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Abstract

Building on the concept of reciprocity in directed weighted networks, we propose a framework to study legislative vote trading. We first discuss the conditions to quantify vote trading empirically. We then illustrate how a simple empirical framework—complementary to existing approaches—can facilitate the discovery and measurement of vote trading in roll-call data. The application of the suggested procedure preserves the micro-structure of trades between individual legislators, shedding light on, so far, unstudied aspects of vote trading. Validation is provided via Monte Carlo simulation of the legislative process (with and without vote trading). Applications to two major studies in the field provide richer, yet consistent evidence on vote trading in US politics.

Keywords

Vote trading, roll-call voting, networks, reciprocity, US Congress

JEL Classification

D72, D85

1 Introduction

Vote trading, also commonly known as logrolling, is a cornerstone of the economic analysis of politicians' behavior in collective decision-making. As a form of hidden cooperation between legislators with typically opposing political views, vote trading is crucial to the legislative process, for better or for worse.

Reciprocity is an obvious aspect of the cooperative behavior in which two legislators engage when voting in favor of each others' preferred bills in order to secure the passage of both bills (Stratmann, 1992, 1995; Cohen and Malloy, 2014). As a logical consequence, vote trading leaves behind reciprocity patterns in voting records. As we will argue, accounting for such patterns is a necessary condition for any valid estimation of vote trading; a condition that is not properly considered by existing empirical approaches. We propose a conceptual and methodological framework to find and quantify such patterns, facilitating the estimation of vote trading on a systemic level (and in a scalable fashion).

Empirically assessing the prevalence of vote trading is very challenging due to its 'hidden' nature. Strategically offering one's vote to a fellow legislator with opposing political views may contrast with the expectation of voters and peers, so legislators on both sides of a vote-trading agreement are keen to keep such deals secret.[1] Hence, quantifying vote trading requires us to measure something that is not directly observable.

Econometric analysis has proven useful to test the likely existence of vote trading among small sets of votes, given specific prior information about the presence of vote-trading coalitions (qualitatively reported) (Stratmann, 1992, 1995; Esteves and Mesevage, 2017). It has also proven useful in the context of broadly applicable theories of vote trading, allowing the consideration of several years of roll call voting across a wide spectrum of policy issues (Cohen and Malloy, 2014). However, all empirical approaches taken in previous studies do not explicitly capture a key theoretical aspect of vote trading: the *reciprocal* exchange of votes between legislators favoring specific bills. Importantly, none

[1] In roll-calls, legislators engaged in vote trading can observe each others' actions once the votes are cast and, thus, know with certainty whether their partner kept their part of the bargain. However, as they have strong incentives to keep such deals secret, they cannot openly punish defection. Under the usual protocol of sequential vote casting, logrolling in the form of a one-shot game would present a sequential prisoner's dilemma. In the case of a potential trade between two legislators, the legislator whose preferred bill is first voted would always be better off by defecting after the other legislator holds their part of the bargain. Knowing this, the other legislator is better off defecting in the first place anyway. Hence, if cooperation exists (in the form of logrolling), it emerges as reciprocal behavior over repeated interactions (Axelrod, 1984). The adoption of a "tit-for-tat" strategy to overcome the prisoner's dilemma in the case of congressional vote trading is also pointed out in the context of historical anecdotal records (Jillson and Wilson, 1994).

of the previous studies rejects the notion that the *reciprocal* exchange of votes between legislators favoring specific bills is a central aspect of legislative vote trading. Arguably, reciprocity in voting favors is the very essence of (if not the definition of) vote trading.[2]

Complementing previous contributions, our method helps to establish, statistically, if a pattern of reciprocity present in the data is sufficient to be considered as evidence for a given theory of vote trading. We argue that statistically documenting a reciprocal pattern in voting favors is an important aspect of providing evidence on vote trading. Our proposal is broadly applicable and builds on three well-accepted theoretical principles: 1) traded votes involve legislators' deviations from their expected voting behavior, 2) these deviations benefit specific legislators (i.e., they are favors to other legislators), and 3) these favors are systematically reciprocal. Building on the concept of reciprocity in directed weighted networks, our framework incorporates these three principles and provides an index quantifying the likely prevalence of vote trading. In simple terms, we construct a network linking the deviating legislators (*deviators*) to legislators who have strong preferences for the passage of the respective bill (*beneficiaries*). Then, we assess whether the resulting network of directed deviations yields a systematic pattern of reciprocity.

We validate the basic method with a generic simulation of the legislative process. In this setting, we have full control over the presence of vote trading in the data. The simulation study shows that our method detects the prevalence (or lack) of vote trading in the legislative process when we increase (or decrease) vote-trading activity among the agents. We also show that a measured pattern of vote trading becomes distinguishable from 0 with more observations (more votes and more legislators).

Finally, we illustrate how our method can be applied and extended to study vote trading in two different, previously investigated, settings (one in the U.S. Senate: Cohen and Malloy 2014, and one in the U.S. House: Stratmann 1992). In the first application, we show how our framework allows quantifying the reciprocal pattern consistent with Cohen and Malloy 2014's theory of vote trading. In a second step, we document the micro-structure of potential trade relationships. In the second application we illustrate how our framework can help to study vote trading over long periods of time and different scales. In both applications we find richer, yet consistent evidence on vote trading in US politics.

The paper is structured in the following way. Section 2 discusses the historical relevance of vote trading in US politics and the literature on the topic. Section 3 introduces the methodology and 4 validates it via simulation. In sec-

[2]See, for example, the definition of vote trading/logrolling used in(Cohen and Malloy, 2014, p. 64):"*Logrolling is the colloquial term for vote trading, and refers to the idea that a politician may, at times, trade away their vote on one issue in return for votes on some other issue of more concern to their constituents*" .

tions 5 and 6, we present the two applications of our method to roll-call voting in the US Congress. Finally, section 7 discusses the method limitations and provides concluding remarks.

2 Background and literature

Historical anecdotal evidence suggests that vote trading plays an important role in US politics (Pastor, 1982; Kingdon, 1989; Schraufnagel, 2011; Bordewich, 2016). For example, Kingdon (1989, p. 100) quotes the statement of an anonymous representative regarding their traded vote on a bill deregulating cigarette advertisement:

"*This will be sort of a buddy vote. I know cigarettes are harmful and I wouldn't touch them myself. But a lot of my friends are concerned about this, because tobacco means a lot to the economy of their areas. They do things for me when I need it, and I'll do this for them. Frankly, it's just a matter of helping out your friends.*"

Historians and political scientists have documented that vote trading can be found across the entire history of the U.S. Congress. In fact, historical records mentioning vote trading date back to the first congress in 1789 (Bordewich, 2016).[3] In the early 19th century, James K. Polk (US President and former Speaker of the House), reportedly "stood against the practice of logrolling, or vote trading, considering it a form of corruption" (Schraufnagel, 2011, p. 170). Reports of recent occurrences of vote trading in the US Congress often mention these activities in the context of special interest politics. A well-known example is the (failed) congressional attempt to revise tariffs to trigger an economic boost during the Great Depression. It led President Herbert Hoover to conclude that "[c]ongressional [tariff] revisions [...] with all their necessary collateral surroundings in lobbies, logrolling and the activities of group interests, are disturbing to public confidence" (Pastor, 1982, p. 69).

Early scientific inquiry into the nature and importance of vote trading dates back to Arthur Bentley's work (Bentley, 1908). Since then, a substantive theoretical body of literature on the issue has emerged (Buchanan and Tullock, 1962; Wilson, 1969; Tullock, 1970; Haefele, 1970; Riker and Brams, 1973; Bernholz, 1974, 1978; Shepsle and Weingast, 1981; Mueller, 2003; Casella et al., 2014; Casella and Palfrey, 2019). Studies on vote trading distinguish two particular forms: *implicit* and *explicit* trades. The former refers to exchanging favors at the

[3]According to Bordewich (2016, p. 148), vote trading was key when deciding on the permanent location of the congress: "No other issue that had come before congress had produced the same frenzy of backroom bartering and vote trading." In this context, Congressman Fisher Ames has been described as having "descried 'this vile and unreasonable business' of feverish vote swapping but nevertheless put his weight and his eloquence into the battle."

drafting stage of so-called 'omnibus bills', resembling a package of policies favoring different groups and, thereby, ensuring passage of all policies in one vote ('trades' thus take place within the same piece of legislation). The latter refers to legislators voting in favor of each other's preferred bills (or amendments on bills) with the aim of ensuring minimal winning coalitions in every roll-call involved in the trade. Most empirical studies–as well as our framework–focus on *explicit* vote trading, whereby legislators trade with each other by voting in favor of each other's favorite bills.[4]

Empirical studies on vote trading are scarce and can be classified into three types. The first consists of laboratory experiments (McKelvey and Ordeshook, 1980; Eckel and Holt, 1989; Casella et al., 2014; Casella and Palfrey, 2017). These studies focus on testing the micro-mechanisms that elicit incentives to trade votes in controlled environments. Second, there are interview studies, as well as case studies based on anecdotal evidence (see, e.g., Wright 1986). Finally, the third type consists of econometric work (Kau and Rubin, 1979; Stratmann, 1992, 1995; Cohen and Malloy, 2014). These studies test hypotheses on how roll-call data would look in the absence of vote trading. Several empirical contributions investigate other forms of vote trading. For instance, Akzoy (2012) studies implicit logrolling ('within-legislation logrolling') in the European Union's Council of Ministers as part of a bargaining process over individual pieces of legislation. Kardasheva (2013), also focusing on EU legislative politics, investigates intercameral logrolling in the form of package deals framed in exchange between the European Parliament and the Council of Ministers. Most recently, Esteves and Mesevage (2017) study the specific historical setting of logrolling between parliamentary subcommittees within the issue of the approval of new railway routes in 19th century Great Britain.

We complement previous contributions by providing a framework to quantify the prevalence (or absence) of reciprocal patterns in the voting data that would be expected under vote trading. In the next section we guide the reader step-by-step through the framework.

3 Methodological framework

Our framework builds on three well-accepted theoretical principles: 1) traded votes involve legislators' deviations from their expected voting behavior, 2) these deviations benefit specific legislators (i.e., they are favors to other legislators), and 3) these favors are systematically reciprocal. Closely following these theoretical pillars, the core of our framework consists of two components. First, we construct a network linking the deviating legislators to legislators who

[4]We use the terms 'vote trading' and 'logrolling' interchangeably for explicit vote trading in what follows.

have strong preferences for the passage of the respective bill. Second, we assess whether the resulting network of directed deviations yields a systematic pattern of reciprocity.

3.1 A networks perspective on vote trading

First, allow us to introduce some notation conventions that we will use throughout the paper. A 'tuple' (i,k) denotes a legislator i and a roll-call k. We use tuples to indicate relationships between legislators and roll-calls, for example, a Yes vote. A tuple of tuples such as $((i,k),(j,l))$ represents a pair of decisions of the same type, for example, legislator i voting in roll-call k and legislator j voting in roll-call l. We denote sets with bold capitalized characters such as $\mathbf{X}$. Usually, we use sets to indicate collections of tuples. Finally, we employ hollow capitalized characters to denote matrices. For example, $\mathbb{A}$ indicates a matrix such that $\mathbb{A}_{ik}$ is the entry in row i and column k. In what follows we apply this notation to discuss the issue of vote trading from a networks perspective.

The empirical problem of measuring the prevalence of vote trading in roll-call data can be generalized as follows. For a specific legislature, we want to assess whether vote trading can explain some of the voting decisions cast in the K roll-call votes taken during a specific period. The starting point is a set of Yes roll-call decisions $\mathbf{V}$ from N legislators voting in K roll-calls. $\mathbf{V}_{ik} = 1$ means that legislator i voted Yes in roll-call k (and No otherwise). In addition, we have a dataset $\mathbf{X}$ with information on legislator characteristics, bill characteristics, constituency characteristics, and any other available information that we consider relevant to explain usual voting behavior (independent of trades). It follows that, as long as $\mathbf{V}$ contains the universe of Yes votes ever cast by the N legislators in it, a traded vote would have some counterpart on one or several other votes. Following this line of thought, the ideal method to detect vote trading would take $\mathbf{V}$ and $\mathbf{X}$ as inputs, and return a subset $\mathbf{V}_{trades} \subseteq \mathbf{V}$ containing all voting decisions $(i,k) \in \mathbf{V}$ involved in all trades.

In practice, any approach to measure the prevalence of vote trading builds on a specific logrolling theory that informs the empirical assessment (Kau and Rubin 1979; Stratmann 1992, 1995; Akzoy 2012; Kardasheva 2013; Cohen and Malloy 2014; Esteves and Mesevage 2017). For example, as suggested by Cohen and Malloy (2014), US Senators might trade votes on bills that are irrelevant to their state's major industries but highly relevant to the major industries of some of their colleagues' states (similar to the anecdotal evidence mentioned in the introduction). In this setting, the implied theory of vote trading is that senators trade their votes on bills affecting industries that are highly relevant for the economies of the senators' states. Thus, we can think of senators having strong preferences for economic policies that favor the major industries/corporations in their state but caring much less about how industries in other states are affected (in fact, it is likely that they are against policies that favor other states' industries based on a limited budget).[5] Such theoretical basis guides the empirical assessment in two ways: First, it shapes the specification of an empirical model to explain the observed voting pattern at the level of individual decisions ($(i,k) \in \mathbf{V}$) in order to rule out alternative explanations of $(i,k) \in \mathbf{V}$ (exactly what has been done in previous empirical studies). Second, it defines which pairs of decisions $((i,k),(j,l))$ in $\mathbf{V}$ are potential trades.

The latter leads to some *necessary conditions* to consider the raw data as a valid basis for empirical evidence for vote trading: First, any meaningful theory of vote trading would at least inform us about legislator i's strong preferences for specific policy proposals j. Second, any meaningful empirical approach to test this theory would rely on these preferences being observable, at least approximately.

Suppose we code the revealed preferences in matrix $\mathbb{P}$, indicating whether legislator i has strong preferences in favor of bill/policy k. It follows that, based on $\mathbb{V}$ and $\mathbb{P}$, we can check the necessary condition of having at least one pair of decisions $((i,k),(j,l))$ in $\mathbb{V}$ that can be considered a potential trade. Given a theory of vote trading that provides $\mathbb{P}$, a trade would appear in the data as a tuple $((i,k),(j,l))$, where legislator j has strong preferences for bill k and legislator i has strong preferences for bill l. That is, i voted in favor of a bill that j strongly supports and vice versa. Such cases can be identified by simply scanning $\mathbb{V}$ and $\mathbb{P}$.[6]

If no reciprocal votes are found in $\mathbb{V}$, none of the voting decisions encoded in these data can be explained by the vote trading hypothesis under investigation.[7]

If we find cases of reciprocal votes in the data, we would like to assess whether this pattern can indeed be considered evidence of vote trading. That is, we want to rule out alternative explanations of that pattern. In a first step, this means differentiating between voting decisions that are to be expected (given a

[5] Similarly, in the case of Stratmann (1992), representatives trade votes in order to secure subventions to the specific agricultural sectors that matter most in their district (for example, dairy farming vs peanut farming).

[6] This can be done by computing the adjacency matrix $\mathbb{W} = \mathbb{V} \bullet \mathbb{P}'$, forcing a zero-diagonal in $\mathbb{W}$ and summing up reciprocal links $W^{\leftrightarrow} = \sum_i \sum_j w_{ij}^{\leftrightarrow}$ (where $w_{ij}^{\leftrightarrow} = \min[\mathbb{W}_{ij}, \mathbb{W}_{ji}] = w_{ji}^{\leftrightarrow}$ as in Squartini et al. 2013). $W^{\leftrightarrow} = 0$ would invalidate the roll-call data as a source to empirically assess vote trading (at least under the considered theory).

[7] Importantly, the previous contributions do not consider this condition but only focus on explaining individual voting decisions. Essentially, they filter $\mathbb{V}$ based on $\mathbb{X}$, and regress a subset of $\mathbb{V}$ on a subset of $\mathbb{X}$, whereby one coefficient (or a linear combination of several coefficients) indicates whether vote trading is likely the reason of some $(i,k) \in \mathbb{V}$. The line of argumentation behind such a regression-driven approach is that a systematic partial correlation between certain factors and some $(i,k) \in \mathbb{V}$, *ceteris paribus*, can only be attributed to vote trading. This, of course, does not return $\mathbb{V}_{trades}$ as an output, as explicitly linking specific legislators trading with each other is not considered. In contrast, the framework that we suggest builds precisely on the idea of linking specific legislators trading with each other and thereby complements the previous regression-based approaches.

legislator's party and other characteristics) and cases where a voting decision is a *deviation* from a legislator's expected voting behavior. Given a specific theory of vote trading, labelling such deviations can, in practice, be done based on observable data. In our applications, we show how this can be done by embedding the previous regression-based approaches (Stratmann, 1992, 1995; Cohen and Malloy, 2014) in our framework as a module to explain the individual voting decisions captured in $\mathbf{V}$. In a second step, we consider that such deviations may not be intended to help other legislators. Instead, they could result from mistakes or other factors associated to randomness. Let us elaborate on this distinction next.

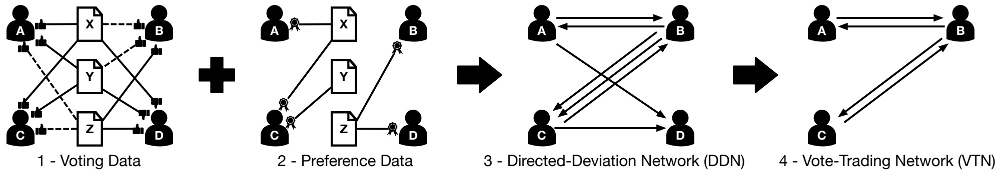
3.2 Directed deviations

Suppose we have constructed a matrix $\mathbb{D}$ indicating whether each entry $\mathbb{V}_{ik}$ is a deviation or not. That is, $\mathbb{D}_{ik} = 1$ if legislator i voted Yes in roll call k where they are expected to vote No (and $\mathbb{D}_{ik} = 0$ otherwise). In a first step, we relate each deviation to those legislators who have a strong preference for passing the corresponding bills. Following the idea of reciprocal patterns in voting records, we build such relations through a directed network of legislators where an edge indicates that the sending node deviated their vote in a bill where the receiving node has a noticeable interest in its passage. In other words, we say that the sender is the 'deviator' and the receiver is the 'beneficiary'. The network of deviators and beneficiaries is called the *directed-deviation network* (DDN). In a second step, we use the DDN to construct a logrolling index, based on the concept of reciprocity in directed weighted networks. If the index is higher than expected under the null hypothesis of no vote trading, we interpret it as a situation in which vote trading is prevalent.

Figure 1 presents a sketch of the method through three bills (X, Y, and Z) and four legislators (A, B, C, and D). From left to right, the first panel illustrates the structure of the roll-call data. For example, A voted Yes on all bills (thumbs up symbol). However, according to A's usual voting behavior, personal characteristics (e.g., party affiliation, age, gender, etc.) and constituency characteristics, they were expected to vote No in bill Z (thumbs down symbol). Hence, this vote is considered a deviation (dashed line). In the second panel, legislators have strong preferences toward specific bills (badge symbol). For example, A signals a strong preference for bill X, but not for Y. By combining the roll-calls with the preference data, we construct the DDN, shown in the third panel. In this step, we compute a logrolling index.

Finally, we extract the reciprocal part of the DDN in order to obtain the *vote-trading network* (VTN) in the fourth panel, which provides further information on the potential micro-structure of vote trading. In this illustration, A deviates in the bill preferred by D ($A \to D$). Since D is always consistent with their voting behavior, they do not reciprocate A's deviation. Hence, the resulting VTN only contains reciprocal deviations between A and B, and C with B.

Figure 1: Sketch of the methodology



Notes: 1: Legislators voting Yes in consistent (solid lines) and inconsistent (dashed lines) ways. 2: Legislators signalling strong revealed preferences in specific bills. 3: Directed deviation network (DDN) built from deviations and signals. 4: Reciprocal part of the DDN.

The VTN facilitates the granular study of vote trading as it results from discovering micro-patterns that fulfill our theoretical criteria. Without relying on prior information on potential logrolling, the VTN represents a scalable way to find voting patterns that are consistent with vote-trading activity. In addition, our logrolling index provides a normalized measure with which to compare the prevalence and evolution of vote trading across different settings (e.g., different legislative assemblies, different time frames, etc.).

3.3 Construction of a logrolling index

Recall that, in order to consider a vote as traded, it has to have a reciprocal deviation in some part of $\mathbb{V}$. Following this idea, we begin with the deviation and preference matrices $\mathbb{D}$ and $\mathbb{P}$. From them, we construct a weighted adjacency matrix

$$\mathbb{W} = \mathbb{D} \bullet \mathbb{P}', \tag{1}$$

containing counts of deviations between legislators. $\mathbb{W}$ corresponds to the adjacency matrix of the DDN, which we use to identify reciprocal deviations.[8] Based on the concept of reciprocity in directed weighted networks (Garlaschelli and Loffredo 2004, Squartini et al. 2013) we define the level of reciprocity between legislators i and j as

[8] Since we consider the case of each contested bill being most strongly preferred by one legislator and individual legislators trading with each other, it follows that $0 \leq \sum_i \mathbb{P}_{ik} \leq 1$ for every roll call k. In practice, where we have to rely on the observability of preference, this is quite a common situation. For example, we can consider the sponsorship of bill amendments as a revelation of strong preferences for a specific piece of legislation. If the preference matrix would contain more than one legislator mostly favoring a specific outcome of a roll call vote, then the dot product from equation 1 would draw multiple edges from a single deviation. In section 5, we propose a procedure in case it is not possible to discriminate multiple non-zero column entries in $\mathbb{P}$.

$$w_{ij}^{\leftrightarrow} = \min\left[\mathbb{W}_{ij}, \mathbb{W}_{ji}\right] = w_{ji}^{\leftrightarrow}. \tag{2}$$

Counting over all legislators, the proportion of reciprocity is

$$r = \frac{\sum_i \sum_j w_{ij}^{\leftrightarrow}}{\sum_i \sum_j \mathbb{W}_{ij}}, \tag{3}$$

which is the rate of reciprocal directed deviations to the total number of directed deviations (weights in the network).

To construct the logrolling index, we need to compute the reciprocity proportion r_0 that would be expected under a null hypothesis. Reciprocity under the null is necessary because we may obtain a high value for r simply because there are many revealed bill/policy preferences and numerous deviations in the data (independently of the legislators' intentions when deviating).

Our method is agnostic of the particular way in which a null model is implemented, as long as an ensemble $r_{0,1}, \ldots, r_{0,T}$ can be constructed in order to compute the expectation under the null as

$$\bar{r}_0 = \frac{1}{T} \sum_i^T r_{0,i}. \tag{4}$$

In our applications, we use a network randomization procedure that is consistent with the control structure used in the corresponding previous econometric studies. Finally, the logrolling index[9] is

$$\hat{\ell} = \frac{r - \bar{r}_0}{1 - \bar{r}_0}. \tag{5}$$

If ℓ is positive, it means that the DDN has more reciprocity than what is expected under the null; the higher the index, the more reciprocity. Systematically positive values of ℓ convey statistical evidence for vote trading that is consistent with our three theoretical pillars.

4 Index validation

Our validation strategy consists of generating synthetic roll call data in such way that we know which deviations are the result of vote trading. It follows that we can compute a 'true' logrolling index which can be compared against the

[9]This index construction is due to Garlaschelli and Loffredo (2004) and it offers several advantages over alternative specifications (Katz and Powell, 1955; Achuthan et al., 1982; Wang et al., 2013; Akoglu et al., 2012), for example, it accounts for the fact that complete anti-reciprocity is more significant in dense networks than in sparse ones (see Garlaschelli and Loffredo 2004 for a discussion on all advantages).

'empirical' index that we obtain from applying our method to the same synthetic data, but without knowing which deviations are actual trades. As we will show, under the simple but common case considered above ($0 \leq \sum_i \mathbb{P}_{ik} \leq 1$ for every roll call k), the proposed logrolling index corresponds to the 'true' one obtained from this process.

The synthetic data-generating process consists of two main steps: (1) producing directed deviations at random (not motivated by vote trading), and (2) inducing vote trades. Step 1 corresponds to the null hypothesis considered in the construction of the empirical logrolling index (the one used to obtain $\bar{r}_0$). Step 2 consists of sampling pairs of legislators who trade votes.

4.1 Synthetic trades and numerical results

First, we consider a deviation matrix $\mathbb{D}$ that is full of zeros, and a given preference matrix $\mathbb{P}$. Then, we generate random deviations unrelated to vote trading. From these deviations, we obtain a null DDN $\mathbb{A}^*$. Next, we determine an arbitrary number of trades and synthetically induce them into $\mathbb{D}$. This means turning entries from zeros to ones in $\mathbb{D}$ (in addition to the random deviations), and adding weights to the corresponding entries of $\mathbb{A}^*$. By the end of this procedure, we construct the adjacency matrix of the empirical DDN $\mathbb{A}$.

Showing that the empirical index corresponds to the true one boils down to showing $\mathbb{A} = \mathbb{A}^*$.[10] A sufficient condition for $\mathbb{A} = \mathbb{A}^*$ is $0 \leq \sum_i \mathbb{P}_{ik} \leq 1$ for every roll call k, which corresponds to the–above mentioned–common situation of having one strong supporter in each bill redundant. This sufficiency condition holds because, when computing the dot product of equation 1, having only one revealed preference per roll call guarantees that only one edge is drawn in the DDN. Failing to hold $0 \leq \sum_i \mathbb{P}_{ik} \leq 1$ could result in a distortion of the empirical index (in any direction). Next, we show the behavior of the index under different levels of vote trading.

Let us create a random binary matrix $\mathbb{P}$ that encodes the agents' revealed preferences for each bill. The density of $\mathbb{P}$ is determined by a parameter $\delta \in [0,1]$ that we can vary in order to affect the amount of vote trading to be induced. In other words, the probability of $\mathbb{P}_{ik} = 1$ is determined by δ. The only constraint when creating $\mathbb{P}$ is that each column contains no more than one non-zero element.

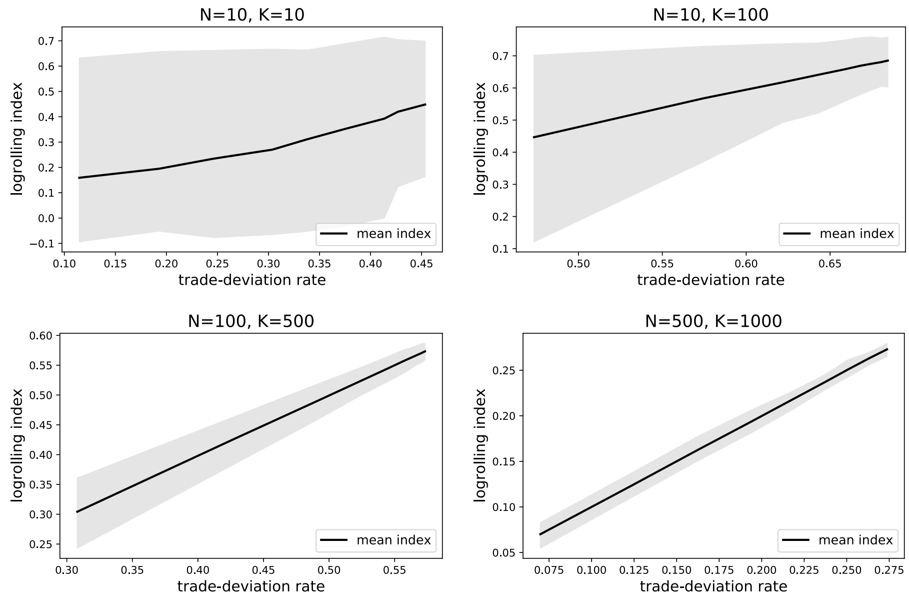
Next, we create a zero-matrix $\mathbb{D}$ which we use to record deviations. The first deviations that we input are those unrelated to vote trading. To introduce vote trading, we need to construct a list with all potential trades that would take

[10] Another source of discrepancy between the empirical and the true index could be the expected proportion of reciprocity from the null ensemble. However, since the null model is the same for the synthetic data and for the empirical index, these differences vanish for a large-enough sample of null networks.

place in the data. An entry in this list looks like $((i,k),(j,l))$, where legislator i deviates in k to benefit j who returns the favor by deviating in l. Once the list is constructed, we select a random sample of potential trades in order to induce vote trading. With each selection, we switch $\mathbb{D}_{ik}$ and $\mathbb{P}_{jl}$ from zero to one. A potential trade is realized only if $\mathbb{D}_{ik} = 0$ and $\mathbb{P}_{jl} = 0$, which means that these deviations have not already occurred.

Finally, we compute the logrolling index from these synthetic data. For a given parameter δ and a set amount of trades to induce, we generate 1000 Monte Carlo synthetic datasets and estimate their logrolling indices, as well as the rate between the number of true trades and the total number of deviations in $\mathbb{D}$, i.e. the trade-deviation rate. For different parameterizations of given dimensions N and K, we bin the resulting indices by the trade-deviation rate and calculate the 95% confidence interval of each bin. We present the results in Figure 2.

Figure 2: Logrolling index as a function of the trade-deviation rate



Notes: Each panel corresponds to data generated for matrices with dimensions $N \times K$. For each panel, we induce different amounts of vote trading by increasing δ and the number of potential trades to be realized. The resulting indices are binned by the trade-deviation rate. The black solid line shows the average index value. The shaded regions correspond to the 95% confidence intervals of the indices within each bin.

With increasing N and K, ℓ is more clearly distinguishable from an index of 0 (no vote trading). Further, we see that the higher the share of real trades over all deviations is, the higher the estimated ℓ is.

In the following section we show how our framework can be applied to two previously studied real-world settings in the context of vote trading in the US Congress. Thereby, we elaborate on how it complements previous econometric approaches by incorporating empirical models of individual voting decisions to get an estimate of $\mathbb{D}$. In addition, we show how our framework can be extended to deal with a situation where legislators may trade one vote with several other legislators simultaneously[11] and to study vote trading over long periods of time and different scales.

5 Application I: vote trading in the US Senate

Cohen and Malloy (2014) (CM, in what follows) study a setting that is theoretically favorable for vote trading: US Senators sharing personal ties (e.g., having studied at the same college/university) and their incentives to support economic policies that favor important industries in their states. CM present convincing evidence in line with this underlying theory, showing that senators tend to vote in favor of bills that particularly matter for colleagues with whom they have personal (school) ties. Yet, it remains unclear whether such favors are systematically reciprocated 'in the same currency' (votes), as their is no statistical assessment of reciprocity in favors.[12]

In this section, we illustrate how to investigate the vote-trading theory of CM by combining their empirical strategy and data with our method. Let us momentarily ignore the sub-hypothesis of the relevance of school-ties between senators for these trades.[13] Following CM's general theory, a senator i is expected to vote in favor of a bill k, explicitly benefiting the major industries in senator j's state, while being explicitly irrelevant for i's state major industries and vice versa for senator j in a roll-call l that benefits i's state industries. Thus, the theory implies that 1) there is a systematic reciprocal pattern in roll-call outcomes between senators for whom specific votes are relevant and 2) senators deviate when they vote Yes if the roll-call is irrelevant to their states' major industries; holding additional factors constant.

[11] This situation occurs if we code the policy preferences such that several legislators have the same most favored bill/policy proposal.

[12] While CM's results indicate that senators make vote favors in the context of bills that are very relevant to their states' major industries, the empirical approach cannot reject the null hypothesis that favors are always rewarded in a different currency (or even that they are not systematically rewarded at all). Using the notation introduced above, the test cannot distinguish the case of observing only $(i,k) \in \mathbb{V}$ due to other favors than vote trading (either inside or outside of politics) and observing $(i,k) \in \mathbb{V}$ *and* $(j,l) \in \mathbb{V}$ due to vote trading.

[13] Since the school-ties aspect of CM's theory is nested in the more general theory (senators with and without school ties exchange vote favors), we first focus on the general case and then show how our method can provide evidence for the specific sub-hypothesis of trading between senators with personal ties.

5.1 Deviations and null model

The implied theory, as well as the econometric model and data used by CM, can be easily integrated in our framework. The two ingredients to do so are a suitable definition of deviations and a null model.

Recall the matrix $\mathbb{D}$ that captures the deviations (voting decisions that cannot be directly explained by other factors). Now, suppose we are given the probability that senator i votes Yes in roll-call k. For all legislators and roll-calls, we can encode this information in a matrix $\mathbb{Q}$. Next, let a Yes vote with a corresponding $\mathbb{Q}_{ik} < 0.5$ be considered a deviation because senator i voted Yes in a bill k when they were expected to vote No. In addition, we can take into consideration additional factors that are relevant to consider a vote really a deviation based on the specific vote trading setting/theory under investigation. In the case of CM, this means we also should consider whether the vote outcome was particularly narrow[14] and explicitly irrelevant to the senator's state's major industries. Thus, we define the deviation matrix $\mathbb{D}$ such that $\mathbb{D}_{ik} = 1$ if $\mathbb{Q}_{ik} < 0.5$ *and* $\mathbb{V}_{ik} = 1$ *and* the vote outcome was very narrow *and* vote k was irrelevant for senator i's state's major industries (directly following the theory and data provided by CM), and $\mathbb{D}_{ik} = 0$ otherwise. That is, we count observed Yes votes as deviations if the *ceteris paribus* prediction of the voting decision was a No, the outcome of the vote was very narrow, and the specific setting/theory would suggest the senator would rather vote No in this roll call.[15]

Now, $\mathbb{Q}$ is the link between the econometric approach put forward by CM and our framework to quantify reciprocal patterns in vote favors. We take the econometric approach and data provided by CM (encoded as $\mathbb{V}$, $\mathbb{X}$ and $\mathbb{P}$) to compute the *ceteris paribus* propensities of each senator i to vote Yes in roll call k, which gives us $\mathbb{Q}$.

5.2 Data and empirical specification

The original data prepared by CM directly provides the basis to construct $\mathbb{V}$, $\mathbb{X}$, and $\mathbb{P}$.[16] It combines individual roll-call decisions of US Senators from 1989 to 2008 (101st to 110th congress) collected from the Library of the Congress' Thomas database with data on which bills are either explicitly relevant for a specific industry, irrelevant for specific industries, or not clearly assignable to

[14]As suggested by CM (and more generally in the literature on vote trading), when a vote is anticipated to be very narrow, getting an additional Yes vote from a colleague can just make the difference. Thus, vote trading would be particularly expected in contested votes.

[15]More generally, No votes can also be considered deviations. A senator might attempt to block the passage of a bill they are expected to be in favor of. However, in order to be consistent with CM, we only focus on situations where deals are made in order to pass bills.

[16]The data is provided via the journal's data repository: https://www.aeaweb.org/aej/pol/data/0603/2011-0174_data.zip.

either of the previous two categories.[17]

We select all observations for which the information of state/bill-specific industry relevance is available, resulting in a sample of 75,608 observations (individual voting decisions). Apart from the necessary roll-call and bill-preference data, the sample also contains the main control variables used in CM's baseline regression specifications (and indicators for narrow vote outcomes): *StateVote (share or sum)*, the share (sum) of senators from the same state as senator i voting Yes in roll-call k, and *PartyVote (share or sum)*, the share (sum) of senators with the same party affiliation as senator i voting Yes in roll-call k. Table 1 shows summary statistics of the main variables used in the following analysis.

Table 1: Roll-call voting on state industry issues: description of the main variables

	Mean	Std. dev.	Median	Min	Max	N
Vote Yes	0.79	0.4	1	0	1	75,608
StateVote (share)	0.77	0.42	1	0	1	75,608
PartyVote (share)	0.77	0.29	0.9	0	1	75,608
StateVote (sum)	0.77	0.42	1	0	1	75,608
PartyVote (sum)	37.96	15.28	43	0	56	75,608
Close outcome (+/- 3 votes)	0.03	0.16	0	0	1	75,608
Close outcome (+/- 5 votes)	0.04	0.19	0	0	1	75,608
Close outcome (+/- 7 votes)	0.12	0.32	0	0	1	75,608

Notes: This table reports summary statistics of the main variables in our sample of roll-call votes in the US Senate. The sample covers the 20-year period from the 101st to the 110th US Congress. *Sources:* All variables/observations are directly taken from the ready-made original dataset provided by Cohen and Malloy (2014).

We prepare this dataset in order to apply our method as follows. First, we generate the $N \times K$ matrix $\mathbb{V}$ based on the *VoteYes* variable. Thereby, $\mathbb{V}_{ik}$ is equal to 1 if senator i votes Yes in roll-call k and 0 otherwise. Based on the information on bill relevance to specific industries, we code the $N \times K$ matrix $\mathbb{P}$ in line with CM's underlying theory. $\mathbb{P}_{ik}$ is equal to 1 if a major industry in senator i's state is affected by the bill voted on in roll-call k. Finally, we estimate different specifications of CM's baseline linear probability model in order to compute the senators' propensities to vote Yes, captured in the $N \times K$ matrix $\mathbb{Q}$, where $\mathbb{Q}_{ik}$ is senator i's propensity to vote Yes in roll-call k.

We estimate $\mathbb{Q}$ by taking exactly the same potential alternative explanations

[17]The data is prepared in such way that rows reflect an individual senator's voting decision in a given roll-call and columns describe senator and vote characteristics. The records directly show an individual senator i's voting decision as well as whether this decision was taken in a narrow–and for i's state explicitly 'relevant' or 'irrelevant'–roll-call (relevant/irrelevant in the sense that the major industries in senator i's state are clearly affected or not by the bill).

for voting decisions into account as CM, by controlling for *PartyVote* and *StateVote*, as well as different fixed-effect indicator combinations.[18] In all specifications we account for unobservable time-invariant senator characteristics and cluster the standard errors at the representative level. Table 2 presents the intermediary results underlying our estimation of $\mathbb{Q}$.

Table 2: Voting on state industry issues in the US Senate

Dependent variable: Vote Yes	(1)	(2)	(3)	(4)
StateVote (share)	0.122	0.125		0.122
	(0.003)	(0.003)		(0.003)
PartyVote (share)	0.934	0.955		0.933
	(0.004)	(0.004)		(0.004)
StateVote (sum)			0.134	
			(0.003)	
PartyVote (sum)			0.017	
			(0.000)	
Senator FE	Yes	Yes	Yes	Yes
Congress FE	Yes	No	No	No
Congress-session FE	No	No	No	Yes
Congress-session-vote FE	No	Yes	Yes	No
Adjusted R^2	0.576	0.576	0.560	0.576
Area under the curve (AUC)	0.940	0.942	0.938	0.940
Share of false negatives (deviations)	0.021	0.021	0.021	0.021
No. of observations	75,608	75,608	75,608	75,608

Notes: This table shows the estimated regression coefficients of different specifications of our baseline linear probability model. The dependent variable is an indicator equal to 1 if a senator voted Yes. Standard errors clustered at the senator level are shown in parentheses.

The estimated regression coefficients are qualitatively identical to CM's results, indicating that both *StateVote* and *PartyVote* are important predictors of the representatives' usual voting behavior. In line with their main results, different specifications regarding the granularity of time-fixed effects (congress, session, or vote-specific) change the overall explanatory power of the model only marginally, as the adjusted R^2s suggest. Similarly, the accuracy of the predictions of Yes/No votes, computed as the area under the ROC curve (AUC), does not vary substantially between the different specifications. The same holds for the share of false negatives (deviations). As the relevant characteristics of the different model specifications do not vary substantially, we proceed with specification 1 to compute $\mathbb{Q}$ and derive $\mathbb{D}$ as outlined above (we show that our main results are qualitatively the same when choosing another specification in the Online Appendix, section A.3). Conceptually, the predicted propensities to

[18]We do not include their main explanatory variable of interest, school-connected votes, since our method captures these in the VTN.

vote Yes captured in $\mathbb{Q}$, imply that what we further work with when generating $\mathbb{D}$ based on $\mathbb{Q}$ and then generate the DDN, is based on the residuals (the part of the voting decisions that cannot be explained by observables). Intuitively, one way to think about the next steps is to consider them as an approach to find systematic pattern in the residuals that are in line with a specific theory of vote trading. In this context, there is also an interesting link between our approach and the approach suggested in the seminal contribution by Stratmann (1992), who shows correlations between residuals from two voting regressions to support the finding that some votes have been traded. In our case this goes, of course, well beyond correlations at the level of roll calls.

For the null model, we construct random networks by generating alternative deviation matrices $\mathbb{D}$ from Bernoulli trials of the observed deviations, each one with a probability of success determined by $\mathbb{Q}$. In addition, to account for the stochasticity from the errors of $\mathbb{Q}$, we generate random probabilities of success in the interval $[\mathbb{Q}_{ik} - \mathbb{E}_{ik}, \mathbb{Q}_{ik} + \mathbb{E}_{ik}]$, where $\mathbb{E}_{ik}$ is the standard error of the estimated probability of a Yes vote by legislator i in roll call k.[19]

5.3 Results

Figure 3 presents the distribution of the logrolling index (grey histogram).[20] As shown by the plot, vote trading is prevalent in the context under study, as the distribution never crosses the zero-threshold. The blue histogram presents an estimation of the index that forces the condition $0 \leq \sum_i \mathbb{P}_{ik} \leq 1$ for every roll call k, assuming that, even if a bill has multiple strong supporters, one deviation can only benefit one of them (i.e., senators do not 'recycle' a deviation to trade it for favors from more than one colleagues).[21] Even in this more conser-

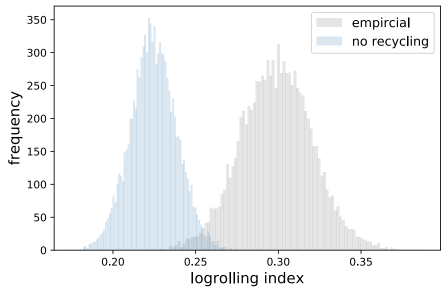
[19]Based on the estimation of the regression model used to construct $\mathbb{Q}$, we compute for each roll-call k $\mathbb{E}_{*k} = \sqrt{\text{diag}(\mathbb{X}\mathbb{V}_\beta\mathbb{X}^t)}$, where $\mathbb{X}$ is the model matrix of roll-call k, and $\mathbb{V}_\beta$ is the estimated variance-covariance matrix of the estimated regression coefficients.

[20]See appendix A.2 for details on how to compute confidence intervals.

[21]In this particular application, multiple senators may exhibit strong preferences towards the same bill, which may distort the index. In order to address this problem, we synthetically generate vote trading dynamics that generate the empirical deviation matrix $\mathbb{D}$. For this, we use the numerical approach introduced in section 4.1, but constraining the potential deviations to the subset of realized deviations in the empirical data. Once the random deviations are generated, we induce vote trading by exhausting the entire list of potential trades (but making sure that no senator repeats a deviation). This means that, while the dot product from equation 1 may draw edges from one deviation to multiple beneficiaries, we are keeping track of which one of the new edges should be counted as a 'real' trade. In this way, once we have completed the procedure and replicated the empirical deviation matrix, we can compute the logrolling index of the trades when senators can only make favors to one (and not several) senators when deviation in a vote. We repeat this multiple times, randomizing the order in which we pick potential trades, in order to obtain the distribution of viable indices, which we show in Figure 3 through the blue shaded region. Appendix A.3 present results for a more relaxed

vative specification, the index remains positive and significant. Consistent with the generalized form of vote trading suggested by CM, we find a systematic reciprocal pattern in voting favors.

Figure 3: Estimated and corrected logrolling index from the CM data



Notes: The grey distribution corresponds to the estimated logrolling index when allowing the same votes to be traded with several senators ('recycling of favors'). The blue distribution denotes the index obtained by enforcing $0 \leq \sum_i \mathbb{P}_{ik} \leq 1$ for every roll call k. Each distributions was obtained from 10000 independent estimations.

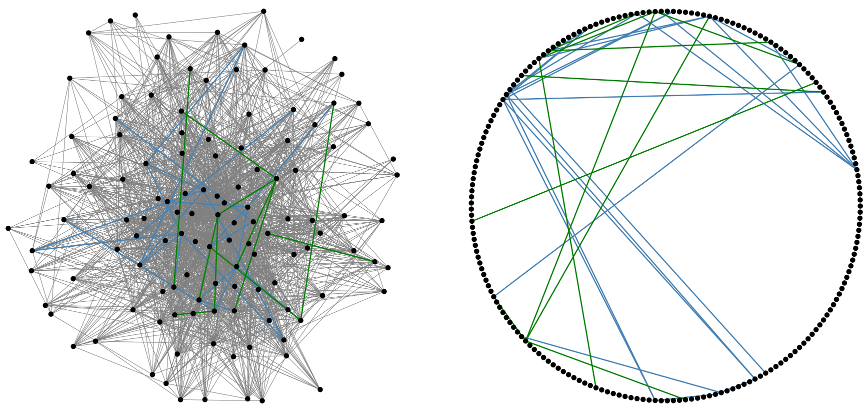
These results constitute novel evidence consistent with the theory that senators trade votes on bills that are highly relevant for some states' major industries. Given the statistically significant ℓ, we can now extract the VTN and analyze which legislators were likely particularly involved in vote trading of this sort. Thereby, we can specifically test CM's main hypothesis: that vote trading between senators is driven by their personal ties with each other, particularly senators belonging to the same alumni networks.

The extracted VTN is presented in Figure 4. Nodes represent senators and edges correspond to reciprocal deviations between them. Potential trades between senators belonging to the same alumni networks are indicated as follows: blue edges denote reciprocal deviations between legislators who went to the same school; green ones correspond to reciprocation between senators who earned the degree from the same school. In total, there are 1566 dyads of senators who potentially traded votes. Approximately 62% were part of one or more dyads. From these reciprocal dyads, only 2% involve senators who went to the same school, while 0.7% correspond to those who went to the same school and degree. In terms of deviations, 4319 individual voting decisions are part of the VTN. From these, 2% are potential trades between senators who went to the same school (same or different degree), and 0.89% involves legislators who went to the same school and degree.

A first look at the descriptive evidence would suggest that school ties are unlikely major drivers of the detected prevalence of vote trading between sena-

implementation where some level of recycling is allowed. Even then, our findings hold.

Figure 4: VTN in the U.S. Senate (CM data)



Notes: Each edge corresponds to a set of reciprocal deviations. Blue edges highlight reciprocal deviations between legislators who went to the same school. Green ones correspond to reciprocation between senators who went to the same school and degree. The width of the edge denotes the amount of reciprocity. Left panel: VTN including non-school ties. Right panel: VTN of school ties only (circle layout).

tors. However, since the VTN makes pairwise reciprocal deviations visible, we can directly test the sub-hypothesis of the relevance of having school-ties vs not having them. Broadly speaking, the CM thesis postulates that personal connections increase the likelihood of trading between two senators. In our framework, this means that one should expect more reciprocal deviations among dyads with a personal connection than in dyads without it. For instance, the average number of reciprocal deviations between dyads without observed school ties is 1.4. When looking at dyads with a personal tie (pooling school-only and school-degree connections), this number goes up to 1.56. Then, if we isolate those dyads with a school-degree tie, we obtain 1.65.

These results are consistent with the sub-hypothesis. First, school dyads seem to reciprocate more. Furthermore, this association becomes stronger among school-degree dyads, as suggested by CM's work. In order to formally assess these claims, we propose a simple statistical test. We separate the dyadic population into four groups: no tie (NT), school ties (ST, regardless if they obtained the same degree or not), school-only ties (SOT, same school but different degree) and school-degree ties (SDT). In each group, the unit of observation is the dyad, and the statistic of interest is $w_{ij}^{\leftrightarrow}$. Hence, we test CM's school-ties hypothesis via the Mann-Whitney U test[22] by breaking it down into four hy-

[22]In this non-parametric test, we seek to reject the null hypothesis that the probability of observing more reciprocity in a group A of dyads than in a group B is lower or equal than the probability more reciprocity in B than in A. The test requires independence between groups,

potheses:

1. $ST > NT$: Senators with school ties reciprocate more deviations than those without any school ties.
2. $SOT > NT$: Senators with school-only ties reciprocate more deviations than those without ties.
3. $SDT > NT$: Senators with school-degree ties reciprocate more deviations than those without ties.
4. $SDT > SOT$: Senators with school-degree ties reciprocate more deviations than those with school-only ties.

In all the previous hypotheses, we seek to reject the 'less-than' null. Table 3 shows the results of these tests. We find some evidence for the school-ties hypothesis. The first test (column I) indicates that senators with school ties are generally more likely to engage in vote trading than senators without any observed school ties. However, these results seem to be predominantly driven by senators with school-degree ties. While senators with SOT do not seem to be systematically more involved in vote trading in this setting (column II), senators with SDT tend to be systematically more involved in vote trading than senators with no ties (column III). When only focusing on the latter two groups, we cannot reject the less-than null (column IV). Given the relatively few SDT and SOT dyads, the statistical power of this last test is rather limited.

Table 3: Mann–Whitney U test for sub-hypothesis of the relevance of school-ties

	(I)	(II)	(III)	(IV)
Hypothesis	$ST > NT$	$SOT > NT$	$SDT > NT$	$SDT > SOT$
MW Statistic	98164.5	56668.0	41496.5	426.0
p-value	0.03	0.16	0.04	0.26
means	1.50 & 1.37	1.29 & 1.37	1.65 & 1.37	1.65 & 1.29
n	34 & 3092	17 & 3092	23 & 3092	23 & 17

Notes: The information on means and sample size (n) has been arranged in pairs, corresponding to the respective groups formulated in the hypotheses of the second row of the table. The data on reciprocal deviations do not exhibit normal-distributed behavior. Therefore, the Mann–Whitney U test is preferred over the t-test.

Taken together, our findings suggest that senators indeed exchange favors in the form of vote trading when a bill is particularly relevant for their state's major industries but irrelevant to the major industries in some of their colleagues'

which aligns with our assumption of pairwise vote trading rather than in coalitions.

states. Moreover, our findings intuitively confirm the results of CM to the extent that school-degree ties seem to facilitate these kind of deals but not necessarily school-only ties. Interestingly, when looking at the overall VTN, the school dyads are not the only connections that seem to play a role in the overall reciprocity in vote favors between senators in the context of industrial policies. There are 3092 edges that cannot be explained by senators knowing each other personally via alumni organizations, suggesting that there are various, so far unstudied, factors that potentially facilitate the exchange of favors in the form of vote trading in this setting.

6 Application II: trading votes on bill amendments in the US House

The seminal contribution by Stratmann (1992) (ST in what follows) emphasises specific votes on five amendments to the Food Security Act of 1985 (i.e., the so-called 'Farm Bill' of 1985; H.R. 2100) in the first session of the 99th US House. ST relies on rather specific prior information on which representatives have likely traded votes on which amendments.[23] While this strategy can be effective for a small number of votes, it is not possible to scale it to a multitude of roll-call votes and policy areas.[24] Since the cross-sectional setting analyzed in ST only consists of a handful of votes, it cannot be directly exploited by our method. However, we can generalize the underlying idea of vote trading put forward by ST. The form of vote trading in the context of bill amendments outlined in ST suggests that representatives with close ties to special interests exchange favors to modify a bill in order to make it more attractive for all special interests involved in the trade. More specifically, the empirical setting in ST focuses on a type of legislative decision that is periodically reoccurring (decisions on agricultural subventions). Given that many bills are not open for amendments in the US House of Representatives, trades between representatives strongly favoring certain amendments (captured by amendment sponsorship) might thus rather be scarce but periodically reoccurring.[25] Thus, vote trading might be particularly prevalent through intermittent (relatively short) periods.

[23] Stratmann (1992) bases his sample on reports about specific vote-trading coalitions in the 1985 *Congressional Quarterly Almanac*.

[24] This is pointed out by Stratmann (1992, p. 1164): "*To test for the presence of logrolling using [this method], one must be able to identify the particular issues on which trading takes place. [...] Thousands of votes are taken during a session of Congress, many of which involve no logrolling. Moreover, the potential patterns of trades are limitless.*"

[25] The majority leader/Speaker of the House essentially controls whether and how amendments are possible when a bill is debated on the floor (via the Rules Committee). Often, the procedural rules are set such that a bill cannot be amended.

In this application we adapt the underlying idea in ST–of representatives trading votes on bill amendments–and demonstrate how our method can be used to filter several years of roll call data to find periods with voting patterns that are consistent with the specific theory of vote trading under consideration.

6.1 Data and specification

We collect data on amendment sponsorships by individual representatives as well as the corresponding roll-call records. We construct the preference matrix $\mathbb{P}$ and the roll-call matrix $\mathbb{V}$. The roll-call data is obtained from the Office of the Clerk of the U.S. House of Representatives (http://clerk.house.gov). Data on amendment sponsorships is collected from the official bill data published by the Library of Congress (www.congress.gov, previously www.thomas.gov). In order to create matrices $\mathbb{V}$ and $\mathbb{P}$, we harmonize the unique identifiers for bills, amendments, votes, and congressmen from both sources. This allows us to link each representative's roll-call decision in a vote on an amendment to the representative sponsoring this particular amendment. We select a 10-year observation period (97th and the 101th congress; 1981 to 1990) around the few votes analyzed by ST (votes on the amendments to the 1985 'Farm Bill'). Moreover we restrict the sample to roll-call outcomes in line with the narrowness criterion applied in ST ($\pm 30\%$). The final dataset consists of roughly 500,000 individual voting decisions in roll-calls on bill amendments in the US House. Table A2 in the Online Appendix presents summary statistics of the key variables in our dataset.

Altogether, our data contain the handful of votes analyzed in ST as well as diverse similar cases of votes on bill amendments taking place over the five years prior and posterior to the the 1985 'Farm Bill'.[26] Since the model used in ST is specified for the cross-sectional setting of the 1985 Farm Bill, we cannot directly employ it to compute $\mathbb{Q}$ for our large sample. Instead, we use a similar approach as in our first application. That is, we model Yes votes as a function of StateVote and PartyVote and include legislator fixed-effects and different time-fixed-effect specifications. Table 4 shows the results of the respective regression estimations. As in the previous application, a large share of the Yes votes is accurately predicted by these models and, again, the differences between the three alternative specifications with regard to accuracy, predictive power, as well as the resulting share of deviations are minimal. We select specification 3 to compute $\mathbb{Q}$ and show that our main results are robust to selecting one of the other specifications (in the Appendix A.3).

In order to provide more nuanced results, we look at vote trading under dif-

[26] Table A1 in the Appendix shows the number of bills, amendments, and roll-calls in our overall dataset vis-à-vis the roll-calls/amendments related to the one bill analyzed in ST.

Table 4: Voting on bill amendments in the US House of Representatives

Dependent variable: Vote Yes	(1)	(2)	(3)
StateVote (share)	0.702	0.701	0.701
	(0.002)	(0.002)	(0.002)
PartyVote (share)	0.803	0.803	0.803
	(0.002)	(0.002)	(0.002)
Representative FE	Yes	Yes	Yes
Congress FE	No	Yes	No
Congress-session FE	Yes	No	No
Adjusted R^2	0.5	0.5	0.5
Area under the curve (AUC)	0.914	0.914	0.914
Share of false negatives (deviations)	0.09	0.09	0.09
No. of observations	504,936	504,936	504,936

Notes: This table shows the estimated regression coefficients of different specifications of our baseline linear probability model, explaining representative's voting decisions on bill amendments. The dependent variable is an indicator equal to 1 if a representative voted Yes. Standard errors are clustered at the representative level are shown in parentheses.

ferent temporal scales. That is, we define a temporal window and roll it over the roll-call dataset in order to partition it by sequential periods sub-periods. We estimate the index for each sub-period, and at different temporal scales defined by the window size. For instance, the smallest partition is 2 because logrolling needs at least two votes to be traded, while the largest would be the entire sampling period. In this application, we focus on smaller window sizes (of 2 to 40 roll calls) since this is consistent with ST's assertion of intermittent logrolling activity in bill amendments. Furthermore, we detect vote trading sub-periods that match ST's sample.

6.2 Results

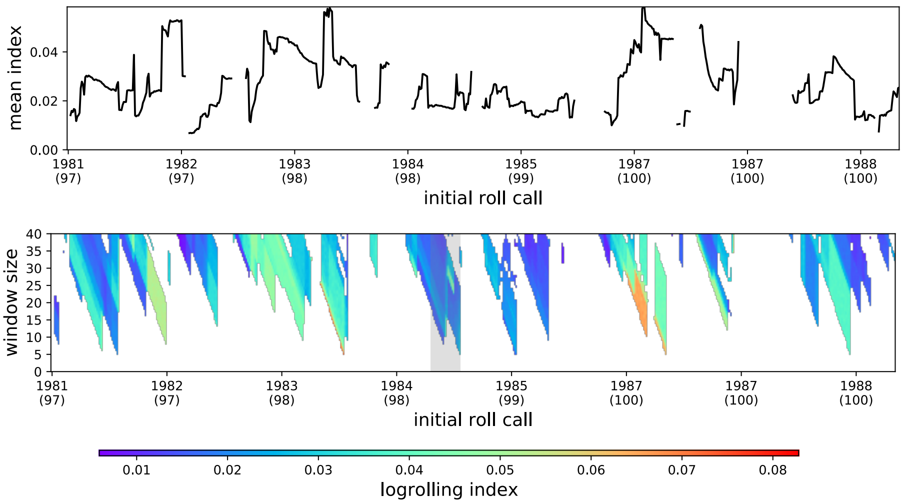
Figure 5 shows the logrolling index across different time windows. First, the pattern shows intermittency at lower temporal scales, consistent with the idea that, in ST's context, logrolling in the context of bill amendments is only prevalent only during specific periods. Second, the shaded area highlights the roll-calls where the votes analyzed in ST took place. Thus, our findings suggest that, during the specific period analyzed in ST, logrolling was prevalent.[27]

Note that this result is entirely based on the detection of systematic reciprocity in directed deviations in various roll-call votes on bill amendments.[28]

[27] Appendix A.3 shows that these results are robust across the three specifications shown in Table 4.

[28] No additional information, such as the bill topic (agriculture related or not) has been taken into consideration.

Figure 5: Discovered DDNs from vote trading in amendments



Notes: Top panel: average positive logrolling index across 49 different time windows. The *x*-axis indicates the initial period of the time window. Bottom panel: positive and significant (at 95%) logrolling indices across the different combinations of initial periods and time windows. The shaded area indicates the period in which the votes analyzed by ST fall.

Moreover, our procedure suggests that there are other periods with a similar vote trading prevalence in the setting of votes on bill amendments as well as periods without any indication of systematic reciprocity in directed deviations.

In a second step, we investigate whether the votes captured in the VTNs over the entire time-frame tend to be related to special interest politics (both in general as well as specifically for the case of agriculture), as the underlying theory in ST would suggest. In order to do so, we extend our roll-call dataset with the policy issue categories developed by Peltzman (1984) and the issue categorization by Clausen (1973). Combining information from both vote-level policy issue codes, we can distinguish four types of roll-calls relevant for the ST hypothesis: votes on issues explicitly related to special interests (*Special Interest*), votes on issues explicitly related to special interests in the domain of agriculture (*Special Interest: Agriculture*; nested in *Special Interest*), votes on issues explicitly related to general interests/the public (*General Interest*), and all votes not explicitly related to special interests (*Other*; nests *General Interest*).[29]

[29] Peltzman (1984) categorizes the policy issues decided in roll-call votes into 13 categories, among them ‘Special Interest Budget’ and ‘Special Interest Regulation’ (including decisions on coal mine regulations, export/import controls, subsidies, etc.) as well as ‘General Interest Budget’ and ‘General Interest Regulation’ (including decisions on debt limits, budget targets, minimum wages, etc.). Clausen (1973) distinguishes five policy issue categories, among them ‘Agriculture’ (including decisions on price supports and subsidies, commodity control, and acreage limitations). Our *Special Interest* and *General Interest* indicators are the combination

We then use chi-squared tests of independence to assess whether the votes captured in the VTNs are systematically more likely to fall into the special interest categories outlined above in comparison to those votes not occurring in the VTNs. Table 5 shows the results.

Table 5: Frequency counts and χ^2-tests of special interest votes in VTNs

	Not in VTN	In VTN	χ^2
Special Interest	240	68	6.507
Other	292	49	(0.017)
Special Interest	240	68	4.172
General Interest	35	3	(0.06)
Special Interest: Agriculture	18	5	0.222
Other	514	112	(0.77)

Notes: This table shows frequency counts of votes on special interest issues vs non-special interest issues (following Peltzman 1984 and Clausen 1973), occurring or not occurring in the discovered VTNs underlying Figure 5. The right-most column presents chi-squared statistics of a test of independence based on the corresponding frequency counts (p-values based on 2,000 bootstrap trials shown in parentheses).

The results indicate an over-proportional representation of special interest related votes captured in the VTNs (when comparing special interest related issues with all other types of votes). The same picture holds for a comparison with votes explicitly related to general interest policies. However, there does not seem to be a particularly over-proportional number of votes related to special interest politics in the domain of agriculture captured in the VTN. In sum, our findings are consistent with the generalization of ST's theory that representatives engage in vote trading when voting on bill amendments related to special interest politics, but cannot confirm whether these types of trades particularly occur in the context of agricultural policies.

7 Discussion and conclusions

While broadly covered in the theoretical literature, only little is known about the prevalence, variability, and underlying mechanisms of legislative vote trading in the real world. One key challenge for empirical research on the topic is

of 'Special Interest Budget' and 'Special Interest Regulation', and 'General Interest Budget' and 'General Interest Regulation', respectively. All votes that occur in our *Special Interest* indicator and occur in Clausen's 'Agriculture' category, we count as *Special Interest: Agriculture*. All data on the issue categories are provided by `voteview.com`.

the hidden nature of vote trading. Our approach can be used to find reciprocal voting patterns consistent with specific theories of vote trading, integrating, and complementing previously suggested regression-based approaches. The method presented might therefore allow a broader empirical assessment and understanding of this form of hidden cooperation in politics.

Following the core assumptions about the incentives to trade votes underlying previous empirical contributions (Stratmann, 1992, 1995; Cohen and Malloy, 2014), our approach builds on three theoretical pillars, characterizing the minimal requirements to consider an observed vote as traded. These pillars are integrated at the heart of the empirical approach and allow to combine our framework with the traditional econometric approaches. By building on the concept of reciprocity in directed, weighted networks, we can incorporate the micro-structure of reciprocal deviations between individual members of a legislature into a measure for the prevalence of vote trading in roll-call data. As the suggested logrolling index is built from bottom up, it is straightforward to explore how the statistical evidence for vote trading likely has emerged based on the underlying individual voting behavior.

Our framework, like the previous ones, has important limitations. It is critical to keep in mind that there might be other forms of vote trading that are not captured by our approach. Two aspects are of particular relevance: first, regarding the two applications of our method, amendment-sponsorships and favored state-specific industrial policies are, of course, not the only perceivable indication of strong preferences toward bills. As in the previous contributions on which we build these applications, alternative observable indications for policy preferences might additionally reveal tendencies to trade votes. Second, it might well be that some deals are arranged between groups. In those cases, our method would likely capture the roll-calls/bills affected by such trades, but not each individual participating in the deal.

Nevertheless, the proposed framework can serve as a valuable tool for future research on vote trading. The flexibility and scalability of the framework can help in the discovery and study of vote trading at various levels of government, across time, as well as across different jurisdictions and institutional settings.

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Appendix

A.1 Data appendix

This section presents additional details on the dataset used in Application II (section 6 in the main text). Table A1 shows the number of bills, amendments, and roll-calls in our overall dataset vis-à-vis the roll-calls/amendments related to the one bill analyzed in Stratmann (1992). Table A2 presents summary statistics of the key variables in the dataset on which application II is based.

Table A1: Number of bills, amendments and votes analyzed

	All	1985 'Farm Bill' (ST)
No. of bills	402	1
No. of amendments	973	9
No. of roll-calls	1245	14

Table A2: Roll-calls on bill amendments in the US House: description of the main variables

	Mean	Std. dev.	Median	Min	Max	N
Vote Yes	0.46	0.5	0	0	1	504,936
StateVote (share)	0.46	0.26	0.44	0	1	504,936
PartyVote (share)	0.46	0.3	0.44	0	1	504,936
Yes vote margin	-0.04	0.13	-0.05	-0.3	0.3	504,936

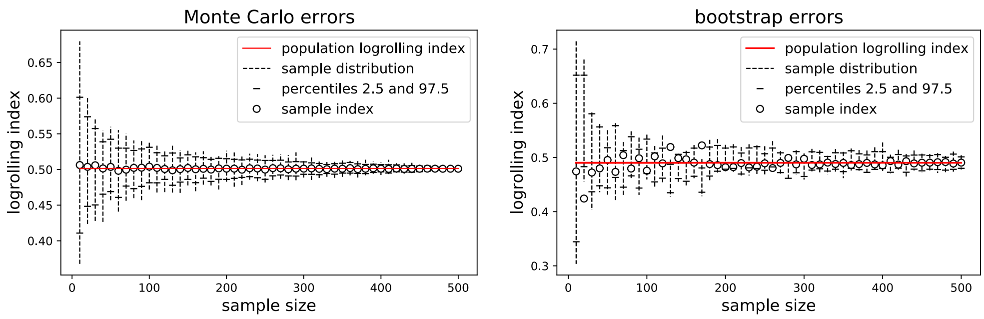
Notes: This table reports summary statistics for the main variables in our sample of amendment-related roll-call votes in the US House. The sample covers the 10-year period from the 97th to the 101th US Congress. *Data source:* Office of the Clerk of the U.S. House of Representatives and Library of Congress.

A.2 Confidence intervals

We employ a bootstrapping procedure to compute the confidence intervals of the logrolling index estimated from empirical roll call data. The procedure consists of resampling (with replacement) the population of legislators, i.e., the rows of $\mathbb{V}$, $\mathbb{P}$, $\mathbb{Q}$ and $\mathbb{E}$. The confidence intervals are then defined as follows: (i) compute the distribution of Δ_i^* by taking the difference $\Delta_i^* = \ell_i^* - \ell$ between each logrolling index from the bootstrap resampling, ℓ_i^*, and the ℓ observed in the data; (ii) sort Δ_i^* in ascending order and define the 95% empirical CIs as $\left[\ell - \Delta_{.025}^*, \ell - \Delta_{0.975}^*\right]$.

The intuition behind this procedure is to account for the uncertainty of having different representations in the legislature under study. Figure A1 compares the bootstrap CIs agains the Monte Carlo CIs for synthetic data. Clearly, the intervals converge to the population ℓ as we increase the sample size. In the worst case scenario, the bootstrap intervals would be wider than the Monte Carlo, which would mean a more conservative estimation of the logrolling index if the hypothesis is $\ell > 0$.

Figure A1: Logrolling index under variable cooperation



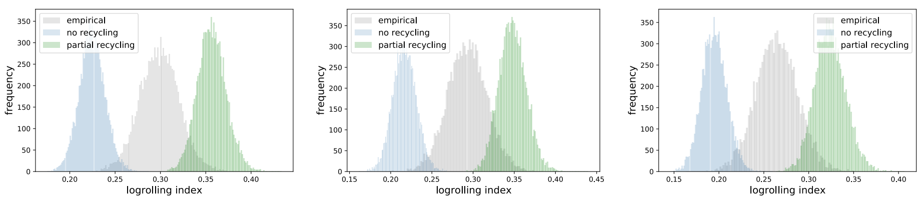
Notes: Left panel: Monte Carlo confidence intervals (i.e., the actual CIs). Right panel: estimated (bootstrap) confidence intervals.

A.3 Robustness

In this section, we demonstrate that the main findings shown in applications I and II (sections 5 and 6 in the main text) are qualitatively robust when applying alternative model specifications to estimate the propensities in $\mathbb{Q}$.

Figure A2 shows the results for the first application (CM – US Senate). For each of the alternative regression specifications presented in Table 2, we compute the respective $\mathbb{Q}$ and $\mathbb{E}$ and the corresponding logrolling indices. We do this by taking only roll-calls with narrow vote outcomes into consideration and once taking non-narrow vote outcomes into consideration. In addition to the no-recycling approximation (blue histograms), we perform one where we allow certain level of recyclability of each deviation. In other words, when inducing a trade in the simulation, we allow for that deviation to be re-used to favor an additional beneficiary. The maximum level of recyclability is established at random for each deviation (recall that the empirical estimation in the CM context would be equivalent to full recyclability). Even with this additional exercise, the logrolling index is positive and significant.

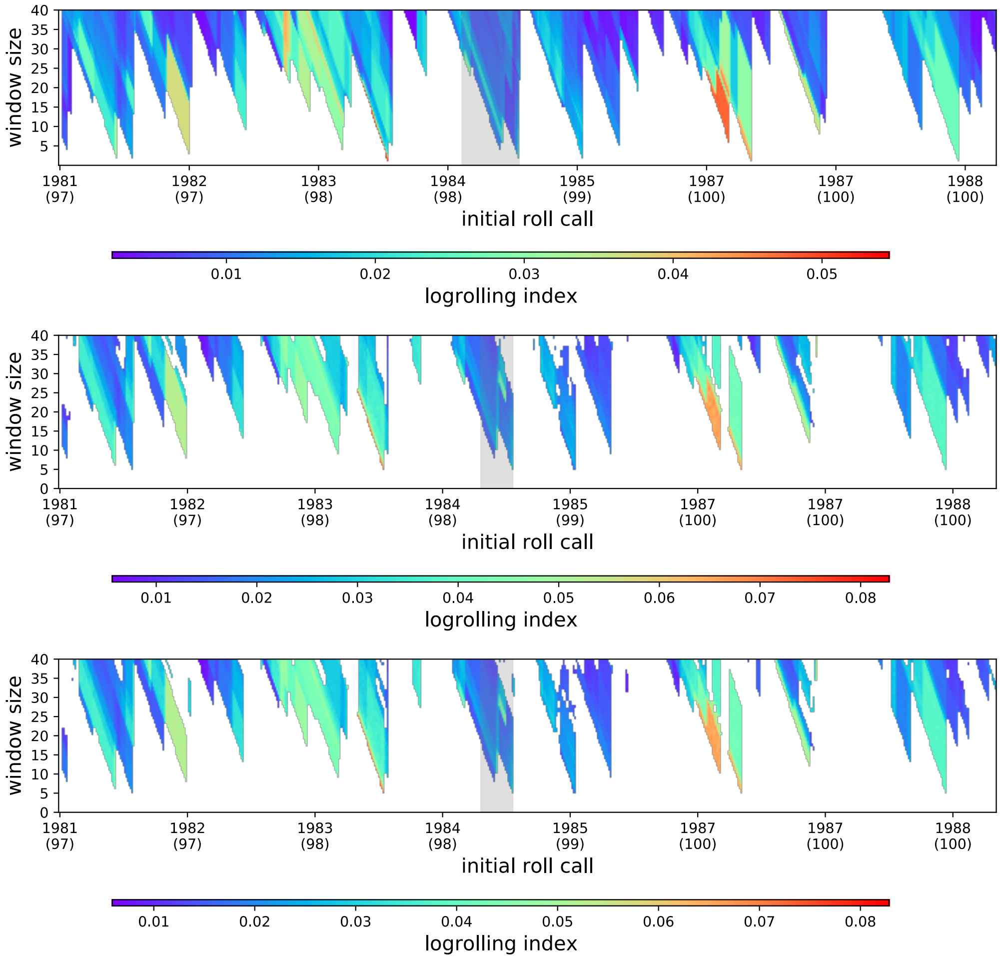
Figure A2: Logrolling index under broad and narrow margins for different model specifications of CM



*Notes:*Left panel: specification 1. Middle panel: specification 2. Third panel: specification 3.

In a similar vein, we repeat the procedure presented in section 6 for each of the regression models shown in Table 4. Again, the findings shown in the main text are robust to these alternative specifications. In each of the specifications, we clearly find a tendency to trade votes exactly in the time-frame of the votes investigated by Stratmann (1992). Moreover, in each specification we find additional time-frames with a prevalence of vote trading in the data. While not identical, these episodes are very similar across all specifications.

Figure A3: Robustness of the discovered DDNs from vote trading in amendments



*Notes:*Positive logrolling indices across the different combinations of initial periods and time windows. Each panel corresponds to one of Stratmann's specifications (1 to 3 from top to bottom).