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Author
Rode, Ashwin

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Essays in Environmental and Natural Resource Economics and Econometrics

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in

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by

Ashwin Anil Rode

Committee in Charge:

Professor Robert Deacon, Co-Chair

Professor Douglas Steigerwald, Co-Chair

Professor Olivier Deschênes

Professor Paulina Oliva

June 2014

The Dissertation of
Ashwin Anil Rode is approved:

Professor Olivier Deschênes

Professor Paulina Oliva

Professor Robert Deacon, Committee Co-Chairperson

Professor Douglas Steigerwald, Committee Co-Chairperson

June 2014

Essays in Environmental and Natural Resource Economics and Econometrics

by

Ashwin Anil Rode

Curriculum Vitæ
Ashwin Anil Rode

EDUCATION

Expected June 2014 Ph.D., Economics, University of California, Santa Barbara.

Primary Fields: Environmental and Natural Resource Economics, Econometrics.

PhD Supervision: Robert Deacon (Co-Chair), Douglas Steigerwald (Co-Chair), Olivier Deschênes, Paulina Oliva.

2010 M.A., Economics, University of California, Santa Barbara.

2008 M.S., Economics, University of Texas at Austin.

2007 B.A. (honors), Economics with minor in Mathematics, University of Chicago.

WORKING PAPERS

Rode, A. "Rent-seeking over Tradable Emission Permits: Theory and Evidence."

Rode, A. "Institutions and the Resource Curse within the United States."

Rode, A. and Steigerwald, D.G. "Estimating Treatment Effects with Interference."

Rode, A. and Shukla, A.J. "Prejudicial Attitudes and Labor Market Outcomes."

PUBLICATIONS

Deacon, R.T. and Rode A. "Rent Seeking and the Resource Curse." forthcoming in *Companion to Rent-Seeking* (eds. Roger Congleton and Arye Hillman).

"Fuel Prices." in *The Economic Future of Nuclear Power: A Study Conducted at the University of Chicago*, 2004 (prepared for the US Department of Energy).

PRESENTATIONS

2014	Oberlin College Department of Economics, Oberlin OH.
2014	Charles River Associates, Washington DC.
2014	Ohio University Department of Economics, Athens OH.
2013	Western Economic Association Annual Conference, Seattle WA.
2013	Association of Environmental and Resource Economists Summer Conference, Banff AB.
2012	Camp Resources, Wrightsville Beach NC.

PROFESSIONAL EXPERIENCE

2011-present	Member, UCSB Econometrics Research Group.
2012	Research Assistant to Professor Douglas Steigerwald.
2012	Participant, UCE3 Summer School in Environmental and Energy Economics.
2012	Discussant, Occasional Workshop on Environmental and Resource Economics, UCSB.
2011-2012	Research Fellow, UCE3.
2004-2006	Analyst, Analysis Group, Inc.
2003	Research Assistant to Professor George Tolley.

FELLOWSHIPS & GRANTS

2013-2014	Joseph L. Fisher Doctoral Dissertation Fellowship, Resources for the Future.
2012-2013	UCSB Dean's Fellowship.
2012	Data Acquisition Grant, UCSB Department of Economics.

AWARDS & HONORS

2010	Distinction on Ph.D. Qualifying Exam in Microeconomics, UCSB.
2010	Distinction on Ph.D. Qualifying Exam in Econometrics, UCSB.
2007	"High Pass" on Ph.D. Qualifying Exam in Microeconomics, UT Austin.

TEACHING EXPERIENCE

2010-2014	Teaching Assistant, Basic Macroeconomics (undergraduate), UCSB.
2013	Teaching Assistant, Environmental Economics (undergraduate), UCSB.

2010	Teaching Assistant, Intermediate Macroeconomics (undergraduate), UCSB.
2009	Teaching Assistant, Probability and Statistics (undergraduate), UCSB.
2008	Teaching Assistant, Managerial Economics (undergraduate), UT Austin.
2006-2007	Teaching Assistant, Urban Economics (undergraduate), UT Austin.

PROFESSIONAL AFFILIATIONS

American Economic Association, Association of Environmental and Resource Economists, National Association for Business Economics.

REFEREE

Empirical Economics

Abstract

Essays in Environmental and Natural Resource Economics and Econometrics

Ashwin Anil Rode

This dissertation is a collection of essays that contributes to the fields of environmental and natural resource economics and econometrics. The role of political economy factors in environmental and natural resource economics is a common theme of the first two chapters. The third chapter is a contribution to econometric methodology.

The first chapter deals with rent-seeking behavior with respect to environmental regulation. The allocation of costless permits during the establishment of an emission permit trading program creates incentives for rent-seeking. I hypothesize that firms with strong political connections have an advantage in rent-seeking, which I model as a low rent- seeking cost in a contest for permits. Low rent-seeking costs are predicted to yield a higher permit allocation for a firm in equilibrium. This prediction is tested by exploiting an unusual feature of the U.K.'s allocation procedure in Phase 1 of the European Union's Emissions Trading Scheme for carbon dioxide emissions. I observe both a firm's actual permit allocation as well as an earlier, provisional allocation that was rooted in technocratic projections of future emissions but was never implemented. Firms had the opportunity to

appeal their provisional allocation. I find that a firm's financial connections to members of the House of Commons strongly predict its post-appeal allocation. Even after controlling for the provisional allocation, along with industry and financial characteristics, a connection to an additional member is associated with a significant increase in a firm's actual permit allocation. Although no data exist on the amount firms spent on rent-seeking, theoretical predictions on rent-dissipation provide a basis for estimating these expenditures. The welfare loss from rent-seeking is estimated to be at least 137 million euros, which represents a significant cost over and above the abatement costs firms incurred to reduce their emissions.

In the second chapter, I investigate the "resource curse" hypothesis in the context of a single country (the United States), focusing in particular on the role of institutional differences across states in mitigating or amplifying the resource curse. I build on the work of Papyrakis and Gerlagh (2007) by explicitly considering the interaction between a state's natural resource wealth and its institutional quality as measured by per capita corruption convictions. I find that natural resource wealth is associated with lower economic growth only in relatively corrupt states. In states with relatively little corruption, natural resource wealth is actually associated with higher economic growth. These results suggest that the institutional aspect of the resource curse hypothesis is relevant even in a developed country context.

The third chapter deals with the identification and estimation of treatment effects under interference. The framework of potential outcomes has historically assumed that the treatment status of other individual's does not influence a given individual's outcome. However, many empirical settings are characterized by "interference", where an individual's outcome depends both on own treatment and the treatments of others. We extend the potential outcomes framework and define treatment effects that are relevant in such settings. Treatment effects may involve a comparison between outcomes when an individual's treatment status changes or when only the treatment statuses of others change. We propose necessary conditions under which a full range of treatment effects are identified and show how these effects can be estimated through a linear regression. Evidence from simulations suggests that incorrect specification of the regression equation can result is highly misleading estimates.

Professor Robert Deacon
Dissertation Committee Co-Chair

Professor Douglas Steigerwald
Dissertation Committee Co-Chair

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Chapter 1

Rent-seeking over Tradable Emission Permits: Theory and Evidence

1.1 Introduction

Market-based emission permit trading programs have become an important part of environmental and climate change regulation worldwide. The aim of these programs is to reduce emissions of a pollutant to the level specified by an emissions cap in the most cost-effective way. While the emissions cap determines the total number of permits in circulation, any emission permit trading program must also specify a method for initially allocating those permits across the polluting entities. The choice of initial allocation necessarily has distributional implications, and if market imperfections are present it has efficiency implications as well (Joskow and Schmalensee, 1998).

There are two main approaches to initially allocating permits: auctioning and grandfathering. Under auctioning, permits are sold to the highest bidder. In contrast, grandfathering entails distribution of costless permits to emitters on the basis of pre-determined characteristics. Grandfathering has by far been the dominant allocation approach in practice, both because it can offset some of the costs of emission reduction as well as for political reasons. Stavins (1998) points out that allocating costless permits offers “a much greater degree of political control over the distributional effects of environmental regulation” as compared to auctioned permits. Fowlie (2010) notes that grandfathering offers “the ability to make concessions to adversely impacted and politically powerful stakeholders”, and that this ability has “been an important factor in the widespread adoption of emissions trading programs”. While the Coase Theorem implies that a well-functioning permit market achieves an economically efficient outcome regardless of how permits are initially allocated (Coase, 1960; Montgomery, 1972), subsequent work has shown that distributional and efficiency concerns cannot be decoupled if the assumptions underlying the Coase Theorem fail to hold. For example, Hahn (1984) and Stavins (1995) show, respectively, that initial allocations do matter for efficiency if the permit market is imperfectly competitive or has transaction costs.

This paper draws attention to another source of distributional and efficiency concerns in emission permit trading programs− rent-seeking behavior. Because permits represent valuable economic assets, a polluting firm stands to gain financially if more permits are allocated to it. It is therefore plausible that a firm will expend resources in lobbying to increase its allocation. Joskow and Schmalensee (1998) argue that because decisions about permit allocations are made by political institutions, these decisions are likely to reflect rent-seeking behavior. Such behavior has been conjectured in the non-academic press.[1] The same idea has also been articulated more formally by Nordhaus (2006, 2007) and has been theoretically modeled by Hanley and MacKenzie (2010). However, with the notable exception of Joskow and Schmalensee's (1998) study on the political economy of the U.S. acid rain program, there has been little formal empirical work exploring how rent-seeking behavior affects permit allocations when emission permit trading programs are implemented.

I contribute to this literature by modeling a rent-seeking context in which polluting firms can influence their permit allocations through lobbying, and then test predictions of the model using unique data from Phase 1 of the European Union's (E.U.'s) Emissions Trading Scheme (ETS) in the U.K.. Unlike the model of Hanley and MacKenzie (2010), firms in my model face heterogeneous marginal costs of lobbying. The differences in lobbying costs affect not only the amount

[1] See for example, "Soot, smoke and mirrors: Europe's flagship environmental programme is foundering" in *The Economist*, Nov. 16, 2006. See also, "Britain's worst polluters set for windfall of millions" in *The Guardian*, Sept. 12, 2008.

of resources a firm devotes to lobbying but also whether or not a firm chooses to pursue lobbying at all. The model generates predictions about the allocation of the permit endowment among firms as well as the welfare loss from resources wasted in rent-seeking.[2] In particular, all else being equal, a firm's equilibrium permit allocation is decreasing in its marginal cost of lobbying, and the total amount spent on rent-seeking by all firms is equal to the value of the rents.

The E.U. ETS gave away tradable carbon dioxide (CO_2) emission permits to nearly 12,000 industrial plants (known as "installations") across Europe. Prior to the beginning of the scheme in 2005, each member state was responsible for allocating its national emissions cap to installations within its borders. My empirical approach exploits an unusual feature of the permit allocation procedure in the U.K. For the vast majority of installations in the U.K. (representing over 90% of the national cap), I observe not only the actual number of permits allocated, but also the number of permits the installation would have received under a provisional allocation plan published one year prior to the final, realized allocation plan. In the intervening year, firms could appeal their provisional allocations. Because the national cap remained virtually identical in the two allocation plans, efforts by firms to secure higher allocations for themselves took place in the context of a zero-sum game. This setting provides a unique opportunity to study

[2] Since the initial work of Tullock (1967), the literature on rent-seeking has viewed rent-seeking efforts as socially unproductive.

the implications of rent-seeking behavior. While the provisional allocation plan was based on technocratic forecasts of future emissions, the final plan reflected lobbying activity during the appeal period (Mallard, 2009; Duggan, 2009). Due to the structure of the allocation process, the appeal period in particular became the locus of the lobbying activity related to allocations. The UK government explicitly invited "consultation" regarding specific allocations only after the release of the provisional allocation plan, and firms responded vigorously to this invitation. For example, on a February day in 2004, thousands of executives filled an exhibition center in Birmingham to question government officials on the recently published provisional plan (Duggan, 2009).

As an empirical proxy for a firm's cost of lobbying, I utilize data on the firm's pre-existing financial connections to members of the House of Commons. Although members were not directly involved in the permit allocation process, a firm's connections to members are plausibly indicative of how easily it can exert influence in diverse regulatory spheres. I find that a connection to an additional member is associated with a significant increase in a firm's realized permit allocation, even after controlling for the firm's provisional allocation, industry and other characteristics. Although there exist no direct records of lobbying expenditures, the theoretical results on rent-dissipation provide a basis for a calculation of how much was spent on rent-seeking over permits in the U.K.. I estimate the welfare loss from rent-seeking in the U.K. alone to be at least 137 million euros. This is a non-trivial amount when juxtaposed against available estimates of total annual abatement costs, which are in the range of 450 million to 900 million euros for the entire E.U. (Ellerman et al., 2010).

My work fits into multiple strands of literature. Most directly, I make a theoretical and empirical contribution to the literature on the distributional and efficiency properties of market-based environmental regulation. My work also extends the theoretical literature on rent-seeking contests by considering a contest where the cost of rent-seeking activity varies across firms, and is one the few examples where the predictions of a rent-seeking model are empirically tested.[3] Finally, my work contributes to an emerging empirical literature outside of environmental economics on the benefits firms derive from political influence (Fisman, 2001; Khwaja and Mian, 2005; Faccio et al., 2006; Jayachandran, 2006; Goldman et al., 2010).

The rest of the paper is organized as follows: Section 2 presents a model of a rent-seeking contest for emission permits; Section 3 explains the institutional details of permit allocation in the E.U. ETS; Section 4 describes the data sources and contains empirical tests of the model's predictions; Section 5 concludes.

[3] Although the rent-seeking literature has not explicitly analyzed the consequences of heterogeneous rent-seeking costs across agents, it has considered contests in which agents have different valuations of the contested prize (Hillman and Riley, 1989; Nti, 1999; Stein, 2002). Certain findings from this line of work are echoed in my theoretical results.

1.2 A Rent-Seeking Contest for Emission Permits

1.2.1 Structure of the Context

Consider a competitive emission permit market with an emissions cap of $\bar{A}$, in which n firms are to participate. Although emission permits are allocated at no cost, the realized allocations are influenced by the lobbying efforts of firms. The contest for permits begins with the regulator announcing a provisional allocation for each firm. The emissions cap constrains the regulator's choice of allocations; letting $A_i \geq 0$ denote the provisional allocation for firm i, it is required that $\sum_{i=1}^{n} A_i = \bar{A}$. After the provisional allocations are announced, a portion of the cap is reallocated based on firms' lobbying efforts. Formally, let $x_i \geq 0$ denote the lobbying effort of firm i and let $\widetilde{A}_i$ denote firm i's realized, post-contest allocation of permits. The post-contest allocation of firm i is given by:

$$\widetilde{A}_i = \begin{cases} A_i^{1-\gamma} + \left(\frac{x_i}{\sum_{j=1}^{n} x_j}\right)\phi & if \sum_{j=1}^{n} x_j > 0 \\ A_i & otherwise \end{cases} \qquad \forall i = 1, 2, ..., n, \tag{1.1}$$

where $\phi \equiv \sum_{i=1}^{n} \left[A_i - A_i^{1-\gamma}\right]$.[4]

The formula specified in (1.1) is purely redistributive in that the sum of the post-contest allocations of all firms is the same as the sum of the provisional, pre-contest allocations of all firms, both equalling the total cap $\bar{A}$.[5] The variable ϕ expresses the number of permits subject to contest, and the number of permits an individual firm captures in the contest is proportional to the ratio of its own lobbying effort to the total lobbying efforts of all firms.[6]

The parameter $\gamma \in [0, \infty)$, which is common knowledge to the firms and regulator, determines the value of ϕ. In particular, γ represents the extent to which the regulator can be swayed by lobbying efforts. For instance, if $\gamma = 0$, lobbying has no influence on the regulator, and provisional allocations stand unchanged.[7] At the other extreme, as γ converges to ∞, the provisional allocations are completely overridden, and the realized allocations depend solely on relative lobbying efforts.

[4]Equivalently, $\phi \equiv \bar{A} - \sum_{i=1}^{n} A_i^{1-\gamma}$.

[5]Unlike Hanley and MacKenzie (2010), I allow rent-seeking to influence only the allocation of the cap, not the cap itself. In the case I study empirically, there is no evidence that lobbying shifted the overall cap.

[6]This contest function dates back to Tullock (1980) and has been widely used since. For example, Grossman (2001) uses it to model the formation of property rights; Hodler (2006) uses it to model competition over natural resource wealth; and Hanley and MacKenzie (2010) use it to model competition for costless pollution permits. Skaperdas (1996) argues that the class of functions propounded by Tullock (1980), which is based on ratios of efforts, is the only class that satisfies a number of desirable and plausible properties of a contest function.

[7]The provisional allocations also remain unchanged regardless of the value of γ if no firm undertakes any lobbying.

After allocations are realized according to (1.1), firms make decisions about how much output to produce and how much of the pollutant to emit. Firm i produces output according to a strictly concave, twice differentiable production function $q_i(e_i, z_i)$, where e_i denotes emissions and z_i denotes a vector of other inputs. Let p, τ, and η respectively denote the prices of output, emission permits, and other factors. Let ω_i denote firm i's unit cost of lobbying effort. Firm i's profit maximization problem is

$$\underset{e_i,x_i,z_i}{\boldsymbol{Max}}\, pq_i(e_i, z_i) - \eta z_i - \omega_i x_i - \tau(e_i - \widetilde{A}_i), \tag{1.2}$$

where $\widetilde{A}_i$ is defined by (1).

The firm's demand for emissions and other inputs to production are implicitly defined by the first order conditions for an interior solution to this profit maximization problem.[8] Although the firm's profits are increasing in its permit allocation, it is straightforward to show that the firm's optimal choice of emissions and other inputs are independent of its permit allocation, and that in equilibrium the marginal product of emissions will be equalized across all firms. These properties, which accord with the textbook case of emission permit trading in a Coasian world,

[8] I assume that firms are price-takers. This assumption would clearly not be reasonable, for example, in the extreme case where a single firm receives all the permits and behaves as a monopolist. However, studies suggest that market power is not a major concern in the E.U. ETS (Convery and Redmond, 2007; Hahn and Stavins, 2011).

allow production decisions to be separated from lobbying decisions. Unlike in models of imperfect competition (Hahn, 1984) or transaction costs (Stavins, 1995), inefficiencies do not arise in this model due to a breakdown of the equimarginal principal; instead efforts wasted in lobbying are the sole source of social losses.[9]

Hence firm i's choice of lobbying effort can be isolated to the following maximization problem:

$$\underset{x_i \geq 0}{\boldsymbol{Max}}\, \tau \widetilde{A}_i - \omega_i x_i, \tag{1.3}$$

where firm i takes as given the lobbying efforts of other firms. Next, I solve for the Nash equilibrium of such a contest with n firms.

1.2.2 Nash Equilibrium with n Firms

Differentiating (1.3) with respect to x_i for $i = 1, 2, ..., n$, yields the following first order conditions for an interior solution:

$$\tau\phi \tfrac{x_{-i}}{(x_i + x_{-i})^2} = \omega_i \quad for\, i = 1, 2, ..., n \ , \tag{1.4}$$

[9]The equimarginal condition will fail to hold if efforts devoted to lobbying somehow hampered production. However, for the purposes of the model, I abstract away from this potential source of inefficiency, focusing only on the efforts wasted in lobbying.

where x_{-i} refers to the sum of lobbying efforts of firms other than firm i. These conditions state that each firm chooses a level of lobbying effort that equalizes its marginal benefits and marginal cost.[10]

The first order conditions together with the non-negativity constraints on lobbying efforts lead to the following best response functions:

$$x_i = \begin{cases} \sqrt{\frac{\tau\phi x_{-i}}{\omega_i}} - x_{-i} & if\, x_{-i} \in (0, \frac{\tau\phi}{\omega_i}) \\ 0 & if\, x_{-i} \geq \frac{\tau\phi}{\omega_i} \end{cases} \qquad for\, i = 1, 2\ , ...n. \tag{1.5}$$

A strategy profile in which all firms exert zero lobbying effort cannot constitute a Nash equilibrium. According to the contest function specified in (1.1), the best response of firm i to $x_{-i} = 0$ is to exert an arbitrarily small amount of lobbying effort, $x_i = \epsilon > 0$, and thereby capture the entire quantity of contested permits. However, this cannot constitute a Nash equilibrium either. Suppose for example that $x_{-i} = 0$ and $x_i = \epsilon > 0$. Although firm i's choice of an arbitrarily small amount of lobbying effort, ϵ, is a best response to $x_{-i} = 0$, the best response function (1.5) indicates that the other (non-i) firms' choices of zero lobbying effort are not best responses to a sufficiently small ϵ. This reasoning implies that a Nash equilibrium cannot involve only one firm with strictly positive effort while all other

[10]The second order sufficient conditions for a maximum are satisfied. The second derivatives of each firm's profit function are negative when evaluated at the interior optimum. Specifically, $-\tau\phi\frac{2x_i}{(x_i+x_{-i})^2} < 0$ for $i = 1, \ldots, n$.

firms refrain from lobbying. Thus at least two firms must exert strictly positive lobbying effort in a Nash equilibrium. I now consider a strategy profile in which all firms exert strictly positive lobbying effort.

All Firms Lobbying

If all n firms undertake strictly positive amounts of lobbying efforts, (1.4) will hold with equality for $i = 1, 2, ..., n$. Summing both sides of (1.4) over all i and rearranging, the total lobbying effort is:

$$x = \frac{(n-1)\tau\phi}{\sum_{j=1}^{n} \omega_j}. \tag{1.6}$$

If $x_i > 0$ for $i = 1, 2, ..., n$, the best response function (1.5) implies

$$x = \sqrt{\frac{\tau\phi x_{-i}}{\omega_i}}\, for\, i = 1, 2, ..., n. \tag{1.7}$$

Combining (1.6) and (1.7), I obtain

$$x_{-i} = \frac{\omega_i \tau\phi(n-1)^2}{\left(\sum_{j=1}^{n} \omega_j\right)^2}\, for\, i = 1, 2, ..., n. \tag{1.8}$$

Consequently,

$$x_i = \frac{\tau\phi(n-1)}{(\sum_{j=1}^{n}\omega_j)^2} \cdot \left(\left(\sum_{j=1}^{n}\omega_j\right) - \omega_i(n-1)\right) \; for\, i = 1, 2, ..., n. \tag{1.9}$$

Upon inspection of (1.9), it is evident that an equilibrium in which all firms exert strictly positive lobbying effort is possible only in the case where $\omega_i < \frac{1}{n-1}\sum_{j=1}^{n}\omega_j \; for\, i = 1, 2, ..., n$.[11] This condition places limits on the dispersion of the marginal costs of lobbying across firms.[12] In the special case of $n = 2$ firms the condition is guaranteed to hold, and the unique Nash equilibrium has both firms lobbying. Moreover, the condition will also hold for any n if all firms face equal lobbying costs. However, if firms face heterogenous lobbying costs, it is not guaranteed that all firms will participate in lobbying. Specifically, those firms whose lobbying costs are too high relative to those of other firms will choose to refrain from lobbying.

Lobbying by a Subset of Firms

Uniqueness of Equilibrium

I now consider equilibria with $k \leq n$ lobbying firms and $n - k$ non-lobbying firms. Without loss of generality, let $j = 1, 2, ..., k$ index the lobbying firms. For

[11] Based on (1.8), the condition $\omega_i < \frac{1}{n-1}\sum_{j=1}^{n}\omega_j \; for\, i = 1, 2, ..., n$ is equivalent to $x_{-i} < \frac{\tau\phi}{\omega_i}$, which is what the best response function (1.5) requires for an interior solution.

[12] In the case of $n = 3$ firms, the condition $\omega_i < \frac{1}{n-1}\sum_{j=1}^{n}\omega_j \; for\, i = 1, 2, ..., n$ is tantamount to the triangle inequality; the marginal lobbying cost of any firm must be strictly less than the sum of the marginal lobbying costs of the other two firms.

these k firms, the first order condition (1.4) holds with equality. Summing both sides of (1.4) over $i = 1, 2, ..., k$ and rearranging, the total lobbying effort is

$$x = \frac{(k-1)\tau\phi}{\sum_{j=1}^{k} \omega_j}, \tag{1.10}$$

and combining (1.10) with the best response function (1.5), the lobbying efforts of individual firms are

$$x_i = \begin{cases} \frac{\tau\phi(k-1)}{(\sum_{j=1}^{k}\omega_j)^2} \cdot \left(\left(\sum_{j=1}^{k}\omega_j\right) - \omega_i(k-1)\right) & for\, i = 1, 2, ..., k \\ 0 & for\, i = k+1, k+2, ..., n. \end{cases} \tag{1.11}$$

A Nash equilibrium can involve $k \leq n$ lobbying firms if the lobbying costs of these k firms are sufficiently close to each other, and the lobbying costs of the non-lobbying firms are sufficiently higher than those of the lobbying firms. In particular, for (1.11) to be a Nash equilibrium, it is required that $\omega_i < \frac{1}{k-1}\sum_{j=1}^{k}\omega_j \, for\, i = 1, 2, ..., k$. Furthermore, the best response function (1.5) requires that $x_{-i} \geq \frac{\tau\phi}{\omega_i} \, for\, i = k+1, k+2, ..., n$. Noting that for the non-lobbying firms $x_{-i} = x$, this requirement can be expressed as $\omega_i \geq \frac{1}{k-1}\sum_{j=1}^{k}\omega_j \, for\, i = k+1, k+2, ..., n$.

The requirements for a Nash equilibrium with k lobbying firms imply that the k lobbying firms must be the k firms with the lowest lobbying costs. Also, because

$\omega_i < \sum_{j=1}^{2} \omega_j \, for \, i = 1, 2$ is trivially true, an equilibrium cannot involve fewer than two lobbying firms. Lastly, it can be shown that the number of lobbying firms in equilibrium is unique. (See Appendix A for proof that k is uniquely determined.)

The above results are summarized as the following proposition:

PROPOSITION 1.1: *Nash equilibrium of the n firm contest*

1. *Only the k firms with the lowest lobbying costs participate in lobbying, where* $k \in \{2, \ldots, n\}$ *and is uniquely determined.*

2. *If firm i participates in lobbying, its lobbying cost must be strictly less than the sum of the lobbying costs of the k lobbying firms, divided by* $k - 1$.

3. *If firm i does not participate in lobbying, its lobbying cost must be greater than or equal to the sum of the lobbying costs of the k lobbying firms, divided by* $k - 1$.

4. *The lobbying efforts of the participating firms are defined by (1.11).*

Lobbying Efforts, Expenditures, and Permit Allocations in Equilibrium

The equilibrium lobbying effort of a lobbying firm is decreasing in the firm's own cost of lobbying, however the effect of an increase in another firm's lobbying cost is ambiguous. (See Appendix A for proof.)

Total expenditures on lobbying effort, which are obtained by summing the lobbying expenditures of all firms (i.e. $\sum_{j=1}^{n}\omega_j x_j$), are $\tau\phi(k-1)\left[1-\frac{(k-1)\sum_{j=1}^{k}\omega_j^2}{\left(\sum_{j=1}^{k}\omega_j\right)^2}\right]$. The following proposition establishes that when the number of lobbying firms is arbitrarily large, rents are fully dissipated, a canonical result in rent-seeking models.

Proposition 1.2: *When the number of lobbying firms, k, is arbitrarily large, total expenditures on lobbying effort equal the value of the rents, $\tau\phi$.*

Proof: For a given k, an upper bound on total lobbying expenditure is $\left(\frac{k-1}{k}\right)\tau\phi$. The upper bound is reached when the lobbying costs of the k lobbying firms are equal; higher variance in the lobbying costs of the k firms leads to lower total lobbying expenditure. This is because the quantity $\frac{\sum_{j=1}^{k}\omega_j^2}{\left(\sum_{j=1}^{k}\omega_j\right)^2}$ is monotonically increasing in the variance of the ω_j's and attains a minimum value of $\frac{1}{k}$ when the variance of the ω_j's is zero (i.e. when $\frac{\sum_{j=1}^{k}\omega_j^2}{k}-\frac{\left(\sum_{j=1}^{k}\omega_j\right)^2}{k^2}=0$).

When the number of lobbying firms, k, is arbitrarily large, total lobbying expenditures are equal to $\tau\phi$. The reason for this is two-fold. First, the larger the value of k, the more stringent are the limitations on the variance of the lobbying costs. Specifically, when k is arbitrarily large, the conditions for a Nash equilibrium imply that the variance of the lobbying costs of the k firms must be zero. (Recall that, for any Nash equilibrium in which k firms lobby, it must be that $\omega_i < \frac{1}{k-1}\sum_{j=1}^{k}\omega_j \, for \, i = 1, 2, ..., k.$) Second, as the variance of the lobbying costs of the k firms reaches zero, total lobbying expenditure must itself reach its upper bound, $\left(\frac{k-1}{k}\right)\tau\phi$. It is evident that this upper bound reaches $\tau\phi$ for an arbitrarily large k. *Q.E.D.*

The Nash equilibrium implies the following post-contest allocations:

$$\widetilde{A}_i = \begin{cases} A_i^{(1-\gamma)} + \phi\left(1 - \frac{(k-1)\omega_i}{\sum_{j=1}^{k}\omega_j}\right) & for\, i = 1, 2, ..., k \\ A_i^{(1-\gamma)} & for\, i = k+1, k+2, ..., n. \end{cases} \tag{1.12}$$

Equation (1.12) forms the basis of the empirical work in Section 4. The relationship between the provisional allocation, A_i, and the realized allocation, $\widetilde{A}_i$, is specified by (1.12). Furthermore, for the lobbying firms, $\widetilde{A}_i$ is decreasing in own lobbying cost but increasing in the lobbying costs of other firms.[13] Taking the natural log of both sides of (1.12) yields

$$ln(\widetilde{A}_i) = (1-\gamma)ln(A_i) + \psi_i, \tag{1.13}$$

where the quantity ψ_i equals zero for non-lobbying firms. For lobbying firms, $\psi_i > 0$ and is decreasing in own lobbying cost but increasing in the lobbying

[13] The relevant derivatives with respect to own lobbying costs, for $i = 1, 2, ..., k$, are $\frac{\partial \widetilde{A}_i}{\partial \omega_i} = \frac{-\phi(k-1)\left[\left(\sum_{j=1}^{k}\omega_j\right)-\omega_i\right]}{\left(\sum_{j=1}^{k}\omega_j\right)^2} < 0$.

costs of other firms. In other words, a firm that can lobby at relatively low cost (compared to other firms) will have a relatively high value of ψ_i and realize a higher permit allocation.

The model implies an exact expression for ψ_i. Specifically

$$\psi_i = ln\left(A_i^{(1-\gamma)} + \phi\left(1 - \frac{(k-1)\omega_i}{\sum_{j=1}^{k}\omega_j}\right)\right) - ln(A_i^{(1-\gamma)}).$$

However, a simpler approximation can be obtained. Using the fact that for any small ϵ, $ln(1+\epsilon)$ is closely approximated by ϵ, $\psi_i \approx \frac{\phi\left(1-\frac{(k-1)\omega_i}{\sum_{j=1}^{k}\omega_j}\right)}{A_i^{1-\gamma}}$.[14] This suggests that even if two firms face identical costs of lobbying, the firm with a higher provisional allocation will have a lower value of ψ.[15]

After describing the institutional details of permit allocation in the E.U. ETS, I test the predictions of the model, as expressed in (1.13), using data from the U.K.'s allocation procedure in Phase 1. I am able to construct firm-level provisional and realized allocations. While I cannot observe actual lobbying effort or expenditures, I develop a proxy measure for a firm's relative cost of

[14] This approximation is valid if the number of permits a firm gains in the contest (i.e. $\phi\left(1 - \frac{(k-1)\omega_i}{\sum_{j=1}^{k}\omega_j}\right)$) is small relative to the permits the firm retains (i.e. $A_i^{(1-\gamma)}$). The high level of "persistence" of the provisional allocation (observed in the data) suggests that the approximation is reasonable.

[15] I address this prediction of the model by including an interaction term between a firm's provisional allocation and a measure of its cost of lobbying. The inclusion of the interaction term does not materially alter the results.

lobbying using data on political connections. This allows me test whether firms that arguably face lower costs of lobbying realize higher allocations, controlling for their provisional allocations. Such a test will shed light on the distributional consequences of rent-seeking for emission permits. As indicated in Proposition 1.2, the efficiency implications of rent-seeking depend centrally on the number of contested permits, ϕ. The value of the contested permits is $\tau\phi$, which is also the total welfare loss associated with lobbying expenditures if k is sufficiently large. By estimating γ it is possible to calculate a value for ϕ. The welfare loss can then be assessed by multiplying ϕ by an expected permit price at the time the allocation procedure took place.

1.3 Permit Allocation in the E.U. ETS

The EU ETS is divided into multi-year trading periods known as phases. Phase 1 spanned the years 2005-2007 and was intended to be a trial phase. Phase 2 spanned 2008-2012, and Phase 3 runs from 2013-2020. Permits from Phase 1 were not valid for Phase 2. However, permits from Phase 2 could be banked to Phase 3 (Ellerman and Joskow, 2008).

For the first two phases, both the cap-setting and allocation processes of the EU ETS were highly decentralized. Prior to each phase, every member state was responsible for setting a national cap and developing a National Allocation Plan that specifies the distribution of the cap to installations located in the state.[16] Each installation was issued a fixed number of permits for every year within a phase,[17] and there was no restriction on banking or borrowing across years within the same phase (Ellerman and Joskow, 2008). A permit confers the right to emit one metric ton of CO_2 in a given year.

1.3.1 Cap-Setting and Allocation to Sectors and Installations

The U.K.'s total cap was informed by its commitments under the European Burden-Sharing Agreement of the Kyoto Protocol[18] as well as its own, more stringent, national emission reduction targets.[19] The installations covered by the

[16] The cap-setting and allocation processes changed considerably in Phase 3. A more centralized approach was adopted that did not involve National Allocation Plans. Also, auctioning played a much bigger role. See European Commission (2013).

[17] It was up to the member states to determine which installations would be covered by the ETS. Annex I of the EU Emissions Trading Directive defines the specific economic activities that fall under the ETS regime. However, Ellerman *et al.* (2007) point out that "the legal interpretation of which installations are captured by Annex I of the Directive differed across Member States, in particular regarding the question of what constitutes a combustion installation" (pg. 16).

[18] The Burden-Sharing Agreement allows the E.U. to distribute its Kyoto target among member states. In June 1998, a political agreement was reached on the distribution of emission reduction efforts within the E.U..

[19] The Burden-Sharing Agreement commits the U.K. to achieve a 12.5% reduction in CO_2 and other greenhouse gas emissions by 2012, relative to 1990 emissions. (Besides CO_2, the Kyoto Protocol covers 5 other gases: methane, nitrous oxide, sulfur hexafluoride, hydrofluorocarbons, and perfluorocarbons (Grubb, 2003).) Beyond the Burden-Sharing Agreement commitments, the U.K. has also set for itself more ambitious national targets specifically for CO_2 emissions,

E.U. ETS accounted for approximately half of UK CO_2 emissions in 2002, and the cap-setting was intended to ensure that the covered installations make an "appropriate contribution" to the overall emission reduction goals (Department of Environment, Food, and Rural Affairs, 2005).[20]

The U.K. Phase 1 National Allocation Plan was the first to be published in provisional form (in January 2004) and influenced the plans of other member states.[21] All Phase 1 permits were distributed at no cost (Ellerman *et al.*, 2007).[22] Although the sector classifications changed drastically from the provisional to the final plan, the two plans were guided by similar mechanical formulae. In both plans, a small fraction of permits were set aside as a "New Entrant Reserve".[23]

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including a 20% reduction by 2010 and a 60% reduction by 2050, relative to 1990 levels. (Ellerman *et al.*, 2007)

[20] Transportation is the largest source of emissions that was completely outside the scope of the E.U. ETS in Phases 1 and 2 (Department of Environment, Food, and Rural Affairs, 2005).

[21] The lead government department in charge of developing the UK plan was the Department of Environment, Food, and Rural Affairs (DEFRA), however the Department of Trade and Industry and the Environment Agency were also involved. In October 2008, the Department of Energy and Climate Change (DECC) was formed, and the climate change related functions of DEFRA were transferred to DECC (UK Civil Service, 2009).

[22] The political expediency of grandfathering is reflected in the allocation practices of the U.K. and other E.U. member states. Under Article 10 of Annex III of the E.U. Emissions Trading Directive, member states had the discretion to sell or auction no more than 5% of permits in Phase 1 and 10% of permits in Phase 2. Markussen and Svendsen (2005) provide a political economy explanation for this rule. In Phase 1, only four member states (Denmark, Ireland, Hungary, and Lithuania) choose to auction any permits, and of these, only Denmark choose to auction the full 5% (Buchner *et al.*, 2006).

[23] The New Entrant Reserve consisted of 5.7% of permits in the provisional plan (Department of Trade and Industry, 2004) and 6.3% in the final plan (Department of Environment, Food, and Rural Affairs, 2005). Installations that began operation in the middle of the phase were entitled to permits out of this reserve, which was allocated across sectors based on expected new entry. Concerns of fairness and competitiveness motivated the provision of costless permits to new entrants. Providing costless permits to existing installations while forcing new installations to buy them was perceived as unfair to new installations. Moreover, the UK did not want to place itself at a competitive disadvantage in attracting new investment (Parker, 2008).

The remaining permits were allocated to existing installations through a two-stage procedure that first involved allocations to sectors followed by allocations to installations within sectors.[24] Firms did not receive explicit consideration in this procedure and could have multiple installations in more than one sector.

The allocations to sectors other than the power generation sector were based on the expected future emissions of those sectors. The power generation sector received only a residual allocation equal to the difference between the total cap and the allocations to all other sectors. Concerns of competitiveness motivated this differential treatment. Because the power generation sector is insulated from international competition compared to other sectors, electricity producers were expected to be able to pass on the costs of permits to their customers.

An individual installation was entitled to a fraction of the permits allocated to the sector to which it belongs. This fraction was equal to the installation's share of the sector's total "relevant emissions", which is a measure of historical emissions. In most cases, an individual installation's relevant emissions were computed by averaging annual emissions over a baseline period after dropping the lowest year's emissions. For Phase 1, the baseline period was 1998-2003.[25] A sector's relevant

[24]Such a two-stage procedure was used in almost all member states, Germany being the notable exception (Ellerman *et al.*, 2007).

[25]If an installation is not in operation during all years in the baseline period, the averaging procedure is carried out only over the years during which the installation is active (Department of Environment, Food, and Rural Affairs 2005, 2007).

emissions are simply the sum of the relevant emissions of all installations in that sector.

To summarize, the following formula guided the allocation for an installation i in sector j:

$$Allocation_i = \frac{RelevantEmissions_i}{RelevantEmissions_j} * SectorAllocation_j. \tag{1.14}$$

1.3.2 Changes between the Provisional and Final National Allocation Plan

The government explicitly invited consultation from industry after the publication of the provisional plan. The sectoral redefinition that emerged from this consultation was the major cause of changes in allocations between the provisional and final plans. Sector categories were the subject of much debate during the formulation of the final plan. The UK's Department of Trade and Industry was already involved in projecting sectoral emissions well before the E.U. ETS, and its projections informed those used in the provisional plan (Ellerman et al., 2007). However, the sector categories of the provisional plan were widely viewed by industry as being too coarsely defined. There was a desire for more disaggregated sector categories whose projections would reflect the particular circumstances of each industry. In response, the government commissioned independent consultants to produce more detailed sectoral projections of output, which the Department of Trade and Industry then used to project emissions (Ellerman et al., 2007; Department of Environment, Food, and Rural Affairs, 2005). The number of sector categories multiplied between the provisional and actual plans. While the provisional plan had 13 sectors for classifying installations (Department of Trade and Industry, 2004), the final plan had 51 sectors (Department of Environment, Food, and Rural Affairs, 2005).[26]

Aside from sectoral redefinition, the application of alternative rules for determining an installation's relevant emissions also contributed to differences between provisional and final allocations. The final plan reflected the application of special rules for determining relevant emissions for installations that underwent commissioning, added capacity, and/or were affected by intersite shifting of production during the baseline period (Ellerman *et al.*, 2007). Installations had to provide evidence in order to be considered for treatment under these special rules (Department of Environment, Food, and Rural Affairs, 2005).

[26] Existing climate change regulation in the UK, in particular the Climate Change Agreements (CCAs), also accounted for this high level of disaggregation. A CCA for a given industry allows participating facilities to receive an 80% discount on a tax on energy use known as the Climate Change Levy (CCL), in exchange for commitments to reduce energy use and greenhouse gas emissions (HM Revenue & Customs, 2012). Sectors in the final plan were differentiated not only by economic activity but also in terms of whether they were subject to a CCA (Ellerman *et al.*, 2007). For example, there were two sectors for chemicals, one that was subject to a CCA and another that was not. This practice was discontinued in Phase 2.

While sectoral redefinition and the application of special rules are proximate explanations for differences between provisional and final allocations, it has been widely emphasized that these were the manifestations of lobbying. According to Buchner *et al.* (2006), the allocation process for Phase 1 of the E.U. ETS in general “can best be described as an extended dialogue between the government and industry” in which there was “much lobbying” on the part of industry. Mallard (2009) remarks that changes between the provisional and final U.K. plans represent “perhaps the clearest example of the effects of lobbying”. Duggan (2009) points out that in their pursuit of the maximum number of costless permits, many companies in the U.K. pleaded to be treated as “’special cases’ or exceptions to the rules”. The empirical tests in the following section aim to evaluate the distributional and efficiency consequences of such lobbying.

1.4 Empirical Tests

1.4.1 Data Sources

Provisional and Actual National Allocation Plans

Table 1.1 summarizes the scope of the provisional and final plans. The two plans do not cover an identical universe of installations. One-hundred sixty four installations are present in the provisional plan but not in the final plan, and a large number of installations were added by the time of the final plan. However, the degree of overlap is considerable. I am able to match 703 installations between the two allocation plans, representing well over 90% of the U.K. national cap.[27] The total number of permits, whether to all installations or to the matched installations, remains almost the same in two plans. Thus any changes in allocations are essentially redistributive.

Considerable redistribution took place at the sectoral level. Table 1.2 displays the total provisional and final allocations of all matched installations in each of the thirteen sector categories of the provisional plan. The oil and gas industry, which encompasses the "Offshore" and "Refineries" sectors, appears to have benefited in the redistribution, while the "Power Stations" sector lost. To account for such sectoral shifts, industry controls are included in the empirical specifications.

Firm-level allocations are constructed by aggregating the allocations of installations operated by the same firm.[28] The matched installations represent a total 270 firms.[29] As described previously, firms do not receive explicit consideration in

[27] Matching between the two plan is possible through a unique identification number assigned to each installation.

[28] The provisional and actual plans report the firm each installation is associated with. In some instances, the reported firm may be a subsidiary of another firm. The firm-level allocations I have constructed include allocations to the firm and its subsidiaries. I have carefully identified subsdiaries by individually ascertaining the ownership status in 2004 of each reported firm. The sources relied upon include company websites, financial reports, press releases, and company descriptions from Hoovers and Bloomberg Businessweek Company Insight Center.

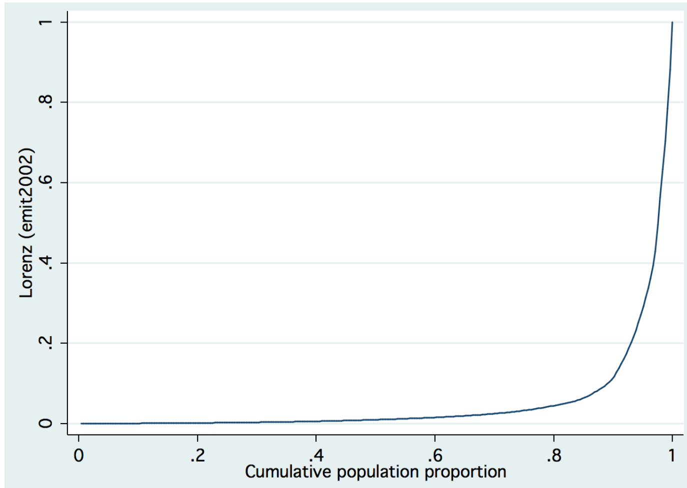
[29] In the regressions the sample sizes are lower because I exclude universities, hospitals, and government entities. These entities account for less than 0.5% of the cap in both plans. Also, lack of financial data accounts for the lower sample size in regressions that include firm financial variables.

the formula for allocation, the two-stages of which involve allocations to sectors and then installations within sectors. The same firm can have multiple installations, not all of which fall into the same sector. For the subsequent, firm-level, emprical analysis, each firm is assigned to one of 8 industry categories based on Standard Industrial Classification (SIC) codes. The industry groupings used are: Chemicals (Major Group 28), Food and Drink (Major Group 20), Fossil Fuels (Major Groups 12, 13, and 29), Metal Manufacture (Major Groups 33 and 34), Pulp and Paper (Major Group 26), Stone/Clay/Glass/Concrete (Major Group 32), Transportation Equipment (Major Group 37), and Utilities (Major Group 49).[30]

The distribution of emissions across firms is highly skewed, with a relatively small number of large emitters accounting for the bulk of emissions. Figure 1.1

[30] A firm was placed into one of these groupings primarily on the basis of the sectors its installations were classified under in the provisional plan. The provisional plan sectors of "Chemicals", "Food & Drink", and "Pulp & Paper" correspond directly to the SIC-based industry groups. The provisional plan sectors of "Bricks/Ceramics", "Cement", "Glass", and "Lime" all map to the "Stone/Clay/Glass/Concrete" industry group, while the "Iron & Steel" and "Non-ferrous sectors" map to Metal Manufacture. "Power Stations" fall under the Utilities group. The Fossil Fuels group includes firms engaged in the extraction of fossil fuels and/or refining; the "Offshore" and "Refineries" sectors in the provisional plan fall in this grouping. Finally, the "Other Combustion Activities" sector includes firms whose business activities may fall into any of the industry groups; companies that manufacture Transportation Equipment are included in this sector. The installations of most firms fall into only one sector of the provisional plan. For firms with installations in more than one sector, there was typically one dominant sector that represented the core business activity. For example, British Petroleum is classified in the Fossil Fuels group even though 2 out of its 20 installations fall in the "Chemicals" sector in the provisional plan. When necessary, the Amadeus database published by Bureau van Dijk was consulted to establish a firm's industry grouping. See `http://www.bvdinfo.com/Products/Company-Information/International/AMADEUS.aspx`.

Figure 1.1: Lorenz Curve of Distribution of Emissions across Firms in 2002



plots a Lorenz curve of emissions; the top 20% of emitting firms accounted for approximately 95% of emissions in 2002.

Political Connections

The data source for political connections is the *Register of Members' Financial Interests*, which is published several times a year by the U.K. House of Commons.[31] This publication documents the financial connections to firms of every member of the House of Commons (MP). Financial connections include gifts from a firm to the MP, shareholdings, remunerated directorships, and employment.[32] I use issues of the register spanning the years 2000-2004.[33] Forty-seven firms had connections

[31] Issues of the *Register* can be downloaded from `http://www.publications.parliament.uk/pa/cm/cmregmem.htm`.

[32] Similar measures have been used in other papers on political connections. See for example Khwaja and Mian (2005), Faccio et al. (2006), and Ferguson and Voth (2008).

[33] In particular, the following issues were used: November 10, 2000; May 14, 2001; May 14, 2002; November 26, 2002; December 4, 2003; and January 31, 2004.

to at least one MP during this time; twenty-two firms had connections to only one MP, fourteen firms had connections to 2-5 MPs, and 11 firms had connections to more than 5 MPs. The most common type of connection was the receipt of gifts by MPs from firms. Thirty-four firms gave gifts to at least one MP.[34] Less common forms of connection include employment of MPs (7 firms), having MPs as shareholders (9 firms), or having MPs on the board of directors (3 firms).

Measuring connections to MPs is not without drawbacks. By focusing on data from the *Register*, it is possible to capture only a specific type of political connection. Other channels through which a firm might wield political influence are ignored. For example, a firm may be able to positively affect its allocation through influence at the particular agencies directly involved in the allocation process. There is no way to quantify such influence.[35] However, although it is incomplete, the data in the *Register* is plausibly representative in that a firm connected to MPs is likely to also be influential in other domains and faces a relatively lower cost of engaging in rent-seeking activities.

Another concern is that instead of being simply an indicator of a firm's cost of lobbying, the cultivation of political connections may represent an endogenous response to the allocation process. To mitigate this concern, I do not consider

[34] For example, MP Peter Hain (Labour) attended Wimbledon on July 4, 1999, as a guest of British Petroleum.

[35] The UK does not systematically collect and release data on the financial connections of employees from any of the involved agencies.

instances of political connections created after the release of the provisional plan. The tight time horizons under which the E.U. ETS came into being also help to rule out the possibility that the pursuit of higher permit allocations was driving the formation of political connections. According to Ellerman et al. (2007), as of late 2001 and for some time after, an operational E.U.- wide emissions trading scheme by 2005 was widely viewed as a low probability scenario. A political agreement on the E.U. ETS among the then 15 member states was reached only in summer of 2003. Furthermore, as suggested by the growing literature on the topic, political connections can secure a range of benefits for firms in various regulatory contexts. The decision of a firm to cultivate connections takes into account the full range of these benefits, which extend far beyond costless permits. In the short run, political connections can be reasonably interpreted as indicative of the ease with which a firm can undertake rent-seeking.

Table 1.3 compares the 47 (privately owned) firms connected to at least one MP, with the 200 privately owned firms that are not connected to any MP.[36]

The data on 2002 emissions reveal that the politically connected firms are on average larger emitters, with the 47 connected firms accounting for well over half of the total 2002 emissions. However, there are small and large emitters among both

[36] Although the matched installations represent a total of 270 firms, the comparison in Table 1.3 excludes universities, hospitals, and government entities and hence covers only 247 firms. Universities, hospitals, and government entities account for less than 0.5% of the cap in both plans.

the connected and non-connected firms.[37] Because permit allocations are based on historical emissions, the 47 connected firms unsurprisingly received the bulk of the permits in both the provisional and final plans. What is notable however, is the redistribution of permits toward the connected firms. In the transition from the provisional to the final plan, connected firms gained 2,630,344 permits, while non-connected firms lost 2,496,817 permits. In percentage terms, firm permit allocations increased by an (unweighted) average of 32.67%, with non-connected and connected firms experiencing average increases of 34.22% and 26.08% respectively. However, these unweighted averages disproportionately reflect the influence of small emitters whose gains in permits were small in absolute terms, but large relative to their provisional allocations. As demonstrated in Figure 1.1, small emitters, though numerous, account for only a small fraction of total emissions. The average percent change, weighted by the firms' provisional allocatons, better reflects the reallocation that occured between the provisional and final plans. By this metric, firms on average experienced negligible change in allocation (0.06%). However, connected firms gained an average 2.2% while non-connected firms lost an average of 2.7%.

[37] Among connected firms, emissions in 2002 ranged between 1,110 tons and 28,439,827 tons. Among non-connected firms, emissions in 2002 ranged between 57 tons and 19,348,748 tons.

1.4.2 Results

Distributional Effects of Political Connections

The empirical specifications are motivated by equation (1.13). In the most basic specification, the natural log of a firm's realized allocation is regressed on the natural log of its provisional allocation and a measure of its political connections. Formally, for firm i,

$$ln(Final\ Allocation_i) = \beta_1 ln(Provisional\ Allocation_i) + \beta_2 Political_i + \epsilon_i, \quad (1.15)$$

where ϵ_i denotes the stochastic error term. The purpose of the *Political* variable is to shed light on ψ_i from equation (1.13), which indicates the additional permits gained by a firm with a lower cost of rent-seeking than the non-lobbying firms. Further specifications also include industry dummy variables and control for other firm characteristics. In the preferred specifications, the observation for each firm is weighted by the firm's provisional allocation. This approach addresses the issue of scale that is evident in Figure 1.1 and Table 3.[38]

[38] Such weighting makes my results comparable to those of Khwaja and Mian (2005), who analyze the effect of political connectedness on firm default rates on loans from state-owned banks in Pakistan. Their unit of observation is a firm-bank pair, and they weight each observation by the number of dollars loaned by the bank to the firm.

Table 1.4 displays the regression results using a binary measure of political connections. The variable $Political_i$ takes on a value of 1 if firm i is connected to at least one MP. Column 1 includes only the provisional allocation and the *Political* variable as regressors, while columns 2 and 3 respectively add industry controls and other firm characteristics. The firm characteristics included are 2003 values of the natural log of fixed assets, natural log of the number of employees, and profit margin.[39] Across all columns, the provisional allocation strongly predicts the realized allocation; the coefficient on $ln(Provisional\,Allocation_i)$ is slightly less than 1. The predictive power of the provisional allocation is also reflected in the extremely high R^2 values.

Using the binary measure of political connections, I find at best weak evidence that politically connected firms benefited in the redistribution of permits. While the coefficient on *Political* is positive, it becomes statistically insignificant with the inclusion of industry controls and firm characteristics.

In the regressions of Table 1.5, the *Political* variable is measured by the number of MPs a firm is connected to and its square. As in Table 1.4, column 1 includes only the provisional allocation and the political variables as regressors, and columns 2 and 3 respectively add industry controls and other firm characteristics.

[39] These are obtained from the Amadeus database published by Bureau van Dijk. See `http://www.bvdinfo.com/Products/Company-Information/International/AMADEUS.aspx`.

The results from Table 1.5 suggest that the degree of connectedness matters. Moving from no connections to a connection with one MP is associated with at least a 3.3% increase in the final allocation, and this amount is even higher (5.4%) when not accounting for firm characteristics and industry.[40] The negative coefficient on the quadratic term suggests diminishing returns from connections to additional MPs. Unlike the results in Table 1.4, the results using the number of MPs are statistically significant and relatively stable in magnitude across columns. The binary measure of political connectedness fails to account for what appear to be important differences across firms in the strength of connectedness.[41]

Table 1.6 reproduces the specifications of Table 1.5, but adds a multiplicative interaction term suggested by the theory. Specifically the interaction term is the product of the number of MPs firm i is connected to and $\frac{1}{Provisional\ Allocation_i}$. The coefficient on the interaction term has the opposite sign as suggested by the theory. However it is never statistically significant, and its inclusion does not materially alter the results.

I also estimate the regressions in Table 1.5 using an unweighted regression, however the results fail to attain statistical significance and are unstable. (See

[40] I also find evidence that connections to MPs are associated with a higher probability of an upward revision (i.e. a realized allocation higher than the provisional one).

[41] Number of connections seems to be the only measure of "strength" that matters. Distinguishing between types of connections (e.g. gifts vs. shareholdings vs. positions on boards of directors) does not yield significant results, nor do the results differ if connections are broken down by political party (e.g. Labour vs. Conservative).

Table B.1 in Appendix B.) The differences between the unweighted and weighted regressions suggest that large firms, which account for the bulk of emissions, are the ones who are able to use political influence to increase their allocations.

Furthermore, I repeat the weighted and unweighted regressions measuring final and provisional allocations in levels rather than logs. Results from weighted and unweighted regressions using levels are reported in Appendix B, tables B.2 and B.3, respectively. The weighted results are statistically significant and are qualitatively consistent with those of Table 1.5. Moving from no connections to a connection with one MP is associated with over 200,000 extra permits on average, representing a 0.07 standard deviation increase in the final allocation. The negative coefficient on the quadratic term suggests diminishing returns to additional MPs. The results from the unweighted regression are statistically insignificant but are qualitatively similar.

Calculation of Welfare Loss

The significant benefits associated with political connections suggest distributional consequences of rent-seeking activity during the allocation procedure in Phase 1 of the E.U. ETS in the U.K.. The reallocation that occurred between the provisional and final allocation plans appeared to have particularly benefited firms with strong political connections. The theoretical framework provides a basis to calculate the welfare losses from efforts wasted in contesting permits. Under full dissipation of rents, the amount firms spent on rent-seeking activity is equal to the value of the contested permits. The value of contested permits is obtained by multiplying the number of contested permits (ϕ) by the expected price of a permit at the time of allocation. Thus any attempt to calculate welfare losses must begin by characterizing the number of contested permits.

Based on the theoretical framework, it is possible to obtain a lower bound on the number of contested permits solely by examining the data on provisional and final allocations, without assuming anything about the relative costs of lobbying firms face. While the net change in the total number of permits between the two plans was negligible (133,527 permits, see Table 1.3), some firms lost permits (losers) while others gained permits (gainers). In particular, the gainers gained 13,862,086 permits, while the losers lost 13,728,559 permits. The losses of the losers constitute a lower bound on the number of contested permits. In terms of the theoretical framework, the observation of losers losing 13,728,559 permits (and gainers gaining virtually the same amount) is incompatible with there being fewer than 13,728,559 contested permits. Such an observation does not preclude higher numbers of contested permits; indeed it is still possible that entire cap was contested. However, it cannot be the case that fewer than 13,728,559 permits were contested.[42] Multiplying this number by an expected permit price in 2004 of 10 euros (Ellerman and Joskow, 2008) yields a lower bound on the welfare losses from rent-seeking (137,285,590 euros), assuming full dissipation of rents.

Another way to infer the number of contested permits is to use the estimated coefficient on $ln(Provisional\,Allocation)$. This coefficient ($\beta_1$ in equation (1.15)) corresponds to the quantity $(1-\gamma)$ from the theoretical model. An estimate of ϕ, denoted $\hat{\phi}$ can be calculated as follows:

$$\hat{\phi} = \sum_{i=1}^{n} \left[A_i - A_i^{\hat{\beta}_1} \right], \tag{1.16}$$

where $\hat{\beta}_1$ is an estimate of $1-\gamma$ and A_i is firm i's provisional allocation. Using $\hat{\beta}_1 = 0.993$ (Table 1.6, Column 1) and computing the expression (1.16) yields 22,189,151 as an estimate of the number of contested permits and 221,891,510 euros as the estimated welfare loss. This loss substantially exceeds the lower bound. Even higher estimates of the number of contested permits and corresponding welfare losses emerge if the value of $\hat{\beta}_1$ is taken from the specifications that control for firm characteristics and industry (Table 1.6, Columns 2 and 3). However, the theory does not suggest the inclusion of these controls and the estimated welfare losses may be implausibly high.

[42]For example, if the number of contested permits was zero, there would be no gainers or losers and allocations would remain unchanged.

The welfare losses from rent-seeking (137,285,590 euros or 221,891,510 euros) are relatively small compared to the value of the the cap, which is over 2.1 billion euros. However, the losses are staggering when juxtaposed against the amount firms spent annually on abatement of emissions. While no estimates exist of abatement or abatement costs for the U.K. as a whole, it is possible to compare the welfare losses with E.U.-wide abatement expenditures. Ellerman et al. (2010) estimate that Phase 1 of the E.U. ETS led to between 40 million and 100 million tons of abatement annually across all member states at a total cost of 450 million to 900 million euros. Thus the welfare losses from rent dissipation in the U.K. *alone* are substantial relative to annual abatement expenditures in the *entire E.U.*.

The discussion of welfare losses has assumed full-dissipation of rents. The theoretical framework predicts full dissipation rents only when there are a sufficiently large number of lobbying firms. The data do suggest that the number of lobbying firms is plausibly large enough to lead to full dissipation. For example the number of firms whose final allocation exceeds the provisional allocation is 160 ($k = 160$). The number of firms connected to at least one MP is 47 ($k = 47$). In either of the cases, $\frac{k-1}{k}$ is very close to one, which generates nearly complete rent-dissipation. It should be emphasized however, that the calculations of welfare loss presented here are valid only in the case of full dissipation.

1.5 Conclusion

This paper uses unique data on allocations from Phase 1 of the E.U. ETS in the U.K. to characterize the distributional and efficiency consequences of rent-seeking behavior in the context of costless emission permits. The evidence suggests that firms connected to MPs were able to improve their allocations and that the degree of connection, as measured by the number of MPs a firm was connected with, mattered. The welfare losses from rent-seeking behavior represent a significant cost over and above the abatement costs firms incurred to reduce their emissions.

Considering that Phase 1 was a trial phase of the E.U. ETS, it is plausible that rent-seeking behavior was more of a factor than in the subsequent phases. As the rules and regulatory procedures became more established over time, opportunities and incentives for rent-seeking diminished. Duggan (2009) notes that the formulation of the U.K.'s National Allocation Plan in Phase 2 involved far less agitation on the part of industry. The welfare loss estimate can be reasonably construed as a one-time loss rather than an ongoing loss. However, it does offer a cautionary tale for countries with institutions less effective at curbing rent-seeking activity. If rent-seeking can occur even in a developed country with strong insitutions like the U.K., it is likely to play a much bigger role as emissions trading is implemented in developing countries like China and India.[43]

[43] See Liu (2013) and Duflo et al. (2010).

My results lend support to the use of auctioning as an allocation method rather than grandfathering. Auctioning avoids rent-seeking over costless permits and also has an efficiency advantage in that the auction revenues can be used to offset distortionary taxes.[44] However, auctions are not entirely free of political economy problems. Cramton and Kerr (2002) point out that vested interests will fight bitterly to oppose auctions in favor of grandfathering. MacKenzie and Ohndorf (2012) point out that the revenues raised from auction may themselves become a rent-seeking prize.

[44] See Goulder et al. (1999) and Cramton and Kerr (2002).

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Table 1.1: Scope of Provisional and Final National Allocation Plans

	Provisional Plan	**Final Plan**
Number of Installations	867	1056
Number of Matched Installations	703	703
Total Permits to All Installations	224,575,161	228,204,110
Total Permits to Matched Installations	214,258,348	214,113,670

Table 1.2: Gains and Losses at Sector Level

Sector in Provisional Plan	*Number of Installations*	*Allocation (Provisional)*	*Allocation (Final)*	*Change*
Bricks/Ceramics	98	2,788,687	1,640,357	-1,148,330
Cement	13	9,084,646	7,598,514	-1,486,132
Chemicals	79	6,981,427	6,661,667	-319,760
Food & Drink	97	3,119,708	3,249,503	129,795
Glass	27	1,673,063	1,732,602	59,539
Iron & Steel	12	21,489,003	19,915,330	-1,573,673
Lime	8	2,117,815	2,204,131	86,316
Non-ferrous	2	2,446,757	2,984,474	537,717
Offshore	113	10,826,881	15,918,647	5,091,766
Other Combustion Activities	77	2,289,186	2,284,408	-4,778
Power Stations	86	129,502,676	124,184,409	-5,318,267
Pulp & Paper	77	4,138,668	5,049,444	910,776
Refineries	14	17,799,831	20,690,184	2,890,353
TOTAL	**703**	**214,258,348**	**214,113,670**	**-144,678**

Table 1.3: Connected vs. Non-connected Firms

	Not Connected	*Connected*	*Total*
Number of Firms	200	47	247
2002 Emissions (tons)	101,455,365	138,441,587	239,896,951
Provisional Allocation	92,616,052	120,595,201	213,211,263
Final Allocation	90,119,235	123,225,555	213,344,790
Change (Permits)	-2,496,817	2,630,344	133,527
Change (Percent, unweighted mean)	34.22%	26.08%	32.67%
Change (Percent, mean weighted by provisional allocation)	**-2.7%**	**2.2%**	**0.06%**

Excludes universities, hospitals, and other government entities.

Table 1.4: Regressions with Binary Political Variable

Dependent Variable: *Ln(Final Allocation)*	*(1)*	*(2)*	*(3)*
Ln(Provisional Allocation)	0.995***	0.958***	0.980***
	(0.030)	(0.059)	(0.026)
Political	**0.131***	**0.142**	**0.055**
	(0.068)	**(0.124)**	**(0.045)**
ln(Total Fixed Assets)			0.033***
			(0.009)
ln(Employees)			-0.024
			(0.018)
Profit Margin			0.347**
			(0.144)
Industry Controls?	No	Yes	Yes
Weighted Regression?	Yes	Yes	Yes
N	247	247	185
R^2	0.90	0.92	0.99

Excludes universities, hospitals, and other government entities. Observations weighted by provisional allocation. Standard errors (in parentheses) are clustered by industry. The superscripts ***, **, and * denote significance at 1, 5, and 10 percent levels respectively.

Table 1.5: Regressions with Number of Connected MPs

Dependent Variable: *Ln(Final Allocation)*	*(1)*	*(2)*	*(3)*
Ln(Provisional Allocation)	0.995***	0.966***	0.960***
	(0.025)	(0.036)	(0.031)
Number of MPs	**0.054****	**0.036***	**0.033*****
	(0.021)	**(0.018)**	**(0.009)**
(Number of MPs)^2	**-0.0021****	**-0.0014***	**-0.0012*****
	(0.0008)	**(0.0007)**	**(0.0003)**
ln(Total Fixed Assets)			0.029***
			(0.008)
ln(Employees)			-0.018
			(0.009)
Profit Margin			0.424**
			(0.176)
Industry Controls?	No	Yes	Yes
Weighted Regression?	Yes	Yes	Yes
N	247	247	185
R^2	0.90	0.91	0.99

Excludes universities, hospitals, and other government entities. Observations weighted by provisional allocation. Standard errors (in parentheses) are clustered by industry. The superscripts ***, **, and * denote significance at 1, 5, and 10 percent levels respectively.

Table 1.6: Regressions with Interaction Term

Dependent Variable: *Ln(Final Allocation)*	*(1)*	*(2)*	*(3)*
Ln(Provisional Allocation)	0.993***	0.965***	0.952***
	(0.027)	(0.037)	(0.032)
Number of MPs	**0.055****	**0.037***	**0.037*****
	(0.021)	**(0.019)**	**(0.009)**
(Number of MPs)^2	**-0.0021****	**-0.0014***	**-0.0013*****
	(0.0008)	**(0.0007)**	**(0.0003)**
Interaction Term	-1048.126	-1316.413	-3403.007
	(1560.926)	(875.517)	(1801.976)
ln(Total Fixed Assets)			0.027**
			(0.009)
ln(Employees)			-0.013
			(0.010)
Profit Margin			0.463**
			(0.178)
Industry Controls?	No	Yes	Yes
Weighted Regression?	Yes	Yes	Yes
N	247	247	185
R^2	0.90	0.91	0.99

Interaction term is the product of $Number\,of\,MPs$ and $\frac{1}{Provisional\,Allocation}$. Excludes universities, hospitals, and other government entities. Observations weighted by provisional allocation. Standard errors (in parentheses) are clustered by industry. The superscripts ***, **, and * denote significance at 1, 5, and 10 percent levels respectively.

Chapter 2

Institutions and the Resource Curse within the United States

2.1 Introduction

The term "resource curse" has been widely used to describe the notion that areas rich in natural resources tend to be poor and often politically oppressed. The idea of a resource curse is paradoxical in that it appears to violate standard economic theory, which unambiguously predicts only beneficial effects from greater resource abundance. However, a cursory glance at the world map reveals numerous societies that seem to be cursed by their resource wealth. Angola, Congo, Nigeria, Venezuela, and the Middle East are particularly noteworthy examples of places that are rich in natural resources, but also characterized by low to negative GDP growth, vast income inequality, state failure, civil war, corruption, and political oppression. On the other hand, the more favorable economic and political outcomes in other resource-rich countries such as Norway, Canada, Australia, Malaysia, and Botswana suggest that the resource curse is neither universal nor unavoidable.

Several theoretical papers argue that the quality of a society's institutions is critical to whether resource wealth becomes a curse or not, and this idea has been corroborated by empirical work. However, most of the empirical work on the resource curse has relied upon cross-country regressions, with one observation per country. This approach is problematic; any results indicating a resource curse are open to skepticism as they may be driven by country-specific unobservable factors that are correlated with resource wealth. In this paper, I examine the resource curse hypothesis, including its institutional dimension, using cross-sectional data from political subdivisions (states) within a single country (the United States). This strategy eliminates cross-country heterogeneity.[1] Furthermore, a within-country study allows the researcher to dispense with certain explanations for the resource curse, such as those involving an exchange rate mechanism, while focusing on the potential of other explanations (e.g. institutions).

As I explain in detail later, the U.S. presents a fruitful venue for studying the resource curse. The variations in resource wealth, economic performance, and institutional quality across U.S. states are salient enough to provide insight

[1] Although unobserved cross-state heterogeneity remains, it is arguably less problematic than cross-country heterogeneity.

into the role of institutions in the resource curse phenomenon. In spite of this, little work has been undertaken on the topic. Papyrakis and Gerlagh (2007) were the first to analyze the resource curse hypothesis with respect to U.S. states, and indeed the first to abandon the dominant cross-country paradigm. They find evidence that resource wealth is correlated with lower economic growth among the cross-section of U.S. states. I build on the work of Papyrakis and Gerlagh (2007) by explicitly considering the role that institutional quality plays in mediating the effects of resource wealth on economic outcomes. This is accomplished by including a multiplicative interaction term between resource wealth (as measured by share of mining in gross state product) and institutional quality (as measured by per capita political corruption convictions) in the regression specifications.[2] In contrast to Papyrakis and Gerlagh (2007), I find robust evidence that among the cross-section of U.S. states, resource wealth in itself is associated with higher economic growth. However, I also find evidence that poor institutional quality can erode or even reverse the advantages of resource wealth. This result is consistent with earlier theoretical and cross-country empirical work arguing that the resource curse is essentially an institutional phenomenon.

The remainder of the paper is organized as follows: Section 2 summarizes previous work on institutions and the resource curse and argues that the U.S.

[2]The inclusion of such an interaction term was first advocated by Mehlum et al. (2006) in a cross-country framework.

provides a valuable setting in which to study this topic; Section 3 presents the empirical strategy and results; Section 4 offers concluding remarks.

2.2 Economic Performance, Resources, and Institutions: the U.S. Context

2.2.1 Institutions and the Resource Curse

Recent theoretical literature on the resource curse has focused on institutional explanations, which stand in contrast to earlier, market-based explanations such as the "Dutch Disease" theory. Formulated to explain the poor economic performance of the Netherlands following the discovery of oil in the North Sea, this theory postulates that a natural resource boom causes a country's currency to appreciate, harming the competitiveness of its manufacturing exports. Dutch disease proponents see manufacturing exports as the engine of growth; according to their view, a resource boom that crowds out manufacturing exports will retard growth (Deacon and Rode 2012). Sachs and Warner (1997, 2001), who provided some of the earliest empirical evidence for the resource curse, emphasize the Dutch Disease explanation. However, as Bulte et al. (2005) point out, the Dutch Disease theory cannot account for the numerous exceptions to the resource curse or the highly varied growth experiences of resource-rich countries. It also cannot explain a widely observed empirical regularity that the resource curse only applies to spatially concentrated natural resources such as mineral and oil deposits, while spatially diffuse (e.g. agricultural) resources seem to be immune.[3] Moreover, the theory cannot apply to the case of U.S. states as they share a common currency and real prices vary relatively little among them (Goldberg et al., 2008).

The idea that natural resource wealth can be a rent-seeking prize plays a central role in institutional explanations for the resource curse. It has been widely argued that governance institutions ineffective at curbing rent-seeking activity underpin the failures of societies to realize benefits from their natural resource wealth (Congleton et al., 2008). In settings with ineffective institutions, economic agents are drawn into a common pool competition over resource rents that ultimately translates into welfare losses. This competition not only leads to full dissipation of the rents, but actually *lowers* welfare by drawing inputs away from productive economic activity (Deacon and Rode, 2012). Important theoretical papers linking resource wealth to welfare losses *via* such a mechanism include Lane and Tornell (1996), Tornell and Lane (1999), Torvik (2002), Mehlum et al. (2006), and Hodler (2006).[4] The logic of rent-seeking is consistent with the

[3] Leite and Weidmann (1999), Isham et al. (2003), and Boschini et al. (2005) all report this result.

[4] Deacon and Rode (2012) provide a survey of theoretical and empirical research at the intersection of rent-seeking and the resource curse. Deacon (2012) surveys political economy aspects of the resource curse more generally.

observation that the resourse curse applies specifically to spatially concentrated resources. The rents from these resources are a readily available and easily tapped source of revenue for governments. In the absence of strong institutional barriers, economic agents find it optimal to compete for a share of this revenue rather than engage in productive economic activity, and a clientelistic political economy takes hold.

Circumstantial evidence, for example from oil-rich Venezuela and Nigeria, bears out the notion that political elites vying for access to resource rents can lead governments to make inefficient economic decisions and ultimately cripple economic growth.[5] During the oil price spike of 1979-81, Venezuela's public spending on infrastructure and industrial subsidies, which mainly accrued to political elites, increased so sharply that the country actually registered a current account deficit. The country's per capita output fell by an average of 1.4% per year between 1970 and 1990 despite a run-up in the price of oil over this period (Lane and Tornell, 1996, pg. 216). In Nigeria, government transfers as a share of GDP more than doubled between 1970 and the early 1980s (Tornell and Lane, 1999). The dramatic rise in income inequality during an extended period of rising oil prices (1970-early 2000's) suggests that these transfers enriched political elites

[5]In extreme cases the struggle to capture natural resource wealth can turn into violent conflict, as exemplified by civil wars in Angola, Nigeria, Sierra Leone, and the former Zaire (Deacon, 2012). Ross (2006) surveys the literature on natural resource wealth and violent conflict. Collier and Hoeffler (1998, 2004) make key empirical contributions in this area.

rather than benefiting the country at large. In 1970, the income of the top 2% of the population equalled that of the bottom 17%; by 2000 the top 2% earned as much as the bottom 55% (van der Ploeg, 2011, pgs. 367-8). At the same time Nigeria's per capita GDP was 30% lower in 2000 than in 1965 (Heston et al., 2002). In contrast, the superior economic performance of another oil-rich country, Norway, over the same period can be attributed at least in part to institutions that were comparatively effective at keeping rent-seeking activity in check (Deacon, 2012).

The critical role of institutional quality has found support in empirical work such as that of Mehlum et al. (2006).[6] Using data from a cross-section of countries, these authors find that natural resource wealth is correlated with slow economic growth when institutional quality is low but do not find evidence for a resource curse in countries where institutional quality is high. They highlight the need to include a multiplicative interaction term between resource wealth and institutional quality in order to allow the effects of resource wealth to vary by institutional

[6] Lane and Tornell (1996) and Collier and Goderis (2009) also empirically validate the importance of institutional quality.

quality.[7] I follow the basic approach of Mehlum et al. (2006) in testing the resource curse hypothesis in the context of U.S. states.[8]

2.2.2 The U.S. Context

Figure 2.1 reveals a negative relationship between economic growth and natural resource wealth across U.S. states. The vertical axis plots the average annual growth in a state's real per capita Gross State Product (GSP) between 1987 and 2000; the horizontal axis plots the share of mining in 1987 GSP. While Figure 2.1 does not in itself constitute evidence of a resource curse[9], the negative relationship between a state-level measure of resource wealth and long-run economic performance mirrors the similar relationship observed across countries.[10] Any study

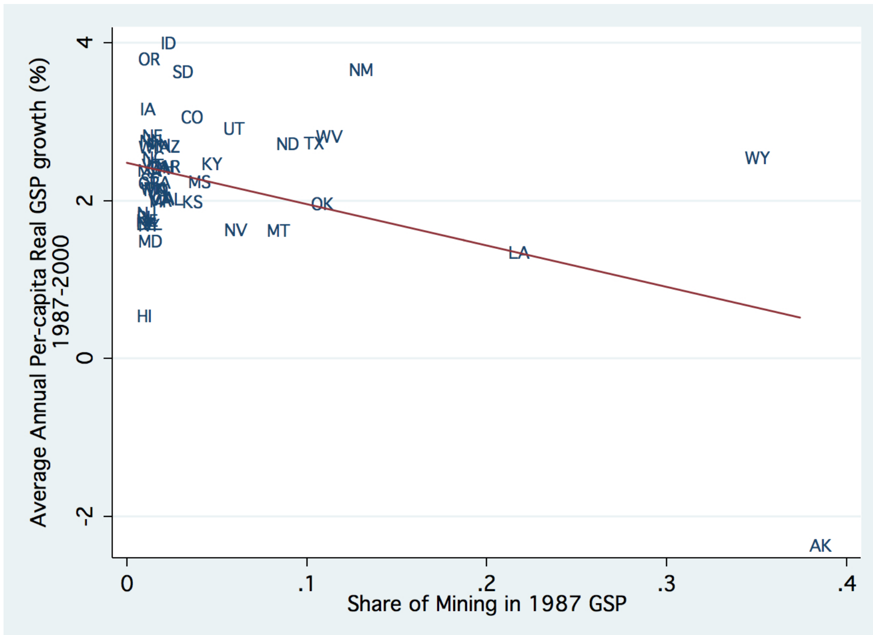
[7]Mehlum et al. (2006) augmented the empirical specification of Sachs and Warner (2001) by including the interaction term. Their outcome variable is a country's growth in per capita income between 1970 and 1990. They represent institutional quality by an index that combines ratings (from the International Country Risk Guide) on corruption in government, risk of contract repudiation, risk of expropriation, bureaucratic quality and rule of law. Following Sachs and Warner (2001), they represent natural resource wealth by the share of primary products in the country's exports. Other control variables include initial income, openness to trade, and investment to GDP ratio.

[8]The institutional literature on the resource curse has recognized that institutions are not immutable and that a windfall of resource wealth can alter institutions for the worse by making them more conducive to rent-seeking and otherwise weakening property rights, democracy, or political stability (Aslaksen and Torvik, 2006; Hodler, 2006; Mehlum et al., 2006; Robinson et al., 2006). This is particularly true if institutions were already weak prior to the windfall. Neither Mehlum et al. (2006) nor this paper empirically test this type of a prediction. However Caselli and Michaels (2013), Tsui (2010), and Vicente (2010) do provide empirical evidence suggesting institutional impairment in the wake of a resource windfall. The effect of resource windfalls on institutional quality in U.S. states is a promising avenue for future research.

[9]It should be noted that the downward slope of the best-fit line in Figure 2.1 is driven by three states (Louisiana, Wyoming, and Alaska) whose economies heavily depend on mining.

[10]See Figure 1 in Sachs and Warner (2001) for a cross-country plot.

Figure 2.1: Economic Growth and Natural Resource Wealth in U.S. States



of the resource curse phenomenon in the U.S. context can thus complement the already extensive body of cross-country studies.

In addition to the variations in resource wealth and long-run economic growth, measures of corruption suggest that U.S. states also differ considerably in the quality of their institutions. The U.S. Department of Justice publishes annual data on the number of federal, state, and local public officials in each state convicted in federal court of a corruption related crime. Because the data covers corruption convictions in a federal rather than a state court, it is relatively uncontaminated by state-level judicial corruption and is therefore comparable across states. While a state's year-to-year fluctuations in convictions may reflect either increased corruption or a "clean-up" effort, it can be plausibly argued that an average of corruption convictions (normalized by population) over a long time span suitably captures the friendliness of the state's institutions to corruption/rent-seeking activities (Grooms, 2012). In their pioneering analysis of the resource curse in the U.S. context, Papyrakis and Gerlagh (2007) use per capita corruption convictions between 1990 and 2000 as a proxy for state-level institutional debility. While they include this proxy as a control variable, I extend their analysis by also interacting it with the measure of natural resource wealth.

Table 2.1 displays the top 10 and bottom 10 states in average per capita corruption convictions between 1976 and 2008. The most corrupt state (Alaska) has on average more than 7 times the per capita corruption convictions as the least corrupt state (Oregon). The data in Table 2.1 broadly conform to *a priori* perceptions about which states are relatively more corrupt. What is particularly striking is that the ten most corrupt states incude several whose economies are centered on natural resource extraction. Indeed five of these states (Alaska, Louisiana, Montana, North Dakota, and Oklahoma) also rank in the top 10 by share of mining in 1987 GSP. However, not all resource-rich states rank high in corruption. For example, Utah and Nevada have relatively high shares of mining in 1987 GSP (around 5% in both states), but respectively rank 5th and 15th from the bottom in corruption.

This paper is not the first to propose that a resource curse might operate within the U.S. through an institutional channel. In public policy debates during the first three decades of the twentieth century, when the U.S. was the major global oil producer, it was not uncommon to associate oil production with "misdirected investment, low growth, and spectacular waste" (Goldberg et al., 2008, p. 498). For instance, Ise (1928) observed that oil wealth fostered misallocation of capital, investment in rent-seeking activities, and an unequal distribution of income. This type of discourse predated any formal academic study of the resource curse by several decades.

Anecdotally, the oil-rich state of Louisiana has been cited as an example of a "resource-cursed" state within the U.S.. A recent article in the *Washington Post* notes that "instead of blessing Louisiana with prosperity, the oil industry fostered dependency, corruption and an indifference to environmental damage". Through much of the twentieth century, Louisiana's government was dominated by members of the Long family, who engaged in clientelistic distribution of oil royalties (Mufson, 2010) *via* a system of control that "more nearly matched the power of a South American dictator than that of any other American state boss" (Key, 1949, p. 156). Despite its oil wealth, Louisiana ranks next to last among U.S. states in life expectancy, infant mortality, and percentage of the population living below the poverty line. It also ranks fourth in the violent crime rate, sixth in income inequality[11], and forty-sixth in the percentage of people over 25 with a college degree (Mufson, 2010).[12]

Aside from anecdotal accounts, a handful of recent academic studies have focused on the institutional aspects of the resource curse in U.S. states.[13] Goldberg et al. (2008) document a negative relationship between resource wealth and economic growth in U.S. states and also present evidence that resource-rich states tend to have less competitive politics than other states. They argue that resource rents allow incumbent politicians to remain in power by purchasing clientelistic support while keeping direct taxes on citizens low.[14] Dunn (2008) finds that natural resource wealth is associated with lower welfare in U.S. states.[15] She argues for an institutional explanation for this "resource curse", noting that a state's resource wealth is positively associated with several variables that could signal rent-seeking activity, including the number of law firms per capita and number of political organizations per capita. Finally, in a result mirroring that

[11] See Adelson (2012).

[12] Gylfason (2001) postulates that natural resource wealth can cause a nation to neglect investment in human capital. Birdsall et al. (2001) also posit a link between natural resource wealth and low investment in education, but through a political channel.

[13] Not all studies of the resource curse hypothesis in the U.S. have focused on institutional aspects. Aadland and James (2011) find evidence of a resource curse among U.S. counties, but do not consider the possibility of county-level institutional differences.

[14] This argument hearkens back to the rentier state theory. See Ross (1999) for a review of the rentier state theory.

[15] Several measures of welfare are used, including the poverty rate, health insurance coverage, birth weight, infant mortality, and income inequality. As in Papyrakis and Gerlagh (2007), resource wealth is measured by the share of GSP in the primary sector.

of Mehlum et al. (2006), Corey (2009) demonstrates that economic freedom can mitigate the negative growth effects of resource wealth in U.S. states.[16]

2.3 Empirical Strategy and Results

2.3.1 Natural Resource Wealth and Economic Growth

The starting point of my empirical specifications is the landmark study of Papyrakis and Gerlagh (2007). In Table 2, I attempt to reproduce their results using currently available data from the Bureau of Economic Analysis (BEA).[17] Their basic specification is

[16] Corey (2009) uses cross-sectional data on U.S. states; a state's level of economic freedom is represented by an index published by the Fraser Institute (Karabegovic et al., 2008). Sobel (2008) claims that the economic freedom index captures a state's institutional quality in that it is positively correlated with various measures of productive entrepreneurship (i.e. birth rate for large firm establishment, birth rate for total firm establishment, venture-capital investment per capita, patents per capita, and birth rate of sole proprietorships) and negatively correlated with measures of rent-seeking behavior (i.e. lobbying organizations per capita).

[17] Since the work of Papyrakis and Gerlagh (2007) the BEA has changed the way it calculates GSP, and figures from previous years were changed to reflect the new methodology. As currently reported, GSP figures from years 1997 and later are based on the North American Industry Classification System (NAICS) while those from 1997 and earlier are based on the Standard Industrial Classification (SIC) system. (See `http://www.bea.gov/regional/docs/product/methods.cfm`.) The use of one system versus another makes a difference not only in the breakdown of GSP by industry, but also in the figures for total GSP (personal communication: Wang, 2012). Because GSP figures for 1997 are reported according to both systems, it is possible to "bridge" the discontinuity. For example, an SIC-based GSP for 1998 could be constructed for a given state by applying a growth rate (derived from the reported, NAICS-based 1997 and 1998 GSP figures) to the reported, SIC-based GSP for 1997. This type of conversion has been done by other U.S. government agencies (see for example the Energy Information Administration (EIA): `http://www.eia.gov/state/seds/sep_use/notes/use_gdp.pdf`) and is also used in this paper.

$$\begin{aligned} Growth_{1987-2000} = \quad & \alpha + \gamma * LnY_{1987} + \beta * Natural\, Resource\, Wealth + \\ & \psi * Corruption + \lambda * x + \epsilon. \end{aligned} \tag{2.1}$$

The dependent variable is average annual percentage growth rate of real per capita GSP between 1987 and 2000.[18] The explanatory variables are natural log of the initial real per-capita GSP in 1987 ($Ln\, Y_{1987}$), the share of mining in 1987 GSP (a proxy for natural resource wealth)[19], corruption convictions between 1991 and 2000[20] per 100,000 citizens (a proxy for institutional debility), and a vector x of other control variables including investment, schooling, openness, and research and development (R&D) spending[21] as a fraction of 1987 GSP. Investment is measured by the share of industrial machinery production in 1987 GSP. Schooling is measured by the share of educational services in 1987 GSP. Openness is measured by the sum of a state's net international migration for 1990-1999, divided by the state's population in 1990.[22] The constant term in

[18] Although Papyrakis and Gerlagh (2007) consider the years 1986-2000, the BEA's real per capita GSP series, as currently reported, begins only in 1987.

[19] Papyrakis and Gerlagh (2007) also use the share of primary products (a sector that encompasses mining of metals and fuels as well as agriculture, forestry, and fisheries) as a proxy for resource wealth. However, they point out that their results are driven by the mining sector. This is consistent with the notion that the resource curse applies only to spatially concentrated resources. Accordingly, I focus specifically on the share of mining rather than primary products.

[20] Data on state-level corruption convictions is obtained from the Public Integrity Section of the Criminal Division of the U.S. Department of Justice.

[21] Data on state-level R&D spending in 1987 is obtained from the National Science Foundation.

[22] Population and net international migration figures are sourced from the Census Bureau.

the regression is denoted by α, while ϵ denotes the idiosyncratic error term. A negative estimate of the coefficient on natural resource wealth (β) would suggest a resource curse.

The results displayed in Table 2.2 are roughly similar in magnitude to those of Papyrakis and Gerlagh (2007) and also exhibit the same general pattern across the different specifications.[23] The strongest evidence for a resource curse comes from the specification in column 2, in which only natural resource wealth and initial income are included as explanatory variables. Although the estimated coefficient on natural resource wealth is not statistically significant at conventional levels, it does represent an economically substantial effect. An increase in the share of GSP from mining by one standard deviation (0.077) decreases the annual growth rate by 0.27 percentage points (0.077*-3.48). The effect however, fails to hold as more explanatory variables are added; in columns 3-7, the coefficient on natural resource wealth becomes increasingly insignificant both from an economic and statistical standpoint. Papyrakis and Gerlagh (2007) construe this as evidence that the resource curse operates through indirect channels. They argue that natural resource wealth negatively affects economic growth *via* its impact on other explanatory variables and provide calculations suggesting that education is the most important transmission channel.

[23]See Table 1 in Papyrakis and Gerlagh (2007) for the analogous results.

2.3.2 The Mediating Role of Institutional Quality

In Table 2.3, I augment the regressions in Table 2.2 with an interaction term obtained by multiplying the variables for natural resource wealth and corruption. The necessity of incorporating such an interaction into empirical specifications was forcefully pointed out by Mehlum et al. (2006) and reiterated by Deacon (2012). The augmented specification is

$$\begin{aligned} Growth_{1987-2000} = \quad & \alpha + \gamma * LnY_{1987} + \beta * Natural\,Resource\,Wealth+ \\ & \psi * Corruption + \phi * Interaction + \lambda * x + \epsilon. \end{aligned} \tag{2.2}$$

Prior theoretical and empirical literature suggests that the coefficient on the interaction term (ψ) should be negative (i.e. natural resource wealth is more of a curse when institutions are weak.)

Inclusion of the interaction term leads to a striking reversal of the results in Table 2.2. For purposes of comparison, column 1 of Table 2.3 reproduces a basic regression without the interaction term. In the subsequent columns of Table 2.3 that do include the interaction term, the estimated coefficient on natural resource wealth (β) actually becomes positive and though it is not statistically significant, its magnitude remains relatively stable even with the addition of other explanatory variables.[24] As expected, the estimated coefficient on the interaction term (ψ) is consistently negative and is significant at the 10% level in columns 3 and 4. Moreover, the two estimated coefficients β and ψ are jointly significant at the 10% level in column 2.

The results in Table 2.3 suggest that among U.S. states, resource wealth does not appear be a curse in itself; if anything it is a blessing. In the absence of corruption, a one standard deviation increase in the share of GSP from mining (0.077) is associated with anywhere from a 0.38 to 0.57 percentage point increase in the annual growth rate, depending on the specification. Poor institutions can, however, substantially reduce or even eliminate these gains. By comparing the estimated coefficients on natural resource wealth and the interaction term, it is possible to characterize the level of corruption needed to eliminate the positive effect of natural resources. According to any of the specifications in Table 2.3, the positive effect is more than eliminated for states with 4 or more corruption convictions per 100,000 citizens during 1991-2000. Ten states meet this criterion, including a number of resource rich states such as Alaska, Louisiana, and the Dakotas.[25]

[24] Although the positive coefficients on natural resources are not statistically significant at conventional levels, the significance does not greatly diminish with the addition of other variables.

[25] The number of corruption convictions per 100,000 citizens during 1991-2000 ranges from 0.58 (in Colorado) to 7.49 (in Mississippi). The average across all states is 3.05 and the standard deviation is 1.61.

2.4 Concluding Remarks

This paper contributes to an understanding of the possible mechanisms that might explain the resource curse phenomenon in the U.S. context. An understanding of the mechanisms is essential if any effective policy recommendations are to be made. As Deacon (2012) points out, simply verifying that resource wealth is correlated with slow growth is of little practical value. On the other hand, showing that the resource curse is a consequence of weak institutions can spur policy discussions aimed at institutional reform.

Focusing on states within the U.S. has several advantages over previous cross-country, cross-sectional studies, which typically lump together observations from developing and developed countries. A within-country approach allows the researcher to avoid comparing the outcomes of countries that differ vastly in unquantifiable and unobservable ways. It also rules out the possibility of exchange rate-based mechanisms such as the Dutch Disease theory, while still leaving scope for institutional mechanisms, especially in a large and diverse country like the U.S.. My results from the U.S. suggest that the resource curse hypothesis and particularly the critical role of institutional quality are relevant even in a developed country context.[26]

[26] In this regard, my work contributes to a broader literature on the importance of cross-state differences in institutional quality within the U.S. (Glaeser and Saks, 2006; Leeson and Sobel, 2008; Grooms, 2012).

The beneficial effect of natural resource wealth on economic growth that I find in the absence of poor institutions is consistent with the historical experience of the U.S. as a whole. Several papers have stressed that natural resource wealth was integral to the growth of the American economy in the late nineteenth and early twentieth century. Wright (1990) ascribes the U.S. dominance in manufacturing at the turn of the 20th century to technological progress originating in the American mining sector. David and Wright (1997) and Wright (2001) note that mining enabled the establishment of prestigious educational institutions that diffused knowledge to other sectors. Walker (2001) and McClean (2005) argue that discoveries of gold and other natural resources propelled long-term economic development in the state of California. However, as a counterpoint to these narratives, I also find that the potential gains from natural resource wealth may not be realized in settings with weak institutions. This is consistent with theoretical models as well as with the experiences of resource-rich states such as Alaska and Louisiana and countries such as Venezuela and Nigeria. My results reinforce the importance of including an interaction term between the measures of resource wealth and institutional quality in any test of the resource curse hypothesis.

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Table 2.1: Most and Least Corrupt States

Average Corruption (per 100,000 citizens), 1976-2008

Lowest 10 states		Highest 10 states	
Oregon	0.084	Oklahoma	0.420
Washington	0.104	Montana	0.425
Minnesota	0.122	Alabama	0.446
New Hampshire	0.123	Illinois	0.452
Utah	0.134	Tennessee	0.472
Vermont	0.139	North Dakota	0.514
Iowa	0.141	South Dakota	0.539
Nebraska	0.142	Louisiana	0.587
Colorado	0.147	Mississippi	0.591
Wisconsin	0.165	Alaska	0.616

This table presents corruption convictions per capita averaged over 1976 to 2008. These data are from the U.S. Department of Justice's "Report to Congress on the Activities and Operations of the Public Integrity Section". (Source: Grooms, 2012)

Table 2.2: Regression Results without Interaction Term

Dependent Variable: $Growth_{1987-2000}$

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Constant	30.58	27.73	28.50	28.04	28.22	32.72	36.20
	(3.35)	(4.88)	(5.27)	(5.57)	(4.93)	(4.81)	(5.41)
$Ln\ Y_{1987}$	-2.81	-2.51	-2.55	-2.53	-2.55	-3.03	-3.41
	(-3.08)	(-4.46)	(-4.76)	(-4.63)	(-4.53)	(-4.40)	(-5.03)
Natural Resource Wealth		**-3.48**	**-2.54**	**-2.03**	**-1.95**	**-0.87**	**0.12**
		(-1.52)	**(-1.19)**	**(-0.91)**	**(-0.86)**	**(-0.39)**	**(0.05)**
Corruption			-0.16	-0.14	-0.14	-0.15	-0.12
			(-3.10)	(-2.50)	(-2.46)	(-2.65)	(-2.04)
Investment				9.22	8.96	12.56	15.22
				(1.83)	(1.71)	(2.26)	(3.07)
Schooling					4.51	8.71	10.63
					(0.24)	(0.49)	(0.59)
Openness						11.51	13.36
						(1.99)	(2.19)
R&D							9.01
							(1.22)
N	50	50	50	50	50	50	49

Heteroskedasticity robust t-statistics in parentheses (White, 1980). Data on R&D is unavailable for Delaware, hence $N = 49$ *in regression 7.*

Table 2.3: Regression Results with Interaction Term

Dependent Variable: $Growth_{1987-2000}$

	(1)	(2)	(3)	(4)	(5)	(6)
Constant	28.50 (5.27)	24.94 (4.98)	23.54 (4.75)	23.68 (4.69)	27.16 (3.99)	31.18 (4.32)
$Ln\ Y_{1987}$	-2.55 (-4.76)	-2.21 (-4.50)	-2.11 (-4.38)	-2.13 (-4.31)	-2.49 (-3.67)	-2.92 (-4.05)
Natural Resource Wealth	**-2.54** **(-1.19)**	**4.95** **(1.04)**	**7.30** **(1.45)**	**7.35** **(1.45)**	**6.37** **(1.17)**	**5.72** **(1.01)**
Corruption	-0.16 (-3.10)	-0.09 (-1.50)	-0.05 (-0.68)	-0.05 (-0.68)	-0.07 (-0.91)	-0.06 (-0.77)
Interaction		**-1.81** **(-1.63)**	**-2.21** **(-1.86)**	**-2.21** **(-1.85)**	**-1.82** **(-1.40)**	**-1.45** **(-1.11)**
Investment			12.56 (2.47)	12.35 (2.37)	13.92 (2.66)	15.83 (3.23)
Schooling				3.43 (0.21)	6.13 (0.37)	8.00 (0.47)
Openness					6.89 (1.16)	9.28 (1.42)
R&D						7.88 (1.01)
Joint Test		0.091	0.112	0.122	0.337	0.541
N	50	50	50	50	50	49

Heteroskedasticity robust t-statistics in parentheses (White, 1980). Data on R&D is unavailable for Delaware, hence N = 49 in regression 6. The penultimate row reports p-values for the test of joint significance of the coefficients on natural resource wealth and the interaction term.

Chapter 3

Estimating Treatment Effects with Interference (co-authored with Douglas G. Steigerwald)

3.1 Introduction

The bulk of the methodological and applied work on treatment effects has assumed that an individual's outcome may vary only with his own treatment. Rubin (1978) calls this the "Stable Unit Treatment Value Assumption" (SUTVA). However, in many contexts, such an assumption is untenable; an individual's treatment response can depend both on his own treatment as well as the treatments of others whom he interacts with. This phenomenon has been termed "interference" in the statistics literature and is salient in a wide range of settings.[1] For example, the probability of an individual contracting an infectious disease depends not

[1]See Sobel (2006), Rosenbaum (2007), and Hudgens and Halloran (2008).

only on his own vaccination status, but also on the vaccination of those whom he interacts with. Job-training programs are another setting where interference matters; the benefits from job-training are likely diminished if the training were extended to the entire workforce.[2]

We develop both a framework to conceptualize treatment effects in situations characterized by interference and a linear regression method to estimate these effects. Our framework distinguishes between *direct* treatment effects (due to a change in an individual's treatment status) and *indirect* treatment effects (due to changes in the treatment status of others). If a researcher can observe at least two, non-overlapping groups within which individuals interact, then under certain assumptions, a full range of direct and indirect treatment effects can be identified and estimated. We highlight the importance of correctly specifying own treatment and others' treatments in a linear regression. Misleading estimates can result if these two components are conflated in the regression specification. The rest of the paper is organized as follows: Section 2 develops a framework

[2]The central concerns of the peer effects literature are not the same as those of the literature on interference. Although interference is implied in peer effects, the peer effects literature has focused on identifying the specific channels through which interference affects outcomes. In particular, the literature has attempted to separately identify exogenous peer effects (i.e. effect of peer characteristics on own outcomes) and endogenous peer effects (i.e. effect of peer outcomes on own outcomes). Any attempt to disentangle these two effects must address the reflection problem (Manski, 1993); see Bramoullé et al. (2009) and Bramoullé and Fortin (2010) for examples of ways to address the reflection problem. We do not attempt to disentangle endogenous and exogenous effects. We instead focus on identifying and estimating the relative impacts on of own treatment and the treatment of other individuals.

of potential outcomes under interference; Section 3 discusses identification of population average treatment effects in various settings; Section 4 deals with estimation of these treatment effects in a linear regression framework; Section 5 concludes.

3.2 Average Treatment Effects under Interference

To fix ideas, consider a randomized experiment on a set of n individuals (henceforth referred to as a "group").[3] Suppose k of these n individuals are selected to receive a treatment. We assume a randomization design such that any set of k individuals is equally likely to be selected for treatment.

Assumption 3.1: *Consider a randomized experiment in which k individuals out of n are to be selected to receive treatment. The probability that any given set of k individuals receives treatment is $\left(\frac{n!}{(n-k)!k!}\right)^{-1}$.*

As a consequence of Assumption 3.1, the probability that any given individual receives treatment is $\frac{k}{n}$.[4]

[3]Throughout this paper, we assume that a group is exogenously formed.

[4]Assumption 3.1 can be generalized to accommodate stratified randomization within the group. An example would be an experiment in which the researcher would like to ensure that a certain number of women are selected for treatment.

The framework of potential outcomes conceptualizes the impact of treatment on the outcome.[5] Let T_i be the indicator of treatment, so that $T_i = 1$ indicates that individual i receives treatment. Historically, the potential outcomes framework has assumed that there is no interference, so that the outcome for individual i, Y_i, depends only on the treatment indicator for individual i: $Y_i = f_i(T_i)$. This framework has also assumed that outcomes have no unobserved error component, so that once treatment is assigned outcomes are deterministic (hence $f_i(1)$ is not a random quantity).[6] We retain the assumption that outcomes have no unobserved error component, while allowing for interference.

In the case of interference, the treatment statuses of all n individuals can potentially impact the outcome of any one individual. The n-dimensional vector of treatment indicators, Z, is defined as

$$Z = (T_1, \ldots, T_n).$$

Because the outcome for individual i depends on the treatment status of other individuals, we represent the outcome as a function of the entire treatment indicator

[5]The potential outcomes framework was developed by Neyman (1923) and Fisher (1926) in the context of randomized experiments and extended by Rubin (1974) to nonrandomized studies.

[6]Technically one can conceive that an individual's outcome is a function of two random processes- 1) assignment to treatment and 2) an unobserved error component. However, because the actual outcome of a given individual is typically observed only once, one can implicitly condition on the realization of the unobserved error component. It is therefore possible to interpret $f_i(1)$ and $f_i(0)$ as non-random. This practice is standard in the potential outcomes literature. See, for example, Angrist et al. (1996).

vector: $Y_i = f_i\left(Z\right)$. In the absence of interference, there are only two potential outcomes for each individual $\left(f_i\left(0\right), f_i\left(1\right)\right)$. Interference leads to an exponential increase in the number of potential outcomes. Because Y_i is a function of the entire vector Z and Z can take on one of 2^n possible values, the set of potential outcomes contains 2^n elements rather than two elements.

To clearly define treatment effects under interference, it is helpful to partition Z as $\left(T_i, Z_{(i)}\right)$ where $Z_{(i)}$ is the vector of dimension $n-1$ that contains the treatment indicators for everyone other than individual i. This allows us to express outcomes as a function of own treatment status and the treatment status of others: $Y_i = f_i(T_i, Z_{(i)})$. We use lower-case letters t_i and $z_{(i)}$ to denote specific values of T_i and $Z_{(i)}$. An individual treatment effect, $\tau_i(t_i, z_{(i)}; t'_i, z'_{(i)}) = f_i(t_i, z_{(i)}) - f_i(t'_i, z'_{(i)})$, compares individual i's outcome under two treatment vectors $(t_i, z_{(i)})$ and $(t'_i, z'_{(i)})$.

In contrast to the case without interference, in which individual i's treatment effect is uniquely defined as $\tau_i = f_i(1) - f_i(0)$, with interference there are multiple possible treatment effects for individual i. Two classes of individual treatment effects deserve special mention. A *direct effect* compares two potential outcomes that vary only due to changes in own treatment ($t \neq t'$ but $z_{(i)} = z'_{(i)}$), for example $\tau_i(1, z_{(i)}; 0, z_{(i)})$. Direct effects can be seen as a natural extension of the case of no interference. In contrast an *indirect effect* compares two potential outcomes that vary only due to changes in others' treatment status ($t = t'$ but $z_{(i)} \neq z'_{(i)}$), for example $\tau_i(0, z_{(i)}; 0, z'_{(i)})$. Indirect effects are novel in that they would not exist (i.e. would be equal to zero) in the case of no interference.

The inability to observe the outcome of the same individual under two distinct treatment statuses has been referred to as the Fundamental Problem of Causal Inference by Holland (1986). To emphasize this point, when there is no interference, researchers often refer to the quantities $f_i(1)$ and $f_i(0)$ as a counterfactual set. In the presence of interference the fact that one cannot observe the same individual with treatment and no treatment is joined by the fact that one cannot hold both the number of treated individuals in a group constant and hold $z_{(i)}$ constant, while allowing t_i to vary.

Because individual treatment effects are based on counterfactual sets, they are impossible to identify. Therefore, researchers have focused on the average treatment effect for the population of individuals. Absent interference the (population) average treatment effect is simply $\frac{1}{n}\sum_{i=1}^{n}(f_i(1) - f_i(0))$.[7] While it is possible to analogously construct an average treatment effect under interference as $\frac{1}{n}\sum_{i=1}^{n}\tau_i(t_i, z_{(i)}; t'_i, z'_{(i)})$, we take a more general approach that encompasses a range of effects identifiable through common research designs. In the general

[7]It should be emphasized that the population average treatment effect involves an average only over heterogenous treatment responses *across* individuals, as we have abstracted away from randomness *within* an individual's own treatment response.

approach, we define average treatment effects in terms of possible sets of $n-1$ length vectors $\mathcal{Z}_A$ and $\mathcal{Z}_B$:

$$\tau(t, \mathcal{Z}_A; t', \mathcal{Z}_B) \equiv \frac{1}{n}\sum_{i=1}^{n}[\mathbb{E}(Y_i|T_i = t, Z_{(i)} \in \mathcal{Z}_A) - \mathbb{E}(Y_i|T_i = t', Z_{(i)} \in \mathcal{Z}_B)]. \quad (3.1)$$

The source of randomness in (3.1) is the assignment of individuals to treatment. The expectations operator applies over the values of $Z_{(i)}$ that fall in the set $\mathcal{Z}_A$ or $\mathcal{Z}_B$. Although the sets $\mathcal{Z}_A$ and $\mathcal{Z}_B$ are defined generally in (3.1), a leading case of empirical interest is when these sets represent a particular fraction of other individuals who are treated.

As a concrete example, consider a case in which $k = 50$ members from a group of $n = 100$ are treated. If $t = 1$ then $\mathcal{Z}_A$ is the set of vectors of length 99 in which 49 elements are 1, while if $t' = 0$, then $\mathcal{Z}_B$ is the set of vectors of length 99 in which 50 elements are 1. In this case, under Assumption 3.1, $\tau(1, \mathcal{Z}_A; 0, \mathcal{Z}_B) = \frac{1}{100}\sum_{i=1}^{100}\left[\left(\frac{99!}{(50)!49!}\right)^{-1}\sum_{\omega\in\mathcal{Z}_A}\left(f_i(1,\omega) - f_i(0,\omega)\right)\right]$.[8] Of course, the definition in (3.1) is general and could nest the difference in expectations under any two scenarios, for instance – a) being treated and having one-half of

[8]There are $\left(\frac{99!}{(50)!49!}\right)$ possible vectors of length 99 containing 49 ones and 50 zeros. Under Assumption 3.1, each of these vectors has equal probability of being realized. There are also $\left(\frac{99!}{(50)!49!}\right)$ possible vectors of length 99 containing 50 ones and 49 zeros, and similarly under Assumption 3.1, each of these vectors has equal probability of being realized.

others treated versus b) being untreated and having one-third of others treated − averaged over all individuals in the population. The concepts of direct effect ($t \neq t'$ but $\mathcal{Z}_A = \mathcal{Z}_B$) and indirect effect ($t = t'$ but $\mathcal{Z}_A \neq \mathcal{Z}_B$) also apply to the average treatment effect.

In summary, the presence of interference alters the potential outcomes framework in two distinct ways. First, and most obviously, the outcome for an individual depends on the treatment status of other individuals. Second, a counterfactual set of potential outcomes may consist of outcomes in which only the treatment status of others changes.

The presence of interference also complicates the population average treatment effect in two important ways. First, it is not a unique quantity and depends on the type of comparison of interest to the researcher. Second, the randomization design used by the researcher is implicit in its definition; in particular, the probabilities involved in determining $\mathbb{E}(Y_i|T_i = t, Z_{(i)} = \mathcal{Z}_A)$ and $\mathbb{E}(Y_i|T_i = t, Z_{(i)} = \mathcal{Z}_B)$ depend on the randomization design.

3.3 Identification of Population Average Treatment Effects

We now ask: What average treatment effects can be identified with data from n individuals in a single group? We address this question under two settings. In the first, the expected outcome of individual i (e.g. $\mathbb{E}(Y_i|T_i = t, Z_{(i)} = \mathcal{Z}_A)$) cannot be reduced to a simpler expression. In the second, the expected outcome of individual i can be expressed in terms of the fraction of others receiving treatment, which requires less information than is contained in entire treatment vectors. We follow this analysis with a corresponding analysis for data from multiple groups, where there is no interference between groups. In such a setting, a full range of treatment effects can be identified if the expected outcome of individual i again depends on the fraction of others treated and, in addition, the outcome is a linear function of this fraction.

Identification with Data from a Single Group

With Assumption 3.1 alone, it is possible to identify one particular average treatment effect that is neither a direct nor an indirect effect. The identifiable treatment effect is $\tau(1, \mathcal{Z}_{k-1}; 0, \mathcal{Z}_k)$, where $\mathcal{Z}_{k-1}$ and $\mathcal{Z}_k$ denote sets of $n-1$ length vectors in which $k-1$ and k individuals are treated respectively.

With outcomes from a single group this average treatment effect is estimated by

$$\hat{\tau} = \widehat{Y}(1) - \widehat{Y}(0),$$

where $\widehat{Y}(t) \equiv \frac{\sum_{i=1}^{n}[Y_i \cdot I(T_i=t)]}{\sum_{i=1}^{n} I(T_i=t)}$ for $t \in \{0,1\}$.

PROPOSITION 3.1: *Under Assumption 3.1,*

$$\tau(1, \mathcal{Z}_{k-1}; 0, \mathcal{Z}_k)$$

is identified with data from n individuals in a single group. The estimator $\hat{\tau}$ provides an unbiased estimate of $\tau(1, \mathcal{Z}_{k-1}; 0, \mathcal{Z}_k)$.

PROOF: Noting that $\tau(1, \mathcal{Z}_{k-1}; 0, \mathcal{Z}_k) \equiv \frac{1}{n}\sum_{i=1}^{n}[\mathbb{E}(Y_i|T_i = 1, Z_{(i)} \in \mathcal{Z}_{k-1}) - \mathbb{E}(Y_i|T_i = 0, Z_{(i)} \in \mathcal{Z}_k)]$, it is sufficient to show that $\mathbb{E}[\widehat{Y}(1)] = \frac{1}{n}\sum_{i=1}^{n}\mathbb{E}(Y_i|T_i = 1, Z_{(i)} \in \mathcal{Z}_{k-1})$ and $\mathbb{E}[\widehat{Y}(0)] = \frac{1}{n}\sum_{i=1}^{n}\mathbb{E}(Y_i|T_i = 0, Z_{(i)} \in \mathcal{Z}_k)$. In order to establish these facts, we note that Assumption 3.1 implies for all i: $Pr(Z_{(i)} = \omega|T_i = 1, Z_{(i)} \in \mathcal{Z}_{k-1}) = \left(\frac{(n-1)!}{(n-k)!(k-1)!}\right)^{-1} \forall\, \omega \in \mathcal{Z}_{k-1}$ and $Pr(Z_{(i)} = \omega|T_i = 0, Z_{(i)} \in \mathcal{Z}_k) = \left(\frac{(n-1)!}{(n-1-k)!k!}\right)^{-1} \forall\, \omega \in \mathcal{Z}_k$.

$$\begin{aligned}
\mathbb{E}[\widehat{Y}(1)] &= k^{-1}\sum_{i=1}^{n}\mathbb{E}[Y_i \cdot I(T_i=1)] \\
&= k^{-1}\sum_{i=1}^{n}\left(\frac{n!}{(n-k)!k!}\right)^{-1}\sum_{\omega\in\mathcal{Z}_{k-1}} f_i(1,\omega) \\
&= k^{-1}\sum_{i=1}^{n}\left(\frac{n}{k}\cdot\frac{(n-1)!}{(n-k)!(k-1)!}\right)^{-1}\sum_{\omega\in\mathcal{Z}_{k-1}} f_i(1,\omega) \\
&= k^{-1}\sum_{i=1}^{n}\left(\frac{n}{k}\right)^{-1}\mathbb{E}(Y_i|T_i=1, Z_{(i)}\in\mathcal{Z}_{k-1}) \\
&= \frac{1}{n}\sum_{i=1}^{n}\mathbb{E}(Y_i|T_i=1, Z_{(i)}\in\mathcal{Z}_{k-1}).
\end{aligned}$$

The first line uses the fact that $\sum_{i=1}^{n} I(T_i = 1)$ is equal to the fixed number k. Assumption 3.1 makes it possible to go from the first to the second line. The third line is an algebraic rearrangement of the second line, and fourth line follows from the implication of Assumption 3.1. It can be similarly shown that $\mathbb{E}[\widehat{Y}(0)] = \frac{1}{n}\sum_{i=1}^{n}\mathbb{E}(Y_i|T_i=0, Z_{(i)}\in\mathcal{Z}_k)$.

An Example

As a concrete example, consider a group of $n = 4$ individuals, $k = 2$ of whom are to be selected for treatment. Under Assumption 3.1, each of the following treatment vectors for the group has probability $\frac{1}{6}$ of being implemented: $(1,1,0,0)$; $(1,0,1,0)$; $(1,0,0,1)$; $(0,1,1,0)$; $(0,1,0,1)$; and $(0,0,1,1)$. Regardless of which of the these six schemes is actually implemented, it is guaranteed that for any treated individual i, $Z_{(i)} \in \mathcal{Z}_1$, and for any untreated individual j, $Z_{(j)} \in \mathcal{Z}_2$. This fact allows the researcher to identify $\tau(1, \mathcal{Z}_1; 0, \mathcal{Z}_2)$, as both its components $\frac{1}{n}\sum_{i=1}^{n} \mathbb{E}(Y_i|T_i = 1, Z_{(i)} \in \mathcal{Z}_1)$ and $\frac{1}{n}\sum_{i=1}^{n} \mathbb{E}(Y_i|T_i = 0, Z_{(i)} \in \mathcal{Z}_2)$ are identifiable. Effects corresponding to different numbers of others treated cannot be identified under a research design with a single group of 4 individuals, 2 of whom are treated. For example, $\tau(1, \mathcal{Z}_0; 0, \mathcal{Z}_3)$ cannot be identified as the researcher will never observe a treated individual with $Z_{(i)} = (0, 0, 0)$ or an untreated individual with $Z_{(i)} = (1, 1, 1)$.

Identifying A Direct Effect

The identified quantity $\tau(1, \mathcal{Z}_{k-1}; 0, \mathcal{Z}_k)$ is neither a direct nor an indirect effect. However if we impose restrictions on the form of the conditional expectation functions in (3.1), a direct effect can be identified in an arbitrarily large group. Specifically, we restrict the conditional expectation function to depend only on the fraction of other group members receiving treatment rather than on vectors of others' treatment statuses. The absolute number and identity of the other treated or untreated group members do not matter in expectation. Furthermore, we impose a continuity requirement on the conditional expectation functions. These restrictions are formally stated in Assumption 3.2.

Assumption 3.2: *Let $\mathcal{Z}_{\mathcal{C}}$ be the set of $n-1$ length vectors in which $P(C)$ fraction of individuals are treated.*

$$\mathbb{E}\left[Y_i|T_i = t, Z_{(i)} \in \mathcal{Z}_C\right] = g_i(t, P(C)),$$

where $\forall\ i$, the function g_i is continuous in the fraction of others treated (i.e. its second argument).

Assumption 3.2 is implicit in much empirical work on interference, which tends to capture the effect of others' treatment statuses through a single number (i.e. fraction treated) rather than through vectors (Duflo and Saez, 2003; Ferracci et al., 2010; Crepon et al., 2013). As the conditional expectations are reduced to functions of only of own treatment and the fraction of others treated, it is possible express to any average treatment effect as a function of the same. Let $\mathcal{Z}_A$ and $\mathcal{Z}_B$ denote the sets of $n-1$ length vectors in which $P(A)$ and $P(B)$ fractions of individuals are treated, respectively. Under Assumption 3.2:

$$\tau(t, \mathcal{Z}_A; t', \mathcal{Z}_B) = \frac{1}{n}\sum_{i=1}^{n}[g_i(t, P(A)) - g_i(t', P(B))] = \tau(t, P(A); t', P(B)), \quad (3.2)$$

and the concepts of direct effects ($t' \neq t,\ P(A) = P(B)$) and indirect effects ($t' = t,\ P(A) \neq P(B)$) apply.

With Assumptions 3.1 and 3.2 and an arbitrarily large group size, the direct effect $\tau(1, \frac{k}{n}; 0, \frac{k}{n})$ can be identified with data from n individuals in a single group. This direct effect is consistently estimated by $\hat{\tau}$.

COROLLARY 3.1: *Under Assumptions 3.1 and 3.2,*

$$\tau(1, \frac{k}{n}; 0, \frac{k}{n})$$

can be identified with data from a single group and $\hat{\tau}$ is a consistent estimator of $\tau(1, \frac{k}{n}; 0, \frac{k}{n})$.

PROOF: Given that $\tau(1, \frac{k}{n}; 0, \frac{k}{n}) \equiv \frac{1}{n}\sum_{i=1}^{n}[g_i(1, \frac{k}{n}) - g_i(0, \frac{k}{n})]$, it is sufficient to show that as $n \to \infty$, $\mathbb{E}[\widehat{Y}(1)] \to \frac{1}{n}\sum_{i=1}^{n} g_i(1, \frac{k}{n})$ and $\mathbb{E}[\widehat{Y}(0)] \to \frac{1}{n}\sum_{i=1}^{n} g_i(0, \frac{k}{n})$. Proposition 3.1 implies $\mathbb{E}[\widehat{Y}(1)] = \frac{1}{n}\sum_{i=1}^{n} g_i(1, \frac{k-1}{n-1})$. Because $\forall\ i$ the function g_i is continuous in its second argument, as $n \to \infty$ (and the fraction of the group treated remains constant), $\mathbb{E}[\widehat{Y}(1)] \to \frac{1}{n}\sum_{i=1}^{n} g_i(1, \frac{k}{n})$. By similar reasoning, $\mathbb{E}[\widehat{Y}(0)] \to \frac{1}{n}\sum_{i=1}^{n} g_i(0, \frac{k}{n})$.

Corollary 3.1 leverages the fact that as the group size grows arbitrarily large (and fraction of the group treated remains constant), the fraction of others receiving treatment converges to the fraction of the entire group receiving treatment. For example, consider a group of individuals, one-half of whom are to be selected for treatment. If $n = 4$, for an untreated individual, the fraction of others treated is $\frac{2}{3}$, while for a treated individual the fraction of others treated is $\frac{1}{3}$. However, if $n = 400$, the corresponding fractions are $\frac{200}{399}$ and $\frac{199}{399}$, both of which are approaching $\frac{1}{2}$. An implication of Corollary 3.1 for empirical research is that in settings where group size is small, it is critical to distinguish between the fraction of others receiving treatment and the fraction of all group members receiving treatment. As we illustrate in Section 4, this issue becomes all the more important when more than one group is observed.

Identification With Data from More than One Group

Now suppose that the researcher can observe two groups of n individuals each.[9] In group 1, k_1 individuals are selected for treatment according to Assumption 3.1, while in group 2 k_2 individuals are selected. If the fraction of individuals treated in each group is not the same (i.e. $k_1 \neq k_2$), it is possible to identify additional forms of (3.2), provided a further assumption is made regarding how treatment is assigned across groups. We refer to the fraction of individual's treated in a group as the group's *treatment regime*.

Assumption 3.3: *Consider a randomized experiment in which 2 groups of n individuals are to be assigned to 2 treatment regimes, with one group assigned*

[9]The groups do not necessarily have to contain the same number of individuals. However, if they contain different number of individuals, it is necessary to weight the averages of observed outcomes in order to estimate a quantity that is comparable to (3.1) or (3.2).

to treatment regime 1 and the other group assigned to treatment regime 2. The probability that a given group is subject to treatment regime 1 is 0.5.

Assumption 3.3 is the cross-group analogue of Assumption 3.1.[10] While Assumption 3.1 pertains to the random assignment of treatment across individuals within a group, Assumption 3.3 pertains to the random assignment of treatment regimes across groups.

Proposition 3.2: Let $\widehat{Y}^1(1)$ and $\widehat{Y}^1(0)$ respectively be the average observed outcome over the treated and untreated in group 1. Let $\widehat{Y}^2(1)$ and $\widehat{Y}^2(0)$ respectively be the average observed outcome over the treated and untreated in group 2. Then, under Assumptions 3.1, 3.2, and 3.3

$$\mathbb{E}[\widehat{Y}^1(1) - \widehat{Y}^1(0)] = \tau(1, \frac{k_1 - 1}{n_1 - 1}; 0, \frac{k_1}{n_1 - 1}),$$

$$\mathbb{E}[\widehat{Y}^2(1) - \widehat{Y}^2(0)] = \tau(1, \frac{k_2 - 1}{n_2 - 1}; 0, \frac{k_2}{n_2 - 1}),$$

$$\mathbb{E}[\widehat{Y}^1(1) - \widehat{Y}^2(0)] = \tau(1, \frac{k_1 - 1}{n_1 - 1}; 0, \frac{k_2}{n_2 - 1}),$$

$$\mathbb{E}[\widehat{Y}^2(1) - \widehat{Y}^1(0)] = \tau(1, \frac{k_2 - 1}{n_2 - 1}; 0, \frac{k_1}{n_1 - 1}),$$

$$\mathbb{E}[\widehat{Y}^1(1) - \widehat{Y}^2(1)] = \tau(1, \frac{k_1 - 1}{n_1 - 1}; 1, \frac{k_2 - 1}{n_2 - 1}),$$

[10] Assumption 3.3 can be generalized to the case where there are more than two groups assigned to more than two treatment regimes.

$$\mathbb{E}[\widehat{Y}^1(0) - \widehat{Y}^2(0)] = \tau(0, \frac{k_1}{n_1 - 1}; 0, \frac{k_2}{n_2 - 1}).$$

where in general $\tau(t, P(A); t', P(B)) = \frac{1}{2n}\sum_{i=1}^{2n}[g_i(t, P(A)) - g_i(t', P(B))]$. Even if the group size is small, a direct effect can be identified if the researcher sets $\frac{k_1-1}{n_1-1} = \frac{k_2}{n_2-1}$ or $\frac{k_2-1}{n_2-1} = \frac{k_1}{n_1-1}$. As discussed previously, if the group size is large, two separate direct effects are identified by the within-group comparisions $\mathbb{E}[\widehat{Y}^1(1) - \widehat{Y}^1(0)]$ and $\mathbb{E}[\widehat{Y}^2(1) - \widehat{Y}^2(0)]$. (See Appendix C for proof of Proposition 3.2).

Finally, if one assumes linearity of the conditional expectation function, then estimates from two or more groups can identify the full spectrum of population average treatment effects. Let $\mathcal{Z}_A$ denote the set of vectors in which $P(A)$ fraction are treated. Assuming linearity:

$$\mathbb{E}\left[Y_i | T_i, Z_{(i)} \in \mathcal{Z}_A\right] = \beta_0 + \beta_1 T_i + \beta_2 P\left(A\right).$$

Alternatively, a more general functional form may be assumed, where the effect of a change in the fraction of others treated is different for treated and untreated individuals:

$$\mathbb{E}\left[Y_i | T_i, Z_{(i)} \in \mathcal{Z}_A\right] = \beta_0 + \beta_1 T_i + \beta_2 P\left(A\right) + \beta_3 T_i \cdot P(A).$$

3.4 Inference

Let W_i be the two-dimensional vector where the first element is the treatment status of individual i and the second element is the fraction of other members of i's group who are treated. If the impact of these treatment indicators is linear on the outcome, then a natural framework is

$$Y_i = \alpha + \tau_1 W_{i,1} + \tau_2 W_{i,2} + U_i. \tag{3.3}$$

Assumptions 3.1 and 3.3 ensure the orthogonality of the regressors and the error term, U_i. The coefficient τ_1 captures the impact of changing individual i's treatment status, while holding the treatment status of others (fraction treated) constant. Hence τ_1 measures the direct effect of treatment on the outcome and, as discussed above, cannot be identified from the assignment of treatments to a single group, as the individual's own treatment status perfectly predicts the fraction of others treated. The coefficient τ_2 captures the impact of changing the fraction of others treated, while leaving the treatment status of individual i unchanged, and so captures an indirect effect. For similar reasons, this quantity also cannot be identified from the assignment of treatments to a single group.

If there are observations from multiple groups, and the treatment design ensures variation across groups in the fraction of individuals treated, then OLS estimators of the coefficients yield consistent estimates of both the direct and indirect effects. One issue to consider, it is likely that the error is correlated within groups, but not across groups, so estimates of precision should be based on cluster-robust standard errors.

An Empirical Example

Duflo and Saez (2003) conduct a field experiment in which randomly selected non-faculty employees in a university are incentivized to attend an information fair on retirement plans. The outcome of interest is whether an employee attends the fair, and the treatment is whether the employee is selected to receive a cash prize as a reward for attendance. The authors are interested not only in the effect of an employee's own treatment but also in the effect of the treatments of other employees in the same department. They argue that interference can occur among employees in the same department but not among employees in different departments. Their setting thus conforms to our framework.

Of the 330 departments in the university, 220 are selected for a treatment regime in which half of the department's employees are treated. In the remaining 110 departments, no employees are treated. Duflo and Saez use the following regression model:

$$\mathbb{E}\left[f_{ij}|L_{ij}, D_j\right] = \gamma_0 + \gamma_1 L_{ij} + \gamma_2 D_j, \tag{3.4}$$

where f_{ij} is an indicator for whether employee i in department j attends the fair. The variable L_{ij} is an indicator for whether the employee is treated and D_j is an indicator for whether the employee's department was one among the 220 selected for treatment. If the treatments of others can be reduced to a fraction and the department sizes are large, the parameter γ_2 identifies an indirect effect $\tau(0, 0.5; 0, 0)$, while γ_1 identifies a direct effect $\tau(1, 0.5; 0, 0.5)$.[11]

Consequences of Incorrect Specification with Small Group Sizes

Regression equation (3.4) differs from (3.3) in an important way. Instead of including as a regressor the fraction of *others* treated in i's department, Duflo and Saez (2003) instead include the fraction of *all* individuals treated in i's department (including i).[12] As discussed in Section 3, if the number of individuals in a group (i.e. department) is large, this should not matter.[13] However, if group sizes are small, the distinction between the fraction of others receiving treatment, and the fraction of all group members receiving treatment, is critical. We demonstrate

[11]Duflo and Saez construe D_j to be dichotomous rather than continuously varying. If they instead interpreted it as continuously varying, a further range of effects could be identfied. For example, $\tau(1, P(A); 1, P(B))$ would be identified by $\gamma_2 * [P(A) - P(B)]$.

[12]This is a common practice in empirical research. See also Ferracci et al. (2010) and Crepon et al. (2013).

[13]The median number of individuals in a department is 15 (Duflo and Saez, 2003).

by simulation that replacing the former with the latter can provide misleading estimates not just of the indirect effect (τ_2) but also the direct effect (τ_1).

The data generating process is specified by (3.3) and the error term U_i is modeled as a standard normal distribution. In the first set of simulations, the parameter values are set as follows: $\alpha = 20$, $\tau_1 = 20$, and $\tau_2 = 20$. An individual's expected outcome is higher by 20 units if assigned to treatment and the individual benefits further as the fraction of others treated increases. We generate 1000 datasets, each consisting of 24 groups of 5 individuals each (120 individuals). The fraction of individuals treated within a group can take on one of six values: 0, $\frac{1}{5}$, $\frac{2}{5}$, $\frac{3}{5}$, $\frac{4}{5}$, or 1, with 4 groups subject to each treatment regime.

From each of the 1000 simulated datasets, we estimate the coefficients using the specification

$$Y_i = \alpha + \tau_1 W_{i,1} + \tau_2 \widetilde{W_{i,2}} + U_i, \tag{3.5}$$

where $\widetilde{W_{i,2}}$ denotes the fraction of all members of i's group (including i) who are treated.

Figures 3.1 and 3.2 depict histograms of the 1000 estimates of the direct effect (τ_1) and the indirect effect (τ_2), respectively. The true values of both these parameters is 20, however the estimates of τ_1 are centered around 15, while the estimates of τ_2 are centered around 25. If an individual is treated, replacing $W_{i,2}$ with $\widetilde{W_{i,2}}$ understates the fraction of others treated. On the other hand, if an individual is untreated, replacing $W_{i,2}$ with $\widetilde{W_{i,2}}$ overstates the fraction of others treated. As a result, some of the true direct effect is attributed to the effect of others' treatments and the estimate of τ_1 is systematically lower than its true value. It is also seen that the estimate of τ_2 is systematically higher than its true value.

The bias in the direct effect estimates becomes even more serious if indirect effects play a larger role in determining an individual's outcome. In the second set of simulations the parameter values are set as follows: $\alpha = 20$, $\tau_1 = 20$, and $\tau_2 = 100$. The marginal positive effect of a change in the fraction of others treated is five times larger than in the first set of simulations. We again generate 1000 datasets, each consisting of 24 groups of 5 individuals each (120 individuals). The fraction of individuals treated within a group can take on one of six values: 0, $\frac{1}{5}$, $\frac{2}{5}$, $\frac{3}{5}$, $\frac{4}{5}$, or 1, with 4 groups subject to each treatment regime. From each of the 1000 simulated datasets, we estimate the coefficients using (3.5).

Figures 3.3 and 3.4 depict histograms of the 1000 estimates of the direct effect (τ_1) and the indirect effect (τ_2), respectively, from the second set of simulations. The true value of τ_2 is 100, but the estimates are centered around 125. What is more striking, however, is the egregious downward bias of the direct effect estimates. The direct effect estimates, which are centered around -5, do not even have the same sign as the true direct effect. These simulations highlight the importance of carefully specifying the treatment of others, especially when group sizes are small.

3.5 Conclusion

The contributions of this paper are two-fold. First, we extend the framework of potential outcomes to situations characterized by interference. This extension gives rise to a wide range of population average treatment effects, including direct effects (due to a change only in an individual's own treatment status) and indirect effects (due to a change only in the treatment status of others). Our second contribution is in the estimation of population average treatment effects. We propose conditions under which a linear regression can be used to estimate the full range of treatment effects. The importance of correctly specifying the linear regression is demonstrated through simulations.

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Figure 3.1: Histogram of Simulated Estimates of Direct Effect (τ_1): Actual Values $\alpha = 20$, $\tau_1 = 20$, $\tau_2 = 20$

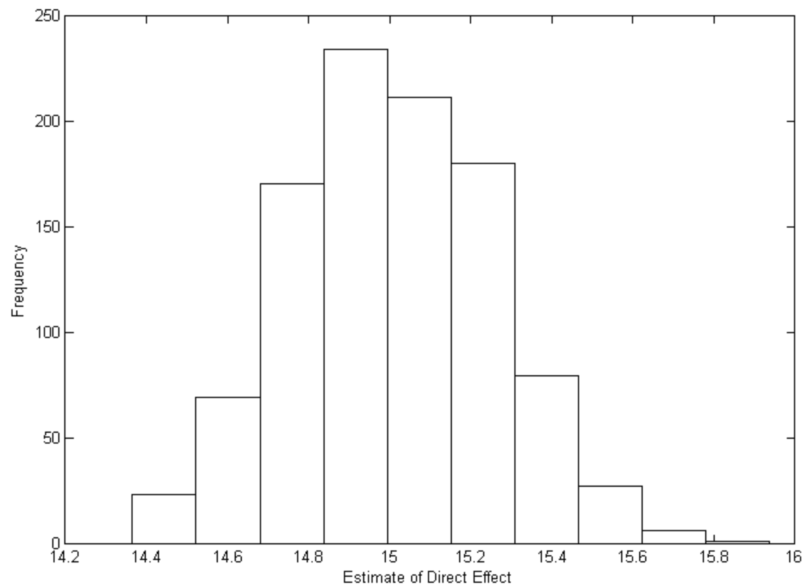


Figure 3.2: Histogram of Simulated Estimates of Indirect Effect (τ_2): Actual Values $\alpha = 20$, $\tau_1 = 20$, $\tau_2 = 20$

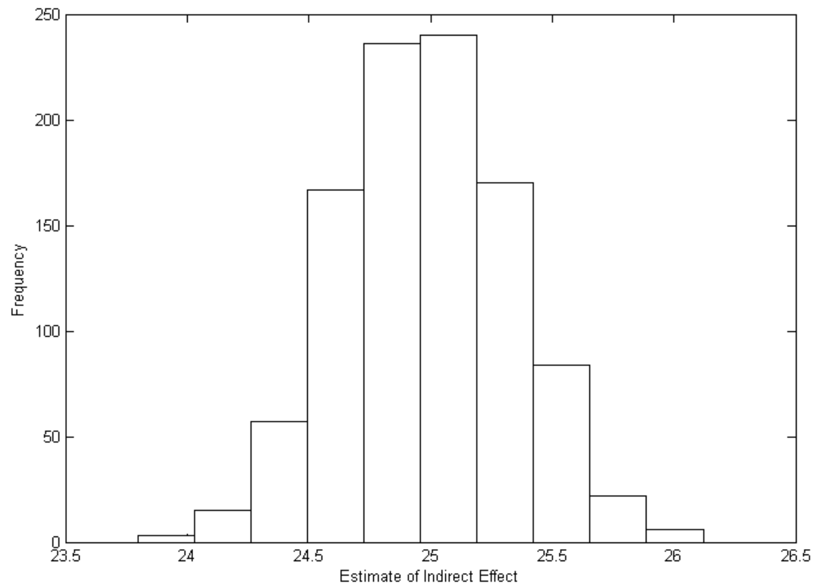


Figure 3.3: Histogram of Simulated Estimates of Direct Effect (τ_1): Actual Values $\alpha = 20$, $\tau_1 = 20$, $\tau_2 = 100$

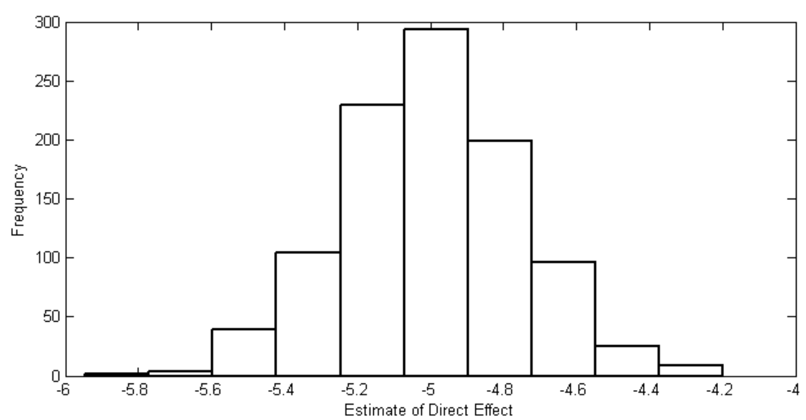
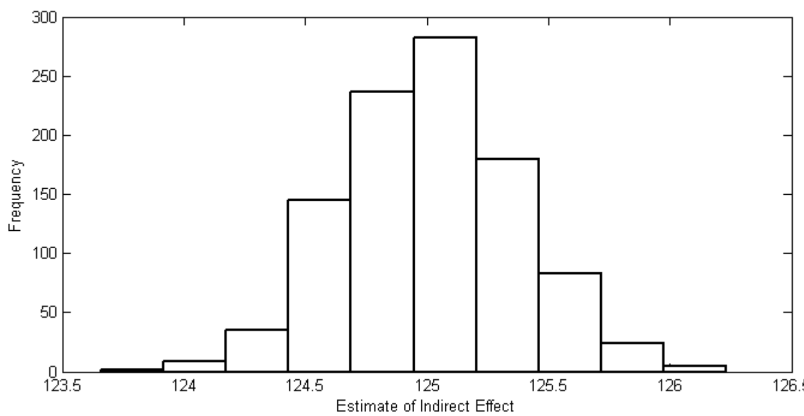


Figure 3.4: Histogram of Simulated Estimates of Indirect Effect (τ_2): Actual Values $\alpha = 20$, $\tau_1 = 20$, $\tau_2 = 100$



Appendices

Appendix A

Additional Proofs in Chapter 1

PROPOSITION A1: The number of lobbying firms in equilibrium, k, is uniquely determined.

PROOF: Suppose there exists a Nash equilibrium with k lobbying firms and another Nash equilibrium with k' lobbying firms. Without loss of generality, suppose $k' > k$. The equilibrium conditions stipulate

$$\begin{array}{ll} \omega_i < \frac{1}{k-1}\sum_{j=1}^{k}\omega_j & for\, i = 1, 2, ..., k \\ \omega_i \geq \frac{1}{k-1}\sum_{j=1}^{k}\omega_j & for\, i = k+1, k+2, ..., n \end{array} \tag{A.1}$$

and

$$\begin{array}{ll} \omega_i < \frac{1}{k'-1}\sum_{j=1}^{k}\omega_j & for\, i = 1, 2, ..., k' \\ \omega_i \geq \frac{1}{k'-1}\sum_{j=1}^{k}\omega_j & for\, i = k'+1, k'+2, ..., n \end{array} \tag{A.2}$$

where i indexes firms in order of lobbying cost (i.e. firm 1 is the firm with lowest lobbying cost; firm n is the firm with the highest lobbying cost).

Condition (A.2) implies $(k'-1)\omega_{k'} < \sum_{j=1}^{k'}\omega_j$, which can be equivalently expressed as $(k-1)\omega_{k'} + (k'-k)\omega_{k'} < \sum_{j=1}^{k}\omega_j + \sum_{j=k+1}^{k'}\omega_j$. Condition (A.1) implies $(k-1)\omega_{k'} > \sum_{j=1}^{k}\omega_j$, therefore it must be that $(k'-k)\omega_{k'} < \sum_{j=k+1}^{k'}\omega_j$. However, because $\forall\, j < k'$, $\omega_j < \omega_{k'}$, $\omega_{k'} > \frac{1}{k'-k}\sum_{j=k+1}^{k'}\omega_j$, which implies $(k'-k)\omega_{k'} > \sum_{j=k+1}^{k'}\omega_j$ leading to a contradiction. Therefore there cannot exist two Nash equilibria with different numbers of lobbying firms. *Q.E.D.*

PROPOSITION A2: The equilibrium lobbying effort of a lobbying firm is decreasing in the firm's own cost of lobbying. The effect of an increase in another firm's lobbying costs is ambiguous.

PROOF: For a lobbying firm i, $x_i = \frac{\tau\phi(k-1)}{(\sum_{j=1}^{k}\omega_j)^2} \cdot \left(\left(\sum_{j=1}^{k}\omega_j\right) - \omega_i(k-1)\right)$.

Differentiating x_i with respect to ω_i yields

$$\frac{\partial x_i}{\partial \omega_i} = \tau\phi(k-1)\left[\frac{2(k-1)\omega_i \sum_{j=1}^{k} \omega_j - k\left(\sum_{j=1}^{k} \omega_j\right)^2}{\left(\sum_{j=1}^{k} \omega_j\right)^4}\right], \tag{A.3}$$

which is strictly negative if and only if $\frac{2\omega_i}{k} < \frac{1}{k-1}\sum_{j=1}^{k} \omega_j$. Because $k \geq 2$, $\frac{2\omega_i}{k} \leq \omega_i$. The condition for a lobbying firm i is that $\omega_i < \frac{1}{k-1}\sum_{j=1}^{k} \omega_j$. Together these imply $\frac{2\omega_i}{k} < \frac{1}{k-1}\sum_{j=1}^{k} \omega_j$.

Differentiating x_i with respect to $\omega_{i'}$ (with $i' \neq i$) yields

$$\frac{\partial x_i}{\partial \omega_{i'}} = \tau\phi(k-1)\left[\frac{2(k-1)\omega_i - \sum_{j=1}^{k} \omega_j}{\left(\sum_{j=1}^{k} \omega_j\right)^3}\right]. \tag{A.4}$$

The denominator is obviously positive. Because ω_i may be greater or less than $\frac{1}{2(k-1)}\sum_{j=1}^{k} \omega_j$, the sign of $\frac{\partial x_i}{\partial \omega_{i'}}$ is ambiguous. *Q.E.D.*

Appendix B

Additional Tables in Chapter 1

Table B.1: Regressions with Number of Connected MPs (Unweighted)

Dependent Variable: *Ln(Final Allocation)*	*(1)*	*(2)*	*(3)*
Ln(Provisional Allocation)	0.937^{***} (0.042)	0.922^{***} (0.057)	0.965^{***} (0.042)
Number of MPs	**0.016 (0.032)**	**0.008 (0.049)**	**-0.059 (0.056)**
(Number of MPs)^2	**-0.00005 (0.0014)**	**-0.00001 (0.0021)**	**0.0025 (0.0024)**
ln(Total Fixed Assets)			0.015 (0.042)
ln(Employees)			0.030 (0.083)
Profit Margin			0.536 (0.427)
Industry Controls?	No	Yes	Yes
Weighted Regression?	No	No	No
N	247	247	185
R^2	0.84	0.86	0.90

Excludes universities, hospitals, and other government entities. Standard errors (in parentheses) are clustered by industry. The superscripts ***, **, and * denote significance at 1, 5, and 10 percent levels respectively.

Table B.2: Regressions with Number of Connected MPs (Weighted, Levels)

Dependent Variable: *Final Allocation*	*(1)*	*(2)*	*(3)*
Provisional Allocation	0.956***	0.979***	0.989***
	(0.021)	(0.029)	(0.009)
Number of MPs	298,565***	247,243***	267,234***
	(30,128)	(19,489)	(38,322)
(Number of MPs)ˆ2	-10,004***	-8,519***	-8,957***
	(2,099)	(1,151)	(1,877)
ln(Total Fixed Assets)			128,232*
			(58,952)
ln(Employees)			-163,150
			(107,381)
Profit Margin			3,638
			(8,000)
Industry Controls?	No	Yes	Yes
Weighted Regression?	Yes	Yes	Yes
N	247	247	185
R^2	0.99	0.99	0.99

Excludes universities, hospitals, and other government entities. Observations weighted by provisional allocation. Standard errors (in parentheses) are clustered by industry. The superscripts ***, **, and * denote significance at 1, 5, and 10 percent levels respectively.

Table B.3: Regressions with Number of Connected MPs (Unweighted, Levels)

Dependent Variable: *Final Allocation*	*(1)*	*(2)*	*(3)*
Provisional Allocation	0.966***	0.967***	0.987***
	(0.012)	(0.016)	(0.016)
Number of MPs	58,883	60,930	68,527
	(60,227)	(55,544)	(69,390)
(Number of MPs)ˆ2	-1,199	-1,632	-1,693
	(2,883)	(2,767)	(2,998)
ln(Total Fixed Assets)			27,156
			(26,945)
ln(Employees)			-28,774
			(39,720)
Profit Margin			-948
			(2,161)
Industry Controls?	No	Yes	Yes
Weighted Regression?	No	No	No
N	247	247	185
R^2	0.99	0.99	0.99

Excludes universities, hospitals, and other government entities. Standard errors (in parentheses) are clustered by industry. The superscripts ***, **, and * denote significance at 1, 5, and 10 percent levels respectively.

Appendix C

Additional Proofs in Chapter 3

Proof of Proposition 3.2: We demonstrate the unbiasedness of $\mathbb{E}[\widehat{Y}^1(1) - \widehat{Y}^2(1)] = \tau(1, \frac{k_1-1}{n_1-1}; 1, \frac{k_2-1}{n_2-1})$. The same reasoning can be used to establish the unbiasedness of the other 5 inter-group comparisions listed in Proposition 3.2.

Let the S_j indicate the treatment regime of group j, where $S_j = 1$ if group j receives treatment regime 1 (corresponding to $\frac{k_1}{n}$ fraction treated) and zero otherwise. Let Y_{ij} denote the observed outcome of individual i in group j. Note that

$$\hat{Y}^1(1) = \sum_{j=1}^{2} \left(\frac{\sum_{i=1}^{n} [Y_{ij} \cdot I(T_{ij} = 1)]}{\sum_{i=1}^{n} I(T_{ij} = 1)} \cdot I(S_j = 1) \right)$$

and

$$\hat{Y}^2(1) = \sum_{j=1}^{2} \left(\frac{\sum_{i=1}^{n} [Y_{ij} \cdot I(T_{ij} = 1)]}{\sum_{i=1}^{n} I(T_{ij} = 1)} \cdot I(S_j = 0) \right).$$

It is sufficient to show that

$$\mathbb{E}[\widehat{Y}^1(1)] = \frac{1}{2n} \sum_{i=1}^{2n} g_i(1, \frac{k_1 - 1}{n_1 - 1})$$

and

$$\mathbb{E}[\widehat{Y}^2(1)] = \frac{1}{2n} \sum_{i=1}^{2n} g_i(1, \frac{k_2 - 1}{n_2 - 1}).$$

$$\begin{aligned}
\mathbb{E}[\widehat{Y}^1(1)] &= \sum_{j=1}^{2} \mathbb{E}\left[\frac{\sum_{i=1}^{n}[Y_{ij} \cdot I(T_{ij}=1)]}{\sum_{i=1}^{n} I(T_{ij}=1)} \cdot I(S_j=1)\right] \\
&= \sum_{j=1}^{2} \mathbb{E}\left(\mathbb{E}\left[\frac{\sum_{i=1}^{n}[Y_{ij} \cdot I(T_{ij}=1)]}{\sum_{i=1}^{n} I(T_{ij}=1)} \cdot I(S_j=1)\right] \mid S_j\right) \\
&= \sum_{j=1}^{2} \left(\frac{1}{2} \cdot \frac{1}{n} \sum_{i=1}^{n} g_{ij}(1, \frac{k-1}{n-1})\right) \\
&= \frac{1}{2n} \sum_{i=1}^{2n} g_i(1, \frac{k_1-1}{n_1-1}).
\end{aligned} \tag{C.1}$$

The second line is obtained by applying the law of iterated expectations to the first line. The third line follows from the second line, along with Corollary 3.1 and Assumption 3.3. Specifically, note that

$$\left(\mathbb{E}\left[\frac{\sum_{i=1}^{n}[Y_{ij} \cdot I(T_{ij}=1)]}{\sum_{i=1}^{n} I(T_{ij}=1)} \cdot I(S_j=1)\right] \mid S_j\right) = \begin{cases} 0 & if\ S_j = 0 \\ \frac{1}{n}\sum_{i=1}^{n} g_{ij}(1, \frac{k-1}{n-1}) & if\ S_j = 1 \end{cases}, \tag{C.2}$$

with the probability $S_j = 0$ being $\frac{1}{2}$.

It can be similarly shown that $\mathbb{E}[\widehat{Y}^2(1)] = \frac{1}{2n}\sum_{i=1}^{2n} g_i(1, \frac{k_2-1}{n_2-1})$.