THE LIMITING EFFECTS OF BANKING REGULATIONS ON EMISSION PERMIT MARKETS: AN EXPERIMENTAL ANALYSIS

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This thesis is dedicated to (in no particular order) my wife Karen, my son Max, my father and mother. Each of them has contributed in their own way to the completion of this work.

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ERMS	Emissions Market Reduction System
VOC	Volatile Organic Compound
EPA	Environmental Protection Agency
IEPA	Illinois Environmental Protection Agency

SUMMARY

It is the aim of this study is to offer a comparison of the costs of four different banking regulations, and their associated benefits. The costs are abatement costs. Two benefits are addressed; first, the reduction in intertemporal emission spikes, and second, the reduction in spatial emissions spikes. The banking regulations tested are: unlimited banking, a one year bank expiration provision, enforcing an upper limit of emissions on each firm, and halving all banked permits. There is often a tradeoff of lower abatement costs with higher emissions spikes, and vice versa. Experimentally the regulation that halves all banked permits controls intertemporal emissions spikes well, but had the highest aggregate abatement costs. Putting an upper limit on the amount of pollution any one firm may emit performed better at reducing individual emission spikes (spatial hotspots) than others, while costs were not statistically different from the unlimited or one year banking regulations. The unlimited and one year banking provisions were not, for the most part, statistically significant with respect to one another. Ultimately, the damage function of the emitted pollution will determine which banking regulation has the greatest ability to maximize welfare.

1. INTRODUCTION

Managing the environment will be this century's greatest task. The market mechanism will play a key roll in the task of allocating this resource that has, until recently in human history, been largely taken for granted. To further the understanding of how a market system of independent agents working for their own self interest can assist in a least cost solution to environmental control, several market rules concerning holding or banking the right to emit will be evaluated.

There have been a number of contrived markets instituted in recent history which attempt to control emissions at least costs. Each of these markets have their own unique rules. Market based programs have been suggested in limiting the emissions of Volatile Organic Compounds, Nitrous Oxide, Ozone, Sulfur Dioxide, and Carbon Dioxide. Each of these proposed or implemented programs have had different regulations. There has yet to be a comprehensive study comparing the different rules associated with these markets. This lack of research is easily understandable, considering each of the programs attempt to regulate different pollutants, in different quantities, with a different makeup of firms and abatement technology. However, there is one common thread to all market based emission reduction programs.

Market based emission reduction programs hold the possibility of reducing aggregate abatement costs when compared to a traditional emission reduction policies, with the same amount of aggregate emissions. Lower aggregate abatement costs without increasing aggregate emissions, when compared to traditional regulation, could lead to more efficient markets in that external costs are internalized at their true market value, rather than mandated abatement costs imposed by the government that may have no relation to the true value of the abated pollutant. Market based programs provide incentives for emission reduction technology innovation, reducing the cost of emission reductions further. Also, firms may be more open to the idea of a market based approach due to the potential for lower abatement costs, and the ability to manage their own emission reductions.

Though market based programs may be superior to traditional regulations in reducing aggregate abatement costs, market based programs also hold the potential for spatial or intertemporal emission spikes. These emission spikes are an artifact of distributing emission reductions to firms in the market whom can accomplish reductions least costly. These elevated emissions in space or time may cause great harm locally. It is the drawback of emissions spikes that additional market rules and regulations attempt to fix.

This study is designed to evaluate the rules pertaining to limit emission spikes in a market based emission reduction framework.. Different market regulations may be appropriate for different external costs, as explained in the thesis. The main thrust of the thesis is that there are tradeoffs between costs and emission spikes when comparing different market based rules. Market based rules should be implemented based on the damage that the pollutant causes. Though several market based regulations have been proposed and used in an actual market, there has not been a formal assessment of these rules. With the correct market based rules, a more appropriate cost can be internalized from emissions released.

To study these different market rules, and the costs and emissions associated with them, computer simulations and experiments conducted in the lab will be employed. Because the implemented market based rules are applied to markets with very different attributes, it is hard to compare the outcomes of the competing rules. By simulating a market, both using computer software and in a lab with human participants, a direct comparison can be made between the rules. It is in these sterilized situations that analysis can be focused on the different market rules, and not be befuddled by other exogenous factors. Computer simulations offer least aggregate cost solutions for these non-linear minimization problems, while experiments allow real humans to experience these market rules, and react to them as they see fit. This type of tight focus on the competing regulations cannot be obtained with field data at the current time.

It is informative, at this point, to state the hypotheses for this study, and discuss the main question this study answers.

The three hypotheses for this study are:

H_o^1: Different banking regulations on market based emissions reduction programs yield statistically significant differences in abatement costs.

H_o^2: Different banking regulations on market based emissions reduction programs yield statistically significant differences in intertemporal emission spikes.

H_o^3: Different banking regulations on market based emissions reduction programs yield statistically significant differences in spatial emission spikes.

These three hypotheses build the overarching question of: What are the costs and benefits of using one banking regulation over another? It is the aim of the study to identify the costs of several banking regulations and their associated benefits. The costs will take the form of abatement costs. The benefits will be defined as, first, the reduction in intertemporal emission spikes, and second, the reduction in spatial emissions spikes. Obviously, if one banking regulation has lower aggregate abatement costs and lower emission spikes compared to another treatment, there can be a strict preference made for the treatment which performs better. However, as demonstrated in this study, there is often a tradeoff of lower abatement costs with higher emissions spikes, and vice versa. Ultimately, the damage function of the emitted pollution will determine which banking regulation is best. This study simply offers a comparison of these different banking regulations, which can then be weighed against a damage function to optimize welfare.

Chapter 2 will conduct a review of the relevant literature. The first topic discussed in this chapter will be environmental economics. This topic will sketch the basic tenets of environmental economics, and give an overview of the different tools that aid in constraining emissions, with special attention paid to the market mechanism. Secondly, experimental economics will be examined with a discussion of what this tool can offer an economist, and the limitations of this device. Thirdly, further attention will be paid to experimental work that has already been completed regarding market based solutions for environmental control[1].

Chapter 3 will further address the topic of experimental economics by constructing the experiment parameters used in this thesis. The chapter will discuss the firms in the market, including: the firm's cost of abatement; emissions produced; and revenue received by the firm. Also, the rules in the market are outlined, noting the actions available to the firm being buying, selling, banking, permits and using permits to

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[1] Obvious omissions of this review are specific environmental programs. There are now numerous programs, many associated with volumes of literature. Specific attributes of a limited number of programs will be discussed later in the study.

reduce abatement costs. This chapter also discusses the conversion rate of experimental dollars to actual US dollars and it's relevance. Detailed experiment protocols are given for the "Banking Without Trade" and the "Banking With Trade" experiments. The chapter concludes with a summarization of the differences in the Cronshaw Brown Kruse (1999a) experiment set up and the study at hand.

Chapter 4 uses the market and firm constructs to simulate market outcomes. Intertemporal aggregate least costs and emissions are calculated from these simulations for each of the 8 different banking rules and two different environments. Emissions evaluated for either intertemporal emission spikes, or spatial emission spikes. The aggregate abatement costs of the 8 different banking rules are then compared to their respective emissions. Ratios of ratios are then calculated comparing the percentage aggregate abatement cost reductions over the percentage emission spike increase for each of the 8 banking treatments and both environments.

Chapters 5 and 6 analyze the results of the experiments conducted in the lab. Chapter 5 gives summary statistics of both sets of experiments. Number of participants, number of trials, treatments applied, summary costs and emissions associated with the different treatments and environments are also included.

Chapter 6 focuses on statistical significance of observed outcomes, by using Ordinary Least Squares to econometrically analyze the results. Five banking treatments applied to the Banking Without Trade experiments are scrutinized, and the four banking treatments applied to the two environments are analyzed. The main conclusions of chapters 5 and 6 are that there tradeoffs of abatements costs and emission spikes when instituting different banking regulations.

Chapter 7 reiterates the notion of cost vs. emission spike tradeoffs by reviewing the supporting simulated and experimental evidence from chapters 4, 5, and 6. Policy recommendations are given, mainly that the banking regulation instituted should match the pollution damage function the market is facing. The thesis concludes with potential future research.

In summary, Chapter 2 sets the stage for the thesis by sketching the issue of external costs, and how environmental economics attempts to address this issue. Chapter 3 presents the market constructs which are needed to build the simulations in chapter 4. After the least cost solutions are derived in Chapter 4, they are used to evaluate the observed outcomes of the experiments conducted in the lab, in chapters 5 and 6. The results from Chapters 4, 5 and 6, as Chapter 7 concludes, are that there are tradeoffs between aggregate abatement costs and emission spikes.

2. LITERATURE REVIEW

2.1 Environmental Economics

The core problem is quite simple: lack of property rights. When there is a resource which has value, yet which is not owned, that resource will be exploited until the net benefit received from it is zero. As Varian notes, the first theorem of economics does not hold in the presence of externalities, due to agents not facing the correct prices (Varian 1992)[2]. More specifically, as Dales (1968) recognized, the rents from the resource will be extracted until there are none left.

Part of the problem lies in an agent's ability to lay claim to specific units of a natural resource. A basic example is the air that is breathed. One has little, if any, control in which atmospheric molecules are draw into ones body. If one draws in air that is tainted with harmful chemicals that person is made worse off. If these harmful chemicals were put there by another party (especially in the case where this party gains profit) then this party has gained at the harmed person's (and possibly many others) expense. Is this an acceptable situation? The harmed party would not think so. Additionally, if one were interested in maximization of surplus, this is not surplus maximizing. Surplus is defined as the difference in the value placed on a good or service and the price actually realized by the market participant, whether it be the consumer or producer.

If it has been decided by society that no one owns the air, then the harming party would not take others' unhappiness into account when maximizing profit because they have just as much right to the natural resource as does anyone else. This problem is

[2] See page 432.

similar to The Tragedy of the Commons (Hardin, 1968). Each agent acts in their own best interest, not taking into account the external cost that is born by others. When positive marginal harm is imposed on other parties corrective action is needed to protect surplus maximization.

In a neoclassical economics framework of surplus maximization in regards to environmental economics, the optimal level of emissions would equate the marginal emission damage with the marginal cost of emission reduction. Once this is determined, the total cost of the damage and emissions reduction are know, and more importantly the level of emissions reduction. As an illustration, assume a firm is creating external costs harming another agent. Also assume the marginal damage of the last unit of pollution emitted is greater than the marginal cost of reducing that unit of pollution. In this case surplus can be increased by reducing emissions by one unit. The one unit decrease in emissions would relieve the harmed party more than the cost to the firm to reduce the pollution. Or, the marginal benefits outweigh the marginal cost. By continuing this illustration, with further reductions in emissions, it can be seen that there is a point where the marginal damage equals the marginal cost of abating emissions[3]. At this point the analysis has reach an optimal outcome. If further abatement takes place beyond where the marginal costs and benefits equal, the marginal benefits are reduced to below the cost of abatement, resulting in a less efficient outcome.

There are several options in obtaining an optimal level of emissions. A seemingly straightforward solution would be for the harmed party to demand compensation (Coase 1960). This would produce an efficient result, as defined above, as the emitter would

[3] This assumes continuous and linear abatement costs and damages with slopes (marginal, marginal costs/benefits) of opposite signs at the very least.

reduce pollution until the marginal cost of abatement was equal to the marginal compensation the harmed party is demanding. The remaining "harm" endured by the harmed party would be offset by a payment from the emitter. It should first be noted that the harmed party would need some sort of leverage, backing by a governing agency for example, to realize their demands. Additionally, in practice it may be hard to determine that a claimants' harm was caused by certain emissions, and potentially harder to determine whom emitted the harmful substances. There are inherently high transactions costs associated with this method.

Another argument made by Coase (1960) is that a harmed party would be willing to pay the emitter to reduce emissions. This would produce an efficient result from the harmed party to pay the emitter up to the point where the marginal harm is equal to the marginal cost of abatement. Again, there is the potential for high transactions costs. First, there may be a number of harmed parties who would be willing to pay small sums which would aggregate into a large abatement cost fund. Second, it may be hard to discern who is emitting the harmful substance. The firm(s) may be unwilling to share the true cost of abatement, and some agent of the harmed party would need to enforce compliance.

One decision a society may make is to directly limit the amount of emissions by each of the emitting parties, or to limit the use of the common natural resource on an agent by agent basis. This requires a governing agency to dictate the level of effluent, as a stock or flow in one period, on a firm by firm basis. This may be done by requiring firms to adopt specific control technologies including abatement equipment, different inputs, or processing procedures. This has been the most common type of regulation in

America[4]. This can be an effective way to ensure emissions reduction, but does have problems. First, as expressed later in this chapter, it is one of the costliest methods of emissions reduction. The governing agency must recognize the appropriate abatement technology for each firm, and monitor the firm to ensure it is adopted, used, and maintained properly. Secondly, the level of emission has the potential to vary if control technology adoption is used. The amount of emissions per unit produced may be limited, but this does not take into account the level of production, and therefore the level of total emissions by one firm. This may also put undue and uneven cost on some firms, making it impractical for certain firms to continue production.

Another tactic that has been used is to tax emissions (Pigou 1920). The optimal tax is equal to the value set when the marginal damage is equated to the marginal abatement cost. At this tax level emitters will partake in abatement activities until the marginal cost of abatement equals the tax. An efficient level of emissions is achieved with some desirable results. First, the firm chooses which and how much abatement technology to invest in, allowing each firm to cost minimize their emissions. This also induces investment in emission reduction technology to reduce emissions further, which reduces a firm's tax liability. This relieves the governing agency from choosing which and how much of each technology adoption are appropriate. Monitoring and enforcement of the tax are still required. Additionally, this program does not ensure that a strict limit on emissions is obtained. Firms are able to increase emissions to any level, provided they pay their tax. Both a plus and a minus of this policy is the tax revenue, welcomed by the governing institution, unwelcomed by the taxed parties. Again, this may put high costs on some firms, making it impractical for the firms to continue

[4] An example would be the US Hazardous Air Pollutants (HAP) program (EPA 2005d)

production, but each firm would observe the same tax rate, and in theory the same final marginal cost.

Finally, a market based solution, as most notably discussed by Dales (1968), Montgomery (1972), and Tietenberg (1985), has been the program of choice in recent air pollution markets. Three examples relating to air are the US Environmental Protection Agency's (EPA) SO2 Acid rain program (EPA 2005a), NOx SIP Call (EPA 2005b), and the Illinois Emissions Reduction Market System (ERMS) (IEPA 2005). The governing agency indirectly obtains least cost emissions reduction by allocating the right to emit, either at a cost or free. Aggregate allocations limit total emissions to an efficient level. Firms then redistribute these rights to pollute, called permits, through a market mechanism. Firms with high marginal abatement costs enter the market to purchase permits until the price of a permit is equal to their marginal abatement costs. Firms with low marginal abatement costs enter the market to sell permits until the price of a permit is equal to or less than the cost of abatement[5]. When the market clears, in theory, marginal abatement costs will be equated among all participants, which will equal the price in the market, which will equal the marginal damage to the harmed party. Again, if the level of permits, and therefore emissions, is set to be efficient, the marginal cost of abatement will be equal to the marginal damage. As in each of the above cases, the overriding outcome of pollution control is for emissions to be reduced to where the marginal damaged is equal to the marginal abatement cost.

How should the permits be allocated? If the governing agency charges a fee for permits, the fee should be equivalent to or less than the cost of the marginal damage at

[5] A clear example of this can be found in the appendix of Kosobud et. al. 2002

the prescribed emissions reductions[6]. If it is equal to the marginal damage it will act as a tax. The firms will purchase permits until the cost of the permit is equal to their marginal abatement cost. If the fee for the permit is less than the marginal damage, as it is has been in US markets where they are given free, the allocation in general does not matter[7]. Again, firms will redistribute the permits, according to permit price and marginal abatement costs.

A few notes should be made. First, if the governing agency is omniscient it can allocate permits in a least cost solution equal to a market redistribution. In this way a governing agency can be as efficient as a market. However the assumption of being omniscient is a monumental one. For this reason a market may be a better choice for distribution of the right to pollute, giving the party closest to the costs, the firm, the option of choosing the best solution. The governing agency must still monitor firms to thwart any emissions misrepresentation, and enforce the program. As in the tax scheme, there will be innovation stimulation in regards to emissions reduction technology. Environmentalists may experience a sense of social justice, due to firms with higher emissions in general needing to purchase permits, and firms who reduce emissions in general needing to sell permits. Emitters pay, while reducers get paid.

A favorable detail of a market system from a political economic viewpoint is that permits in US programs are in general given free of charge (Joskow et al. 1998, Ellerman et al. 2000)[8]. Anecdotally most firms in the US are tax averse. The thought of being

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[6] If the fee is equal to the marginal damage at optimal levels of emissions (marginal damage equal marginal abatement cost) it will also be equal to the optimal tax.
[7] There must be some reasonable assumptions including that the market is efficient, and no firm has market power. There may be allocations with certain transactions costs which would not lead to an efficient market (Stavins 1995). This topic, however is outside of our objective.
[8] In the US SO2 market, a small percentage of permits are auctioned at the beginning of the compliance period (EPA 2005e, Joskow et al., 1996, Cason and Plott 1996).

taxed on emissions does not sit well with industry. However, a market based system, where permits are given free seems to be more acceptable. Giving permits for free can be a strong negotiation token when instituting emissions reductions.

Another positive aspect of this program is that emissions are capped at a specific level. For each unit of emissions not abated, a permit must be surrendered to the governing agency. If a firm increases emissions, they must obtain more permits from other participants.

But this system is not without faults. First, the price in the market will not only reflect marginal abatement costs, but transactions costs also. If finding a trading partner is a costly experience, trading will be diminished, and prices will be higher, reducing the efficiency in the market (Cason and Gangadharan 1998, 2001, Stavins 1995). More serious are spatial and intertemporal hotspots.

Spatial hotspots occur when a particular firm, or several firms close in geographic location increase their emissions in any one period via purchasing a large share of the permits. This has the potential to allow firms to emit at dangerously high levels. This type of malady will be monitored throughout this study, and noted where appropriate.

Intertemporal hotspots occur when, in the aggregate, permits are allowed to build up over time, and a large number of this buildup, or bank, are used in one time period (Kosobud et. al. 2004a). The governing agency must decide if permits can be saved from one reconciliation period to another, and if so, what rules surround this act. This wart on the face of market based solutions is the very source of environmental concerns toward this type of scheme. Differing market rules addressing this downfall have received some attention, with different rules being instituted in several markets. These rules are at the heart of this study. A number of rules will be discussed and their performance simulated and tested in later chapters. For now the attention will turn to experimental economics.

2.2. *Experimental Economics*

Experimental economics is a tool in which the researcher can emulate an existing or possible economic environment and institution in a controlled setting to observe behavior of participants and outcomes of prescribed treatments (Hagel and Roth 1995, Davis and Holt 1993). The experimenter can then modify the environment and/or institution in a specific parameter and observe the results. Experiments are done for a number of reasons, one of which is to compare environments and/or institutions (Smith 1994, Friedman and Cassar 2004). The intention of this study is to compare four different banking rules and two different environments using this very informative tool. With experimental economics a variable change in an existing market can be tested relatively inexpensively compared to testing the same change in a real market. This is especially true if the experimenter is testing a number of different variables.

Experimentation can be thought of as collecting data in a laboratory, rather than in the field. The experimenter organizes human participants into a setting found, or that could be found, in the field. The participants act as agents in this stylized setting. The experimenter sets the treatments, informs the participants of select parameters, and allows them to act in their own best interests.

The laboratory allows the experimenter to modify the institutions and environment to test hypotheses. The obvious criticism is that it is so far removed from naturally occurring situations. This is what makes it valuable. The laboratory can scrutinize a specific economic phenomenon, holding all other variables constant. One specific criticism is that participants are usually undergraduate economics majors. Though this is entirely true, there have been studies conducted addressing this concern. A study by Smith et al. (1988) shows that in an experiment concerning asset bubbles, "professionals and business people" (pp. 1130) do no better than undergraduate students.

Before specific environmental experimental studies are discussed, a brief review of the experimental lexicon is undertaken. A participant is a human taking part in an experiment. An experiment is a set time period in which participants are able to make decisions under a specific environment and treatment. This includes instructions on how to make decisions and potential use of a computer interface. In this study it is a set of 9 periods. A session is a group of experiments taking place during a set time with the same group of participants, either one morning or one afternoon. A cohort is a group of participants who participated under that same set of parameters. An environment is a set of parameters in which all participants are subject to within an experiment. A treatment is a set of parameters that remain constant over two or more environments. These terms are also found in the DEFINITIONS at the conclusion of the study.

2.3. Emissions Rights Markets

2.3.1. Theoretical and Empirical Studies

Montgomery (1972) was not the first to discuss decentralized environmental regulation through contrived markets, but he did prove that markets are a least cost solution to reducing environmental externalities. However his brilliant model was static in nature, and did not address important intertemporal properties, namely banking. Banking gives agents in the environmental market the ability to save permits from one period, and use them in a later period. To build upon Montgomery's work, Cronshaw and Brown Kruse (1996), and Ruben (1996) offer an intertemporal model which included banking, profit regulation, regulation risk, and interest rates. These models show that decentralized markets are at least as cost effective in reducing aggregate emissions as traditional command and control regulations, and may be less costly. Additionally it is shown in these studies that banking increases the ability of firms to further reduce costs over trading alone. Banking is allowed both forward and backward, that is banking and borrowing permits from future time periods. Also included in these calculations are a discount rate for use of future permits and discounting borrowed permits.

Yates and Cronshaw (2001) further the mathematical analysis by adding a function modeling damages from emissions. They show that it may be socially optimal to institute banking in a market even if the damage caused by one emitter is fairly large. They also employ a discount rate for the use of banked permits.

Again Cronshaw and Kruse (1999b) take up the issue of optimal banking over time. One of their main conclusions is a firm would prefer a market which allowed banking over a market that did not. This does not guarantee that a firm will benefit from banking, only that there may be an opportunity to do so. It is only through experimentation or a real market that we can discover if participants can realize the gains from banking, especially with alternate banking rules.

Rubin and Kling (1993) and Kling (1993) model an environmental market with the intent of discovering the potential cost reduction of banking in an actual market. This study revolves around a proposed, yet abandoned, program of reducing hydrocarbon emissions from light-duty vehicles. With detailed emission control cost data, they create a computerized model of the market. In their model they include intra-firm permit averaging, banking, and inter-firm permit trading. Their main conclusion is that, with a discount rate of zero, the combined cost savings of averaging, banking, and trading is 11.8%. Of that 11.8%, 3.5% is due to the ability to bank emissions reduction, or nearly 33% of the total cost savings.

Schennach (2000) also addresses banking in environmental markets under the framework of the 1990 Clean Air Act Amendments. Included in this study are changes in demand, environmental regulations (regulation risk), technology, uncertainty and borrowing permits from future periods. It is noteworthy that she does show that with uncertainty introduced into the model, contrary to Cronshaw and Kruse, banking may take place even if the expected price rises less than the interest rate.

A direct empirical study by Ellerman and Montero (2002) was conducted regarding the US SO2 market. They took the first seven years of market data, created a model of optimal banking, and estimated the optimal banking under different discount rates and counterfactual emissions. Their conclusion was "…reasonably efficient banking is indicated."[9] The participants were able to extract a good portion of cost savings due to banking in an market with unlimited banking.

2.3.2. Economic Experiments In Permit Banking

The subject of emission rights banking has been studied in a number of different papers and experiments. Though the main thrust of the majority of papers has been on the trading aspect of a tradable emissions permit market, banking has also been tested. Mimicking the SO_2 allowance market as prescribed by the Clean Air Act Amendments of 1990, Cronshaw and Brown-Kruse (1999a) explore the efficiency of trading permits,

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[9] Ellerman and Montero, 2002, p 24

banking, and permit trading with banking, each with its own treatment. They report on 51 subjects in a Banking Only treatment, and 50 subjects, or 5 experiments under a Trading with Banking treatment. Trading here is through a discriminative auction, not an open outcry double auction. In a discriminative auction buyers submit bids to a single seller, the conductor in this case, who offers a fixed number of permits to the highest bidders at the price offered[10].

In each period, participants were asked to maximize their profit by choosing how to reveal their supply and/or demand schedules to the experimenter. A market clearing mechanism was administered and participants were informed of their buys or sells for that session. The participants were then asked to make usage and banking decisions regarding their current stock of permits, where their usage decisions affected their per period production costs. Then, permit and monetary balances were updated and recorded.

A fixed amount of experimental dollars were allocated to each participant at the beginning of every period. Emissions were constant at 10 units for each period. Six permits were allocated in each of periods 1-4, and three permits (half of earlier periods) in each of periods 5-12. The banking horizon was unlimited in this experiment. Participants were also allowed to purchase permits from the experiment conductor at a very high predetermined cost.

The results were based on 5 different sessions. On average, participants achieved cost reductions of 56.3% of potential maximum cost reductions when compared to a command and control environment. When disentangling the efficiency gains due to

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[10] It should be noted that the experiments in this thesis use the basic setup from the Trading with Banking treatment with modifications. This will allow previously accepted protocol and participant directions to be used, as well as allow direct comparison of these results with other similar papers.

banking and trading, it was found that gains due to banking ranged from –10% to 92%, and gains from trading ranged from 39% to 76% of potential maximum gains. A –10% efficiency gain would mean that on average the participants would have been better off if there had been no banking.

Other experiments involving banking looked at the Canadian tradable permit program for nitrous oxides in southern Ontario. Muller and Mestelman (1994) and Mestelman et. al. (1999) discussed experiments conducted at three different laboratories to explore this program with an experiment similar to the Cronshaw studies. These experiments included banking, but also included both trading in shares of emissions rights allotment where each share represented a certain number of coupons, and trading of coupons which represent actual emissions rights. Coupons were allotted in relation to how many shares of the emissions rights the participant held. One share in period 1 was worth 14 coupons, in periods 2, 3, and 4 a share was worth two coupons, and in periods 5 through 12 a share was worth one coupon.

The authors in these experiments used participants from McMaster University and compared their results with the results from the University of Arizona, and the University of Colorado, which was this paper's major theme. In both the McMaster University and University of Arizona sessions there was under-banking of coupons in the beginning of the experiment, with under use of permits in the latter sessions. In the University of Colorado sessions participants were unable to realize gains from banking of permits in many sessions due to over banking in early periods. Average cost savings over Command And Control were 26.4% for Arizona participants, 56.3% for Colorado participants, and 74% for McMaster participants.

In papers by Godby et. al. (1997) and Mestelman (McMaster Experimental Economics Laboratory Publications #21) the use of banking was very effective, when applied to emissions markets. Not only did they smooth out announced reductions of permits halfway through the experiment, they also reduced emissions reductions costs. In both experiments, emissions in future periods were uncertain. Treatments were no banking allowed, allowed, and banking allowed with trading of shares of emissions rights. When allowed, banking made a positive impact on reducing the cost of abatement. In Mestelman (#21) there was over-banking in the middle periods.

Godby et. al. (1997) suggest two reasons for banking causing the increase of efficiency. First there is the direct result of participants being able to reallocate permits over time to reduce differences in abatement in different time periods. Second, banking reduces the need to build up reserves for unforeseen increases in emissions. This is due to banking for intertemporal efficiencies acting as a back up for these unforeseen increases.

Note that all studies cited above allow unrestricted banking, when banking is present. There are no banking horizons, or other limitations on banking or trading. Godby et. al. (1997) suggests there are some efficiency degradation due to the complexities when banking and trading in one or more markets (shares and permits) is combined. Cronshaw et. al. (1999a) circumvent this with a treatment that does not involve trading. The study in the remaining chapters includes banking without trading and banking with trading, and will institute different banking rules, which is where this work will depart from past experiences.

3. RESEARCH METHODS

3.1. Introduction

Experimental economics will be used to build a data set that will be analyzed in chapters 5 and 6. This dataset will come from a controlled environment with real people. Experimental economics affords us the opportunity to directly observe behavior of market participants in real time. Additionally, constructing the "economy" allows modification of the environment that the agents act in. This includes dictating the rules of the market, initial allocations, and even values of scarce resources.

In order to gauge the experiments with respect to costs and emission spikes, predictions and benchmarks are needed. These benchmarks will come from computer simulations. The simulations will solve for the least cost solution, as that is what the participants, and more technically, firms in a real market, are solving for. The results will give the lowest abatement cost associated with each of the banking regulations while also offering aggregate and individual emission levels per period, under two different environments, as discussed later. The parameters of the simulations, and ultimately the experiments, are defined in this chapter.

The simulations and experiments consist of 5 or 10 independent participants, each representing 1 of five firm types. Each firm's production, revenue, and potential emissions are predetermined. Each firm is allocated the right to emit a specific number of units. The firm must surrender one right for each unit of pollution they emit. Any remaining units of emissions must be abated. Abatement has a positive costs to the firm. The firm's objective is to minimize cost. They can do this by redistributing their allocated permits over time (banking), and/or trading with other participants. Participants are not allowed to borrow permits from future periods, nor can they obtain permits from outside sources (from the regulatory agency for instance). Participants are informed of their predetermined production (emissions), revenue, abatement costs, permit allocation per period, number of periods, and number of participants prior to the start of the experiment. Participants are not informed of others' revenue or abatement costs.

For the simulations and experiments conducted without trading, one firm type is used, one environment type and five banking treatments. Simulations for banking with trading include five firm types, two environments, two different levels of pollution (emissions), two levels of permit allocation, and eight banking rules. Four of the eight simulated banking rules are used as benchmarks, therefore only four banking rules will be employed in the banking with trading experiments. Each of the eight banking rules, along with the other parameters, will be discussed and justified in the following section. The firms' abatement cost structure will be explained first, which remains constant throughout the study.

3.2. *Firm Types*

Each banking with trading simulation (and experiment) includes five independent participants, each representing one of five firm types[11]. Five firm types are used to ensure a large array of differing marginal costs, the array allows the participants to cost minimize via trading, and reduces monopoly or monopsony power any one participant may have. Note that when 10 firms are used in an experiment, firms 6-10 have the same costs, revenue, emissions, allotment and so on as firms 1-5. The information firms 6-10

[11] Each firm type is duplicated when ten participants are used.

receive are simply duplicates of firms 1-5, with different headings. There is no fundamental difference between the two sets of firms.

The total abatement cost function is defined as:

$$f(c) = a_i c_{it}^2$$

The variable a_i is 2, 4, 8, 10 and 12 for firms 1 through 5 respectively, and c_{it} is the number of units of pollution abated by firm i in time t. Note that the total cost function is differentiable, and marginal cost is linear, continuous and increasing. As abatement increases, so does the marginal cost of abatement. This equation was chosen because it was used in the flagship study by Cronshaw et. al. (1999a). This also creates a nice variety of marginal costs, which fosters trading. Firms are also given unique per period revenues, depending on the environment (see section 3.4 and tables VI and VIII). Below are tables I, II, III, and IV which list the total and marginal costs for each of the five firm types. Costs for abatement of 1-10, and 1-19 units of pollution have been listed. These are included because in Environment A emissions are 19, while Environment B only has 10 units of emissions. Units of abatement 1-10 for both tables are the same, but have been included for convenience.

TABLE I

TOTAL COSTS FOR ABATEMENT OF 1-19 UNITS (ENVIRONMENT A)

Units Abated	Firm 1	Firm 2	Firm 3	Firm 4	Firm 5
1	2	4	8	10	12
2	8	16	32	40	48
3	18	36	72	90	108
4	32	64	128	160	192
5	50	100	200	250	300
6	72	144	288	360	432
7	98	196	392	490	588
8	128	256	512	640	768
9	162	324	648	810	972
10	200	400	800	1000	1200
11	242	484	968	1210	1452
12	288	576	1152	1440	1728
13	338	676	1352	1690	2028
14	392	784	1568	1960	2352
15	450	900	1800	2250	2700
16	512	1024	2048	2560	3072
17	578	1156	2312	2890	3468
18	648	1296	2592	3240	3888
19	722	1444	2888	3610	4332

TABLE II

TOTAL COSTS FOR ABATEMENT OF 1-10 UNITS (ENVIRONMENT B)

Units Abated	Firm 1	Firm 2	Firm 3	Firm 4	Firm 5
1	2	4	8	10	12
2	8	16	32	40	48
3	18	36	72	90	108
4	32	64	128	160	192
5	50	100	200	250	300
6	72	144	288	360	432
7	98	196	392	490	588
8	128	256	512	640	768
9	162	324	648	810	972
10	200	400	800	1000	1200

TABLE III

MARGINAL COSTS OF ABATEMENT OF 1-19 UNITS (ENVIRONMENT A)

Units Abated	Firm 1	Firm 2	Firm 3	Firm 4	Firm 5
1	2	4	8	10	12
2	6	12	24	30	36
3	10	20	40	50	60
4	14	28	56	70	84
5	18	36	72	90	108
6	22	44	88	110	132
7	26	52	104	130	156
8	30	60	120	150	180
9	34	68	136	170	204
10	38	76	152	190	228
11	42	84	168	210	252
12	46	92	184	230	276
13	50	100	200	250	300
14	54	108	216	270	324
15	58	116	232	290	348
16	62	124	248	310	372
17	66	132	264	330	396
18	70	140	280	350	420
19	74	148	296	370	444

TABLE IV

MARGINAL COSTS OF ABATEMENT OF 1-10 UNITS (ENVIRONMENT B)

Units abated	Firm 1	Firm 2	Firm 3	Firm 4	Firm 5
1	2	4	8	10	12
2	6	12	24	30	36
3	10	20	40	50	60
4	14	28	56	70	84
5	18	36	72	90	108
6	22	44	88	110	132
7	26	52	104	130	156
8	30	60	120	150	180
9	34	68	136	170	204
10	38	76	152	190	228

3.3. *Banking Treatments*

The banking treatments Unlimited, One Year Banking Horizon, Limited Use, and the Budget Model Rule (BMR), which are applied to experiments, are estimated for minimum cost and associated permit use are. Treatments Command and Control, Banking Only, Trading Only, Both, BMR = 10, and BMR = 20, which are used as comparison, are also estimated for minimum cost and associated permit use but not used in the lab. These treatments were chosen to be either benchmarks, or are regulations that have been proposed or implemented by governing agencies.

Banking regulations are used to control intertemporal and spatial spikes in emissions. Different regulations control spikes differently. However, each are imposed for the purpose of emissions spike control, and if binding, also cause aggregate total costs of abatement to increase as controls are tightened. All banking treatments are discussed next.

The Command And Control (CAC) banking treatment does not allow for any banking or trading. This acts as traditional regulation, where firms are subject to a specific pollution reduction regiment or level of pollution. Most often, real world Command And Control regulations limit emissions to a certain rate, not strictly capping emissions. In this contrived market, production, therefore emissions, is set and predetermined. Traditional regulations may stipulate that a certain rate or flow of emissions are allowed, not specifically allotting permits, but in these simulations it is assumed that they are equal regulations.

Command and control, as well as the Banking Only, and Trading Only treatments, will act as baselines in comparing banking treatments to alternative traditional schemes.

Banking Only allows firms to bank as many permits for as long as they want. It does not allow firms to trade with one another. Trading Only allows firms to trade as many permits as they wish with any other participant, but does not allow for any banking of permits for later use.

The Unlimited banking treatment acts as the baseline, is used in the Cronshaw (1999a) experimental study, and used in the US SO2 market today. This does not limit banking or trading. Most notably, participants may bank as many permits for as long as they wish, and trade with any other participant.

The One Year Banking treatment stipulates that permits are only valid for the period issued, and the period after. At the end of the second period of the permits life, the permit becomes unusable and expires. This is the rule used in the ERMS market. The maximum number of permits that can be banked is equal to the yearly allotment of the firm holding the permits. However the firm is not necessarily totally constrained by this regulation. They may hire another firm, who would not otherwise hold permits, to bank permits for sale in latter periods. This may not explicitly happen, but if the price of permits are below optimal in early periods, some firms may buy and bank permits solely for the sale of said permits in the future to gain profit. In aggregate, though, emissions are constrained to two times the yearly allotment.

The Limited Use treatment is not a banking regulation per se, but limits the number of permits that can be used by any one firm. This can be viewed as an upper bound, set at baseline emissions. This differs from Command And Control in that, under Command And Control, the regulation states that emissions must be reduced below baseline for each participant. In the Limited Use treatment, permits are allotted so in aggregate emissions are reduced below baseline, but individual firms may bank or purchase permits which allows them to emit above their allotted number of permits. However, this treatment limits each individual firm from emitting above their baseline. Therefore one individual firm my emit more than their allotted number of permits, but not above their baseline. Not only does this limit the aggregate amount of emissions that can be emitted, but directly limits spatial emission spikes. This was suggested, but not used, in the ERMS market.

The Both treatment combines the One Year Banking Horizon and the Limited Use treatments. This was suggested in for the ERMS market. As can be seen in the next chapter, one of the regulations is binding, while the other is partially binding, or not binding at all.

The Discounted banking treatment, used only in the banking without trade experiments is a modification of the Budget Model Rule, discussed below. This rule simply subtracts 10% from any permits banked each period it is banked. If one permit were banked one period, it would be worth nine tenths of a permit in the second period.

3.3.1. Budget Model Rule (BMR – AKA Modified Banking Rule/MBR)

The NOx Budget Model Rule (EPA 2005c) was designed to limit permit banking in the NOx trading program, while offering the flexibility of not strictly limiting the amount a firm, or firms in aggregate can bank. This is a seemingly complex rule, that has simple intuitive results. It should first be noted that firms are allowed to bank as many permits as they wish, for as long as they wish, and withdrawal any number of their banked permits in any time period. Additionally, the rule states that a certain percentage of banked permits may be withdrawn on a one-to-one basis. The remainder of permits may be withdrawn, but with a charge. In these simulations two permits must be withdrawn from the number of banked permits for the use of one permit in the current period (EPA 2004). This percentage, or ratio, is formed by a constant set by the regulatory agency as the numerator, and the number of permits banked in aggregate as the denominator, or:

Regulatory constant / aggregate bank = ratio

As an example, assume the regulatory constant, c, is set at 50 units, and the aggregate bank, b, is 100 units. The ratio, r, would then be 50 / 100 = .50. Therefore fifty percent of banked permits may be withdrawn on a one-to-one basis, with the remaining fifty percent at a two for one basis. To further the example, assume a particular firm holds 8 banked permits. They may withdrawal 4 permits on a one-to-one basis, for 4 usable permits. They may also withdrawal the remaining 4 permits on a two for one basis, for 2 usable permits. The maximum number of permits that this firm can use from their bank is 6, from their stock of 8 permits.

There are several nuances of this treatment that should be noted. First, if the ratio is greater than one, all banked permits are eligible for withdrawal at a one-to-one rate. Second, as the number of banked permits increases, the total number of permits that can be withdrawn in one period on a one-to-one basis decreases (provided the rule is binding). Therefore the total number of permits that can be withdrawn in any one period decreases (due to the two for one basis), limiting the amount of emissions in any one period. Third, regardless of how many permits are banked in aggregate, the value of the regulatory constant will always be available for withdrawal at a one-to-one ratio in the aggregate. To solidify this idea, note the simple math:

Regulatory constant / aggregate bank = ratio

=>

Aggregate bank * ratio = regulatory constant (3)

So if the regulatory constant (c) is 50 units, no matter how many permits are banked in aggregate, 50 units will always be eligible for a one to one withdrawal in aggregate.

Why does this matter? Note that the rule concerns permits that are withdrawn only. This rule does not affect banked permits that are not withdrawn, and remain in the bank for later use. Provided that, on aggregate, firms do not draw the bank down by more than c permits in any one period, all permits withdrawn from the bank will be on a one to one basis. From a regulatory agency's viewpoint, the regulatory constant can be set at a level that environmentalists do not want exceeded (a reduction from baseline levels, for example – see BMR 10). If the level of withdrawal is exceeded, permit prices will be driven to double what they would otherwise be (explained later). This creates a great disincentive for a market to exceed the regulatory constant. Not only that, a withdrawal rate of two to one directly reduces the amount of pollution that the firms can emit over a certain period of time. This is because one permit is effectively thrown away for each permit withdrawn if the regulation is binding. Therefore the potential emissions tied to that one discarded permit will never be seen. These results will be borne out with review of the simulations concerning the BMR in the next chapter.

At the firm level, banked permits are also subject to the ratio. Two things to note about this. First, because permits are non-divisible, firms may be left with a whole permit that can be partially withdrawn both on a one to one, and two to one basis. This may reduce the number of permits that can be redeemed in a particular period depending on how the regulatory agency treats this permit. This is especially true in this simulation, due to the limited number of permits allotted to the firms (8 or 4). For larger systems, with hundreds or thousands of permits, this may not be a concern. Solutions to the partial permit situation may be to allow firms to redeem partial permits as a whole permit, which would increase the aggregate permits redeemable. Or, not allow the firm to redeem partial permits, as is the rule in this study. One could also devise a market for partial permits, though there may not be much of a market. The regulatory agency could purchase partial permits at a predetermined price[12]. Fortunately most of the simulations produced round numbers for comparison.

Secondly, barring the partial permit problem, firms may mistakenly make their banking decisions based on others' banking decisions. If a firm were to over-bank to ensure the ability to have permits at a one-to-one basis, they must incur the abatement cost associated with banking an extra permit. Additionally the firm will face the opportunity cost of using the extra banked permit withdrawn on a one-to-one basis instead of selling it on the open market, when others are buying permits on a two-to-one basis. It is for this reason firms should react to the market price, provided it is efficient and the firm does not have any market power, rather than try to create some form of reactionary formula in relation to others decisions[13].

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[12] Possibly at the average market price.

[13] Uncertainty of emissions has been assumed away with this analysis. This would be a fine topic for future research.

3.4. Environments of the Banking With Trading Experiments

There are two different environments in the banking and trading experiments, A, and B. Environments differ in amounts of emissions, permit allocations, and firm revenues. Both environments use nine emissions periods. Table V summarizes environment attributes of the four treatments used in the laboratory experiments, though all 8 treatments are simulated by the computer for both environments.

TABLE V
ENVIRONMENT ATTRIBUTES

Environment Attributes					
Environment	Number of Periods	Emissions	Permit Allocation	Treatments	Firm Revenue Per Period
A	9 (6, 3)	10 (6 periods), 19 (3 periods)	8 (6 periods), 8 (3 periods)	Unlimited, 1 Year Bank, Limited Use, BMR = 0	200, 200, 300, 375, 450 and 225, 275, 525, 650, 775
B	9 (3, 6)	10 (6 periods), 10 (3 periods)	8 (3 periods), 4 (6 periods)	Unlimited, 1 Year Bank, Limited Use, BMR = 0	200, 200, 300, 375, 450

3.4.1. Environment A

This first environment contains 10 units of emissions in periods 1-6, 19 units of emissions in periods 7-9, 8 permits allocated to each participant every period, and differing revenue for each participant. Note the constraint is in the units of emissions, as it nearly doubles in the latter three periods. Cost minimizing firms should, in aggregate, save permits in the early six periods, for use in the latter three. Due to the high marginal and total abatement costs that firms may endure, emissions were not increased until the 7th period, giving ample opportunity to save permits for the latter periods.

Nine periods were chosen to facilitate multiple experiments being run in one session[14]. In the original studies of emission permit markets by Cronshaw et. al. (1999a), emissions were constant, but permits were halved. The Cronshaw study specifically looked at the US SO2 market in the short term, as permits were to be reduced after the start of the market. Environment A was chosen to emulate a mature market which has an unchanging policy, with production increasing. These permit and emissions differences are another novelty of the current study.

It is assumed here that in these regulated industries growth would occur. Prior studies held growth constant. One could argue that growth could occur without an increase in emissions (or an increase in marginal costs). This could be argued two ways. First, real world emissions reduction takes many forms including afterburners, scrubbers, and production processes modifications. The initial cost of equipment and process modifications may be large, but the reduction in emissions may be great. These changes may also offer a minimal marginal cost of reducing one more unit of emissions.

Additionally, one could argue that new innovative technologies would be introduced that would keep the marginal cost of emissions abatement near the market value of a permit. Therefore production could increase, but with these new innovations emissions do not increase, or the marginal abatement cost decreases to keep permit prices at a stable level. Otherwise, as production increases, so will emissions and total and marginal abatement costs.

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[14] In previous studies 12 periods were used. The use of 9 periods enabled two to three experiments to be run in one session.

3.4.1.1. Per Period Revenue

With the shell of the experiment formed, the remaining variable which must be declared is the per-period revenue. This seemingly unimportant variable is, in fact, key to creating a successful experiment. There must be a balance between the choices the revenue allows the participant to make, and the value of the experimental dollar. To support the hypothesis that participants will cost minimize by banking and trading, every participant is implicitly offered the option of doing nothing. If a participant can simply sit in front of a computer screen and use the allotted permits each period without incurring a loss, and then choose not to, these actions will reinforce the notion that the participant trying to cost minimize through market activities. Another byproduct of this is that the chosen revenues minimize the potential of firms going bankrupt. Participants should not walk out empty handed, nor simply come to collect money with no effort.[15]

Conversely, the more experimental dollars that are given, the higher the conversion rate into US dollars, and therefore the lower the real value of the experimental dollar, holding average payment per participant constant. In all but six cases the participant has the opportunity to make at least 2 times their do-nothing option. The lowest increase in profits is 65% of the do-nothing strategy, with the highest increase being over 1000% of the do-nothing strategy.

The conversion rate is set to $10 per participant per experiment, provided the market is completely efficient and the participant makes optimal cost minimizing decisions. The worst case scenario is that US $6.05 is paid for the do-nothing strategy, and a maximum of US $10 for cost minimizing strategy in a perfectly efficient market,

[15] The reputation of an experiment (and experimentalist) among potential participants can have a large impact on recruitment.

according to the simulations. In the majority of cases, though, less than US $5 is potentially paid for the do-nothing strategy, and therefore more of the potential US $10 is for the cost-minimizing strategies. This does not preclude the participant from making unwise decisions, and earning a negative profit, nor a participant from making profits above the estimated maximums due to others' unwise decisions. Additionally, even with optimal decisions some firms may earn negative profits in an individual period, funding this loss with prior earnings.

The revenues were also chosen to maximize the number of choices that participants can make. Firm 1, the low-cost firm, should not be limited in the number of permits it can bank. Also, high-cost firms should be afforded the opportunity to purchase high cost permits in the latter periods. Therefore per-period revenue is different for each firm type. Revenue is the same for every period in 1-6 and 7-9, but differs in the former and latter periods in environment A,.

In the first 6 periods firm 1 makes a revenue 200 in lab dollars[16] each period, the cost of not using any permits (or banking all permits). Firm 2 makes 200 also, and is able to bank 5 permits per period, disregarding any transactions with other firms or prior earnings. Firms 3-5 are able to each bank 4 permits each period, making 300, 375, and 450 respectively. These are listed in table VI

In periods 7-9 Firm 1 makes 225, allowing it to still abate only 10 units of emissions. Optimally firm 1 will sell a number, if not all, of their permits for a profit. Firm 2 makes 275, which can be used for the first 8 units of abatement. Firms 3, 4 and 5 make enough each period to cover 8 units of abatement, earning 525, 650, 775

[16] From this point forward, if earnings are not marked as US$, they are experimental dollars. US dollars are denoted as US$.

respectively. Again, these do not include prior earnings, banking, or trading. All firms have the opportunity to do-nothing. Also note that as the price in the market fluctuates, particular firms may make more or less profit, depending on their cost structures. Table VII lists, for each firm type, the do-nothing profit, the profit with optimal aggregate banking and trading, percentage increase in profit for aggregate banking and trading over do-nothing, and the conversion rate of experimental dollars to $US.

TABLE VI
ENVIRONMENT A FIRM REVENUE

Environment A		
Revenue	Periods 1-6	Periods 7-9
Firm 1	200	225
Firm 2	200	275
Firm 3	300	525
Firm 4	375	650
Firm 5	450	775

TABLE VII
DO-NOTHING PROFIT, EFFICIENT MARKET PROFIT, POTENTIAL INCREASE IN PROFIT, AND CONVERSION RATIOS FOR ENVIRONMENT A WITH ALL TESTED BANKING TREATMENTS.

Unlimited				
	Do-nothing	Efficient Market	% Increase in Profit	Conversion into US $
Firm 1	$1,101	$2,415	119.35%	242 to US $1
Firm 2	$477	$1,188	149.06%	119 to US $1
Firm 3	$279	$1,809	548.39%	181 to US $1
Firm 4	$330	$2,472	649.09%	247 to US $1
Firm 5	$381	$3,216	744.09%	322 to US $1

1 Year Bank				
	Do-nothing	Efficient Market	% Increase in Profit	Conversion into US $
Firm 1	$1,101	$3,382	207.18%	338 to US $1
Firm 2	$477	$1,626	240.88%	163 to US $1
Firm 3	$279	$2,388	755.91%	239 to US $1
Firm 4	$330	$3,266	889.70%	327 to US $1
Firm 5	$381	$4,234	1011.29%	423 to US $1

Limited Use				
	Do-nothing	Efficient Market	% Increase in Profit	Conversion into US $
Firm 1	$1,101	$2,122	92.73%	212 to US $1
Firm 2	$477	$1,518	218.24%	152 to US $1
Firm 3	$279	$2,190	684.95%	219 to US $1
Firm 4	$330	$2,762	736.97%	276 to US $1
Firm 5	$381	$3,364	782.94%	336 to US $1

BMR - 0				
	Do-nothing	Efficient Market	% Increase in Profit	Conversion into US $
Firm 1	$1,101	$3,409	209.63%	341 to US $1
Firm 2	$477	$1,098	130.19%	110 to US $1
Firm 3	$279	$2,118	659.14%	212 to US $1
Firm 4	$330	$3,305	901.52%	331 to US $1
Firm 5	$381	$4,261	1018.37%	426 to US $1

Bankruptcy in this experiment is a possibility. The do-nothing strategy helps reduce the probability of this. There was insufficient ability of the programmer (the author) to explicitly exclude this possibility. The conductor is allowed to forward experimental dollars to the bankrupt participant. This allows the experiment to continue. However, the period after the money has been forwarded, the participant shows a negative balance, and cannot buy permits in the trading screen. They can sell permits, to gain cash, and obtain a positive balance. If the participant finishes all sessions with a negative balance, it is not deducted from their show-up fee. Also note that if a participant earns negative profit for more than 2 periods, they are not allowed to continue the experiment, but are allowed to continue the session.

3.4.2. Environment B

Environment B contains 10 units of emissions in periods 1-9, 8 permits allocated to each participant in periods 1-3, and 4 permits allocated in periods 4-9, and differing revenue for each participant which is constant over all 9 periods. Banking treatments that were experimentally tested in this environment are the same as in Environment A: Unlimited; One Year Banking; Limited Banking; BMR = 0. Note that the constraint is in the allocated permits, as permits are halved in the latter six periods. Cost minimizing firms should, in aggregate, save permits in the early three periods, for use in the latter six.

This is the basic setup of the Cronshaw, Brown Kruse study (Cronshaw Brown Kruse 1999b). The main difference between this environment and theirs is that 6 permits were allocated to each participant in the first three periods, and 3 permits allocated per participant in the last six. The simulations in this study show that participants had greater latitude in decision making with the permit allocation employed here. With the

Cronshaw setup, participants were "cornered" into high banking in early periods for latter periods, by threat of bankruptcy. This setup is emulating a scenario where a regulating party institutes a cap and trade market, then ratchets the cap tighter. This was the case for the SO2 market, NOx market, and possibly the ERMS market.

Because production (emissions) is constant in all periods in this case, revenue is also constant for all periods. Note that the revenues in this Environment are the same revenues used in the first 6 periods of Environment A. This is to add consistency to the experiment to better discover if participants react differently to Environment A or B. This revenue structure again allows all participants to choose the do-nothing strategy.

In all periods firm 1 makes a revenue of 200 lab dollars each period, the cost of not using any permits (or banking all permits). Firm 2 makes 200 also, and is able to bank 5 permits per period in periods 1-3, disregarding any transactions with other firms or prior earnings. Firms 3-5 are able to each bank 4 permits in the first 3 periods, making 300, 375, and 450 respectively. Table VIII lists the firm's revenues. Table IX lists do-nothing Profit, Efficient Market Profit given aggregate optimal banking and trading, Potential Increase in Profit given aggregate optimal banking and trading, and Conversion Ratios of experimental dollars to $US for Environment B with all tested banking treatments.

TABLE VIII
ENVIRONMENT B FIRM REVENUE

Environment B	
Revenue	Each period
Firm 1	200
Firm 2	200
Firm 3	300
Firm 4	375
Firm 5	450

TABLE IX
DO-NOTHING PROFIT, EFFICIENT MARKET PROFIT, POTENTIAL INCREASE IN PROFIT, AND CONVERSION RATIOS FOR ENVIRONMENT B WITH ALL TESTED BANKING TREATMENTS.

Unlimited				
	Do-nothing	Efficient Market	% Increase in Profit	Conversion into US $
Firm 1	$1,344	$3,000	123.21%	300 to US $1
Firm 2	$888	$1,704	91.89%	170 to US $1
Firm 3	$876	$2,202	151.37%	220 to US $1
Firm 4	$1,095	$2,940	168.49%	294 to US $1
Firm 5	$1,314	$3,768	186.76%	377 to US $1

1 Year Bank				
	Do-nothing	Efficient Market	% Increase in Profit	Conversion into US $
Firm 1	$1,344	$3,144	133.93%	314 to US $1
Firm 2	$888	$1,734	95.27%	173 to US $1
Firm 3	$876	$2,100	139.73%	210 to US $1
Firm 4	$1,095	$2,868	161.92%	287 to US $1
Firm 5	$1,314	$3,672	179.45%	367 to US $1

Limited Use				
	Do-nothing	Efficient Market	% Increase in Profit	Conversion into US $
Firm 1	$1,344	$2,220	65.18%	222 to US $1
Firm 2	$888	$1,626	83.11%	163 to US $1
Firm 3	$876	$2,250	156.85%	225 to US $1
Firm 4	$1,095	$2,862	161.37%	286 to US $1
Firm 5	$1,314	$3,474	164.38%	347 to US $1

MBR - 0				
	Do-nothing	Efficient Market	% Increase in Profit	Conversion into US $
Firm 1	$1,344	$3,591	167.19%	359 to US $1
Firm 2	$888	$1,860	109.46%	186 to US $1
Firm 3	$876	$1,944	121.92%	194 to US $1
Firm 4	$1,095	$2,652	142.19%	265 to US $1
Firm 5	$1,314	$3,423	160.50%	342 to US $1

A note about why 9 periods are used. In the Cronshaw (1999a) study and the Banking Only experiment conducted in this study, 12 periods, which correlates to years or months, were used. The reason for using 9 periods in the Trading With Banking experiments rather than 12 is simple experimenter money budget constraints. With fewer periods, more experiments can be run with the same participants with a lower cost per data point in one session. Payment is based on total time spent in the lab (about $10 per hour). If each computer screen takes 90 or 120 seconds, and there are 3 screens per period, that is about 4.5 to 5 minutes per period. Running 9 periods instead of 12 saves about 13.5 minutes per experiment, and makes one session about 40.5 minutes. This allows for plenty of time for instructions and 3 trial periods in a two hour time frame. Note that two or more computer screens, depending on the treatment, end when all participants are finished making decisions. With this in mind there may be more than 2 experiments run with one cohort. This will cost more in participant fees but will add more data with a single cohort.

3.5. Protocol – Banking Only Experiments

Participants were undergraduate students recruited from upper-level economics courses at the University of Illinois at Chicago. Each participant completed a truncated training session (2 periods), and each of the five treatments. All training and treatments were completed in a single session.

The training consisted of completing a sample of the first two periods of the Unlimited treatment. The training informed the participant of how to fill out the decision sheets, and how their decisions effected their final payment. Participants in these experiments were able to bank as many permits as they wished, for as long as they wished, but were subject to the constraints of the differing treatments and could not have traded permits with one another.

The protocol for all experiments are listed below.

- Each participant was seated and given a set of written instructions which contain a sample balance sheet and a production cost schedule.
- Following the written instructions there were 10-15 minutes of verbal instructions. Verbal instructions cover an explanation of the permit allocation, production cost schedule, and banking.
- An example of how to bank was explained. Participants were asked to complete the example sheet in an MS Excel file.
- Questions about recording decisions were solicited and answered, but no questions were answered about optimal decisions.
- The following sequence of events comprised the rest of the experiment:
  - Participants made permit usage decisions for all periods, recording their decisions to bank and permit usage in each of the 12 periods in an MS Excel file.
  - Participants acknowledged completion.
  - The conductor verified the decisions, saved the data, and recorded experimental earnings.
  - If another treatment was being run, the conductor then reviewed the instructions, explained the new instructions and opened a new decision spreadsheets.
- After all treatments were run, total experimental earnings were tallied.

- Experimental earnings were converted into US currency at a rate of 500 experimental dollars to $1.00 US.

3.6. *Protocol – Banking With Trading Experiments*

At least two experiments were run with each cohort. Time permitting, three experiments were run. Experiment one was either Environment A or B. The second experiment was the remaining environment, B or A. Each cohort was comprised of 5 or 10 participants, each firm type representing 1 or 2 participants. The third environment was chosen by participant majority vote. Participants were invited to participate in another experiment (testing a different treatment) at a later date. Participants were undergraduate students from the University of Illinois at Chicago, recruited from all levels of economics courses[17]. Students participated in up to four different sessions. There were 4 banking treatments (Unlimited, 1 Year Banking, Limited Use, BMR = 0). All participants and the alternate (when available) were paid the show up fee of $10 after logging into the computer. If all participants were present at the beginning of the experiment, the alternate was allowed to leave.

Participants were paid at the end of the experiment, based on how many experimental dollars they earned. There was a conversion rate of experimental dollars to US dollars. This rate was set before the experiment starts. The rate was set so that if participants were cost minimizing, and equilibrium permit prices were obtained, all participants would make $20 for 2 experiments. The previous assumptions set a maximum that could be earned by all participants in aggregate[18]. The expectation was

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[17] An economics major was not required

[18] It is not immediately obvious what that maximum is. For instance if in Environment B Unlimited treatment, Firm 2 with a conversion rate of 170, sells numerous permits to firm 5 at a price well above

that participants did not make perfect cost minimizing decisions, thus reducing their payment. The expected payment for the experiment for each participant was $15, for a total of $25 including the show up fee. This in no way guaranteed any payment for any participant. A total payment of $20 was chosen to be competitive with a students alternative use of time. It was expected that most students do not make more than $10 an hour if employed.

The experiment was conducted using the zTree software created by Urs Fischbacher at the Institute for Empirical Research in Economics, University of Zurich, Switzerland. This software is an interface allowing several different subjects on different computers to interact in real time with the conductor and/or other subjects. In these experiments participants were able to interact with each other via a trading screen, and also input private information on a subsequent screen. Actions facilitated through the zTree interface, like offers to buy and sell, were recorded and compiled into a summary file. The actions available to the participant are listed in the remaining protocol as follows:

- Participants were seated at computers in one of the UIC public computer labs.
- They were given a consent form which was completed by the participant in accordance with the OPRS office[19].
- When all show up fees were paid, and consent forms collected participants were given written instructions.

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predicted optimum, firm 2 with a conversion rate of 170 may make more than the predicted 1,704 experimental dollars, while firm 5 may make much less than the predicted 3,768 experimental dollars. In essence, this transfers experimental dollars from firm 5 at a conversion rate of 377 to firm 2 at a conversion of 170.

[19] Protocol number 2003-0828

- The written instructions were read to the participants as they followed along.
- After introductory comments were made about the study, the trial periods were introduced to the participants.
- Participants were informed of the following:
    - The number of periods.
    - The number of permits allocated to each participant for all periods.
    - The production level of each participant for all periods.
    - Their own cost structure but not others.
    - Their own revenue but not others.
    - Their own conversion rate from experimental dollars to US dollars but not others.
- The remainder of the instructions were read as the participants completed the first trial period.
- Questions were answered, and the remaining two trial periods were run, for a total of 3 trail periods.
- No payment was given for the three trial periods.

One experimental period, or trial period, consisted of (see the Appendix for a screen shot):

- The trading screen listing…
    - Permits held, updated in real time. Each participant could see their own permits held but not others.

- Revenue (cash), updated in real time. Each participant could see their own revenue held but not others.
- Prices of offers to buy one permit, updated in real time, viewed as public information (listed on all participants screens).
- Prices of offers to sell one permit, updated in real time, viewed as public information.
- Prices of completed contracts for one permit, updated in real time, viewed as public information.

- The trading screen allowed participants to…
  - Offer to buy one permit at a time at a specific price (but may have bought as many as they wished, constrained by their cash on hand). The offer was listed on all participant's screens, where any participant could have accepted the offer.
  - Offer to sell one permit at a time at a specific price (but may have sold as many as they wished, constrained by their permits on hand). The offer was listed on all participant's screens, where any participant could have accepted the offer.
  - Complete a transaction at a price offered by another participant, for both buys and sells. When an offer was accepted, and the zTree interface had transferred the permit and money (in real time), the accepted offer was listed for all to see, in real time. The completed trade did not list the buyer or seller.

- At the completion of the trading screen, a banking decision screen was displayed showing:
  - Total revenue (cash) after trading.
  - Total permits held after trading.
  - Emissions in the current period.
- The participant was then asked to make a permit usage decision.
  - The participant made a permit usage decision by entering in how many permits they wished to use in the current period.
  - After the permit usage decision had been made, the zTree program calculated the number of units of emissions that would have been abated, the respective costs, and the number of permits banked.
  - The participant was then forwarded to the summary screen.
- The summary screen listed:
  - The number of emissions abated.
  - The total cost of abatement.
  - The number of permits that would have been banked for next period.
  - The total revenue (cash) after abatement costs.
- The BMR = 0 treatment contained a pre-trading screen that was similar to the summary screen, but also listed:
  - The number of permits held in the bank.
  - The number of permits that can be withdrawn in the next period.

At the end of the 8th period participants were verbally reminded that the next period was the last. After all 9 periods had been completed, the participants complete a "questionnaire" mandated by the zTree program[20]. The questionnaire, as modified for this experiment, had only one field which offered the participant to enter a unique code, that would be used when payment for the experiment was made. An experiment was completed when the questionnaire was completed. After the questionnaire was over, a second set of instructions were handed out and a new experiment was started, with one trial period.

The second set of instructions explained that the number permits were changed, the number of potential units of emissions were changed, the truncated (Environment B) or expanded (Environment A) marginal and total costs, and the conversion ratio had been changed, or how Environment A differed from Environment B.

After all experiments were completed, participants were called by their firm number (type), and paid their experimental earnings in US dollars. The participant would then leave the experiment. When the questionnaire was completed by all participants, the zTree program saves all experimental information. The participant level information includes total earnings, per period earnings, trades, prices, permit usage, and costs. This data was exported from the zTree program into MS Excel files. The data was then compared to the predictions made from the simulations in the next chapter.

In summary the experiments in this study used market and firm attributes previously employed by published journal articles. Table X lists the specifications of the experiments by Cronshaw Brown Kruse (CBK) (1999), Franciosi et. at. (1999), and Mestelman et. al. (1999) and the experiments of the study at hand. The three studies conducted prior to this one acted as a template for the current research. In all three studies the basic firm abatement cost structures, number of participants per experiment,

[20] This is hard coded in the zTree program. This module could not be disentangled from the main program.

permit allocation, emissions, and number of periods were the same, as defined by the CBK experiments.

CBK experiments had 12 periods, these experiments had only 9. It may be questioned if 9 periods is enough time for participants to reach a stable equilibrium. However in the Mestelman experiments, allocations of permits were changed after 4 periods, and shares who's value changed with each period, were traded with valid results. The experiments in this thesis use two different environments, which is also done by Franciosi, while the CBK and Mestelman studies use only one environment. Environment B used in this study is very similar to setups used in all three prior studies, differing only in number of periods and permit allocation. However, permit allocation is still halved in the latter 2/3s of the experiment in all studies, including the one at hand. Environment A is a variation of the constraint in the CBK study where allocations remain the same, but emissions rise. In Environment A there is still a well defined constraint, which should induce participants to bank in early periods, for use in later periods. Additionally the Franciosi study also reports on studies which were different than the CBK study in permit allocation (permits were not distributed evenly among participants), and the Mestelman study also employs trading shares of permits and permits. Therefore all studies employ slightly different market rules. This emphasizes the robustness of the CBK setup.

Though shown in chapters 5 and 6, it will be said now that there was little evidence of learning bias over exposure to experiments. This bias comes as participants become more accustomed to the experiment and the computer interface. Later analysis of the observed values lay questions of learning bias to rest. There is little support for extra training of subjects. That is, it is believed that final results would differ little if participants had more access to training. Additionally, there were explicit instructions both read allowed and given in written form for the participants to study, as in the three prior studies reported here. There were three trial periods that participants could become familiar with the computer interface, trading, banking, permit use, and abatement costs, without effecting their final payment, which took about a half an hour to complete.

Even with copious amounts of training, there were still participants who made sub-optimal decisions in these experiments, as shown in chapters 5 and 6. However, CBK (1999a) also contended that there were participants in their study which made sub-optimal decisions. As stated in Chapter 2, in the CBK experiments there was the opportunity to purchase permits from the conductor for prices well above any marginal cost. Because participants did exercise this right to buy from the conductor shows that there were some participants that were making sub-optimal cost minimizing decisions. It is for this reason that the option of purchasing permits from the conductor was eliminated from this study.

The auction types were different between all the studies. The discriminative auction was specifically used by CBK to emulate an actual market. However, there have been a number of other published experiments which use the open outcry double auction[21]. Franciosi et. al. used both the discriminative auction for permits that were forced on the market, and open outcry double auction through a computer interface. Mestelman et. al. used a pit where participants conducted trades face to face. Participants in the Mestelman study were able to trade shares and permits at the same time.

[21] For a list of published works see Kagel and Roth 1999.

In the CBK study, pen and paper were used by participants to record their actions and the use of a computer terminal was used to facilitate the experiments. Franciosi et. al. (1999) also used a computer interface, while the Mestelman et. al (1999) used pen and paper to record transactions, but used a "pit" where participants conducted trades face to face. The computer interfaced employed for the thesis experiments allowed for participants to interact in real time, have real time updated permits and revenue, and calculate abatement cost and permit banking automatically. The use of the computer interface was an attempt to make it easier for the participant to make decisions, and reduce participant calculation errors.

The intent of the CBK and Franciosi papers were to study the newly implemented US SO2 market. The Mestelman study was geared towards a proposed NOx market. The aim of the thesis is to study emission markets in general, with specific attention to banking based rules.

It should be noted that CBK conducted 87 banking only experiments and 5 experiments with banking and trading. Franciosi conducted 8 experiments with and 4 without trading. Mestelman conducted 5 experiments with trading. This study conducted 284 banking only experiments, and 44 banking with trading experiments. The hypotheses for this study specifically state that statistical significance is to be obtained. It is for this reason that nearly 8 times more experiments than any other study are conducted.

Again it will be stated that the experiments conducted for this thesis are firmly grounded in not one but three published experiments studying similar markets. The modifications are slight, but create a more robust experiment by allowing participants greater levity in their decisions through more permits being allotted, and less chance of error by not allowing permits to be bought from the conductor and using a computer interface to calculate all non-decision variables for the participant.

TABLE X
ATTRIBUTES OF PREVIOUS ENVRIONMENTAL EXPERIMENTS AND THE BANKING WITH TRADE EXPERIMENTS

Author/ Attribute	Cronshaw Brown Kruse 1999a	Franciosi et. al. 1999	Mestelman et. al. 1999	Banking With Trade Experiments
Periods Per Experiment	12	12	12	9
Firm Types	5	5	5	5
Firm Costs Structures	CBK defined	CBK defined	CBK defined	CBK defined
Emissions	10 per firm per period	10 per firm per period	10 per firm per period	10 per firm per period and 19 per firm per period
Permit Allotment Per Firm	6 in periods 1-8, 3 in periods 9-12	6 in periods 1-6, 3 in periods 6-12	3 shares Shares worth: 2 coupons (permits) in periods 1-4, 1 coupon (permits) in periods 1-4	8 in periods 1-6, 4 in periods 7-9 and 8 in periods 1-9
Ability To Buy Permits From Conductor?	Yes	No	No	No
Auction	Discriminative Auction	Market = Double Auction Auction (forced) = Discriminative Auction	Open Outcry Double Auction	Open Outcry Double Auction
Experiment Interface	Computer Interface and Pen and Paper	RNA3 Computer Interface	Pen and Paper	zTree Computer Interface
Purpose	To test the rules of the (then) newly instituted SO2 market	To test the rules of the (then) newly instituted SO2 market	To test the rules of a proposed Canadian NOx market	To evaluate 4 different banking regulations
Experiments (With trade/ banking only)	5/86	8/4	5	44/284

4. PREDICTED OUTCOMES

In order to analyze the outcomes of the experimental data, a baseline, or yardstick of sorts, is needed. For this purpose least cost solutions for individual participants and the aggregate market over all periods have been generated. This chapter will review how the least cost solutions were discovered, what the least cost solutions are for each banking rule, and comment on how well these solutions function as predictions, and list their limitations for the real world.

4.1. Simulated Optimums For Banking Only Experiments

The cost associated with the Command And Control treatment will be considered the maximum cost, or minimum cost savings of these 12 period experiments. The experiment uses Environment A explained above, and firm one only. It is not necessary to add additional firms, because trading is not allowed, and therefore differing cost structures will not decrease aggregate costs.

Optimal banking decisions and minimum costs are listed below in table XI. Table XI lists the Treatment first. The next column shows the number of permits banked for each of the first 8 periods. As noted before, this experiment was conducted with 12 periods, for environment A and firm type 1 only. The number of permits withdrawn from the bank per period are listed next. The minimum cost is the final column, and is estimated for all 12 periods.

TABLE XI
OPTIMAL BANKING AND MINIMUM COSTS

Treatment	Number of permits banked each period for periods 1-8	Number of permits to reduced bank (use) for periods 9-12	Minimum Cost
CAC	0	0	$2064
Unlimited	3	6	$600
One Year	1 (with 2 permits banked in 2 periods)	2 (with 3 permits used in 2 periods)	$752
Limited Use	1	2	$792
Both	1	2	$792
Discounted	0, 0, 0, 0, 0, 2, 2, 4	3, 2, 0, 0	$950

4.2. Simulated Optimums For Banking With Trading

Least cost solutions have been generated for the aggregate market summed over all 9 periods. The approach for cost minimization is taken from basic supply and demand analysis. The premise is if surplus is maximized, the outcome is the least cost solution. Surplus is defined as being the difference in the marginal value of the permit to the individual firm and the permit price actually paid or received.

The aggregate demand schedule is calculated by the summing each firm's demand for permits, or emissions not covered by allotment. The marginal value given to these permits is derived from the marginal abatement cost equations above, who's values are listed in tables III and IV. For the former periods[22], the demand for permits per firm would be the 2 units of the baseline that are not covered by their 8 allotted permits for both environments A and B. In the later periods, in A it is the 11 units of emissions that

[22] From this point on, "former" refers to A's periods 1-6 and B's periods 1-3. "Latter" refers to A's periods 7-9 and B's periods 4-9.

the 8 allotted permits do not cover, and in B it is the 6 units of emissions that the 4 permits do not cover.

The supply schedule would be the marginal abatement costs associated with each permit held. For a firm to part with a valuable permit, the firm must receive a price equal to or greater than the marginal abatement cost they must incur by not using that permit. For all periods, the firm's supply schedule for permits is the marginal abatement cost associated with each of the 8 or 4 permits allotted to the firm plus any banked permits.

When the reservation prices for permits demanded of all firms are gathered and sorted, largest to smallest, they make the market demand schedule. When the abatement costs associated with each permit allocated to the firm are gathered and sorted for all firms, lowest cost to highest, they make the market supply schedule. Returning to our original goal of cost minimization, it is concluded that maximizing surplus minimizes cost. Therefore, armed with the supply and demand schedules, permits are simply paired by the highest reservation price for permits demanded, with the supplied permit associated with lowest abatement cost. This is continued until surplus is exhausted, or when the reservation price for demanded permits is less than the abatement cost of the next permit supplied[23]. Note that the reservation price for a banked permit is the opportunity cost of not using that permit when issued, or the marginal abatement cost associated with that permit.

Also note that there are 9 periods in these simulations, periods 1-6 are low emissions, and periods 7-9 high emissions in A, and 1-3 are high permit allocations while

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[23] This same task can be accomplished by, again, obtaining the supply and demand schedules, and offering a price to the market. If the market does not clear, or the number of buys do not equal the number of sells, a different price is offered to the market. Once the correct market price has been offered, the market will clear. I, however, do not employ this method.

periods 4-9 are low permit allocations. This is a two low emissions periods to one high emissions period ratio for A and a one high permit allotment period to two low allotment periods in B. This means that what will happen in all 9 periods can be predicted by simply looking at 3 representative periods. They are two low emissions periods, and one high emissions periods (A) or one high permit allotment period to two low permit allotment periods (B). These three periods can then be multiplied 3 times to give a 9 period prediction of least aggregate abatement cost for all periods. The cost minimization process will be the same for a 3 period case, as for a 9 period case, because the information in each period does not change, and is only duplicated three times. The only exception to this is the 1 year banking treatment in Environment B. In this case all 9 periods of Environment B are modeled, because in period 4 the usable aggregate bank can be up to 40 units, while in periods 5-9 it can only be 20. A 3 period case is used in all other treatments, thus saving time and computational resources.

Additionally, for the computer simulations the b34s matrix command NLPMIN2 is used, which calls the IMSL routine DN2CON to minimize costs. This program's function is to solve non-linear equations with non-linear constraints. This program is needed due to the non-linear nature of the banking constraints. This program is used by inputting the abatement cost minimization function, the permit allocation constraint, and any banking restrictions. The program then offers the least cost allocation of permits, specifically giving the aggregated abatement costs, and permit usage by firm and time period. Equations 1 through 17 are used to calculate least cost solutions.

Results similar to those listed below are obtained. Because the computer program uses continuous equations and variables, whereas the "by hand" method uses discrete variables, it does give different results. When results from the B34s software are rounded, the permit usage of each firm in each period is the same for the 14 simulations ran minus three instances[24]. Lower aggregate abatement costs are estimated by the b34s software, again due to cost minimization using continuous variables. The simulates predicted by the b34s software were used when evaluating the experiments.

The computer program optimizes the market by stating the cost structure for each firm, then minimizes aggregate costs subject to constraints. The initial constraint is that the sum of all permits used by all participants in all periods must equal all permits allocated. A second constraint states that permit usage by any firm cannot go below zero (they can't make negative emissions). The program then simply allocates permits to minimize cost.

The mathematics for the most general "Unlimited" treatment are:

$$\min_{xi} \sum_{i=1}^{5} \sum_{t=1}^{3} c_i\,(\phi_t^i - x_t^i) \qquad (1)$$

$$\text{s.t. } \sum_{i=1}^{5} \sum_{t=1}^{3} x_t^i = \text{Aggregate Allotment} \qquad (2)$$

$$x_t^i \geq 0 \text{ for time t = 1, 2, 3 and for firms i = 1, 2, 3, 4, 5} \qquad (3)$$

Where c_i is defined as the abatement cost structure for firm i, ϕ_t^i is the emissions of firm i in time t, and x_t^i is the number of permits used by firm i in time t. Note in environment A ϕ_t^i is 10 for each firm in the first two periods, and 19 in the last one, with aggregate allotment of permits summed over all periods being 120, while in B there are

[24] In these instances there was one permit that was split by the software nearly equally by three different firms in the last 3 periods, whereas it is predicted by hand that one permit being used by a specific participant.

10 units of emissions for all firms in each period, and 80 permits total summed for all participants in all periods.

The remaining constraints for the individual treatments are listed below

Command and Control

Environment A: $x_t^i = 8$ for time t = 1, 2, 3 (4)

Environment B: $x_t^i = 8$ for time t = 1 (5)

$x_t^i = 4$ for time t = 2, 3 (6)

Trade With No Bank

Environment A: $\sum_{i=1}^{5} x_t^i = 40$ for time t = 1, 2, 3 (7)

Environment B: $\sum_{i=1}^{5} x_t^i = 40$ for time t = 1 (8)

$\sum_{i=1}^{5} x_t^i = 20$ for time t = 2, 3 (9)

Bank With No Trade

Environment A: $\sum_{t=1}^{3} x_t^i = 24$ for firms i = 1, 2, 3, 4, 5 (10)

Environment B: $\sum_{t=1}^{3} x_t^i = 16$ for firms i = 1, 2, 3, 4, 5 (11)

1 Year Bank

Environments A and B: $\sum_{i=1}^{5} x_t^i \leq A_{t-1} + A_t$ (12)

Where A_t is the per period aggregate allotment. In environment B, the programming code must be extended to the whole 9 periods. This is because in period 4, there can be a bank associated with the high allotment period (40 permits in aggregate), while in periods 5-9 the bank is associated with low allotment periods (20 permits in aggregate).

Limited Use

Environment A: $x_t^i \leq 10$ for firms i = 1, 2, 3, 4, 5 and time t = 1, 2, 3 (13)

Environment B: $x_t^i \leq 6$ for firms i = 1, 2, 3, 4, 5 and time t = 1, 2, 3 (14)

BMR

The first constraint, or equality, in the Budget Model Rule states that the summation of all permit usage by all participants for all periods must equal the total permits allocated. However, if firms use more permits from the aggregate bank than the regulatory constant allows, there will be a one permit charge. 2:1 permit charges must be subtracted from the total number of permits allocated. To capture the effect of the 2:1 withdrawal ratio it is first stated that all banked permits used in the third period of environment A, and the second and third periods of B, will have a 2:1 charge. The regulatory constant is added to a second set of allocated permits in the third (or second and third) periods allocations. In this manner firms can use up to their allotted permits plus the regulatory constant essentially without a charge, and any usage larger than the last period allocation added to the regulatory constant would result in a reduction of one extra permit from the total permits allocated for all periods.

Note that by using this method, if the regulatory constant is not binding, there will be an over-allocation of permits. Binding values cause cost minimizing firms to use all permits allotted in the last period, plus at least the maximum number of permits allowed by the regulatory constant.

For a three period Environment A, the above method can be expressed mathematically as:

$$\min_{xi} \sum_{i=1}^{5} \sum_{t=1}^{3} c_i (\phi_t^i - x_t^i) \tag{15}$$

$$\text{s.t.} \sum_{i=1}^{5} \sum_{t=1}^{3} x_t^i = T - (\sum_{i=1}^{5} x_3^i - (A_3 + R)) \tag{16}$$

Which can be rewritten as:

$$\text{s.t.} \sum_{i=1}^{5} \sum_{t=1}^{3} x_t^i + \sum_{i=1}^{5} x_3^i = T + A_3 + R \tag{17}$$

Where T is the total aggregate allocated permits, A_3 is the aggregate permits allotted in period 3, and R is the regulatory constant.

Note that in the above methods, the least aggregate cost is obtained for several firms with differing cost structures and over a set period of time. These results are not completely static, in that it affords cost minimization over time, however it does require pre-determined time horizon. The merits and costs of each treatment will now be discussed.

4.2. Tables Of Simulations

Tables XII and XIV encapsulate the results of the simulations. Each of the numbers, unless otherwise indicated, reflects a per-period aggregate value. For instance, if the value of Bank is listed as 5, an aggregate of 5 permits were banked for each of the periods. Table XII is divided into 4 parts: results for Periods 1-6, Period 8 ending bank, Periods 7-9 results, and the final aggregate cost. Table XIII is similar to table XII, with Periods 1-3 listed first, Period 3 ending bank, Periods 4-9, and final aggregate cost.

TABLE XII
EMISSIONS AND COSTS - ENVIRONMENT A

Periods 1 - 6 (per period)						End of per 6	Periods 7 - 9 (per period)					Final
Treatment	Bank	B/S	Max	Use	Permit price	Total bank	Bank	B/S	Max	Use	Permit price	Total Cost
CAC	0	0	8	40	NA	0	0	0	8	40	NA	$13,932
Bank only	15	0	5	25	NA	90	-30	0	14	70	NA	$8,100
Trade only	0	3	9	40	18-24	0	0	13	14	40	124-132	$9,720
Unlimited	14	0	8	26	NA	84	-28	21	17	68	50-52	$5,400
1 year	6.5	5	9	33.5	30-36	39	-13.3	12	15	53.3	74-84	$6,585
Limited	5	2	9	35	28-30	30	-10	8	10	50	28-38	$10,044
Both	5	5	9	35	28-30	30	-10	0	10	50	NA	$10,044
BMR - 20	10	1	8	30	36-68	60	-20	23	16	60	66-68	$5,772
BMR - 10	10	1	8	30	36	60	-20	21	16	55	74-76	$6,834
BMR - 0	12	1	8	28	40	72	-24	21	15	52	80-88	$8,034

TABLE XIII
EMISSIONS AND COSTS - ENVIRONMENT B

Periods 1 - 3 (per period)						End of per 3	Periods 4 - 9 (per period)					Final
Treatment	Bank	B/S	Max	Use	Permit price	Total bank	Bank	B/S	Max	Use	Permit price	Total Cost
CAC	0	0	8	40	NA	0	0	0	4	20	NA	$8,208
Bank Only	10	0	6	30	NA	30	-5	0	5	25	NA	$7,128
Trade Only	0	3	9	40	23	0	0	7	7	20	70	$5,808
Unlimited	14	0	8	26	NA	42	-7	10	8	27	50	$4,686
1 year	10	0	8	30	NA	30	-5	10	8	25	54	$4,870
Limited	14	0	6	26	NA	42	-7	4	6	27	33	$5,868
Both	10	0	6	30	NA	30	-5	4	6	25	35	$5,964
BMR 0	8	1	9	32	35	24	-4	10	7	22	66	$5,622

Under the first set of periods, the Treatment Type is listed first. BMR = 10 and BMR = 20 are excluded from Environment B because they are not binding and would equal the unlimited banking treatment. The Treatment Type is simply the type of regulation that is placed on the market. Bank, or the aggregate bank per period is listed next. The buys and sells of permits are denoted as B/S, this column lists the minimum number of transactions in the least cost system. There might be more transactions than the number listed, especially if one firm decides to hold more permits in early periods for use in later periods than what the simulation method predicts. The B/S column is only useful when looking at the lower bound of trading.

The value Max is the maximum number of permits used by any one firm in any one period in the first or second set of periods of each environment. This value can be viewed as the potential for a spatial hot spot, if one exists. Use is the aggregate maximum number of permits used or redeemed in any one period for the first (second) set of periods. This will indicate if any intertemporal hot spots, or spikes actually occurred in the simulation.

The Permit Price, or the price of permits in the market, may contain a value of N.A. It may also contain different values in the first set of periods when compared to second set of periods. A value of N.A. either means that trading was not allowed or the regulations were such that trading was not optimal in those periods. Differing prices in early and latter periods illustrates how restricting permits in early periods from being used in latter periods creates a discontinuous aggregate cost structure. Because firms are not allowed to fully cost minimize over time, the higher cost periods have a shortage of cheap permits compared with earlier periods. This causes the price for permits and aggregate costs to rise. Permit price changes do not change the aggregate cost. This is because each unit bought is also sold. Money that is paid to obtain a permit is also received as payment for parting with a permit. A range of values for price are given due to the discrete nature of the cost structures. These numbers are the reservation values for both the buyer and seller of the last transaction completed. The price used in the simulations was the average of the two numbers, because the predicted price could be any price between the high and low price.

The Total Bank at the End of Period 6 (3) (End prd 6 (3)), is the aggregate bank of the former periods. This gives the reader an indication as to the level of restrictions, and should be indicative of the spread in permit prices between earlier periods and later periods. The greater the ability to bank permits from low cost periods to high cost periods, the lower the spread in permit prices.

Finally, the Total Cost for each treatment is given. This is the lowest aggregate cost for 5 participants for all 9 periods given the market restrictions[25]. Again, the permit price in the market does not affect this number. Later in this section we will rank the costs (which will also act as a rough ranking of the Max variable), and compare the cost savings and emissions for each of the treatments. We will now turn our attention to some of the finer details of the simulations

Tables XIV and XV encapsulate the results of the simulations and are sorted in order of descending aggregate Total Cost. Max and Use reflect a per period value as discussed previously. The tables are ranked by Total Costs in descending order. This is a rough ranking of the Max variable in the latter periods, and is expected in that the higher

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[25] Transactions, aggregate ending bank of the former periods, and total cost are doubled for analyzing experiments with 10 participants.

abatement costs should be correlated with lower emissions, and vice versa, because reducing emissions is costly in these simulations. Though Use does not strictly follow this trend. Tables XIV and XV will be used to create the ratios below, and are used as a quick reference for the reader. The attention will now turn to tables XVI, XVII, XVIII, XVIII, XIX, and XX.

TABLE XIV
ENVIRONMENT A VALUES, SORTED

Treatment	Max	Use	Max	Use	Total Cost
CAC	8	40	8	40	13932
Limited	9	35	10	50	10044
Both	9	35	10	50	10044
Trade only	9	40	14	40	9720
Bank only	5	25	14	70	8100
BMR - 0	8	28	15	52	8034
BMR - 10	8	30	16	55	6834
1 year	9.0	33.5	15.0	53.3	6585
BMR - 20	8	30	16	60	5772
Unlimited	8	26	17	68	5400

TABLE XV
ENVIRONMENT B VALUES, SORTED

Treatment	Max	Use	Max	Use	Total Cost
CAC	8	40	4	20	8208
Bank Only	6	30	5	25	7128
Both	6	30	6	25	5964
Limited	6	26	6	27	5868
Trade Only	9	40	7	20	5808
BMR 0	9	32	7	22	5622
1 Year	8	30	8	25	4870
Unlimited	8	26	8	27	4686

Table XVI provides percentage cost savings of choosing one banking treatment over another for Environment A, and table XVII for Environment B. This percentage of cost savings is calculated as the aggregate cost of the vertical treatment (V) subtracted from the aggregate cost of the horizontal treatment (H), which is then divided by the aggregate cost of the horizontal treatment (H). Or, (H - V) / H, (18)

where "horizontal" indicates the cost or emissions of the treatment listed in the horizontal heading, and "vertical" the cost or emissions of the treatment listed in the vertical column.

Tables XVIII and XIX are the emissions increase of the maximum emissions (Max) of any one firm in any one period in choosing one treatment over another for Environments A and B respectively. The latter set of periods for each Environment are used. The treatments are again sorted from the highest cost treatment to lowest cost. The percentage is calculated in the same manner as the cost ratios. Tables XX and XXI are calculated in the same manner, and are the ratio of Use emissions increase for the aggregate maximum emissions of all firms in any one period in choosing one treatment over another, again using the latter set of periods.

TABLE XVI
ENVIRONMENT A, COST RATIOS

Treatment	CAC	Limited	Both	Trade only	Bank only	BMR - 0	BMR - 10	1 Year	BMR - 20	Unlimited
CAC	0.0000	.	.	.	.	.	.	.	.	.
Limited	0.2791	0.0000	.	.	.	.	.	.	.	.
Both	0.2791	0.0000	0.0000	.	.	.	.	.	.	.
Trade only	0.3023	0.0323	0.0323	0.0000	.	.	.	.	.	.
Bank only	0.4186	0.1935	0.1935	0.1667	0.0000	.	.	.	.	.
BMR - 0	0.4233	0.2001	0.2001	0.1735	0.0081	0.0000	.	.	.	.
BMR - 10	0.5095	0.3196	0.3196	0.2969	0.1563	0.1494	0.0000	.	.	.
1 Year	0.5273	0.3444	0.3444	0.3225	0.1870	0.1804	0.0364	0.0000	.	.
BMR - 20	0.5857	0.4253	0.4253	0.4062	0.2874	0.2816	0.1554	0.1235	0.0000	.
Unlimited	0.6124	0.4624	0.4624	0.4444	0.3333	0.3279	0.2098	0.1800	0.0644	0.0000

TABLE XVII
ENVIRONMENT B, COST RATIOS

Treatment	CAC	Bank Only	Both	Limited	Trade Only	BMR 0	1 Year	Unlimited
CAC	0.0000	.	.	.	.	.	.	.
Bank Only	0.1316	0.0000	.	.	.	.	.	.
Both	0.2734	0.1633	0.0000	.	.	.	.	.
Limited	0.2851	0.1768	0.0161	0.0000	.	.	.	.
Trade Only	0.2924	0.1852	0.0262	0.0102	0.0000	.	.	.
BMR 0	0.3151	0.2113	0.0573	0.0419	0.0320	0.0000	.	.
1 Year	0.4067	0.3168	0.1834	0.1701	0.1615	0.1338	0.0000	.
Unlimited	0.4291	0.3426	0.2143	0.2014	0.1932	0.1665	0.0378	0.0000

TABLE XVIII
ENVIRONMENT A, MAX RATIOS

Treatment	CAC	Limited	Both	Trade only	Bank only	BMR - 0	BMR - 10	1 year	BMR - 20	Unlimited
CAC	0.0000	.	.	.	.	.	.	.	.	.
Limited	-0.2500	0.0000	.	.	.	.	.	.	.	.
Both	-0.2500	0.0000	0.0000	.	.	.	.	.	.	.
Trade only	-0.7500	-0.4000	-0.4000	0.0000	.	.	.	.	.	.
Bank only	-0.7500	-0.4000	-0.4000	0.0000	0.0000	.	.	.	.	.
BMR - 0	-0.8750	-0.5000	-0.5000	-0.0714	-0.0714	0.0000	.	.	.	.
BMR - 10	-1.0000	-0.6000	-0.6000	-0.1429	-0.1429	-0.0667	0.0000	.	.	.
1 year	-0.8750	-0.5000	-0.5000	-0.0714	-0.0714	0.0000	0.0625	0.0000	.	.
BMR - 20	-1.0000	-0.6000	-0.6000	-0.1429	-0.1429	-0.0667	0.0000	-0.0667	0.0000	.
Unlimited	-1.1250	-0.7000	-0.7000	-0.2143	-0.2143	-0.1333	-0.0625	-0.1333	-0.0625	0.0000

TABLE XIX
ENVIRONMENT B, MAX RATIOS

Treatment	CAC	Bank Only	Both	Limited	Trade Only	BMR 0	1 Year	Unlimited
CAC	0.0000	.	.	.	.	.	.	.
Bank Only	-0.2500	0.0000	.	.	.	.	.	.
Both	-0.5000	-0.2000	0.0000	.	.	.	.	.
Limited	-0.5000	-0.2000	0.0000	0.0000	.	.	.	.
Trade Only	-0.7500	-0.4000	-0.1667	-0.1667	0.0000	.	.	.
BMR 0	-0.7500	-0.4000	-0.1667	-0.1667	0.0000	0.0000	.	.
1 Year	-1.0000	-0.6000	-0.3333	-0.3333	-0.1429	-0.1429	0.0000	.
Unlimited	-1.0000	-0.6000	-0.3333	-0.3333	-0.1429	-0.1429	0.0000	0.0000

TABLE XX
ENVIRONMENT A, USE RATIOS

Treatment	CAC	Limited	Both	Trade only	Bank only	BMR - 0	BMR - 10	1 year	BMR - 20	Unlimited
CAC	0.0000	.	.	.	.	.	.	.	.	.
Limited	-0.2500	0.0000	.	.	.	.	.	.	.	.
Both	-0.2500	0.0000	0.0000	.	.	.	.	.	.	.
Trade only	0.0000	0.2000	0.2000	0.0000	.	.	.	.	.	.
Bank only	-0.7500	-0.4000	-0.4000	-0.7500	0.0000	.	.	.	.	.
BMR - 0	-0.3000	-0.0400	-0.0400	-0.3000	0.2571	0.0000	.	.	.	.
BMR - 10	-0.3750	-0.1000	-0.1000	-0.3750	0.2143	-0.0577	0.0000	.	.	.
1 year	-0.3325	-0.0660	-0.0660	-0.3325	0.2386	-0.0250	0.0309	0.0000	.	.
BMR - 20	-0.5000	-0.2000	-0.2000	-0.5000	0.1429	-0.1538	-0.0909	-0.1257	0.0000	.
Unlimited	-0.7000	-0.3600	-0.3600	-0.7000	0.0286	-0.3077	-0.2364	-0.2758	-0.1333	0.0000

TABLE XXI
ENVIRONMENT B, USE RATIOS

Treatment	CAC	Bank Only	Both	Limited	Trade Only	BMR 0	1 Year	Unlimited
CAC	0.0000	.	.	.	.	.	.	.
Bank Only	-0.2500	0.0000	.	.	.	.	.	.
Both	-0.2500	0.0000	0.0000	.	.	.	.	.
Limited	-0.3500	-0.0800	-0.0800	0.0000	.	.	.	.
Trade Only	0.0000	0.2000	0.2000	0.2593	0.0000	.	.	.
BMR 0	-0.1000	0.1200	0.1200	0.1852	-0.1000	0.0000	.	.
1 Year	-0.2500	0.0000	0.0000	0.0741	-0.2500	-0.1364	0.0000	.
Unlimited	-0.3500	-0.0800	-0.0800	0.0000	-0.3500	-0.2273	-0.0800	0.0000

4.2.1. Ratio of Ratios

Finally, tables XXII through XXV are "ratios of ratios". These ratios of ratios are the percentage cost savings of one banking treatment over another, divided by the percentage emissions increase (decrease) of the same banking treatment over the other. The ratios are calculated for both Max and Use. These are reference tables that show the cost effectiveness of different emissions reduction banking schemes at their optimal cost minimizing level. The higher the ratio in absolute value, the greater percentage cost savings compared to lower relative percentage emissions increase. The lower the ratio the lower percentage cost savings compared to higher relative percentage emissions increase.

The ratios are figured as:

((Horizontal Cost – Vertical Cost) / Horizontal Cost) /

((Horizontal Emissions– Vertical Emissions) /

Horizontal Emissions)

or ((H – V) / H) / ((H – V) / H) (19)

Bold numbers indicate both costs and emissions have been reduced. Positive italicized numbers indicate that costs have been reduced without increasing emissions. When comparing banking regulations, an italicized or bold number would be a strong argument for using the horizontal banking provision over the vertical. A large positive number would indicate a relatively high cost savings compared to a relatively low savings in emissions. A small positive number would indicate a relatively small cost savings compared to a relatively large emissions reduction. The gain in welfare would (as always) be dependent on the damage function of higher emissions. The highlighted cells will be explained in the next chapter, and are the comparison of the treatments used in the experiments.

TABLE XXII
ENVIRONMENT A, RATIO OF RATIOS - MAX

Treatment	CAC	Limited	Both	Trade only	Bank only	BMR - 0	BMR - 10	1 year	BMR - 20	Unlimited
CAC	0.0000	.	.	.	.	.	.	.	.	.
Limited	-1.1163	0.0000	.	.	.	.	.	.	.	.
Both	-1.1163	**0.0000**	0.0000	.	.	.	.	.	.	.
Trade only	-0.4031	-0.0806	-0.0806	0.0000	.	.	.	.	.	.
Bank only	-0.5581	-0.4839	-0.4839	*0.1667*	0.0000	.	.	.	.	.
BMR - 0	-0.4838	-0.4002	-0.4002	-2.4284	-0.1141	0.0000	.	.	.	.
BMR - 10	-0.5095	-0.5327	-0.5327	-2.0784	-1.0941	-2.2405	0.0000	.	.	.
1 year	-0.6027	-0.6888	-0.6888	-4.5154	-2.6185	*0.1804*	**0.5830**	0.0000	.	.
BMR - 20	-0.5857	-0.7089	-0.7089	-2.8432	-2.0119	-4.2233	*0.1554*	-1.8519	0.0000	.
Unlimited	-0.5444	-0.6605	-0.6605	-2.0741	-1.5556	-2.4589	-3.3573	-1.3497	-1.0312	0.0000

TABLE XXIII
ENVIRONMENT B, RATIO OF RATIOS - MAX

Treatment	CAC	Bank Only	Both	Limited	Trade Only	BMR 0	1 Year	Unlimited
CAC	0.0000	.	.	.	.	.	.	.
Bank Only	-0.5263	0.0000	.	.	.	.	.	.
Both	-0.5468	-0.8165	0.0000	.	.	.	.	.
Limited	-0.5702	-0.8838	**0.0161**	0.0000	.	.	.	.
Trade Only	-0.3899	-0.4630	-0.1569	-0.0613	0.0000	.	.	.
BMR 0	-0.4201	-0.5282	-0.3441	-0.2515	**0.0320**	0.0000	.	.
1 Year	-0.4067	-0.5280	-0.5503	-0.5102	-1.1305	-0.9363	0.0000	.
Unlimited	-0.4291	-0.5710	-0.6429	-0.6043	-1.3523	-1.1654	**0.0378**	0.0000

TABLE XXIV
ENVIRONMENT A, RATIO OF RATIOS - USE

Treatment	CAC	Limited	Both	Trade only	Bank only	BMR - 0	BMR - 10	1 year	BMR - 20	Unlimited
CAC	0.0000	.	.	.	.	.	.	.	.	.
Limited	-1.1163	0.0000	.	.	.	.	.	.	.	.
Both	-1.1163	*0.0000*	0.0000	.	.	.	.	.	.	.
Trade only	*0.3023*	**0.1613**	**0.1613**	0.0000	.	.	.	.	.	.
Bank only	-0.5581	-0.4839	-0.4839	-0.2222	0.0000	.	.	.	.	.
BMR - 0	-1.4111	-5.0030	-5.0030	-0.5782	**0.0317**	0.0000	.	.	.	.
BMR - 10	-1.3586	-3.1959	-3.1959	-0.7918	**0.7294**	-2.5890	0.0000	.	.	.
1 year	-1.5860	-5.2180	-5.2180	-0.9700	**0.7840**	-7.2143	**1.1788**	0.0000	.	.
BMR - 20	-1.1714	-2.1266	-2.1266	-0.8123	**2.0119**	-1.8301	-1.7094	-0.9822	0.0000	.
Unlimited	-0.8749	-1.2843	-1.2843	-0.6349	**11.6667**	-1.0655	-0.8878	-0.6525	-0.4834	0.0000

TABLE XXV
ENVIRONMENT B, RATIO OF RATIOS - USE

Treatment	CAC	Bank Only	Both	Limited	Trade Only	BMR 0	1 Year	Unlimited
CAC	0.0000	.	.	.	.	.	.	.
Bank Only	-0.5263	0.0000	.	.	.	.	.	.
Both	-1.0936	**0.1633**	0.0000	.	.	.	.	.
Limited	-0.8145	-2.2096	-0.2012	0.0000	.	.	.	.
Trade Only	**0.2924**	*0.9259*	*0.1308*	*0.0394*	0.0000	.	.	.
BMR 0	-3.1506	*1.7607*	*0.4779*	*0.2264*	-0.3202	0.0000	.	.
1 Year	-1.6267	**0.3168**	**0.1834**	*2.2960*	-0.6460	-0.9809	0.0000	.
Unlimited	-1.2260	-4.2824	-2.6786	**0.2014**	-0.5519	-0.7326	-0.4723	0.0000

4.3. Conclusions

These simulations act as a reference for how one banking provision compares to another. They are a strong assessment of the different banking treatments, in that they compare the to important aspects of these treatments, the cost savings of a trading scheme, and the emissions associated with the cost savings. Again, this highlights the tradeoff of abatement cost savings through the market mechanism, and the "cost" of increased emission spikes. The bold and negative values in the ratio of ratios, given the particular situation, indicates which regulations are superior.

By reviewing the Ratio of Ratios tables, one can build a loose ranking of different treatments by finding the highest ratio in each column (relative high cost savings vs. relative low emissions increase), or the greatest number of italicized and bold numbers. High (absolute value) ratios or a number of bold or italicized values in one column indicates a treatment which may have better alternatives. When looking at table XXII (A, Max), The Trade Only and Bank Only columns hold high absolute values and at least one italicized number, with relatively low absolute values in their rows. This would flag these regulations as being higher cost with little gain in spatial emission spike reduction. In table XXIV (A, Use), the Bank Only regulation again does not fare very well. However the Trade Only does somewhat better in this Environment, maintaining or reducing emissions and reducing costs over the CAC, Limited, and Both regulations.

Table XXIII (B, Max), the Trade Only regulation does not have high ratios in its vertical listing, and is strictly not preferred to the BMR 0 treatment. In table XXV (B, Use), the Trade Only, BMR 0, and One Year regulations are strictly better than the CAC, Bank Only, Both, and Limited regulations, but have tradeoffs, negative numbers, when compared with one another.

No strict judgment can be made about "the best" banking treatment. From these simulations though, the Banking Only and Trading Only treatments had higher relative costs, with lower relative emission spike savings, indicating a regulator should carefully consider these treatments before implementing them. Final judgment of any particular banking treatment will depend on the damage that the effluent in question inflicts on the harmed party. The simulations in this study are ready to be paired with damage functions to determine the "best" rule to regulate banking. Let us not forget that the main reason to institute any environmental regulation is to control external costs.

After comparing the different banking regulations the theoretical optimum can now be compared to the observed experimental results. The larger question of what the tradeoffs of the banking treatments are should be revisited. Specifically, what are the simulated tradeoffs of the banking treatments that will be employed in the experimental part of the study? The Unlimited, One Year, Limited, and BMR 0 (BMR only from here on out) will be applied to the two different environments. As a general overview, there are tradeoffs between these four treatments within the different environments, who's boxes are marked in the tables above in heavy outline.

The only strict judgments that can be made, according to the simulations, are that first, when looking at the MAX ratios, Environment A shows the One Year treatment being preferred to the BMR treatment. Environment B indicates that the Unlimited treatment is preferred to the One Year treatment. When looking at the USE ratios, there are no strict judgments for Environment A, but the Unlimited, One Year, and BMR treatments are strictly preferred to the Limited treatment. These strict judgments are simply due to the fact that the preferred treatment has lower abatement costs and lower emission spikes than the lesser-preferred treatment. The basic statistics from the experiments will now be reviewed.

The inequalities below summarize the predicted outcomes of the experimental regressions of Banking With Trade:

Cost; UNL<ONEYR<BMR<LIMIT

Max – A; UNL>ONEYR>BMR>LIMIT

Max – B; UNL=ONEYR>BMR>LIMIT

Use – A; UNL>ONEYR>BMR>LIMIT

Use – B; UNL=LIMIT>ONEYR>BMR

5. RESULTS

5.1. Banking Only Experiments

The results concerning cost savings were as theory would predict. In all treatments, on average, participants were able to extract over 90% of the potential gains from banking. This is somewhat higher than the Cronshaw Brown Kruse (1999a) study, but not wide of the mark. The results of the study at hand can be seen in table XXVI. Due to the increasing marginal abatement costs, the largest gains in savings were realized by the first intertemporal permit transfers. In light of this, it is not surprising that such high gains were realized. The cost rankings, as predicted, from lowest cost to highest are: Unlimited; One Year; Limited; which equals Both; and Discount.

TABLE XXVI
AVERAGE TOTAL COST SAVINGS

Unlimited	91.55%
One Year	94.04%
Limited	95.23%
Both	95.84%
Discount	90.37%

There were 5 sessions conducted with 57 participants in total. One participant did not complete the "Both" treatment.[26] There were three cohorts with different treatment orderings. Cohorts were used to identify treatment ordering bias. All five treatments

[26] The experiment was running late, and the participant needed to leave.

were conducted during each session. Cohorts are listed in table XXVII below. Group A consisted of 16 participants, with 14 in group B, and 27 in group C.[27]

TABLE XXVII
COHORT GROUPS

Order	Group A	Group B	Group C
1	Unlimited	Both	One Year
2	One Year	Limited Use	Discounted
3	Limited Use	One Year	Limited
4	Both	Unlimited	Both
5	Discounted	Discounted	Unlimited

5.1.1. Emissions Spikes

One of the more interesting results from these experiments is that the most effective tool in limiting emissions spikes is the Limited treatment. Table XXVIII lists each of the treatments, the highest (Max) and the lowest (Min) permit use by any one participant for all 12 periods. Additionally the highest average of all participants in any one period for periods 9-12 (Ave high) and lowest average for any one period for periods 9-12 (Ave low) are listed. The significance of the highest permit usage for any one participant is that it can be viewed as a spatial "hot spot" caused by intertemporal permit reallocation. The significance of the highest average for any one period can be viewed as the severity of intertemporal spikes and the lowest average for any one period can be viewed as the severity of the duration of increased levels of pollution.

[27] 5 participants were expected in the last session, when in fact 17 showed up.

Under the treatments Unlimited, One Year, and Discounting, the highs were at their maximum potential of 19, 18, and19 units respectively. Whereas in the Limited treatment the high was at its maximum of 10. The Limited treatment diminishing pollution spikes is not surprising due to the very nature of the policy. Compare the high of 19 and average of 13 units for the Unlimited treatment and a high of 18 and an average of 10 units for Limited, and one can see these individual spikes are upwards to 80% higher than the average high.

TABLE XXVIII
MAX AND MIN INDIVIDUAL EMISSIONS, AND AVERAGE HIGH AND LOW EMISSIONS FOR PERIODS 9-12

Treatment	Max of	Min of	Ave high	Ave low
Counterfactual	8	8	8	8
Unlimited	19	0	13	12
One Year	18	0	10	10
Limited Use	10	0	10	9
Both	10	1	9	9
Discounting	19	0	10	8

A high of 19 used permits occurred nine times in the Unlimited treatment, 18 used permits occurred three times in the One Year treatment, and 19 permits occurred used one time in the Discounting treatment, but note that 14 permits occurred four times in this last treatment. These results were not all from the same participant. Nor were these results all taken from the 12th period, indicating that the participant did not over bank and dump the permits during the last period.

It is not clear as to why a participant would act in this reoccurring manner. Using 19 permits in any one period would drive abatement costs to zero and increase profits to their highest levels of 250 experimental dollars for that period, as stipulated by the experiment parameters. Perhaps the perceived profit for that particular period was quite high compared to the meager profits obtained in the last four periods. This is especially true when comparing it to low abatement cost periods (1-8). Or this behavior could be due to confusion.

As discussed below only a small percentage of people did not obtain cost savings in the 75%+ range. The participants who had the highest emissions were also often found to not obtain the 75%+ cost savings. Sub-optimal decision making, high emissions spikes, and low total intertemporal cost savings go hand in hand and would be expected. This would strengthen the theory that some participants were confused. In the case of discounting, the participant who used 19 permits, used them in the last period while leaving over 4 permits in the bank (therefore becoming worthless). In any case, it does not diminish that fact that the only treatments that did not have emissions spikes were ones that directly limited the amount of pollution a participant could emit.

Average highs and lows for the last four periods were similar when any banking restriction was implemented. The treatment involving both the One Year banking horizon and Limited Use had lower average highs and lows when compared to the Unlimited, One Year, and Limited treatments. This was predicted from theory that the spikes would be less and the duration of elevated emissions would be shorter.

5.1.2. Cost Savings

As stated earlier, total cost savings on average were above the 90% level. This is in stark contrast to earlier studies, where participants did not optimally bank permits as well. Also striking were the distribution of firms by cost reductions as listed in table XXIX. In all treatments conducted, greater than 89% of participants were able to extract at least 75% of the cost savings. Additionally, there were no participants who did worse than Command And Control. Again this differs from the Cronshaw et. al. (1999a) study where some participants did do worse. As reasoned in their paper, participants who had the worst cost savings were purchasing permits at a very high cost. This option was not available in this study[28]. Also note table XXX which lists how many participants obtained the theoretical optimal bank, or 100% cost savings. The Discounted treatment had no participants obtaining 100% cost savings.

TABLE XXIX
DISTRIBUTION OF FIRMS BY COST REDUCTION

Distribution of Firms By Cost Reduction				
	75%+	50%-74%	25%-49%	0%-24%
Unlimited	89.47%	8.77%	0.00%	1.75%
One Year	98.25%	1.75%	0.00%	0.00%
Limited use	98.25%	1.75%	0.00%	0.00%
Both	98.21%	1.79%	0.00%	0.00%
Discounted	91.23%	7.02%	0.00%	1.75%

[28] The option of buying permits from the conductor was part of the real world market Cronshaw Brown Kruse (1999a) were studying. Though there is usually a backstop option of obtaining permits outside the market in the real world, it is not the thrust of this thesis to study this phenomenon.

TABLE XXX
PERCENT OF PARTICIPANTS WITH OPTIMAL BANK

Percent of participants with the optimal bank		
Treatment	Number	Percent
Unlimited	26	45%
One Year	18	31%
Limited Use	33	57%
Both	35	62%
Discount	1	2%

In this experiment there seems to be little learning bias, in that the later treatments do not seem to exhibit greater cost savings. However, in table XXXI it can be seen that cohort A does much better at cost reductions that cohorts C and B. Also cohort C does better than cohort B. This leads to the belief that there is some sort of treatment bias. To further support this hypothesis, it can be seen from table XXVII that cohort A's treatments follow a natural progression of least restrictions to most restrictions. In each consecutive treatment, the decision set grew smaller. Cohort C had just the opposite, meaning that the decision set grew larger. The direction of cohort B's decision set range was changing throughout the experiment.

TABLE XXXI
COHORT COST SAVINGS

Cohort Cost Savings														
	Unlimited	spread		One Year	Spread		Limited	Spread		Both	Spread		Discount	Spread
A	97.83%	7.83%	A	97.10%	4.18%	A	99.27%	5.33%	A	98.86%	3.20%	A	91.93%	1.54%
B	87.37%	2.63%	B	92.92%	0.12%	B	93.10%	0.84%	B	92.72%	2.94%	B	88.51%	1.89%
C	90.00%		C	92.81%		C	93.94%		C	95.66%		C	90.40%	

5.1.3. Conductor Observations

There are observations that should be noted. First, the only action that can be taken is to bank or not to bank. Therefore it leads the participant to bank. This may explain the high cost savings. Second, all treatments in the banking without trade experiments were conducted using Microsoft Excel. Within the spreadsheet participants were limited in their actions to only entering in their permit use decisions which consisted of 12 cells (or decisions). In this respect they could enter in one number, see how their final payment is affected, then try a different number. There is question as to what the decisions really were. Was a participant creating a game plan of sorts to increase their profit by optimally banking? Or was it more of a "video game" where participants take guesses as to what the correct numbers should be, then revise their decision to see how their profit is affected? From visual observation of the participants, it seems that at least a little of both took place.

One observation that should be noted is that participants were quite competitive even though they were not directly playing against each other. They would ask what the maximum payment they could receive, and on more than one occasion participants attempted to communicate with each other to see who had made more money. Indeed, participants emailed the conductor requesting the optimal banking decisions after all sessions had been conducted. This leads to the believe that even though the marginal benefit of changing a banking decision may have been less than one-one hundredth of a cent, there were other incentives to make good decisions.

It is clear that the majority of participants in this study were able to cost minimize to a large degree. However these decisions were made, there was incentive to maximize profit. This reinforces that decentralized decision making on the part of the regulatory agency attempting to limit emissions, even without intra-participant trading, is well worth investigating for new environmental policy.

These experiments also show that emission spikes occur under all treatments but the ones that strictly prohibit the practice. Emission spikes were not wide spread. The Unlimited treatment showed the largest levels in aggregate emissions, while the other polices were fairly close in size and time frame, with Discounting reducing elevated levels the most.

5.2. Banking With Trading Experiments

The results of the Banking with Trading experiments are summarized in table XXXII.

TABLE XXXII
SUMMARY STATISTICS FOR BANKING WITH TRADING EXPERIMENTS

		Environments		Participants		Sequence				Time	
Treatment	No.	A	B	5	10	Total Part.	1st Exp.	2nd Exp.	3rd Exp.	<Noon	>Noon
Unlimited	10	4	6	8	2	60	4	4	2	10	0
Limited Use	13	5	8	9	4	85	5	5	3	3	10
1 YR Banking	11	5	6	4	7	90	5	4	2	5	6
BMR	10	5	5	5	5	75	5	4	1	3	7
Totals	44	19	25	26	18	310	19	17	8	21	23

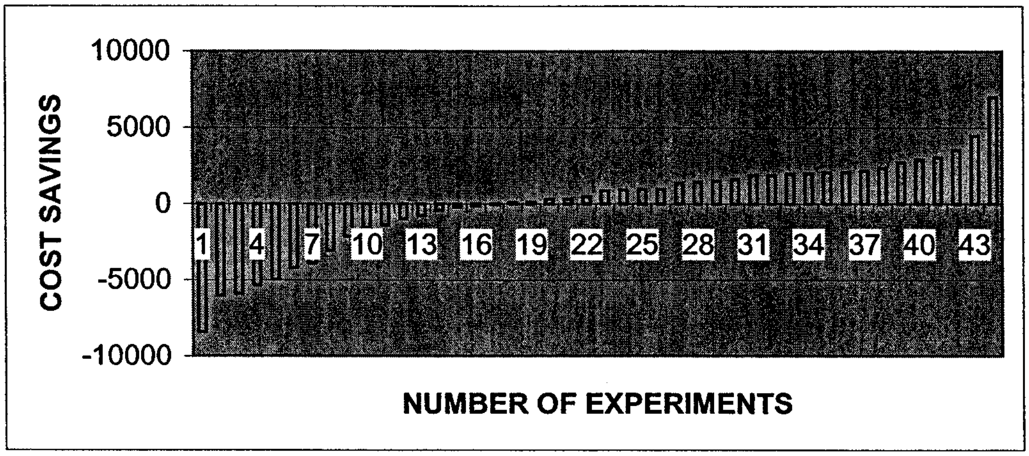
The treatments are listed first, with totals given in the final row. The No. Column, and Environments A and B list the number of experiments that were run under each treatment and environment. Two Environment B experiments were discarded due to computer malfunction. Participants 5 and 10 list how many experiments were run with either 5 participants or 10. Multiplying the participant number by the value listed in the column and adding the 5 participant and 10 participant columns together gives the total number of participants per treatment. Note that all participants were subject to both Environments A and B. The extra 6 experiments with Environment B are due to sessions in which 3 experiments were conducted. The Environment of the third session was by popular vote of the participants. Though not every participant voted for Environment B, it was the chosen Environment in 8 cases. Also, 310 total participants are listed, however there were only 77 individuals who participated, where 29, or 38% of which, repeated the experiment under a different treatment. Additionally each individual completed two or more experiments during a session. Participants' payment for a sessions ranged from a low of the $10 show up fee, to over $66 for a two hour session. Total cost for the experiment was over $3500.

The last two variables reported are how many of each sequence of experiment was conducted, and when the experiments were conducted. The Sequence variable is simply how many experiments of that treatment were run first in the session, second, or third. The Time variable lists how many experiments were conducted during the morning session, from 9:00 am to 11:00 am, and the afternoon sessions, 1:00 pm to 3:00 pm.[29]

The first interesting set of data are the abatement costs of each experiment. The costs are calculated by taking the total amount of revenue given to all participants over all time periods for one experiment, then subtracting the aggregated profit of all participants in the experiment. The result is the amount spent on emissions reduction[30]. With this information a simple picture of cost savings over Command And Control can be created. This has been done by taking the observed total aggregate abatement cost for each experiment subtracted from the aggregate Command And Control abatement cost, as shown in Figure I. This shows the cost savings over CAC, and is sorted from lowest cost savings to highest costs savings[31]. The striking observation is that 17 of the 44 experiments cost MORE than if strict Command And Control regulations had been imposed. Theory predicts emission markets would outperform CAC in emission abatement costs.

[29] Sessions were run every day of the week.
[30] Remember trading does not directly effect the total cost of pollution abatement.
[31] Inherently the cost savings are effected by the treatments. The Limited treatment cannot, by definition, obtain the same cost savings as the lowest aggregate cost of the Unlimited treatment.

FIGURE I
COST SAVINGS OVER COMMAND AND CONTROL, SORTED FROM LOWEST TO HIGHEST COST SAVINGS



This is not completely surprising in that Cronshaw Brown Kruse (1999a) also experienced experimental outcomes where abatement costs were above Command And Control costs. Though somewhat disturbing, the goal of this study is to compare banking regulations against each other, whether they yield lower costs that CAC or not[32].

It is interesting to note table XXXIII, showing the attributes of the experiments, ordered from lowest abatement cost savings over CAC to lowest. There are no distinguishing characteristics, minus maybe the number of participants, that define the experiments with higher abatement costs than CAC. Most important to this study is that all four treatments tested show up in the high abatement cost experiments, showing that these outcomes are not strictly based on treatment type.

[32] This is not to say that these outcomes are not very important and should not be studied. Individual participant data may hold the answers, to be studied at a later date.

The first column, Cost Savings, is calculated as the observed experiment costs subtracted from the cost of Command And Control. Negative numbers indicate the experiment was more costly in aggregate abatement costs, than a strict Command And Control environment. The next three columns list the experiment number (ExpNo.), or what order the experiment lies in with respect to all experiments conducted, the Treatment applied to the experiment, the environment (Env.) the experiment was conducted in, the number of participants (Part.), and the sequence of the experiment (Seq.). The experiment number is listed as it will be used later to determine if there is any experimenter bias.

The final two variables in table XXXIII are the number of contracts per person (NConPP) and the number of trades per person (NTraPP). These numbers are calculated by aggregating all contracts (trades) over all periods and participants, and dividing by the number of participants. They are an average of how active one participant was in all periods. Contracts are offers to buy or sell that may or may not be accepted by another participant. Trades are transactions that take place between two participants, and are for one permit only. This variable indicates how "thick" the market was, or how active participants were in the market. The number of contracts and trades have been modified by dividing the observed value by the number of participants in the experiment. It is assumed that experiments with 10 participants should have double the number of contracts and trades as an experiment with 5 participants.

TABLE XXXIII
STATISTICS OF BANKING WITH TRADING EXPERIMENTS

Cost Savings	ExpNo.	Treatment	Environment	Part.	Seq.	NConPP	NTraPP
-8426	45	BMR 0	B	10	1	218.8	21.9
-6016	22	Limited	A	5	2	46.2	13.4
-5916	10	BMR 0	A	10	1	79.6	20
-5342	5	One Year	A	5	1	56.8	13.8
-4990	16	One Year	B	10	1	137.8	36.6
-4230	46	BMR 0	A	10	2	233.2	25.1
-3918	14	Unlimited	B	10	1	165.7	20.6
-3070	12	BMR 0	A	10	1	152.4	25.1
-2136	13	BMR 0	B	10	2	205.2	24.7
-1576	27	Unlimited	B	5	1	54.4	18.4
-1396	21	Limited	B	5	1	43	11.8
-998	4	Limited	B	10	2	158.2	17.3
-768	23	Limited	B	5	3	25.8	6.8
-426	15	Unlimited	A	10	2	227.2	22.3
-226	24	BMR 0	B	5	1	140.2	16
-156	18	Limited	B	5	1	73.8	32
-64	40	One Year	B	10	1	84.1	21.1
88	38	Limited	A	10	1	74.5	22.2
116	36	BMR 0	B	5	1	300	8.8
312	19	Limited	A	5	2	50.2	12.6
324	26	BMR 0	B	5	3	62.6	12.8
472	20	Limited	B	5	3	33.6	5.8
872	3	Limited	A	10	1	121.1	48.1
914	29	Unlimited	B	5	3	37.6	9.6
958	25	BMR 0	A	5	2	120.8	21
962	2	Unlimited	B	5	2	192	27.6
1356	33	One Year	B	5	1	280.2	16
1440	30	Limited	B	5	1	78.6	8.8
1516	8	One Year	B	5	2	340.4	26.6
1618	39	Limited	B	10	2	87.7	22.2
1884	37	BMR 0	A	5	2	265.6	12
1886	35	One Year	B	5	3	265.2	17.2
1968	28	Unlimited	A	5	2	46.8	11
1984	31	Limited	A	5	2	28.6	7.4
2046	32	Limited	B	5	3	37.8	4.8
2074	44	Unlimited	B	5	3	71	10.4
2152	7	One Year	A	5	1	236.6	8.8
2352	9	One Year	B	5	3	389.4	18.8
2706	17	One Year	A	10	2	123.4	30
2908	43	Unlimited	B	5	2	156.6	16.8
3046	1	Unlimited	A	5	1	166.6	25.8
3530	41	One Year	A	10	2	104	20.6
4480	34	One Year	A	5	2	306.8	15.6
7024	42	Unlimited	A	5	1	228.4	20.6

One possibility for the poor outcome of the 17 experiments more costly than CAC may be participant experience. It was hypothesized that an experiment with experienced subjects would perform better than an experiment with inexperienced subjects. These experiments allowed subjects to participate in up to each of the 4 different environments. One particular experiment may contain both experienced and inexperienced subjects. In fact, most experiments did contain both. However, two experiments had no participants with experience. Also, 5 experiments were held with all experienced participants. In both the exclusively inexperienced and experienced groups there were experimental outcomes with both lower and higher costs than the aggregate abatement cost (ACC) of CAC. Additionally in all 7 cases the ACC of each treatment was clustered around the ACC of CAC. For experienced subjects to have a major impact on the outcomes, one would expect experienced experiments performing better than mixed experiments, and vice versa with inexperienced experiments. This was not the case.

5.3. Comparisons To Prior Experiments

As a comparison to other experiments table XXXIX below lists the results of the three prior experiments from table X, with the experimental parameters in this thesis were built from. The values listed are a percentage of cost savings realized in each experiment. That is the percentage of the potential cost savings the experiment was able to obtain. This cost savings is calculated as:

(CAC Cost – Observed Cost) / (CAC Cost – Simulated Cost) (20)

Banking only experiments are segregated in the listing. Also, in Franciosi et. al. (1999) the “Arizona” experiments, allocations of permit differed from the other experiments.

Percentage cost savings obtained in the thesis Banking With Trade experiments range from 87.2 to negative 161.4. The Banking Without Trade experimental values ranged from 100% to about 1%. As will be shown, these results are very similar to prior experimental results.

The Cronshaw Brown Kruse (CBK) (1999) reveal 70.9 to 29.9 percent of cost savings were realized in the Banking With Trade (BWT) experiment. In the Banking Only (BO) experiments cost savings range from around 100 percent to negative 2000 percent[33]. Comparing these results to the thesis results, the BWT thesis results had both higher and lower cost savings, and had higher cost savings in the BO experiments. The negative 2000 percent cost savings shows that experimental research can be, at times, unpredictable, but makes the negative thesis cost savings results less alarming.

The Franciosi (1999) results range from 100 percent to 7.5 percent for the CBK setup, 61.9 to 30.6 percent for the "Arizona" setup, and 64.5 to 52.8 percent for the BO experiments. The 100 percent cost savings seems somewhat out of place (indicating optimal trading, banking, and use) but eh results obtained are again similar to the thesis results.

The Mestelman (1999) study reports results ranging from 92.9 to 55.0 percent. These results, obtained using and open "pit" for trading, seem better than the prior two. It is noteworthy that the experiment generating cost savings of 74.1 percent contained economics graduate students. This result shows that using highly educated participants does not guarantee better results.

In summary, the results of all four studies, the current one included, have very similar results. This is not surprising, given that they all use the same basic setup. The

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[33] Actual percentages were not given, only rages.

compared results add credence to the notion that the parameters in the thesis were set in line with prior published research and that the experiments were implemented in line with these prior important studies.

TABLE XXXIV
PERCENTAGE COST SAVINGS OF PREVIOUS ENVRIONMENTAL EXPERIMENTS AND THE BANKING WITHOUT TRADE EXPERIMENTS

Author/ Attribute	Cronshaw Brown Kruse 1999a	Franciosi et. al. 1999	Mestelman et. al. 1999	Thesis Experiments
Percentage Cost Savings Sorted Highest to Lowest	70.9 65.9 60.0 54.8 29.9 Banking Only 100 – -2000 (15 observations negative)	100.0 46.0 24.7 7.5 Arizona 61.9 50.8 46.4 30.6 Banking Only 64.5 63.8 61.9 52.8	92.9 82.8 74.1* 65.1 55.0	87.2 to -161.4 (17 observations negative) Banking Only 100-1

* Contained graduate economics students

6. ECONOMETRIC ANALYSIS

Ordinary Least Squares was the method of regression, using the b34s econometrics software package. OLS was appropriate because the data being analyzed was panel data. There showed little evidence of heteroscedasticity from von Newmann ratios. Nonlinear relationships between dependent and independent variables were not present as tested by viewing residual scatter plots, so there was no need for modifying variables into a nonlinear fit.

All dependent variables were not dichotomous or of discrete values. Therefore Probit, nor Logit analysis was appropriate. The data in this study was not observed over time, and therefore does not need time series analysis. A contrived simultaneous system of equations was attempted, with dismal results. Finally, OLS results represented the data sufficiently. Data will initially, for the most part, be used in it's observed form and not modified. Later, in the experimental section, the data will be manipulated to cleanse the results of some of the peripheral information.

6.1. Banking Without Trading

This section will answer questions relating to cost savings of one treatment over another, and emission levels, or hotspots, and include the first set of experiments, Banking Without Trade. Results from the Banking With Trading experiments will follow.

6.1.1. Total Cost Regressions

TABLE XXXV summarizes 5 regressions with cost of abatement as the dependent variable when there was no trading allowed. Because there was no variation in the number of participants or environment, the Cost variable is the observed cost associated with each experiment, and range from 600 to 1928.

The independent variables are all dummy variables, besides the constant term, which represent the five banking treatments. The regression, where the Unlimited treatment variable is omitted, is expressed as:

$$COST_i = \beta_0 + \beta_1 \mathrm{ONEYR}_i + \beta_2 \mathrm{LIMIT}_i + \beta_3 \mathrm{BOTH}_i + \beta_4 \mathrm{DISC}_i + \varepsilon_i$$

These dummy variables were coded as 0/1, where one of the variables is omitted. Therefore the intercept, or constant term, will capture the variance of the dependent variable explained by the omitted variable, along with any other omitted variables. Interpenetration of these regressions should be done with care. When statistical significance is witnessed in the included treatment variables, they are to be viewed as significant in relation to the omitted variable. But the included treatment variables are not necessarily statistically significant in relation to the other included variables. It is for this reason, each banking treatment variable, in turn, will be omitted from the regression. In one regression the included variables have coefficients and t-statistics in relation to the omitted variable, and should be viewed as such. Also, because the five regressions are simply rotating the different treatment variables in, the adjusted r-square will remain the same. That is, the coefficient of the omitted variable will be captured by the constant variable, while the other treatment variables remain in the regression. Therefore all treatment variables are explicitly or implicitly included in each regression, and the sum of the information of the treatment variables remains constant. This type of regression analysis is found throughout the remainder of the thesis.

The lower bounds of COST for the five banking treatments, representing the least costly outcome and expected ranking of lowest cost to highest, are: Unlimited (UNL) 600; One Year 1(ONEYR) 752; Limited (LIMIT) 792; Both (BOTH) 792; and Discounted (DISC) 950. Note that the Limited and Both treatments have the same lower bound, therefore it was expected that there will be no statistically significant difference between the two. The number of observations in all banking without trade regressions are 284. Table XXXV lists the regression coefficients, with the t-statistics in parenthesis below each coefficient. Also included in the table are the R-squares, and each regression contains the same 284 observations. Bolded t-statistics are at the 95% confidence level, while italicized t-statistics are at the 90% confidence level. The convention of this table is found throughout the thesis.

TABLE XXXV
COST – BANKING WITHOUT TRADE

COST	UNL	ONEYR	LIMIT	BOTH	DISC
UNL		-106.52	-128.98	-121.24	-333.66
		(-4.11)	**(-4.98)**	**(-4.66)**	**(-12.90)**
ONEYR	106.52		-22.45	-14.71	-227.13
	(4.11)		(-0.86)	(-0.56)	**(-8.77)**
LIMIT	128.98	22.45		7.73	-204.68
	(4.98)	(0.86)		(0.29)	**(-7.911)**
BOTH	121.24	14.71	-7.73		-212.41
	(4.665)	(0.56)	(-0.29)		**(-8.17)**
DISC	333.66	227.13	204.67	212.41	
	(12.9)	**(8.77)**	**(7.91)**	**(8.17)**	
CONSTANT	723.64	830.17	852.63	844.89	1057.31
	(39.55)	**(45.38)**	**(46.60)**	**(45.77)**	**(57.79)**
Adj. R Sqr	0.377	0.377	0.377	0.377	0.377
N =	284	284	284	284	284

Note: t-statistics are parenthesis, bolded t-statistics are at the 95% confidence level, italicized t-statistics are at the 90% confidence level.

As can be seen from the second column of the table above, where the omitted dummy variable UNL is listed above for convenience, the least costly treatment in these regressions was unlimited banking. The included variables in the first regression were highly significant, with t-scores above 4, were of the correct sign, with coefficients in the expected order except the limited treatment of 128.98 (t-statistic of 4.98), which was more expensive than the both treatment of 121.24 (t-statistic of 4.66). The limited and both treatments were expected to be the same. Again, the first regression cannot be used to determine statistical significance between the both and limited treatments, only how the included treatments fared against the omitted variable.

Moving across the regressions it is clear that the unlimited banking treatment was least costly with the least cost savings of 106.50 experimental dollars over other treatments, and the lowest t-statistic of 4.11, while the discounted treatment was most costly, increasing costs above other banking treatments at least 204.60 experimental dollars, with the lowest t-statistic of 7.91. Both were highly statistically significant, well above the traditionally 95% level.

The One Year, Limited, and Both treatments were not statistically different from each other, with the greatest (absolute value) t-statistic for any of the three in any of the regressions of 0.86. This was not surprising, in that first, the limited and both banking treatments were expected to be the same. Secondly, at their optimal or lowest levels, the both and limited treatments have a value of 792 while the one year treatment was 752. The 40 experimental dollar difference is small when compared to the total costs of each of the five treatments. The other anomaly is the Limited treatment having a higher cost coefficient than the Both treatment, when no difference was expected. Again, though, their difference was a paltry 7.73. Therefore these discrepancies are not troubling, especially in light of the fact that all three were statistically different from the Unlimited and Discount treatments with the correct sign.

The consistent result from the table above shows the following inequalities of abatement costs:

$$\text{UNL} < \text{ONEYR} \cong \text{BOTH} \cong \text{LIMIT} < \text{DISC}$$

where the Unlimited and Discounted treatments were statistically different from each other and the One Year, Limited, and Both treatments. The One Year, Limited, and Both treatments were not statistically significant from each other.

As a final note concerning differences in cohorts. When all data points were included as shown in the table above, the adjusted R Squared is 0.377. When data from the first set of cohorts only was used (Cohort A), when compared to the full dataset, the adjusted R Squared jumps to 0.828, the t-statistics in general increase nearly double, and coefficients were higher. When the second and third set of cohorts were combined and used in the cost regression, the adjusted R Squared falls to 0.288 compared to the full dataset, t-statistics in general were diminished by about a third, and coefficients were also reduced. These regressions were not reported because the qualitative results were the same as in the full dataset. The Unlimited and Discounted treatments were statistically significant from one another, and from the One Year, Limited, and Both Treatments. The One Year, Limited and Both treatments were statistically insignificantly different from each other, with the Limited treatment having a meager coefficient higher than the Both treatment. There was a difference in cohort one from cohorts two and three, but the differences in banking treatments remain.

6.1.2. Spike Regressions

The cost data is important, but only part of the bigger picture. The resulting emissions of each treatment must also be evaluated, as is done in this section. The five different treatments were the independent variables regressed on the dependent variable SPIKE. SPIKE is the maximum emissions observed for each experiment, or the maximum emissions from any one participant in any one period. This analysis shows which emissions reduction programs were more prone intertemporal hotspots. Again the treatment variables, listed in expected levels of emissions, are Unlimited, One Year, which should equal Discounted, Limited, which should equal Both. The treatments were again coded as dummy variables, with one of the variables omitted in each regression. The SPIKE dependent variable ranges from 8 to 17 units.

The regression is represented as:

$$SPIKE_i = \beta_0 + \beta_1 \mathrm{ONEYR}_i + \beta_2 \mathrm{LIMIT}_i + \beta_3\ \mathrm{BOTH}_i + \beta_4\ \mathrm{DISC}_i + \varepsilon_i$$

TABLE XXXVI
MAXIMUM EMISSIONS FROM ANY ONE PARTICIPANT IN ANY ONE PERIOD

SPIKE	UNL	ONEYR	LIMIT	BOTH	DISC
UNL		2.47	4.03	4.01	3.06
		(7.81)	**(12.75)**	**(12.65)**	**(9.69)**
ONEYR	-2.47		1.56	1.54	0.59
	(-7.81)		**(4.94)**	**(4.86)**	*(1.88)*
LIMIT	-4.03	-1.56		-0.01	-0.96
	(-12.75)	**(-4.93)**		(-0.04)	**(-3.05)**
BOTH	-4.01	-1.54	0.01		-0.95
	(-12.65)	**(-4.86)**	(0.05)		**(-2.99)**
DISC	-3.06	-0.59	0.96	0.95	
	(-9.69)	*(-1.88)*	**(3.05)**	**(2.99)**	
CONSTANT	13.89	11.42	9.86	9.87	10.82
	(62.10)	**(51.05)**	**(44.07)**	**(43.75)**	**(48.39)**
Adj. R Sqr.	0.431	0.431	0.431	0.431	0.431
N =	284	284	284	284	284

Note: t-statistics are parenthesis, bolded t-statistics are at the 95% confidence level, italicized t-statistics are at the 90% confidence level.

As expected and shown in table XXXVI, the Unlimited treatment claimed the highest level of emissions, with 2.47 (t-statistic –7.81), 4.04 (-12.75), 4.01 (-12.65) and 3.07 (-9.69) units above the One Year, Limited, Both and Discounted treatments respectively. The t-statistic for the unlimited treatment was no less than 7.8 in each of the regressions. The Limited (t-statistic –4.93) and Both (t-statistic –4.86) treatments reduced emissions by 1.56 units each, when compared the One Year treatment, and were found to be statistically significant well above the traditional 95% level. The Discounted treatment was statistically significant at only the 93% level (-1.881), reducing emissions by less than 1 unit, at –0.59 when compared to the One Year treatment, and was expected to be the same. The Limited and Both treatments were statistically significant when compared to the Discounted treatment, with t-scores of –3.05 and –2.99 respectively, but not statistically different from each other. They did reduce emissions nearly 1 unit (-0.96 and –0.95) compared to the Discounted treatment. These results were as expected with only a higher adjusted R Squared desired. Though an adjusted R Squared of only 0.431 was obtained, it is still better than the cost regressions.

When regressing only the cohort variables on the SPIKE term (not reported) there revealed no significant terms. However, when the data was sorted by cohort and separated, Cohort 1 in general had higher adjusted R Squares (0.654), and higher t-statistics, but followed the results of the whole dataset. Additionally, cohort 2 and 3 had lower adjusted R Squares (0.382), and lower t-statistics. The only other mentionable is the statistical significance between the One Year and the Discounting treatments was not statistically significant in the cohort 1 only dataset.

6.1.3. Banking Without Trading Summary

There were no real surprises in these regressions, in that the variables used had the correct signs, the correct coefficient ranking, and were statistically significant as predicted. As a qualitative overview of the results, the least costly banking treatment was the Unlimited treatment. The treatments which controlled emission spikes were Limited and Both, which were not the most costly. When comparing cost to emissions spikes, the Discounting treatment, a modification of the NOx rule, was more costly than the Limited and Both treatments, but had higher emission levels by any one participant in any one period. Given the experiment parameters, this was expected. For now the attention will turn to the Banking With Trade experiments.

6.2. Banking With Trading

The first hypothesis that was tested was that banking regulations have differing aggregate costs associated with them. That is to say that the Limited treatment was associated with higher aggregate abatement costs (AAC) than the treatment, which has higher AAC's than the BMR treatment, which has higher AAC's than the One Year treatment, which has higher AAC's than the Unlimited treatment. To test this hypothesis the dependent variable Cost was obtained for each experiment. This was simply the observed cost of each experiment, which ranges from a high of 33,780 to a low of 5,300.

The main independent variables to test the above hypothesis are the four implemented treatments: Unlimited; One Year; BMR; and Limited. Again, these were coded as 0/1 dummy variables, where one of the variables was omitted for each regression. Therefore the intercept, or constant term, will capturer the variance of the dependent variable explained by the omitted variable, along with any other omitted variables. These dummy variables were coded as 0/1, where one of the variables was omitted. Therefore the intercept, or constant term, will capture the variance of the dependent variable explained by the omitted variable, along with any other omitted variables. Interpenetration of these regressions should be done with care. When statistical significance was witnessed in the included treatment variables, they are to be viewed as significant in relation to the omitted variable. But the included treatment variables were not necessarily statistically significant in relation to the other included variables. It was for this reason, each banking treatment variable, in turn, will be omitted from the regression. In one regression the included variables have coefficients and t-statistics in relation to the omitted variable, and should be viewed as such. Also, because the five regressions are simply rotating the different treatment variables in, the adjusted r-square will remain the same. That is, the coefficient of the omitted variable will be captured by the constant variable, while the other treatment variables remain in the regression. Therefore all treatment variables were explicitly or implicitly included in each regression, and the sum of the information of the treatment variables remains constant.

When ordered from expected lowest aggregate abatement cost to highest as shown from both tables XIV and XV they were: Unlimited (UNL); One Year (ONEYR); BMR (BMR); and Limited (LIMIT). In the tables below, if the omitted variable was BMR, it was predicted that Unlimited and One Year be positive in sign, and Limited be negative.

Other variables which were expected to have an effect on the cost of each experiment were either Number of Contracts Per Person or Number of Trades Per Person, The Number of Participants (5 or 10), The Variance in Banking from the Simulated Bank, the Environment, and the Experiment Number.

There were two ways for these contrived markets to reduce costs below that of Command And Control. There can be trading of permits among participants who have differing marginal abatement costs. Secondly, there can be intertemporal trading of permits with differing marginal abatement costs in different periods. To test for the first way to reduce costs, the Number of Contracts Per Person (NCONPP) or the Number of Trades Per Person (NTRAPP) were used as independent variables. A contract was defined as an offer to buy or sell, but not necessarily accepted by another participant. The rational behind this variable was that the more contracts that were offered to the market, the more the marginal cost and marginal benefit of each participant was revealed. In general, this variable shows how robust the trading market was.

A trade was defined as a contract that has been accepted, which results in an actual permit being exchanged for money. Again, the more trades there were, the more robust the market was, and the better chance it has at becoming efficient. A case could be made that an extremely high level of trading would indicate a volatile market, where an efficient equilibrium would not be obtained. A positive coefficient would be obtained if this were the case, indicating the more trades there were, the higher the abatement costs. In either case, the number of trades or number of contracts, represent market activity, as compared to banking activity.

To account for the difference in 10 versus 5 participants, the "raw" trades and contracts were divided by the number of participants. As the number of participants increases, it was expected that the number of trades would also increase. It was expected that the "thicker" or more robust the market, the closer the market will be to an efficient price. This would result in a negative coefficient, or the more robust a market, the lower aggregate abatement costs. The observed values of contracts per person range from 389.4 to 25.8 and the range of trades per person were 48.1 to 4.8.

To test how banking effects the left hand side variable, the Variance in Banking from the simulated bank in absolute terms (BANKAB). This was calculated as the absolute value of the difference in the simulated number of permits banked minus the observed number of permits banked, which was divided by the simulated number of banked permits.

The number of permits banked were the aggregate number of permits banked from the 6th to the 7th period for environment A, and the aggregate number of permits banked from the 3rd to 4th period in environment B. These particular periods were chosen because prior to these key periods, there was no reason to increase or decrease per period banked permits, and after these key periods, the banks should be drawn down in a linear fashion. As an example, there was no reason for a firm, or in aggregate, to bank permits from period one to be used in period two. This was because abatement costs, emissions, and permit allocation were exactly the same in each period. It was only after these key periods that emissions/permit allocations change.

The rational of this variable was that the more permits banked from low abatement cost periods (1-6 in A, and 1-3 in B), to high cost periods, the lower the AAC. This is true up to a point. There can be over-banking as well. Under-banking, or not saving enough permits, and over-banking leads to higher AACs. To capture this, the simulated number of permits that were banked in aggregate over these key periods were compared to the observed number of permits banked in aggregate. Both over-banking, resulting in a positive BANKAB variable, and under-banking, resulting in a negative BANKAB, would lead to a higher AAC. Therefore the absolute value was taken. A positive coefficient would be expected, as a higher BANKAB value, or a greater variance from the simulated value, would lead to a higher AAC. The observed values of BANKAB range from .9047 (nearly optimal banking) to .1190.

The Environment should have some effect on abatement costs, therefore the variable POLINC was used to account for this. This was coded as 0 for Environment A, and 1 for B. As shown in Chapter 4, environment B was predicted to have lower abatement costs, due only to emissions not rising above 10 units, where environment A emissions can rise to 19 units, with much higher abatement costs.

The Experiment Number (NOEX) was simply the number sequence that a particular experiment was conducted. The first experiment run on September 21st, 2004 at 1pm was coded 1, and the last experiment, run on November 23rd at 2:28pm was coded as 46. In theory this should have no relation to the cost. However, there may be two factors that would give this a negative coefficient. First, for the first few experiments, all participants were new to the experiment. As the number of experiments grows, there was a greater probability that participants have participated in a prior experiment. Participants were allowed to participate in up to 4 experiment, that is, up to each of the four treatments.

Additionally, there may be some experimenter bias. This would be the conductor becoming more familiar with how participants react to the experiment, expecting what questions may be asked and how best to answer them, more familiarity with reading the instructions, and a smoother experiment in general. This can simply not be ignored, even if the expected coefficient was 0 (or an insignificant t-statistic). It was assumed here that if this coefficient was not statistically different from zero, that it was linear in nature. That is to say that the effect of the first experiment will have the same marginal impact on aggregate abatement costs, as the 46th experiment.

The Sequence (SEQ), either 1, 2 or 3, was the order in which a particular experiment was conducted during one session. This was included to test learning bias. In early regressions (not reported), not unlike the banking without trade experiments, there was found to be differences in the first experiment of the session, when compared to the second and third. But there was little difference in abatement costs in the second experiment of the session when compared to the third. It was for this reason the variable was coded as 0/1, where 0 represents the first sequential experiment, and 1 represents both the second and third sequential experiments. Traditional economics would predict this variable to be statistically insignificant. But past literature regarding these types of experiments indicates that there may very well be some learning bias, creating a negative coefficient.

When conducting experiments with 5 participants, the aggregate abatement costs, regardless of the environment, should be much less than experiments conducted with 10 participants. This was simply due to twice the number of participants generating abatement costs. To account for this the Participant Number (NOPART) was included. The variable was coded 0 for 10 participants, and 1 for 5 participants. The expected sign was negative.

Table XXXVII lists the regression coefficients, with the t-statistics in parenthesis below each coefficient. Also included in the table were the R-squares, and each regression contains the same 44 observations. Bolded t-statistics were at the 95% confidence level, while italicized t-statistics were at the 90% confidence level. The convention of this table is found throughout the thesis.

The equation below shows the regression for explaining cost performance, omitting the BMR variable:

$$\begin{aligned} COST_i &= \beta_0 + \beta_1 \text{NOEX}_i + \beta_2 \text{POLINC}_i + \beta_3 \text{NCONPP}_i + \beta_4 \text{UNL}_i + \beta_5 \text{ONEYR}_i + \\ &\beta_6 \text{LIMIT}_i + \beta_7 \text{BANKAB}_i + \beta_8 \text{SEQ}_i + \beta_{10} \text{NOPART}_i + \varepsilon_i \end{aligned} \quad (26)$$

6.2.1. Cost Regressions

The independent variables listed above were regressed on the dependent variable Cost, as listed in table XXXVII. Note that four regressions are reported, each with one of the treatment variables omitted. The Adjusted R Square was 0.89.

TABLE XXXVII
COST – BANKING WITH TRADE

TOTCOST	UNL	ONEYR	LIMIT	BMR
NOEX	-52.20	-52.20	-52.20	-52.20
	(-1.61)	(-1.61)	(-1.61)	(-1.61)
POLINC	-6585.12	-6585.12	-6585.12	-6585.12
	(-7.80)	**(-7.80)**	**(-7.80)**	**(-7.80)**
NCONPP	-9.90	-9.90	-9.90	-9.90
	(-1.74)	*(-1.74)*	*(-1.74)*	*(-1.74)*
UNL		-652.61	-406.49	-3466.45
		(-0.51)	(-0.33)	**(-2.68)**
ONEYR	652.60		246.11	-2813.84
	(0.51)		(0.17)	**(-2.31)**
LIMIT	406.49	-246.11		-3059.95
	(0.33)	(-0.17)		**(-2.19)**
BMR	3466.44	2813.83	3059.95	
	(2.68)	**(2.31)**	**(2.19)**	
BANKAB	49.04	49.04	49.04	49.04
	(0.02)	(0.02)	(0.02)	(0.02)
SEQ	-1024.35	-1024.35	-1024.35	-1024.35
	(-1.18)	(-1.18)	(-1.18)	(-1.18)
NOPART	-13004.5	-13004.5	-13004.5	-13004.5
	(-13.30)	**(-13.30)**	**(-13.30)**	**(-13.30)**
CONSTANT	28978.42	29631.03	29384.91	32444.86
	(17.09)	**(16.07)**	**(20.88)**	**(17.51)**
R Squared	0.895	0.895	0.895	0.895
N =	44	44	44	44

Note: t-statistics are parenthesis, bolded t-statistics are at the 95% confidence level, italicized t-statistics are at the 90% confidence level.

The Number of experiments comes in negative as expected but not statistically significant indicating little if any experimenter bias. The Environment was significant at the 95th percentile with a coefficient value of –6,585.12 and a t-statistic of –7.80. This was expected and welcomed, given the fact that the experimental constructs, as noted earlier, heavily support this result of environment B having lower abatement cost.

The Number of Contracts Per Person was significant at the 90th percentile in all regressions, with an expected negative sign and a value of –9.90. Note that the number of contracts range from 389.4 to 25.8. The more robust the trading aspect of the market was, the lower the aggregate abatement costs was shown in this value.

The next four banking treatment variables, Unlimited, One Year, Limited, and BMR, are at the heart of this study. Note that one treatment variable was omitted and used for comparison in each of the four regressions. The results were surprising in sign and statistical significance. The Unlimited, One Year, and Limited treatments were statistically insignificant with respect to one another, but statistically significant showing lower costs than the BMR treatment.

The second column in table XXXVII shows the Unlimited treatment was 652.60 experimental dollars less costly than the One Year treatment, 406.50 less than the Limited treatment, and 3466.50 less costly than the BMR treatment. However, the only treatment which was statistically significant was the BMR treatment with a t-statistic of 2.68, with the One Year treatment obtaining a t-statistic of 0.51 and 0.33 for the Limited treatment.

The One Year treatment, found in the third column of table XXXVII followed the same trend in significance and signs in relation to the other treatments. The Limited coefficient in this regression of –246.11 showed a lower cost under that type of treatment when compared to a One Year treatment (t-statistic of –0.17). Again the BMR treatment had the highest abatement costs, adding 2813.83 experimental dollars to the abatement costs over a One Year banking treatment, with a t-statistic of 2.31.

The Limited treatment shows little difference in coefficient sign and no difference in statistical significance when compared to the other regressions in this table, but the surprising result in sign was most clear in this regression. The expected signs in the Limited regression were negative for the other three treatments indicating a lower cost associated with the included treatments. The outcome in this case was an expected negative sign for the Unlimited treatment, but an unexpected positive sign for both the One Year and BMR treatments, indicating a lower cost for the Limited treatment. The Limited treatment was expected to be the treatment with the highest aggregate abatement costs, but ranks second only to the Unlimited treatment. This was much different than what was predicted in the simulations. Comfort can be taken in the fact that the t-statistic for the One Year treatment was very low at 0.17, but it was still troublesome that the t-statistic for the Unlimited treatment was little better at –0.33, and the BMR treatment holds a high 2.19 t-statistic.

The need to report all four permutations of omitted treatment variables was most clear in the BMR regression presented in the last column of table XXXVII. All treatment variables were significant, and of expected sign minus the Limited treatment. Simply, this regression states that when compared to the other treatment variables, the BMR treatment's aggregate abatement costs, measured in experimental dollars, were 3466.44 more than the Unlimited treatment (t-statistic –2.68), 2813.83 more than the One Year treatment (t-statistic of –2.31), and 3059.95 more than the Limited treatment (t-statistic of 2.19). This statistically significant result can be explained in that it was expected for the Unlimited and One Year treatments, but not for the Limited treatment.

When viewing all four treatments as a group two things are noticed. First, the Limited treatment was out of place in regards to aggregate abatement cost savings. It should be the most costly, and in these results, less costly than two other treatments. This result is suspect due to the lack of statistical significance. Second, the BMR treatment was most costly when compared to the other three treatments, and was supported heavily by statistical significance. Which result is more viable, that the Limit treatment was out of place, or that in these constructed markets the BMR treatment simply does not perform as well as other? The latter result holds more weight due to it's statistical significance, and therefore should be taken more seriously. Additionally, it is entirely plausible that the participants in this study had a hard time organizing their decisions in the light that half of all permits saved were to be discarded.

The Banking variable was quite statistically insignificant with a t-score of 0.02 and coefficient of 45.04. The sign was as expected, indicating that as the banking in the third or sixth period deviated from the simulated models, the costs increased. Even with the low significance levels, it was still used in the regressions, as comparison to the other variable explaining cost savings – Number of Contracts Per Person (discussed above). When comparing the two variables, the conclusion can be drawn that trading (statistically significant at the 90th percentile) had a greater impact on abatement costs than banking.

The Sequence variable (SEQ) had a coefficient of –1024.35 but a t-statistic of only –1.18. The sign was correct showing that the abatement costs in the last two experiments were 1,024.35 less than the first experiment of the session. Because of the low statistical significance the value of this variable greatly diminished.

The Number of Participants (NOPART) was highly significant with a t-statistic of -13.30. The coefficient was –13,004.50 and has an expected negative sign. Not unlike the Environment variable this was reassuring, in that the constructed market should have a much larger aggregate abatement cost associated with double the number of participants. Note the variable was coded 0 for ten participants, and 1 for five, so a negative coefficient would be expected.

The coefficient was highly significant with a low t-statistic of 16.07, and a high of 20.88. As discussed earlier the coefficient captures the omitted treatment variable and any other omitted variable, therefore the coefficient was interpreted loosely as the value of the omitted variable. As a final overview of the regressions, the adjusted r-squared values was .895, indicating the explanatory variables addressing much of the variation on the aggregate cost of abatement. We now turn to the contrasting dependent variable of this study, the emissions associated with the different treatments.

6.2.2. Maximum Emissions By Any One Participant In Any One Period Regressions

As explained earlier, the Maximum emissions by any one participant (MAX) was the equivalent to a spatial hot spot in these experiments. Indeed it is these emission hot spots, both spatial and intertemporal, that the treatments in this study are grappling with. When looking at the maximum emissions by any one participant as a dependent variable, the independent variables include, the four treatment variables (UNL, ONEYR, LIMIT, and BMR), Variance in Banking (BANKAB), and the Number of Trades Per Participant (NTRAPP).

All variables were explained above, but the Number of Trades Per Participant were used in these regressions instead of Contracts Per Participant. Statistically this gives better results. Intuitively this one could argue that the greater number of contracts the greater revelation of marginal costs and benefits, hence a more efficient cost reducing market. On the other hand, in the end it is the contracts that actually transfer the permits from one participant to another, which allows the buyer to emit more. So the fact the contracts perform better in reducing costs, and trades perform better when explaining pollution seems believable.

The expected signs for the dependent variable should be roughly opposite of the cost regressions. That is, the lower the aggregate abatement costs, the better the market allocated the permits, causing higher levels of emissions for individuals and periods. Under this line of reasoning, the Variance in Banking (BANKAB) should yield a negative coefficient, indicating the further from simulated banking, the lower cost savings, and the more inefficiently the market allocated permits.

The Trades Per Person (TRAPP) should have a positive sign showing that the more robust the market, the better permit allocation, and the higher level of emissions. Again an opposite case could be made, in that a less robust market would leave some participants with extra permits that would be used to reduce costs if costs were too high, or firms over buying permits and using them if prices were too low. A lower price than simulated would foster higher emitting firms, or high abatement cost firms, to emit even more.

Additionally, the Number of Experiments (NOEX), Sequence (SEQ) and Number of Participants (NOPART) variables (not reported) were statistically insignificant in these regressions, detract statistical significance from other coefficients, and reduce the r-squared statistic. Additionally, there was no economic rationale explaining why more participants engaged in one experiment would have higher maximum emissions for one individual emitter. With regards to the Number of Experiments and Sequence variables, learning behavior did seem to explain some of the cost savings in the regressions above, but may not directly effect emissions. The reasoning was that the participants goal was to maximize profit by reducing abatement costs, not maximize (or minimize) emissions. For these reasons the Sequence and Number of Participants variables were dropped from the regressions. The variables included in the regression are listed in equation form below:

$$MAX_i = \beta_0 + \beta_1 \text{ UNL}_i + \beta_2 \text{ ONEYR}_i + \beta_3 \text{ LIMIT}_i + \beta_4 \text{ BANKAB}_i + \beta_5 \text{ NTRAPP}_i + \varepsilon_i \quad (27)$$

6.2.2.1. Environment A

The ranking of the four treatments, from highest emissions of one participant in any one period to lowest, was dependent on the Environment. It was for this reason the dataset was segregated by Environment. In this section Environment A was discussed. The MAX values range from 19 to 10 for Environment A.

Environment A's rankings from highest MAX emissions to lowest, as listed in table XIV are: Unlimited; One Year, which was equal to BMR; then Limited. In the case where the Unlimited treatment was omitted, the other three treatment variables should be negative, indicating lower emissions. However, when either the One Year or BMR treatments were omitted, the included of the two should be statistically insignificant. These results were, for the most part, obtained with an adjusted R Squared of .757, as shown in table XXXVIII below.

TABLE XXXVIII
MAXIMUM NUMBER OF PERMITS USED BY ANY ONE PARTICIPANT IN ANY ONE PERIOD – ENVIRONMENT A

MAX – A	UNL	ONEYR	LIMIT	BMR
UNL		0.947	7.99	0.71
		(0.74)	**(6.28)**	(0.57)
ONEYR	-0.94		7.05	-0.23
	(-0.74)		**(6.05)**	(-0.19)
LIMIT	-7.99	-7.05		-7.28
	(-6.28)	**(-6.05)**		**(-6.25)**
BMR	-0.71	0.23	7.28	
	(-0.57)	(0.19)	**(6.25)**	
BANKAB	-1.48	-1.48	-1.48	-1.48
	(-0.53)	(-0.53)	(-0.53)	(-0.53)
NTRAPP	0.06	0.06	0.06	0.06
	(1.30)	(1.30)	(1.30)	(1.30)
CONSTANT	16.97	16.02	8.97	16.25
	(8.43)	**(9.52)**	**(5.15)**	**(8.85)**
R Squared	0.757	0.757	0.757	0.757
N =	44	44	44	44

Note: t-statistics are parenthesis, bolded t-statistics are at the 95% confidence level, italicized t-statistics are at the 90% confidence level.

When the Unlimited treatment was omitted, the other three treatments have coefficients that were negative in sign (ONEYR –0.94, LIMIT –7.99, and BMR –0.71), which conforms to expectations. The Limited treatment's negative coefficient shows that it was associated with nearly 8 less units of emissions from the maximum of any one participant in any one period, when compared to the Unlimited treatment. Only the Limited variable was significant, with a t-statistic of –6.28, while the One Year and BMR were insignificant at –0.74 and –0.57 respectively. It was surprising that these last two were not statistically significant from other variables, but comforting in that they were with respect to one another, as they were not expected to be statistically significant from each other.

And, in fact, when the One Year treatment was omitted, the BMR variable has a low coefficient of 0.23 and an equally low t-statistic of 0.19. The Limited treatment was again statistically significant, with a t-statistic of –6.05 and a negative coefficient of -7.05. It is interesting to note that, even though the Unlimited, One Year and BMR treatments were statistically insignificant when compared to one another, they do follow the predicted rankings in terms of coefficients. Ranked from highest maximum individual emissions in any one period to lowest, they are: Unlimited; BMR; One Year; and Limited.

When the Limited treatment was omitted it was clear that this was the most powerful treatment in reducing maximum individual emissions. Not unlike the other included variables, the BMR variable holds a positive coefficient of 7.28 with a statistically significant t-statistic of 6.25. The power of the Limited variable was not surprising, as it was the only market rule strictly limiting emissions.

The BANKAB treatment has a coefficient of –1.48, showing that the further the bank in the critical period was from the simulated bank, the lower the emissions.

However it was statistically insignificant with a t-statistic of –0.53, indicating that optimal aggregate banking was not a major actor in individual emission spikes. Additionally, the Number of Trades Per Person were not statistically significant, with a t-statistic of 1.30, and a coefficient of 0.06. The banking treatments, the Limited especially, was most powerful in determining the maximum number of permits used by any one participant in any one period.

6.2.2.2. Environment B

The Max values range from 10 to 6 for Environment B. The rankings from highest MAX values to lowest of the simulated, and predicted, treatments are: Unlimited, which was equal to One Year; BMR; and Limited.

TABLE XXXIX
MAXIMUM NUMBER OF PERMITS USED BY ANY ONE PARTICIPANT IN ANY ONE PERIOD – ENVIRONMENT B

MAX – B	UNL	ONEYR	LIMIT	BMR
UNL		-0.94	2.52	-0.15
		(-1.59)	**(4.61)**	(-0.25)
ONEYR	0.94		3.47	0.79
	(1.59)		**(5.85)**	(1.26)
LIMIT	-2.52	-3.47		-2.67
	(-4.61)	**(-5.85)**		**(-4.65)**
BMR	0.15	-0.79	2.67	
	(0.25)	(-1.26)	**(4.65)**	
BANKAB	-0.06	-0.06	-0.06	-0.06
	(-0.08)	(-0.08)	(-0.08)	(-0.08)
NTRAPP	0.04	0.04	0.04	0.04
	(1.46)	(1.46)	(1.46)	(1.46)
CONSTANT	7.99	8.94	5.47	8.15
	(12.26)	**(11.75)**	**(9.88)**	**(11.9)**
R Squared	0.697	0.697	0.697	0.697
N =	44	44	44	44

Note: t-statistics are parenthesis, bolded t-statistics are at the 95% confidence level, italicized t-statistics are at the 90% confidence level.

The results for both environments are quite similar as shown in table XXXIX. The adjusted R Squared in B was 0.697. Again the only two variables which hold statistical significance were the Limited treatment variable, and the Constant. When looking at the regression which omits the Limited variable, the Unlimited, One Year, and BMR treatments have coefficients of 2.52 (t-statistic of 4.61), 3.47 (t-statistic of 5.85) and 2.67 (t-statistic of 4.65) respectively. This again shows the power of this treatment with respect to restraining emissions.

The regression which omits the One Year treatment shows negative coefficients for the BMR (-0.79), Unlimited (-0.94), and Limited (-3.47) treatments, showing the One Year treatment as having the highest level of emissions by any one participant in any one period. The BMR and Unlimited treatments were not statistically significant from the unlimited treatment (t-statistics of -1.26 and -1.59 respectively), but the ranking of the BMR treatment was not as expected. The BMR treatment should have a negative coefficient in the One Year regression, according to the simulations. This disorder adds credence to the hypothesis that participants had a hard time adapting to the BMR rules, and misallocating permits. When looking at the omitted BMR regression, the Unlimited treatment was insignificant with a t-statistic of -0.25, and a negative coefficient of -0.15, showing the mis-ordering of treatments in more detail.

As confirmed by the Banking Without Trade results, and shown in both environments, the consistent way to reduce high levels of emissions by any one participant in any one period was to limit the participant with a strict upper bound.

Though the other three variables were not statistically significant from each other, they were significantly different from the limited treatment, and produce positive coefficients.

6.2.3. Maximum Aggregate Emissions In Any One Period Regressions

The aggregate maximum emissions in any one period regressions, found in table XXXX and XL below, will look at the highest level of aggregate emissions in any one period for each experiment, and use the dependent variable called USEN. In these experiments this dependent variable represents intertemporal emission spikes. The raw USE variable was divided by 2 if ten participants were present in the experiment. This reduces the power of the Number of Participants variable (which was excluded from these regressions), to more narrowly focus the results on the included variables. The USEN variables range from 69 to 44 for Environment A, and 36 to 22 for Environment B. As represented in the mathematical representation of the regression below, the Number of Contracts Per Person variable was used instead of the Number of Trades Per Person variable:

$$USEN_i = \beta_0 + \beta_1 \text{ UNL}_i + \beta_2 \text{ ONEYR}_i + \beta_3 \text{ LIMIT}_i + \beta_4 \text{ BANKAB}_i + \beta_5 \text{ NCONPP}_i + \varepsilon_i \qquad (28)$$

6.2.3.1. Environment A

The expected values for the included variables were the same as in the MAX regressions, where the BANKAB variable should have a negative coefficient, and the Number of Contracts Per Person was predicted again to be positive. The ranking of the four banking variables were somewhat different when regressed on USEN. From highest maximum number of permits used in aggregate in one period, they were strictly ranked: Unlimited; BMR; One Year; and Limited. The actual rankings differ from predicted rankings, as can be seen in table XL below.

TABLE XL
MAXIMUM NUMBEROF PERMITS USED BY ALL PARTICIPANTS IN ANY ONE PERIOD – ENVIRONMENT A

USEN - A	UNL	ONEYR	LIMIT	BMR
UNL		7.11	13.89	11.81
		(3.56)	**(6.11)**	**(5.96)**
ONEYR	-7.11		6.77	4.69
	(-3.56)		**(3.23)**	**(2.54)**
LIMIT	-13.89	-6.77		-2.07
	(-6.11)	**(-3.23)**		(-0.97)
BMR	-11.81	-4.69	2.07	
	(-5.96)	**(-2.54)**	(0.97)	
BANKAB	-11.73	-11.73	-11.73	-11.73
	(-2.96)	**(-2.96)**	**(-2.96)**	**(-2.96)**
NCONPP	0.01	0.01	0.01	0.01
	(0.41)	(0.41)	(0.41)	(0.41)
CONSTANT	64.28	57.17	50.39	52.47
	(23.49)	**(23.62)**	**(27.98)**	**(21.05)**
R Squared	0.752	0.752	0.752	0.752
N =	44	44	44	44

Note: t-statistics are parenthesis, bolded t-statistics are at the 95% confidence level, italicized t-statistics are at the 90% confidence level.

The adjusted R Squared value of 0.752 was not nearly as interesting as the fact that all independent variables were statistically significant, minus the Number of Contracts Per Person, in the USEN Environment A regressions. As can be seen from the omitted Unlimited regressions the treatment coefficients were all negative indicating that the highest level of permits used in aggregate in any one period were greatest under the Unlimited treatment. The One Year variable in this regression shows an addition of 7.11 permits when looking at the maximum aggregate permits used under an Unlimited banking regime, when compared to the One Year banking regime. Coefficients for the Limited and BMR treatments were -13.89 and 11.81, indicating the lowest maximum was associated with the Limited treatment. The t-statistics for all three banking treatments were statistically significant at values of -3.46 for the One Year variable, -6.11 and -5.96 for the Limited and BMR treatments respectively.

The last column of table XL shows the omitted BMR regression, with the Limited treatment statistically insignificant (t-statistic -0.97), while the One Year treatment (t-statistic 2.54) and Unlimited treatments statistically significant above the 95% level. The attention is brought to this regression, as the coefficients highlight unexpected outcomes. The coefficients for the Unlimited, One Year, and Limited were as follows: 11.81, 4.69, and -2.07. The One Year coefficient should be negative, according to predicted values in table XIV. This is explained as a remnant from the cost regressions, in that the BMR treatment has a statistically significant higher cost than all other treatments, higher than predicted when compared to the Limited treatment. It would stand to reason that the higher than expected costs would also hold a lower level of maximum number of permits used in aggregate. When viewing the ranking of treatments in light of USEN, it looks like the BMR treatment simply didn't fare as predicted, in that it had lower maximum aggregate emissions than the One Year treatment, and was statistically insignificant when compared to the Limited treatment. The BMR simply dropped two rankings due to the misallocation of permits, shown here and in the COST regressions of table XXXVII.

The One Year omitted regressions also showed statistically significant treatment coefficients, with the Limited variable having a coefficient of -6.77 and a t-statistic of -3.23. The BANKAB variable was also statistically significant (t-statistic of -2.96) showing an 11.73 permit reduction in the maximum number of aggregate emission for any one period for each BANKAB unit away from predicted levels. Remember the range of BANKAB was between .91 and .10. This indicates that optimally allocating permits through banking increases the USEN variable, a proxy for intertemporal emission spikes. The Number of Contracts Per Person was not statistically significant, with a t-statistic of 0.41 and a coefficient of the right sign of 0.01, showing the trading aspect of the market having less effect on the USEN variable than the banking variables.

6.2.3.2. Environment B

Again, the dependent variables, and their predicted signs remain the same, with some exceptions to the ranking of the treatment variables, as predicted from the simulations in chapter 4. The expected ranking of the treatment variables in Environment B as taken from table XV, from highest to lowest Maximum aggregate emissions in any one period are: Unlimited, which was equal to Limited; One Year; and BMR. This can be seen below in table XLI

TABLE XLI
MAXIMUM NUMBEROF PERMITS USED BY ALL PARTICIPANTS IN ANY ONE PERIOD – ENVIRONMENT B

USEN – B	UNL	ONEYR	LIMIT	BMR
UNL		-0.75	0.07	3.83
		(-0.47)	(0.06)	**(2.59)**
ONEYR	0.75		0.83	4.59
	(0.47)		(0.48)	**(3.20)**
LIMIT	-0.07	-0.83		3.75
	(-0.06)	(-0.48)		**(2.44)**
BMR	-3.83	-4.59	-3.75	
	(-2.59)	**(-3.20)**	**(-2.44)**	
BANKAB	0.27	0.27	0.27	0.27
	(0.14)	(0.14)	(0.14)	(0.14)
NCONPP	0.01	0.01	0.01	0.01
	(2.02)	**(2.02)**	**(2.02)**	**(2.02)**
CONSTANT	26.15	26.90	26.07	22.31
	(18.06)	**(12.55)**	**(22.13)**	**(11.66)**
R Squared	0.394	0.394	0.394	0.394
N =	44	44	44	44

Note: t-statistics are parenthesis, bolded t-statistics are at the 95% confidence level, italicized t-statistics are at the 90% confidence level.

The adjusted r-squared was quite low compared to the previous regressions, even though the same variables were used, with similar results. Two things to note about this. First, in the other emissions type of regressions, the t-statistics of the statistically significant variables were all quite high, compared to the ones found in this regression, indicating the power of those variables were greater, explaining more, than what can be found here. The exception was the constant variable. Also, the Number of Contracts Per Person comes in as statistically significant with a t-statistic of 2.02 (coefficient of 0.01), which was a departure of what was found in the other regressions. This indicates that trading activity was stronger at predicting the variance in maximum aggregate emissions than banking activity, the opposite of other regressions. Why this matters for Environment B, and not A, remains unclear.

When looking at the last column of table XLI, the omitted BMR variable, the remaining treatment variables were statistically significant, and of the expected negative sign. The coefficients of the Unlimited, One Year, and Limited treatments were 3.83, 4.59, and 3.75 respectively, with associated t-statistics of 2.59, 3.20, 2.44.

The second to last column, the omitted Limited regression, shows coefficients and t-statistics of 0.07 and 0.06 for the Unlimited treatment, and 0.83 and 0.48 for the One Year treatment, which was surprising. The statistical insignificance between the Unlimited and Limited treatments were predicted, but the positive coefficient of the One Year treatment was not. However, the One Year treatment was not statistically significant, so this result was not terribly troubling.

The statistical insignificance between the One Year variable and the Unlimited variable in column 3, shows a coefficient of -0.75 and a t-statistic of -0.47. The BANKAB variable was statistically insignificant with a coefficient of 0.27 and a t-statistic of 0.14. Again reiterating what was said at the beginning of this section, the banking variables explain less in these regressions when compared to other regressions, with trading taking on more of an active role.

6.2.4. Banking With Trade Summary

Due to the different Environments having different expected values and ranking of treatments with respect to emissions spikes, overarching results were few. One qualitative consistent result was that statistically significant treatment variables were the ones at one end of a spectrum. This can be seen in the COST regressions with the BMR variable. This variable, though more costly than expected, holds the highest cost in relation to the other treatments, and was the only treatment that was statistically significant. In the maximum emissions regressions, the statistically significant variables were the Limited treatment in the MAX regressions, and the BMR treatment in the USE regressions. These variables were at either the high cost end of the spectrum, as in the case of the BMR treatment, or the low emissions end of the spectrum, as in the BMR and Limited treatments. The only anomaly was in the USEN Environment A regressions, where the Limited and BMR treatments were statistically insignificant from each other, at the low end of the spectrum, and statistically significant from the other two treatments.

The Unlimited and One Year treatments were nearly identical statistically speaking. They were only statistically different from one another in the USE-A regressions, but not in Cost, MAX, or USE-B. Though the ranking in the COST regressions were as expected (Unlimited greater than One Year), in regards to the emissions regression, they switch places and the signs were not as expected as often as they were. This leads to the conclusion that, in these regressions anyway, the performance of one was not better or worse than the other with respect to cost or emission spikes.

The Limited treatment was statistically significant in reducing emissions, and performed better than the other treatments in this function in all but the USEN-B regression where it was beat out only by the BMR treatment. This Limited treatment was also statistically insignificant, with respect to cost, when compared to the Unlimited and One Year treatments, though the coefficient ranked as a higher cost, but statistically significant in reducing costs over the BMR treatment. When pairing these two pieces of information together, one or two things may be said. First, the Limited treatment outperforms the Unlimited and One Year treatments in reducing emissions, but was not statistically significant different in cost in regards to these two treatments. This indicates a Limited treatment, in these experiments, was preferred to an Unlimited or One Year banking treatment. Second, the Limited treatment, when looking at statistical significance, performed better at both reducing MAX emissions, AND reducing costs over the BMR treatment. Therefore it was concluded that this treatment was preferred to the BMR treatment in reducing MAX emission spikes. This conclusion was in stark contrast to the simulated results, where in Environment B at least, the other three treatment were strictly preferred due to the Limited treatment's high cost of aggregate abatement and high USE emissions.

The BMR treatment was statistically significant as the highest aggregate abatement cost, but not as the lowest MAX emissions. It did, however, hold the lowest USE emissions. The BMR treatment was not statistically significant to the Limited treatment in USEN-A regressions, but was in the USEN-B regressions. In both cases the coefficients showed the BMR treatment having lower USEN values than the Limited treatment. One cannot make a strict judgment as to which is better, the Limited or BMR treatment, as the former was less costly in regards to aggregated abatement costs but has higher USE emissions, while the latter has higher costs but lower USE emissions. This underscores a main theme in this study: there are tradeoffs for using one treatment over another.

The performance of BANKAB, Number of Contracts Per Person, and Number Trades Per Person was spotty at best, and do not consistently explain the variation in the dependent variables, even though the r-squared values were above .69 in all but one set of regressions (USEN-B). However, these variables, though often statistically insignificant, were left in the regressions deliberately. Economic theory suggests that these were at the heart of the market and should be included. Their insignificance is not an indication that the market has broken down in these experiments, but rather that the banking rules were the main drivers in the variance in costs and emission spikes, and this variance had less to do with individual behavior.

As a recap in regards to costs compared to emission spikes: first, the Unlimited and One Year treatments were viewed as substitutes for each other. The Limited treatment outperformed the other treatments when looking at costs and the maximum number of emissions from any one participant in any one period (spatial emission spikes). Finally, there is a tradeoff between using the Limited and the BMR treatment when looking at the maximum number of aggregate emissions in any one period, as one has higher emissions and lower costs, while the other has lower emissions but higher costs.

6.3 Econometric Analysis Summary

As clearly shown by the banking without trade results there were tradeoffs between costs of emissions reduction programs and the emission spikes associated with the programs. The spectrum of abatement costs ran from the least costly unlimited treatments to the most costly Limited, Both, and Discounted treatments. The emission spikes followed that trend, only in the opposite direction, with respect to emissions. The Unlimited treatment having the highest emissions spike, and the Both and Limited treatments having the lowest. These regressions indicate a definite exchange of cost savings and emissions spikes. Only the Discounted treatment did not fit this trend, in that it had the highest abatement cost and high emissions.

The experiments that included trading show this same tradeoff, with differences in the type of emission spike, intertemporal or spatial. The Unlimited and One Year treatments were viewed as substitutes for each other when looking at costs, intertemporal and spatial emission spikes. The Limited treatment outperformed the other treatments with respect to costs and the maximum number of emissions from any one participant in any one period (spatial emission spikes). Finally, there is a tradeoff between using the Limited and the BMR treatment when looking at the maximum number of aggregate emissions in any one period, as one has higher emissions and lower costs, while the other has lower emissions but higher costs. Finally, in both the excluded and included trading experiments each program has it's own weakness and strength, depending on the goal of the banking regulation, whether it be to control spatial or intertemporal emission spikes.

Table XLII below summarizes the regression results of the treatment variables through inequalities. The Treatments were ordered by coefficient. The cost inequalities were ordered from lowest cost to highest. The emission inequalities were ordered from highest emission spikes to lowest. The greater than or less than sign (>, <) indicates statistical significance at the 95th percentile. The similar but not equal sign ($\cong$) indicates that the variable was not statistically significant at the 95th percentile. For instance, the first row lists the Cost of Banking Without Trade. The Unlimited treatment was statistically larger than all other treatments, where the One Year, Both And Limited treatments were statistically insignificant from each other (but still statistically significant from the Unlimited and BMR treatments), and the Discounting treatment was statistically more costly than all other treatments.

TABLE XLII
REGRESSION SUMMARY

Cost of Banking Without Trade	UNL < ONEYR ≅ BOTH ≅ LIMIT < DISC
Emissions of Banking Without Trade	UNL > ONEYR > DISC > LIMIT ≅ BOTH
Cost of Banking With Trade	UNL ≅ LIMIT ≅ ONEYR < BMR
Spatial Emission Spikes of Banking With Trade – A	UNL ≅ BMR ≅ ONEYR > LIMIT
Intertemporal Emission Spikes of Banking With Trade – A	UNL > ONEYR > BMR ≅ LIMIT
Spatial Emission Spikes of Banking With Trade – B	ONEYR ≅ BMR ≅ UNL > LIMIT
Intertemporal Emission Spikes of Banking With Trade – B	ONEYR ≅ UNL ≅ LIMIT > BMR

7. CONCLUSIONS AND POLICY RECOMMENDATIONS

7.1. Conclusions

The three hypotheses for this study are restated:

H_o^1: Different banking regulations on market based emissions reduction programs yield statistically significant differences in abatement costs.

H_o^2: Different banking regulations on market based emissions reduction programs yield statistically significant differences in intertemporal emission spikes.

H_o^3: Different banking regulations on market based emissions reduction programs yield statistically significant differences in spatial emission spikes.

These three hypotheses build the overarching question of: what are the costs and benefits of using one banking regulation over another? Obviously, if one banking regulation has lower aggregate abetment costs and lower emission spikes compared to another treatment, there can be a strict preference made of the treatment which performs better. As shown, there is a tradeoff of lower abatement costs with higher emissions spikes, and vice versa. Ultimately, the damage function of the emitted pollution will determine which banking regulation is best.

Comparisons different banking treatments are made in Chapter 4, built from the parameters in chapter 3. In the *Banking Without Trade* and the *Banking With Trade* experiments, the ranking of the four main treatments from lowest abatement costs to highest abatement costs and highest maximum emissions from any one person in any one period to lowest are: Unlimited, One Year, BMR (Discounted), and Limited. Though this ranking is not strict, in that in some cases one treatment is simulated as having the same value as another, but weakly (not strictly) consistent across simulations. From these simulations it is suggested that, for the highest maximum emissions from any one participant at least, there is a direct tradeoff between individual emission spikes and abatement costs.

When comparing the maximum aggregate abatement costs and aggregate emissions in any one time period, the relationships differ, and are only appropriate for the Banking With Trading experiments. The simulations show that under a constant permit allocation and increasing emissions, the rankings of the four main treatments from highest to lowest aggregate emissions in one time period are: Unlimited; BMR; One Year; and Limited. In light of the abatement costs, the BMR treatment is clearly sub-optimal, while the others' rankings show a tradeoff between intertemporal emission spikes and aggregate abatement costs.

Under constant emissions over time, and permit allocation being reduced, the rankings from highest aggregate emissions in one time period are: Unlimited, which is equal to Limited; One Year; and BMR. Here the Limited treatment simulated result is sub-optimal, having higher abatement costs than the other treatments, but not reducing intertemporal emissions spikes as well. The other treatments again show a tradeoff between intertemporal emissions spikes and abatement costs.

The banking without trading experiments mainly support the simulated results with statistical significance, except the Discounted treatment. Though the cost of the One Year treatment was statistically insignificant compared to the Both and Limited treatments, the coefficient was ranked in order. The big surprise was the Discounted treatment which mimics the NOx Budget Model Rule. The treatment held the highest cost, but not the lowest emission spikes.

The most striking result of the experimental trials involving banking with trading was that 17 out of 44 experiments had higher aggregate abatement costs than a traditional Command And Control market scheme would have. To date this remains a mystery, and will continue to be until further research is conducted. There are no individual characteristics, such as banking treatment, environment, or number of participants, strictly associated with this phenomenon. It is hoped that firm level data, to be explored later, will explain this outcome.

The Limited treatment outperformed other treatments in regards to reducing maximum emissions by any one participant in any one period for both the Banking Without, and Banking With Trading experiments. This was a consistent result throughout the experimental results. The simulated results show a Limited treatment reducing individual emission spikes the most, with the highest costs. Experimentally, the emission spikes remained lowest under this treatment, but costs were not statistically significant above other treatments except for the BMR (Discount) treatment, and the Unlimited treatment in the Without Trading experiments.

The Limited and Budget Model Rule treatments performed well in regards to reducing maximum aggregate emissions in any one period, or intertemporal emissions spikes. The Budget Model Rule results are statistically significant, and lowest in intertemporal emission spikes. However, as mentioned before, the highest costs. The Limited treatment was also statistically significant, and it's coefficient indicated it also had low intertemporal emission spikes, second only to the Budget Model Rule. The

Limited treatment was not as costly, in aggregate abatement costs, as the Budget Model Rule. Again this shows a tradeoff between intertemporal emission spikes and aggregate abatement costs.

The main conclusions, reiterated, are that there is in fact a tradeoff between abatement cost reductions and emission spikes. The Unlimited and One Year treatments were less costly, but had higher emissions than the other two. However, they experimentally were statistically insignificant from one another, in all but the USEN variable under the Environment A. This adds more credence to the call for eliminating the one year banking rule associated with the ERMS market (Kosobud 2004b).

Strictly limiting emissions, in theory, has the highest abatement costs, limits individual emission spikes the most, and may or may not limit aggregate emission spikes depending on the market it is applied to. Experimental results support the simulated results concerning emissions, in coefficient sign at least, if not at the traditional 95% statistical significance level, but remains one of the lower cost treatments.

The Budget Model Rule, or the modifications of the rule used in this study, illustrates a robust market rule, that limits emissions well, but with a higher associated abatement cost. Experimental results heavily support the high costs, indicating the highest costs tested, but not always the lowest emissions, particularly when looking at individual (spatial), as opposed to aggregate (intertemporal), emissions spikes. Experimentally, it does equal or surpass the aggregate emission spike reductions of all other treatments.

Finally, as stated several times throughout this study, the final word does not lie in the aggregate abatement costs and the associated emission spikes. But rather how these interact with the damage function the market is associated with. More on this will be addressed directly below.

7.2. *Policy recommendations*

Though the Budget Model Rule, as stated in this thesis, is associated with the highest costs, the use of the Implement BMR to control aggregate intertemporal emissions spikes is not discouraged. It does have some unique qualities that have been discussed in theory and explored experimentally. In theory, if the damage function is non-linear, makes a large "step" or becomes very steep after a certain point, it may make sense to implement this type of strategy. It does, experimentally, control intertemporal emissions spikes well, and would be appropriate if these spikes were very damaging. Even with these higher aggregate abatement costs, in theory it still out performs a Command And Control scheme with respect to aggregate abatement costs.

Implementing a policy that allows trading and banking, but puts an upper limit on the amount of pollution any one participant may emit, sounds costly but may be very useful. In theory it is the most costly of the four main treatments explored. However, it performed better at reducing individual emission spikes, here explained as spatial hotspots, while costs were statistically insignificant or lower (or both) when compared to other treatments.

This treatment did not perform as well when limiting intertemporal emission spikes, and may not be appropriate for that task. But if a regulator wishes to maintain a certain level of emissions in a particular location, or ensure spatial hot spots do not occur which may be associated with a high damage costs, putting an upper limit on the number of permits, or the amount of pollution one firm may emit, would be recommended.

Finally, allowing unfettered trading and banking is associated with the lowest levels of aggregate abatement costs, but the highest levels of both kinds of hot spots. If a damage function is linear, or even flatter with higher levels of emissions (a negative third derivative - which may or may not exist in application), a straight market mechanism, with little government interference would be recommended.

The one year banking rule associated with the ERMS market in Illinois, to date, has not been a binding constraint. Additionally, as shown in the experiments, even when applied to a market where it should have been a constraint, it still did not reduce emissions spikes over time, nor cost more. A case could be made that the one year banking rule is not harming the market, because the costs are not statistically significant from an unlimited market. However, it does not perform it's task of reducing intertemporal hot spots, so why force this extra regulation on a market, with added regulation and frustration? It is suggested that this market rule be dropped from the active market. As a final note, the damage function and the political economy should/will ultimately decide on the "right" program for each individual market.

7.3. Future Work

The first task is to explore some of the more subtle experimental results. From these results, and the results of the individual participants, it is hoped that understanding will be gained as to why 17 of the 44 experiments were more costly than the Command And Control. Also, there may be more information as to which banking treatment performed better at an individual level.

In addition to the current set of data, numerous other experiments have been suggested. Additions to the experiments have been to change abatement costs over time, introduce futures and options, and add other types of banking rules.

Also, it would be interesting, after these market based emissions controls are more mature, to try to make some cross comparisons between real markets, and their banking rules and regulations. As explained by Harrison (2004):

"…we see the beauty of lab experiments within a broader context—when they are combined with field data, they permit sharper and more convincing inference." (Harrison 2004, p.1009).

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DEFINITIONS

Experiment – a set time period in which participants are able to make decisions. This includes instructions on how to make decisions and use the computer interface. In this study it is a set of 9 periods.

Environment – a set of parameters in which all participants are subject to within an experiment.

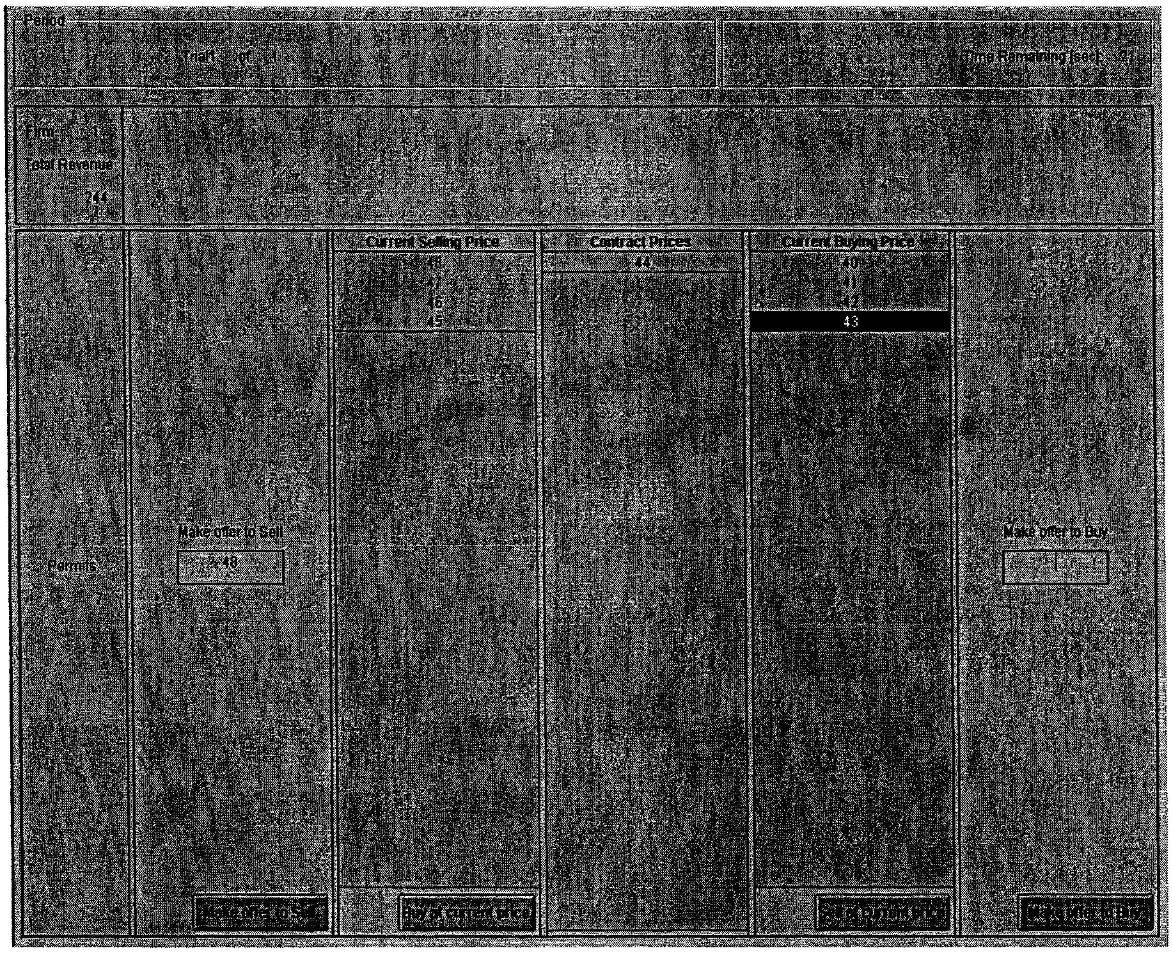
Session – a group of experiments taking place during a set time, either one morning or one afternoon.

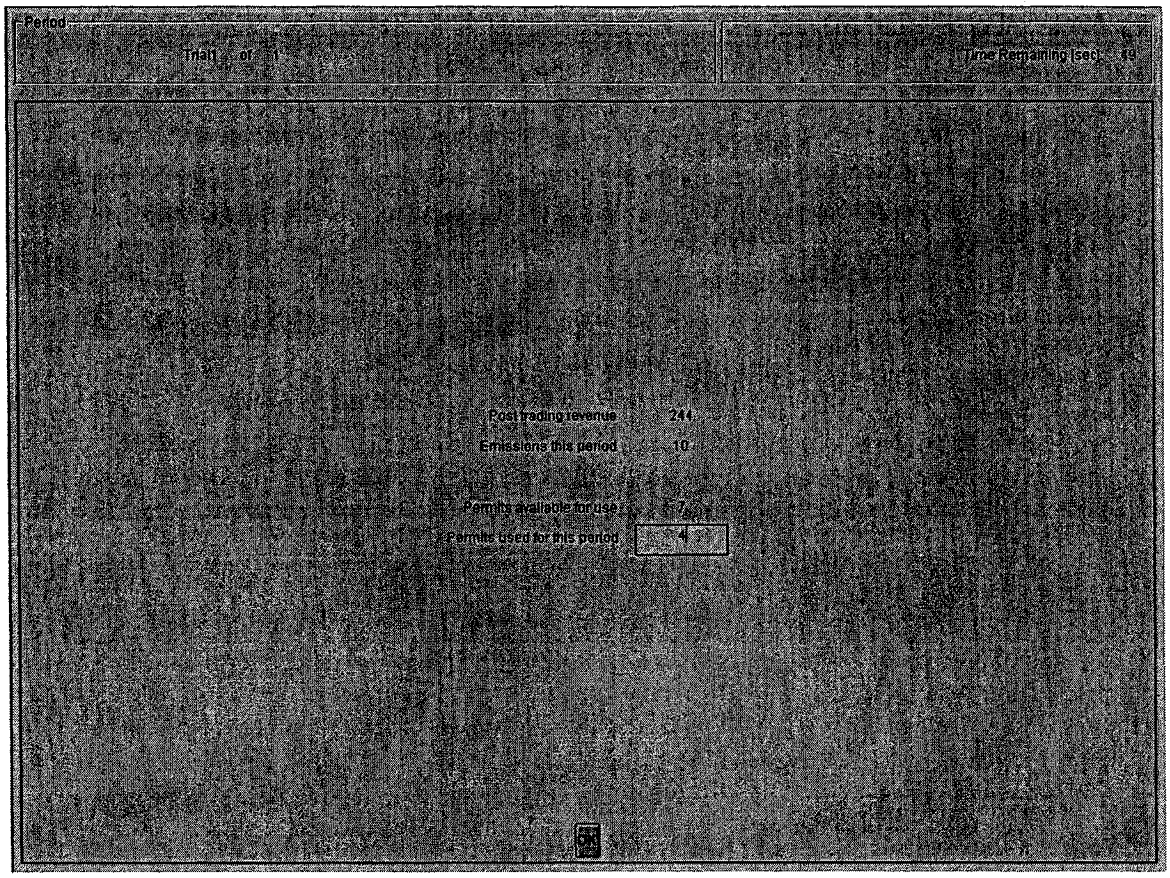
Participant - a human taking part in an experiment. Also referred to as a "firm"

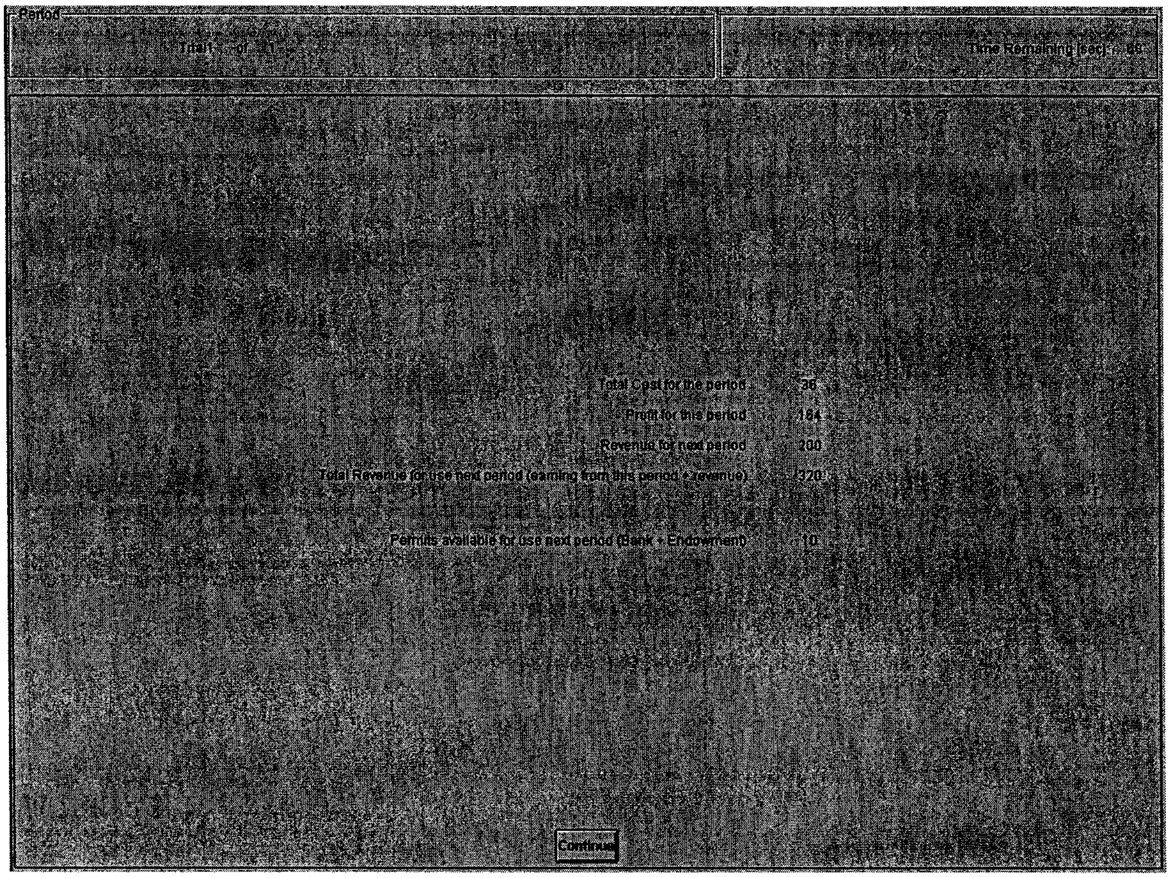
Treatment – a subset of parameters of an environment that remain constant over two or more environments

APPENDIX I

The following are screens from the experiment as the participants saw them during the experiment. Shown are screens from the Unlimited treatment. Screen One is the trading screen. Screen Two is the Banking/Use Decision screen. Screen Three is the Period Summary screen.




Period
Trial 1 of 1
Time Remaining [sec]
Post trading revenue
Emissions this period 10
Permits available for use 7
Permits used for this period 4


Period
Time Remaining (sec)
Total Cost for the period 36
Profit for this period 184
Revenue for next period 200
Total Revenue for use next period (earning from this period + revenue) 320
Permits available for use next period (Bank + Endowment) 10
Continue

APPENDIX II

In this appendix the code for solving least cost solutions in the b34s program are given.

Unlimited:

```
b34sexec matrix;
program test;
func=(2.*(10. - x1)**2. + 4.*(10. - x2)**2. + 8.*(10. - x3)**2. +
10.*(10. - x4)**2. + 12.*(10. - x5)**2. + 2.*(10. - x6)**2. +
4.*(10. - x7)**2. + 8.*(10. - x8)**2. + 10.*(10. - x9)**2. +
12.*(10. - x10)**2. + 2.*(19. - x11)**2. + 4.*(19. - x12)**2. +
8.*(19. - x13)**2. + 10.*(19. - x14)**2. + 12.*(19. - x15)**2.)   ;

if(%active(1)) g(1)=x1 + x2 + x3 + x4 + x5 + x6 + x7 + x8 + x9 +x10
 + x11 + x12 + x13 + x14 + x15 - 120.   ;

if(%active(2)) g(2)=(x1) + 0.;

if(%active(3)) g(3)=(x6) + 0.;

return;
end;
```

```
call print(test);

call echooff;

call NLPMIN1(func g :name test :parms x1 x2 x3 x4 x5 x6 x7 x8 x9 x10
x11 x12 x13 x14 x15
            :ivalue array(:1.,5.,10.,10.,10.,1.,5.,10.,
10.,10.,1.,5.,10.,15.,15.)
                :nconst 3 1
            :lower  array(:-1.d+1, -1.d+1, -1.d+1,
-1.d+1, -1.d+1, -1.d+1, -1.d+1, -1.d+1, -1.d+1, -1.d+1, -1.d+1, -1.d+1,
 -1.d+1, -1.d+1, -1.d+1)
            :upper  array(: 1.d+4,  1.d+4,  1.d+4,
1.d+4,  1.d+4,  1.d+4,  1.d+4,  1.d+4,  1.d+4,  1.d+4,  1.d+4,  1.d+4,
  1.d+4,  1.d+4,  1.d+4)
            :print :maxit 100
            :iprint final);
b34srun;
```

Limited Use:

```
b34sexec matrix;
program test;
func=(2.*(10. - x1)**2. + 4.*(10. - x2)**2. + 8.*(10. - x3)**2. +
10.*(10. - x4)**2. + 12.*(10. - x5)**2. + 2.*(10. - x6)**2. +
```

```
4.*(10. - x7)**2. + 8.*(10. - x8)**2. + 10.*(10. - x9)**2. +
12.*(10. - x10)**2. + 2.*(19. - x11)**2. + 4.*(19. - x12)**2. +
8.*(19. - x13)**2. + 10.*(19. - x14)**2. + 12.*(19. - x15)**2.)   ;

if(%active(1)) g(1)=x1 + x2 + x3 + x4 + x5 + x6 + x7 + x8 + x9 +x10
 + x11 + x12 + x13 + x14 + x15 - 120.   ;

 IF(%ACTIVE(2)) G(2)=(X1) + 0.$
 IF(%ACTIVE(3)) G(3)=(X6) + 0.$
 IF(%ACTIVE(4)) G(4)=(X11) + 0.$
IF(%ACTIVE(6)) G(6)=(-1.*(X11)) + 10.$
 IF(%ACTIVE(7)) G(7)=(-1.*(X12)) + 10.$
 IF(%ACTIVE(8)) G(8)=(-1.*(X13)) + 10.$
 IF(%ACTIVE(9)) G(9)=(-1.*(X14)) + 10.$
 IF(%ACTIVE(10)) G(10)=(-1.*(X15)) + 10.$

 return;
 end;
 call print(test);
 call echooff;

 call NLPMIN1(func g :name test :parms x1 x2 x3 x4 x5 x6 x7 x8 x9 x10
 x11 x12 x13 x14 x15
```

```
            :ivalue array(:1.,5.,10.,10.,10.,1.,5.,10.,
10.,10.,1.,5.,10.,15.,15.)
                :nconst 10 2
            :lower  array(:-1.d+1, -1.d+1, -1.d+1,
-1.d+1, -1.d+1, -1.d+1, -1.d+1, -1.d+1, -1.d+1, -1.d+1, -1.d+1, -1.d+1,
 -1.d+1, -1.d+1, -1.d+1)
            :upper  array(: 1.d+2,  1.d+2,  1.d+2,
1.d+2,  1.d+2,  1.d+2,  1.d+2,  1.d+2,  1.d+2,  1.d+2,  1.d+2,  1.d+2,
  1.d+2,  1.d+2,  1.d+2)
            :print :maxit 100
            :iprint final);
b34srun;
```

One Year Banking Treatment:

```
b34sexec matrix;
program test;
func=(2.*(10. - x1)**2. + 4.*(10. - x2)**2. + 8.*(10. - x3)**2. +
10.*(10. - x4)**2. + 12.*(10. - x5)**2. + 2.*(10. - x6)**2. +
4.*(10. - x7)**2. + 8.*(10. - x8)**2. + 10.*(10. - x9)**2. +
12.*(10. - x10)**2. + 2.*(19. - x11)**2. + 4.*(19. - x12)**2. +
8.*(19. - x13)**2. + 10.*(19. - x14)**2. + 12.*(19. - x15)**2.)   ;
```

```
if(%active(1)) g(1)=x1 + x2 + x3 + x4 + x5 + x6 + x7 + x8 + x9 +x10
 + x11 + x12 + x13 + x14 + x15 - 120.  ;

 IF(%ACTIVE(2)) G(2)=(X1) + 0.$
 IF(%ACTIVE(3)) G(3)=(X6) + 0.$
 IF(%ACTIVE(4)) G(4)=(X11) + 0.$

 IF(%ACTIVE(5)) G(5)=(-1.*(X11 + X12 + X13 + X14 + X15)) + 53.3$
return;
end;
call print(test);
call echooff;
call NLPMIN1(func g :name test :parms x1 x2 x3 x4 x5 x6 x7 x8 x9 x10
x11 x12 x13 x14 x15
                :ivalue array(:1.,5.,10.,10.,10.,1.,5.,10.,
10.,10.,1.,5.,10.,15.,15.)
                     :nconst 5 1
                :lower  array(:-1.d+1, -1.d+1, -1.d+1,
-1.d+1, -1.d+1, -1.d+1, -1.d+1, -1.d+1, -1.d+1, -1.d+1, -1.d+1, -1.d+1,
 -1.d+1, -1.d+1, -1.d+1)
                :upper  array(: 1.d+2,  1.d+2,  1.d+2,
1.d+2,  1.d+2,  1.d+2,  1.d+2,  1.d+2,  1.d+2,  1.d+2,  1.d+2,  1.d+2,
  1.d+2,  1.d+2,  1.d+2)
```

```
            :print :maxit 100
            :iprint final);
b34srun;

BMR = 0:

b34sexec matrix;

program test;
func=(2.*(10. - x1)**2. + 4.*(10. - x2)**2. + 8.*(10. - x3)**2. +
10.*(10. - x4)**2. + 12.*(10. - x5)**2. + 2.*(10. - x6)**2. +
4.*(10. - x7)**2. + 8.*(10. - x8)**2. + 10.*(10. - x9)**2. +
12.*(10. - x10)**2. + 2.*(19. - x11)**2. + 4.*(19. - x12)**2. +
8.*(19. - x13)**2. + 10.*(19. - x14)**2. + 12.*(19. - x15)**2.)   ;


 IF(%ACTIVE(1)) G(1)=X1 + X2 + X3 + X4 + X5 + X6 + X7 + X8 + X9 +X10
 + X11 + X12 + X13 + X14 + X15 + X11 + X12 + X13 + X14 + X15 - 160.$

 IF(%ACTIVE(2)) G(2)=(X1) + 0.$
 IF(%ACTIVE(3)) G(3)=(X6) + 0.$
```

```
IF(%ACTIVE(4)) G(4)=(X11) + 0.$
return;
end;
call print(test);
call echooff;
call NLPMIN1(func g :name test :parms x1 x2 x3 x4 x5 x6 x7 x8 x9 x10
x11 x12 x13 x14 x15
        :ivalue array(:1.,5.,10.,10.,10.,1.,5.,10.,
10.,10.,1.,5.,10.,15.,15.)
            :nconst 4 1
        :lower  array(:-1.d+1, -1.d+1, -1.d+1,
-1.d+1, -1.d+1, -1.d+1, -1.d+1, -1.d+1, -1.d+1, -1.d+1, -1.d+1, -1.d+1,
 -1.d+1, -1.d+1, -1.d+1)
        :upper  array(: 1.d+2,  1.d+2,  1.d+2,
1.d+2,  1.d+2,  1.d+2,  1.d+2,  1.d+2,  1.d+2,  1.d+2,  1.d+2,  1.d+2,
  1.d+2,  1.d+2,  1.d+2)
        :print :maxit 100
        :iprint final);
b34srun;
```

Environment B:

The objective function is the same as in Environment A, except emissions for each firm are 10 units, and do not jump to 19 in the latter period. The initial constraint is the same as environment A, except there are now only 80 permits to be allocated.

```
FUNC=(2.*(10. - X1)**2. + 4.*(10. - X2)**2. + 8.*(10. - X3)**2. +
10.*(10. - X4)**2. + 12.*(10. - X5)**2. + 2.*(10. - X6)**2. +
4.*(10. - X7)**2. + 8.*(10. - X8)**2. + 10.*(10. - X9)**2. +
12.*(10. - X10)**2. + 2.*(10. - X11)**2. + 4.*(10. - X12)**2. +
8.*(10. - X13)**2. + 10.*(10. - X14)**2. + 12.*(10. - X15)**2.)   $
```

```
IF(%ACTIVE(1)) G(1)=X1 + X2 + X3 + X4 + X5 + X6 + X7 + X8 + X9 +X10
 + X11 + X12 + X13 + X14 + X15 - 80.   $
```

Limited:

The following restrictions are placed on the Limited use case.

```
IF(%ACTIVE(21)) G(21)=(X2) + 0.$
IF(%ACTIVE(6)) G(6)=(-1.*(X6)) + 6.$
IF(%ACTIVE(7)) G(7)=(-1.*(X7)) + 6.$
IF(%ACTIVE(8)) G(8)=(-1.*(X8)) + 6.$
IF(%ACTIVE(9)) G(9)=(-1.*(X9)) + 6.$
IF(%ACTIVE(10)) G(10)=(-1.*(X10)) + 6.$
IF(%ACTIVE(11)) G(11)=(-1.*(X11)) + 6.$
IF(%ACTIVE(12)) G(12)=(-1.*(X12)) + 6.$
```

```
IF(%ACTIVE(13)) G(13)=(-1.*(X13)) + 6.$

IF(%ACTIVE(14)) G(14)=(-1.*(X14)) + 6.$

IF(%ACTIVE(15)) G(15)=(-1.*(X15)) + 6.$

IF(%ACTIVE(16)) G(16)=(-1.*(X1)) + 6.$

IF(%ACTIVE(17)) G(17)=(-1.*(X2)) + 6.$

IF(%ACTIVE(18)) G(18)=(-1.*(X3)) + 6.$

IF(%ACTIVE(19)) G(19)=(-1.*(X4)) + 6.$

IF(%ACTIVE(20)) G(20)=(-1.*(X5)) + 6.$


BMR = 0:

IF(%ACTIVE(1)) G(1)=X1 + X2 + X3 + X4 + X5 + X6 + X7 + X8 + X9 +X10

 + X6 + X7 + X8 + X9 +X10 + X11 + X12 + X13 + X14 + X15

 + X11 + X12 + X13 + X14 + X15 - 120.$
```

APPENDIX III

Regression analysis code. All code is written for the b34s software. This is representative code for the Banking With Trading Experiments, BMR treatment excluded.

```
b34sexec regression;
model TotCC=
Unl       Oneyr     Limit
PolInc    BankAb    NoEx      NConPP    Seq
NoPart    $
```

APPENDIX IV

Research Protocol # 2003-0829 is found on the following two pages

UNIVERSITY OF ILLINOIS
AT CHICAGO

Office for the Protection of Research Subjects (OPRS)
Office of the Vice Chancellor for Research (MC 672)
203 Administrative Office Building
1737 West Polk Street
Chicago, Illinois 60612-7227

Exemption Granted

January 14, 2004

Brian Scott, MA
Economics
601 S. Morgan
2103 UH, M/C 144
Chicago, IL 60612
Phone: (312) 996-2683

RE: Research Protocol # 2003-0828
"Are There Limiting Effects of Banking Horizons on Pollution Permit Markets?"

Dear Mr. Scott:

Your Claim of Exemption was reviewed on January 5, 2004 and it was determined that your research protocol meets the criteria for exemption as defined in the U. S. Department of Health and Human Services Regulations for the Protection of Human Subjects [(45 CFR 46.101(b)]. You may now begin your research.

Your research may be conducted at UIC.

The specific exemption category under 45 CFR 46.101(b) is:

(2) Research involving the use of educational tests (cognitive, diagnostic, aptitude, achievement), survey procedures, interview procedures or observation of public behavior, unless:
(i) information obtained is recorded in such a manner that human subjects can be identified, directly or through identifiers linked to the subjects; and (ii) any disclosure of the human subjects' responses outside the research could reasonably place the subjects at risk of criminal or civil liability or be damaging to the subjects' financial standing, employability, or reputation.

You are reminded that investigators whose research involving human subjects is determined to be exempt from the federal regulations for the protection of human subjects still have responsibilities for the ethical conduct of the research under state law and UIC policy. Please be aware of the following UIC policies and responsibilities for investigators:

1. Amendments You are responsible for reporting any amendments to your research protocol that may affect the determination of the exemption and may result in your research no longer being eligible for the exemption that has been granted.

2. Record Keeping You are responsible for maintaining a copy all research related records in a secure location in the event future verification is necessary, at a minimum these documents include: the research protocol, the claim of exemption application, all

questionnaires, survey instruments, interview questions and/or data collection instruments associated with this research protocol, recruiting or advertising materials, any consent forms or information sheets given to subjects, or any other pertinent documents.

3. Final Report When you have completed work on your research protocol, you should submit a final report to the Office for Protection of Research Subjects (OPRS).

4. Information for Human Subjects UIC Policy requires investigators to provide information about the research protocol to subjects and to obtain their permission prior to their participating in the research. The information about the research protocol should be presented to subjects in writing or orally from a written script. When appropriate, the following information must be provided to all research subjects participating in exempt studies:
   a. The researchers affiliation; UIC, VACHCS-WS or other institutions,
   b. The purpose of the research,
   c. The extent of the subject's involvement and an explanation of the procedures to be followed,
   d. Whether the information being collected will be used for any purposes other than the proposed research,
   e. A description of the procedures to protect the privacy of subjects and the confidentiality of the research information and data,
   f. Description of any reasonable foreseeable risks,
   g. Description of anticipated benefit,
   h. A statement that participation is voluntary and subjects can refuse to participate or can stop at any time,
   i. A statement that the researcher is available to answer any questions that the subject may have and which includes the name and phone number of the investigator(s).
   j. A statement that the UIC IRB/OPRS or VACHCS-WS Patient Advocate Office is available if there are questions about subject's rights, which includes the appropriate phone numbers.

Please be sure to:
→Use your research protocol number (listed above) on any documents or correspondence with the IRB concerning your research protocol.

We wish you the best as you conduct your research. If you have any questions or need further help, please contact me at (312) 355-2908 or the OPRS office at (312) 996-1711. Please send any correspondence about this protocol to OPRS at 203 AOB, M/C 672.

Sincerely,

Charles Hoehne, BS
Assistant Director, IRB # 2
Office for the Protection of Research Subjects

Enclosure(s): None

cc: Barry Chiswick, Economics, M/C 144
Helen H. Roberts, Economics, M/C 144

CURRICULUM VITA

Brian L. Scott

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EDUCATION

Ph.D. Candidate, Economics, University of Illinois at Chicago

Date of Defense: June 7, 2005

Dissertation: "*The Effects of Banking Regulations On Emission Permit Markets: An Experimental Analysis*"

Fields of Specialization:

- Environmental and Resource Economics
- Experimental Economics
- Industrial Organization

M.A., Economics, April 2002, University of Illinois at Chicago

International Business Linkage Program, August 1993 to May 1994, Helsinki University of Technology, Espoo, Finland

Postgraduate Areas of Study: International Marketing and International Business Strategy

B.A., Economics, Business Administration, Emphasis: Finance, May 1993, Doane College, Crete, Nebraska

PUBLICATIONS

Kosobud, Richard F., Houston H. Stokes, Carol D. Tallarico, Brian L. Scott, "Valuing Tradable Private Rights to Pollute the Public's Air", *Review of Accounting and Finance*, (forthcoming) Vol. 4, No. 1, 2005

Kosobud, Richard F., Houston H. Stokes, Carol D. Tallarico, Brian L. Scott, "The Chicago VOC Trading System: The Consequences of Market Design for Performance", Massachusetts Institute of Technology Center for Energy and Environmental Policy Research Working Paper Series, 04-019, November 2004

WORKING PAPERS

"Performance of a Cap-and-Trade Market for VOC Emissions Control", with Kosobud, Richard F., Houston H. Stokes, Carol D. Tallarico, submitted to *Environmental and Resource Economics*

"The Incompatibility of Market Incentives and Traditional Regulations in the Chicago VOC Trading System," with Kosobud, Richard F., Houston H. Stokes, Carol D. Tallarico, submitted to *Contemporary Economic Policy*

"Permit Banking Regulations and Their Effects on Costs and Emissions: A Simulated Analysis"

"The Effects of Banking Regulations On Emissions Permit Markets: An Experimental Analysis"

"Cost-Effective Control of Urban Smog: The Significance of the Chicago VOC Cap-and-Trade Program", with Kosobud, Richard F., Houston H. Stokes, Carol D. Tallarico, book under contract, Routledge Publishing

PRESENTATIONS

"The Limiting Effects of Banking Regulations On Pollution Permit Markets: An Experimental Analysis", revised, Midwest Economics Association, March 2005

"The Chicago VOC Trading System: The Consequences of Market Design for Performance", with R.F. Kosobud, H.H. Stokes, C.D. Tallarico, MIT Energy and Environmental Policy Workshop, Center for Energy and Environmental Policy Research, December 2004

"What Are The Limiting Effects of Banking Regulations On Pollution Permit Markets: A Simulated Analysis", Missouri Valley Economics Association, 41st Annual Meeting, October 2004

"Dual Environmental Regulation: The Puzzling Performance of the Chicago VOC Cap-and-Trade Market", with R.F. Kosobud, H.H. Stokes, C.D. Tallarico, Illinois Environmental Protection Agency, August 2004

"The Limiting Effects of Banking Regulations On Pollution Permit Markets: An Experimental Analysis", Midwest Economics Association, Annual Meeting, March 2004

"What Are The Limiting Effects of Banking Regulations On Pollution Permit Markets: An Experimental Analysis", revised, Missouri Valley Economics Association, 40th Annual Meeting, February 2004

"What Are The Limiting Effects of Banking Regulations On Pollution Permit Markets: An Experimental Analysis", University of Illinois Economic Seminar Series, October 2003

"Banking of Tradable Credits for emissions Regulation: Results from the Market and Experimental Design to Reduce Market Failure," with C.D. Tallarico, Illinois Economics Association, UIC Chicago, Illinois, October 2003

"Experimental Economics at George Mason University: The Vernon Smith Experience", University of Illinois Economic Seminar Series, October 2002

TEACHING EXPERIENCE

University of Illinois, Chicago
Visiting Lecturer
Microeconomics for Business Decisions (MBA), Summer 2004*
Environmental Economics, Spring 2003*
Intermediate Microeconomics, Spring 2003*, Fall 2002*, Spring 2002*
Introduction to Microeconomics, Summer 2002*
Introduction to Macroeconomics, Fall 2001*

*Letter of commendation from the Dean of the College of Business Administration for teaching excellence

Teaching assistant
Microeconomics for Business Decisions (MBA), Fall 2001
Principles of Economics for Business, Fall 2000 – Spring 2001

Loyola University, Chicago

Visiting Lecturer
Economic Principles One (Microeconomics), Spring 2003*, Fall 2004

*Highest ranking student evaluations for economics instructors and economics courses, fourth highest ranking for all instructors and courses in the School of Business Administration

FELLOWSHIPS AND AWARDS

Institute for Environmental Science and Policy Ph.D. Fellowship Renewal, 2004 – 2005
$14,000
Sylvia Saffrin Memorial Award – 2004
$1,000
Provost Award for Graduate Research – Spring 2004
$2,000
FMC Graduate Fellowship recipient, 2003 – 2004
$9,000
Institute for Environmental Science and Policy Ph.D. Fellowship, 2003 – 2004
$14,000
Oscar Miller Teaching Award, 2002 – 2003
$500

CONFERENCES AND SYMPOSIUMS

Session Chair, "Environmental Economics I", Midwest Economics Association, March 2005

Discussant, "Contract Completeness, Product Complexity, and Regulation in the Market for Coal: Evidence from Coal-fired Power Plants", Maria Kozhevnikova, Midwest Economics Association, March 2005

Discussant, "Private Firm, Public Agency and R&D Spillovers", Li Wang, Midwest Economics Association, March 2004

Emissions Marketing Association, 7th Annual Fall Meeting, September 2003

UIC Environmental Symposium, at the Institute for Environmental Science and Policy, April 2003, April 2004

MEMBERSHIPS

Association of Environmental and Resource Economists
American Economics Association
Midwest Economics Association
Missouri Valley Economics Association

POINTS OF SCHOLASTIC INTEREST

Attended Visiting Graduate Student Workshop in Experimental Economics, conducted by the International Foundation for Research in Experimental Economics (IFREE), hosted by Prof. Vernon Smith, Summer 2002
Organized semester-long graduate seminar in applied game theory for economics, Spring 2002
Organized summer study program for comprehensive examinations, Summer 2001
Completed Economics 441 "Teaching Economics", Spring 2001
Phi Beta Lambda, Business Club
Second Place, State Finance Competition, 1993
First Place, State Economics Competition, 1992

WORK HISTORY

The Northern Trust, Chicago, Illinois
Trust Officer - Business Process Analyst, April 1999 - August 2000
Led and supported projects related to electronic securities movement. Project leadership roles included forming project goal, estimating project feasibility, gathering business requirements, scheduling up to two years out, coordinating with partners in up to 7 different departments, conducting system and user acceptance testing, managing installation, and providing post installation support.
Securities Technician, February 1998 - March 1999
Technician on the depository trust company (DTC) equity buy desk and the federal bond sell desk. Researched failing trades and, if necessary, expedited issues to relationship managers. Conducted daily desk reconciliation and balancing.

Mid City Bank, Omaha, Nebraska
Branch Manager, January 1995-October 1997
Managed branch for $170 million asset bank. Solicited new commercial business and conducted cross marketing to current commercial and retail customers. Instructed personnel in account maintenance, financial product sales and operational aspects of their jobs.