Self-reinforcing Corruption: Evidence from the MPs' Expenses Scandal

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Abstract

Models of self-reinforcing corruption have been influential in describing corruption traps. However, there is a lack of evidence regarding their underlying mechanisms. This dissertation tests for one such mechanism, peer effects, using data on the 2009 MPs' expenses scandal, which I collected for this purpose. I use the influx of newly elected MPs in 2005 to construct a novel IV that addresses the reflection problem. I find some evidence of peer effects, which mainly operate through geographical networks. There is also initial evidence of asymmetric influence: The behaviour of senior party-officials and worst offenders has a bigger impact than others.

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1 Introduction

Corruption begets corruption.[1] This maxim is fundamental to models of self-reinforcing corruption, which provide three key insights, as formalised by Aidt (2003).

Firstly, these models assume that the incentive to be corrupt increases alongside the prevalence of corruption. An agent's decision to be corrupt changes the 'culture', which in turn changes other agents' decisions, which changes the culture again. Secondly, it is shown that such self-reinforcing feedback loops can result in a system having multiple equilibria. This helps explain why corruption still varies between societies with similar institutional incentives (Golden and Picci, 2005; Charron, 2010). Lastly, these models show that some of these equilibria are stable, creating a corruption trap whereby if society attempts a small deviation from the high-corruption equilibrium they will revert back to it.

The implication drawn from this is that a 'big-push' appears the most effective solution, which moves society past the tipping point or eliminates high-corruption equilibria all together. This has greatly influenced policy makers, including the IMF (Mauro, 2004). Fisman and Golden (2017) state that "the choice is largely between rapid change versus no change at all". Whilst some recent theoretical challenges have been made (Stephenson, 2019), this remains the mainstream interpretation.

However, there is surprisingly little micro-evidence on the mechanisms underlying models of self-reinforcing corruption, such as peer effects. Existing research either investigates if culture affects individuals (Fisman and Miguel, 2007), a necessary but insufficient condition for feedback loops, or provides qualitative case-studies of big-pushes (Skidmore, 1996; Persson et al., 2013), a prediction of such models. Neither approach tests corruption peer effects directly and this dissertation seeks to help address this gap.

[1] This dissertation defines corruption as "an act in which the power of public office is used for personal gain in a manner that contravenes the rules of the game" (Jain, 2001)

Estimating peer effects in corruption is challenging due to the scarcity of data and issues with identification. To overcome the first challenge, I build a new data set in the context of the 2009 MPs' expenses scandal, using audit reports of British Members of Parliament (MPs) that cover 2004-2008, and observable networks constructed from autobiographical records as well as various parliamentary sources.

To address the issue of identification, I propose an empirical strategy that avoids the reflection problem that occurs when studying peer effects (whereby if i affects j and simultaneously j affects i, a multiplier is created that biases estimates upwards). Using the influx of new MPs following the 2005 election, prior corruption by incumbents can be used to construct a novel instrumental variable (IV) that overcomes this issue.

This dissertation makes three interesting insights. Firstly, it presents direct evidence of peer effects in corruption. These appear to primarily influence already corrupt politicians to behave even worse, rather than affecting honest MPs, and have a seemingly low reflection multiplier effect.

Secondly, some observable networks matter more than others. Testing networks based on geography, university, and select-committees, it appears that corruption peer effects primarily operate between MPs from nearby constituencies. This is the only network that has results robust to a full set of controls and instrumentation.

Lastly, there is also initial evidence of some MPs having asymmetrical influence, that is some agents create larger peer effects than others. In the geography network, a £1 increase in sum of corruption-exposure by senior party-official peers causes a MP to over-claim 4.8p more, versus 0.2p by non-senior peers. Out of these senior peers, more influence seems to be exerted by 'bad-apples' (i.e. the peer who claims the most expenses): 4.9p versus 2.6p. Thus, whilst senior bad-apples make up 13% of the sample, they account for approximately 65% of observed peer effects in corruption.

This last finding somewhat challenges existing models of self-reinforcing corruption, which implicitly assume that all agents affect culture equally. I propose that asymmetric influence could have important implications for tackling corruption traps.

This dissertation is structured as follows. Section 2 provides a literature review on peer effect mechanisms. Section 3 outlines my conceptual framework and 4 the empirical strategy. Section 5 explains how data was collected and 6 presents the results. Section 7 concludes, reflecting on limitations and further areas of work.

2 Peer Effect Mechanisms

Peer effects are when the behaviour of one agent affects that of another. This could occur through a number of mechanisms. Although I cannot separate these empirically, this section categorises the existing literature into [2.1] information externalities, [2.2] pay-off interactions, and [2.3] conformity preferences. I briefly explain each concept, outline seminal studies, and relate it to the context of the MPs' expenses scandal. Since evidence on peer effects in corruption is scarce, relevant papers in other settings are also drawn on.

2.1 Informational Externalities

In a world with imperfect information, agents may use peers to gain better information. Such social learning (Mobius and Rosenblat, 2014) has been used to explain technology adoption behaviour (Conley and Udry, 2010; Kremer and Miguel, 2007) and consumers discovering the quality of a service (Moretti, 2011). Informational externalities have also been used to model informational cascades, whereby other peoples' signal can trump private information and lead agents to blindly follow the crowd (Bikhchandani et al., 1992). This has been applied to bank runs (Kelly and O Grada, 2000) and protests (Lohmann, 1994).

In a context of corruption, Sah (2007) provides a model wherein agents learn from experiences so "if bureaucratic corruption has been more pervasive in the past, [agents] are more likely to choose those behaviours". I believe information transmission via social networks are highly applicable to the MPs' expenses scandal. MPs are fundamentally uncertain about the risks of being caught and what loopholes they can use to over-claim expenses. Peers may thus communicate with each other or treat actions as signals.

2.2 Pay-off Interactions

An alternative channel through which peer effects work is strategic complementarities. In contrast to information externalities, individuals are certain of underlying parameters.

For example, the personal harm from being 'wrong' can be smaller when others share the blame. This has been found to explain behaviour by financial analysts Bedke et al. (2008) and NASA's Columbia disaster (Ferraris and Carveth, 2003). Conversely, interdependency also arises if people benefit from choosing the same action: Arthur (1989) argues this is why economies lock-in into specific technologies; Kuran (1987) uses it to explain political preference falsification and unanticipated revolutions.

In the context of political corruption, many specific pay-off interdependencies have been proposed, underlying Aidt (2003)'s model. In more corrupt societies [1] the risk of being caught is lower as detection resources are spread out (Becker, 1968), [2] search costs to meet a willing bribee fall (Andvig and Moene, 1990), [3] the rewards from entrepreneurship relative to rent-seeking decline (Acemoglu, 1995), and [4] there is a smaller incentive to be honest when others already perceive your group as corrupt (Tirole, 1996).

I propose that the last mechanism [4] is particularly relevant to the MPs' expenses scandal, relating it to recent work on cynical elections (Klašnja et al., 2018). If voters think most politicians over-claim expenses, they might not 'boot out' a MP that get caught, since their successor is likely also corrupt and may be worse in other regards. The lower risk of punishment gives MPs a greater incentive to be corrupt in the first place.

2.3 Conformity Preference and Social Sanctions

Lastly, peer effects could work through agents' preference to conform or groups punishing deviation with social sanctions. In contrast to pay-off interactions, the direct benefit from a action does not necessarily change and instead this effect occurs through social channels.

Conformity has been found to matter in educational performance (Coleman, 1975), drug use (Gaviria and Raphael, 2001; Duncan et al., 2005), political views (Algan et al., 2015), and crime (Stevenson, 2017; Bayer et al., 2009). In psychology, Asch (1951) found people give blatantly wrong answers to conform to the group, although the extent of stigma is highly contextual (Ainlay et al., 1986).

For the MPs' expenses scandal, I relate conformity preference to Ashforth and Anand (2003), who argue that "naive newcomers are induced to view corruption as permissible if not desirable" within organisations. It is plausible that a MP feels less guilty engaging in corrupt behaviour when peers normalise it. Norman Baker, a MP who actively campaigned for the release of expenses prior to the scandal, recounts the stigma he faced:

> "There was a degree of hostility [...] to MPs who had been trying to blow this open [...] because it was seen as not part of the club, letting the side down [...] including by people from my party. It wasn't very pleasant. I was seen as disloyal to the team." (Baker, 2019)

3 Conceptual Framework

This dissertation seeks to identify corruption peer effects in the context of the MPs' expenses scandal. Underlying this is the notion that there exists a network among MPs, through which corruption peer effects operate but that cannot be observed directly. Instead, I look at three observable networks that may influence an MP's decision to over-claim expenses: geography, university, and select-committees. To start, networks are assumed to be unweighted, with all MPs having the same influence. I then relax this assumption and allow for certain individuals - senior party-officials and worst offenders - to exert more influence than the rest.

3.1 Observable Networks

MPs' characteristics can be used to identify and construct three observable networks that potentially link MPs socially and could thus transmit corruption: geography, university and select-committees.[2]

Geography The First Past the Post electoral system assigns MPs to specific constituencies. MPs may interact closely with nearby 'neighbours'. This could be because they know each other from local politics, cooperate on issues facing their similar constituents, or regularly encounter at regional events.

University MPs may interact closely with those who went to the same university. Social connections could be formed during university or afterwards due to their affiliation, such as at the 'Oxford and Cambridge Club' located five minutes from Parliament.

[2]Other networks were excluded due to lack of data e.g. seating arrangements and co-sponsoring bills

Select-committees MPs may interact closely with those serving on the same select-committee. Select-committees are where MPs conduct much of their governmental work and thus come into frequent contact with each other.

3.2 Asymmetric Influence

Having identified plausible and observable connections, one must now consider how to weight these. The majority of the peer effect literature uses the linear-in-means model, whereby peer effect are assumed uniform and work exclusively through a group's mean (Sacerdote, 2011). However, it seems worth exploring the possibility that some MPs have more influence than others. I focus on two aspects that appear most pertinent in my context.

Senior Studies in other settings have found that a peer's influence can depend on certain background characteristics (Duncan et al., 2005; Harmon et al., 2019). In political corruption, it may be that MPs are disproportionately influenced by senior party-officials, who may have authoritative status or better quality information. This fits anecdotal evidence, which emphasizes the role of the party leadership:

> "MPs were encouraged almost by the whip's offices to exceed their expenses limits or to claim for what they wouldn't normally claim for as a substitute for a proper pay increase" (Baker, 2019)

Bad-apple MPs may also give more weight to the worst offender in a given network (Sacerdote, 2001; Lavy et al., 2007). In political corruption, bad-apples may have the most knowledge about over-claiming expenses or act as a moral benchmark to justify one's own corruption ("at least I'm not as bad as so-and-so").

4 Empirical Strategy

This section outlines the main empirical challenges facing this dissertation and how it address these: [4.1] censored dependent variable, [4.2] the reflection problem, and [4.3] omitted variables. The latter two are commonly cited as why identifying peer effects is notoriously difficult (Manski, 1993; Brock and Durlauf, 2001; Moffitt et al., 2001).

Throughout points are illustrated using a simple model of two MPs, i and j: y_{it} is i's corrupt expenses in period t, x_{it} a vector of controls, y_{jt} corruption-exposure, and ε_{it} the residual. I am interested in estimating β_2, which is plausibly in the range $0 \leq \beta_2 \leq 1$.[3]

$$\begin{aligned} y_{it} &= \beta_0 + \beta_1 x_{it} + \beta_2 y_{jt} + \varepsilon_{it} \\ y_{jt} &= \beta_0 + \beta_1 x_{jt} + \beta_2 y_{it} + \varepsilon_{jt} \end{aligned} \tag{4.1}$$

4.1 Censored Dependent Variable

The minimum expenses a MP can over-claim is zero. Thus the dependent variable y_{it} is censored left of 0, making the standard OLS approach inappropriate (Wooldridge, 2016).

4.1.1 Tobit Model

The generalised Tobit model appears to be the most appropriate solution. Following Humphreys (2013)'s categorisation of censoring models, this is because zero over-claimed expenses are 'genuine' corner responses and the decision to be corrupt likely influences the magnitude of corruption.[4]

[3] i.e. Corruption-exposure does not discourage corruption (no insidious comparison) and peer effects are less than unity (else expenses spiral out of control)

[4] Some propose using two-part models (Cragg, 1971), which treat discrete and continuous behaviour as separate and thus allow alternative explanations for zero responses, particularly infrequent demand (Blundell and Meghir, 1987). I disregard this since over-claiming expenses is free and available every period

Tobit can be interpreted as two joint decisions:

$$
\begin{aligned}
y^* &= \beta_0 + \beta_1 x_{it} + \beta_2 y_{jt} + \varepsilon_{it} \\
y &= \max(0, y^*)
\end{aligned}
\tag{4.2}
$$

Coefficients of a Tobit regression represent changes in the latent variable $\frac{dE(y^*|\mathbf{x})}{dx_k} = \beta_k$. For real-world interpretation one must take marginal effects, typically evaluated at the mean (Wooldridge, 2010). There are two such components: how much already corrupt MPs change claims $\frac{dE(y|\mathbf{x}, y > 0)}{dx_k}$ and the probability of being corrupt $\frac{dP(y > 0|\mathbf{x})}{dx_k}$. These are combined to obtain the marginal effect on the observed variable $\frac{dE(y|\mathbf{x})}{dx_k}$.

4.1.2 Limitations

For Tobit estimators to be consistent, the underlying latent variable y^* must satisfy the classical linear model assumptions (Wooldridge, 2016). Unfortunately, Tobit is not very robust to violations in homoskedasticity or normality, and testing for these is hard when IVs are used (Cameron and Trivedi, 2009).

Where possible, I take precautions, verifying results with bootstrapped standard errors as per Greene (2003), but this is an imperfect fix. Several advanced solutions have been proposed but are unsuitable or beyond the scope of this dissertation.[5]

[5] To tackle heteroskedasticity Andersen et al. (2013) suggests clustering and Shehata (2011) proposes a TOBITHETM regression. This is not easily implementable with IV Tobit. To tackle normality, Cameron and Trivedi (2009) suggest logarithmic transformations of the dependent variable. However, this requires transforming zero values and the censoring point, considerably complicating interpretation. Perhaps most promising for both issues are advanced models, like CLAD (Powell, 1984) or censored quantile regressions (Chernozhukov et al., 2015)

4.2 Reflection Problem

To empirically identify the causal effect, each explanatory variable must be uncorrelated with the error term in the dependent variable. However, Manski (1993) notes that if there are peer effects, then y_{jt} and ε_{it} are necessarily correlated. This is because y_{jt} itself depends on y_{it} and hence ε_{it}. This produces biased estimates as $Cov(y_{jt}, \varepsilon_{it}) \neq 0$:

$$\begin{aligned} Cov(y_{jt}, \varepsilon_{it}) &= Cov(\beta_0 + \beta_1 x_{jt} + \beta_2 y_{it} + \varepsilon_{jt}, \varepsilon_{it}) \\ &= \frac{\beta_2}{1-\beta_2^2} Var(\varepsilon_{it}) \end{aligned} \tag{4.3}$$

Intuitively, suppose there is an unobserved shock ε_{it}, whereby i becomes more corrupt after reading Machiavelli's The Prince and thus claims more expenses y_{it}. Peer effects results in j claiming more expenses y_{jt}. Yet precisely because there is a peer effect, y_{jt} must somewhat 'reflect' back to increase y_{it}. This cycle repeats infinitely creating a multiplier effect that biases the estimate of the peer effect upwards.

4.2.1 Lagged Approach

To cleanly identify peer effects, the two-way feedback between a MP and their peer group must be eliminated. A first step might be to proxy corruption-exposure using past values, as current behaviour cannot affect the past: y_{jt-1} affects y_{it} but y_{jt} does not affect y_{it-1}.

However, this approach alone is unlikely to be valid. Let us decompose the composite error term so $\varepsilon_{it} = u_i + e_{it}$, assuming politicians have an idiosyncratic component in corruption that is constant over time u_i. This causes the identification condition to fail:

$$\begin{aligned} Cov(y_{jt-1}, u_i + e_{it}) &= Cov(\beta_0 + \beta_1 x_{jt} + \beta_2 y_{it-1} + u_j + e_{jt-1}, u_i + e_{it}) \\ &= \frac{1}{1-\beta_2^2} Var(u_i) \end{aligned} \tag{4.4}$$

Intuitively, j's lagged expenses y_{jt-1} pick up two effects: the true peer effect β_2^{True} (solid line in Figure 1-left) and the persistent error u_i caused by unobserved heterogeneity (dashed line in Figure 1-left). This latter component biases the peer effect estimate.

$$
\begin{tikzcd}
y_{jt-1} \arrow[r, "u_j"] \arrow[d, dashed, "\beta_2^{True}"'] & y_{jt} \arrow[d, "\beta_2^{True}"] \\
y_{it-1} \arrow[r, dashed, "u_i"'] & y_{it}
\end{tikzcd}
\qquad\qquad
\begin{tikzcd}
y_{j04} \arrow[r, "u_j"] \arrow[d, dashed, "\cancel{\beta_2^{True}}"'] & y_{jt} \arrow[d, "\beta_2^{True}"] \\
\cancel{y_{i04}} \arrow[r, dashed, "\cancel{u_i}"'] & y_{it}
\end{tikzcd}
$$

Figure 1: Naive & IV Lagged Approaches

4.2.2 Instrumental Variable Approach

To eliminate the indirect effect caused by unobserved heterogeneity, I propose an instrumental variable that, to the best of my knowledge, has not yet been used in the peer effect literature. Using the exogenous source of variation created by the 2005 general election, MPs divide into two groups: 136 'new' MPs i (first elected in 2005) and 523 'old' MPs j (re-elected in 2005).

By definition, new MPs i were not elected until 2005 and thus could not claim any expenses before then.[6] Hence j's 2004 expenses y_{j04} cannot be correlated with i's 2004 expenses y_{i04}, as the latter does not exist. This method removes the endogenous channel (dashed line in Figure 1-right). Once i is elected they are subject to peer effects from j and thus this channel is preserved (solid line in Figure 1-right): j's 2004 expenses y_{j04} are correlated with j's post-2005 expenses y_{jt}, which affect i's current expenses y_{it}.

Hence, when considering only peer effects that go *from* old MPs j *to* new MPs i, 2004 corruption-exposure are a plausibly exogenous instrument for 2005-2008 corruption-exposure.

[6]The 2004 tax year ended 05/04/05 and the general election was held 05/05/05

This two stage model is shown:

$$
\begin{aligned}
y_{it} &= \beta_0 + \beta_1 x_{it} + \beta_2 \hat{y}_{jt} + \varepsilon_{it} \;\forall\; t \in [05, 08] \\
\hat{y}_{jt} &= \alpha_0 + \alpha_1 y_{j04} + \varepsilon_{jt}
\end{aligned}
\tag{4.5}
$$

Table 4 will later confirm that instruments are also relevant. Ideally, I would use many periods before the 2005 election but data only goes back to 2004. Fortunately, this does not appear an issue: 2004 expenses still strongly correlate with 2008 values and, although 2004 expenses are used for each period, network connections change over time so the final instruments still vary.

4.2.3 Limitations

Note that by restricting the sub-sample to only peer effects that work old-to-new, IVs picks up a distinct Local Average Treatment Effect (LATE). It is plausible that this differs from the Average Treatment Effect (ATE), as will be discussed in Section 6.3.

4.3 Omitted Variables

In a simultaneous equations model like Equation 4.1, coefficients are biased if there is a correlation between individuals' unobserved errors. In the context of peer effects, this may be due to homophily (i.e. similar people self-select into similar groups) and common shocks.

If unobservable errors are positively correlated $Cov(\varepsilon_{it}, \varepsilon_{jt}) > 0$ in the presence of peer effects, the estimate of β_2 is biased upwards. The second term below captures this:

$$
\begin{aligned}
Cov(y_{jt}, \varepsilon_{it}) &= Cov(\beta_0 + \beta_1 x_{jt} + \beta_2 y_{it} + \varepsilon_{jt}, \varepsilon_{it}) \\
&= \frac{\beta_2}{1-\beta_2^2} Var(\varepsilon_{it}) + \frac{\beta_2^2}{1-\beta_2^2} Cov(\varepsilon_{jt}, \varepsilon_{it})
\end{aligned}
\tag{4.6}
$$

Intuitively, suppose i and j become peers because they both have amoral personalities. An outside econometrician may mistakenly conclude that since i and j both over-claim expenses, there is evidence of peer effects, when in fact they independently choose to be corrupt and this is what causes them to be peers. The same logic extends to common shocks. Suppose a new law makes it less likely for corruption to be detected. Both i and j independently choose to over-claim more expenses. An econometrician observes both MPs move in tandem and mistakenly attributes this to peer effects.

4.3.1 Controls for Homophily

Hence I endeavour to control for observables that correlate with corruption and cause MPs to group together. I first consider general sources of homophily and then the three observable networks specifically. These are summarised in Table 1.

Demographics It is plausible that all observable networks pick up differences in age and gender, which may also correlate with corruption. For example, young MPs are more likely to come from regions with young populations, have attended newer 'plate glass' universities, or be part of youth-issue committees. Younger MPs are also likely to have been elected more recently and thus have less experience claiming expenses. Similarly, a case can be made for gender. I thus explicitly control for these demographics.

Political Characteristics MPs with certain political characteristics may self-select into groups due to similar electoral pressures or backgrounds. I propose that the following factors could also be relevant for over-claiming expenses and are thus controlled for: Parties have different ideologies, affecting corruption attitudes; senior MPs are under more media scrutiny, increasing their detection rate; longer serving MPs have more experience over-claiming expenses; and MPs with large majorities are less likely to be 'booted out' if caught.

Geographic Characteristics MPs typically get elected to constituencies where they already live or where the party committee allocates them to, a process they have little control over. There thus appears to be little scope for self-selection. However, regions may have different cultural attitudes towards corruption. Thus I control for the UK's official regions, so only the effect of very close proximity is picked up by the geographic network's variables. Controlling for London is particularly vital, since only MPs outside London qualified for the ACA's second-home policy and could abuse it.

University Characteristics MPs made their university choice before they ran for office and education concerns likely dominated this decision. There thus is little scope for self-selection. However, university choice could affect corruption incentives. For example, MPs with better qualifications may earn a higher wage in the private sector and thus have a lower cost of being 'booted out'. To account for this, I control for the level of education and explicitly for Oxbridge.

Select-committees Characteristics MPs' committee membership is determined alongside over-claiming expenses, creating potential for self-selection. Note that before 2010 MPs were allocated to committees by whips, who did so largely based on party loyalty:

> "Selection has more to do with a record of past or expectation of future service, and proven loyalty, than evidence of interest or expertise in a particular departmental or other area." (Wright, 2009)

Thus, for example, whips may punish rebellious MPs by putting them in the same 'boring' committees. This creates potential homophily in corruption attitudes, as rebellious MPs may also be more prone to rule-breaking or less receptive to whips' encouragement to over-claim. Hence, I use an MP's voting record to control for party loyalty.

Table 1: Summary of Control for Homophily

Variable	Source	Description
female	Every-Politician (2020)	Dummy = 1 if MP identifies as female
age	Every-Politician (2020)	Use birth date to calculate age in years was on 01/01/2000
educ	Who's-Who (2020)	Dummy = 1 if MP completed X (undergrad, master, PhD)
oxbridge	Who's-Who (2020)	Dummy = 1 if attended Oxford or Cambridge (yes = 1; no = 1)
party_X	Every-Politician (2020)	Dummy = 1 if member of X (CONservative, LABour, LIBdem)
senior	Parliament-API (2020)	Dummy = 1 if (shadow) minister, spokesperson, or whip
rebel	Public-Whip (2020)	% votes cast against party majority
incumbent	Parliament-API (2020)	Dummy = 1 if elected before 2005 General Election
majority	Norris (2020)	(votes won - runner-up votes) ÷ all votes [updated if by-election]
r_X	Norris (2020)	Dummy = 1 if constituency in ONS region X (12 total)

4.3.2 Controls for Common Shock

Controlling for common shocks is considerably harder. I include period dummies to account for shocks that affect everyone, such as the 2008 lawsuit to publish MP expenses. However, there is no information on group-specific shocks. Fortunately, this may not be a severe limitation: A thorough review of how the ACA system worked and how the scandal unfolded does not suggest that groups were specifically targeted. Thus financial-year dummies should pick up all shocks.

5 Data

In order to empirically identify peer effects in political corruption, I make use of the UK Parliament's Additional Costs Allowance (ACA), which created an opportunity for MPs to over-claim expenses and culminated in a political scandal. I briefly lay out the relevant context before explaining how data were collected and turned into variables. For control variables see Table 1 above.

5.1 Context

Before 2009, there was much room for MPs' discretion in over-claiming expenses due to The Green Book's vague wording and the Fee Office's "culture of deference" (Legg, 2010). The full extent of corruption emerged when in February 2008 a Freedom of Information request asked for the full release of MPs' expenses. Although the House of Commons initially blocked this request, a whistle-blower leaked all receipts to The Daily Telegraph in April 2009. Subsequent newspaper reports indicted several prominent politicians.

Following the scandal, an independent audit was conducted by Sir Thomas Legg, which reviewed all ACA claims made by living MPs between the financial years 2004/05-2008/09. It found that around half of all MPs broke the rules of The Green Book and recommended a total of £1.3m be repaid. Three MPs were prosecuted for false accounting. Eggers and Fisher (2011) found that MPs implicated in the scandal were less likely to stand for re-election, and those who did suffered electorally. Expenses are now managed by the new Independent Parliamentary Standards Authority, which publishes annual reports.

5.2 Dependent Variable

5.2.1 Measuring Corruption

To separate out corrupt from valid expenses, I use the Legg Report (2010), which independently audited the ACA claims of 659 MPs between 2004/05-2008/09. This contains an Annex that qualitatively discusses its verdict of each MP and how much they should repay. Using this, I manually identified 892 instances of corruption, dated them to the correct financial year, and thereby constructed a panel data set.[7] This generates the dependent variable

[7] There were 39 cases where claims ranged over two or more financial years. I broke these down into monthly averages and added them to the respective period, assuming constant behaviour

y_{it}: how much MP i over-claimed in expenses during period t. To ensure this not instances of corruption were missed, I cross-checked my own totals $\sum_t y_{it}$ with how much the Legg Report recommends each MP i repay overall.[8]

5.2.2 Limitations

There are two main limitations to this measure of corruption. Firstly, while I have a reliable indicator of how much MPs over-claimed, this does not capture other forms of corruption, in particular acting against the spirit of rules. For example, a MP who buys an unnecessarily expensive toaster but keeps their total kitchen cost below £10,000 may not technically violate The Green Book but can still be regarded as 'corrupt'. There may not be external validly in applying estimates based on rule-breaking to rule-stretching. However, given that newspapers and the public seemingly treated both acts as interchangeable, it is plausible that politicians' peer effects do so too.

Secondly, the Legg Report excludes issues that were at the time under investigation by the Parliamentary Commissioner for Standards or police. To get these data, I filed a Freedom of Information request. Whilst I learned that this affected six MPs, actual expenses could not be revealed due to "the cost of complying". However, I was able to recover some over-claimed expenses through mentions in legal judgements and newspaper articles, adding these to y_{it}. Although this issue affected few MPs, it concerns some of the worst offenders who play a key role in my analysis and so I would have liked a more full solution.

[8] 12 MPs had rounding issues of $<$£1; one was miscalculated in the Legg report; four differed irreconcilably but by no more than 1.4% so I maintain trust in my data

5.3 Variables for Peer Effects

As outlined in Section 3, I wish to test peer effects using three observable networks and two plausible causes of asymmetric influence. To do so, I construct two types of variables for each MP i: i's connections in a given network and the 'network corruption' i is exposed to.

5.3.1 Peer Connections

Let $k = Geo, Uni, SC$ denote one of the three networks observable, as discussed in Section 3. If a pair of MPs ij are connected in period t, $z^k_{ijt} = 1$ else 0. I now explain when i and j are defined as connected in each network.

$$z^k_{ijt} = \begin{cases} 1 \text{ if satisfy criteria of } k \\ 0 \text{ otherwise} \end{cases} \qquad \forall k \in \{Geo, Uni, SC\} \tag{5.1}$$

Geography $z^{Geo}_{ij} = 1$ if ij serve constituencies in the same 600km^2 bloc as per the Open Location Code system (Plus-Codes, 2020), something that does not change across periods. To do this I map MPs to their constituencies via Parliament-API (2020) and then assign each constituency a geographical center using Bell (2020)'s dataset. Note that MPs who are close to each other but in different blocs are not picked up by this, but other methods proved too computationally intensive.

University $z^{Uni}_{ij} = 1$ if ij have at least one university in common, though not necessarily attending it at the same time.[9] This connection does not change across periods. I use Who's-Who (2020) autobiographical entries to get data on university attendance and, if no none is listed, check an MP's own/party's website before assuming they did not attend any.

[9] University enrolment dates are not recorded, but future work may want to interact university with age to capture this

Select-committee $z_{ijt}^{SC} = 1$ if ij served in at least one select-committee together simultaneously. Connections in this network can thus change over time. Data is collected from Parliament-API (2020).

5.3.2 Corruption-Exposure Variables

For each network in which MP i is a member, I now construct the amount of corruption that i is exposed to though a given network. This produces corruption-exposure variables that can test for peer effects.

Linear-In-Means In the linear-in-means model (LIM) every MP connected to i has equal influence on i. C_{itk}^{LIM} follows standard procedure in taking the average spending in i's network k at time t, excluding i themselves (Sacerdote, 2011). If i has no connections, their empty network is treated as having zero spending.

$$C_{itk}^{LIM} = \begin{cases} \dfrac{\sum_{j \neq i} y_{jt} \cdot z_{ij}^{k}}{\sum_{j \neq i} z_{ij}^{k}} \text{ if } \sum_{j \neq i} z_{ij}^{k} > 0 \\ 0 \text{ otherwise} \end{cases} \qquad \forall k = \{Geo, Uni, SC\} \tag{5.2}$$

Senior To allow for asymmetric influence based on seniority, a network's total corruption is split into a senior (C_{itk}^{S}) and non-senior (C_{itk}^{NS}) component. Differences between these variables' coefficients would indicate differences in peer effects.

This split is done by including a dummy variable, which has two definitions depending on the network used. For Geo and Uni, MP j is considered senior ($senior_{jt} = 1$) if Parliament-API (2020) shows they held a [shadow] ministerial, spokesperson or whip role in period t. For SC, MP j is instead considered senior in period t if they are the chair of a committee that i is a member of ($chair_{ijt} = 1$). The need for this second definition is because select-committees consist of almost exclusively backbenchers, so $senior_{jt}$ is meaningless, and chairs are more appropriate authority figures in this context (Wright, 2009).

To construct i's corruption-exposure variables, these dummies are interacted with peers' expenses at time t. Unlike LIM, I use the sum, not average, to keep interpretation between C^S_{itk} and C^{NS}_{itk} consistent across different group sizes. Suppose one used averages when C^S_{itk} contained one peer but C^{NS}_{itk} contained six: A £1 increase in C^S_{itk} would represent one peer claiming £1 more, but the same for C^{NS}_{itk} represent six peers claiming £1 more, so £6 total.

$$\begin{aligned} C^S_{itk} &= \begin{cases} \sum_{j\neq i} y_{jt} \cdot z^k_{ij} \cdot senior_{jt} \text{ if } k = \{Geo, Uni\} \\ \sum_{j\neq i} y_{jt} \cdot z^k_{ij} \cdot chair_{ijt} \text{ if } k \in \{SC\} \end{cases} \\ C^{NS}_{itk} &= \sum_{j\neq i} y_{jt} \cdot z^k_{ij} - C^S_{itk} \quad \forall k = \{Geo, Uni, SC\} \end{aligned} \tag{5.3}$$

However, using the sum of corruption-exposure instead of average means that these variables are not directly comparable to the LIM model. This is because C^S_{itk} and C^{NS}_{itk} pick up two separate sources of peer effects: average peer effect *and* number of corrupt peers. C^{LIM}_{itk} only picks up the former. Alternative approaches were considered, but I believe using sums is the most sensible.[10] Note this does mean that LIM and asymmetric influence analysis are not directly comparable directly if group sizes vary, as Section 5.4 will show.

Bad-apple Separating out the worst offender is akin to above. To identify i's senior bad-apple in period t, I take the maximum y_{jt} where j is both senior and connected to i, giving C^{SB}_{itk}. The senior-non-bad-apple network contains all remaining corruption, giving C^{SNB}_{itk}.

$$\begin{aligned} C^{SB}_{itk} &= \max\{y_{jt} \cdot z^k_{ij} \cdot senior_{jt}\} \quad \forall k = \{Geo, Uni, SC\} \\ C^{SNB}_{itk} &= C^S_{itk} - C^{SB}_{itk} \quad \forall k = \{Geo, Uni, SC\} \end{aligned} \tag{5.4}$$

[10] Modelling asymmetric influence using LIM and controlling for group size complicates interpretation considerably. Using the sum of corruption-exposure instead of the LIM model for un-decomposed groups goes against the majority of the peer effects literature

For IVs, the relevant peer effect formula is repeated but with 2004 over-claimed expenses (y_{j04} not y_{jt}). Critically, this means that any peers elected before 2005 are dropped and only old-to-new connections are considered. IVs are identified by adding the prefix “04#”.

5.4 Summary Statistics

An overview of key variables is given in Table 2. The dependent variable is clearly censored: 74% of observations equal zero and 52% of MPs never over-claim expenses at all. This calls for a Tobit model, as per Section 4.1.1. When MPs do over-claim, Figure 2 shows there is a long-tail of extreme values, suggesting there are small groups of serious abusers.

Variable	N	mean	sd	min	max
Legg [y]	2636	346	1395	0	41057
Legg>0 [$y > 0$]	695	1312	2474	6	41057
female	2636	0.20	0.40	0	1
age	2636	50	9.35	22	82
educ	2636	0.81	0.39	0	1
oxbridge	2636	0.27	0.45	0	1
party_CON	2636	0.31	0.46	0	1
party_LAB	2636	0.55	0.50	0	1
party_LIB	2636	0.10	0.30	0	1
senior	2636	0.60	0.49	0	1
rebel	2636	0.01	0.03	0	0.61
incumbent	2636	0.79	0.40	0	1
majority	2636	0.19	0.12	0.00	0.64

Table 2: Summary Statistics of Key Variables

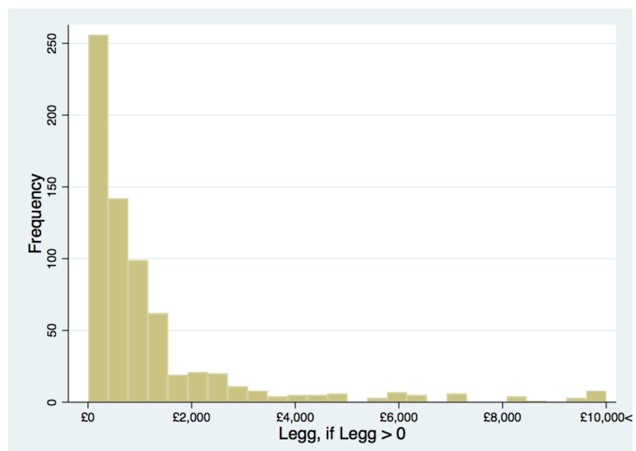

Figure 2: Histogram of Dependent Variable

Breaking down expenses by characteristics, senior MPs do not appear more corrupt than average, accounting for 60% of the sample and 56% of corruption (or 5.3% each for committee chairs). By definition, bad-apples are more corrupt than average, but their total share depends on what network is used to define them: For C_Geo/Uni/SC senior bad-apples make up 13%/7%/14% of the sample and 53%/41%/55% of all over-claimed expenses.

Table 3 summarises the corruption-exposure variables. Due to the unequal proportion of both senior and bad-apple MPs in the sample, there is clear variation of group sizes along these characteristics. Comparing average connections by seniority (see #S versus #NS) and bad-apples (see #SB versus #NSB), a two-sample t-test rejects the null of mean equality at the 1%-level in all cases. As per Section 5.3 this means estimates from asymmetric influence variables [sum] pick up a source of peer effects that LIM [average] does not. Analysis thus refrains from any direct comparisons between these two.

Table 3: Summary of Corruption-Exposure Variables (in £)

Variable	Equ.	mean	sd	min	max	avg. conn
C_Geo#LIM	$C^{LIM}_{it,Geo}$	494	1437	0	25201	3.30
C_Uni#LIM	$C^{LIM}_{it,Uni}$	1019	1081	0	11750	32.8
C_SC#LIM	$C^{LIM}_{it,SC}$	835	1003	0	11750	16.6
C_Geo#S	$C^{S}_{it,Geo}$	422	1295	0	12811	1.83
C_Geo#NS	$C^{NS}_{it,Geo}$	328	1677	0	41057	1.47
C_Uni#S	$C^{S}_{it,Uni}$	6239	8515	0	46217	20.5
C_Uni#NS	$C^{NS}_{it,Uni}$	7209	12171	0	72765	12.4
C_SC#S	$C^{S}_{it,SC}$	291	894	0	9767	0.88
C_SC#NS	$C^{NS}_{it,SC}$	5221	6829	0	45199	15.8
C_Geo#SB	$C^{SB}_{it,Geo}$	382	1213	0	12650	0.62
C_Geo#SNB	$C^{SNB}_{it,Geo}$	369	1707	0	41584	1.21

Notes: "conn" is how many peers an MP is connected to in a given network; corruption-exposure variables have 2,636 observations

Comparing the similarity of observable networks, no combination produces a correlation coefficient above 0.003. Thus networks appear to pick up distinct social groups and multi-collinearity should be negligible in regressions that contain corruption-exposure variables from different networks.

Lastly, Table 4 reports the first-stage results of all IV regressions. In every case the relevant 2004 instrument is large and statistically significant in an F-test, indicating it is valid.

Table 4: First-Stage Results from Instrumentation

Variable	(1a) C_Geo#LIM	(1b) C_Uni#LIM	(1c) C_SC#LIM	(2a) C_Geo#S	(2b) C_Geo#NS	(3a) C_Geo#SB	(3b) C_Geo#SNB
04C_Geo#LIM	0.384***	-0.0101	0.0157				
	(0.0262)	(0.0194)	(0.0189)				
04C_Uni#LIM	0.0217	0.469***	0.00968				
	(0.0866)	(0.0640)	(0.0623)				
04C_SC#LIM	-0.157***	0.0313	0.334***				
	(0.0472)	(0.0349)	(0.0340)				
04C_Geo#S				0.824***	-0.0364		
				(0.0218)	(0.0338)		
04C_Geo#NS				0.00224	0.578***		
				(0.0208)	(0.0322)		
04C_Geo#SB						0.785***	0.0417
						(0.0242)	(0.0416)
04C_Geo#SNB						-0.0123	0.216***
						(0.0149)	(0.0257)
Constant	1,202**	-834.1**	-217.7	158.7	311.6	144.7	-206.5
	(507.9)	(375.4)	(365.6)	(224.3)	(347.3)	(230.1)	(395.5)
N [Obs]	544	544	544	544	544	544	544
R2	0.370	0.383	0.254	0.743	0.428	0.755	0.183
F-test [p]	0.000***	0.000***	0.000***	0.000***	0.000***	0.0000***	0.000***

Notes: Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1; F-test is of joint-significance of all variables

6 Results

6.1 LIM Model

I first test for peer effects assuming all MPs in a network's group have the same influence. This is done by including the LIM corruption-exposure variables of all three observable networks. Results are presented in Table 5.

Table 5: Linear-In-Means Results

Variables	(1a) Legg [y^*]	(1b) E(y)	(2a) Legg [y^*]	(2b) E(y)	(3a) Legg [y^*]	(3b) E(y)
C_Geo#LIM	0.131**	0.0307**	0.122**	0.0263**	-0.180	0.0257
	(0.0528)	(0.0124)	(0.0521)	(0.0113)	(0.331)	(0.0425)
C_Uni#LIM	0.0885	0.0207	0.0469	0.0101	0.0835	0.0170
	(0.0760)	(0.0178)	(0.0879)	(0.0190)	(0.708)	(0.0888)
C_SC#LIM	0.0795	0.0186	0.0687	0.0148	0.450	0.0168
	(0.0806)	(0.0189)	(0.0808)	(0.0175)	(0.556)	(0.0697)
Constant	-2,613***		-5,230***		-1,517	
	(164.5)		(899.0)		(2,238)	
Controls	No	No	Yes	Yes	Yes	Yes
IV	No	No	No	No	Yes	Yes
N [Obs]	2,636	2,636	2,636	2,636	544	544
Pseudo R2	0.0006		0.0118		n/a	
F-test [p]	0.0351**	0.0352**	0.0881*	0.0883*	0.736	0.938

Notes: Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1; y^* represents the latent variable, $E(y)$ the average marginal effect of the observed-variable; F-test is of joint-significance of all variables

6.1.1 Naive Specification

Columns (1a)-(1b) do not include controls or instrumentation, providing a first estimate of peer effects on latent- and observed corruption respectively. In both cases, all corruption-exposure coefficients are positive and jointly significant in a F-test ($p = 0.0351$ and $p = 0.0352$). This suggests that peer effects exist, functioning through some combination of observable networks.

However, the geography network appears the most important, with its point-estimate much large than the rest and the only one that is individually significant. Coloumn (1b) shows that a £1 increase in an MP's exposure to average corruption by geographic peers is associated with that MP over-claiming 3.1p more in observed expenses. Thus corruption seems primarily transmitted through geography, consistent with later specifications too.

Other observable networks may pick up some peer effects but these appear obscured by noise. For example, Appendix A.1 discusses how the Oxbridge network contained in universities appears significant, but its peer effects cannot be adequately distinguished from other reasons why Oxbridge MPs may claim more.

Peer effects appear to be transmitted by affecting MPs that are already corrupt. This can be shown by decomposing Column (1b)'s unconditional marginal effect $E(y)$ into $E(y|y > 0)$ and $P(y > 0)$ parts, as per Section 4.1.1. The effect on probability is negligible, as to be expected when half of MPs remain honest throughout. Even a large £1,000 increase in an MP's exposure to average corruption by geographic peers raises the probability of being corrupt themselves by only 1.23%. Instead, peer effects seem to work by changing the behaviour of already corrupt MPs, a finding that holds across later specifications as well.

6.1.2 Addressing Endogeneity

Columns (2a)-(2b) add a full set of controls to the naive specification to address homophily and common shocks. The change in coefficients is relatively small and significance remains, so peer effects appear robust to it.

Columns (3a)-(3b) uses IVs based on 2004 expenses to account for the reflection problem. Some latent variables turn large and negative, which is counter-intuitive, but since they are insignificant and have no real-world interpretation, I discount this throughout.

More important is that joint and individual significance of observed peer effects disappear with instrumentation. However, interestingly the point-estimate of the geography network's observed peer effect remains at 2.6p in (3b) versus (2b). Change in significance can thus be largely attributed to the drastic reduction in observations that is needed by IVs.

There is also reason to believe that, even after instrumentation, peer effects are underestimated. This follows recent work by Caeyers and Fafchamps (2016), who note that 'exclusion bias' may mechanically arise because agents cannot be peers with themselves. Intuitively, the most corrupt MP necessarily belongs to a group that is less corrupt than they are. Generalising, if i is more [less] corrupt than average, then their group is expected to be less [more] corrupt than them. This creates a negative correlation between an MPs' own corruption and their corruption-exposure, which biases peer effect estimates downwards.

The authors suggest that "to eliminate exclusion bias, it is necessary to control for i's own value of the instrument", which is not possible here as new MPs have no 2004 values to control for. However, note that exclusion bias is most pertinent when fixed effects or relatively small groups are used, neither of which applies to this dissertation.[11] Nonetheless, this largely unexplored source of downwards bias may imply that peer effects than all results report.

6.2 Asymmetric Influence Models

I now test for asymmetric influence in peer effects, decomposing networks by MPs' seniority and then isolating bad-apples. As per Section 5.3, this analysis must be treated separately to LIM, since corruption-exposure variables are calculated via sums not averages.

[11] Average connections in Geo/Uni/SC are 3/33/17 out of a possible 659. This may also contribute to why geography's peer effects appear to matter the most, with its groups being the smallest overall

6.2.1 Senior

Separating an MP's senior and non-senior peers produces Table 6. Geography now appears to be the only observable network that matters, with F-tests rejecting joint significance even without controls in Columns (1a)-(1b) ($p = 0.153$ and $p = 0.154$). Thus, to preserve degrees of freedom, university and select-committee networks are excluded when using IVs in Columns (3a)-(3b).

Table 6: Senior Results

Variables	(1a) Legg [y^*]	(1b) E(y)	(2a) Legg [y^*]	(2b) E(y)	(3a) Legg [y^*]	(3b) E(y)
C_Geo#S	0.116*	0.0273*	0.145**	0.0313**	0.0353	0.0477*
	(0.0628)	(0.0147)	(0.0639)	(0.0138)	(0.214)	(0.0290)
C_Geo#NS	0.0693	0.0162	0.0509	0.0110	0.153	0.00212
	(0.0434)	(0.0102)	(0.0420)	(0.00909)	(0.296)	(0.0377)
C_Uni#S	0.00483	0.00113	-0.0127	-0.00274		
	(0.0161)	(0.00377)	(0.0200)	(0.00433)		
C_Uni#NS	0.00654	0.00153	-0.00405	-0.000875		
	(0.0111)	(0.00261)	(0.0127)	(0.00274)		
C_SC#S	-0.0569	-0.0133	-0.0750	-0.0162		
	(0.100)	(0.0235)	(0.0992)	(0.0214)		
C_SC#NS	0.00179	0.000420	-0.0121	-0.00261		
	(0.0126)	(0.00295)	(0.0129)	(0.00279)		
Constant	-2,529***		-5,086***		-2,408	
	(154.1)		(894.1)		(1,954)	
Controls	No	No	Yes	Yes	Yes	Yes
IV	No	No	No	No	Yes	Yes
N [Obs]	2,636	2,636	2,636	2,636	544	544
Pseudo R2	0.006		0.0121		n/a	
F-test [p]	0.153	0.154	0.123	0.124	0.871	0.871

Notes: Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1; y^* represents the latent variable, $E(y)$ the average marginal effect of the observed-variable; F-test is of joint-significance of all listed corruption-exposure variables

In the geography network, senior MPs appear much more influential than others. The senior peer effect (C_Geo#S) is statistically significant, even after controls and instrumentation: Column (3b) shows a £1 increase in the sum of corruption-exposure by senior peers is associated with an MP over-claiming 4.8p more expenses. By contrast, the peer effect caused by non-senior MPs (C_Geo#NS) is 0.2p, although with large standard errors.

The implied percentage share of senior MPs out of all observed peer effects in a given network can be calculated using Equation 6.1, where P_k^X is the coefficient of X in Table 6-(3b). This implies that senior MPs, though less corrupt than average and making up 60% of the sample, account for approximately 97% of peer effects in the geography network.

$$\frac{\sum_{t=1}^{T}\sum_{i=1}^{N} C_{itk}^{S} \times P_k^S}{\sum_{t=1}^{T}\sum_{i=1}^{N} C_{itk}^{S} \times P_k^S + C_{itk}^{NS} \times P_k^{NS}} \tag{6.1}$$

Nonetheless, a t-test cannot reject the null that coefficients are the same (giving $p = 0.731$). This is unsurprising given the large standard errors in C_Geo#NS, attributable to instrumentation (see Table 4) and the small IV sub-sample. Indeed, the same t-test with controls but no IVs is closer to significance (giving $p = 0.252$). Thus, my results are somewhat suggestive of differences by seniority, but less noisy data is needed to make a stronger statement.

6.2.2 Bad-apple

Bad-apples are now isolated from the senior groups in the geography network to see if they are more influential. Results are presented in Table 7.

Senior bad apples (C_Geo#SB) appear more influential than other senior MPs (C_Geo#SNB), with its coefficient statistically significant even after controls and instrumentation. Column (3b) shows a £1 increase in over-claimed expenses by the worst senior geographic neighbour is causes a MP to over-claim 4.9p more, almost double that of other senior peers (2.6p). Using the principle of Equation 6.1, it appears that senior bad-apples, whilst only making up 13% of the sample, account for 53% of all corruption and 65% of geography peer effects.

However, I am again unable to reject the null that coefficients are the same in a t-test (giving $p = 0.816$). As with seniority, part of this is attributable to C_Geo#SNB's instrument being noisier (see Table 4) and the small IV sub-sample. A check of the same t-test with controls but without instrumentation produces somewhat better results, though still not significant (giving $p = 0.176$). Hence care must be taken with interpretation.

Table 7: Bad-apple Results

Variables	(1a) Legg [y^*]	(1b) E(y)	(2a) Legg [y^*]	(2b) E(y)	(3a) Legg [y^*]	(3b) E(y)
C_Geo#SB	0.133**	0.0312**	0.161**	0.0348**	-0.0261	0.0489*
	(0.0667)	(0.0157)	(0.0678)	(0.0147)	(0.218)	(0.0297)
C_Geo#SNB	0.0611	0.0143	0.0446	0.00964	-0.879	0.0256
	(0.0430)	(0.0101)	(0.0418)	(0.00904)	(0.706)	(0.0914)
Constant	-2,462***		-5,150***		-1,733	
	(124.9)		(893.6)		(1,690)	
Controls	No	No	Yes	Yes	Yes	Yes
IV	No	No	No	No	Yes	Yes
N [Obs]	2,636	2,636	2,636	2,636	544	544
Pseudo R2	0.0005		0.0119		n/a	
F-test [p]	0.0245**	0.0247**	0.0179**	0.0180**	0.442	0.251

Notes: Standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1; y^* represents the latent variable, $E(y)$ the average marginal effect of the observed-variable; F-test is of joint-significance of all listed corruption-exposure variables

6.3 Reflection Multiplier Effect

When using IVs, the reflection bias should be eliminated, and thus coefficients are expected to be smaller in Column (2b) than (3b) across all Results Tables. How much they shrink gives a preliminary indication of how large the reflection multiplier effect is.

Interestingly, coefficients do not shrink by much in any specification. In the LIM model, C_Geo remains stable (see Table 5), and under asymmetric influence models some point-estimates even increase (of particular interest are C_Geo#S in Table 6 and C_Geo#SB in Table 7). Whilst it thus appears that the reflection bias and its upwards 'multiplier effect' are small, it also needs to be explained why coefficients in some cases actually rise with IV.

I propose that what reflection bias does exist may be dominated by differences in ATE and LATE. Recall that Column (2b) provides an estimate using the whole sample but (3b) is restricted to old-to-new connections to use IVs. Hence, if peer effects going old-to-new are stronger than the rest (old-to-old, new-to-new, new-to-old), coefficients may appear to increase.

Indeed, the causal channels outlined in Section 2 are consistent with old-to-new peer effects being larger than others. In an informational externality framework, new MPs, who have little experience, may be more influenced by old MPs, who have much experience. In a conformity preference framework, it is plausible that social norms are set by those who have been part of the system the longest.

In both cases, one would expect there to be a large initial gap between old and new MPs, which declines over time as new MPs integrate into the system. Data is suggestive of this: Old MPs over-claim £344 more than new MPs to begin with (close to the sample average of £346), but this gap steadily decreases by £59 each period, significant at the 1%-level and robust to controls. However, this is not direct evidence; it could be that new MPs learn corrupt practices independently of peer effects. Nonetheless, a difference between LATE and ATE seems an intuitive explanation for IV coefficients increasing.

7 Conclusion

I begin my conclusion by reflecting on the main limitations that must be kept in mind when interpreting results and that future work may build one. Firstly, the use of IVs, whilst critically helping to overcome the reflection problem, is costly in degrees of freedom, as few MPs were newly elected in 2005. This makes it challenging to establish statistical significance, particularly when comparing coefficients. Testing corruption peer effects using a data set bigger than the one manually constructed appears a way forward.

Secondly, coefficients may not capture the true magnitude of peer effects. On the one hand, 'exclusion bias', which cannot be address by my instruments, may imply that peer effects are even larger than estimated. On the other hand, IVs' LATE estimates are plausibly larger than the average treatment effect, since there is reason to believe that old-to-new connections are of particular importance. Further note that the methodology used was highly specific to the context of British politics, making use of FPTP, cabinet minister positions, and Oxbridge. There may thus be limited external validity, particularly in a development context where studies of corruption appear most pertinent.

That said, this dissertation presents several interesting findings. Foremost, to the best of my understanding, this dissertation provides the first direct evidence of peer effects in political corruption. This justifies models of self-reinforcing corruption. Interestingly, peer effects overwhelmingly influence those who are already somewhat corrupt and the reflection multiplier effect appears small.

Moreover, testing three observable networks (geography, university, select-committees), it appears that peer effects are primarily transmitted between geographic neighbours, with its estimates robust to controls and IV. In the geography network, a £1 increase in an MP's total corruption-exposure by senior peers causes them to over-claim 4.8 pence more themselves. Geographic connections may be particularly important because MPs cooperate on issues facing their similar constituents, or know each other from local politics.

Lastly, and perhaps most interestingly, this study provides preliminary evidence that some peers matter more than others. Senior MPs, though not more corrupt than average, cause larger peer effects in the geography network (4.8p versus 0.2p). And out of senior MPs, the 'bad-apple' has the most influence (4.9p versus 2.6p). In fact, whilst senior bad-apples make up only 13% of the sample, they account for 53% of over-claimed expenses and 65% of observed peer effects in the geography network.

To my understanding, models of self-reinforcing corruption do not yet fully incorporate asymmetrical influence, but this could have critical implications for tackling corruption traps. If a small subset of 'key-players' account for the vast amount of peer effects, smartly targeting these individuals could produce out-sized results. That is, not only would it reduce the corruption of these key-players, but this may then be amplified by peer effects to reduce the corruption of others too. Such a 'smart push' could push society past the 'tipping-point' or, by undermining the feedback loop, even eliminate high-corruption equilibria. A smart push seems more feasible for resource constrained reformers, who do not have the capacity to enact the big-push that is typically recommended.

These implications are only preliminary and must be integrated into existing models to be fully understand. They appear to closely relate to key-player network models (Ballester et al., 2006; Zenou, 2016), which have been developed in the context of gang crime (Liu et al., 2012) and lobbying (Battaglini and Patacchini, 2018).

If anything, this dissertation shows that much more work is needed on self-reinforcing corruption, both theoretical and empirical. Synthesising existing research in Political Economics with Network Economics seems a particularly promising avenue for future work.

A.1 Oxbridge Network

Isolating Oxbridge from the university network in the LIM model's naive specification produces large coefficients significant at the 10%-level: A £1 increase in Oxbridge peer's over-claimed expenses is associated with a 21p in latent- and 5.0p in observed over-claimed expenses. Intuitively, this could be because Oxbridge connections are more valuable (Tholen et al., 2013; Watters, 2016), and thus maintained.

However, Oxbridge peer effects may be subject to homophily, such as these MPs having a better outside option in the private market and thus less worried about being caught. Including an Oxbridge dummy, isolates variation in corruption-exposure between Oxford and Cambridge. However, this variation is small, and so coefficients lose significance. A better approach would be to directly control for potential homophily (e.g. income, wealth, social class) but no adequate data could be found.

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