Essays in Empirical Corporate Finance

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To my grandfather, who'll probably get a kick out of a dedication

Preface

This report is a result of a research project carried out at the department of Finance at the Stockholm School of Economics (SSE).

This volume is submitted as a doctor's thesis at SSE. The author has been entirely free to conduct and present his research in his own ways as an expression of his own ideas.

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Filip Wijkström
Associate Professor
SSE Director of Research

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Introduction

Ideally, the board of directors represents the interests of shareholders and oversees the work of the CEO. This is especially important in situations where the CEO and shareholders have conflicting interests, such as when setting the CEO's compensation. In the first chapter of this thesis, I study situations where board members are unlikely perform their role as safeguards of shareholder interests. Specifically, I am interested in cases of board overlaps, i.e. when the CEO of a given company also serves on the board of some other company, and the CEO of that other company serves on the board of the first company. Such a situation would allow for a kind of compensation game to be played, in which the two CEOs reciprocally raise each other's compensation. Alternatively, the personal relationship between the two could influence how they behave in the compensation neg8otations, and hence the compensation outcome, even in the absence of any kind of reciprocal understanding. The study of these issues is of vital importance, as it informs the ongoing debate, both academicals and popular, on managerial excesses. The idea that both board members and top management belongs to an "old boys club", whose common interests take precedence over those of the shareholders, would very much be captured in the kind of situations that I study. I find very suggestive evidence that individuals engaged in the kind of overlapping relationships described above, let their loyalties shift in economically significant ways. Much time in the first chapter is spent on ensuring that the documented effects are not spurious, e.g. by mapping and controlling for the wider social networks of the involved individuals, and by performing placebo like tests on non-CEO managers.

The second thesis chapter, which is co-authored with Robert Tumarkin, is concerned with behavioral effects in the consumer real estate market. Earlier research has documented how seemingly irrelevant factors often influence economic actions through so called behavioral biases, i.e. systematic mistakes made when (often unconscious) rules of thumbs are used to make complicated decisions. However, studies of these kinds of effects in the consumer real estate market have been scarce, even though purchasing a home is by far the largest and most important financial decision in most people's lives. The main reason for the scarcity of studies in this area is likely related to a lack of appropriate micro-level data and institutional settings that lend themselves to econometric tests. Specifically, buyers and sellers each employ their own realtor in a complicated negotiation process in the US and data access in other countries is typically very limited. In our study, we take advantage of the different structural characteristics of the Australian real estate market to test for a specific behavioral effect; does the attractiveness of a realtor affect the final purchase price of a home? And, if so, through what mechanism does realtor attractiveness translate into price? This test is feasible because Australian homebuyers almost never use a realtor to find homes or to negotiate price. In-

stead, the agent hired by the seller serves as a single intermediary between her and the buyer.
Technically, the phenomenon we are interested in is known as the *halo effect*, which is a cognitive bias, through which positive perceptions of personal traits can affect perceptions of other, unrelated characteristics. The halo effect is well established in the psychology literature, especially for physical attractiveness. For instance, researchers have found that attractive people are perceived as more intelligent and socially competent, are less likely to be punished receive better academic and professional evaluations etc. We propose that the same effect plays an important role in the consumer real estate market as the attractiveness of realtors influences the valuation of pieces of real estate in an economically significant way. Our results show that buyers that encounter realtors one standard deviation of attractiveness above average tend to pay a 2.3% premium for their house. At the mean house price, this corresponds to about AUD 16,700 during the sample period.
In the last chapter, I focus on the behavior of corporate insiders in the presences of legislation that impose occasional general bans on the stock of their own company. I hypothesize that insiders react to such bans by trading in substitute assets, i.e. assets for which they are not classified as insiders but for which their inside information is still material to some extent. I test this hypothesis using the introduction of new UK legislation. Although the results and welfare effects are ambiguous, the study highlights little understood implications of mandatory trading bans. This is especially relevant as there is an ongoing process of introducing UK style legislation all over Europe as part of the effort to harmonize security laws.

Tit-for-Tat Compensation*

In this paper I study tit-for-tat pairs, i.e. situations where CEOs serve on each others' boards in a way that makes it possible for them to reward (punish) favourable (negative) compensation outcomes partly influenced by the other player. I find that the residuals from predictive regressions of CEO compensation are positively correlated in such pairs, implying that these relationships indeed play a role in the compensation decisions. The result is robust to the inclusion of various CEO and board centrality measures as well as a control for the average effect of tit-for-tat relationships. When studying a sample of non-CEO top executives (whose compensation is typically recommended by the CEO, rather than the board) none of the effects are present. This indicates that the results are not driven by some unobserved characteristic of the firms whose CEOs form tit-for-tat pairs.

*I am indebted to Per Strömberg for continuous advice and feedback. I would also like to thank SamuliKnüpfer, Paolo Sodini, and seminar participants at the CEPR Summer Symposium evening sessions in Gerzensee, the Nordic Finance Network workshop in Copenhagen, SIFR and the SNS Corporate Governance Network in Stockholm for helpful comments. I'm also grateful to Studieförbundet Näringsliv och Samhälle and Bankforskningsinstitutet for financial support.

1 Introduction

The board of directors is responsible, among other things, for setting the compensation of the CEO on behalf of the shareholders. It is crucial for the integrity of this arrangement that board members do not have a personal interest in the CEO's compensation, or at least that they are not unduly influenced by such an interest. Consequently, CEOs that also serve as board members are routinely excluded from voting on their own compensation. In this paper I examine a situation where the interests are more subtle and where the affected board members are therefore not barred from voting. This situation occurs when a board member serves as a CEO in some other company on whose board the CEO of the first company serves, i.e. the two individuals switch roles in the two companies. As it is the shareholders who ultimately pay for compensation that directly benefits the CEOs, these individuals are potentially able to play a non-zero-sum tit-for-tat game where they reciprocally raise or lower each other's compensation levels. I refer to such relationships as tit-for-tat pairs and show how tit-for-tat-like behaviour is indeed common within these pairs.

I first demonstrate how the positive effect of interlocks on compensation that has been documented by e.g. Hallock (1997) is likely due to the covariance of such interlocks with social network centrality. I expand on the literature by building a social network spanning both CEOs and board members simultaneously. Being able to control for the highly correlated centralities of both these groups, I show that it is CEO centrality that is economically relevant. Although the separation of CEO and board centrality is novel, the general finding that social network centrality is associated with higher CEO compensation is not. However, that finding fits several alternative stories. An obvious candidate is that CEO centrality is a proxy for ability. It is likely that other individuals in the network are more interested in forming social ties to skilled and influential CEOs. That is, other agents act in a way that increases the centrality of high performing CEOs. A slight twist to the story would be that high performing CEOs would themselves act to become more central, although those acts themselves would be economically unimportant. Specifically, high performing CEOs would be more likely to end up on many and important boards, thus increasing their centrality measures. Yet another slight variation to this story is that there is some other characteristic that is associated with both performance and centrality, such as being exceptionally sociable. Quite different stories emphasize the functional role of centrality itself. For instance, a large professional network may allow CEOs to better acquire information, solicit advice or call in favours. If this is true centrality should be priced directly, thus causing the observed correlation. Yet another possible explanation is that central CEOs use their social influence in their own interest, rather than in that of the shareholders, for instance by putting pressure on board members to raise their compensation. This kind of influence has received much attention in the literature, most prominently in the well-cited book by Bebchuk and Fried (2004).

In short, interpreting the correlation between social network centrality and CEO compensation is very difficult. Recognizing this, I focus on effects of tit-for-tat relationships that are left after controlling for centrality and that therefore cannot be ascribed to the stories above. Rather than focusing on the average compensation effects of tit-for-tat relationships as in Hallock (1997), I view these relationships as a stage on which tit-for-tat games are potentially being played and examine the covariation of compensation within such pairs. If the individuals in such pairs are explicitly agreeing to cooperate or if they are in some other way motivated by the effects on their own compensation, they would be engaged in an ethically and legally highly dubious practice. I would not expect everyone who is given the opportunity to engage in such practices to take it. Furthermore, the extent to which the game is played should vary even among pairs that fall for the temptation. I exploit this variation to show that tit-for-tat behaviour is indeed common when the situation allows for it and its effects are economically significant. I discuss whether the covariation of compensation within tit-for-tat pairs can be the result of varying personal relationships between the two individuals and argue that this is a more likely explanation than explicit corruption.

Specifically, I estimate a number of regressions of CEO compensation. Apart from the standard controls proposed in the literature, the effects of CEO and board centrality in the social network are controlled for. I also include a dummy variable indicating whether the CEO in question is in a position to play the kind of tit-for-tat compensation game that I want to examine.[1] My focus is on the residuals from these regressions. As this is the unexplained part of the observed compensation, the effects of any tit-for-tat behaviour would be in there. This should cause the residuals of CEOs within these pairs to line up, i.e. an unduly (and otherwise unexplained) high compensation would be repaid with a similarly high compensation. By regressing the average residuals in each pair on each other, I show that this is indeed the case and that the effect is statistically and economically significant. This is the case regardless of whether I use only contemporaneous compensation decisions or allow for favours to be returned with some lag. The point estimates from these residual regressions are also quite stable to the specification of the original compensation regressions. The lowest estimate, which occurs when using the full set of controls and only contemporaneous compensation decisions, is 0.27. This should be interpreted as an elasticity, i.e. a one percent increase of the compensation given by the first individual in a pair is repaid by an average 0.27 percent increase given by the second individual. A one standard deviation increase in the residual would result in an average increase of $426,000 of the other individual if evaluated at the mean compensation of the entire sample and in an increase of $1,223,000 if evaluated at the mean of those individuals that are part of tit-for-tat pairs (and who tend to work for larger firms

[1] The definition of this dummy variable will differ slightly from that in Hallock in order to exploit the time dimension in my dataset.

which give higher compensation). This corresponds to 18 percent of their annual compensation.

The research design depends crucially on the specification of the original compensation regressions. Although the results seem robust to the choice of control variables, the concern remains that there is some omitted variable that relates to both compensation and the formation of tit-for-tat pairs. Such a variable would show up in the residuals and possibly be responsible for the covariation within the pairs. To get at this I replicate my methodology on a sample of non-CEO executives, whose compensation is typically set either by the CEO directly or by the board on the recommendation of the CEO. The scope for tit-for-tat games between board members and these executives should be greatly reduced whereas most stories depending on a misspecification of the compensation regressions should apply equally in this case. However, there is no corresponding effect in the sample of non-CEO executives in spite of comparable sample sizes. It is therefore highly unlikely that my results are driven by an omitted variable in the original compensation regression.

The main contribution of this paper is to show how specific and economically significant actions are taken within the context of social networks. This is the first paper to document this kind of tit-for-tat behaviour and tie it to specific interpretations. The actions taken are of particular interest as they appear to have little to do with the interest of shareholders, on whose behalf they are ostensibly taken. Even if we are not dealing with explicit corruption, which is one potential explanation of the behaviour, these actions arguably constitute neglect of the board members' fiduciary responsibility to the shareholders.

2 Related Literature

Although studies of the conditions described above are scarce, other but similar situations have received considerate attention. In 1914 the US congress passed the Clayton Antitrust Act, making it illegal for directors of competing companies to serve on each others' boards. The intent of this law was to discourage collusion between companies, such as price fixing, and did not focus on agency problems between the board members and the owners. Interlocking directorships therefore remained legal and quite common in non-competing firms. There is an old and vast literature examining the functions of such interlocks. Theories range from giving influence to important stakeholders (Thompson & McEwan (1959) and Stiglitz (1985)) to signalling legitimacy (DiMaggio & Powell (1983) and selection issues (Zajac (1988) and Mills (1956)). In a more recent paper, Fich& White (2005) examine the determinants of interlocks and find that they are more likely to occur when boards have more outside directorships and less likely to occur when the board is more active or when the CEO receives more of her compensation in the form of stock options.

A recent empirical literature focuses on the potential agency conflicts inherent in interlocks of different kinds. Much of this literature draws on methods in sociology to map the entire social networks of financial agents. Measures of an individual's centrality in such networks are then related to economic outcomes. Fracassi& Tate (2008) map social, educational and professional ties between CEOs and board members and associate this with fewer company-initiated earnings restatements and more unprofitable takeovers, which they argue indicates weaker monitoring.

Particularly relevant for this study are papers that relate these measures to executive compensation. Barnea&Guedj (2007) map the network of directors in S&P 1500 firms and find a positive relation between the centrality of a company's board and the compensation of its CEO. They interpret this as a sign of weaker monitoring by more connected board members. Others have studied the connections of CEOs themselves, rather than those of board members. Hwang & Kim (2008) map the dependence between CEOs and board members via social ties, as proxied by a shared alma mater, military service, regional origin, academic discipline or industry. They find that boards with a majority of independent members that lack social ties to the CEO give lower compensation. Larcker at al (2005) map the network of US board members (not counting CEOs and board members of the same company as directly linked) and calculate the geodesic distance between CEOs and board members in the same company. They find these measures negatively correlated to CEO compensation. In an early, but methodologically somewhat different paper, Hallock (1997) defines interlocks as occurring when the CEO of some company A serves on the board of some other company B, while the CEO of company B serves on the board of company A. He shows that firms whose board members and CEO interlock in this sense tend to give their CEOs higher compensation. Like most papers in this literature, Hallock struggles to nail down the mechanism through which the effect works. He observes that two interlocking CEOs "may have both the incentive and the opportunity to raise each other's pay" but lacking a good understanding of how such interlocks arise this remains an unproven hypothesis. Nguyen-Dang (2008) study interlocks in a sample of French firms and find evidence suggesting that interlocking CEOs are less likely to be fired due to poor performance.

3 Data

I obtain data on board membership and board member characteristics for the years 1996 to 2006 from the RiskMetrics (former IRRC) Directors dataset. This dataset covers all directors of S&P 500, S&P MidCaps and S&P SmallCaps companies during the period. Data on executives is retrieved from the ExecuComp dataset for the same time period. I merge the two datasets and match companies on CUSIP codes and individuals on name and their affiliated companies. In order to ensure a full and accurate match I double check it by matching both datasets to the Thomson Reuters Insider Filing dataset, which assigns a personal ID number to each individual regardless of why she has insider status, e.g. whether she is a board member or a manager. The result is a dataset with a total of 26196 unique individuals and 2708 unique firms. Control variables are obtained from Compustat and the RiskMetrics Governance datasets. Following Barnea&Guedj (2007), I drop all observations where the CEO has a salary that is lower than $50,000 a year. The purpose is to avoid cases where the CEO has voluntarily taken an exaggerated pay cut (or completely waived a salary) as a gesture of good will. Some additional variables are taken from the ExecuComp database which is already matched to ExecuComp. Summary statistics are given in Table 1.

3.1 Social network centrality

In order to get a measure of each individual's importance in a social network I calculate standard measures of network centrality. To construct the network, I let each individual in my dataset be a node and let two nodes be linked in a given year if the two individuals are affiliated with the same company that year. To be affiliated with a company an individual can either be a member of its board of directors or be reported as a manager in the company's proxy statement (and hence appear in ExecuComp). These nodes and links, i.e. the network, is described by an adjacency matrix, G, in which each row represents a node and each element the linking status of two nodes so that $G(i, j) = G(j, i) = 1$ if individual i and j are linked and $G(i, j) = G(j, i) = 0$ otherwise. By convention $G(i, i) = 0$, i.e. individuals are not considered to be linked to themselves. The network is remapped each year. I calculate a number of centrality measures that give a sense of the importance of each node. The most straightforward is degree centrality, which is simply the number of links of each node. The vector of degree centralities is $Degree = G \cdot \mathbb{1}$. Two more sophisticated measures, *betweenness* and *closeness centrality*, is based on the idea of *geodesics*, or *shortest paths*. A path between two nodes, i and j, exists when they are linked to each other (possibly via other nodes) such that no node is passed twice. The shortest path between the two nodes is the path with the fewest intermediate nodes and the required number of steps is called the *geodesic distance*, $d(i, j)$. Summing the geodesic distances from a particular node to all other nodes gives the *closeness centrality* of that node as in Sabidussi (1966). This measure is commonly inverted so that higher closeness means that a node is more central in the network, $Closeness_i = \left(\sum_{j \neq i} d(i,j)\right)^{-1}$. Counting the number of times that a node lies on the shortest path between two other nodes gives the *betweenness centrality* measure of Freeman (1979). I further calculate two measures based on *walks*. A walk is like a path, except it places no restrictions on the repetition of nodes or links. The eigenvector associated with the largest eigenvalue of the adjacency matrix, *G*, is the vector of *eigenvalue centralities*. The eigenvector centrality of a given node is proportional to the sum of the centralities of the nodes to which it is connected and it is therefore important to connect to central rather than to peripheral nodes. The final measure considered is the *Bonacich centrality* of Bonacich (1972). The measure is the (weighted) number of walks starting in a given node *i*, $Bonacich_i = \sum_j \sum_{k=0}^{\infty} a^k G^k(i,j)$. The parameter *a* determines the relative weight of walks of different lengths in that walks of length *k* are weighted by a^k. There is little theory to guide the choice of *a*. Hanaki et al (2006) set *a = 0.1* and claim that it is a standard choice. I have set *a = 0.02*, mainly for computational tractability.[2] If *a* is very low, the Bonacich centrality measure converges to Degree centrality.

These centrality measures give a value to each node (representing an individual) each year. For executives, this value makes up the relevant variables. I take the average of each centrality measure of all directors in a company a given year to make up the board

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[2] If *a* is low enough the Bonacich centrality will converge to $\left[I - aG\right]^{-1}$ $\cdot 1$, considerably simplifying computations.

centrality variables. Summary statistics for the different centrality measures are given in Table 2. A correlation matrix is given in Table 3. The high correlation between the different measures makes it less important exactly which one is used in the regression specifications below. For briefness I will often report results for the eigenvector measure only. In these cases all analysis has been made for all centrality measures with the same qualitative results.

4 Research hypotheses

My main concern in this paper is the compensation games that are potentially being played within tit-for-tat pairs (H_3 below). I will, however, examine two other issues of some interest in preparation of that analysis (H_1 and H_2 below). Much of the existing literature focuses on links between board members even though the person with the highest stake in CEO compensation is arguably the CEO herself. Intuitively, it seems that hers would be the most important links. This is especially troublesome as the CEO by definition works in the same company as the board members and CEO centrality measures are highly correlated to those of board members as is evident in Table 3. Failing to account for this introduces a risk that board member centrality simply proxies for the centrality of the CEO. This is primarily an econometric issue, as the CEO centrality measures are themselves proxies for some more intangible social phenomenon. If we use board member centrality rather than CEO centrality we simply use a weaker proxy. However, interpretations may differ greatly depending on what we think the proxy captures. Using the centrality of board members as a proxy is a short step away from thinking that there is something about board member centrality that effects CEO compensation, for instance that the social ties of more central board members make them weaker monitors .

Some papers, such as Larcker et al (2005), recognize the importance of CEO connections, but do not use these as controls when investigating board connections. Furthermore, their network measures are not as developed as those of e.g. Barnea & Guedj (2007). I fill this gap by studying the networks of CEOs and board members simultaneously. As CEO centrality intuitively seems more relevant for compensation decisions, my first testable hypothesis is:

H_1: Board member centrality is correlated to CEO compensation even when controlling for CEO centrality.

As I will show later, the tit-for-tat pair dummy is positively correlated with measures of network centrality. This makes intuitive sense, as individuals with many board and management positions tend to be more central and are more likely to end up in a tit-for-tat pair. It is therefore hard to interpret a tit-for-tat dummy in a compensation regression that does not control for centrality. Hallock's results may, at least in part, be driven by such a misspecification. These concerns result in my second hypothesis:

H_2: The estimated effects of tit-for-tat relationships are robust to controls for network centrality.

The central hypothesis of the paper concerns tit-for-tat pairs, i.e. situations where individual 1 is the CEO of company A while serving on the board of company B and individual 2 is the CEO of company B while serving on the board of company A. These individuals are in a position to reciprocally change each other's compensation and I expect them to seize that opportunity. I hypothesize:

H_3: A high (low) compensation for one CEO in at tit-for-tat pair is not associated with a high (low) compensation for the other CEO.

By focusing on correlations rather than on the level effects of centrality, which has been the main approach in the existing literature, I will be able to separate my story from the alternatives.

5 Empirical analysis

In this section I proceed to test the three hypotheses given in section 3, with focus on H_3.

5.1 CEO vs. board member centrality

In order to test H_1, I first seek to establish that both CEO and board centrality are positively related to CEO compensation. In order to do this I regress compensation on centrality and a set of control variables. Specifically, my dependent variable is the natural log of total executive compensation including the value of any option grants.[3] I use three sets of control variables. Specification 1 includes only basic firm characteristics. Specification 2 also includes corporate governance variables. Specification 3 adds several additional controls, which are all standard in the literature. The *Pair dummy* variable is a dummy that takes the value of one if the executive is part of tit-for-tat pair that year and zero otherwise. It will be important to control for this level effect when testing H_2. It has no significant effect in this regression and may

[3]Varible TDC1 in the ExecuComp dataset.

be excluded without affecting the results. The regressions take the following form:

$$\ln\left(TC_{i;t}\right)=\alpha+\sum_j \beta_j^{Ctrl}Ctrl_{i;t}^j+\beta_{Pair}Pair_{i;t}+\beta_{Cent}Cent_{i;t}+\beta_{BoardCent}BoardCent_{i;t}$$
$$+\sum_y \beta_y^{Year}Year_t^y+\sum_z \beta_z^{Industry}Industry_{i;t}^z+\varepsilon_{i;t}$$

Cent and *BoardCent* vary between the different centrality measures described above. I alternatively restrict either β_{Cent} or $\beta_{BoardCent}$ or neither to zero. The full regression results using the Eigenvector measure of centrality are given in Table 4. For testing H_1, the variables of interest are the centrality measures on the first two rows. When one measure is restricted to zero the other has a significantly positive impact, as one would expect from the earlier literature. When both measures are included, however, only CEO centrality appears to matter and board member centrality even gets a negative (but statistically insignificant) point estimate. I re-estimate these regressions while varying the way I calculate CEO and board member centrality. The other control variables are not sensitive to this variation, and in Table 5 I only report the two variables of interest. The findings are still in line with H_1, except when using the smallest set of controls and *Betweenness* to measure centrality. For specification 2 and 3 when using the *Betweenness* measure, the point estimates of the board member variable are positive smaller than that of the CEO centrality variable and statistically insignificant. As the two centrality variables are highly correlated that insignificance is possibly due to collinarity. With this caveat H_1 should be rejected. It is likely that the CEO centrality is the relevant factor and this should be kept in mind when interpreting any findings concerning the centrality of board members.

5.2 CEO centrality and tit-for-tat pairs

H_2 can be evaluated directly by looking at Table 4. The tit-for-tat pair dummy is positive but not statistically significant even in the absence of a control for CEO centrality. Though not immediately comparable to the results in Hallock (1997) this is in line with his findings. When controlling for CEO centrality the point estimate is roughly halved, in accordance with H_2. It is likely that Hallock is, at least in part, capturing the underlying factor that drives CEO centrality.

5.3 Tit-for-tat compensation games

I collect the residuals from the compensation regressions and match them within each tit-for-tat pair whenever the relationship is active in both companies in a given year. That is, when in a given year individual X is the CEO of some company A and serves on the board of some other company B while in that same year individual Z is the CEO of company B and serves on the board of company A. Formally, I construct the indicator variable $I_{i;j;t;s}$ that takes the value of one when individual i is an executive in a company on which board individual j serves in year t, *and* individual j is an executive at some other company on which board individual i serves in year s:

$$I_{i;j;t;s} = \begin{cases} 1 \text{ if } (i;j;t;s) \text{ constitutes a tit-for-tat pair as described above} \\ 0 \text{ otherwise} \end{cases}$$

Using this indicator variable, I define the two vectors ${}^{1}P^{1}$ and ${}^{1}P^{2}$ as follows:

$$
\begin{aligned}
{}^{1}P^{1}_{(i;j);t} &= I_{i;j;t;t}\varepsilon_{i;t} \\
{}^{1}P^{2}_{(i;j);t} &= I_{j;i;t;t}\varepsilon_{j;t}
\end{aligned}
$$

The first superscript refers to the window length which I shall vary below. For now it is not important. The second superscript is arbitrarily assigned to designate one individual player one and the other individual player two. For simplicity, I will assign each pair *(i,j)* an ID so that ${}^{1}P^{1}$ and ${}^{1}P^{2}$ can be given scalar indices rather than be indexed by ordered pairs. $\varepsilon_{i;t}$ refers to the residual from the CEO compensation regression for individual *i* in year *t*. If the individuals in tit-for-tat pairs are indeed playing some kind of reciprocal compensation game, the effects of this will be in $\varepsilon_{i;t}$. As the extent of any coordination is likely to vary, an implication is that these residuals will be correlated. Eyeballing the residuals, which are plotted against each other in Figure 1, gives some support to this idea. The figure shows residuals from the compensations regression using all controls and the eigenvector measure of centrality, but is representative for the residuals from variations of that regression. To make a formal test I regress the residuals on each other and present the results in the first three columns of Table 6. As these are residuals from regressions of logged total compensation, the point estimates can be interpreted as elasticities. That is to say that 27 percent or more of a compensation increase in the average tit-for-tat pair is reciprocated, depending on the specification. The standard deviation of the residuals plotted in Figure 1 is 0.67, meaning that a one standard deviation increase in the residual translates to an average total compensation increase of 18 percent. At the mean compensation levels that are given in Table 1, this corresponds to \$426,000 for the entire sample and \$1,223,000 for the subsample of active tit-for-tat pairs. It is worth reiterating that this is the average for all CEOs in tit-for-tat pairs. Since it is improbable that all of them actually take part in these ethically dubious games, the true elasticity conditional on the CEO actually playing the game is likely higher.

It is not obvious how the decision to start playing the game is reached. It is possible that an explicit agreement is made, but it could also be more subtle. Since both individuals in a pair work closely together they are likely to develop a personal relationship of some sort. Such a personal bond may induce both individuals to be more generous in the compensation decision even if there has been no formal agreement. Helping out a friend and college in this way may not even be perceived as unethical by the players themselves. Of course, personal relationships between board members and CEOs are not unique to individuals in tit-for-tat pairs. I am not claiming that such individuals on average have more cordial relationships than others only that the relationships, and whatever effects they have on compensation, carry over from one company to the other. That personal relationships are unlikely to be more or less cordial within tit-for-tat pairs may help to disentangle that story from a story that builds on more explicit agreements. I would expect blatant corruption to be highly beneficial to the players on average since I am disregarding any legal or career consequences it might have. The two stories therefore have different predictions on the tit-for-tat

dummy, which was found to be positive but not significantly significant in Table 4. This supports the relationship rather than the explicit corruption story.
Furthermore, there is no reason to think that the effects of personal relationships would be restricted to simultaneous compensation decisions. A CEO that has developed a cordial (or antagonistic) relationship with a board member is likely to act on that relationship even if their roles are not reversed until later. Explicit agreements, on the other hand, are less likely to be made when the repayment cannot be made until years later or when the opportunity for repayment is not certain. To explore this I calculate moving averages of the residuals in each tit-for-tat pair. Formally I construct the variables

$$^{w}P^{1}_{(i;j);u} = \sum_{t=u-w/2}^{u+w/2} \sum_{s=u-w/2}^{u+w/2} I_{i;j;t;s}\varepsilon_{i;t} \Bigg/ \sum_{t=u-w/2}^{u+w/2} \sum_{s=u-w/2}^{u+w/2} I_{i;j;t;s}$$

$$^{w}P^{2}_{(i;j);u} = \sum_{t=u-w/2}^{u+w/2} \sum_{s=u-w/2}^{u+w/2} I_{j;i;t;s}\varepsilon_{j;t} \Bigg/ \sum_{t=u-w/2}^{u+w/2} \sum_{s=u-w/2}^{u+w/2} I_{j;i;t;s}$$

where *w* is the window length over which the moving averages are taken and the other variables are defined as above. I then regress $^{w}P^{1}_{(i;j)}$ on $^{w}P^{2}_{(i;j)}$ for varying values of *w*. The results are presented in columns four to twelve in Table 6. Standard errors are clustered on both tit-for-tat pairs and years as described in

Cameron et al (2008).[4] The results using these moving averages are even stronger. The point estimates are higher, as is the statistical significance and R^2. When considering this together with the weak average effect of being in a tit-for-tat pair, it seems plausible that the observed behaviour is due to varying cordiality of personal relationships rather than explicit agreements on corruption.

5.4 The position on the board

Regardless of how explicit the agreements on adjusting compensation are, it is crucial that the players have some real influence over the compensation decision. The more power a board member in a tit-for-tat pair has on her board the more important any personal relationships or explicit corruption will be to the board's compensation decision. I will let the board members formal position on the board proxy for such power. Specifically I construct dummy variables for being the chairman of the board, a member of the compensation committee and being in any relevant position, to which apart from the two aforementioned positions I also count the board vice chairmanship and membership on the governance committee. For instance, if the board member in a tit-for-tat pair is the chairman of the board in any year included in the window over which moving averages are taken, the *Chairman* and *AnyPos* variables will take the value one. I also construct a dummy that takes the value of one whenever the board has less than seven members. The idea here is that each

[4] I am gratefully for the Stata code implementing this that I retrieved from Douglas Miller's webpage.

individual member should have more influence if the board is smaller. I include these variables alone and interacted with the residuals from the compensation regressions on the right hand side and run regressions very similar to those estimated in Table 6. The variables of interest are the interactions, which are interpreted as additional correlations that kick in whenever the relevant conditions are fulfilled. The results that are presented in Table 7 for a window length of one and in Table 8 for a window length of eleven are mixed. There is no statistically significant effect for wither window length, although the point estimates of the *SmallBoard* interactions are fairly large. I struggle to find a plausible explanation for these results.

5.5 Using lagged residuals

Regressing the residuals on each other is not the only way to test my hypothesis. In order to further exploit the time dimension of the data I re-estimate the compensation regressions, while including lagged residuals from the original regressions as an explanatory variable. That is, I estimate the following regressions:

$$\ln\left(TC_{i;t}\right) = \alpha + \sum_j \beta_j^{Ctrl} Ctrl_{i;t}^j + \beta_{Pair} Pair_{i;t} + \beta_k^{Cent} Cent_{i;t}^k + \beta_k^{BoardCent} BoardCent_{i;t}^k$$

$$+\sum_y \beta_y^{Year} Year_t^y + \sum_z \beta_z^{Industry} Industry_{i;t}^z + \beta_{\hat{\varepsilon}} \sum_j \sum_{s=1}^{S} I_{j;i;t-s;t} \hat{\varepsilon}_{j;t-s} + \varepsilon_{i;t}$$

$\hat{\varepsilon}_{i;t}$ are the residuals from the predictive regression described above and estimated in Table 4. The parameter S determines the maximum lag used when summing up these residuals. Since the individuals forming a pair are not randomly selected I want to cluster my standard errors on pairs. As a few individuals are part of more than one pair this is cumbersome and I therefore arbitrarily limit the number of pair relations to one. The results are qualitatively the same if I estimate the regressions with all pairs included and do not cluster on pairs. The one year lagged residuals should be interpreted as the unexpected compensation that the CEO gave her pair partner last year. If it was high (or low) I expect the partner to repay in kind, and I therefore expect to find a significant positive effect of this variable. I also estimate the regressions using the sum of the one and two year lagged residuals, as well as the sum of the one to five year lagged residuals. All standard errors are clustered on pairs, individuals, years and firms. The results for the variables of interest are given in Table 9. For brevity I do not report the control variables which are all similar to the original estimates given in Table 4. The effects are consistently very significant and positive. When using longer lags the point estimates are lower, as one would expect. This again supports H_3. It may be worth noting that the tit-for-tat pair dummy remains statistically insignificant, again supporting the notion that the compensation coordination is based on relationships rather than explicit agreements.

6 Robustness Tests

Since the story I'm telling depends crucially on the ability of individuals in tit-for-tat pairs to influence each other's wages, my findings should be less evident or absent when that ability decreases. Since the compensation of non-CEO executives is typically recommended to the board by the CEO, the influence of board members on these compensations should be lower and tit-for-tat gaming with non-CEO executives less common. I will use this to address some potential econometric concerns. If there were some unobserved characteristics that made executives with low compensation (residuals) more likely to enter tit-for-tat pairs with other low compensation executives, this could give rise to the residual correlation that I have documented. One possibility could be that my industry classification is too coarse and that executives in lower compensation sub industries, for business reasons or otherwise, tend to serve on boards in the same sub industry. The sub industry fixed effects (that are omitted from my original regressions) would then show up as tit-for-tat correlations. However, unless those sub industry effects (or whatever effects I might have failed to control for) are present for CEOs but not for non-CEO executives (which seems implausible) they would give me significant results when re-estimating my regressions on a sample of top non-CEO executives. I do this in Table

10 (corresponding to Table 6) and Table 11 (corresponding to Table 9). Despite roughly comparable sample sizes, none of my earlier results are replicated in this sample. There is, of course, still no way of knowing for certain whether there is some relevant omitted variable, but I cannot think of any candidate that would not be relevant in the non-CEO executive sample as well.

7 Conclusion

In this paper I have studied tit-for-tat pairs, i.e. situations where CEOs serve on each other's boards in a way that makes it possible for them to affect each other's compensation. I have found that the residuals from predictive regressions of CEO compensation are positively correlated in such pairs, implying that these tit-for-tat pair formations are indeed important for CEO compensations. When studying a sample of non-CEO top executives (whose compensation is typically set by the CEO rather than the board) none of the effects are present. This indicates that the results are not driven by some unobserved characteristic of the firms whose CEOs are part of tit-for-tat pairs.

I cannot say with certainty whether the results are due to more or less explicit agreements between the parties or whether it has more to do with how cordial their personal relationship is, but given that the existence of a tit-for-tat pair relationship does not seem to have much of an average effect on compensation the latter explanation appears more likely. This is also supported by the fact that the tit-for-tat effect is present even over lags of several years, where explicit agreements would be hard or impossible to make. However, since the alternative explanation has different and more direct policy implications, research that could definitely disentangle the two stories is warranted.

Although illustrated well in this setting, there is no reason to believe that the effects of personal relationships are specific to individuals in tit-for-tat pairs. The seemingly large weight given to such relationships by board members making compensation decisions provides new evidence of economically relevant agency problems in large firms and validates the extensive attention that has been paid to such issues.

When building up to these results I also document how CEO and board centralities are highly correlated. When controlling for both in CEO compensation regressions board centralities tend to become insignificant. This warrants caution when interpreting any results that crucially depend on the social network centralities of board members rather than CEOs.

8 Appendix: Control variables

The following variables are used as explanatory variables in one or more of the compensation regressions. Since my aim is not to take a firm stand on what determines executive compensation I am not very concerned with the interpretation or significance of these variables. It is sufficient for my purposes that whatever factor is driving their effects is removed from the residuals. I nevertheless give a short motivation for their inclusion. I also give one or more references to papers related to this one that have included them.

Assets. This is the reported total value of the firm's assets (in millions of dollars). The variable corresponds to the ASSETS data item in ExecuComp. I use the natural log of this variable in all regressions. The decisions of top executives are likely to have larger economic consequences in larger firms. Larger companies should therefore be able to outbid smaller firms in the competition for skilled managers. Several studies have found that CEOs of larger companies indeed tend to earn higher compensation (Hwang & Kim (2008), Barnea & Guedj (2007)).

Tobin's q. This variable is constructed from the market value of equity (data item MKTVAL in ExecuComp), the total book value of debt (data item DLTT + data item DLC in CompuStat) and shareholders' equity (data item SEQ in ExecuComp). Tobin's q is then defined as q = (MKTVAL + DLTT + DLC)/(SEQ + DLTT + DLC). This is often thought to proxy for a firm's growth opportunities. If management of growing firms requires greater skill, one should expect to see an association with higher levels of compensation. Such firms may also be harder to monitor, resulting in a higher reliance on risk premium carrying option programs to align incentives. The variable is used as a control by Barnea & Guedj (2007).

Market-to-book. This measure is very similar to Tobin's q and motivated in the same way. It is defined as the ratio of the market value of equity (SEQ) to book value of common equity (data item COMMEQ in CompuStat). Market-to-book is used by Larcker et al (2005) and Hwang & Kim (2008).

Volatility. This is the idiosyncratic volatility. It is calculated as the standard deviation of the residuals from a simple CAPM regression using the S&P 500 index to proxy for the market portfolio. The standard deviation is taken over the last three years, requiring at least one year of data. This variable is motivated in much like Tobin's q and market-to-book. Larcker et al (2005) and Hwang & Kim (2008) both control for volatility measures.

Return on Assets. This is the net income before extraordinary items and discontinued operations divided by total assets multiplied by 100 (data item ROA in ExecuComp). If a CEO is doing well she is likely to reap some financial rewards, either through an outright raise or through her incentive programs. The variable is used by e.g. Larcker et al (2005) and Hwang & Kim (2008).

5 Yr Return to Shareholders. This is the 5 year total return to shareholders, including the monthly reinvestment of dividends (data item TRS5YR in ExecuComp). This variable is motivated in the same way as Return on Assets. Shareholder returns are used by e.g. Larcker et al (2005) and Hwang & Kim (2008).

GIM Governance index. This is the governance index constructed in Gompers et al (2003). The data is taken from the RiskMetrics (former IRRC) Governance database. Data is not available with yearly frequency, so I interpolate between the two closest data points when necessary. If CEOs find it easier to influence their own compensation in firms with weak governance this variable may matter. It is used by Barnea & Guedj (2007).

Board independence. This is a dummy variable that takes the value of one whenever the majority of a firm's board members are classified as independent. The dummy approach is the same as in Barnea & Guedj (2007), but all results are robust to a specification using the fraction of the board that is independent instead.

Interlock. This is the INTERLOCK data item from ExecuComp. It is a dummy that takes the value of one if the executive is involved in a relationship requiring disclosure in the "Compensation Committee Interlocks and Insider Participation" section of the proxy. This is typically due to one of the following situations:

1. The officer serves on the board committee that makes his compensation decisions.
2. The officer serves on the board (and possibly compensation committee) of another company that has an executive officer serving on the compensation committee of the indicated officer's company.
3. The officer serves on the compensation committee of another company that has an executive officer serving on the board (and possibly compensation committee) of the indicated officer's company.

This variable is most often motivated by a reference to Hallock (1997). However, there is no way of knowing to which of the situations above the dummy reacts. It is very related to the Tit-for-Tat dummy variable.

Board interlock. This is the average across all board members of the INTERLOCK variable from the RiskMetrics Directors database. The INTERLOCK variable takes the value of one if the director is classified as interlocking.

Board size. This is the number of members of the board of directors. Yermack (1996) finds evidence suggesting that larger boards are poorer monitors. As this would make it easier for the CEO to raise her own compensation and make economic incentives more attractive relative to direct monitoring the variable is often included as a control in compensation regressions. Papers doing this include Core et al (1999), Larcker et al (2005) and Hwang & Kim (2008).

Male Dummy. This is a dummy variable that takes the value of one when the executive is a man. The variable is included to control for the gender gap in wages that is documented by e.g. O'Neill (2003) and Bertrand & Hallock (2001). The variable is used as a control in an executive compensation context by e.g. Bertrand & Hallock (2001) and Barnea & Guedj (2007).

Executive's age. This is the age of the executive. It may be correlated to compensation via experience and human capital. It is used as a control by Bertrand & Hallock (2001), Larcker et al (2005) and Barnea & Guedj (2007).

CFO dummy. This is a dummy that takes the values of one if the executive is the CFO. It is most relevant in the robustness tests on non-CEO executives.

Executive share ownership. This is the fraction of the company owned by the executive. A larger stake in the company may reduce the incentive to demand excessive compensation. On the other hand, it may give the executive more power to do so. Arguably, the most important effect of the variable is to align the interests of the executive and the other owners, thus reducing the need for compensation incentives. The variable is used by e.g. Lambert et al (1993) and Hwang & Kim (2008).

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Figure 1. Scatter plot of residuals

This figure shows a simple scatter plot of the residuals from the regression using all controls and the Eigenvector measure of centrality. Each dot represents a pair of residuals from the same year, i.e. the window length is one. The figure is representative for other regression specifications and window lengths.

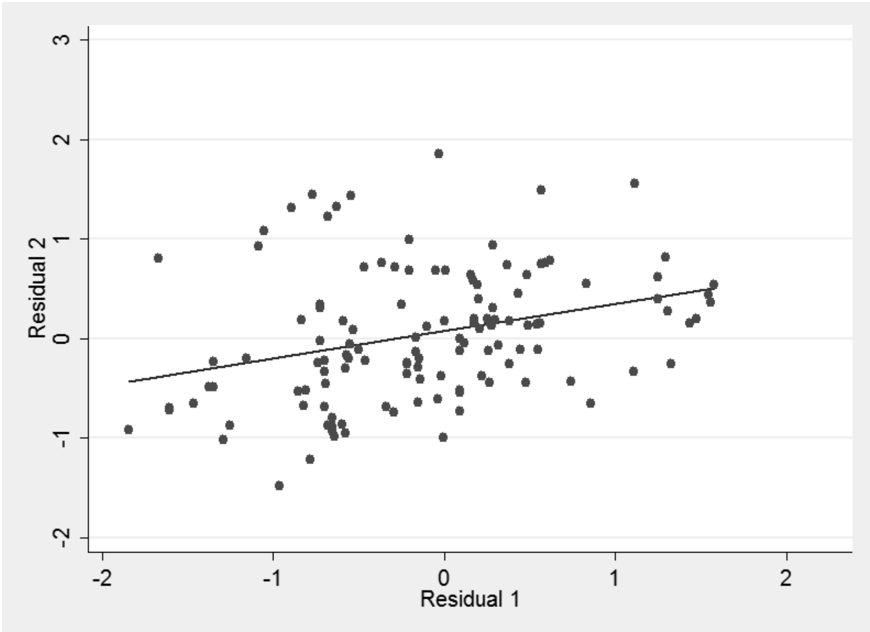

Table 1. Summary statistics

This table gives summary statistics for the control variables. Columns one to three includes all observations in the sample whereas columns four to six includes only observations where the executive is in an active tit-for-tat pair.

	Entire sample			Tit-for-tat pairs only		
Variable	Observations	Mean	Standard deviation	Observations	Mean	Standard deviation
Salary	115851	374.71	262.69	977	758.22	448.93
Bonus	115851	357.32	1055.66	977	1070.27	1740.27
Other anual compensation	108637	29.94	658.94	943	104.56	945.20
Restricted stock grants	108637	229.96	2502.27	943	725.34	2397.27
LTIP payments	108637	86.93	633.36	943	405.24	1421.30
All other compensation	115851	130.20	1114.92	977	516.89	2497.32
Option grants (B&S value)	97827	1069.98	4300.40	915	3158.88	9366.87
Total compensation	105036	2335.47	5845.07	949	6710.09	11083.97
Assets	115713	12138	60488	977	41676	136941
Tobins q	112405	2.49	17.96	969	2.38	4.09
Return on Assets	115669	2.10	51.85	977	3.97	8.73
Total Debt-to-Assets	114826	0.24	0.25	974	0.25	0.17
Volatility	99927	0.0263	0.0130	948	0.0210	0.0089
Male dummy	115851	0.94	0.24	977	0.97	0.18
GIM governance index	84651	9.31	2.62	882	9.77	2.32
Independence dummy	87370	0.84	0.37	976	0.80	0.40
Board interlock	87370	0.009	0.036	976	0.049	0.068
Boardsize	87370	9.65	2.90	976	11.98	3.46
5 Year Return to Shareholders	112823	96.61	4671.54	977	15.64	41.74
Market-to-book	113255	3.88	60.97	972	3.36	6.35
Executive's Age	46631	53.04	8.34	768	58.50	7.85
CFO dummy	115851	0.01	0.11	977	0.00	0.03
Executive share ownership	94017	0.0000	0.0001	902	0.0000	0.0001
Executive is listed as interlocked	115851	0.02	0.13	977	0.18	0.38
Pair dummy	115851	0.01	0.09	977	1	0

Table 2. Centrality measure summary statistics

This table gives summary statistics for the centrality measures used in the analysis.

Variable	Obs	Mean	Std. Dev.	Max
Betweenness	13257	.0001555	.0002498	.0029081
Board Betweenness	13943	.0001983	.0002222	.0023197
Closeness	13257	.0007028	.0005094	.0048483
Board Closeness	13943	.0008053	.0004428	.0035121
Eigenvector	13257	.00421	.0106311	.220851
Board Eigenvector	14623	.0041574	.0078595	.157268
Bonacich	11801	1.673397	.6391964	7.41978
Board Bonacich	14618	1.596235	.4714465	4.89108

Table 3. Centrality measure correlation matrix

This table gives the correlations between the used centrality measures. The bold numbers are the correlations b tween the CEO centrality and board centrality for each measure.

	Betweenness	Board Betweenness	Closeness	Board Closeness	Eigenvector	Board Eigenvector	Bonacich	Board Bonacich	Pair dummy
Betweenness	1								
Board Betweenness	**0.6573**	1							
Closeness	0.8264	0.6174	1						
Board Closeness	0.5761	0.8417	**0.7879**	1					
Eigenvector	0.4637	0.3982	0.5223	0.4407	1				
Board Eigenvector	0.3101	0.4465	0.3776	0.4735	**0.7871**	1			
Bonacich	0.7703	0.6303	0.9466	0.7985	0.6084	0.4577	1		
Board Bonacich	0.4708	0.6707	0.6613	0.8200	0.4821	0.6043	**0.7730**	1	
Pair dummy	0.2155	0.1877	0.2978	0.2539	0.1974	0.1805	0.3054	0.2673	1

Table 4. Basic regressions using Eigenvector centrality

This table shows regression results for Eigenvector centrality and the three sets of control variables presented above. The dependent variable is the natural log of total CEO compensation. All specifications include year and industry fixed effects. Standard errors are clustered on individuals. Robust p-values are given in brackets.

Eigenvector	3.202		3.454	2.775		3.735	3.592		4.891
	[0.00114]***		[0.02123]**	[0.00314]***		[0.00771]***	[0.00036]***		[0.00153]***
Board Eigenvector		3.306	-0.518		2.763	-1.886		2.867	-2.424
		[0.01552]**	[0.81401]		[0.03586]**	[0.36177]		[0.02716]**	[0.25728]
ln(Assets)	0.480	0.493	0.490	0.482	0.492	0.484	0.474	0.479	0.476
	[0.00000]***	[0.00000]***	[0.00000]***	[0.00000]***	[0.00000]***	[0.00000]***	[0.00000]***	[0.00000]***	[0.00000]***
Tobins q	0.002	0.016	0.017	0.068	0.028	0.068	0.060	0.059	0.060
	[0.40013]	[0.02014]**	[0.08856]*	[0.00000]***	[0.04783]**	[0.00000]***	[0.00001]***	[0.00000]***	[0.00001]***
Return on Assets	0.002	0.005	0.005	0.003	0.006	0.003	0.003	0.003	0.003
	[0.09014]*	[0.00025]***	[0.00252]***	[0.07814]*	[0.00557]***	[0.07839]*	[0.15069]	[0.07629]*	[0.15109]
Total Debt-to-Assets	-0.282	-0.248	-0.285	-0.216	-0.219	-0.216	-0.217	-0.129	-0.217
	[0.00446]***	[0.00493]***	[0.00657]***	[0.04855]**	[0.02272]**	[0.04869]**	[0.05708]*	[0.18315]	[0.05704]*
Volatility	8.309	11.747	10.076	10.908	12.460	10.913	10.306	10.817	10.306
	[0.00000]***	[0.00000]***	[0.00000]***	[0.00000]***	[0.00000]***	[0.00000]***	[0.00000]***	[0.00000]***	[0.00000]***
GIM governance index				0.025	0.020	0.025	0.026	0.022	0.025
				[0.00000]***	[0.00003]***	[0.00000]***	[0.00001]***	[0.00001]***	[0.00001]***
Independence dummy				0.108	0.146	0.110	0.071	0.108	0.072
				[0.00739]***	[0.00004]***	[0.00649]***	[0.08346]*	[0.00234]***	[0.07584]*
Board interlock				0.311	0.014	0.318	0.697	0.376	0.707
				[0.39417]	[0.96577]	[0.38392]	[0.06595]*	[0.29146]	[0.06266]*
ln(Boardsize)				-0.049	-0.060	-0.046	-0.068	-0.074	-0.065
				[0.50210]	[0.32266]	[0.52695]	[0.34948]	[0.22279]	[0.37415]
Male dummy				-0.081	-0.058	-0.083	-0.169	-0.116	-0.171
				[0.56504]	[0.63087]	[0.55808]	[0.21743]	[0.31673]	[0.21212]
5 Yr Return to Shareholders							0.001	0.001	0.001
							[0.01011]**	[0.00085]***	[0.01034]**
Market-to-book							0.001	-0.000	0.001
							[0.10724]	[0.64745]	[0.10826]
Executive's Age							-0.002	-0.003	-0.002
							[0.42936]	[0.15974]	[0.40314]
CFO dummy							0.262	-0.786	0.259
							[0.14517]	[0.11136]	[0.14947]
Executive share ownership							-183.565	-241.220	-183.526
							[0.30781]	[0.29128]	[0.30732]
Executive is listed as interlocked							-0.299	-0.275	-0.300
							[0.00029]***	[0.00002]***	[0.00029]***
Pair dummy							0.043	0.085	0.043
							[0.48744]	[0.16044]	[0.48650]
Observations	11896	13630	10418	9545	12417	9545	8802	11411	8802
R-squared	0.46009	0.47803	0.47385	0.49496	0.49033	0.49502	0.49869	0.50485	0.49880

Table 5. Centrality measure regressions

This table shows regression results for the three sets of control variables presented above. Each of the four panels represent a different regressions with a different centrality measure. The dependent variable is the natural log of total CEO compensation. All specifications include year and industry fixed effects. Standard errors are clustered on individuals. Robust p-values are given in brackets.

Betweenness	193.125		97.470	153.574		118.976	171.602		139.558
	[0.00004]***		[0.08532]*	[0.00288]***		[0.03112]**	[0.00088]***		[0.01120]**
Board Betweenness		211.698	167.990		147.733	76.301		141.816	70.748
		[0.00003]***	[0.01921]**		[0.00533]***	[0.27924]		[0.00843]***	[0.32526]
Specification	1	1	1	2	2	2	3	3	3
Observations	11896	13016	10381	9545	11941	9527	8802	10975	8789
R-squared	0.46091	0.47901	0.47543	0.49547	0.49039	0.49570	0.49907	0.50595	0.49935
Closeness	73.656		61.195	66.689		79.038	91.476		106.180
	[0.00856]***		[0.10174]	[0.03991]**		[0.03019]**	[0.00646]***		[0.00453]***
Board Closeness		76.119	22.119		41.701	-26.991		45.099	-30.460
		[0.01540]**	[0.64203]		[0.22493]	[0.57578]		[0.20359]	[0.54238]
Specification	1	1	1	2	2	2	3	3	3
Observations	11896	13016	10381	9545	11941	9527	8802	10975	8789
R-squared	0.46008	0.47812	0.47427	0.49490	0.48984	0.49507	0.49871	0.50548	0.49895
Eigenvector	3.202		3.454	2.775		3.735	3.592		4.891
	[0.00114]***		[0.02123]**	[0.00314]***		[0.00771]***	[0.00036]***		[0.00153]***
Board Eigenvector		3.306	-0.518		2.763	-1.886		2.867	-2.424
		[0.01552]**	[0.81401]		[0.03586]**	[0.36177]		[0.02716]**	[0.25728]
Specification	1	1	1	2	2	2	3	3	3
Observations	11896	13630	10418	9545	12417	9545	8802	11411	8802
R-squared	0.46009	0.47803	0.47385	0.49496	0.49033	0.49502	0.49869	0.50485	0.49880
Bonacich	0.056		0.034	0.048		0.049	0.071		0.080
	[0.00923]***		[0.25609]	[0.04277]**		[0.09449]*	[0.00449]***		[0.00686]***
Board Bonacich		0.090	0.048		0.073	-0.002		0.064	-0.024
		[0.00381]***	[0.32031]		[0.02765]**	[0.95813]		[0.06325]*	[0.60331]
Specification	1	1	1	2	2	2	3	3	3
Observations	10750	13628	9900	9072	12416	9072	8360	11411	8360
R-squared	0.46616	0.47831	0.47808	0.49776	0.49041	0.49776	0.50240	0.50486	0.50243

Table 6. Residual regressions

This table shows the results of regressing the residual of the first player in a tit-for-tat pair on that of the other player. *Specification* refers to the set of controls used to generate the residuals as shown in Table 4. *Window length* refers to the window over which residuals are averaged, e.g. one for contemporaneous observations only and eleven for the entire sample. Standard errors are clustered on pair and year where applicable. p-values are given in brackets.

Specification	1	2	3	1	2	3	1	2	3	1	2	3
Window length	1	1	1	3	3	3	7	7	7	11	11	11
Average residual	0.361***	0.319**	0.272**	0.388***	0.341***	0.298**	0.472***	0.424***	0.398***	0.472***	0.424***	0.398***
	[0.0090]	[0.0116]	[0.0403]	[0.0006]	[0.0019]	[0.0125]	[0.0000]	[0.0000]	[0.0001]	[0.0000]	[0.0001]	[0.0012]
Constant	0.0999	0.0732	0.0735	0.0752	0.0648	0.0884	0.0579	0.0553	0.0735	0.0579	0.0553	0.0735
	[0.2043]	[0.3886]	[0.4633]	[0.2849]	[0.3776]	[0.2918]	[0.3105]	[0.3574]	[0.2869]	[0.3713]	[0.4176]	[0.3494]
Observations	162	142	130	265	243	228	270	240	225	54	48	45
R-squared	0.1626	0.1507	0.0957	0.1939	0.1691	0.1083	0.3043	0.2604	0.2032	0.3043	0.2604	0.2032

Table 7. Controlling for Chairmanship and Compensation Committee membership, contemporanious observations

This table shows effects and cross effects of the board member in a tit-for-tat pair being the chairman of the board (*Chairman*), on the compensation committee (*CompCom*), serving on a board with less than seven members (*SmallBoard*) or being any position of influence (*Any*). Apart from the board chairmanship and compensation committee membership, the board vice chairmanship and membership on the governance committee counts as a position of influence. The regression includes contemporaneous observations only, i.e. corresponding to columns one to three of Table 6. Standard errors are clustered on pairs and year. p-values are given in brackets.

Specification	1	1	1	1	1	2	2	2	2	2	3	3	3	3	3
Residual	0.361***	0.357***	0.369**	0.335**	0.29	0.319**	0.371***	0.370***	0.277**	0.285	0.272**	0.330***	0.308**	0.214	0.209
	[0.0090]	[0.0027]	[0.0180]	[0.0280]	[0.1888]	[0.0116]	[0.0001]	[0.0075]	[0.0493]	[0.1934]	[0.0403]	[0.0028]	[0.0456]	[0.1415]	[0.3922]
Chairman		0.15					-0.0065					-0.0515			
		[0.2259]					[0.9506]					[0.6765]			
Chairman*Residual		0.0115					-0.201					-0.266			
		[0.9648]					[0.4745]					[0.4082]			
CompMem			0.0595					0.0426					0.0646		
			[0.4411]					[0.6831]					[0.5807]		
CompMem*Residual			0.000589					-0.0925					-0.0598		
			[0.9963]					[0.3658]					[0.5952]		
SmallBoard				-0.00319					0.061					0.0599	
				[0.9652]					[0.4891]					[0.5355]	
SmallBoard*Residual				0.108					0.175					0.234	
				[0.6183]					[0.4779]					[0.2492]	
AnyPos					0.000786					-0.114					-0.17
					[0.9954]					[0.4895]					[0.3872]
AnyPos*Residual					0.12					0.0257					0.0562
					[0.6269]					[0.9197]					[0.8404]
Constant	0.0999	0.0507	0.0781	0.0974	0.108	0.0732	0.0697	0.0524	0.0508	0.151	0.0735	0.0808	0.0472	0.0455	0.195
	[0.2043]	[0.5168]	[0.3797]	[0.2291]	[0.4491]	[0.3886]	[0.4436]	[0.6152]	[0.5229]	[0.3969]	[0.4633]	[0.4454]	[0.7056]	[0.6301]	[0.3597]
Observations	162	162	162	162	162	142	142	142	142	142	130	130	130	130	130
R-squared	0.1626	0.1751	0.1647	0.1654	0.1668	0.1507	0.1622	0.1558	0.16	0.1581	0.0957	0.1113	0.0998	0.1094	0.1097

Table 8. Controlling for Chairmanship and Compensation Committee membership, Averages over the entire sample period

This table shows effects and cross effects of the board member in a tit-for-tat pair being the chairman of the board (*Chairman*), on the compensation committee (*CompCom*), serving on a board with less than seven members (*SmallBoard*) or being any position of influence (*Any*). Apart from the board chairmanship and compensation committee membership, the board vice chairmanship and membership on the governance committee counts as a position of influence. The regression uses averages over the entire time period, i.e. corresponding to columns ten to twelve of Table 6. Standard errors are clustered on pairs. p-values are given in brackets.

Specification	1	1	1	1	1	2	2	2	2	2	3	3	3	3	3
Residual	0.472***	0.409***	0.513***	0.348***	0.432**	0.424***	0.412**	0.469***	0.365***	0.425**	0.398***	0.406**	0.420***	0.361**	0.355*
	[0.0000]	[0.0027]	[0.0002]	[0.0073]	[0.0105]	[0.0002]	[0.0100]	[0.0011]	[0.0044]	[0.0219]	[0.0019]	[0.0248]	[0.0081]	[0.0103]	[0.0977]
Chairman		0.163					0.0656					0.0412			
		[0.2162]					[0.6443]					[0.8012]			
Chairman*Residual		0.105					0.0125					-0.0207			
		[0.6122]					[0.9542]					[0.9337]			
CompMem			-0.14					-0.12					-0.0755		
			[0.3184]					[0.4122]					[0.6753]		
CompMem*Residual			-0.162					-0.186					-0.121		
			[0.4485]					[0.4255]					[0.6791]		
SmallBoard				0.0559					0.0706					0.0293	
				[0.6619]					[0.5956]					[0.8456]	
SmallBoard*Residual				0.215					0.102					0.0654	
				[0.2144]					[0.6172]					[0.7683]	
AnyPos					0.00311					-0.136					-0.197
					[0.9837]					[0.4387]					[0.3145]
AnyPos*Residual					0.0646					-0.00974					0.0439
					[0.7567]					[0.9654]					[0.8659]
Constant	0.0579	-0.0365	0.0988	0.0235	0.0571	0.0553	0.015	0.0879	0.0166	0.165	0.0735	0.048	0.0891	0.0525	0.232
	[0.3696]	[0.7098]	[0.2250]	[0.8031]	[0.6668]	[0.4140]	[0.8925]	[0.3197]	[0.8522]	[0.2974]	[0.3426]	[0.7116]	[0.3745]	[0.6290]	[0.1867]
Observations	54	54	54	54	54	48	48	48	48	48	45	45	45	45	45
R-squared	0.3043	0.3272	0.322	0.3207	0.3056	0.2604	0.264	0.2784	0.2678	0.2705	0.2032	0.2048	0.2083	0.205	0.2233

Table 9. Lagged residuals regressions

This table presents the results of including lagged pair residuals in the previously estimated regressions. Each regression is first estimated yearly (with no residuals among the independent variables). The regressions are then re-estimated using various lags of the residuals tit-for-tat partners as independent variables. $Residual_{t-1}$ is the one year lagged residual of each individual's tit-for-tat partner (if any). For individuals that are not part of a tit-for-tat pair at time *t-1*, the variable takes the value zero. $Residual_{t-1:t-2}$ is the sum of the one and two years lagged residuals. $Residual_{t-1:t-5}$ is the sum of the one to five years lagged residuals. The specifications one to three correspond to the specifications in Table 4. Standard errors are clustered on tit-for-tat pairs, years, companies and individuals. Robust p-values are given in brackets.

Specification	1	1	1	2	2	2	3	3	3
$Residual_{t-1}$	0.500			0.562			0.463		
	[0.00021]***			[0.00000]***			[0.00079]***		
$Residual_{t-1:t-2}$		0.355			0.402			0.373	
		[0.00000]***			[0.00000]***			[0.00003]***	
$Residual_{t-1:t-5}$			0.226			0.285			0.257
			[0.00000]***			[0.00000]***			[0.00001]***

Table 10. Residual regressions for non-CEO executives

This table shows the results of regressing the residual of the first player in a tit-for-tat pair on that of the other player. *Specification* refers to the set of controls used to generate the residuals as shown in Table 4. *Window length* refers to the window over which residuals are averaged, e.g. one for contemporaneous observations only and eleven for the entire sample. Standard errors are clustered on pair and year where applicable. p-values are given in brackets. This table corresponds to Table 6 but the sample is top non-CEO executives rather than CEOs.

Specification	1	1	1	1	2	2	2	2	3	3	3	3
Window length	1	3	7	11	1	3	7	11	1	3	7	11
Average residual	-0.137	-0.0740	-0.100	-0.100	-0.219	-0.231	-0.169	-0.169	0.239	0.430	0.366	0.366
	[0.4212]	[0.6467]	[0.4217]	[0.4776]	[0.3647]	[0.2846]	[0.3379]	[0.3997]	[0.6279]	[0.2110]	[0.1681]	[0.2416]
Constant	0.406**	0.336**	0.385***	0.385***	0.405**	0.285*	0.337**	0.337**	0.592**	0.473**	0.398**	0.398**
	[0.0183]	[0.0292]	[0.0008]	[0.0043]	[0.0344]	[0.0971]	[0.0167]	[0.0396]	[0.0193]	[0.0184]	[0.0101]	[0.0364]
Observations	105	247	295	59	77	184	215	43	44	81	95	19
R-squared	0.0096	0.0029	0.0065	0.0065	0.0223	0.0227	0.0144	0.0144	0.0149	0.0795	0.0620	0.0620

Table 11. Lagged residuals regressions for non-CEO executives

This table presents the results of including lagged pair residuals in the previously estimated regressions. Each regression is first estimated yearly (with no residuals among the independent variables). The regressions are then re-estimated using various lags of the residuals tit-for-tat partners as independent variables. *Residual*$_{t-1}$ is the one year lagged residual of each individual's tit-for-tat partner (if any). For individuals that are not part of a tit-for-tat pair at time *t-1*, the variable takes the value zero. *Residual*$_{t-1:t-2}$ is the sum of the one and two years lagged residuals. *Residual* $_{t-1:t-5}$ is the sum of the one to five years lagged residuals. The specifications one to three correspond to the specifications in Table 4. Standard errors are clustered on tit-for-tat pairs, years, companies and individuals. Robust p-values are given in brackets. This table corresponds to Table 9 but the sample is top non-CEO executives rather than CEOs.

Specification	1	1	1	2	2	2	3	3	3
Residual$_{t-1}$	-0.023			-0.010			-0.031		
	[0.83260]			[0.94030]			[0.75340]		
Residual$_{t-1:t-2}$		-0.044			-0.016			-0.017	
		[0.33955]			[0.72222]			[0.69249]	
Residual$_{t-1:t-5}$			0.007			0.039			0.051
			[0.89646]			[0.53898]			[0.57679]

Pretty Pennies: Realtor Attractiveness and Home Prices *

with Robert Tumarkin

Abstract

Although buying one's home is by far the largest financial transaction in most people's lives, micro-level behavioral research on home pricing has been quite limited. We test for a simple behavioral effect; does the attractiveness of a realtor influence the final purchase price of a home? To do so, we take advantage of two unique structural characteristics of the Australian real estate market in which (i) buyers rarely use their own realtor and (ii) homes are commonly sold both by auction and by private treaty. Our results show that buyers pay a 2.3% average premium (approximately USD 16,000) for their house when the realtor is one standard deviation of attractiveness above average. This premium is concentrated in homes sold by auction, which is consistent with a behavioral interpretation where private treaty home buyers partially filter the impact of realtor appearance over time.

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1 Introduction

It is perhaps not surprising that empirical research has found evidence of behavior biases in the way individual retail investors trade financial securities. For most people, the combined value of security holdings through investment and retirement accounts is not their most significant asset. Instead, real estate is their largest single investment. While the cost of an inefficient transaction in financial securities is small due to low transaction fees, high liquidity, and relatively small transaction sizes, the cost of mispricing a house is great. Transaction fees are often between 5 and 6 percent, liquidity is very low, and buying a house is a major undertaking. Therefore, individuals have a greater motivation to rationally price a home than to ensure that each security transaction is perfectly valued.

Still, the literature has documented substantial behavioral anomalies arising in circumstances that are likewise unfavorable. For instance, Kamstra et al (2003) show that the length of day affects risk aversion and aggregate stock market returns through a depressive disorder brought on by lessened sunshine exposure. Even soccer results appear to affect the market via their effect on investor moods, as shown by Edmans et al (2007). In spite of this, behavioral effects in individual's real estate transactions have been ignored for the most part in the existing financial literature. One exception is Northcraft:1987p4350, which shows that home values anchor to listed prices in experimental settings. Although anecdotes of irrational behavior in real estate markets abound, they tend to be hard to confirm with econometric techniques. Furthermore, microeconomic data on individual real estate transactions can be hard to analyze given the structure of markets such as that found in the US where buyers and sellers each employ their own realtor in a complicated negotiation process.

In this paper, we take advantage of the unique structural characteristics of the Australian real estate market to test for a simple behavioral effect; does the attractiveness of a realtor affect the final purchase price of a home? And, if so, through what mechanism does realtor attractiveness translate into price? This test is feasible because Australian homebuyers almost never use a realtor to find homes or to negotiate price. Instead, the agent hired by the seller serves as a single intermediary between her and the buyer. For contrast, a US homebuyer will almost never meet the realtor of the seller, instead employing her own realtors to identify properties and as agents in negotiations.

We collect a unique, comprehensive data set of real estate transactions throughout Australia. For each home sale, we identify the principal realtor employed by the seller. Then, for each realtor, we visit her agency web site and download the highest resolution portrait that is available. Each realtor is rated for attractiveness and on a number of other perceived characteristics using survey data collected through the crowdsourcing service Amazon Mechanical Turk (AMT).

We exploit this dataset to investigate the behavioral phenomenon of the halo effect, which is a cognitive bias, through which positive perceptions of personal traits can affect perceptions of other, unrelated characteristics. The halo effect is well established in the psychological literature, especially for physical attractiveness. For instance, researchers have found that attractive people are perceived as more intelligent and socially competent [**?**] and are less likely to be punished [**?**]. Physical attractiveness has also been found to affect academic evaluations [**?**], professional evaluations [**?**], and employment opportunities [**?**, **?**]. The list could easily be made longer; the halo effect has strong and diverse academic support as a powerful behavioral bias.

We propose that the same effect plays an important role in the consumer real estate market as the attractiveness of realtors influences the valuation of pieces of real estate in an economically significant way. Our results show that buyers that encounter realtors one standard deviation of attractiveness above average tend to pay a 2.3% premium for their house. At the mean house price, this corresponds to about AUD 16,700 or approximately USD 16,000 during the sample period. Furthermore, this premium is unlikely to be created by realtor characteristics that may rationally influence home prices, such as perceived professionalism, trustworthiness, or reputation.

The halo would initially be established during the home inspection, when the realtor and potential buyer view the house together. The psychology literature shows that as a subject repeatedly observes an item, the halo effect is reduced [**?**]. Thus, as the buyer and realtor interact more, the halo effect should decrease. Our results confirm this. In Australia, homes can be sold by English auction or for a negotiated price in a private treaty. When a house is sold by auction, the home buyer has little further interaction with the selling realtor, resulting in a potentially large halo. However, when a house is sold by private treaty, the buyer has multiple interactions with the selling realtor, which should reduce the halo effect. Consistent with this, our results show a larger halo effect in auctions than in private treaties.

The paper contributes to both the finance and psychology literature. One weakness of the psychological literature referred to earlier is that the impact on the person subject to the behavioral bias is quite limited. For example, while a student's opinion of an instructor may be subject to a halo effect, there is no economic cost to the student of having an inflated opinion. We provide evidence that the halo effect plays an important role in the real estate market; the attractiveness of realtors influences home values in a significant way. We, therefore, contribute to the psychology literature by showing how halo effects create substantial economic losses.

In the finance literature, Ravina:2008p3805 makes an important contribution by showing how physical attractiveness can be relevant in a finance setting. She relates loan rate data from an on-line lending market to the physical attractiveness of the borrowers and finds a significant price effect. Although not contributing her results to a halo effect, Ravina argues, the findings are consistent with personal characteristics affecting loan supply through lenders' preferences.

We make several contributions beyond that paper. First, real estate is perhaps the most important asset class for most people. The average size of our transaction is significantly greater than those found by Ravina. Her Prosper.com data set has an average loan request of 9,000 USD. Our average home size costs over 500,000 AUD (approximately 500,000 USD as of this writing). The value of the price distortion created by a one standard deviation increase in realtor attractiveness is more than 50% larger than the average loan request in Ravina.

Furthermore, we provide evidence that economic influences from physical attractiveness, as included in Ravina, are consistent with a halo effect. In our study, the buyer is trying to evaluate a house rather than the realtor. Although it's arguably possible to draw some conclusions about a person's status and personality from their attractiveness as in Ravina, it is hard to argue that the appearance of the realtor would allow for rational inference on the specific piece of real estate that a realtor is marketing.

Of course, alternate explanations exist for the observed association between the physical attractiveness of a realtor and the average price at which she sells real estate. In general, these arguments can be grouped into four categories. First, physical attractiveness may covary with other factors that directly impact the realtor's labor output. Second, the buyer may make a rational inference on the home quality based on realtor appearance. Third, a halo effect may exist but it is based on other factors than attractiveness. Fourth, as our results are concentrated in auctions, we may be picking up an effect of the auction process. We consider each of these in turn.

Physical attractiveness could covary with other factors, such as depression or stress, that may affect the effort or skill the realtor makes to achieve a good price. Better-looking realtors may be more confident and as such better salesmen. However, if a rational inference on home quality or realtor confidence, skill, or effort create the observed price effect, we would expect that as the realtor and buyer interacted more, the final transaction price would increase. Our results argue strongly against these alternate rational stories as the price effects are concentrated in homes sold by auction.

The home buyer may make a rational inference on home quality based on realtor appearance. As the inferred home quality is unobservable in the data, our results would then represent a spurious relationship. However, if this were the case, we would expect our results to be stronger for private treaties. Additionally, attractive realtors may also be associated with higher quality or more reputable firms. As such, we control for firm fixed effects in all our empirical tests.

A possible caveat is that the buyers may assess other appearance related characteristics of the realtor. For example, the buyer may rationally use an assessment of realtor trustworthiness when evaluating the information she presents. If this is the case, and if assessments of trustworthiness tend to correlate with assessments of physical attractiveness, the presumed relationship between realtor attractiveness and house prices may have a rational basis. We consider three possible appearance-related covariates with attractiveness; perceived realtor age, trustworthiness, and professionalism are included in our empirical specifications.

Other mechanisms could produce an association between physical attractiveness of realtors and house prices. For example, a different explanation would be that of taste based discrimination, as described by Becker:1971p4281 and Krueger:1963p4007. Originally applied to race based discrimination, it assumes that the discriminating party gets direct disutility from dealing with the discriminated party, in some sense making it rational. Though we find no evidence for racial preferences, it is quite conceivable that people get direct utility from dealing with attractive people (or conversely disutility from dealing with unattractive people). Lemay:2010p4025 suggest that the halo effect, as it is documented in many studies, may be driven by a desire to form personal bonds with attractive people and a projection of that desire to the attractive people themselves. One could also imagine a halo effect where the buyers would be the evaluated object, i.e. buyers want to associate with attractive realtors since their attractiveness spills over to the buyers. Whatever the reason for the preference, buyers may rationally pay a premium on their house in order to satisfy it. In short, attractive realtors could function in the same way as extravagant offices, i.e. as a luxury used to attract rich clients. As with extravagant office buildings, though, investments in attractive realtors should be made on the realtor firm level. It is harder to come up with a story for why attractiveness of the realtors should covary with house prices within a given firm. Furthermore, if taste based discrimination was important, we would expect other charactheristics that are generally valued, such as professionalism, to have similar effects.

Finally, we note that rational explanations in general would struggle with the fact that there is a small industry of buyer agents in Australia. For a small commission, typically around 1%, the home buyer can employ an agent to help them price the house and to act as their representative during the auction. This fee corresponds to approximately a 0.35 standard deviation move in realtor attractiveness. Any rational explanation would therefore either have to affect buyer agents as well, or beg the question why buyers do not use agents to remove the effect. This makes explanations based on taste based discrimination implausable.

The remainder of this paper is organized as follows, Section A presents a brief summary of the unique characteristics of the Australian real estate market. Our data is described in Section B. Our main empirical findings are explored in Section C. We test for robustness to alternate empirical specifications in Section D. Section E concludes.

2 The Australian Real Estate Market

Australia's real estate market has two unique structural characteristics that enable a behavioral study of retail real estate transactions: (i) buyer self-representation during the home search and pricing process and (ii) the common use of both auctions and negotiated prices (private treaties) for transactions. The first allows us to examine the implications of buyer perception without that perception being influenced by a real estate professional working in the interest of the buyer. The second lets us evaluate different explanations for the observed positive association between realtor attractiveness and final home price. For this, we use the established fact that multiple observations diminish the influence of the halo effect Cooper:1981p4015e.g.. Thus, multiple interactions between the selling realtor and the buyer during private treaties let us distinguish between a halo effect due to realtor attractiveness and a spurious endogenous relationship between attractiveness and skill.

In a typical Australian real estate transaction, only one realtor is involved. This agent is hired by the seller and works for a commission, which is typically a fraction of the selling price. If the buyer wants professional advice, they must hire a consulting realtor and pay them directly. By contrast, in the US, the home seller pays a commission out of the final closing price. When the buyer uses a realtor, the commission is split between the buyer's realtor and the seller's. If the buyer does not use a realtor in the US, the commission is taken entirely by the selling realtor. Thus, professional advice from a realtor is a free option for the buyer.

Buyers find prospective houses either by contacting realtor firms or by going through multi-realtor listings on web pages or in newspapers. The buyer inspects the property together with the agent and then either makes a private offer or participates in an English auction. Since the (amateur) buyer directly interacts with, and gets most of her information from, a professional acting in the seller's interest, the scope for behavioral biases to affect the behavior of the buyer is increased. Specifically, the physical presence of the realtor at the buyer's inspection of the property makes a halo effect possible.

The market tendency towards using only seller realtors is seen in the business structure of the real estate industries. According to the Real Estate Buyers Agents Association of Australia (REBAA), as of May 2010, between 75 and 100 firms act exclusively as buyer agents in Australia. This compares with over 10,600 agencies selling homes in the country found using a directory search.

The seller and her realtor are responsible for the decision to sell the property by auction or private treaty. Conditional on a sale, private treaties comprise about two-thirds of our sample whereas English auctions account for one-third of observed sales. The use of English auctions is highly dependent on region. Popular areas, such as those in cities or near beaches, are much more likely to have a sale through an auction. The selling realtor is responsible for marketing the property, with marketing costs paid directly by the seller. The commission, typically between 2.5 and 3.0%, goes directly to the seller's realtor, aligning their interests with those of the seller in achieving a high final sale price. In rare cases, a realtor can show and sell property that is listed with another realtor, in which case the commission is split.

3 Data

Data on Australian real estate transactions was collected from public data sources, including domain.com.au and realestate.com.au, from August to November 2010. These sites represent the most popular entry points for searching for homes and therefore comprehensively cover the market. For each listing, the website lists property characteristics such as number of bedrooms, bathrooms, and location, and the realtor firm and specific realtor in charge. When a transaction is completed, the sites are updated to include final sales price and whether the sale was an auction or a private treaty. In approximately 28% of finalized transactions final price is withheld. In our main specification, these observations are simply dropped from the data.

As we are interested in understanding how realtor attractiveness may create a halo effect on home price, we require high quality images of the realtor. Portraits available on multi-agency real estate directories are generally poor quality. Therefore, we used company websites as our source for realtor pictures. Each realtor was matched on name and employer to their company website, where their picture, if present, was collected.

3.1 Amazon Mechanical Turk survey

In order to measure the physical characteristics of the realtors, we recruited subjects from the Amazon Mechanical Turk website. Users at the website sign up to do simple tasks, called Human Intelligence Tasks (HITs), in exchange for a small payment. We designed HITs in which the subjects were presented with a picture of a realtor and were asked to rate their physical attractiveness, professionalism and trustworthiness on a five point scale. We also asked the subjects to estimate the sex, ethnicity, and age group of the realtor and to judge the picture quality. Each picture was rated by ten subjects and the mean ratings are used in our core analysis.

For ratings to be meaningful, we required realtor pictures to have a minimum height of 150 pixels. We also required realtors to have made at least one sale during our sample period in order to be rated. Descriptive statistics of the ratings are given in Table 1. Finally, the answers of four Mechanical Turk subjects were excluded as they were clearly not serious, frequently giving a different answer to the sex question than the other subjects.

There is substantial co-variation in the answers, as is evident from the correlations in Table 2. Perceived physical attractiveness, professionalism and trustworthiness are all strongly positively correlated to each other. As suggested earlier, it is important to take these correlations into account when studying the relationship of attractiveness to house prices. All three characteristics correlate strongly with perceived picture quality. Unreported results show correlation among the characteristics holds true even within firms. This is not because prestigious firms tend to hire both more attractive realtors and better photographers, as the correlation is similar even within firms, e.g. 0.42 for the largest firm in our sample. The disagreement among the subjects, and hence the noise in our measures, is also correlated to the perceived picture quality as well.. When doing the analysis we take care to account for the measurement errors introduced by these factors. Although realtor age and gender also correlate strongly with perceived physical attractiveness, these correlation are unlikely to be caused by subject preferences or photo quality and therefore likely to be present in the perceptions of actual property buyers. As such, they are not problematic in our analysis.

There is another potential source of error stemming from the fact that our subjects are unlikely to be representative of Australian real estate buyers. Ipeirotis (2010) finds that women and young people are overrepresented among AMT users and that their education level tends to be higher. About 47% of users reside in the US, 34% in India and the remaining 19% elsewhere. For obvious reasons, the property buyers in our sample almost exclusively live in Australia. It is plausible that these demographic differences may covary with subjective perceptions of attractiveness etc., in which case our measures of realtor physical characteristics will be biased. However, that should make it harder for us to document any association between those measures and house prices, and as such it is mainly a power issue.

3.2 Descriptive Statistics

A total of 2,119 sales were matched to 1,288 unique realtors that had publicly available pictures of sufficient size. Descriptive statistics for these sales are presented in Table 1. We have divided sales prices into five groups and the average attractiveness of realtors making sales in these groups is monotonically increasing and the difference between the average attractiveness in the highest and lowest groups are statistically significant. It is clear, however, that physical attractiveness is not randomly distributed. Realtors in high income areas tend to be more attractive than realtors in low income areas and there is a difference in average attractiveness across firms, e.g. McGrath and LJ Hooker, which are the largest firms in our sample. Both these differences are significant on a one percent level. We will take care to control for location and firm fixed effects in the formal analysis. About one third of sales are made as auctions. These typically involve more expensive real estate with average prices at auctions being about 50% higher than average prices in private treaties.

4 Empirical Results

To isolate the effects of realtor physical characteristics on house prices, we require some empirical model of how those prices are determined. There are two main approaches to specifying such a model. One method is to rely on repeated sales of the same property, which has the advantage of differencing out all house characteristics that remain constant. The disadvantage is that house characteristics may change and that any individual piece of property is unlikely to sell very frequently. The alternative, so called hedonistic, method attempts to explicitly control for the relevant characteristics of each house. In doing so, the method does not rely on these characteristics being constant and does not exclude property that has only been transacted once in the sample period. The disadvantage is that it is not obvious what the relevant characteristics are and even when identifying them, the appropriate data may not be available. Our short sample period makes a reliance on repeated sales completely unfeasible and so we are faced with the task of identifying a suitable set of house price determinants. As Nicholas:2010p4030 points out, there is no consensus in the literature on what determinants are crucial and what the appropriate functional form of a house price regression should be. As for the determinants, we are largely guided by data availability. It is not standard practice in Australia to give exact size measures, such as square footage, when listing a house for sale, even though the size of the property should be a major factor in determining its value. Instead, we must rely on characteristics such as the number of bedrooms, bathrooms and car spaces. Still, these variables are likely to covary stronlgy with the overall size and should therfore function as good control variables. In addition, they allows us to differentiate between different uses of space. As we are not interested in predicting prices as such, we restrict our sample to houses of "normal" size, where our model is likely to perform better. Specifically, we exclude 61 transactions where the sold house had no bathroom and 42 transactions where there were more than 5 bedrooms or bathrooms. In order to control for the location of the property, which is another major determinant of house prices, we have collected the postcodes in which they are located. The sales mechanism is likely to play a smaller role, but we nevertheless construct a dummy variable that takes the value of one when a property was sold in a private treaty and zero when it was sold on an auction. As for the functional form, we use the natural log of the sales price as our dependent variable. There are reasonable arguments both for and against taking the log of our continuous house characteristics variables. We somewhat arbitrarily opt for not using logs in our main analysis and show that our central results do not depend on this choice in the robustness section.

Having created a set of control variables, we proceed to construct our main variables of interest, i.e. our measures of physical characteristics. As discussed above, and as is shown in Table 2, there is a strong and positive correlation between desirable physical characteristics and picture quality. This may be because the features that mark a picture as high quality, e.g. good lighting and composition, also make the person in the picture appear more attractive. As we are trying to construct variables that capture how the realtor appeared at the time of interaction with the buyer, we seek to remove any picture quality effect by regressing each physical characteristic on picture quality and saving the residuals. The results of these regressions are presented in Table 4. With R-squares ranging from 15 to 27 percent in a simple univariate regression, we appear to pick up an important factor influencing the ratings our subjects gave. An alternative explanation would be that some firms value aesthetics more than others and that this preference would manifest itself both in the attention they pay to taking well composed pictures and in the hiring of people with better physical characteristics. To check this we re-estimate the regressions with firm fixed effects. As the R-squares rise considerably, it appears that some firms do indeed hire people that look more attractive and professional. However, the influence of picture quality on the assessment of physical characteristics also rises, suggesting that we are capturing a real effect of varying picture quality rather than just a sorting of high quality pictures to certain firms. The residuals from these regressions should be interpreted as measures of the underlying physical characteristics, cleaned from the influence of photographers. It is these residuals that are used in the analysis below, and we add a subscript R to denote that they are not the original measures. The perceived age of the realtors also correlate to physical characteristics, especially physical attractiveness. Age is different from picture quality in the sense that it would have been present at the time of the realtor-buyer interaction as well, which is why we don't control for it in these regressions.

However, we include the realtor's perceived age group among the set of control variables we use.

In the next step, we regress the natural log of the sales prices on the residuals described above. We sequentially add fixed effects for firms, month of sale and postcodes and present the estimation results in Table 5. The addition of firm and postcode fixed effects affect the results considerably. A priori, there are strong reasons to include both. If we do not control for firm fixed effects, we expose ourselves to the risk that attractive people may self-select into some firms that deal in particular market segments. The postcode fixed effects capture the crucial importance of location for property values. Should we leave it out, any results could be driven by the selection of good-looking realtors into certain neighborhoods. That the models without firm and postcode fixed effects are likely to be poorly specified is apparent from the relative point estimates of the effects of bedrooms, bathrooms and car spaces. It would be very brave to assign a specific value to each of these features a priori, but it is reasonable to expect that a bedroom should be worth more than a bathroom which in turn should be worth more than a car space. This is indeed the result of the estimation in column four, where the price effect of an additional bedroom is roughly twice that of a bathroom, which in turn is roughly twice that of an additional car space. F-tests reject the null hypotheses of other orderings of the effects with p-values of less than one percent. When not including postcode fixed effects bathrooms appear to be the most valuable addition to a house. The significantly negative coefficient for the private treaty-dummy also makes intuitive sense, in that sellers of more valuable houses would find it easier to carry the fixed costs of an auction. There is also a positive effect on price due to realtor age. This may capture some aspect of salesmanship, either as a proxy for experience or as an expression of survivorship, where only skilled realtors remain in the business to old age. With 348 postcode dummies and 47 firm dummies in a sample of 2,119 observations, we run the obvious risk of over controlling. In spite of this, the attractiveness coefficient is highly significant with a t-value of 3.6. The point estimate of 5.6% should be interpreted as the increased selling price when the attractiveness of a realtor moves up one level on the five point scale. This means that an increase in attractiveness by one standard deviation would raise the price by approximately 2.3%. Evaluated at the mean selling price, that corresponds to about AUD 16,700 (approximately USD 16,000 during the sample period), which is a very substantial amount for the average Australian house buyer. These findings are consistent with the hypothesis that home buyers commonly let their decisions be influenced by sizable halo effects. The result is not qualitatively affected by the high correlation between attractiveness and the other realtor characteristics, as is evident from the last column in Table 5. reference

A possible objection is that attractiveness may be endogenous. Realtors that take their job more seriously could appear more attractive, e.g. by wearing makeup or tailored suits. The same group of realtors could drive up the price by putting more effort into their sales pitch and search for clients. If this was the case, however, we would expect to see the realtors' perceived trustworthiness and, especially, professionalism having a similar effect on prices. On the contrary, both these effects are statistically zero.

The insignificance of the coefficients of trustworthiness and professionalism should also allay concerns that the price effect is a result of buyers drawing (possibly rational) inference about the proposed transaction from the appearance of the realtor. Since there is no obvious reason that attractiveness as such should make a person give more truthful information, stories along these lines must rely on attractiveness correlating with other, more relevant, cues. However, not only are we holding the impression of trustworthiness constant, but there are no direct effects of trustworthiness itself. In light of this, it appears highly unlikely that our results are driven by buyers evaluating information they receive from the realtor based on visual cues.

An interpretation of the results that rely on taste-based discrimination is also unconvincing. As discussed above, this story becomes less plausible once we control for firm fixed effects. Still, one could tell a version of it in which each firm send their best looking people to their largest deals, in the same way other firms may send a limo to pick up potentially large clients from the airport. This way, attractive realtors would be selected to large transactions, but there would be no causal effect of their attractiveness on the selling price. The insignificance of trustworthiness and professionalism implies that the firms only select on attractiveness, though, and do not care about sending their most professional looking realtors to important clients. Though technically possible, that would probably not be the case.

If what we are picking up is indeed a halo effect, we would expect it to express itself differently according to the nature of the buying decision and the interaction between the buyer and the realtor. Specifically, Bernardin:1978p4023 suggests that the halo effect weakens with the exposure to its source. Since the buyer is likely to interact more times with the realtor in a private treaty deal, we would expect it to have more time to wear off. Additionally, the buying decision in a private treaty is not taken under time pressure, unlike that taken in auctions. It is likely that the scope for behavioral biases is larger in situations that do not allow for calm considerations. For instance, Beggs:2009p4352 show pronounced anchoring effects in art auctions. For these reasons, we would expect the effect of realtor attractiveness to be more pronounced in auctions than in private treaties. We test this prediction by interacting the attractiveness variable with the dummy for private treaties and present the estimation results in Table 6. All estimates other than attractiveness and its interaction are robust to this inclusion. The interaction effect itself is significantly negative, in line with what we expected, and the point estimate of the direct effect of realtor attractiveness increases to 8.6% from 5.6%. This is to be interpreted as the attractiveness premium in auctions of a one step increase on the attractiveness scale. A one standard deviation increase in realtor attractiveness in this setting would mean a premium of 4.4%, or a price increase of AUD 45,000 (approximately USD 42,500) evaluated at the mean auction selling price. Although the halo effect is significantly stronger in an auction setting, it is present in private treaties as well. The point estimate of the total effect in a private treaty is the sum of the direct and interacted effects of realtor attractiveness, which is 3.9%. An F-test confirms that this significantly positive with a p-value of 2.6%.

An alternative to the interacted model is to estimate the original regressions on subsamples containing only auctions or only private treaties. We follow this approach and present the results in columns two and three in Table 6. When dividing the sample in this manner the statistical power is reduced and realtor attractiveness is no longer significant. The point estimates, however, are in line with our previous estimations. The 6.6% estimate in column two should be compared to the 8.6% estimate in column one. The 3.12% estimate in the last column corresponds to the previously discussed figure of 3.9%. The fact that the effect of realtor attractiveness differs depending on the transaction setting lends further support to our interpretation of the results as originating in a halo effect.

The finding that realtor attractiveness influences the price at which a property sells may appear to imply that unattractive realtors should be driven out of the market. Realtors may on average be more attractive than non-realtors, but we have no way of testing this as we only know how attractive realtors are relative to each other, i.e. our identification comes from the variation among realtors. There are, however, several factors that would work to limit the selection of attractive realtors into the market. For instance, attractiveness is only one part of the skill set that makes for a successful realtor. Furthermore, in so far that attractive people have better outside options they would require higher compensation when working as realtors. This also means that there need not be any irrationality on the part of sellers, as attractive buyers may simply pocket the premiums they create in the form of higher fees.

5 Robustness Tests

We perform a number of robustness checks on our findings, without any qualitative differences in the results. As discussed above, we take the natural log of our continuous variables and present the results in column one of Table 7. The sample shrinks to 1,834 sales, and there are minor differences in the results, such as the insignificance of the difference between the bathrooms and car spaces variables. This should not be over interpreted, as the main reason for our sample shrinking is that houses with no car spaces drop out. Both the point estimate and statistical significance of the attractiveness variable increase.

In the second column of Table 7, we include our measures of physical characteristics directly, without cleaning them from the effects of picture quality. It does not affect our results. In columns three and four, we recalculate our physical characteristics measures as modes and medians rather than averages. Doing so could decrease the variation in the measures, but at the same time limit the influence of any negligent subjects that may still be in our group of picture raters. The effects of physical attractiveness measured these ways appear to be somewhat smaller than it is for the average measure, but the general picture remains unchanged.

A potential concern with our interacted model is that the selection of houses for auction is surely not random. Although it is not clear how this affects our main findings, we investigate what factors influence the probability of a house selling in a private treaty by estimating probit regressions. The results are presented in Table 8. In column one, we have estimated a basic model with no fixed effects and in column two we include firm and month fixed effects, and some postcode characteristics.

The mean postcode income is meant to capture how fashionable the area where a house is sold is and the postcode population is meant to capture how urban it is. In the last column we control for any postcode fixed effects. Both postcode variables are statistically significant, and houses in more fashionable, urban areas appear to sell on auction with a higher probability. The number of bedrooms, which is a fair proxy for the total size of the house, is highly significant in all three specifications and it appears that larger houses, too, are more likely to go to auctions. However, with the exception of realtor age group in the non-fixed effects regression, realtor physical characteristics are of no importance. Since realtor attractiveness is unrelated to sale type, it is unlikely that our results are driven by some selection along those lines.

6 Conclusion

In this paper we have presented results based on a unique dataset of transaction level, Australian home sales. The institutional settings of Australia make it an ideal place to test behavioral biases in consumer real estate finance. Exploiting this, we have shown how home buyers let their valuations of houses be influenced by such biases in ways that expose them to substantial economic losses. Specifically, we argue that in the minds of buyers, the positive values associated with attractiveness spill over from attractive realtors to the houses they are selling, through the so called halo effect. The size of the attractiveness price premium depends on the setting in which the transaction takes place. A one standard deviation increase in the attractiveness of the realtor results in a price premium of 1.6% in private treaties and 8.6% in auctions. In USD terms this corresponds to $9,000 and $16,000 respectively, evaluated at the sample means. The large influence of the sale type is consistent with a halo effect interpretation.

We contribute to the literature in documenting large behavioral effects in a setting where people have strong incentive to remain rational. To our knowledge, we are the first to do this for the halo effect. We have discussed and rejected alternative explanations of our findings. Any such interpretation is faced with the dual challenge of explaining why the effect is concentrated in sales that are done by auction, and why other physical characteristics of the realtors, such as professionalism and trustworthiness, have no effect. Our findings also contribute to the finance literature in providing additional evidence that behavioral effects are unlikely to be limited to situations where the costs of violating rationality are small.

Although there is no feasible way of constructing trading strategies on the irrational behavior that we have documented, there are valuable practical lessons for the individual home buyer. Since the halo effect tends to wear off with prolonged exposure, a conscientious buyer should have multiple interactions with realtors, especially if they are good looking.

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Table I: Summary statistics of picture ratings

The total number of rated pictures is 1,288, with ten subjects rating each picture. There were a total of 107 unique subjects, rating anywhere from two to 1,275 pictures, with a median of 379 pictures. The physical attractiveness, professionalism, trustworthiness and picture quality scales ranged from 1 (lowest) to 5 (highest). Realtors were classified in one of five possible age categories: 'Younger than 30', '30-39', '40-49', '50-59' and 'Older than 60'. There were seven ethnic descent categories: 'European descent', 'East Asian', 'South Asian', 'Hispanic', 'Middle-eastern', 'African' and 'Other'. For ease of exposition the ethnic descent categories are here collapsed into European and non-European descent.

		Mean	S.D.	Min	Max
Average ratings	Physical attractiveness	3.353	0.503	1.778	4.900
	Professionalism	3.909	0.395	2.400	4.900
	Trustworthiness	3.643	0.336	2.000	4.700
	Age group	2.620	0.775	1.000	4.800
	Picture quality	3.867	0.432	2.300	4.800
Median ratings	Physical attractiveness	3.332	0.580	1.500	5.000
	Professionalism	4.009	0.521	2.000	5.000
	Trustworthiness	3.665	0.475	1.000	5.000
	Age group	2.655	0.845	1.000	5.000
	Picture quality	3.923	0.589	2.000	5.000
Mode ratings	Physical attractiveness	3.321	0.695	1.000	5.000
	Professionalism	4.127	0.670	2.000	5.000
	Trustworthiness	3.664	0.625	1.000	5.000
	Age group	2.662	0.887	1.000	5.000
	Picture quality	4.069	0.792	1.000	5.000
	Male	0.714	0.452	0.000	1.000
	European	0.824	0.381	0.000	1.000
Standard deviations of ratings (for a given picture)	Physical attractiveness	0.847	0.212	0.316	1.643
	Professionalism	0.865	0.226	0.316	1.647
	Trustworthiness	0.883	0.201	0.316	1.549
	Age group	0.610	0.170	0.000	1.265
	Picture quality	0.932	0.269	0.422	1.729
	Male	0.030	0.097	0.000	0.516
	European	0.417	0.131	0.000	0.527

Table II: Rating correlations

This table shows correlation between different rating measures. It is based on a sample ten times 1,288 pictures. Correlations that are statistically different from zero on a five precent significance level are shown in bold.

	Average					Mode		Standard deviation						
	Attractive.	Pro.	Average Trust.	Age.	Picture.	Male	European	Attractive.	Pro.	Average Trust.	Age.	Picture.	Male	European
Average physical attractiveness	**1.00**													
Average professionalism	**0.38**	**1.00**												
Average trustworthiness	**0.54**	**0.61**	**1.00**											
Average age group	**-0.61**	-0.01	**-0.09**	**1.00**										
Average picture quality	**0.45**	**0.42**	**0.52**	**-0.12**	**1.00**									
Mode male	**-0.18**	**0.19**	**-0.15**	**0.12**	-0.03	**1.00**								
Mode European	0.00	-0.03	0.01	**0.11**	-0.02	-0.05	**1.00**							
S.D. of physical attractiveness	**-0.20**	-0.03	-0.03	**0.10**	-0.02	0.05	-0.02	**1.00**						
S.D. of professionalism	**-0.14**	**-0.29**	**-0.06**	0.04	-0.04	0.05	0.03	**0.35**	**1.00**					
S.D. of trustworthiness	**-0.23**	**-0.07**	**-0.24**	**0.13**	**-0.11**	**0.15**	-0.01	**0.33**	**0.30**	**1.00**				
S.D. of age group	**-0.12**	**-0.08**	-0.02	**0.20**	-0.05	-0.03	0.02	0.02	**0.05**	-0.02	**1.00**			
S.D. of picture quality	**-0.21**	**-0.12**	**-0.14**	**0.06**	**-0.59**	0.02	0.02	**0.24**	**0.31**	**0.17**	0.00	**1.00**		
S.D. of male	**0.07**	**-0.07**	**0.06**	-0.04	0.00	**-0.36**	0.03	0.00	0.02	**-0.09**	0.03	0.04	**1.00**	
S.D. of European	**-0.07**	**-0.12**	**-0.14**	-0.00	**-0.09**	**0.08**	**0.14**	-0.03	-0.00	0.05	0.05	-0.02	-0.08	**1.00**

Table III: Summary statistics of sales

Low (high) income post codes have mean house prices below (above) the 25th (75th) percentile. Low (high) physical attractiveness is defined analogously. The firm with the third most observations in our sample made 81 sales. The group of low frequency sellers consists of realtors making one or two sales during the period, the medium frequency sellers made between three and six sales and the high frequency sellers made more than seven sales.

	Obs.	Bedrooms		Baths		Car spaces		Price		Private treaty	
		Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Salestype											
Auction	689	2.92	[1.00]	1.67	[0.77]	1.28	[0.92]	936656	[602868.16]	0	[0.00]
Private Treaty	1430	2.86	[1.03]	1.59	[0.68]	1.43	[0.89]	613938	[439967.19]	1	[0.00]
Post code income											
Low income area	513	3.04	[0.86]	1.53	[0.64]	1.5	[0.90]	442360	[230950.91]	0.8	[0.40]
Medium income area	1027	2.97	[0.99]	1.65	[0.72]	1.44	[0.90]	675456	[414129.00]	0.69	[0.46]
High income area	579	2.59	[1.12]	1.62	[0.74]	1.16	[0.88]	1040869	[684264.17]	0.55	[0.50]
Physical attractiveness											
Low attractiveness	518	2.97	[0.93]	1.57	[0.67]	1.47	[0.98]	581880	[372190.88]	0.74	[0.44]
Medium attractiveness	1018	2.85	[1.02]	1.6	[0.71]	1.36	[0.84]	724472	[530794.71]	0.7	[0.46]
High attractiveness	583	2.86	[1.07]	1.67	[0.74]	1.33	[0.93]	830807	[586039.60]	0.57	[0.49]
Firm											
McGrath	791	3.03	[1.01]	1.74	[0.74]	1.48	[0.89]	805152	[528918.97]	0.54	[0.50]
L J Hooker	370	2.76	[0.94]	1.53	[0.64]	1.29	[0.83]	556751	[356277.46]	0.72	[0.45]
Others	958	2.8	[1.04]	1.54	[0.69]	1.33	[0.94]	710244	[551962.61]	0.77	[0.42]
Sex											
Female	610	2.96	[1.03]	1.62	[0.70]	1.41	[0.95]	767626	[570083.48]	0.69	[0.46]
Male	1509	2.85	[1.01]	1.61	[0.71]	1.37	[0.89]	699162	[498760.86]	0.67	[0.47]
Area											
Sydney	1532	2.78	[1.03]	1.59	[0.71]	1.3	[0.86]	804291	[552913.51]	0.61	[0.49]
Other urban area	318	3.21	[0.88]	1.68	[0.70]	1.61	[0.85]	477135	[312256.45]	0.86	[0.35]
Regional - high urbanisation	138	3.09	[0.88]	1.61	[0.66]	1.51	[0.91]	419188	[216071.64]	0.88	[0.33]
Regional - low urbanisation	30	3	[0.74]	1.43	[0.50]	1.63	[1.19]	391100	[144770.53]	1	[0.00]
Rural	38	3.53	[0.86]	1.71	[0.69]	2.03	[1.70]	540847	[268714.41]	0.89	[0.31]
House prices											
Lowest 10% prices	207	2.32	[0.87]	1.12	[0.32]	1.07	[0.80]	253291	[49689.01]	0.88	[0.32]
10%-25% prices	321	2.67	[0.89]	1.32	[0.50]	1.23	[0.76]	359900	[27247.25]	0.9	[0.30]
25%-75% prices	1060	2.78	[1.02]	1.58	[0.63]	1.38	[0.87]	594602	[125234.15]	0.7	[0.46]
75%-90% prices	319	3.18	[0.86]	1.81	[0.72]	1.52	[1.05]	1009065	[124564.97]	0.41	[0.49]
Highest 10% prices	207	3.78	[0.84]	2.4	[0.85]	1.7	[1.00]	1901690	[775005.99]	0.42	[0.49]
Realtor sells											
Low frequency sellers	615	2.97	[0.93]	1.57	[0.67]	1.47	[0.98]	581880	[372190.88]	0.74	[0.44]
Medium frequency sellers	1000	2.85	[1.02]	1.6	[0.71]	1.36	[0.84]	724472	[530794.71]	0.7	[0.46]
High frequency sellers	504	2.86	[1.07]	1.67	[0.74]	1.33	[0.93]	830807	[586039.60]	0.57	[0.49]
Total	**2119**	**2.88**	**[1.02]**	**1.61**	**[0.71]**	**1.38**	**[0.91]**	**718871**	**[521081.51]**	**0.67**	**[0.47]**

Table III: Summary statistics of sales (continued)

	Obs.	Area income		Postcode income		Postcode population		Physical attractiveness	
		Mean	SD	Mean	SD	Mean	SD	Mean	SD
Salestype									
Auction	689	49764	[3452.02]	70264	[25907.35]	12726	[7222.28]	3.54	[0.52]
Private Treaty	1430	46951	[5654.87]	61089	[22327.88]	15627	[9777.00]	3.39	[0.50]
Post code income									
Low income area	513	45972	[5978.96]	45362	[2735.93]	16489	[10420.10]	3.35	[0.51]
Medium income area	1027	47325	[5423.97]	56743	[5642.95]	15974	[9262.14]	3.4	[0.52]
High income area	579	50800	[1296.81]	97181	[25672.80]	10346	[5329.59]	3.59	[0.48]
Physical attractiveness									
Low attractiveness	518	46418	[6052.81]	58736	[20735.47]	16077	[10298.89]	2.76	[0.22]
Medium attractiveness	1018	48154	[5016.93]	63883	[24385.20]	14647	[9056.16]	3.43	[0.19]
High attractiveness	583	48640	[4451.26]	69117	[24745.79]	13521	[7910.43]	4.06	[0.23]
Firm									
McGrath	791	47354	[5335.58]	66112	[23166.86]	14038	[8887.61]	3.62	[0.47]
L J Hooker	370	47242	[5665.78]	58559	[18411.26]	17672	[11889.85]	3.2	[0.45]
Others	958	48516	[4854.83]	64481	[26039.00]	14073	[7781.52]	3.39	[0.52]
Sex									
Female	610	47554	[5359.04]	67250	[26736.61]	14131	[8318.08]	3.64	[0.55]
Male	1509	47983	[5155.58]	62760	[22571.10]	14916	[9432.98]	3.36	[0.47]
Area									
Sydney	1532	50833	[867.78]	68576	[26020.65]	13437	[7758.81]	3.48	[0.51]
Other urban area	318	40464	[91.25]	50474	[3704.76]	22085	[12034.91]	3.46	[0.53]
Regional - high urbanisation	138	37919	[317.17]	51440	[7421.41]	14956	[8131.35]	3.18	[0.43]
Regional - low urbanisation	30	35038	[448.34]	48154	[6133.05]	6702	[2061.52]	3.02	[0.38]
Rural	38	36096	[162.87]	53676	[9179.55]	8656	[5119.91]	3.1	[0.52]
House prices									
Lowest 10% prices	207	43481	[5856.79]	49740	[11153.56]	20032	[12108.47]	3.21	[0.51]
10%-25% prices	321	44983	[6019.71]	51256	[9345.57]	18304	[10923.41]	3.31	[0.53]
25%-75% prices	1060	48448	[4821.79]	63459	[23458.44]	13756	[8040.70]	3.46	[0.49]
75%-90% prices	319	49990	[3210.28]	74059	[24204.36]	12198	[6661.35]	3.55	[0.53]
Highest 10% prices	207	50475	[2216.70]	85816	[27851.79]	12259	[7168.17]	3.62	[0.48]
Realtor sells									
Low frequency sellers	615	46418	[6052.81]	58736	[20735.47]	16077	[10298.89]	2.76	[0.22]
Medium frequency sellers	1000	48154	[5016.93]	63883	[24385.20]	14647	[9056.16]	3.43	[0.19]
High frequency sellers	504	48640	[4451.26]	69117	[24745.79]	13521	[7910.43]	4.06	[0.23]
Total	**2119**	**47860**	**[5217.30]**	**64052**	**[23925.11]**	**14690**	**[9130.87]**	**3.44**	**[0.51]**

Table III: Summary statistics of sales (continued)

	Obs.	Professionalism		Trustworthiness		Age group		European		Male	
		Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Salestype											
Auction	689	4	[0.38]	3.75	[0.32]	2.47	[0.77]	0.9	[0.30]	0.73	[0.45]
Private Treaty	1430	3.95	[0.37]	3.71	[0.31]	2.63	[0.80]	0.89	[0.31]	0.7	[0.46]
Post code income											
Low income area	513	3.92	[0.39]	3.68	[0.30]	2.6	[0.83]	0.87	[0.33]	0.74	[0.44]
Medium income area	1027	3.99	[0.35]	3.72	[0.32]	2.6	[0.79]	0.88	[0.33]	0.72	[0.45]
High income area	579	3.97	[0.39]	3.75	[0.30]	2.51	[0.78]	0.95	[0.23]	0.67	[0.47]
Physical attractiveness											
Low attractiveness	518	3.78	[0.40]	3.51	[0.29]	3.15	[0.77]	0.89	[0.31]	0.79	[0.41]
Medium attractiveness	1018	4.02	[0.33]	3.72	[0.28]	2.62	[0.69]	0.89	[0.32]	0.76	[0.43]
High attractiveness	583	4.05	[0.36]	3.9	[0.27]	1.99	[0.56]	0.91	[0.28]	0.57	[0.50]
Firm											
McGrath	791	4.06	[0.34]	3.84	[0.28]	2.46	[0.68]	0.92	[0.27]	0.68	[0.47]
L J Hooker	370	3.82	[0.35]	3.57	[0.28]	2.7	[0.84]	0.88	[0.32]	0.76	[0.43]
Others	958	3.95	[0.39]	3.68	[0.31]	2.62	[0.85]	0.88	[0.33]	0.73	[0.45]
Sex											
Female	610	3.93	[0.40]	3.83	[0.31]	2.45	[0.76]	0.86	[0.34]	0	[0.00]
Male	1509	3.99	[0.36]	3.68	[0.30]	2.63	[0.81]	0.91	[0.29]	1	[0.00]
Area											
Sydney	1532	3.98	[0.38]	3.72	[0.32]	2.52	[0.77]	0.87	[0.34]	0.72	[0.45]
Other urban area	318	4	[0.33]	3.78	[0.29]	2.65	[0.85]	0.99	[0.08]	0.66	[0.47]
Regional - high urbanisation	138	3.84	[0.40]	3.62	[0.29]	2.81	[0.74]	0.89	[0.31]	0.76	[0.43]
Regional - low urbanisation	30	3.89	[0.35]	3.61	[0.24]	3.42	[0.85]	1	[0.00]	0.8	[0.41]
Rural	38	3.88	[0.33]	3.63	[0.24]	2.97	[0.94]	1	[0.00]	0.47	[0.51]
House prices											
Lowest 10% prices	207	3.9	[0.38]	3.67	[0.31]	2.82	[0.83]	0.91	[0.28]	0.72	[0.45]
10%-25% prices	321	3.91	[0.39]	3.7	[0.30]	2.67	[0.86]	0.91	[0.28]	0.73	[0.45]
25%-75% prices	1060	3.98	[0.36]	3.71	[0.31]	2.52	[0.78]	0.87	[0.33]	0.73	[0.45]
75%-90% prices	319	4	[0.38]	3.76	[0.34]	2.53	[0.77]	0.92	[0.27]	0.68	[0.47]
Highest 10% prices	207	4.02	[0.37]	3.79	[0.30]	2.55	[0.75]	0.91	[0.28]	0.65	[0.48]
Realtor sells											
Low frequency sellers	615	3.78	[0.40]	3.51	[0.29]	3.15	[0.77]	0.89	[0.31]	0.79	[0.41]
Medium frequency sellers	1000	4.02	[0.33]	3.72	[0.28]	2.62	[0.69]	0.89	[0.32]	0.76	[0.43]
High frequency sellers	504	4.05	[0.36]	3.9	[0.27]	1.99	[0.56]	0.91	[0.28]	0.57	[0.50]
Total	**2119**	**3.97**	**[0.37]**	**3.72**	**[0.31]**	**2.58**	**[0.80]**	**0.89**	**[0.31]**	**0.71**	**[0.45]**

Table IV: Effects of picture quality

This table shows how ratings of physical characteristics depend on picture quality. t-stats are given in parentheses. ***, ** and * denote statistical significance on the one, five and ten percent level respectively.

Dependent variable	Attractiveness	Attractiveness	Professionalism	Professionalism	Trustworthiness	Trustworthiness
Picture Quality	0.489***	0.594***	0.328***	0.382***	0.368***	0.393***
	(21.44)	(19.27)	(19.38)	(16.27)	(28.29)	(20.61)
Constant	1.501***	1.086***	2.670***	2.457***	2.261***	2.161***
	(16.48)	(8.862)	(39.55)	(26.34)	(43.55)	(28.48)
Firm fixed effects	No	Yes	No	Yes	No	Yes
Observations	2,119	2,119	2,119	2,119	2,119	2,119
R-squared	0.178	0.349	0.151	0.291	0.274	0.322

Table V: Effects of realtor attractiveness

The dependent variable in these regressions is the natural log of the selling price. R denotes residuals from the fixed effects regressions described in table X. Standard errors are clustered on realtors and firms. t-stats are given in parentheses. ***, ** and * denote statistical significance on the one, five and ten percent level respectively.

	(1)	(2)	(3)	(4)	(5)
Bedrooms	0.0790***	0.125***	0.125***	0.230***	0.230***
	(4.727)	(4.774)	(4.807)	(14.5)	(14.62)
Bathrooms	0.293***	0.226***	0.225***	0.124***	0.123***
	(8.794)	(10.4)	(10.33)	(8.943)	(8.854)
Car spaces	-0.00981	0.0348***	0.0351***	0.0657***	0.0654***
	(-0.397)	(3.467)	(3.572)	(4.909)	(4.85)
Age group	-0.0149	0.0348	0.0352	0.0257*	0.0198
	(-0.349)	(1.441)	(1.477)	(1.856)	(1.472)
Private treaty	-0.390***	-0.339***	-0.337***	-0.166***	-0.166***
	(-5.915)	(-3.982)	(-4.023)	(-10.45)	(-10.29)
Attractiveness_R	0.155***	0.220***	0.216***	0.0563***	0.0417***
	(2.628)	(5.162)	(5.246)	(3.539)	(2.661)
Professionalism_R	0.0263	0.0136	0.0168	-0.0135	
	(0.749)	(0.415)	(0.506)	(-0.719)	
Trustworthiness_R	-0.0778	-0.104**	-0.0949**	-0.0294	
	(-1.455)	(-2.101)	(-2.032)	(-1.049)	
Constant	12.93***	12.72***	12.69***	12.54***	12.56***
	(144.6)	(124.3)	(130)	(196)	(188.8)
Firm fixed effects	No	Yes	Yes	Yes	Yes
Month fixed effects	No	No	Yes	Yes	Yes
Postcode fixed effects	No	No	No	Yes	Yes
Observations	2,119	2,119	2,119	2,119	2,119
R-squared	0.352	0.498	0.5	0.799	0.799

Table VII: Robustness checks

This table shows variations to our main specifications. The dependent variable is the natural log of the selling price. All regressions control for month, firm and postcode fixed effects. Standard errors are clustered on realtors and firms. t-stats are given in parentheses. ***, ** and * denote statistical significance on the one, five and ten percent level respectively.

	(1)	(2)	(3)	(4)
Bedrooms		0.231***	0.230***	0.230***
		(14.57)	(14.78)	(14.68)
Bathrooms		0.124***	0.124***	0.124***
		(8.938)	(9.103)	(9.003)
Car spaces		0.0656***	0.0658***	0.0652***
		(4.94)	(4.959)	(4.902)
log(Bedrooms)	0.532***			
	(14.99)			
log(Bathrooms)	0.234***			
	(10.5)			
log(Car spaces)	0.178***			
	(5.691)			
Age group	0.0270*	0.0253*	0.0149	0.0155
	(1.846)	(1.762)	(0.876)	(1.012)
Private treaty	-0.158***	-0.166***	-0.165***	-0.165***
	(-6.307)	(-10.34)	(-10.24)	(-10.01)
Attractiveness_R	0.0637***			
	(4.449)			
Professionalism_R	-0.024			
	(-0.892)			
Trustworthiness_R	-0.0158			
	(-0.638)			
Picture quality		-0.0245		
		(-0.849)		
Attractiveness		0.0558***		
		-3.484		
Professionalism		(0.0136)		
		(-0.718)		
Trustworthiness		(0.029)		
		(-1.020)		
Attractiveness_R Mode			0.0254**	
			(2.165)	
Professionalism_R Mode			0.00635	
			(0.625)	
Trustworthiness_R Mode			-0.00472	
			(-0.385)	
Attractiveness_R Median				0.0304**
				(2.243)
Professionalism_R Median				0.00711
				(0.433)
Trustworthiness_R Median				-0.00644
				(-0.289)
Constant	12.75***	12.62***	12.57***	12.57***
	(142.7)	(93.7)	(175.9)	(181.1)
Observations	1,834	2,119	2,119	2,119
R-squared	0.808	0.799	0.799	0.799

Table VIII: Sale type determinants

This table shows estimations if a probit regression, where the dependent variable is a dummy taking the value one if a sale was an auction and zero otherwise. Standard errors are clustered on firms. t-stats are given in parentheses. ***, ** and * denote statistical significance on the one, five and ten percent level respectively.

	(1)	(2)	(3)
Bedrooms	-0.0588	-0.165***	-0.289***
	(-1.119)	(-3.932)	(-6.294)
Bathrooms	-0.161*	0.00238	0.077
	(-1.784)	(0.029)	(1.025)
Car spaces	0.197***	0.144***	0.0645
	(4.16)	(3.391)	(1.137)
Age group	0.208***	0.0825	-0.0214
	(2.812)	(0.878)	(-0.125)
Attractiveness_R	0.0694	-0.029	-0.0598
	(0.724)	(-0.194)	(-0.250)
Professionalism_R	-0.11	-0.116	0.0599
	(-0.426)	(-0.408)	(0.472)
Trustworthiness_R	-0.0644	-0.0186	-0.161
	(-0.526)	(-0.123)	(-0.778)
log(Mean postcode income)		-0.710**	
		(-1.962)	
log(Postcode population)		0.107*	
		-1.856	
Constant	0.094	7.263**	0.671
	(0.354)	(2.114)	(1.086)
Firm fixed effects	No	Yes	Yes
Month fixed effects	No	Yes	Yes
Postcode fixed effects	No	No	Yes
Observations	2,119	1,990	1,639
Pseudo R-squared	0.023	0.129	0.274

Greener Pastures
Substitute Trading in Blackout Periods *

Abstract

Using new UK legislation that imposes mandatory trading bans prior to company reports, I test the hypothesis that insiders will respond to such bans by trading in correlated stocks for which they are not classified as insiders. I fail to replicate the results from the existing literature on voluntary trading bans and find an ambiguous effect of the new legislation on bid-ask spreads. Furthermore, I find no evidence for substantial stock substitute trading. Possible reasons for the differences between my findings and the existing literature is discussed, and I conclude that previously found effects of insider blackout periods on bid-ask spreads are likely to be overstated.

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1 Background and related research

While undoubtedly illegal in most jurisdictions, there is no clear consensus on whether a ban on insider trading is socially desirable. The debate goes back to Manne (1966) and Carlton and Fischel (1983) who argue that insider trading may be desirable as it improves the accuracy of stock prices. A company would clearly be reluctant to disclose all information that is relevant to the stock price if this includes business secrets. In the case of “soft” information the company might be willing to disclose it, but unable to do so in a credible way. Insider trading is one way to incorporate such information in the stock price without publicly disclosing it. There are, however, several substantial drawbacks. Increasing the number of insider trades in a stock will decrease its liquidity. This in turn will increase the firm’s cost of capital. Firms may nevertheless find insider trading to be a tax efficient way of compensating its managers. The legislator will obviously see less advantages of such a practice. It may also lead to substantial agency problems. Managers that are allowed to trade in their own stock would have incentives to increase its volatility or disseminate false information in order to create opportunities to make profitable trades. Furthermore, it may be in their interest to perform below the expectation of the market as this is easier than beating the same expectations and since this would allow them to profit on the resulting price reaction.

Bettis et al. (2000) study voluntarily adopted restrictions on insider trading in the United States. Using survey data collected in 1996 they construct a sample of 626 members of the American Society of Corporate Secretaries and find that 92 % of these firms enforced some sort of voluntary trading restriction and 78 % imposed an insider trading blackout period similar to the one that is now mandatory in the UK. Although many companies reserved the right to allow insiders to trade after special approval, these blackout periods successfully limited insider trading preceding company reports. Furthermore, a somewhat smaller subsample, consisting of 403 firms, is used to examine the effects of these restrictions on stock liquidity. Regressing bid-ask spreads on a dummy variable indicating whether a company is in a blackout period or not and on a number of control variables they find that the spread narrows by about two basis points when insiders are not allowed to trade. This indicates that insiders do possess material information and that their access to the market is taken into account when bid-ask spreads are determined. Furthermore, the widespread practice of voluntarily restrictions suggests that most companies perceive insider trading in their own stock to be a net disadvantage. The analysis of stock substitute trading differs on several points from that of ordinary insider trading. This is discussed within a theoretical legal framework by Ayres and Bankman (2001). Though a company presumably has little interest in the accuracy of the stock price of its competitors and suppliers, they may be very interested in compensating their managers by allowing them to profit on insider information by trading in such companies. The liquidity of the companys own stock would not be affected and such trades would essentially mean a transfer of wealth from the shareholders of the substitute company to those of the company itself. Raising the cost of capital of a competitor may even be advantageous in its own right. Furthermore, many of the agency problems discussed above would disappear as the manager would have less ability to affect the performance and information releases of other companies. Ayres and Bankman report that voluntary company restrictions of stock substitute trading are unusual and anecdotal evidence suggest that the practice itself is common. Since it does not raise the cost of capital of the own firm, it cannot be said to be in violation of the managers fiduciary responsibilities. Although somewhat of a gray area, the authors therefore maintain that such trading is not illegal in the United States and quote a former SEC chief economist saying that the SEC has never prosecuted anyone for such trades or even questioned an episode of such trading. The authors argue that the current US legal code is likely to be undermined by stock substitute trading and should be rewritten to include a more general notion of illegal insider trading.

2 Theory

2.1 Hypotheses

Although my main interest is in stock substitute trading, my dataset offers a new testing ground for the predictions made by Bettis et al. My dataset has several advantages over theirs. Since the blackout period regulations in their study are voluntary, there is always a question of endogeneity. It is plausible that companies that take active measures against insider trading differ systematically from those that do not. Hence, it does not follow that imposing similar bans on all companies will have the same effect. For the purpose of evaluating such legislation, a dataset in which blackout period policy is guaranteed to be unrelated to firm characteristics, is obviously preferred. Since the blackout periods of Bettis et al. are not stipulated by law, they are also heterogeneous. In particular, some allow for exceptions under certain circumstances. In my dataset all firms have the same blackout period policy and there are no legal exceptions. Since blackout periods in the UK can be backed out of widely available report date data, there is no need to rely on surveys and I can obtain a larger dataset. Finally, if Bettis et al. are correct the introduction of the law making blackout periods mandatory offers an exogenous event suitable for difference-in-difference estimation.

The main proposition to be tested in this paper, however, is that corporate insiders respond to legislation imposing blackout periods by migrating to stock substitutes. Ideally this should be tested using micro data on the trades of individual insiders. Lacking such data, I will test the hypothesis indirectly by examining how the liquidity of potential stock substitutes reacts when a firm enters a blackout period. Specifically, I will analyze how the bid-ask spread reacts. Since blackout periods should cause insider traders to migrate to substitute stocks and thus raise the risk for market makers in that stock to trade with informed traders, I expect that companies whose substitute stocks are in a blackout period should experience widening bid-ask spreads, all else equal. However, even absent blackout period-legislation it is reasonable to assume that bid-ask spreads should vary with the distance to company reports. This is because the bid-ask of a stock depends, among other things, on the amount of asymmetric information in the market. This, in turn, is likely to depend on when credible information about the company was released to the market. To the extent that company reports contain information that is relevant to its substitutes, and this is an implicit assumption in the argument, the time distance to substitute company reports should also affect the bid-ask spread. Fortunately, the introduction of the Financial Services and Markets Act of 2000, which took effect on November 30th, 2001, offers a nice exogenous event that allows us to control for such effects. Although some companies may have enforced voluntary trading restrictions prior to this date, it is a reasonable assumption that the effect should be stronger once the law made such restrictions mandatory and disallowed exceptions. My main hypothesis therefore is that companies whose substitute stocks are in a blackout period after November 30th, 2001, all else equal, should experience a larger widening of bid-ask spreads than they did in corresponding periods before that date.

2.2 Control variables

There is a large literature on the determinants of bid-ask spreads and there is wide agreement on which variables are most important. Madhavan (2000) offers a good survey of these issues. The controls used in my regressions are briefly motivated here. Theory predicts that equilibrium bid-ask spreads are set so as to allow the market maker to make up for her fixed costs and the losses she makes to informed traders (that are on the right side of the trades) with the gains she gets from trading with uniformed traders (that are on the right side of the trade on average half of the times). Higher trading volumes tend to be associated with a larger proportion of uninformed traders, thus allowing the market maker to make a smaller profit on each of them while still breaking even. High trading volumes also makes it easier for the market maker to adjust her holdings to match her inventory target. Therefore, higher trading volumes should be associated with narrowing bid-ask spreads.

Since prices are not set continuously, the tick size could potentially set a lower bound on the bid-ask spread. I expect the inverse of the price to be positively correlated with the spreads. Large firms generally attract more attention and are followed more closely by analysts and the media. This should bring more information about the firm to the publics attention, thus making it harder for insiders to amass material non-public information. In reducing the asymmetric information in the market it should narrow the bid-ask spreads. Firms with high market-to-book ratios tend to have a large proportion of their expected cash flows far away in time, making them harder to analyze. This should tend to increase the information asymmetries in these stocks, thus widening the bid-ask spreads.

In order to accommodate buy orders, market makers hold an inventory of the stock they are trading. There is a risk that this inventory changes in value, and risk averse market makers will seek compensation for this risk by widening their quotes. As the risk increases with the volatility of the stock, so will the demanded compensation. Increasing volatility should therefore be correlated with widening bid-ask spreads.

As firms release information to the market, one could argue that the information asymmetries should decrease (since information that was previously only held by insiders is now public). However, Kim and Verrecchia (1994) show in a theoretical framework that the information dissemination process around earnings announcements exacerbates the problem of information symmetry and show a widening of the bid-ask spread. In this model agents process information differently when hearing information about the firm fundamentals. Heterogeneous opinions among the market participants cause a widening of the bid ask spread after earnings announcement even though market liquidity is increasing in the precision of public information. In another paper, Lee et al. (1993) document this effect empirically. The authors find that spreads increase during the half-hour containing the earnings announcement and are persistently wider for the rest of the day. As Bettis et al. (2000) point out, it should matter whether a firm is in a blackout period. When insiders are prohibited from trading their own stock, the risk of encountering an informed trader should, all else equal, be lower. This should be reflected in narrower bid-ask spreads.

3 Data

3.1 Sample

My original sample consists of all companies in the FTSE all share index as of July 13th, 2006. For these companies I collect annual and quarterly report dates for the period between December 1st, 1997 and June 30th, 2006 from the Reuter database. Data on bid and ask prices, market-to-book ratios, market capitalization, SIC industry codes and trading volume is collected from Datastream. Companies for which no data is available for one or more of these variables are dropped, leaving us with a sample of 647 companies. Furthermore, I drop 13 observations where the trading volume is reported as negative and an additional 4548 observations where the ask bid price is reported as higher than the ask price. The resulting panel contains 670 149 observations.

3.2 Construction of variables

I define the bid-ask spread as the difference between the closing bid and ask prices divided by the mean of the two. The volatility is calculated as the standard deviation of closing prices using a two month rolling window. Trading volume is included as the log of the number of traded shares each day. Market capitalization is also included as a log value. The inverse of the last trading price each day is used to control for tick size effects. To avoid potential endogeneity issues, the volume, market capitalization and price variables are lagged one day. Following Bettis et al., the post announcement dummy suggested by Lee et al. (1993) is set to three days after and including the announcement day.

I would like to identify as substitute assets any two assets where inside information on one asset would be material to the other. I use a number of different proxies for such a relationship. One approach is to classify any two stocks with the same SIC industry code as substitutes and using three different industry code levels I construct as many classifications. Another approach, which is the one generally followed in this paper, is to use the correlation between the prices of two stocks. Since insider information is likely to concern firm specific information rather than market conditions I estimate a simple CAPM model and calculate the correlations of the residuals. Exactly how high a correlation should be demanded for two stocks to be considered substitutes is an open question. Therefore I do the classifications for 17 different thresholds ranging from 5 % to 90 %. For simplicity, most results will be shown for the thresholds 0.2, 0.35 and 0.5 only.

Given a certain way of identifying which stocks are potential substitutes, there are several alternative ways to include this in a regression. The most straightforward way is to construct a simple dummy variable to indicate whether a firm has some substitute stock that is in a blackout period on a given day. Since additional substitute firms in blackout periods should increase the spreads even more, one might instead include the number of substitute firms. Note that this implicitly assumes that the effect of additional firms is linear. The characteristics of the substitute firms should also have implications for the magnitude of the effect. Large firms, for instance, might have more and richer insiders. To check this, the cumulative market value of the substitute firms could be used. Similarly, more traded stocks should have more insiders ready to migrate. Therefore one might include the cumulative trading volume of substitute stocks as an explanatory variable. It is also a reasonable conjecture that the opportunity set of the insiders that are affected by a blackout period should matter. Specifically, the more substitute companies an insider has to choose from, the lesser should be her expected impact on the bid-ask spread on any given one of them. This hypothesis may be explored by including the average number of substitute firms available for relevant insiders at a given day.

I shall begin by examining the entire dataset without controlling for time fixed effects. This offers a comparison to the results of Bettis et al. When proceeding to include time fixed effects, I are forced to only use a subsample of my dataset. This is due to its large size and the computer power required to run fixed effects regressions on it. I therefore calculate the average turnover for each stock and select the 250 most traded firms. I pick companies with high trading volume (in pounds) since the data should be less noisy, e.g. the bid-ask spread should be less sticky. For robustness I also randomly divide the dataset into three subsamples of roughly equal size. The companies included in the study and the composition of the subsamples are available from the author.

3.3 Descriptive statistics

The average number of companies that are classified as substitutes for different subsamples and criteria are shown in table 1. As might be expected, the number of substitutes decreases quickly with the correlation threshold and in most subsamples it is not possible to estimate the regressions for the highest thresholds. The industry codes classify many companies as substitutes and may be to coarse to pick up the effect.

Descriptive statistics for the different samples are given in table 2. The high volume sample is, of course, not representative for the entire sample. It consists of larger, less volatile stocks with lower market-to-book values and narrower bid-ask spreads.

Company reports tend to be somewhat clustered in time especially in March and September. Figure 1 shows the distribution of reports across months.

The clustering of company reports make the use of dummy variables that are connected to the report dates problematic. It becomes essential to control for time effects that may otherwise be picked up by such dummies.

Table 3 gives the correlation coefficients between a number of variables that could proxy for the probability of stock substitute trading. Unfortunately, they are highly correlated and regressions involving two or more of them may have multicollinarity problems.

4 Methodology and results

4.1 The Bettis et al methodology

Before testing the for stock substitute trading, I will attempt to replicate the results of Bettis et al. on my dataset. My first approach is to run the exact same regression on bid-ask spreads as they do, i.e. I regress bid-ask spreads on a dummy indicating whether a company is in blackout period and the controls discussed above. I only use observations after the law making blackout periods mandatory was introduced. The results are given in table 4. When not controlling for fixed effects, the blackout period dummy appears to have a highly significant positive effect on the bid-ask spreads. Once fixed effects are controlled for, however, there is no significance. This result casts serious doubts on the results in the Bettis et al. paper, as they do not include fixed effects in their regression. It is of course possible that there is some other reason that spreads are widening during the blackout period. Perhaps more asymmetric information accumulates when the company report date approaches. My interest is in the causal effect of blackout periods on bid-ask spreads, not in whether bid-ask spreads tend to be higher during blackout periods or not. If blackout periods indeed affect bid-ask spreads by shutting insiders out from the market, the sudden sharpening of these regulations in 2001 should significantly lower the spreads during blackout periods as compared to the period before 2001. I therefore interact the blackout period dummy with a dummy variable that takes the value of one after the blackout period law was introduced and zero before. The results from this regression are shown in table 5. Yet, there is no indication that the mandatory blackout periods introduced by the law had an effect on stock liquidity in any of my subsamples.

4.2 Stock substitute trading

In spite of the lacking support for the blackout period effect previously documented by Bettis et al., I proceed to test the implications of stock substitute trading on stock liquidity. If there is indeed such implications, the regressions estimated above and in Bettis et al. are misspecified. I therefore add to the regressions a dummy variable taking the value of one whenever a company has a substitute that is in a blackout period. Since report dates tend to cluster in time, the effect of the companies own insiders ceasing to trade the stock may simply be canceled by insiders from substitute companies moving in. I therefore introduce a dummy variable that takes the value of one whenever the company has some substitute stock in a blackout period. This regression is estimated for three different correlation thresholds on the period after the law was introduced and the results are given in table 6. Again, there is no support for the theory that blackout periods work to narrow spreads. If anything, bid-ask spreads tend to be wider during blackout periods. Furthermore, the empirics offer no support to the concerns of Ayres and Bankman that blackout periods cause insiders to migrate with their information to substitute stocks. I attempt different variables to proxy for this presumed effect, but the (non)result is robust to these different specifications. Estimates for regressions using other proxy variables are given in table 7. The results fit the Ayres and Bankman theory when using cumulative trading volume or market cap of the substitute stocks to proxy for stock substitute trading, while controlling for the number of potential targets, i.e. substitute stocks, available to insiders. The companies own blackout period, however, tend to widen the spreads, which does not fit the Bettis et al. story.

In these regressions, too, there may be other things going on in periods close to the release of information that may blur the result. I therefore proceed to another difference-in-difference estimation. These results are given in table 8. As indicated by the product of the regulation dummy and the blackout period dummy, bid-ask spreads tend to be narrow for firms that enter blackout periods after the regulation was introduced, but the effect is not robust across samples. Specifically there is no significant effect in the high volume sample, the data of which should be most reliable. There are also some indications that stock substitute trading affects the liquidity. The signs are as predicted by theory, i.e. the bid-ask spread of a given company appear to widen more during the blackout periods of its substitute companies after such blackout periods were made mandatory. The effect is significant for high correlation thresholds in all subsamples. It is to be expected that the effect should be less significant for lower values of the threshold as low correlation companies should be poorer candidates for substitute trading. This expected pattern of point estimates is present in the subsample selected for high volume and in random sample one. The random subsamples two and three, however, do not show this pattern. Figure 2 shows the point estimates from the high volume sample for different correlation thresholds. Although the point estimates vary with sample and thresholds, the stock substitution effect generally must be said to be economically significant. For the high volume sample, bid-ask spreads are on average 0.29 percentage points higher for firms which have a substitute firm (with a correlation threshold of 0.5) in a blackout period. This is substantial as the average spread in the high volume sample is 0.68 percent.

I further estimate the above regression, using different proxies for stock substitute trading. The results are given in table 9. The effect appears to remain significant for other specifications as well. The inclusion of the investment opportunity variable is less reassuring. One would expect the impact of stock substitute trading to be lower on any given firm, the larger the opportunity set of the substitute companys insider is. These regressions, however, point to the opposite. Furthermore, the effects of substitute volume and market value are not robust to this inclusion, possibly because it proxies for the number of substitute firms in a blackout period. This would also introduce multicollinearity into regression (4). The correlation is over 0.9, and hence regressions (4) to (6) should perhaps be disregarded.

I next exclude the half of the blackout period that is closest to the company report. The reason for this is that there may be concerns that increased information asymmetries close to the report may drive the result. Such asymmetries should be much less problematic two weeks before the report, though. Thus redefining the substitute variables only to include half the blackout periods I re-estimate the regressions and give the result in table 10. Again, the law appears to have an effect.

As an additional robustness test I re-estimate the regression (using the full blackout periods) on data from the period one year before to one year after the 30th November for each year between 1999 and 2004, treating the day in the middle as the introduction of the law. If the result found above is not spurious, I would expect there to be no significant effect for the other break points. The estimates for the relevant variables from these regressions are given in table 11. Since I find a significant effect of introducing the law, even in years when no law was actually introduced, the above result loses much of its credibility and can probably be dismissed as spurious. Even more devastating to the result is that the effect of the actual introduction of the law is reversed in this smaller sample. Therefore, I cannot claim to have found any support for the hypothesis of stock substitute trading.

5 Conclusion

I fail to replicate the effect documented by Bettis et al., that bid-ask spreads tend to narrow during company blackout periods. There may be several reasons for this. I are looking at UK data, whereas Bettis at al. have a dataset from the US. It is, however, difficult to point to any institutions that would nullify the effect of insider trading bans in the UK, while not doing so in the US. Since Bettis et al. are using survey data, there is always the issues of response bias and endogeneity. It is possible that companies that have taken active measures against insider trading are more likely to respond to a survey examining such measures. It is furthermore plausible that the companies choosing to implement voluntary blackout periods have perceived insider trading to have some impact on their stocks liquidity. If this is the source of the difference between mt results and those of Bettis et al., it would in no way invalidate theirs. It would, however, have implications for the legislator. If the introduction of mandatory blackout periods had no effect on liquidity because companies for which it would have had an effect had already voluntarily adopted similar policies, the law is redundant. Another possible source of the difference in results is econometric. Bettis et al. do not control for firm or time fixed effects. If report dates are clustered in time, as is the case in my data, market wide variations in bid-ask spreads may, although unrelated to the trading bans, be caught by the blackout period dummy. The danger of this is particularly large as they only have access to one year of data. I believe this to be the most likely cause of the different results and that there is thus a high possibility that the findings of Bettis et al. are spurious. I also fail to document any robust effect of stock substitute trading, as hypothesized by Ayres and Bankman. The fact that I fail to document an effect does not, of course, mean that there cannot be one. It does, however, set some upper limit to its magnitude. This should ease their concerns that such trading is a major drain on stock market liquidity, as well as their eagerness to adjust the law in order to control it.

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Figure 1. Total number of company reports for different months.

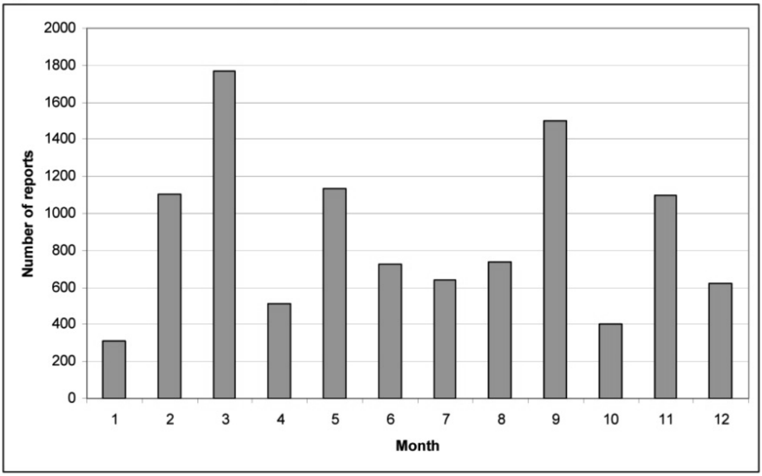

Figure 2. Point estimates for different correlation thresholds.

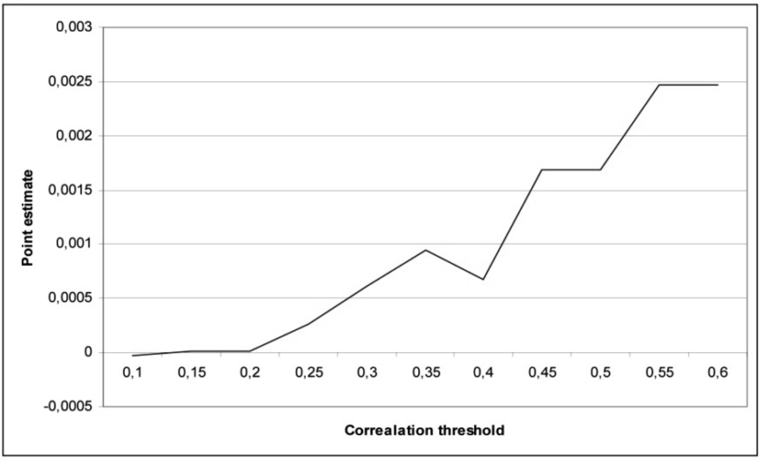

Table 1. Average number of substitute companies for different definitions.

Classification threshold	Subsample 1	Subsample 2	Subsample 3	High volume sample
0,10	124,360	133,180	126,220	70,227
0,15	45,153	49,891	45,600	18,207
0,20	22,090	24,204	21,253	5,600
0,25	13,324	14,335	12,000	2,093
0,30	7,419	8,204	7,018	0,993
0,35	3,829	4,127	3,871	0,527
0,40	2,045	1,869	2,018	0,273
0,45	1,158	0,842	1,031	0,073
0,50	0,662	0,421	0,542	0,053
0,55	0,500	0,267	0,320	0,027
0,60	0,374	0,163	0,227	0,020
0,65	0,293	0,104	0,164	0,007
0,70	0,221	0,090	0,098	0,007
0,75	0,104	0,032	0,040	0,007
0,80	0,009	0,005	0,009	0,007
0,85	0	0,005	0	0,007
0,90	0	0,005	0	0,007
ICBIC	136,840	139,810	134,430	101,370
ICBSC	43,162	44,339	43,996	22,647
ICBSUC	30,757	32,778	32,089	9,440
Number of companies	222	221	225	150

Table 2. Descriptive statistics.

Sample	Statistic	Number of observations	Mean	Std. Deviation
Complete Sample	Bid-ask spread	1242655	.0201814	.0658642
	Market value	1311199	2204.16	8928.582
	Market-to-book	1300914	2.318071	43.46811
	Volume	1146285	2920.2	16342.29
	Volatility	1310235	.2962671	.2073482
	Price	1311199	447.7746	4418.013
High volume subsample	Bid-ask spread	182159	.0067767	.0118817
	Market value	189633	11320.76	19466.41
	Market-to-book	185376	1.769211	35.30113
	Volume	182977	14507.23	38487.74
	Volatility	189472	.3573792	.1938933
	Price	189633	472.3505	446.9255
Random subsample 1	Bid-ask spread	407810	.0230606	.0327243
	Market value	424432	2553.793	10312.8
	Market-to-book	419731	3.280913	28.94228
	Volume	373464	3650.463	25870.78
	Volatility	424116	.3075751	.2094333
	Price	424432	392.0957	842.6764
Random subsample 2	Bid-ask spread	416407	.0226701	.0360824
	Market value	442215	1787.698	5198.529
	Market-to-book	438671	1.221825	67.29926
	Volume	388695	2502.202	8352.959
	Volatility	441933	.2916934	.2080628
	Price	442215	530.7704	7414.427
Random subsample 3	Bid-ask spread	400156	.0223654	.0358667
	Market value	425753	1993.965	9006.445
	Market-to-book	423713	2.50547	16.7343
	Volume	367629	2564.682	8454.363
	Volatility	425400	.2893429	.2057878
	Price	425753	418.2589	1513.63

Table 3. Correlation coefficients for substitute trading proxies for different correlation thresholds.

Correlation threshold			0.2			
	Blackout dummy	Substitute dummy	Number of substitutes	log(substitute_volume)	log(substitute_marketvalue)	Number of twins of twins
Blackout dummy	1.0000	0.0438	0.0238	0.0679	0.0632	0.0145
Substitute dummy	0.0438	1.0000	0.6043	0.8907	0.9591	0.5857
Number of substitutes	0.0238	0.6043	1.0000	0.6934	0.7068	0.9741
log(substitute_volume)	0.0679	0.8907	0.6934	1.0000	0.9501	0.6577
log(substitute_marketvalue)	0.0632	0.9591	0.7068	0.9501	1.0000	
Substitute substitutes	0.0145	0.5857	0.9741	0.6577	0.6783	1.0000

Correlation threshold			0.35			
	Blackout dummy	Substitute dummy	Number of substitutes	log(substitute_volume)	log(substitute_marketvalue)	Number of twins of twins
Blackout dummy	1.0000	0.0334	0.0188	0.0442	0.0434	0.0133
Substitute dummy	0.0334	1.0000	0.7930	0.9325	0.9806	0.7451
Number of substitutes	0.0188	0.7930	1.0000	0.8006	0.8233	0.9735
log(substitute_volume)	0.0442	0.9325	0.8006	1.0000	0.9571	0.7418
log(substitute_marketvalue)	0.0434	0.9806	0.8233	0.9571	1.0000	0.7731
Substitute substitutes	0.0133	0.7451	0.9735	0.7418	0.7731	1.0000

Correlation threshold			0.5			
	Blackout dummy	Substitute dummy	Number of substitutes	log(substitute_volume)	log(substitute_marketvalue)	Number of twins of twins
Blackout dummy	1.0000	0.0123	0.0113	0.0198	0.0206	0.0043
Substitute dummy	0.0123	1.0000	0.8572	0.8987	0.9764	0.7003
Number of substitutes	0.0113	0.8572	1.0000	0.8369	0.8541	0.9242
log(substitute_volume)	0.0198	0.8987	0.8369	1.0000	0.9310	0.7186
log(substitute_marketvalue)	0.0206	0.9764	0.8541	0.9310	1.0000	0.6863
Substitute substitutes	0.0043	0.7003	0.9242	0.7186	0.6863	1.0000

Notes: Blackout dummy takes the value of one when a firm is in a blackout period and zero otherwise. The substitute dummy takes the value one when a firm has some substitute firm that is currently in a blackout period. Substitute_volume is the cumulative trading volume of substitute firms on a given day. Substitute_marketvalue is the cumulative market value of substitute firms on a given day. Substitute substitutes is the number of firms classified as substitutes for the substitutes of a given firm, i.e. the choice set of an insider from a substitute firm. The correlation threshold is the minimum correlation required to classify two companies as substitutes.

Table 5. Difference-in-difference regression results.

Sample	High volume sample	Random Sample 1	Random Sample 2	Random Sample 3
Blackout period dummy	.000182	.0006532**	.0001969	.0004231**
	(0.216)	(0.016)	(0.544)	(0.040)
Blackout period dummy*Regualtion dummy	.0000538	-.0004127	-.0006263	-.0003979
	(0.781)	(0.354)	(0.116)	(0.280)
1/price	.4350508***	.3656862***	.2444588***	.4317796***
	(0.000)	(0.000)	(0.000)	(0.000)
log(volume)	-.000865***	-.0012405***	-.0010187***	-.0007847***
	(0.002)	(0.000)	(0.000)	(0.000)
log(marketvalue)	-.0019329***	-.0109085***	-.0147054***	-.0089513***
	(0.004)	(0.000)	(0.000)	(0.000)
Volatility	.0049429**	.0195189***	.0183429***	.0144223***
	(0.016)	(0.000)	(0.000)	(0.000)
Post-announcement dummy	.0002018	-.0002122	-.0002917	-.0002569
	(0.171)	(0.532)	(0.364)	(0.238)
Market-to-book	-3.38e-06***	1.57e-06	-3.98e-06***	-5.89e-06
	(0.000)	(0.851)	(0.000)	(0.655)
Observations	261815	352397	360501	340991
R^2	0.5982	0.3880	0.4088	0.5176

Notes: The dependent variable is bid-ask spreads. Blackout period dummy takes the value of one when a firm is in a blackout period and zero otherwise. The regulation dummy takes the value of one once the law takes effect, and zero otherwise. Volume is number of traded shares. Volatility is estimated using a two month rolling window and annualized. All regressions use time and firm fixed effects and cluster the standard errors on firms. All regressions use HAC robust standard errors. p-values are given in parentheses and stars signify significance on 10%, 5% and 1% confidence levels correspondingly.

Table 6a. Stock substitute trading after the regulation took effect.

Sample	High volume sample			Random sample 1		
Correlation threshold	0.2	0.35	0.5	0.2	0.35	0.5
Blackout period dummy	.0002965*	.0003217**	.0003469**	.0003178	.0003693	.0002959
	(0.059)	(0.042)	(0.021)	(0.291)	(0.185)	(0.309)
Substitute stock blackout dummy	.0003516	.0002916	-.0004371**	-.0022408	-.0012392	.0001287
	(0.310)	(0.604)	(0.012)	(0.349)	(0.244)	(0.859)
1/Price	.3087696***	.3084324***	.3085401***	.1555427**	.1545798*	.1549528*
	(0.000)	(0.000)	(0.000)	(0.049)	(0.052)	(0.052)
log(Volume)	-.0009426***	-.0009465***	-.0009434***	-.0010043***	-.0010054***	-.0010067***
	(0.000)	(0.000)	(0.000)	(0.003)	(0.003)	(0.004)
log(Marketvalue)	-.0030031***	-.0029983***	-.0029879***	-.0153875***	-.0153762***	-.0154006***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Volatility	.006789***	.0067959***	.0068008***	.0161898***	.0161948***	.0162463***
	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)
Post announcement dummy	.0003872**	.0004022**	.0004092**	-.0004443	-.0004221	-.0004897
	(0.029)	(0.021)	(0.018)	(0.353)	(0.367)	(0.276)
Market-to-book	-1.48e-06***	-1.39e-06***	-1.39e-06***	1.63e-06	1.58e-06	1.74e-06
	(0.000)	(0.000)	(0.000)	(0.727)	(0.734)	(0.710)
Observations	101260	101260	101260	214824	214824	214824
R^2	0.3135	0.3135	0.3135	0.3368	0.3368	0.3367

Notes: The dependant variable is bid-ask spreads. Blackout period dummy takes the value of one when a firm is in a blackout period and zero otherwise. The substitute stock dummy takes the value of one when a substitute firm is in a blackout period and zero otherwise. Volume is number of traded shares. Volatility is estimated using a two month rolling window and annualized. Correlation thresholds give the minimum correlation required to classify two stocks as substitutes. All regressions use time and firm fixed effects and cluster the standard errors on firms. All regressions use HAC robust standard errors. p-values are given in parentheses and stars signify significance on 10%, 5% and 1% confidence levels correspondingly.

Table 6b. Stock substitute trading after the regulation took effect.

Sample	Random sample 2			Random sample 3		
Correlation threshold	0.2	0.35	0.5	0.2	0.35	0.5
Blackout period dummy	-.0004412	-.0004536	-.0004773	.0000914	.0000873	.0000876
	(0.275)	(0.246)	(0.247)	(0.715)	(0.727)	(0.731)
Substitute stock blackout dummy	-.0016777*	-.0001154	.000387	.0002931	.0001323	.0001035
	(0.096)	(0.863)	(0.239)	(0.587)	(0.692)	(0.790)
1/Price	.213239***	.214615***	.2146572***	.3130893***	.312831***	.3127789***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
log(Volume)	-.0007011***	-.0007005***	-.0007004***	-.0007904***	-.0007903***	-.0007905***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
log(Marketvalue)	-.0194662***	-.0194534***	-.0194615***	-.0107329***	-.0107398***	-.0107403***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Volatility	.0179532***	.0179454***	.0179394***	.0102164**	.0102215**	.0102152**
	(0.005)	(0.005)	(0.005)	(0.012)	(0.012)	(0.012)
Post announcement dummy	-.000731*	-.0007323**	-.0007453*	-.0002071	-.000209	-.0002093
	(0.057)	(0.050)	(0.054)	(0.495)	(0.491)	(0.493)
Market-to-book	-2.66e-06**	-2.66e-06**	-2.67e-06**	-9.55e-06**	-9.54e-06**	-9.55e-06**
	(0.040)	(0.040)	(0.039)	(0.012)	(0.012)	(0.012)
Observations	219832	219832	219832	211956	211956	211956
R^2	0.3610	0.3610	0.3610	0.4454	0.4454	0.4454

Notes: The dependant variable is bid-ask spreads. Blackout period dummy takes the value of one when a firm is in a blackout period and zero otherwise. The substitute stock dummy takes the value of one when a substitute firm is in a blackout period and zero otherwise. Volume is number of traded shares. Volatility is estimated using a two month rolling window and annualized. Correlation thresholds give the minimum correlation required to classify two stocks as substitutes. All regressions use time and firm fixed effects and cluster the standard errors on firms. All regressions use HAC robust standard errors. p-values are given in parentheses and stars signify significance on 10%, 5% and 1% confidence levels correspondingly.

Table 7. Alternative proxies for stock substitute trading.

	(1)	(2)	(3)	(4)	(5)	(6)
Blackout period dummy	.0003469**	.000341**	.0003388**	.000345**	.0003466**	.0003447**
	(0.021)	(0.023)	(0.024)	(0.022)	(0.021)	(0.022)
Number of substitutes	-.0004371**			-.0009002*		
	(0.012)			(0.058)		
Cumulative substitute trading volume		-2.86e-09			5.87e-09**	
		(0.314)			(0.016)	
Cumulative substitute market value			-6.49e-11			1.91e-08***
			(0.962)			(0.002)
Investment opportunities of substitutes				.0003015	-.0003535**	-.0008711***
				(0.219)	(0.016)	(0.006)
Observations	153268	153268	153268	153268	153268	153268
R^2	0.4614	0.4614	0.4614	0.4614	0.4614	0.4615

Notes: The dependent variable is bid-ask spreads. Blackout period dummy takes the value of one when a firm is in a blackout period and zero otherwise. Cumulative substitute trading volume is the sum of the substitutes' trading volume. Cumulative substitute market value is the sum of the substitutes' market value. Investment opportunities of substitutes is the average number of substitutes available to insiders from substitute firms. All regressions use a correlations threshold of 0.5 and are estimated on the sample selected for high volume. The regressions also control for the inverse of the price, the log of trading volume, the log of market value, volatility, market-to-book value and a three day post-announcement period. All regressions use time and firm fixed effects and cluster the standard errors on firms. All regressions use HAC robust standard errors. p-values are given in parentheses and stars signify significance on 10%, 5% and 1% confidence levels correspondingly.

Table 8a. Difference-in difference regression results.

Sample	High volume sample			Random sample 1		
Correlation threshold	0.2	0.35	0.5	0.2	0.35	0.5
Blackout period dummy	.0002653	.000288*	.0002655	.00064**	.0007637***	.0008368***
	(0.103)	(0.094)	(0.111)	(0.018)	(0.009)	(0.004)
Substitutestock blackout dummy	-.000442	-.0010316***	-.0018023***	.0014582	-.0027711**	-.0025579***
	(0.367)	(0.009)	(0.000)	(0.451)	(0.011)	(0.003)
Regulation dummy*Blackout period dummy	.0000587	.0000662	.0000934	-.0003792	-.000536	-.0007226
	(0.756)	(0.749)	(0.636)	(0.387)	(0.219)	(0.111)
Regulation dummy*Substitute stock blackout dummy	.0009857*	.0015381**	.0029138***	-.0046101	.0028198	.003927***
	(0.060)	(0.020)	(0.000)	(0.313)	(0.123)	(0.003)
1/Price	.3841752***	.3840291***	.3840668***	.3666025***	.3732316***	.3728694***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
log(Volume)	-.0008439***	-.0008387***	-.0008426***	-.0012422***	-.0012318***	-.0012154***
	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)
log(Marketvalue)	-.0026381***	-.0026509***	-.0026422***	-.0108518***	-.0108688***	-.0108584***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Volatility	.0063748***	.0062891***	.0063255***	.0194691***	.0194181***	.0194497***
	(0.002)	(0.002)	(0.002)	(0.000)	(0.000)	(0.000)
Post announcement dummy	.0002363*	.0002482*	.0002438*	-.000179	-.0001997	-.0002529
	(0.099)	(0.080)	(0.082)	(0.617)	(0.571)	(0.461)
M2B	-1.33e-06**	-1.20e-06**	-1.21e-06**	1.17e-06	2.67e-06	2.70e-06
	(0.013)	(0.023)	(0.023)	(0.881)	(0.752)	(0.749)
Observations	261815	261815	261815	352397	352397	352397
R^2	0.5803	0.5804	0.5805	0.3882	0.3884	0.3887

Notes: The dependent variable is bid-ask spreads. Blackout period dummy takes the value of one when a firm is in a blackout period and zero otherwise. The substitute stock dummy takes the value of one when a substitute firm is in a blackout period. The regulation dummy takes the value of one once the law takes effect, and zero otherwise. Volume is number of traded shares. Volatility is estimated using a two month rolling window and annualized. Correlation thresholds give the minimum correlation required to classify two stocks as substitutes. All regressions use time and firm fixed effects and cluster the standard errors on firms. All regressions use HAC robust standard errors. p-values are given in parentheses and stars signify significance on 10%, 5% and 1% confidence levels correspondingly.

Table 8b. Difference-in difference regression results.

Sample	Random sample 2			Random sample 3		
Correlation threshold	0.2	0.35	0.5	0.2	0.35	0.5
Blackout period dummy	.0002596	.0003918	.0003251	.0004448**	.0005016**	.0004893**
	(0.430)	(0.261)	(0.359)	(0.036)	(0.018)	(0.018)
Substitutestock blackout dummy	-.0046823***	-.0041071***	-.0035112***	-.0031587***	-.0020362**	-.0011618*
	(0.005)	(0.000)	(0.001)	(0.002)	(0.013)	(0.078)
Regulation dummy*Blackout period dummy	-.0007001*	-.0009043**	-.0008072*	-.0004257	-.0005107	-.0005041
	(0.080)	(0.040)	(0.055)	(0.258)	(0.164)	(0.159)
Regulation dummy*Substitutestock blackout dummy	.0054036**	.0056505***	.0051979***	.005345***	.0035798***	.0022536**
	(0.011)	(0.001)	(0.000)	(0.000)	(0.002)	(0.018)
1/Price	.2459203***	.2466308***	.2460698***	.4308424***	.4310605***	.4318461***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
log(Volume)	-.0010218***	-.0009918***	-.000964***	-.0007791***	-.0007672***	-.0007715***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
log(Marketvalue)	-.0147099***	-.0146248***	-.0146842***	-.0090027***	-.0090032***	-.0089408***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Volatility	.0181764***	.0178461***	.0177416***	.0142934***	.0144006***	.0144084***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Post announcement dummy	-.0002869	-.0003079	-.0003142	-.0002437	-.0002624	-.0002704
	(0.370)	(0.320)	(0.327)	(0.263)	(0.227)	(0.215)
M2B	-4.05e-06***	-4.40e-06***	-4.57e-06***	-6.10e-06	-4.82e-06	-5.90e-06
	(0.000)	(0.000)	(0.000)	(0.629)	(0.695)	(0.642)
Observations	360501	360501	360501	340991	340991	340991
R^2	0.4092	0.4101	0.4100	0.5183	0.5186	0.5180

Notes: The dependent variable is bid-ask spreads. The blackout period dummy takes the value of one when a firm is in a blackout period and zero otherwise. The substitute stock dummy takes the value of one when a substitute firm is in a blackout period. The regulation dummy takes the value of one once the law takes effect, and zero otherwise. Volume is number of traded shares. Volatility is estimated using a two month rolling window and annualized. Correlation thresholds give the minimum correlation required to classify two stocks as substitutes. All regressions use time and firm fixed effects and cluster the standard errors on firms. All regressions use HAC robust standard errors. p-values are given in parentheses and stars signify significance on 10%, 5% and 1% confidence levels correspondingly.

Table 9. Alternative proxies for stock substitute trading

	(1)	(2)	(3)	(4)	(5)	(6)
Blackout period dummy	.0002655	.0002358	.0002474	.0002671	.0002683	.0002686
	(0.111)	(0.154)	(0.135)	(0.109)	(0.108)	(0.107)
Number of substitutes	-.0018023***			-.0013156		
	(0.000)			(0.205)		
Cumulative substitute trading volume		-2.96e-08***			8.59e-09	
		(0.000)			(0.110)	
Cumulative substitute market value			-1.71e-08***			1.77e-08***
			(0.003)			(0.006)
Investment opportunities of substitutes				-.0002919	-.0011282***	-.0016234***
				(0.585)	(0.000)	(0.000)
Regulation dummy*blackout period dummy	.0000934	.0001326	.0001111	.0000897	.0000872	.0000863
	(0.636)	(0.499)	(0.572)	(0.651)	(0.660)	(0.663)
Regulation dummy*number of substitutes	.0029138***			.0019786		
	(0.000)			(0.136)		
Regulation dummy*cumulative substitute trading volume		4.72e-08***			-1.18e-08*	
		(0.001)			(0.088)	
Regulation dummy*cumulative substitute market value			3.38e-08***			-1.24e-08**
			(0.005)			(0.049)
Regulation dummy*investment opportunities of substitutes				.0005767	.0018612***	.0021148***
				(0.430)	(0.000)	(0.000)
Observations	261815	261815	261815	261815	261815	261815
R^2	0.5803	0.5802	0.5802	0.5803	0.5803	0.5803

Notes: The dependent variable is bid-ask spreads. Blackout period dummy takes the value of one when a firm is in a blackout period and zero otherwise. The regulation dummy takes the value of one once the law takes effect, and zero otherwise. Cumulative substitute trading volume is the sum of the substitutes' trading volume. Cumulative substitute market value is the sum of the substitutes' market value. Investment opportunities of substitutes is the average number of substitutes available to insiders from substitute firms. All regressions use a correlations threshold of 0.5 and are estimated on the sample selected for high volume. The regressions also control for the inverse of the price, the log of trading volume, the log of market value, volatility, market-to-book value and a three day post-announcement period. All regressions use time and firm fixed effects and cluster the standard errors on firms. All regressions use HAC robust standard errors. p-values are given in parentheses and stars signify significance on 10%, 5% and 1% confidence levels correspondingly.

Table 10. Regression with halved blackout periods.

Correlation threshold	0.2	0.35	0.5
Half blackout period dummy	.0002449	.0002628	.000246
	(0.130)	(0.119)	(0.136)
Half substitute stock blackout dummy	-.0003288	-.0010467***	-.0017997***
	(0.463)	(0.006)	(0.000)
Regulation dummy*Half blackout period dummy	.0001048	.0001015	.0001228
	(0.581)	(0.613)	(0.530)
Regulation dummy*Half substitute stock blackout dummy	.0007264	.0015989**	.0028978***
	(0.159)	(0.020)	(0.000)
1/Price	.3841444***	.3840433***	.3840783***
	(0.000)	(0.000)	(0.000)
log(Volume)	-.0008444***	-.0008407***	-.0008435***
	(0.001)	(0.001)	(0.001)
log(Marketvalue)	-.0026369***	-.0026475***	-.0026402***
	(0.000)	(0.000)	(0.000)
Volatility	.0063545***	.0063019***	.0063266***
	(0.002)	(0.002)	(0.002)
Post announcement dummy	.0002437*	.0002443*	.0002431*
	(0.082)	(0.081)	(0.082)
Market-to-book	-1.28e-06**	-1.20e-06**	-1.21e-06**
	(0.016)	(0.023)	(0.023)
Observations	261815	261815	261815
R^2	0.5803	0.5804	0.5805

Notes: The dependent variable is bid-ask spreads. The half blackout period dummy takes the value of one when a firm is in the first half of its blackout period and zero otherwise. The half substitute stock blackout dummy takes the value of one when a substitute firm is in the first half of a blackout period. The regulation dummy takes the value of one once the law takes effect, and zero otherwise. Volume is number of traded shares. Volatility is estimated using a two month rolling window and annualized. Correlation thresholds give the minimum correlation required to classify two stocks as substitutes. All regressions use time and firm fixed effects and cluster the standard errors on firms. All regressions use HAC robust standard errors. p-values are given in parentheses and stars signify significance on 10%, 5% and 1% confidence levels correspondingly.

Table 11. Estimates for different break points in the difference-in-difference regression.

Break point	Blackout dummy	Substitute blackout dummy	Regulation*blackout dummy	Regulation dummy*substitute blackout dummy
1999-11-30	.0003764**	.0001012	-.0001234	.0000895
	(0.044)	(0.781)	(0.779)	(0.913)
2000-11-30	.0000939	-.0008917	.0006819	.0021904***
	(0.843)	(0.167)	(0.247)	(0.002)
2001-11-30	.0005587*	.000417*	-.0002794	-.00119**
	(0.024)	(0.074)	(0.557)	(0.017)
2002-11-30	.0003296	-.0009308**	-.0004171	.0014159***
	(0.485)	(0.023)	(0.489)	(0.006)
2003-11-30	-.000322	-.001505***	.0007127**	.0027986***
	(0.225)	(0.000)	(0.031)	(0.000)
2004-11-30	.0004459***	-.0009147***	-.0003075*	.0016152***
	(0.003)	(0.000)	(0.073)	(0.000)

Notes: The break point is the date where the regulation dummy starts taking the value one. Regressions are estimated on a rolling window two years around the breakpoint. We control for firm and time specific fixed effects. Standard errors are robust and clustered on firms.