expression simply tells us that an increase in coercion guarantees getting an additional vote in municipality m . However, since some of the free voters are supporting A, there is also an electoral cost of increasing coercion: the loss of $1/2 + \phi_m(\sigma_m - \mu_m)$ free votes. The difference is the *net* gain in votes with a marginal increase in coercion appearing in (6), and is increasing in the media scandal μ_m . Since a media scandal makes the parapolitician less popular, coercion is 'cheaper' in that less free votes are lost when the parapolitician exercises coercion. This last observation also explains why the other term in the indirect effect, $\partial\lambda_m^*/\partial\mu_m$, is positive as noted already in Proposition 1. In short, both coercion and the return to each unit of additional coercion increase when μ_m increases, implying that under some conditions the media scandal may increase the total electoral support for the parapolitician.

Finally, suppose that to capture the impact of a national media scandal, parametrized by μ , we can write the local impact of this scandal simply as $\mu_m = \mu l_m$ where $l_m \in [0, 1]$ measures the quality of local media. Then,

$$\frac{\partial\Pi^A}{\partial\mu} = \sum_{m=1}^N \frac{\partial\pi_m^A}{\partial\mu_m} \frac{\partial\mu_m}{\partial\mu} = \sum_{m=1}^N \frac{\partial\pi_m^A}{\partial\mu_m} l_m, \quad (7)$$

where $\partial\pi_m^A/\partial\mu_m$ is given by (6). These results, and other features of the electoral response to a media scandal, are summarized in the next proposition.

Proposition 2 *Equilibrium support for the Parapolitician*

⁷That the indirect effect is always nonnegative follows from noting in our expression for coercion (3) that coercion λ_m^* is positive and $\partial\lambda_m^*/\partial\mu_m > 0$ whenever $1/2 + \phi_m(\mu_m - \sigma_m) > 0$.

In the equilibrium of the electoral game, total votes for party A (the parapolitician) are given by π_m^A in (4) in each municipality m and by Π_A in (5) at the national level.

Moreover, $d\pi_m^A/d\mu_m$ is given by (6) and satisfies the following property:

$$\frac{d\pi_m^A}{d\mu_m} \geq 0 \quad \text{if} \quad \lambda_m^* \geq \frac{1}{2} \quad \left(\text{or} \quad \frac{1}{2} + \phi_m(\mu_m - \sigma_m) \geq \frac{1}{2}\psi_m \right)$$

Finally, the impact of a national media scandal μ on total support for the parapolitician, where $\mu_m = \mu l_m$ with $l_m \in [0, 1]$, is given by:

$$\frac{\partial \Pi^A}{\partial \mu} = \sum_{m=1}^N \frac{\partial \pi_m^A}{\partial \mu_m} l_m \geq 0$$

Proof. All results were derived in the main text, except for the conditions under which $\partial \pi_m^A / \partial \mu_m \geq 0$. This follows from using λ^* in (3) to write (6) as

$$\frac{d\pi_m^A}{d\mu_m} = -(1 - \lambda_m^*)\phi_m + \frac{\partial \lambda_m^*}{\partial \mu_m} \lambda_m^* \psi_m \geq 0.$$

From (3), we also have $\partial \lambda_m^* / \partial \mu_m = \phi_m / \psi_m$. After substituting, we get $d\pi_m^A / d\mu_m = -(1 - 2\lambda_m^*)\phi_m$ and the expressions in the proposition follow. ■

The most important feature of the solution is that, with the endogenous response by parapoliticians, the media scandal need not reduce their vote share in places where they can exert coercion. More specifically, the media scandal may *increase* parapolitician vote share in coerced municipalities when the direct negative impact of the scandal is more than compensated by the endogenous response in coercion effort from parapoliticians through collusion with paramilitaries. Proposition 2 shows that this occurs as long ϕ_m and μ_m are large enough. Each of these parameters increase parapoliticians' incentives to exert coercion because more votes are prevented from going to the alternative candidate as a result of the scandal when ϕ_m and μ_m are large. Instead, it is less likely that after the media scandal a parapolitician obtains more votes in municipality m if ψ_m is high, as strong institutions increase the marginal cost of coercion, reducing incentives to compensate for the media scandal with increases in coercion. Also, when the population of municipality m is strongly biased ideologically towards A (when σ_m is large), the response of coercion to the media scandal will also be weaker, as the population is inclined to vote, even freely, for the parapolitician.

Since the media scandal need not reduce the vote share for paramilitaries in any given municipality m , total electoral support for the parapolitician following a media scandal does not necessarily fall either. This is the second key result in Proposition 2: media exposure of parapoliticians does not make them more electorally accountable when they can hide in the shade. Of course, this does not mean that parapolitician wellbeing is unaffected by the media scandal. It is straightforward to

show that following a national media scandal, politician A is worse off, even if he can sustain his electoral success⁸.

Needless to say, a number of simplifying assumptions drive our results. Indeed, more than proposing a very general model of electoral competition under coercion and media exposure, we attempted to present the simplest model to capture the idea that an endogenous response in coercion may at least partly compensate the electoral impact of a media scandal, especially in areas with a weak institutional environment. In Appendix A, we consider one particularly important extension. In the baseline model, an increase in media exposure of the parapolitician only affects his popularity. However, one could conceivably argue that more media exposure also makes coercion more costly. In the Appendix, we incorporate this additional ingredient and show that, as long as coercion costs do not increase linearly with the media scandal, similar results follow.

2.3 Discussion

This simple framework has a number of important implications. First of all, as is clear from equation (5), other things being equal, parapoliticians (politician A) have an edge over non-parapoliticians (politician B) because they can exert coercion. Second, equation (2) shows that as a result of coercion paramilitary-controlled areas tend to receive less than full attention in terms of policy, and that this lack of attention is increasing with the extent of coercion. The intuition is simple: coerced voters do not respond to policy, so there is no incentive to cater to their demands.

The more important implications, however, concern the impact of the media scandal and the endogenous response of parapoliticians to such scandal in Propositions 1 and 2. Despite the existence of coercion, a natural intuition is that a media scandal revealing ties of paramilitaries with politicians ought to bring greater political accountability. The framework above shows that this intuition is not necessarily true when paramilitaries are able to exert sufficient electoral coercion, or hide in the shade. In particular, our results highlight that there are three ways in which a media scandal may have negative unintended consequences. First, it increases coercion. Second, as a result, politicians pay less attention to voters' needs. Third, if this endogenous response of coercion is sufficiently strong, the media scandal may not even reduce the overall vote share for politicians that are shown to have links with paramilitaries, an arguably desirable objective of revealing the scandal.

These simple results highlight the complementarity between the different dimensions of institutions in a democracy. Having a free, active media may not be sufficient and may even bring

⁸Direct differentiation of politician's A equilibrium welfare $\Pi^A - C(\{\lambda_m^*\})$ shows that

$$\frac{d\Pi^A}{d\mu} - \frac{dC(\{\lambda_m^*\})}{d\mu} = - \sum_m l_m (1 - \lambda_m^*) \phi_m < 0,$$

which is simply an application of the Envelope Theorem.

undesirable unintended consequences if other dimensions of institutional quality, like the existence of free and fair elections, are not guaranteed. Indeed, the framework above suggests that these negative consequences of the media scandal would be reduced with sufficiently strong institutions (sufficiently high ψ).

With this framework in mind, we next turn to the empirical strategy that will guide our data analysis.

3 Empirical Strategy

We face two main limitations in testing the implications of our theoretical framework. First, because of its illegal nature and its reliance on threats and intimidation, it is very hard to observe coercion at the local level and measure it systematically. Hence, we focus on the electoral consequences of the ‘parapolitics’ scandal in Colombia. Second, we do not have a good measure of the local impact of the scandal in Colombia (μ_m in our model). Indeed, circulation figures for the main national newspaper, *El Tiempo*, and other national or regional newspapers are not regularly published or verified in Colombia⁹. Moreover, even if these were available, readership in municipalities outside the capital Bogotá is mostly of regional newspapers, rather than national newspapers. Hence, a key source of variation of the local impact of the scandal is coverage by the regional press of the parapolitics scandal. However, regional newspapers typically have very limited electronic archives. Hence, we cannot construct a good measure of parapolitician exposure in the regional press as we do with the national press using *El Tiempo’s* electronic archive (except, of course, following the prohibitively costly strategy of reading the print editions for the many regional newspapers).

These limitations are important, but with our coding of stories from *El Tiempo* we have a very good measure of the national media scandal (μ in our model). In particular, while with differing degree of emphasis or even tone, we expect the stories exposing parapoliticians that we capture in our *El Tiempo* news search to have been reproduced at the local level (not just in newspapers, but also in both national and local radio stations and TV broadcasts). Direct evidence showing that the news search in *El Tiempo* proxies not just what this newspaper was presenting to the audience but what other media outlets were uncovering is presented in Section 5.5.2, where we compare *El Tiempo* with another national newspaper, *El Espectador*. Using a detailed, human coding of all stories referring to any of our set of candidates (detailed in the Appendix, section B) we also measure very precisely when and to what extent each of the Senate candidates were exposed by the media as connected with paramilitaries.

⁹Unlike in the US, where the industry complies with the Audit Bureau of Circulation which holds a reliable record of newspaper circulation, in Colombia the industry converged to using surveys to have independent measures of penetration. These surveys, the most important of which is the *Encuesta General de Medios*, only focus on a few cities. While useful for most purposes for newspapers (in particular, for selling advertisement space as large cities concentrate the bulk of consumers), this information is not comprehensive enough to allow us to run regressions at the municipality level.

For all these reasons, we focus on regressions at the candidate level. More specifically, given that our theoretical framework predicts that parapoliticians should have a different distribution of votes than non-parapoliticians (and that such distribution is likely to be affected by the media scandal) we construct the following variables for each candidate c and time period t :

$$\bar{y}_{ct} = \sum_m \pi_{mt}^c y_m. \quad (8)$$

In (8), y_m is a variable capturing one of the key dimensions that, in our theory, affect the distribution of votes for parapoliticians. We weight this municipality characteristics by π_{mt}^c , the vote share of candidate c (out of his own total votes) in municipality m at time t . Hence, \bar{y}_{ct} is a weighted average that summarizes the characteristics of municipalities where candidate c got his votes from at time t .¹⁰

We use proxies for paramilitary presence, local efficiency of the judiciary, and local presence of the state as our key variables y_m to compute \bar{y}_{ct} . We then run regressions of the following form:

$$\bar{y}_{ct} = \beta_0 + \beta_1 \text{Para}_{ct} + \beta_2^T \mathbf{X}_c + \delta_t + \varepsilon_{ct}. \quad (9)$$

In (9), Para_{ct} is one of our alternative (media-based) measures for whether candidate c is a parapolitician. Our model predicts that parapoliticians should have more votes in areas with high paramilitary presence (where we expect there to be more coercion) and weak institutions (small ψ_j in the model). Hence, in regressions where \bar{y}_{ct} is the average paramilitary presence of the areas where candidate c obtained votes, we expect $\beta_1 > 0$. Instead, when \bar{y}_{ct} is a measure of the average quality of local institutions in the municipalities that supported candidate c we expect $\beta_1 < 0$. Our specifications cover the legislative elections of 2002, 2006, and 2010, and we include a full set of time fixed effects δ_t to allow for change across these elections.¹¹ Also importantly, since being a parapolitician may be correlated with other candidate characteristics, we include a set observable candidate-level controls in the vector \mathbf{X}_c .

¹⁰When calculating \bar{y}_{ct} using different municipality characteristics, we do a simple rescaling such that every y_m varies between 0 and 100. In particular, we compute y_m as

$$y_m = 100 \frac{\tilde{y}_m - \min_m \tilde{y}_m}{\max_m \tilde{y}_m - \min_m \tilde{y}_m},$$

where \tilde{y}_m is simply the unscaled municipality characteristic. This rescaling has no effects on our results, but makes the units of our dependent variable comparable across the tables that we present below.

¹¹From 2003 to 2006 large numbers of paramilitaries (allegedly around 35,000) demobilized collectively in the context of negotiations between the paramilitary umbrella organization AUC and the government of president Uribe. However, paramilitaries hardly lost their capacity of exerting coercion in parts of the Colombian territory, both because some paramilitary blocks refrained from participating in the process, and because several of the blocks that did quickly resorted to arms and formed what is now known as “neo-paramilitary” criminal bands. Indeed, Figure 2 shows that, in spite of a downward trend starting in 2003 (when the demobilization process started), attacks by paramilitary groups peaked in 2005, just before the (March) 2006 Congress elections which drive most of our empirical results.

Equation (9) is a useful description of the patterns of electoral support for parapoliticians and non-parapoliticians, unveiling correlations that can be interpreted under the lens of our theory. However, the more interesting implications of our model concern the impact of the media scandal on such vote distribution. In particular, we expect that following exposure by the media, parapoliticians exacerbate their efforts to obtain votes in coerced areas, especially in the context of weak institutions. Hence, we also run the following regression, similar to equation (9) but only for the set of candidates that we identify as parapoliticians:

$$\bar{y}_{ct} = \beta_0 + \beta_1(\textit{Exposed before election}_{s_{ct}}) + \beta_2^T \mathbf{X}_c + \delta_t + \varepsilon_{ct}, \quad (10)$$

where $c \in \{\textit{Parapolitician}\}$, the set of senators in our sample that were linked by the media with paramilitaries at any point in time. Our key independent variable, *Exposed before election*_{ct}, is a dummy variable that equals 1 if candidate c was linked to the parapolítica scandal before the election at time t . Again, when \bar{y}_{ct} is the average paramilitary presence of the areas where candidate c obtained votes, we expect $\beta_1 > 0$ and when \bar{y}_{ct} is a measure of the average quality of local institutions in the municipalities that supported candidate c we expect $\beta_1 < 0$.

Since this regression includes only the set of parapoliticians, it allows us to check the effect of the media scandal, excluding the overall differences in the patterns of electoral support between parapoliticians and non-parapoliticians that (9) explores. However, one threat to identification remains when using (10) to examine the consequences of the media scandal: within the group of parapoliticians, those exposed before elected may differ systematically to those exposed after elected. Unobserved ability or other candidates characteristics could therefore bias our results. To assuage this concerns, however, we show that the set of parapoliticians exposed before elections have similar observable characteristics as those exposed once elected, and that our results are robust to including observable candidate characteristics \mathbf{X}_c . An additional solution to this problem would be to follow a regression discontinuity approach, comparing only the set of candidates exposed by the media “shortly” before and “shortly after” election day, controlling flexibly for distance (days, weeks, or months) to elections. Unfortunately, however, we do not have a large enough sample of politicians to follow this regression discontinuity approach¹².

We can be more precise about the interpretation of β_1 in regressions (9) and (10) by referring to our theoretical model. Indeed, notice that a regression of \bar{y}_c on a parapolitician dummy simply uncovers the difference in \bar{y}_c for a parapolitician and a non-parapolitician. That is, in terms of our model, $\beta_1 = \bar{y}_A - \bar{y}_B$. Thus, from the definition of \bar{y} in (8) and writing the vote shares obtained by each candidate in each municipality using the model notation, we have:

¹²Indeed, the number of parapoliticians competing in our elections is ‘only’ 77, corresponding to 119 observations through our three election years. Hence, when attempting a regression discontinuity approach, there is a large uncertainty attached to our estimation of β_1 .

$$\beta_1 = \bar{y}_A - \bar{y}_B = \sum_m \left(\frac{\pi_m^A}{\bar{\Pi}^A} \right) y_m - \sum_m \left(\frac{1 - \pi_m^A}{N - \bar{\Pi}^A} \right) y_m.$$

Let $\bar{y}_m = \sum_m y_m / N$ be the average of characteristic y and $\bar{\pi}_m^A = \sum_m \pi_m^A / N = \bar{\Pi}^A / N$ the vote share for A across municipalities. Then, using $\text{Cov} \{y_m, \pi_m^A\} = (1/N) \sum_m y_m \pi_m^A - \bar{y}_m \bar{\pi}_m^A$, we can rewrite the above the expression after some algebra as:

$$\beta_1 = \frac{\text{Cov} \{y_m, \pi_m^A\}}{\bar{\pi}_m^A (1 - \bar{\pi}_m^A)}. \quad (11)$$

The key implication of (11) is that the expected sign of β_1 is governed by the expected correlation between each characteristic y_m and parapolitician support π_m^A . Thus, for instance, since we know that all else equal parapoliticians are able to exert more coercion and thus get more votes in places with weak institutions, $\text{Cov} \{y_m, \pi_m^A\} < 0$ when y_m is a measure of local institutions. This logic also underlines our expectations regarding the effects of the media scandal. For example, since following exposure of the parapolitician by the media we expect him to react by increasing coercion especially in places with poor institutions, we also have $\text{Cov} \{y_m, \pi_m^A\} < 0$ in regressions that include only the set of parapoliticians and compare those exposed before elections with those exposed after, as in equation (10).

Finally, since our framework also has implications for the votes that parapoliticians get (both relative to non-parapoliticians and as a result of exposure compared to other unexposed parapoliticians), we run regressions as (9) and (10) with each candidates' vote share as the dependent variable. When using equation (9) with vote share as the dependent variable, we expect $\beta_1 > 0$ since parapoliticians have an advantage over non-parapoliticians by exerting coercion. Instead, when using (10) the theoretical implications are ambiguous, depending on whether parapoliticians are able to compensate via coercion their reduced popularity. The interesting insight, however, is that β_1 need not be negative, since the endogenous response of coercion may avoid an electoral cost of the media scandal.

4 Data

4.1 Media exposure of Parapoliticians

Our main source of information is our own coding of news stories connecting national politicians with paramilitaries, based on the online archive of Colombia's largest newspaper with nation-wide circulation and coverage, *El Tiempo*. The coding process is described in detail in the Appendix (Section B). The dataset is event-based and relies on information on every news story (and op-ed)

published from January 1st 1997 to August 1st 2011 in *El Tiempo*. We searched for stories in every section of the newspaper about each of the incumbent congressmen in this 15-year period. For every story we record: the date, the newspaper section in which it appears, and whether it links the politician with ‘parapolitics’ activity, other corruption practices or an electoral crime (like fraud or vote-buying).

We focus in this paper on senators, elected in Colombia since the 1991 Constitutional reform on the basis of a single nationwide district. This reform, as well as subsequent changes in the electoral system in Colombia, have sought to create incentives for senators to appeal to a broader electorate (Roland & Zapata, 2004). While senators may still be elected successfully by obtaining a large share of their votes from a few municipalities or regions (and they often do), they capture more national attention and votes than candidates to the (Lower) House of Representatives. Indeed, candidates to the House are elected on the basis of regional districts (each of Colombia’s 32 Departments, the equivalent of the US State) and a number of special districts (as the Capital District of Bogotá and special districts for minorities and Colombians living abroad). Hence, they receive much less national attention and, crucial for our empirical strategy, coverage in national newspapers than national senators. This is shown in Table 1, which compares news coverage for Senate and House candidates. Even though this table only includes relatively well known candidates that had previously served as either senators or representatives (we limited our search to this set of candidates), there is an apparent difference in coverage between the two types of legislators. Indeed, House candidates get on average a third of the stories that Senate candidates get, and before elected and during the electoral periods these figure goes down to a fourth. For these reasons, we focus on senators. Moreover, to have a set of more comparable candidates, we focus on senators with previous experience as members of the Senate or the House of Representatives.

Based on our news search, we construct three key measures of the connectedness of each politician with the parapolitics scandal. First, we use a simple dummy variable that equals 1 if there are any stories connecting the candidate to paramilitaries at any point in time (we call this variable our *parapolitician dummy*). Second, with *percent parapolitics news* we attempt to take into account that some politicians are more deeply involved in the scandal than others. This variable is the percentage of total news stories of the candidate that relate him with the parapolitics scandal. We normalize by total candidate stories as some candidates are much more popular than others, and get greater press coverage. Finally, for several of our exercises, it is key to distinguish whether the politician in question was linked to parapolitics before or after his election. Hence, we also use a simple dummy variable, *news before elections*, that equals 1 if the candidate was linked to paramilitaries before the election.

Our method to identify a parapolitician has a number of advantages relative to those that have been used in the literature, particularly given our interest on the effects of media exposure. For instance, since several parapoliticians belong to ‘third parties’ (that is, parties other than

the Liberals, Conservatives, and the Socialists), some papers have relied on the rise of these non-traditional third parties in certain areas of the country as a proxy of the strength of parapoliticians (e.g. López (2007), Valencia (2007), and Acemoglu et al. (forthcoming)). Nevertheless, there is significant measurement error when relying on third parties as a proxy for being a parapolitician. On the one hand, some politicians in the traditional parties have been linked to paramilitaries, both by the press and by judicial institutions. On the other hand, some of the third parties, far from being dominated by politicians connected to paramilitaries, are in fact independent parties seeking alternatives to the traditional parties (perhaps the most notable example is the ‘Partido Verde’, which was particularly important player in the 2010 presidential campaign and also elected 5 senators in 2012). So, although as demonstrated by Acemoglu et al. (forthcoming), on average the vote share for third parties in legislative elections is indeed correlated with paramilitary presence, there is considerable noise in this relation. Our media-based measure is more precise and, of course, better suited for our specific purpose of verifying the implications of media exposure.

A second important advantage of our measures of parapoliticians has to do with the prevalence of political heirs. Judicial and disciplinary actions by government authorities have been taken against several parapoliticians, with some of them losing their seats as well as being sent to jail¹³. While unable to run for reelection, their relatives or political friends have run (sometimes successfully) for legislative seats. These “political heirs” have often been exposed by the media, and thus we also take them into account when evaluating the impact of media exposure on electoral outcomes.¹⁴

4.2 Paramilitary presence

To test the implications of our theory, we also need proxies for the extent of paramilitary presence, as well as indices for the quality of local institutions, which we proxy with measures of state presence and efficiency of the judiciary. To measure paramilitary presence, we use two data sources and four alternative measures. First, we use the paramilitary presence index from Acemoglu et al. (forthcoming). These authors rely on paramilitary attacks from think tank CEDE at Universidad de Los Andes in Bogotá, as well as data on displaced people by paramilitaries from Acción Social, an agency created by the presidency.¹⁵ Acemoglu et al. (forthcoming) use these data to construct

¹³The website <http://www.verdadabierta.com/parapolitics> follows the judicial cases against all politicians accused of ‘parapolitics’.

¹⁴Even when counting the heirs of parapoliticians investigated or sent to jail between 2006 and 2010 the number of senators involved with the scandal elected in 2010 decreased (from 37 to 31) relative to 2006 (MOE, 2010). This is consistent with our empirical results, which are mostly driven by the 2006 elections.

¹⁵CEDE collects data from the Observatory of Human Rights of the Vicepresidency and the National Department of Planning and aggregates variables in several categories by armed actor (paramilitaries and guerrillas) and type of action (explosive terrorist acts, incendiary terrorist acts, other terrorist acts, assaults to private property, attacks on civil organizations, political assassination attempts, road blockades, armed contact between state and non-state armed forces initiated by the latter, ambushes of civilians, harassing (mainly threats to civilians), incursion into ‘villages’, overland piracy, illegal checkpoints, armed forces wounded by the non-state armed group, murders of civilians, murders of politicians, massacres, deaths of members of the state armed forces, kidnappings of members

various measures of the presence of non-state armed actors. In particular, because the time series variation in both the attacks and the displaced measures is quite noisy, they focus on averages of these data (total paramilitary attacks between 1997 and 2005 per 1000 inhabitants, a dummy that takes the value of 1 if the municipality has a value of paramilitary attacks above the 75th percentile, and similar measures for displacement). They also use a complementary strategy and extract the principal component of the attacks and displacement measures, and use this principal component as their measure of paramilitary presence. The advantage of this strategy is that both attacks and displaced numbers are noisy, thus their common component may contain more information. We focus on this summary measure as our first measure of paramilitary presence. Our second measure is related, and is a dummy variable that equals 1 if the municipality is in the top 30th percentiles in the paramilitary presence index.

Our third and fourth measures of paramilitary presence are similar in spirit but rely on a completely independent data source, hence confirming the robustness of our results. In particular, we use an original event-based dataset colombian dataset of the Colombian conflict that covers the period 2002-2009. For each conflict event we record the date, location, type, perpetrator, and victims involved in the incident. We distinguish whether the incident was an uncontested *attack*, carried out by an identified armed group against a specific military or civilian target, or a *clash*, which involves an exchange of fire between two or more groups. We also record whether attacks were carried out by the guerilla, the paramilitary or the government, and the groups involved in a clash. Finally, we code the number of casualties separately for combatants and civilians. Our data updates the data of Restrepo, Spagat, and Vargas (2004) and relies on the same sources (and we have first used it in Fergusson, Robinson, Torvik, and Vargas (2012)). In particular, we create our database using events listed in the periodical *Noche y Niebla* published quarterly by the Colombian NGO CINEP. The publication presents a detailed description of chronologically ordered violent events in Colombia, including date of occurrence, geographical location, the group, or groups, deemed responsible for causing an event, individuals killed and injured, and the group to which the victims are thought to belong. As primary sources, *Noche y Niebla* relies on press articles from more than 20 daily newspapers with both national and regional coverage, as well as reports gathered directly by members of human rights NGOs and other organizations on the ground such as local public ombudsmen and, particularly, the clergy. Since the Catholic Church is present even in the most remote areas of the country, we can be confident of the coverage and accuracy of these data.

Using this source, we compute total paramilitary attacks per capita in each municipality over the period, and use this as our third measure of paramilitary presence. Our fourth measure is analogous

of the armed forces, kidnappings of politicians and kidnappings of civilians). The original data are a compilation of news stories from newspapers and from reports of the national police. Data on displaced population is collected from the Registro Único de Población Desplazada (unique register of displaced population). These data specify the municipality where the displacement originated, the year of the displacement and the armed actor that originated it.

to the second: a dummy variable that equals 1 for municipalities in the top 30th percentiles in paramilitary attacks per capita.

4.3 Local institutions: efficiency of the judiciary and state presence

As proxies for the efficiency of the judiciary at the local level, we compute two indices at the municipality level. Both rely on cases that entered the criminal justice system from 2008 to 2010.¹⁶ The criminal justice system aggregates crimes in 18 different categories ranging from homicide to terrorism/International Humanitarian Law violations and electoral crimes. We use all categories to compute our first index as follows:

$$\begin{aligned} \text{Efficiency Index}_m &= \frac{\text{Cases Closed}_m}{\text{Total Cases}_m} \times \frac{\text{Total Resolved Cases} - \text{Total Unresolved Cases}_m}{\text{Cases Closed}_m} \\ &= \frac{\text{Total Resolved Cases} - \text{Total Unresolved Cases}_m}{\text{Total Cases}_m}. \end{aligned}$$

Hence, our measure can be thought of as the efficiency of judges, adjusted for quality. Indeed, the first ratio in the first line of the expression measures the share of cases entering the judicial system that are resolved (efficiency). However, cases are often closed without resolution, meaning that either no one is found guilty, or terms expire and the judge is forced to close the case with no definite action. Thus, we adjust by the second ratio (quality): the difference between resolved and unresolved cases, normalized by total closed cases. Quality is therefore equal to -1 when all cases are closed unresolved, and 1 when all cases are closed with a meaningful resolution. In short, low values of the first ratio indicate congestion of cases and slow decision-making, and low values of the second translate to high levels of impunity. We also construct the same index including the subcategory of electoral crimes only, as efficiency along this dimension is presumably the most important one to deter electoral coercion. The source for these data is the National Office of the Attorney (*Fiscalía General de la Nación*).

We computed one state presence index using data on the total number of judges, attorneys and general prosecutors stationed in each municipality. In this sense our measure captures the presence of law enforcing institutions at the municipal level. We computed the index using principal components analysis, applied over the standardized variables. For the index we take the first principal component, which alone explains over 96% of the variance.

¹⁶Colombia started implementing a new criminal justice system in 2005, before which municipal judges were not forced to report systematically on the investigations carried out by them. Hence data on criminal judicial outcomes is sparse and unreliable for the years when the old system was in place. Because the adoption of the new system was staged in several years across different geographic regions, there is consistent data for all the municipalities starting from 2008.

A second version of the state presence index is similar to the first, but excludes the judges, attorneys and prosecutors whose mandate is to focus on investigating crimes that we judge likely to be unrelated with the incidence of paramilitary coercion and parapolitics activity. In particular, we exclude law enforcement offices that deal with sexual, intra-family and underage and juvenile crime, as well as labor crimes. We also exclude the attorney customer service offices.

4.4 Other variables and controls

Our theory also has implications for the overall success of parapoliticians, not just the distribution of their votes. Hence, we also rely on each candidate's vote share as a key dependent variable. We use the total number of votes for the candidate as a share of total votes for the senate at time t . Our source is the national electoral authority, the *Registroudría Nacional del Estado Civil*. From the same source, we also compute turnout in each municipality (the ration of total votes to total adult population).

As a placebo, we also use measures of guerrilla activity. In particular, we want to make sure that parapoliticians' votes are concentrated not simply where the Colombian conflict is strong, but rather where, specifically, paramilitary activity is significant. Hence, we also rely on measures of guerrilla activity. However, since guerrilla activity is correlated with paramilitary activity, we consider two variables which aim to identify places of high guerrilla presence but little paramilitary presence. Our first measure comes from López (2010): a dummy variable that equals 1 if there is evidence (investigation and judicial processes, ongoing or finalized) of political linkages between local politicians of a municipality (councilmen or mayor) and guerrillas, at any time in the last 15 years. Thus this is a measure of the capture of local institutions and decision-making by guerrilla groups. The information for this variable comes from a rich set of regional case studies conducted by the NGO *Corporación Nuevo Arco Iris*.

As an alternative measure, we also construct a dummy that equals 1 if guerrilla presence is in the top 30th percentiles of the guerrilla presence index *and* not in the top 30th percentile of the paramilitary presence index (where we use the paramilitary presence index of Acemoglu et al. (forthcoming) referred to before, and a guerrilla presence index from the same source with analogous construction).

We include a number of politician controls in our specifications to make sure that these are not conflating our results for the impact of the scandal. A key control is the total number of stories about the politician from our own press search, which may capture both the overall popularity of the candidate and his national exposure in the press. From *Congreso Visible*, an NGO that collects information on the Colombian Congress and makes it available through its website in an effort to increase transparency in the legislative process, we coded each politicians' gender, profession, whether the politician is a former member of the house or the senate, his years in politics, periods in office, and a dummy variable that equals one if the senator did not complete his last term.

Tables 2 and 3 present descriptive statistics for our main variables (at the municipality and candidate level, respectively). Also, in Table 3 we show the description of candidate characteristics for parapoliticians (those involved in the scandal at any point in time) and non-parapoliticians. It is interesting to observe some key correlations in these simple summary statistics. For instance, when looking at the municipality characteristics weighted by candidate vote share (our \bar{y}_{ct} variables), it is clear that parapoliticians get votes in areas with more paramilitary presence, less state presence, less efficiency of the judiciary (though the difference here is small) and less presence of the guerrilla (not paramilitaries). We next explore these correlation more carefully with our regression analysis.

5 Results

5.1 Distribution of electoral support for Parapoliticians

5.1.1 Paramilitary presence

We start by estimating equation (9) for our sample of senators using the average presence of paramilitaries in the municipalities supporting each candidate as the dependent variable (that is, using our paramilitary presence variables y_m to compute \bar{y}_{ct}).

In panel A of Table 4, we use the paramilitary presence index of Acemoglu et al. (forthcoming) as y_m . In column 1, we find that the parapolitician dummy is positive and highly significant. The estimated coefficient (1.036) implies that, after controlling for time fixed effects, politicians connected to the parapolitics scandal have on average a paramilitary presence index that is one-half of a standard deviation larger and more than 30% larger than the mean (2.79) of the sample. Columns 2 and 3 verify that this result is robust to using alternative measures for identifying a candidate as a parapolitician. Column 2 uses our ‘intensive’ measure of percent of candidate news stories that have to do with the parapolitics scandal. Again we find a positive (4.582) and very significant coefficient (the standard error is 0.874). This coefficient implies that a one-standard deviation increase in the percent of parapolitics stories (equal to 0.14 see Table 3) is associated with an average increase in the weighted index of paramilitary presence for such candidate of 0.64 (4.6×0.14).

Finally, column 3 uses the stories before elections, rather than those at any point as in column 1, to define the parapolitician dummy. Again, we find a positive and significant coefficient. It is slightly larger than the coefficient in column 2, which is suggestive of the idea that parapoliticians exposed before the elections compensate for the media scandal by exerting extra effort in paramilitary areas. However, the difference is small, and this is an issue that we study more carefully below when focusing on regressions *within* parapoliticians.

More important are the results from columns 4 to 6, which add our set of candidate controls to the regressions. The results are still highly significant and, also important, the coefficients are

remarkably stable. This suggests that the coefficient β_1 is not spuriously capturing other differences between parapoliticians and non-parapoliticians.

In panel B we present the same set of regressions as in panel A, but using the dummy for high paramilitary presence (municipalities above the 70th percentile according to the measure used in panel A) as our municipality presence variable. Given our rescaling of y_m variable for comparability, the dependent variable would take a value of 100 if all votes for candidate c came from places with high paramilitary presence, and 0 if no votes came from such area. Notice in column 1 that the parapolitician dummy increases the share of votes coming from high paramilitary areas by 8.4 percentage points. From an average of 22.2 this implies an index that is about 30% larger for parapoliticians. Columns 2 and 3 show that this positive and strong association is also observed when using the percent of candidate news stories about parapolitics and the dummy variable for parapolitics stories before the election. Also, as in panel A, once we control for candidate characteristics in columns 3 to 6, we observe virtually no changes in the coefficients and their significance.

In Table 5 we run the same specification as in Table 4 using our alternative source to code paramilitary presence. In panel A, the presence measure is the number of attacks per capita using our updated version of the Restrepo et al. (2004) dataset, whereas panel B relies on a dummy for high paramilitary presence (municipalities above the 70th percentile by attacks per capita). Our results are very similar to those from Table 4.

The main message from this first set of result is clear: parapoliticians get significantly more votes from areas with paramilitary presence.

5.1.2 Anecdotal discussion

A telling case study is that of politician Alvaro Araujo (see Acevedo (2010)). Coming from a family with a longstanding presence in the political arena of the department of Cesar, Araujo was elected to the House of Representatives for two consecutive legislative periods, in 1994 and 1998. Araujo's votes were then concentrated in northern Cesar, the political stronghold of his family. In 2002 Araujo was elected Senator with a geographical vote pattern substantially different than the one he had during his previous experience in the House: most of his votes were concentrated in southern Cesar, where his family traditionally had very little electoral support. Instead, this region was controlled by the Northern Block of the AUC, headed by the paramilitary warlord "Jorge 40". Extradited to the US in 2008 facing drug trafficking charges, Jorge 40 is more famous in Colombia for his participation in massacres, selective homicides and the forced displacement of civilians. In 2006 Araujo was accused by the leftist senator Gustavo Petro of having ties with Jorge 40. He was removed from the Senate and sent to jail in 2007. The men of Jorge 40 ensured the election of Araujo (as well as that of other politicians under their sponsorship) through a mix of fraudulent practices and the intimidation of voters in their region of influence. Particularly telling is the

kidnap of Araujo’s political competitor Victor Ochoa. Moreover, another rival political family from the region, the Gneccos, was effectively banned from politics through threats and intimidation.

More generally, paramilitaries of the Caribbean region (where Cesar is) have been accused of infiltrating the local branches of the National Registry Bureau to be able to manipulate votes and bought the support of oversight officials in electoral posts to be able to mark ballots (Semana, 2006a).

A second case study of interest is that of politician Juan Carlos Martinez, from the department of Valle (Cruz, 2010). Martinez’ links with the paramilitaries were revealed by paramilitary leader “HH” in 2008. According to HH, Martinez was elected Senator in 2002 largely with the support of the Calima block of the AUC. However, after the demobilization of this block in 2004 in the context of the AUC negotiation with the government, Martinez lost support in the areas controlled by the paramilitary in the south-western part of the country, notably Tumaco, Zarzal, Candelaria, Tuluá. In an effort to offset this loss, Martinez looked for the support of alias “Rasguño”, the chief of the “Norte del Valle” drug cartel (Laverde, 2012). Thus, Martinez’ geographical distribution of votes changed substantially from 2002 to 2006 and, in spite of the demobilization of his original illegal sponsors, Martinez was re-elected in the Senate with 3,354 more votes relative to 2002. Martinez’ 2006 votes were obtained in areas controlled by the drug cartel.

We now examine if the votes of parapoliticians are also concentrated in areas with weak institutions, proxied by the efficiency of the judiciary and measures of state presence.

5.1.3 Institutions

In Table 6 we examine whether parapoliticians concentrate their votes in places with a more inefficient judiciary. This table has the same structure as our previous two tables. In panel A, we use the efficiency index that includes all types of crimes as our key municipal variable. In column 1, we find that the parapolitician dummy is positive and highly significant. The estimated coefficient, while negative (-0.1), is not significant at conventional levels (the standard error is 0.0674). However, when using in column 2 our ‘intensive’ measure (percent of candidate stories having to do with parapolitics), we find a negative and very significant coefficient of -0.6 . The estimated coefficient implies that, after controlling for time fixed effects, a one-standard deviation increase in the percent of parapolitics stories is associated with an average decrease in the weighted index judiciary efficiency for such candidate of -0.08 from a sample mean of 10. Interestingly, in column 3, which uses the dummy for stories before elections to define parapoliticians, we do find a negative and very significant coefficient. Again, this is suggestive of the idea that parapoliticians exposed before the elections compensate for the media scandal by exerting extra effort in areas with weak institutions. Columns 4 to 6 add our set of candidate controls to the regressions, with very similar results.

In panel B we present the same set of regressions as in panel A, but using only electoral crimes to define the efficiency of the judiciary. Interestingly, all our estimations in this panel suggest a statistically significant correlation, unlike panel A where the simple parapolitician dummy in columns 4 to 6 was not significant. Since inefficiency in electoral crimes specifically is what could create incentives for electoral coercion by parapoliticians, this result is suggestive of the validity of our theoretical framework. Also, notice that our results from columns 1 to 3 are robust to controlling for candidate characteristics in column 4 to 6.

Moving to state presence, Table 7 examines whether parapoliticians concentrate their votes in places with less state presence. This table has the usual structure, with panel A using the presence index that includes all types of offices of the judicial branch. In column 1, we find that the parapolitician dummy is negative and highly significant. The estimated coefficient (-8.817 , with standard error of 1.585) implies that, after controlling for time fixed effects, the average state presence index for politicians connected to the parapolitics in municipalities that vote for them is almost exactly one-half as large as the mean of the sample. Column 2 uses the percent of candidate stories having to do with parapolitics and again finds a negative and very significant coefficient. Similar results are found with the dummy for news stories before elections in column 3, and all these results are virtually unchanged with the inclusion of candidate controls in columns 4 to 6. The results in panel B are also similar, which exclude the law enforcement offices likely to be unrelated with the incidence of paramilitary coercion when computing the state presence index.

Thus, Tables 4 to 7 are largely supportive of our predictions in Propositions 1 and 2. In particular, they reveal that politicians that have colluded with paramilitaries get disproportionate votes from areas with significant paramilitary activity and weak institutions. A major concern, however, is that this could not simply be the result of such politicians exerting stronger coercion in such areas, the main underlying mechanism suggested in the model. For instance, it could be that such areas are also places with ideological leanings that both create a favorable environment for paramilitary activity and increase support for certain types of candidates who happen to sympathize with such illegal groups. Since our results are virtually unchanged when we control for candidate characteristics, this possibility seems unlikely. However, to be more confident about coercion being the key channel, we need a set of more comparable candidates sharing, for instance, similar ideological leanings. We turn to such an exercise next and, moreover, we focus on the implications of media exposure specifically, the main focus of our theory.

5.2 The media scandal and the distribution of electoral support

We now estimate equation (10) only for the sample of parapoliticians. Our key independent variable of interest is exposure before the election: the dummy variable that equals 1 if the candidate was linked to the parapolítica scandal before the election at time t . As noted, since this regression includes only the set of parapoliticians, it allows us to check the effect of the media scandal,

excluding the overall differences in the patterns of electoral support between parapoliticians and non-parapoliticians that might contaminate the results of Tables 4 to 7.

For the coefficient on exposure to measure the effect of the media scandal, we need the set of parapoliticians exposed before elections to be otherwise comparable to the set of parapoliticians exposed after the election. To deal with a potential problem stemming from possible differences between those who manage to stay under the radar before the elections and those who do not, we include as before the set of candidate controls \mathbf{X}_c to control for differences between them. But, moreover, we can examine whether there are any observable significant differences between these two sets of parapoliticians.

This is done in Table 8. The table shows a regression for the dummy variable that equals 1 if the politician is exposed *before* elections on a full set of year fixed effects and our set of observable candidate controls \mathbf{X}_c . Hence, the coefficient of each of the observable characteristics tells us if, within each election, these candidate traits help predict the exposure dummy. It is reassuring that there are few significant differences in our set of observable candidate controls. Indeed, only the average periods in congress (at the 10% level) and the total number of news stories in *El Tiempo* (at the 5% level) are significant. Moreover, in column 2 we include only non-paramilitary related news stories as a regressor, instead of the total number of stories for the candidate. In this case, the coefficient is not significant, showing that the differences in total stories corresponds to the parapolitician exposure itself. Hence, instead of differences in popularity or newsworthiness between politicians exposed before or after the elections, the difference in column 1 simply reflects the fact that exposed parapoliticians mechanically have more stories as their links with paramilitaries are being reported. Overall, therefore, we take Table 8 as giving strong evidence that our results are unlikely to be driven by a selection bias between parapoliticians exposed before and after elections. Moreover, to give more credence to our results, we nonetheless control for observable characteristics in all the regressions that follow.

Notice also that our results comparing parapoliticians with non-parapoliticians and those comparing, within parapoliticians, those exposed before and those exposed after elections, if contaminated with omitted variable bias, are likely to be biased in opposite directions. In particular, a concern when looking at parapoliticians relative to non-parapoliticians is that the former may have better ability than those who were not able to make deals with paramilitaries. Instead, among parapoliticians, those exposed before presumably lacked the ability to avoid leaks of their relationship with paramilitaries by the media. Since the omitted (and difficult to measure) ability is positive in one case and negative in the other, and since both results point nonetheless in the same direction, we see this as suggesting that our results are unlike to be driven by omitted ability bias.

5.2.1 Paramilitary presence

We start by estimating equation (10) for our sample of senators using the average presence of paramilitaries in the municipalities voting for each candidate as the dependent variable.

In panel A of Table 9 we use each of our four paramilitary presence variables in columns 1 to 4 (respectively, the presence index of Acemoglu et al. (forthcoming), the dummy for high presence based on such index, attacks per capita using our attacks dataset, and the dummy for high presence based on attacks). In all columns, we find a positive coefficient for our exposure variable. It is also statistically significant except in the case of columns 2 and 3 using the dummy variable for high presence and our own paramilitary attacks measure. Notice also that the estimated coefficient is close to the one for the paramilitary dummy in column 1 of Tables 4 and 5. Recall, moreover, that although we find parapoliticians exposed before and after elections to be similar on covariates, all these regressions control for our full set of candidate controls¹⁷. These regressions therefore suggest that exposed parapoliticians concentrated their votes in areas of paramilitary presence more than the unexposed ones.

5.2.2 Institutions

Panel B of Table 9 uses the average quality of institutions in the municipalities voting for each candidate as the dependent variable. Similar to the previous table, we use each of our two efficiency of the judiciary and two state presence indices in columns 1 to 4 (respectively, the efficiency of the judiciary in electoral crimes, the efficiency in all crimes, the state presence index using all offices, and the state presence index using only the main offices). All regressions include candidate controls. In all columns, we find as expected a negative coefficient for our exposure variable. It is also statistically significant except in the case of column 1. Again, this is suggestive that exposed parapoliticians concentrated their votes in areas of weak institutions more than the unexposed ones.

5.2.3 Anecdotal discussion

Take again the case of politician Alvaro Araujo discussed in section 5.1.1. Even if Araujo's 2002 Senate campaign had the support of Jorge 40 and the Northern Block of the AUC, this was not made public until well after Araujo was elected. The first accusations linking Araujo and Jorge 40 came from journalist Claudia López in 2005 (Semana, 2006b). As a result, in the 2006 elections Araujo lost most of his electoral support in places with a stronger institutional background. For instance, compared to the 2002 elections, Araujo lost 12,000 votes in capital city Bogotá. However, he reached again for the support of Jorge 40, who ordered his men to get him votes in the department

¹⁷Without candidate controls, or including only those covariates that are significantly different for politicians exposed before and after according to Table 8, the magnitude of the coefficients is very similar, and the estimate in column 3 is significant at conventional levels.

of Atlántico in addition to Cesar. This strategy was successful and Araujo went from 146 votes in 2002 in Atlántico’s capital (Barranquilla) to 6,752 in 2006. Overall, having been exposed as parapolitician before the 2006 elections, Araujo only lost in total 2,848 votes relative to 2002. He was elected Senator for the second time.

Other incumbent Senate candidates had similar trajectories. For instance, Dieb Maloof, Vicente Blel and Habib Merheg all were accused by the media of their ties with well known paramilitaries (respectively Jorge 40, Diego Vecino and Salvatore Mancuso, and alias “Rasguño” and “Macaco”) before the 2006 elections. As a result, even if the three politicians had belonged to the Liberal Party in the past, they were denied the opportunity to use the name of that party in the elections, or that of any other of the main political parties for that matter. The politicians founded their own party, “Colombia Viva” and were elected to the Senate with votes concentrated in paramilitary-dominated areas of departments of Magdalena (Maloof), Bolivar (Blel) and Risaralda (Merheg).

5.3 The success of Parapoliticians

Our framework implies that the vote share of parapoliticians should be larger than that of non-parapoliticians, since parapoliticians have an advantage over non-parapoliticians by exerting coercion. Moreover, when focusing within parapoliticians, it also implies that the effect of exposure need not be negative, since the endogenous response of coercion may avoid an electoral cost of the media scandal. To test these implications, we now run regressions (9) and (10), with candidate vote share as the dependent variable.

Table 10 presents the results. In columns 1 to 3, for each of our three variables to identify parapoliticians (the simple dummy, the percent of parapolitics news stories, and the dummy for news stories before elections), we find that parapoliticians are indeed much more successful than non-parapoliticians. For example, the estimated coefficient in column 1 implies that politicians connected to the parapolitics scandal have on average a vote share that is .09% larger than the average of the sample, which is at 0.41%. Also, these results are robust to the inclusion of candidate controls in columns 4 to 6.

More interesting, columns 7 and 8 compare the vote shares of exposed and unexposed parapoliticians. The estimated coefficient for exposure is positive, and not significant. That is, exposed parapoliticians did no worse than unexposed parapoliticians. Taken together with the rearrangement of the distribution of votes towards areas with more paramilitary presence and weaker institutions, this result is large consistent with the mechanisms of our model: the media scandal prompted parapoliticians to double-down on coercion in places where this was less costly, and by doing this they compensated for the overall fall in popularity among free voters.

5.4 Parapoliticians and unusually high turnout

Table 11 presents an additional piece of evidence that supports the purported underlying mechanism that explains the voting patterns for parapoliticians and their change following exposure. In this table, we present regressions with average unusually high turnout in the municipalities voting for each candidate as the dependent variable. In particular, for each of our election years, we calculate turnout in the legislative elections (the ratio of total votes to population over 18 years of age), and construct a high turnout dummy which equals 1 if the municipality is above the 70th percentile of turnout. Our dependent variable is then simply our standard \bar{y} variable as defined in (8) with “high turnout” as the municipality characteristic that we aggregate using candidate vote shares.

Columns 1 to 6 present specifications as in (9), that is, including all candidates and verifying whether parapoliticians get their votes from municipalities with unusually high turnout. Columns 1 to 3 indicate that this is indeed the case, for any of our parapolitician measures (the parapolitician dummy, the percent of parapolitica news, and the dummy for news before elections). Columns 4 to 6, moreover, show that these results are robust to controlling for candidate characteristics.

Columns 7 (without candidate controls) and 8 (with candidate controls), on the other hand, look at parapoliticians only and focus on the effects of being exposed by the media, running specifications as in (10). Again, we find that parapoliticians exposed by the media before elected are more likely to get their votes from areas with unusually high turnout than those exposed once elected.

These results lend further support to the idea that paramilitary coercion is the underlying mechanism explaining our results. In particular, while there are various (fraudulent) methods that politicians in general could use to get electoral support, and to offset the negative impact of a media scandal, anecdotal and judicial evidence in the case of parapoliticians in Colombia points to methods of coercion that would tend to inflate vote totals.

Some of these methods were already mentioned in our discussion of anecdotal evidence. Acemoglu et al. (forthcoming) also offer a useful summary of the parapoliticians fraudulent practices. As they point out, “A salient strategy seems to have been to terrorize people into voting for specific candidates. In the municipality of San Onofre in the coastal department of Sucre, for example, this was arranged by the paramilitary leader ‘Cadena’: for the elections of 2002, the trucks sent by ‘Cadena’ went through neighborhoods and rural areas of San Onofre picking people up. According to some people in this municipality in Sucre, thousands of peasants were taken to the corregimiento ‘Plan Parejo’ so they could see the candidate for whom they had to vote for in the legislative elections.” Admittedly, terror was also used to keep people away from the polls so, but this was done so “that ballot stuffing and other forms of manipulation of vote totals could occur.” Moreover, “another strategy, where coercion also played an important role, involved collecting people’s cedula (national identity cards which a person must produce to vote) from their houses, using them to collect the ballots (the ‘tarjeton’) and filling them in for people.” All of these strategies should have increased the likelihood of unusually high turnout in places where parapoliticians could exert coercion, and

Table 11 indeed suggests that parapoliticians, especially when exposed, got votes from places with unusually high turnout areas.

We do not claim, however, that every form of parapolitician fraud involved armed coercion, nor that it implied increased vote totals. For instance, in judicial investigations it was revealed that the Presidential intelligence service (Administrative Security Department, DAS), now closed after these and other scandals, designed a computer program to create counterfeit ballots to include fake votes of the people who did not vote, and to replace those of voters who did turn out to vote for candidates allied with paramilitaries. However, since most strategies did imply increased vote totals, and especially in the places where coercion is more likely, we expect the positive association that is apparent in Table 11.

5.5 Further robustness

5.5.1 High guerrilla areas: a placebo

We want to make sure that parapoliticians votes are concentrated not simply where the Colombian conflict is strong, but rather where, specifically, paramilitary activity is significant. For example, since guerrilla activity is correlated with paramilitary activity, we may be spuriously finding a correlation that really stems from guerrilla, not paramilitary, presence. Hence, in this section, as a placebo, we also use measures of guerrilla activity and examine the distribution of parapolitician votes along this dimension as well as how they changed with the media scandal. As noted before, we consider two variables which aim to identify places of high guerrilla presence but little paramilitary presence: a dummy variable that equals 1 if the municipality has been captured by the guerrilla, and a dummy that equals 1 if guerrilla presence is in the top 30th percentiles of the guerrilla presence index *and* not in the top 30th percentile of the paramilitary presence index.

Table 12 presents the results for all politicians. The message is clear: parapoliticians votes are not concentrated in areas with high guerrilla presence. If anything, they are concentrated in places without a strong guerrilla presence. Table 13 sends a similar message regarding exposed parapoliticians within the set of parapoliticians: following exposure, their votes are not more concentrated in areas with high guerrilla presence. Again, if anything, they shift away from such areas.

5.5.2 Checking media bias

We used the electronic archive of *El Tiempo*, the largest newspaper with nation-wide coverage in Colombia, to code the extent to which every incumbent Senate candidate was allegedly involved with the parapolitics scandal. We rely on this media outlet for practical reasons: *El Tiempo* is the only nation-wide newspaper in Colombia with an electronic archive going back beyond the period in which the media scandal broke out. In particular, *El Tiempo's* electronic archive currently goes back to 1991.

Focusing on this one paper is, however, a potential source of concern. Indeed, *El Tiempo* has traditionally been viewed as a pro-government, center-right newspaper. Moreover, since 55% of the paper's shares were bought by Spain's media conglomerate *Grupo Planeta* in August 2007, the perception that *El Tiempo* has further leaned to the right is not uncommon.

This is potentially problematic if *El Tiempo's* editorial position affected the objectivity of its news coverage during the period of analysis. For instance, *El Tiempo* supported Uribe's reelection in 2006, at the time the parapolitics scandal was gaining momentum (see Figure 1) and most of the incumbents then exposed as parapoliticians belonged to the president's coalition. It may be argued that, by relying on *El Tiempo* to code the intensity of the involvement of incumbent senate candidates with the scandal we are underestimating the scope of the parapolitics phenomenon. This in turn may be a source of bias for our results.

To address this possibility we coded the news coverage of all incumbent Senate candidates and their alleged involvement with parapolitics by *El Espectador*, which is the second largest nationwide newspaper in Colombia. *El Espectador* has a reputation of independence, and a track record of exposing political (and other kind of) misdeeds. For instance, *El Espectador* denounced the drug mafias in the 1980s and their links with politics like no other medium. In 1986, the paper's lifetime director, Guillermo Cano, was murdered by Pablo Escobar in retaliation. Also, in 2007, marking a fundamental dissociation with *El Tiempo's* editorial stance, *El Espectador's* director criticized president Uribe (Semana, 2007).

Using the *El Espectador* data, we can contrast the *El Tiempo* coverage on parapolitics-related news stories for all the politicians in our sample. One caveat is that the electronic archive of *El Espectador* is not available before October 2007. Hence for the comparison we rely on the overlapping period of available information from the two papers, spanning between October 2007 and August 2011. Candidates exposed only by *El Tiempo* before this period, are dropped from our regressions.

For each paper we rank our sample of politicians according to the ratio of parapolitics-related stories to the total number of stories on each individual. We use this rough measure of intensity in the involvement with the scandal to classify politicians into quintiles of parapolitics news coverage. Comparing the two papers suggests that 84% of the politicians are in the same quintile of scandal intensity. Less than 5% of all the candidates that are exposed by *El Tiempo* as parapoliticians are not exposed by *El Espectador*. Similarly, less than 5% of all the candidates that are exposed by *El Espectador* are not exposed by *El Tiempo*. More generally, we conduct a combined Kolmogorov-Smirnov test to investigate whether the distribution of candidates according to their share of parapolitics-related stories is the same across papers. The resulting p-value is near 1, indicating that the null that the two distributions are statistically indistinguishable cannot be rejected.

Moreover, in Table 14 we use these classifications of parapoliticians (based on news searches from *El Tiempo* and *El Espectador*, but relying on the overlapping period of available information

from the two papers from October 2007 to August 2011) to redo our main regressions characterizing the electoral success and vote distribution of parapoliticians. Hence, column 1 looks at regressions for vote share, whereas columns 2 to 7 present regressions for the municipality characteristics (paramilitary presence, judicial efficiency, and state presence measures) weighted by candidate municipal vote shares. In panel A, the independent variable is a parapolitician dummy based on *El Espectador* news searches, and in panel B the same variable is based on *El Tiempo* news searches.

The results suggest that there are no big biases in our results by focusing on *El Tiempo* news archive. Indeed, both panels reveal that parapoliticians are electorally more successful (column 1), and that their votes are relatively concentrated in paramilitary areas (columns 2 and 3), in areas with more inefficient judicial institutions (columns 4 and 5), and in areas with less state presence (columns 6 and 7). Even the size of the estimated coefficients on the parapolitician dummy are similar when relying on *El Espectador* or *El Tiempo* news searches. In fact, if anything *El Tiempo* underestimates the consequences of exposure¹⁸.

In short, our results are unlikely to be contaminated by possible biases in our main news source, *El Tiempo*, concerning exposure of parapoliticians.

6 Final remarks

A widespread belief that has been confirmed by recent research on the economics of media is that, by uncovering incompetent or corrupt politicians, free mass media enhance political accountability and help voters make better decisions. When considering weaker overall institutional environments, most of the literature has focused on situations in which the media is not free, but rather it is captured by political or economic interest groups. This impedes the free flow of unbiased information that Jefferson had wished for. However, even when weak institutions predominate, the media sometimes still plays an active and unbiased role of providing useful information about political corruption or other wrongdoings by politicians.

In this paper, we argue that in such circumstances the disinfectant role of media can be limited and even have unintended negative consequences. For instance, an uneven democracy in which vote casting is not completely free may provide the incentives for certain type of politicians to concentrate their constituencies in places in which coercion or fraud is easier to undertake, even more so if exposed and demonized by mass media. We explored this *dark side* of an active mass media, first by laying out a simple political economy model that features a free media, but a fraction of politicians who compete in elections that might seek votes in non-democratic ways, and second by offering empirical evidence for the Colombia Senate elections of the past 10 years.

¹⁸Also, within this sample, when looking at concentration of votes in judicially efficient places using all set of crimes, the coefficient on the parapolitician dummy using either news source is negative, but is significant only in the case of *El Espectador*.

We derive testable implications from our model, namely that a media scandal that exposes tainted candidates generates an increase in the coercion exerted by these politicians in weakly institutionalized environments. This happens in order to offset the media-driven negative popularity shock. Hence, on top of the direct cost of coercion, an unintended consequence of the media scandal is that exposed candidates may not even see a reduction in their vote share. An additional negative consequence, since coercion increases, is that the equilibrium attention from politicians to voters in coerced areas decreases, as less voters respond to policy platforms in these areas. In sum, having a free and active media where other desirable institutions like free and fair elections are not guaranteed may result in unintended negative outcomes.

Our empirical results focus on one salient episode of the recent political history of Colombia, the so called ‘parapolitics’ scandal, that featured national and local politicians colluding with illegal armed paramilitary groups to obtain votes by exerting violent coercion in areas of little state presence. This scandal was uncovered by the mass media and this led to judicial investigations that ended up in many of the involved politicians being sent to jail. However, as we showed, it also had negative consequences. We look at the 2002, 2006 and 2010 Senate elections and show that incumbent candidates who are exposed by the national press to allegedly have ties with paramilitaries, are not only more successful than non-exposed politicians (in terms of their vote share), but also more likely to get their votes in areas controlled by the armed group (but not in areas controlled by other armed groups) and less likely to obtain votes in areas with higher presence –and better quality– of law enforcement institutions.

Perhaps more interesting (and harder to reconcile with potential alternative explanations), we provided evidence on the effects of the media scandal by showing that within the set of candidates exposed by the mass media as allegedly having ties with the paramilitaries, those exposed *before* elections are also more likely to get their votes from paramilitary-controlled regions and less likely to get them from places with strong institutions. Also, this rearrangement in vote distribution seems to counteract the effects of media scandal, as these set of candidates fare no worse in elections.

To complement Brandeis famous remark, sunlight may well be the best of disinfectants, but not when one can hide in the shade. In Colombia, the media exposure of parapoliticians sent some of them further into the shade.

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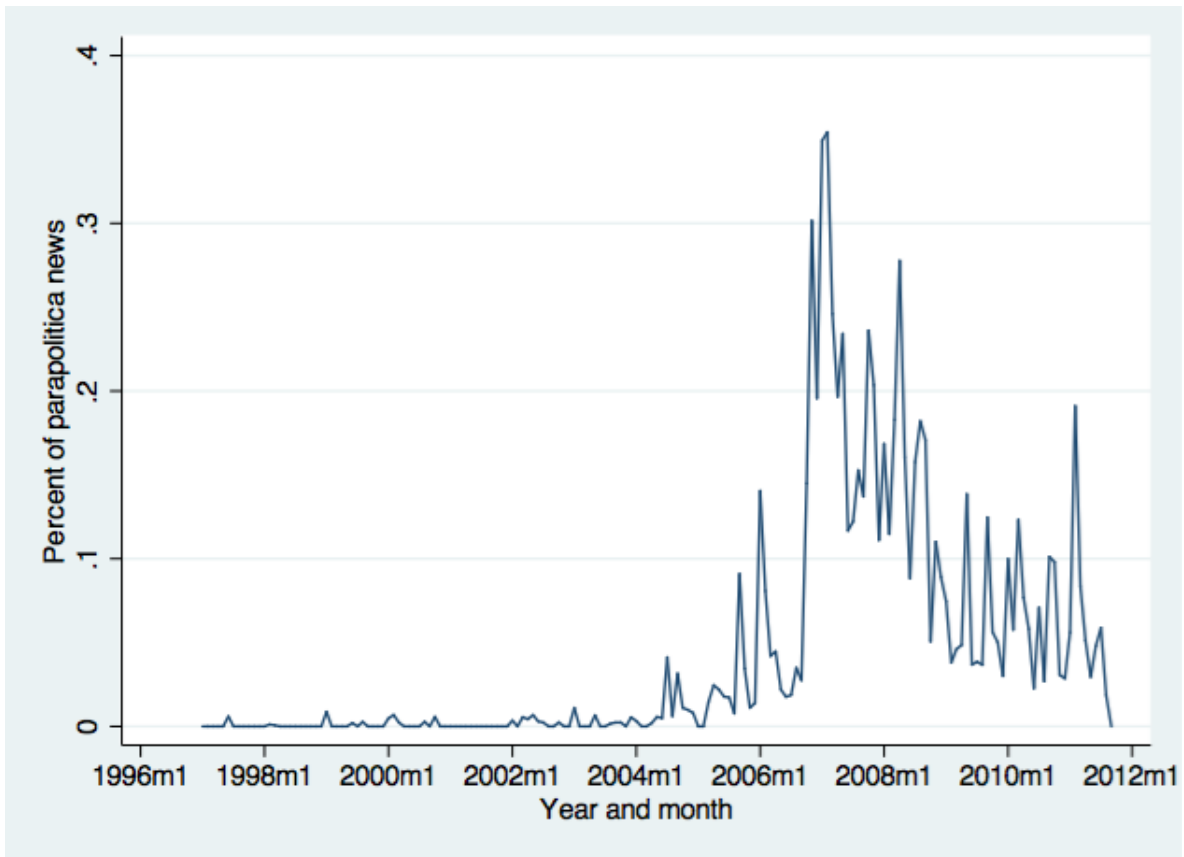
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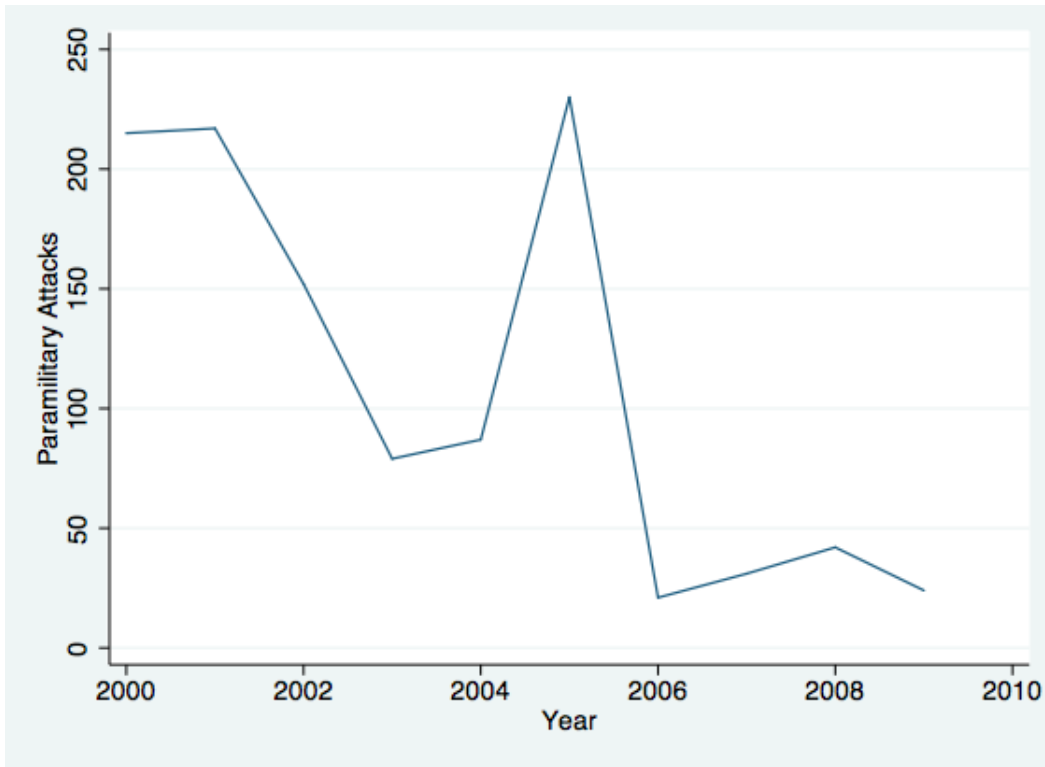
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Figure 1: Percent of ‘parapolítica’ news stories, out of all recorded stories about Congress candidates



Authors' coding from *El Tiempo* online archive, see Appendix section B for details. The sample of politicians included are all candidates to the House or Senate elections with previous experience either in the House or Senate.

Figure 2: Evolution of paramilitary attacks: 2000-2009



Source: Authors' update of Restrepo et al. (2004).

Table 1: Coverage of Senate vs House candidates

	Senate Candidates	House Candidates
Number of incumbent candidates	356	322
Average stories per candidate	362 (625)	109 (231)
Average stories before the election	183 (298)	55 (77)
Average stories during the electoral period	93 (163)	28 (46)

Standard deviations in parenthesis. Source: Authors' coding from *El Tiempo* online archive, see Appendix section B for details. The sample of politicians included are all candidates to the House or Senate in the 2002, 2006 and 2010 elections with previous experience either in the House or Senate.

Table 2: Municipal level variables, descriptive statistics

Variable	Mean	Standard deviation	Min	Max
Paramilitary presence (ARS measure)	5.4	9.559	0	100
Dummy for high paramilitary presence (ARS measure)	40.259	49.042	0	100
Paramilitary attacks per capita	5.412	10.898	0	100
Dummy for high paramilitary attacks per capita	41.126	49.206	0	100
State presence index	.551	3.799	0	100
Sate presence index (only main offices)	.645	3.986	0	100
Judicial efficiency index (only electoral crimes)	48.183	11.072	0	100
Judicial efficiency index (total)	10.329	3.98	0	100
Dummy for municipality captured by guerrilla	3.163	17.502	0	100
Dummy for high guerrilla attacks per capita	12.98	33.608	0	100
Dummy for unusually high turnout	39.413	48.866	0	100

All displayed variables are scaled to vary between 0 and 100 for comparability. In particular, we compute $y_m = 100 \frac{\tilde{y}_m - \min_m \tilde{y}_m}{\max_m \tilde{y}_m - \min_m \tilde{y}_m}$, where \tilde{y}_m is the unscaled municipality characteristic. For sources and variable definitions see Section 4.

Table 3: Candidate level variables, descriptive statistics

Variable	All politicians				Parapoliticians		Non-parapoliticians	
	mean	std. dev.	min	max	mean	std. dev.	mean	std. dev.
Panel A: Involvement with parapolitics scandal								
Parapolitician Dummy	.34	.47	0	1	1	0	0	0
Perc. parapolitics news	.06	.14	0	.86	.17	.2	0	0
News stories before elections (Yes=1)	.19	.39	0	1	.55	.5	0	0
Panel B: Municipality characteristics, weighted by municipal candidate vote share								
Paramilitary presence (ARS measure)	2.79	2.05	0	22.21	3.45	2.53	2.46	1.66
Dummy for high paramilitary presence (ARS measure)	22.2	14.4	0	81.430	27.6	16.92	19.47	12.09
Paramilitary attacks per capita	2.74	1.92	0	14.69	3.26	1.98	2.48	1.83
Dummy for high paramilitary attacks per capita	48.26	19.15	0	90.28	55.86	18.15	44.41	18.51
Judicial efficiency index (total)	10.13	.8	0	13.34	10.01	1.06	10.18	.63
Judicial efficiency index (only electoral crimes)	41.87	5.36	0	53.9	40.82	5.98	42.4	4.95
State presence index	19.03	15.97	0	79.07	12.83	11.89	22.18	16.85
Sate presence index (only main offices)	19.8	16.03	0	79.22	13.5	12.01	22.99	16.87
Dummy for municipality captured by guerrilla	2.28	6.23	0	54.65	1.99	6.1400	2.43	6.28
Dummy for high guerrilla attacks per capita	4.23	4.61	0	27.57	3.72	4.07	4.48	4.85
Dummy for unusually high turnout	24.52	19.03	1.1	96.72	32.59	22.77	20.47	15.37
Panel C: Candidate level outcomes and controls								
Candidate vote share	.0041	.0028	.0001	.0209	.00466	.00225	.00378	.00294
Average News in <i>El Tiempo</i>	361.9	624.88	1	4769	355.13	336.78	365.29	728.44
Male	.88	.32	0	1	.87	.34	.89	.31
College Degree	.9	.3	0	1	.89	.31	.91	.29
Replaced Candidate	.04	.2	0	1	.04	.2	.04	.2
Average Years in Politics	13.18	8.71	2	46	12.29	8.18	13.63	8.94
Average Periods in Congress	2.1	1.37	1	11	2.08	1.52	2.12	1.29

Source for Panel A is authors' coding from *El Tiempo* online archive, see Appendix section B for details. The sample of politicians included are all candidates to the Senate in the 2002, 2006 and 2010 elections with previous experience either in the House or Senate. In Panel B, for each candidate c and time period t we compute $\bar{y}_{ct} = \sum_m \pi_{mt}^c y_m$, for municipality characteristic y_m and with π_{mt}^c , the vote share of candidate c (out of his own total votes) in municipality m at time t . In Panel C, except for candidate vote share which is from Official Sources (*Registradura Nacional del Estado Civil*), candidate controls are from the ONG *Congreso Visible*.

Table 4: Parapoliticians: Vote concentration in paramilitary areas I

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: Dependent variable: paramilitary presence (ARS measure) weighted by vote share</i>						
Parapolitician Dummy	1.036*** (0.267)			1.056*** (0.266)		
Perc. parapolitics news		4.582*** (0.874)			4.625*** (0.896)	
News before elections (Yes=1)			1.134*** (0.385)			1.163*** (0.364)
Constant	2.120*** (0.158)	2.146*** (0.144)	2.412*** (0.152)	2.016*** (0.513)	2.044*** (0.459)	2.391*** (0.512)
R-squared	0.069	0.109	0.056	0.091	0.129	0.079
<i>Panel B: Dependent variable: dummy for high paramilitary presence (ARS measure) weighted by vote share</i>						
Parapolitician Dummy	8.427*** (1.764)			8.606*** (1.739)		
Perc. parapolitics news		33.50*** (7.298)			33.37*** (7.228)	
News before elections (Yes=1)			8.217*** (2.314)			8.453*** (2.267)
Constant	17.54*** (1.310)	17.98*** (1.282)	19.93*** (1.421)	17.15*** (4.004)	17.75*** (3.870)	20.26*** (4.319)
R-squared	0.084	0.113	0.055	0.100	0.123	0.071
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Candidate controls				Yes	Yes	Yes
Observations	356	356	356	356	356	356
Number of candidates	254	254	254	254	254	254

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Candidate-level regressions. The sample of politicians included are all candidates to the Senate in the 2002, 2006 and 2010 elections with previous experience either in the House or Senate. ‘Parapolitician Dummy’ equals 1 if there are any stories connecting the candidate to paramilitaries at any point in time. ‘Percent parapolitics news’ is the percentage of total news stories of the candidate that relate him with the parapolitics scandal. ‘News before elections’ is a dummy variable that equals 1 if the candidate was linked to paramilitaries before the corresponding election. Candidate controls in columns 4 to 6 include: number of news stories about the politician, gender, profession, years in politics, periods in office, and incomplete term dummy.

Table 5: Parapoliticians: Vote concentration in paramilitary areas II

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: Dependent variable: paramilitary attacks per capita weighted by vote share</i>						
Parapolitician Dummy	0.798*** (0.213)			0.793*** (0.213)		
Perc. parapolitics news		3.622*** (0.883)			3.643*** (0.885)	
News before elections (Yes=1)			0.814*** (0.265)			0.850*** (0.266)
Constant	2.164*** (0.169)	2.178*** (0.160)	2.389*** (0.162)	2.262*** (0.477)	2.268*** (0.459)	2.544*** (0.482)
R-squared	0.051	0.082	0.039	0.060	0.091	0.051
<i>Panel B: Dependent variable: dummy for high paramilitary attacks per capita weighted by vote share</i>						
Parapolitician Dummy	11.72*** (2.029)			12.01*** (2.037)		
Perc. parapolitics news		37.43*** (7.239)			38.72*** (7.349)	
News before elections (Yes=1)			11.34*** (2.431)			12.16*** (2.420)
Constant	41.23*** (1.906)	42.40*** (1.881)	44.55*** (1.902)	42.13*** (4.757)	43.65*** (4.809)	46.45*** (4.844)
R-squared	0.099	0.091	0.066	0.120	0.112	0.091
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Candidate controls				Yes	Yes	Yes
Observations	356	356	356	356	356	356
Number of candidates	254	254	254	254	254	254

Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Candidate-level regressions. The sample of politicians included are all candidates to the Senate in the 2002, 2006 and 2010 elections with previous experience either in the House or Senate. ‘Parapolitician Dummy’ equals 1 if there are any stories connecting the candidate to paramilitaries at any point in time. ‘Percent parapolitics news’ is the percentage of total news stories of the candidate that relate him with the parapolitics scandal. ‘News before elections’ is a dummy variable that equals 1 if the candidate was linked to paramilitaries before the corresponding election. Candidate controls in columns 4 to 6 include: number of news stories about the politician, gender, profession, years in politics, periods in office, and incomplete term dummy.

Table 6: Parapoliticians: Vote concentration in areas with efficient judiciary

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: Dependent variable: efficiency index index (total) weighted by vote share</i>						
Parapolitician Dummy	-0.100 (0.0674)			-0.0893 (0.0671)		
Perc. parapolitics news		-0.603*** (0.174)			-0.585*** (0.188)	
News before elections (Yes=1)			-0.148** (0.0728)			-0.127* (0.0733)
Constant	10.18*** (0.0650)	10.19*** (0.0630)	10.15*** (0.0612)	10.03*** (0.144)	10.04*** (0.143)	9.995*** (0.144)
R-squared	0.009	0.023	0.012	0.042	0.055	0.044
<i>Panel B: Dependent variable: efficiency index (only electoral crimes) weighted by vote share</i>						
Parapolitician Dummy	-1.143** (0.562)			-1.165** (0.551)		
Perc. parapolitics news		-3.239* (1.737)			-3.768** (1.754)	
News before elections (Yes=1)			-1.115* (0.673)			-1.283** (0.648)
Constant	42.04*** (0.552)	41.90*** (0.535)	41.72*** (0.524)	43.02*** (1.244)	42.87*** (1.222)	42.60*** (1.205)
R-squared	0.022	0.018	0.017	0.092	0.091	0.089
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Candidate controls				Yes	Yes	Yes
Observations	356	356	356	356	356	356
Number of candidates	254	254	254	254	254	254

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Candidate-level regressions. The sample of politicians included are all candidates to the Senate in the 2002, 2006 and 2010 elections with previous experience either in the House or Senate. ‘Parapolitician Dummy’ equals 1 if there are any stories connecting the candidate to paramilitaries at any point in time. ‘Percent parapolitics news’ is the percentage of total news stories of the candidate that relate him with the parapolitics scandal. ‘News before elections’ is a dummy variable that equals 1 if the candidate was linked to paramilitaries before the corresponding election. Candidate controls in columns 4 to 6 include: number of news stories about the politician, gender, profession, years in politics, periods in office, and incomplete term dummy.

Table 7: Parapoliticians: Vote concentration in areas with strong state presence

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: Dependent variable: state presence index (total) weighted by vote share</i>						
Parapolitician Dummy	-8.817*** (1.585)			-9.028*** (1.501)		
Perc. parapolitics news		-29.10*** (3.233)			-29.83*** (3.232)	
News before elections (Yes=1)			-8.009*** (1.848)			-8.712*** (1.783)
Constant	22.23*** (1.713)	21.41*** (1.674)	19.73*** (1.651)	25.51*** (3.762)	24.43*** (3.714)	22.25*** (3.724)
R-squared	0.080	0.078	0.049	0.185	0.181	0.157
<i>Panel B: Dependent variable: state presence index (only main offices) weighted by vote share</i>						
Parapolitician Dummy	-8.963*** (1.593)			-9.180*** (1.508)		
Perc. parapolitics news		-29.57*** (3.270)			-30.31*** (3.251)	
News before elections (Yes=1)			-8.111*** (1.864)			-8.817*** (1.796)
Constant	22.97*** (1.716)	22.14*** (1.678)	20.43*** (1.657)	26.50*** (3.781)	25.39*** (3.733)	23.17*** (3.751)
R-squared	0.082	0.079	0.050	0.188	0.184	0.159
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Candidate controls				Yes	Yes	Yes
Observations	356	356	356	356	356	356
Number of candidates	254	254	254	254	254	254

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Candidate-level regressions. The sample of politicians included are all candidates to the Senate in the 2002, 2006 and 2010 elections with previous experience either in the House or Senate. ‘Parapolitician Dummy’ equals 1 if there are any stories connecting the candidate to paramilitaries at any point in time. ‘Percent parapolitics news’ is the percentage of total news stories of the candidate that relate him with the parapolitics scandal. ‘News before elections’ is a dummy variable that equals 1 if the candidate was linked to paramilitaries before the corresponding election. Candidate controls in columns 4 to 6 include: number of news stories about the politician, gender, profession, years in politics, periods in office, and incomplete term dummy.

Table 8: Comparability of Exposed and Unexposed Parapoliticians

<i>Dependent variable: candidate is exposed as Parapolitician before elections</i>		
	(1)	(2)
Replaced Candidate	0.0252 (0.129)	0.0544 (0.114)
College Degree	-0.1000 (0.0928)	-0.108 (0.0931)
Male	0.146 (0.0988)	0.155 (0.100)
Change of House	0.0431 (0.0710)	0.0264 (0.0726)
Average Years in Politics	-0.000743 (0.00600)	0.000621 (0.00626)
Average Periods in Congress	0.0381* (0.0211)	0.0366* (0.0217)
Total news stories in <i>El Tiempo</i>	0.000223** (9.89e-05)	
Non-paramilitary news stories in <i>El Tiempo</i>		0.000112 (0.000103)
Observations	119	119
R-squared	0.803	0.797

Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Dependent variable is a dummy that equals 1 if the politician is exposed *before* the corresponding election. A full set of year fixed effects are included. Sample includes only the set of Parapoliticians (politicians exposed as having links with paramilitaries at any point in time).

Table 9: Parapoliticians: Vote concentration in paramilitary, strong state and efficient institutions areas after the scandal

	(1)	(2)	(3)	(4)
<i>Panel A: Paramilitary areas</i>				
<i>Dependent variable: characteristic weighted by vote share</i>				
Presence index (ARS)		Dummy high presence (ARS)	Paramilitary attacks per capita	Dummy high para. attacks per capita
News before elections (Yes=1)	1.065** (0.513)	6.748 (4.160)	0.729 (0.579)	13.64*** (4.661)
Constant	5.039*** (1.011)	40.66*** (8.374)	3.714*** (0.933)	59.20*** (7.929)
R-squared	0.238	0.236	0.179	0.191
<i>Panel B: Strong state and efficient institutions areas</i>				
<i>Dependent variable: characteristic weighted by vote share</i>				
Judicial efficiency (electoral crimes)		Judicial efficiency (all crimes)	State presence index (all offices)	State presence index (main offices)
News before elections (Yes=1)	-0.911 (1.136)	-0.281* (0.150)	-4.239* (2.402)	-4.269* (2.477)
Constant	41.81*** (1.944)	9.694*** (0.261)	12.23*** (4.236)	12.80*** (4.351)
R-squared	0.125	0.090	0.293	0.287
Time fixed effects	Yes	Yes	Yes	Yes
Candidate controls	Yes	Yes	Yes	Yes
Observations	119	119	119	119
Number of candidates	77	77	77	77

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Sample includes only the set of Parapoliticians (politicians exposed as having links with paramilitaries at any point in time). 'News before elections' is a dummy variable that equals 1 if the candidate was linked to paramilitaries before the corresponding election. Candidate controls in columns include: number of news stories about the politician, gender, profession, years in politics, periods in office, and incomplete term dummy.

Table 10: Parapoliticians: Relative success and effects of the scandal

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Panel A: Dependent variable: candidate vote share</i>								
Parapolitician Dummy	0.000931*** (0.000271)			0.00101*** (0.000244)				
Perc. parapolitics news		0.00319*** (0.000949)			0.00385*** (0.000861)			
News before elections (Yes=1)			0.000874*** (0.000326)			0.000849*** (0.000326)	0.000734 (0.000518)	0.000563 (0.000522)
Constant	0.00521*** (0.000335)	0.00529*** (0.000318)	0.00547*** (0.000307)	0.00300*** (0.000577)	0.00307*** (0.000577)	0.00337*** (0.000602)	0.00633*** (0.000300)	0.00602*** (0.000895)
R-squared	0.133	0.134	0.123	0.331	0.339	0.315	0.217	0.282
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Candidate controls				Yes	Yes	Yes	Yes	Yes
Observations	356	356	356	356	356	356	119	119
Number of candidates	254	254	254	254	254	254	77	77

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. In columns 1 to 6, the sample of politicians included are all candidates to the Senate in the 2002, 2006 and 2010 elections with previous experience either in the House or Senate. In columns 7 and 8, we further restrict to the set of Parapoliticians (politicians exposed as having links with paramilitaries at any point in time). 'Parapolitician Dummy' equals 1 if there are any stories connecting the candidate to paramilitaries at any point in time. 'Percent parapolitics news' is the percentage of total news stories of the candidate that relate him with the parapolitics scandal. 'News before elections' is a dummy variable that equals 1 if the candidate was linked to paramilitaries before the corresponding election. Candidate controls in columns 4 to 6 and 8 include: number of news stories about the politician, gender, profession, years in politics, periods in office, and incomplete term dummy.

Table 11: Parapoliticians: Vote concentration in high turnout areas and effects of the scandal

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Dependent variable: unusually high turnout weighted by vote share</i>								
Parapolitician Dummy	12.42*** (2.304)			12.37*** (2.259)				
Perc. parapolitica news		41.02*** (8.959)			39.44*** (8.757)			
news before elections (Yes=1)			14.83*** (3.100)			14.88*** (3.038)	10.38* (5.802)	11.04* (6.228)
Constant	24.52*** (1.915)	25.68*** (1.963)	28.01*** (1.941)	19.75*** (4.497)	21.36*** (4.467)	24.09*** (4.733)	35.02*** (4.233)	51.26*** (11.15)
R-squared	0.109	0.105	0.101	0.154	0.143	0.148	0.031	0.159
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Candidate controls				Yes	Yes	Yes	Yes	Yes
Observations	356	356	356	356	356	356	119	119
Number of candidates	254	254	254	254	254	254	77	77

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. In columns 1 to 6, the sample of politicians included are all candidates to the Senate in the 2002, 2006 and 2010 elections with previous experience either in the House or Senate. In columns 7 and 8, we further restrict to the set of Parapoliticians (politicians exposed as having links with paramilitaries at any point in time). 'Parapolitician Dummy' equals 1 if there are any stories connecting the candidate to paramilitaries at any point in time. 'Percent parapolitics news' is the percentage of total news stories of the candidate that relate him with the parapolitics scandal. 'News before elections' is a dummy variable that equals 1 if the candidate was linked to paramilitaries before the corresponding election. Candidate controls in columns 4 to 6 and 8 include: number of news stories about the politician, gender, profession, years in politics, periods in office, and incomplete term dummy.

Table 12: Parapoliticians: Vote concentration in guerrilla areas (placebo)

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: Dependent variable: dummy for municipality captured by guerrilla weighted by vote share</i>						
Parapolitician Dummy	-0.393 (0.667)			-0.255 (0.668)		
Perc. parapolitics news		-3.650*** (1.000)			-3.093*** (0.938)	
News before elections (Yes=1)			-1.545*** (0.588)			-1.460*** (0.563)
Constant	2.121*** (0.675)	2.227*** (0.663)	2.021*** (0.630)	-0.779 (0.766)	-0.613 (0.708)	-0.813 (0.723)
R-squared	0.002	0.008	0.010	0.029	0.033	0.036
<i>Panel B: Dependent variable: dummy for high guerrilla attacks per capita weighted by vote share</i>						
Parapolitician Dummy	-0.781 (0.495)			-0.688 (0.491)		
Perc. parapolitics news		-5.601*** (1.227)			-5.089*** (1.248)	
News before elections (Yes=1)			-1.281** (0.582)			-1.170** (0.580)
Constant	4.139*** (0.447)	4.249*** (0.424)	3.923*** (0.422)	0.886 (0.913)	1.050 (0.893)	0.662 (0.901)
R-squared	0.008	0.030	0.013	0.049	0.067	0.053
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Candidate controls				Yes	Yes	Yes
Observations	356	356	356	356	356	356
Number of candidates	254	254	254	254	254	254

Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Candidate-level regressions. The sample of politicians included are all candidates to the Senate in the 2002, 2006 and 2010 elections with previous experience either in the House or Senate. ‘Parapolitician Dummy’ equals 1 if there are any stories connecting the candidate to paramilitaries at any point in time. ‘Percent parapolitics news’ is the percentage of total news stories of the candidate that relate him with the parapolitics scandal. ‘News before elections’ is a dummy variable that equals 1 if the candidate was linked to paramilitaries before the corresponding election. Candidate controls in columns 4 to 6 include: number of news stories about the politician, gender, profession, years in politics, periods in office, and incomplete term dummy.

Table 13: Parapoliticians: Vote concentration in guerrilla areas after the scandal (placebo)

	(1)	(2)	(3)	(4)
<i>Dependent variable: guerrilla area measure ... weighted by vote share</i>				
Dummy captured by guerrilla		Dummy high guerrilla attacks	Dummy captured by guerrilla	Dummy high guerrilla attacks
News before elections (Yes=1)	-2.857 (2.121)	-2.171** (1.087)	-3.630 (2.192)	-2.839*** (1.006)
Constant	2.131** (1.034)	3.496*** (0.777)	-4.454 (2.728)	-4.173** (1.598)
R-squared	0.031	0.038	0.100	0.211
Time fixed effects	Yes	Yes	Yes	Yes
Candidate controls			Yes	Yes
Observations	119	119	119	119
Number of candidates	77	77	77	77

Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Sample includes the set of Parapoliticians (politicians exposed as having links with paramilitaries at any point in time). 'News before elections' is a dummy variable that equals 1 if the candidate was linked to paramilitaries before the corresponding election. Candidate controls in columns 3 and 4 include: number of news stories about the politician, gender, profession, years in politics, periods in office, and incomplete term dummy.

Table 14: Verifying media bias: Parapoliticians vote shares and distribution using *El Espectador*

Dependent variable:	Municipal characteristic ... weighted by vote share						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Panel A: Parapolitician Dummy based on El Espectador news searches</i>							
Vote share							
Parapolitician dummy	0.00112*** (0.000271)	1.203*** (0.212)	1.192*** (0.230)	-1.664*** (0.583)	-0.175** (0.0731)	-10.96*** (1.327)	-11.02*** (1.348)
R-squared	0.332	0.102	0.097	0.104	0.051	0.206	0.208
<i>Panel B: Parapolitician Dummy based on El Tiempo news searches</i>							
Parapolitician dummy	0.00106*** (0.000264)	1.198*** (0.296)	0.920*** (0.225)	-1.082* (0.585)	-0.0902 (0.0732)	-9.170*** (1.585)	-9.318*** (1.592)
R-squared	0.330	0.105	0.069	0.092	0.039	0.183	0.186
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Candidate controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	339	339	339	339	339	339	339
Number of candidates	244	244	244	244	244	244	244

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Candidate controls include: number of news stories about the politician, gender, profession, years in politics, periods in office, and incomplete term dummy. 'Parapolitician Dummy' equals 1 if there are any stories connecting the candidate to parapoliticians at any point in time in the corresponding newspaper. Parapolitician dummy based on *El Tiempo* news stories limited to stories during the time at which *El Espectador* news archive is available. We drop all candidates exposed as parapoliticians only by *El Tiempo* prior to when *El Espectador* online archive is available.

A Appendix. Model Extension: Media Scandal and Coercion Costs

The endogenous increase in coercion after a media scandal is the main driver of our key theoretical results. However, in our baseline model we ignored the fact that more media exposure can also make coercion more costly, discouraging it and potentially eliminating the main mechanisms we highlighted in the main text. In this section, we extend our baseline model in such direction and show that, while this indeed attenuates the increase in coercion, our main results are still present under admissible parameter values.

Let the cost of coercion be given by,

$$C(\{\lambda_m\}_{m=1}^N) = \sum_{m=1}^N \psi_m \mu_m^\alpha c(\lambda_m), \quad (\text{A-1})$$

where $\alpha \geq 0$. The baseline model corresponds to the special case with $\alpha = 0$.

Following the same steps as before, the first order conditions of politician A indicate that equilibrium coercion λ_m^* is now given by:

$$\lambda_m^* = \max \left\{ 0, \frac{1}{\psi_m \mu_m^\alpha} \left(\frac{1}{2} + \phi_m (\mu_m - \sigma_m) \right) \right\},$$

Hence, we can write

$$\frac{\partial \lambda_m^*}{\partial \mu_m} = \frac{\phi_m}{\psi_m \mu_m^\alpha} - \alpha \frac{\lambda_m^*}{\mu_m}.$$

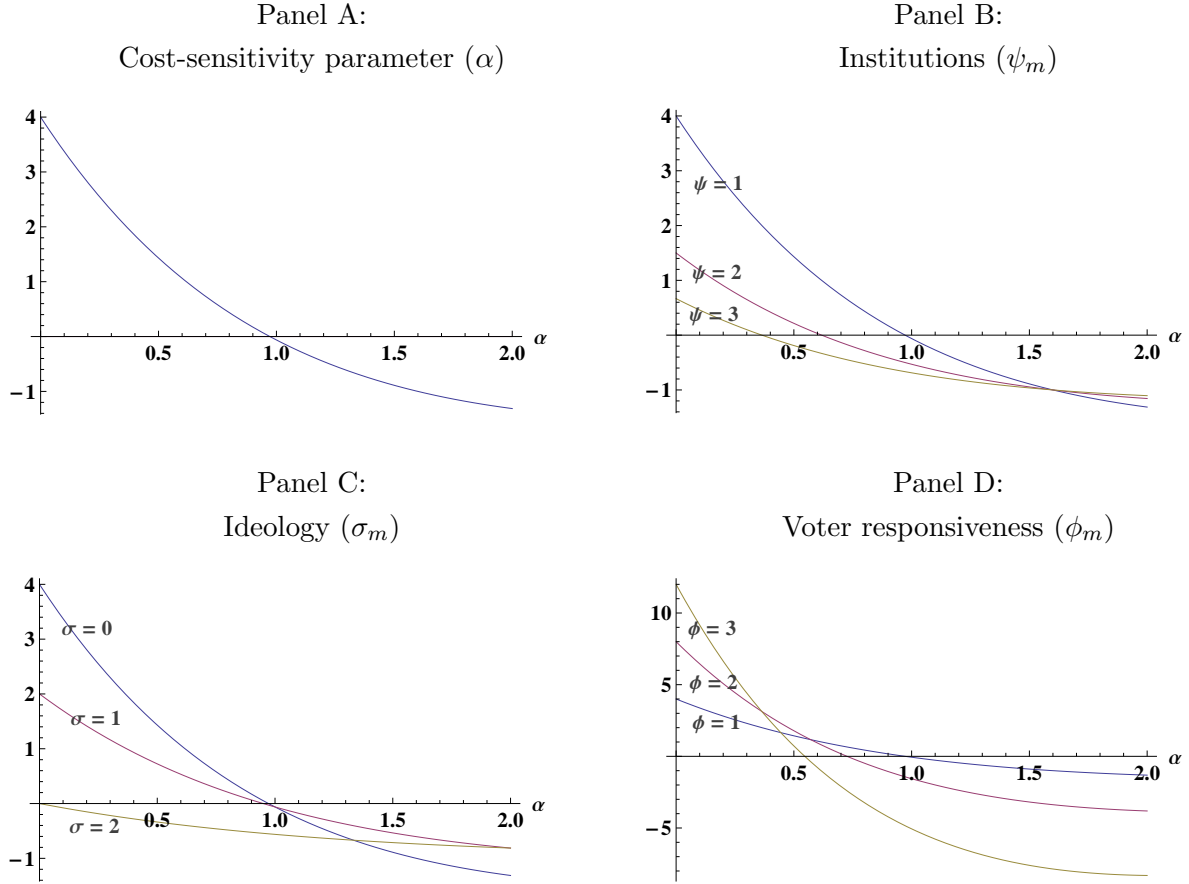
After some algebra, this implies that our main object of interest, the change in parapolitician support in each municipality after the media scandal, is given by:

$$\frac{d\pi_m^A}{d\mu_m} = -(1 - 2\lambda_m^*)\phi_m - \alpha(\lambda_m^*)^2\psi_m\mu_m^{\alpha-1} \quad (\text{A-2})$$

While in this extension there is no simple closed-form parameter restriction that establishes the conditions under which $\frac{d\pi_m^A}{d\mu_m} \geq 0$, the graphical analysis that follows shows that the endogenous response of coercion may still imply that electoral support for politician A does not fall after the media scandal. Also, we show that the main comparative static predictions of the baseline model still hold.

We set the following baseline values for the model parameters: $\phi_m = \psi_m = 1$, $\mu_m = 2$ and $\sigma_m = 0$. Panel A in Figure A-1 shows that $\frac{d\pi_m^A}{d\mu_m} > 0$ for sufficiently low α . That is, as long as coercion costs do not increase too sharply with the media scandal, the main result in the baseline model still holds under admissible parameter values: given the endogenous response of coercion, the media scandal may not hurt the parapolitician's total electoral support.

Figure A-1: Change in Politician Electoral Support with Media Scandal
 $(d\pi_m^A/d\mu_m)$



Notes: Each panel graphs $d\pi_m^A/d\mu_m$, the change in total electoral support to candidate A (the parapolitician) as a result of a media scandal in municipality m . The algebraic expression is given in (A-2). This derivative is plotted as a function of the cost-sensitivity parameter, α , capturing the extent to which a media scandal increases coercion costs in a given municipality. From (A-1), the cost of coercion λ_m in each municipality is given by $\psi_m \mu_m^\alpha \lambda^2 / 2$. Baseline values for the model parameters are $\phi_m = \psi_m = 1$, $\mu_m = 2$ and $\sigma_m = 0$. Panel B checks sensitivity of $d\pi_m^A/d\mu_m$ to different values of the quality of institutions (ψ_m), Panel C to ideology (σ_m), and Panel D to voter responsiveness (ϕ_m).

Panels B-D show the main comparative static implications of the model. In Panel B, we plot $\frac{d\pi_m^A}{d\mu_m} > 0$ for the baseline value $\psi_m = 1$ for the quality of local institutions, together with $\psi_m = 2$ and $\psi_m = 3$. Clearly, an increase in the quality of institutions makes it less likely that the parapolitician may compensate for his loss in popularity through an increase in coercion. In Panel C, we allow voter ideology σ_m to increase from its baseline value of 0 to 2. Recall that for the purpose of our

example we set $\mu_m = 2$, meaning that the key value of σ_m is 2. Indeed, for $\sigma_m > 2$ the net average partisanship of voters ($\sigma_m - \mu_m$) favors politician A . Thus, free voters are on average supporting politician A , which reduces the returns to increasing coercion. In line with this intuition, Panel C shows that starting at $\sigma_m = 2$, the electoral support for the parapolitician falls with the media scandal even if exposure does not make coercion more costly¹⁹. Finally, Panel D explores changes in voter responsiveness, from the baseline value of $\sigma = 1$ to $\sigma = 3$. We observe that more responsive voters implies that as voters become more responsive, parapolitician support increases after the scandal for lower values of α . Intuitively, when voters are more responsive many votes are lost as a result of the media scandal, and it is less likely that the parapolitician may compensate for the fall in popularity with coercion. Notice, however, that when they do compensate for the fall in popularity and $d\pi_m^A/d\mu_m > 0$, the increase is larger for large ϕ_m , as more votes are prevented from going to the opponent through coercion in this case.

¹⁹Indeed, recall from the baseline model that $\frac{d\pi_m^A}{d\mu_m} \gtrless 0$ if $\frac{1}{2} + \phi_m (\mu_m - \sigma_m) \gtrless \frac{1}{2} \psi_m$. Hence, for every $\mu_m - \sigma_m < 0$ we have $\frac{d\pi_m^A}{d\mu_m} < 0$ even if coercion costs are independent of the media scandal.

B Appendix. Coding of Politician News Stories

Our data set is event-based and relies on information on every news story (and op-ed) published from January 1st 1997 to August 1st 2011 in *El Tiempo*, Colombia’s largest newspaper with nationwide circulation and coverage. We search for stories in every section of the newspaper about each of the incumbent congressmen in this 15-year period. For every story we record the date, the newspaper section in which it appears (and if it appears in more than one section), and whether it links the politician with “parapolitics” activity, other corruption practices or an electoral crime (like fraud or vote-buying).

The first phase of the data gathering took place from August to October 2010. A team of over 30 undergraduate students at Universidad del Rosario were trained to use the *El Tiempo* electronic archive and code stories about each candidate. We searched for all possible variations of the (frequently two) names and two last names of each candidate. The heading of each story (or the whole story if necessary) was checked to discard homonyms and corroborate it referred to the politician.

We double-coded the set of news for a fourth of all the candidates in the sample (the rest of the candidate sample were coded again in the second phase of the data gathering). This was done by different coders in different coding sessions taking place in different days, to ensure the two codings of the same candidate were independent. Where we found large and systematic differences between the two codings, the candidate was again coded twice in the second phase²⁰. In this first phase, coders were paid according to their productivity, as measured both by the amount of candidate-stories coded per session, as well as the average coding quality.

The second phase of the data gathering was part of an economic experiment conducted by Mariana Blanco and Patricio Dalton between August and September of 2011. During this phase, we completed the double checking for the candidates of the first phase, coded the *El Tiempo* news stories between July 2010 and August 2011 for of all candidates, and coded stories from *El Espectador*²¹, coding these twice for each candidate. In the experiment, 200 undergraduate students from Universidad del Rosario and Universidad de los Andes were hired for the codification and divided in 12 groups (half doing news from *El Tiempo* and the other half doing news from *El Espectador*) and the same instructions from the first phase were given to the students²².

In each session the coders received a number of random candidates with the information of the start date to code. In the experiment, students had the incentive to do a good job because they were likely to be “fired” and do instead other activity with lower salary or were paid a smaller amount for just attending the session without working. From the first day of work and in each

²⁰All coders were double-coded by a random peer in at least one of the politicians assigned.

²¹*El Espectador* only became a daily newspaper since October of 2007 and only since then are its news stories available in the online archive

²²The instructions for *El Espectador* codification differed in some minor aspects because of differences in its website.

session, the coders within each group were classified according to their productivity and quality and in the next session only the more productive ones codified news stories. The number of students working varied in each session and fired coders had the chance to be hired again. Thus, checking quality was of extreme importance for the experiment and after each session at least 15% of the news stories coded by each student were checked. Also, codifications by any student in one session with more than 40% of errors were excluded altogether. With all candidates coded twice in both *El Tiempo* and *El Espectador* newspaper, this allowed us to cross-check the quality of the coding.