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Salinity in Water Markets: An Experimental Investigation of the Sunraysia Salinity Levy, Victoria

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Abstract

Irrigation can have a significant negative impact on the environment. Irrigation impacts contribute a significant portion to the estimated forty six million dollar cost per annum of salinity in the Murray River, Australia. Policies available to regulators include externality taxes and levies. In 2002 the Victorian Government introduced a system of salinity levies in the irrigation regions of Sunraysia, northern Victoria. These levies differ from typical taxes because they also introduce trade barriers between certain locations. These trade barriers may increase the cost of reducing salinity. We use experiments to compare the *salinity levy with trade barriers* to an alternative *salinity tax* which removes the trade barriers and replaces them with a 'large' tax relative to other geographic zones. Our results show that the salinity tax reduces the cost of salinity interception by the government by twenty five percent as compared to the salinity levy. We observe water prices do not increase when regulation is introduced, this may be because the introduction of taxes and levies encourages buyers to act more aggressively preventing sellers from extracting surplus on trades. Further, the introduction of regulation does not increase variability in average outcomes for these markets.

JEL Classification: Q25, Q28, C90.
Keywords: Externality Taxes, Experiments, Double Auction, Environmental Policy, Salinity.

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I. Introduction

The export of pollutants from non-point sources is a significant problem in watersheds internationally. The United States Environmental Protection Agency reports that agricultural non-point sources impact 48% of impaired rivers nationally (EPA, 2000).[1] In the Murray Darling Basin in Australia, irrigation practices contribute a significant portion to the estimated $46 million per annum cost of salinity (MDBC, 1999). Regulatory policy mechanisms have often excluded non-point sources because identification is impossible or prohibitively expensive.[2] Recent advances in hydrology modelling have however improved knowledge about the cause and effects between production and water quality, making it possible to estimate the relationships between production practices and external environmental impacts. This allows regulators to use point-source policies in some non-point source situations.[3] The Sunraysia region of the Murray Darling Basin is one such example. Salinity impact zoning commissioned by the Victorian Government, divides the river into five separate zones such that irrigation contributions to salinity concentrations at the end of system monitoring point can be estimated based on location and water use volume (SKM, 2001).[4] Table 1 reports the salinity impact zones and salinity concentration impacts.

Once the regulators obtain information about the impact of irrigation practices of different sources, on salinity, they can consider different regulatory policies to reduce their impact. Market based instruments, which include taxes/levies, are one type of policy mechanism available to regulators. These mechanisms equalise private

[1] Impaired rivers are those in which the water quality is too poor to support the designated beneficial uses. Designated uses include, drinking water, swimming or the survival of aquatic species. Further information can be found at www.epa.gov

[2] Beavis and Walker, 1983; Oates, 1995; and Shortle and Horan, 2001, discuss the difficulty of attributing control responsibility to non-point source emissions.

[3] Shortle and Horan, 2001 survey this literature and provide a good discussion about the relationship between production practices and their environmental impact.

marginal control costs across different polluters. Hence economic instruments such as taxes can allocate control responsibility at least cost (Tietenberg, 2001).[5] The Victorian Government, in April 2002, introduced a new system of salt impact levies in the Sunraysia region in the Victorian Mallee (SRWA, 2002). This region is a collection of irrigation districts and private irrigators that are located along the Murray River from Nyah to the South Australian border. Water trade in the region is very active with approximately 46000 net megalitres of permanent water licences traded into the region over the last decade (DNRE, 2001).[6] While the increase in water use has supported growth in the region, it also increases salt concentrations in the river.[7]

In this policy only buyers pay the levy; sellers of water incur no levy on sales. Furthermore, the levy is only imposed upon buyers who buy water from lower impact zones. In other words this policy creates a disincentive for water trades that increase salinity concentrations but no incentive for water trades that decrease salinity concentrations. Irrigators located in the 'High Impact Zone' (HIZ) can only buy water from sellers also located in the HIZ. Irrigators located in the ‘Low Impact Zones’ (LIZ) 1 to 4 can purchase water from sellers in any impact zone but must pay a salt levy per unit of water traded if they buy water from a lower impact zone (SRWA, 2002). Table 2 reports the salinity levies as implemented in the Sunraysia region.

The levy imposed in this region differs from standard externality taxes that are discussed in theory. Firstly, the tax is on the input (water) and not on the externality

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[4] Within each zone the interactions between irrigation and salt concentrations in the river are homogenous, and each property is located in one zone (only) for the purposes of the levy (SRWA, 2002).

[5] Tradable pollution permits is an alternative instrument using economic incentives to regulate the environment.

[6] A permanent water right transfers the freehold property right to the owner. Temporary water rights, transfer a lease for the water for a pre-determined period (usually one irrigation season). Temporary trade accounts for approximately 3 to 8 percent of total water licences held in the region (DNRE, 2001). Permanent water rights have been used in this experiment as salinity can take up to 30years to manifest itself in the river. Permanent rights therefore seem the most appropriate right to model as they are held for longer periods of time.

[7] Excess water can enter the groundwater system via vertical drainage; depending on soil type, gradient and distance from the river, salt contained in the soils and groundwater is moved towards the river.

(salt).[8] Secondly, the policy does not allow for free trade of water across impact zones. For example, no water can be traded into the HIZ. Economic theory would suggest the use of a higher tax rate, equal to the magnitude of the externality between the HIZ and LIZ 1 to 4, as opposed to a trade barrier. The trade barrier may prevent the use of water in high valued industries located within the HIZ, thereby reducing economic profits in the region.

In this paper we evaluate the Sunraysia Salinity Levy using experiments and compare the salinity levy to an alternative salinity tax and to a baseline no regulation policy. The salinity tax more closely approximates economic theory as it removes the water trade barrier between the HIZ and LIZ 1 to 4, and replaces it with a 'large' tax. Table 2 reports the salinity tax as used in this testbed. Given that policy makers cannot extrapolate directly from theory, empirical analysis of policy performance is important. Further, investigating the performance of the Sunraysia Levy is increasingly important as other regulators in other regions are beginning to consider the implementation of similar schemes (PIRSA, 2004).

Our results show that both the salinity levy and salinity tax lead to significant environmental improvement as compared to the no regulation case. Both of these policies exhibit high performance levels with prices not higher or more volatile due to regulation. Comparing the salinity tax and levy, we find that the salinity tax policy leads to lower water prices and more trades in the water market in the early periods. The salinity tax in addition reduces the cost of salinity control for the regulator and also has a superior environmental outcome as compared to the salinity levy.

I.I. Policy Testbedding

[8] Freebairn 2000 discusses the issue of targeting inputs that generate the pollution externality or targeting the pollution externality directly.

Laboratory experiments can be a powerful and low cost approach for policy evaluation. Experiments particularly in the area of environmental economics are very useful for 'whispering in the ears of princes' (Roth 2002, Cummings, McKee and Taylor 2001).[9] Experiments use special cases to inform more complex field situations and this methodology is called 'experimental testbedding' (Plott 1994).[10] Policy decisions must often be made with incomplete information. Policy design therefore is a process exposed to significant financial and political costs.[11] Data for the Sunraysia region are few in number as this is a recent policy initiative (operating since April 2002). In addition, there is no centralised database for the trades, so data are not easily available.[12] This therefore precludes econometric analysis of policy performance using actual field data. However, it is possible to implement 'testbed' laboratory experiments which can test the performance of the main features of the policy in a controlled and replicable environment.

The policy testbed is implemented using a double auction institution. In the double auction all bids, offers and transaction prices are public information. The double auction was chosen, as the electronic water trade market in the field called *Watermove*[13], resembles most closely the double auction. The double auction

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[9] Cummings et. al. 2001 draw on Roth's 1986 description of how experimental economics offers the possibility of using scientific methods to advise policy makers about practical questions whose answers lie beyond the reliable knowledge of the profession.

[10] Testbeds can be used to compare the performance of different policies, for example, Plott 1983 used experiments to examine the performance of tradable emissions permits and taxes. They are useful to develop policy where the institution upon which the policy is designed may not be well specified, for example, Cason, Gangadharan and Duke 2003a. investigate the information structure of procurement auctions for non-point source emissions. Also, in situations where the environment does not conform to theory, for example, Cason, Gangadharan and Duke 2003b. investigate the performance of tradable emissions permits when there is one large player in the market, and where field data is not available for empirical analysis as is the case reported in this paper.

[11] For example, in 2000 the British 3G package bid spectrum auction raised revenue of $US34 billion (Binmore and Klemperer, 2002). In contrast the New Zealand independent bid auction, conducted in 1990, failed to account for substitutability between licences resulting in inefficient allocation of licences across bidders and prices paid were often much less than the winning bidder's highest bid (Milgrom, 2004). In one case the highest bid was NZ$7 million and the second bid was NZ$5,000. The realised revenue was NZ$36 million much less than the expected NZ$250 million (McMillan, 1994).

[12] In the future *Watermove* will create a database on trades and report historical data on electronic water trades (www.watermove.com.au). Data on water trades is only available from individual brokers, who would have information about the trades that they oversee, not the total trades in the market.

[13] www.watermove.com.au

institution performs efficiently (it converges to the theoretical competitive equilibrium) even with a small number of traders who know nothing about the market conditions ex-ante (Smith 1962 and 1964). The strong performance of the double auction is generally attributed to the sequential nature of bids. This sequential property means sellers offer high and decrease their bids and buyers bid low and increase their bids. This overall improvement rule means the highest marginal value and lowest marginal cost units should be traded first (and so on forming the demand and supply array). This forces price formation towards the value of marginal units (Plott and Smith 1978).[14]

The continuous improvement rule accommodates situations where bidders are indifferent with whom they trade. In some markets, however, subjects may have different quality characteristics such that agents prefer to trade with one particular agent over another. These quality characteristics may arise, for example, in markets that involve trust or markets where social factors may be important. The salinity levy is similar to a market with different qualities. Sellers of irrigation water have different quality attributes from the point of view of buyers: buyers will prefer to buy from sellers with lower levies/taxes (higher quality from the buyers' perspective). Therefore, a buyer may be willing to purchase water from a seller with a higher marginal cost but a lower levy/tax than a seller with a lower marginal cost but higher levy/tax. Given this heterogeneity the overall continuous improvement rule of the standard double auction may result in some gains from trade not being realised. In this paper therefore, we relax this rule as explained in the following section and the resulting trading institution can be thought of as a modified double auction. We examine the performance of this institution and compare with other double auction experiments in the literature.

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[14] Holt.C., In Kagel and Roth eds. 1995, provides an excellent discussion of the double auction trading

II. Experimental Design

II.I. Environment and Procedures

As described above, the differential taxes in the field imply that sellers are heterogenous; buyers are not indifferent as to which seller they trade with. In the experiments we therefore modify the standard double auction by removing the overall improvement rule on the seller side and instead introduce an individual improvement rule. Namely, a new offer to sell must be lower than the individual seller's previous offer but does not need be lower than the best current offer to sell. Sellers in effect become different markets, and their offers to sell including any levy or tax are displayed separately. Buyers continue to face the overall improvement rule. This design attribute should allow buyers to trade with sellers whose quality they most prefer.

The levies and taxes are public information and are known at the beginning of each trading period. This creates a situation where buyers have more information about sellers' relative costs than sellers do about buyers: Buyers know the relative tax competitiveness of each seller matched to each buyer (see Table 2). Therefore, buyers know the minimum price wedge – the tax – each seller must allow for in their bids, to remain competitive against other sellers. This could create a situation similar to that investigated by Cason and Plott, 2004, where one side of the market is forced to disclose more information than the other. Cason and Plott suggest that asymmetric information disclosure worsens market performance and harms the discloser. Further, the introduction of the tax and levy reduces some buyers' possible surplus as compared to the no regulation case. The loss in possible surplus may harden these buyers' bargaining as they try to secure a portion of the reduced surplus (Smith 1962 and Smith and Williams 1982 and 1989).

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institution.

As the aim of this research is to specifically test the performance of an operating field policy we employ parameters that correspond to field conditions. Subjects in the experiment represent irrigators in the Sunraysia region. The allocation of subjects across different industries, irrigation technologies and locations approximates that in the field as reported by SunRise 21, 2004 and discussed with regional government officers. Cost and value ranges are taken from the Victorian and New South Wales Departments of Primary Industries' reported gross margins. The exact costs and values for each industry were drawn randomly from a uniform distribution with the indicated ranges. Subjects therefore had different cost and value parameters to represent the heterogeneity between different industries (due to different production systems) and within the same industry (due to different skill levels and irrigation technology). Table 3 shows the cost and value ranges, the distribution of land-use and the allocation of experimental subjects. Cost and value ranges remained private information throughout all sessions.

Experimental subjects were undergraduate students from the University of Melbourne. Ten subjects participated in each session, randomly assigned as 5 sellers and 5 buyers. We report 11 sessions each with 14 trading periods in which subjects participated in a computerised modified double auction. Two training periods were conducted to verify subjects' understanding of the instructions, computer software and accounting rules; the training periods were not included in subjects' final earnings. Subjects were read aloud the instructions as shown in the appendix. Each market period was open for 5 minutes, during which subjects were free to make or accept offers to buy and sell units. Subjects could only sell or buy one unit at a time, and could buy or sell up to three units in a trading period.[15] Neutral terminology was used throughout the instructions and sessions, as is the common practice in experimental economics.[16] The computerised auction was implemented using web-based software designed and developed at the University of Melbourne. The trading screens are reproduced in the appendix. Subjects recorded their end of period profits on record sheets as shown in the appendix, and their end of period earnings were also shown on their computer screens at the end of each period. Subjects were paid their earnings in cash at the end of the session, and earnings ranged between $20 - $30 Australian dollars. Sessions lasted between one and a half to two hours.

II.II. Treatments

Table 4 summarises the experimental design. Table 5 presents the price, transaction quantity, surplus and salt impact predictions for the water market (treatment 1), salinity tax (treatment 2) and salinity levy (treatment 3). We conducted three sessions for treatment 1 to measure market performance when subjects are free to trade water with no salinity regulation. We implemented treatment 1 as a policy comparison to treatments 2 and 3; but, as described above, we are also interested in how the double auction institution performs when the improvement rule is relaxed from the seller's side. As is well known, the standard double auction yields high efficiencies (Smith 1962).[17] While in this paper we do not implement a standard double auction treatment, we do attempt to provide some preliminary information on the effect of relaxing the overall improvement rule by comparing the efficiency estimates found in (our) treatment 1 to those reported by other researchers.

In treatment 2, we conducted four salinity tax sessions in which the water trade barrier is removed and replaced with a 'large' tax. This tax is equal to the relative

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[15] If a seller did not sell a unit then he did not incur the cost of the unit.

[16] Subjects traded fictitious units not water. The term tax was however used in the experiment.

[17] For example, Smith and Williams 1981 observed efficiencies of 99% with experienced subjects while with inexperienced subjects efficiency was 95% (Smith and Williams 1982). These findings have been replicated in many studies and across different environments, using conditions of shifting supply (Davis, Harrison and Williams, 1993), market power (Smith, 1981, Smith and Williams, 1989) and multiple unit trades (Plott and Gray, 1990).

difference in salt cost across impact zones.[18] We were interested in how this policy, which more closely approximates the theoretical externality tax, would operate relative to the field policy. In treatment 3, we conducted four sessions with salinity levy and the trade barrier as per the field policy.

III. Data analysis and Results

We focus on the impact of the salinity levy (treatment 3) and the alternative salinity tax (treatment 2) – as compared to the water market (treatment 1) – on four market outcomes: prices, quantities, surpluses and salinity concentrations. In Section III.III, we examine the individual offer behaviour of sellers and buyers to investigate further the difference in price across the three treatments.

III.I. Overview of Results

We start the discussion of results by examining the figures that summarise the data for these outcomes. Figure 2 presents average transaction prices across all sessions for the three treatments. We observe average transaction prices for treatment 1 (no regulation) are higher than those for treatments 2 and 3 (regulation). In all periods, except for period 4, average transaction price for the salinity levy is higher than for the salinity tax. Transaction prices for all treatments lie within the predicted price range for treatment 1. Convergence occurs when outcomes are closer to the competitive equilibrium predictions in the late periods than in the early periods. Using the convergence measure introduced in Noussair, Plott and Riezman 2003, |Average outcome in the late periods – predicted outcome | < |Average outcome in the early periods – predicted outcome|, we find that transaction prices in treatment 1 (not

[18] From Table 2, we can observe that the difference in the cost of trading a unit of water between LIZ 1 and 2, and LIZ 2 and 3, is $26. Further, the difference in the cost of trading a unit of water between LIZ 2 and 3, and LIZ 3 and 4, is $65. Therefore the relative difference in cost is 2.5 (65/26 = 2.5). If we extend this to trades of water between LIZ 3 and 4, and LIZ 4 and 5, we find the tax between LIZ 4 and 5 should be equal to 162 (162/65 = 2.5).

in treatments 2 and 3) converge to the equilibrium prediction.[19] Figure 3 shows average transaction quantity for water across all sessions for the three treatments. We can observe transaction quantity increasing in treatments 2 and 3 from approximately 6 units in period 2, to 10 units in period 11. Treatment 1, however, does not visually exhibit this upward trend with quantity traded remaining between 8 and 9 units except for period 9 when it is 7 units.

Figure 4 shows the average change in salt concentration at the monitoring point across all sessions for the three treatments. The change in salinity concentration is measured as a reduction (improvement) in electro-conductivity (EC) at the monitoring point. Improvement in concentration appears to be greater for treatments 2 and 3 as compared to treatment 1. Further, observed improvement in EC is greater for the salinity tax (treatment 2) as compared to the salinity levy (treatment 3) except for the early periods 1 to 3 and period 12.

Figures 5 and 6 show average buyer and seller surplus respectively. Figure 7 shows total economic surplus. In all treatments buyer surplus appears to increase towards the prediction of $91.875 for treatment 1 and $93.875 for treatments 2 and 3.[20] In early periods 1 to 7, buyers achieve on average 54% of the predicted surplus in treatment 1, 67% in treatment 2, and 60% in treatment 3. In later periods 8 to 14, buyers in treatment 1 achieve 72% of possible surplus and in treatments 2 and 3 they achieve 73%. Seller surplus appears to be higher than the prediction of $51.50 for treatment 1, and $50.88 in treatments 2 and 3. In early periods sellers achieve on average 140% of available surplus in treatment 1, 107% in treatment 2 and 120% in

[19] Early periods are defined as periods 1-7 and late periods as 8-14. Prices have a big equilibrium range and that might explain why we do not observe convergence using this measure in treatments 2 and 3. We take the middle point of the range as the predicted price.

[20] Predicted buyer surplus increases when the tax and levy are introduced as compared to the no regulation baseline. Buyer 3, however, located in the high impact zone suffers a loss in surplus when the tax and levy are introduced. The increase in treatments 2 and 3 arises as buyer 1 is able to trade an additional unit as compared to treatment 1, and the midpoint of the equilibrium range in treatments 2 and 3 is lower as compared to treatment 1 because the equilibrium range widens as the tax and levy

treatment 3. In later periods sellers achieve 120% in treatment 1, 112% in treatment 2 and 130% in treatment 3. Total surplus exhibits an upward trend towards the prediction for all treatments. In early periods total surplus is 84% of possible surplus for treatment 1, 82% for treatment 2 and 87% for treatment 3. In later periods subjects achieve 91% of total surplus in treatment 1, and 87% and 93% in treatments 2 and 3 respectively.

The efficiency outcome for treatment 1 (91% in later periods) is reasonably consistent with the efficiency levels found in standard double auction experiments conducted by other researchers, in which efficiency generally lies between 95% and 99%. While we need to look closer at individual offer behaviour (Section III.III), we can tentatively conclude that the double auction is reasonably efficient when the overall bid improvement rule is removed from one side of the market. The differences in total surplus between treatment 1 and 3, could be influenced by subjects' experience (Smith and Williams 1982).[21] Treatments 2 and 3 used subjects experienced in this trading institution as the environment was more difficult to understand. Experience, however, did not lead to higher surpluses in treatment 2 as compared to treatment 1. The field parameters used in the paper may also explain this slightly lower surplus in treatment 2 as compared to treatments 1 and 3. Namely, that efficiency can be influenced by the structure of the demand and supply schedules (Plott 1983): In this experiment the extra-marginal units are close to the predicted equilibrium - 12 experimental dollars on the demand side and 14 experimental dollars on the supply side (see Figure 1). The two extra-marginal traders, seller 5 and buyer 4, can only sell one unit profitably in equilibrium. Any variability in price will give these subjects a chance to trade their second unit, and these trades reduce efficiency. This

force a reorganising of buyer and seller matches, allowing some lower value buyers to trade with lower cost sellers as compared to treatment 1.

[21] Smith and Williams find convergence to the theoretical equilibrium is more rapid with experienced subjects.

could also partly explain the slightly lower efficiency observed in our modified double auction as compared to the standard double auction. In all three treatments however we observe convergence towards the predicted efficiency level using the convergence measure described above (Noussair, Plott and Reizman 2003).

III.II Econometric Tests

To examine if these market outcomes or performance indicators are statistically different between treatments, we conduct parametric t-tests and non parametric Wilcoxon ranksum tests. These tests presented in Table 7, use one observation per session hence providing powerful and statistically independent comparisons between treatments. Average transaction prices are seen to be significantly lower in treatment 2 as compared to treatment 1. This finding supports the visual observation from Figure 2. Prices in treatment 3 are lower than in treatment 1, however the difference is not significant. Typically transaction prices would be expected to increase in treatments with regulation. We gain some additional insights into this result when we explore the offer behavior of sellers and buyers in section III.III. Average quantity is not significantly different across treatments 1 and 2 using a t-test or the Wilcoxon test. Average quantity is however significantly lower in treatment 3 as compared to treatments 2 and 1.

Average surplus is not significantly different across treatments. The environmental improvement is significantly higher in treatments 2 and 3 as compared to treatment 1 due to the introduction of a tax or a levy to reduce the salt impact. Comparing treatments 2 and 3, the point estimates show that the environmental improvement is higher in treatment 2 than in 3 though the difference is not statistically significant.

To examine if the outcomes have different volatility across treatments we present variance ratio tests in Table 8, which determine whether the standard deviation of prices, quantities and the environmental impact are different across treatments. We find that there are no significant treatment differences in prices, quantity traded and the environmental impact. The hypotheses that the variability in these outcomes are equal in the three treatments cannot be rejected. If these standard deviations are used to measure stability in these markets, then we would conclude that there is no difference in terms of stability and volatility in any of the three treatments. Hence introducing regulations (as in treatments 2 and 3) does not lead to more price or quantity dispersion.

The graphs and the pairwise comparisons provide valuable information about the behaviour of subjects in these three treatments. To explore further these general impressions we conduct parametric regression analysis to determine whether the conclusions are robust when controlling for other factors like experience (captured by the time periods that the subjects participate in the market) and session effects.

Table 9 presents results from random effects estimation for price, averaged for the different sessions in each treatment. In the random effects models, the session represents the random effect to account for the correlation of market outcomes within a session. We present results from 11 sessions and 14 time periods in each session (11*14 =154 data points).

The equation we estimate is the following:

$$Y_{it} = \text{constant} + \beta_1 t + \beta_2 D_2 + \beta_3 D_3 + e_{it} \qquad \text{(Model 1)}$$

Where Y indicates the outcome that we want to examine (transaction prices, quantity traded, surplus from trading, environmental improvement), i the particular treatment (1, 2 or 3) and t represents time as measured by the number of market periods in the treatment (1 to 14), therefore coefficient β_1 captures the impact of time on average outcomes. D_2 and D_3 are dummy variables for treatments 2 and 3 respectively (D_2 takes the value of 1 when the treatment being considered is treatment 2 and takes the value of 0 otherwise and similarly D_3 takes the value of 1 for data relating to treatment 3). The coefficients on the dummy variables D_2 and D_3, capture the impact of treatment 2 and treatment 3 respectively, relative to the baseline treatment 1.

Model 1 in Table 9 presents results when the dependent variable is average price. The estimates show that the prices in treatment 2 are significantly lower than in the baseline treatment, treatment 1. However the prices in treatment 3 are not significantly different from treatment 1. This is consistent with the results obtained from the pairwise tests. The magnitude of the coefficient for treatment 2 is double the coefficient for treatment 3 indicating that prices are lower by a larger amount in treatment 2 than in 3. In Model 1a in this table we present results from an equation where we include the environmental impact as an explanatory variable in the price equation. We include salinity as an explanatory variable to explore if prices for water increase as environmental (salinity) quality improves. Our results show that the environmental improvement has no statistically significant impact on prices. This could imply that implementing policies that would improve the environmental quality by reducing salinity concentrations would not lead to significantly higher prices. Model 2 explicitly analyses the effect of time on the outcomes by separating out the early period and the later period effects in the three treatments. To estimate convergence levels we use the framework described below and estimate models with a random effect error structure where session is the random effect:

$$Y_{it} = \text{constant} + \beta_1(1/t) + \beta_2 D_2(1/t) + \beta_3 D_2((t-1)/t) + \beta_4 D_3(1/t) + \beta_5 D_3((t-1)/t) + e_{it}$$

(Model 2)

The variable $1/t$ captures short run effects and the variable $(t-1)/t$ captures the long run patterns in the data. When t=1, ie in the first period, the coefficients of D_2 (β_2) and D_3 (β_4) capture initial price in that treatment. As t gets large the weight of the coefficient of $D_i 1/t$ becomes small because $1/t$ approaches zero. Meanwhile the weight of the coefficient of $D_i(t-1)/t$ becomes large as t becomes large because $(t-1)/t$ approaches 1, hence β_3 and β_5 capture the long run tendencies in the data. The coefficient β_1 indicates the impact of treatment 1 (the baseline treatment) in the short run and the constant term is the long run indicator for the baseline treatment.[22]

Model 2 in Table 9 compares both the short term and long term price differences in the three treatments. The estimates from this regression show that the transaction prices in treatments 2 and 3 are significantly lower than in treatment 1 in the short term (as shown by the negative and significant coefficient on ($D_2$1/t) and ($D_3$1/t)). In the long term prices in the two treatments are not significantly different than in treatment 1. The indicators for the baseline treatment show that prices are higher in both the early periods and the late periods (the coefficient of the 1/t variable and the constant term which is an indicator of the long run impact of treatment 1, are both positive and significant) and that prices fall over time in this treatment (the constant 994.3, which is the long run impact is less than 994.3 plus 37.77 = 1032.1, which is the short run impact).

Table 10 reports the results from models where the dependent variable is average quantity traded. Model 1 shows that the coefficient for treatment 3 is negative and significant, implying that the quantity traded is significantly lower in treatment 3 as compared to treatment 1. Model 2 indicates that this reduction in quantity traded is mainly due to the lower number of trades in the early period as treatment 3 exhibits different convergence rates in the early periods. The average quantity traded is

[22] We estimated models with alternative specifications suggested by Noussair et al. (1995) and

significantly lower (1.51 units lower) than in treatment 1 in the early periods, as captured by the coefficient for the interaction variable $D_3 1/t$. In the later periods however the quantity traded is not significantly different in treatment 3 as compared to treatment 1.

Table 11 reports results from random effects models on economic surplus or earnings of subjects in the experiment. In Model 1, the coefficient of period (t) is positive and significant indicating that the total surplus increases over time in all three treatments. Surplus in treatments 2 and 3 are not significantly different than in treatment 1. The convergence model (Model 2) also shows that the surplus is not different in the short or long run in treatments 2 and 3 as compared to treatment 1. In treatment 1, the surplus is higher in the later periods as compared to the earlier periods (137.82 exceeds 137.82-84.38 = 53.44).

Tables 12 and 13 separate out the total surplus into buyer surplus and seller surplus. In Model 1 the variable *period* is positive and significant, implying that buyer surplus increases over time. In both treatments 2 and 3 the buyer surplus is not significantly different than in treatment 1. The convergence model (Model 2) also shows that the buyer surplus is not different across treatments either in the short run or in the long run. Buyer surplus increases over time in treatment 1 as evidenced by the positive and significant sign on the constant term and the negative sign on the coefficient of *1/t*. Models for seller surplus are reported in Table 13. Model 1 indicates that seller surplus is significantly lower in treatment 2 as compared to treatment 1. The convergence model (Model 2) suggests that this is mainly driven by the low seller surplus in the early periods in treatment 2 as compared to treatment 1. The point estimates also suggest that the seller surplus is lower in the early periods in treatment 3 as compared to treatment 1 however the magnitude of the reduction in

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Ashenfelter et. al. (1992) and find that the results are exactly the same.

surplus is a lot higher for treatment 2 (in addition, the coefficient for *$D_3 1/t$* is not significant). Seller surplus is not significantly different in the later periods for treatments 2 and 3. Examining the coefficient of *$1/t$* and the constant term shows that the seller surplus is falling over time in treatment 1 (60.4 in later periods and 60.4 + 51.7= 112.1 in the early periods).

The results for the seller and buyer surplus indicate that the seller surplus is higher and buyer surplus lower in treatment 1 (at least in the early periods) when we compare across treatments 2 and 1. In treatment 2, buyers have to pay a tax and information about this explicit tax perhaps makes them unwilling to pay a higher price in the water market. This is because buyers perceive a reduction in possible surplus in treatment 2, and this reduction probably hardens the bargaining of buyers. Though point estimates indicate that this effect exists in treatment 3 as well, the difference between treatments 3 and 1 is not statistically significant. This is perhaps because in treatment 3, Buyer 3 can only buy from Seller 2 and therefore cannot bargain with other sellers to lower price.

Table 14 presents results from random effect regressions where we examine whether the environmental impact (reduction in salinity concentration) is different across treatments. Model 1 shows that the average environmental impact is significantly higher in both treatments 2 and 3 and that makes sense as treatment 1 does not have any regulation on the level of salt generated by the water trades. The convergence model (Model 2) suggests that the average environmental impact is higher in the initial periods in treatment 3 as compared to treatment 1 (coefficient of $D_3 1/t$). Environmental improvement however increases in treatment 2 as compared to treatment 1 in the later periods (coefficient of *$D_2 t\text{-}1/t$*) and the environmental improvement in the later periods is greater in treatment 2 as compared to treatment 3.

III.III Offer Behaviour

In this section, we explore the strategies employed by the individual sellers and buyers by examining the prices at which they offer to sell or buy. Some of these offers would be accepted and would form the transaction prices in the market, while others would not be accepted. Analysing individual strategies could help us understand if subjects exhibit behavioural differences in the three treatments.

We examine individual offer behaviour for all periods and for the last five periods.[23] Table 15 presents the average buy and sell offer price for each treatment. Average buy offers are lower in treatments 2 and 3 as compared to treatment 1 when averaged across all periods. Average buy offers are 26 experimental dollars lower in treatment 2, and 4 experimental dollars lower in treatment 3 as compared to treatment 1. Across the last five periods, average buy offers in treatment 2 are 14 experimental dollars lower but in treatment 3 average buy offers are 6 experimental dollars higher as compared to treatment 1. These averages provide some preliminary support for the hypothesis that buyers are acting more competitively in the tax treatment.

Table 16 presents results from both ordinary least squares and random effects models (with buyer representing the random effects) for buy offers for all periods and the last five periods. Buyer offers are a function of value of the unit, time period and the treatments. Buy offers are increasing over time as shown by the positive and significant period estimate for the all periods data set and falling, although not significantly so for the last five periods. Buy offers are significantly higher the higher the buyer's value for a unit, and this makes sense as higher marginal value buyers are willing to pay higher prices under the rules of demand and supply. Buy offers are not significantly different between treatments 1 and 3. The point estimates indicate that

[23] By the end of the experiment subjects would have identified the strategies that are optimal for them, hence examining the data from the last five periods of the experiment, would help reduce the impact of subject error.

buy offers are lower in treatment 2 relative to treatment 1, but the estimates are not significant (except in the OLS regression for treatment 2 for all periods).

Table 17 presents the results for seller offers'. Seller offers are falling over time as seen by the significant negative estimate on the period variable for the all periods data set. In the last five periods the offers are not significantly different across time. As is expected sellers with higher cost units ask for significantly higher prices (cost is positive and significant). Treatment 2 dummy is positive and significant: sellers are asking for higher prices in treatment 2 as compared to treatment 1 (significantly higher according to the OLS regressions, but not according to the Random effects model).

To investigate if the trade barrier in treatment 3 is influencing offer behaviour, we explore the behaviour of Buyer 2, the buyer located in the highest impact zone, and Seller 3, the seller (also) located in the highest impact zone (see Table 2) between treatments 2 and 3. In treatment 3 both Buyer 2 and Seller 3 are located behind a water trade barrier. This means that Buyer 2 can only buy from Seller 3 (but Seller 3 can trade with all buyers). Seller 3 knows this (as do all other subjects) and therefore it is reasonable to expect that this monopoly supplier (to Buyer 2) will attempt to extract more surplus on trades with Buyer 2. In treatment 2 the trade barrier is removed and replaced with a large tax. Buyer 2 can then begin to act more competitively and attempt to extract more rent on each trade.

In summary the offer behaviour indicates that in treatment 2, sellers want to sell at higher prices and buyers are offering to buy at lower prices as compared to treatment 1. This is confirmed by the OLS results, but gets less support from the Random effects models. Buyers seem to be relatively more aggressive, hence traded prices are lower in treatment 2 than in treatment 1. In treatment 3, one of the buyers: Buyer 2 can only trade with Seller 3 and this prevents Buyer 2 from acting competitively. In treatment 2

Seller 3 is no longer protected by the trade barrier and Buyer 2 can now participate in bargaining with multiple sellers. Therefore traded prices are lower in treatment 2 as compared to 3. This difference in behaviour becomes much less when we examine the offer data from the last five periods and this is consistent with the results from the transaction price regressions (Table 9), where we see that the long term effects are not significant across treatments.

IV. Conclusion and Policy Observations

The specific design of policies can have an enormous impact on the performance of the regulatory policy being considered. The laboratory experiment reported here was implemented to specifically explore the performance of the Sunraysia Salinity Levy, as implemented in the irrigation regions located on the Victorian side of the Murray River between the town of Nyah and the South Australia Border. We compared the performance of this field policy to an alternative salinity tax. The salinity tax removed trade barriers used in the salinity levy, and replaced the barrier with a 'large' tax. We compared the performance of these policies to a no regulation water market baseline using a modified double auction. This modified design has similar levels of efficiency and the offer behaviour of subjects is consistent with other standard double auctions.

Salinity improvement, measured as a decrease in electro-conductivity (EC) at the Morgan monitoring point, is higher for the salinity tax than the salinity levy. Our estimates show (Model 1, Table 14) that overall environmental improvement is 0.0001 EC units greater under the salinity tax than the salinity levy when compared against no regulation. Discussions with regional water brokers indicate that the average size of each (permanent) water trade in the region is at least 100 megalitres. If each trade is at a minimum 100 megalitres, then the difference in EC improvement between the salinity tax and salinity levy is 0.01. The cost of extracting 1 EC of salt ranges between approximately $550,000 and $4,250,000 capitalised cost per EC unit extracted from the river (MDBC, 2003).[24] Creating new opportunities for salt interception locations, which could potentially reduce the salt concentration, are becoming increasingly scarce. Hence we can expect to observe costs in the higher end of the range mentioned above (Stakeholder workshop, Mildura, June 2002). If we assume the median of this range, which is $2,400,000, as a representative estimate of interception costs, then in this laboratory testbed the salinity levy saves society $48,000 ($2,400,000*0.02) and the salinity tax saves $72,000 ($2,400,000*0.03) as compared to no regulation. The long-run differences estimated in model 2, which account for subject experience across periods, are 0.04 for the salinity tax and 0.01 for the levy.[25] Therefore, in the long-run the salinity tax saves society $96,000 and the salinity levy $24,000 as compared to no regulation. From this we can imply that, given the parameters used in this testbed the salinity tax provides a 4:1 benefit to society, or only a quarter of the public funds required to intercept salinity under the levy are required under the tax.

While both policies improve environmental outcomes as compared to the no regulation case, it was unexpected to observe transaction price for water higher in the no regulation as compared to one of the regulation treatments. Taxes (and levies) generally increase the cost of goods (in this case water) by introducing a per unit cost on each unit purchased. Therefore, we would expect water prices to rise in the salinity levy and salinity tax treatments as compared to the water market only. Instead we observe (Model 1, Table 9) that average transaction price for water is lower in the tax treatment as compared to no regulation treatment. The price in the salinity levy treatment is also lower but the difference is not statistically significant. This

[24] The costs are discounted over thirty years with a 4% discount rate. They include construction of engineering works and operation and maintenance costs.
[25] Double auctions generally perform better (equilibrate faster and closer to the expected equilibrium) in the later periods. This is called the sawtooth effect (Plott, 2004).

difference relative to no regulation treatment is maintained in the short term for both treatments but not in the long term (Model 2). An examination of individual offer behaviour helps us understand this better. When buyers are faced with a tax they know the tax will reduce their potential surplus (ie. the tax decreases demand). Sellers also know this, and both buyers and sellers know the relative (potential) loss of surplus for each buyer matched to each seller. When buyers face a decrease in potential surplus they may compete more aggressively to secure a larger portion of their smaller possible surplus (Smith 1962, Smith and Williams 1982). Sellers know buyers face taxes, and therefore expect buyers to attempt to push some of the tax burden across to sellers. Sellers will therefore (also) act more aggressively as they are faced with a potential decrease in surplus if buyers are successful in shifting some tax. This effect is stronger in treatment 2 as the trade barrier is removed therefore removing the 'protection' Seller 3 enjoyed in treatment 3 (where he was the sole supplier to Buyer 2) and allowing Buyer 2 to partake in the competitive behaviour.

The ability of buyers to tacitly force sellers to lower price when a levy or tax is introduced could (also) be linked to the information structure of this double auction institution: buyers have more information than sellers, as buyers know the tax/levy margin tagged to each seller. The taxes/levies are possibly some form of exogenously imposed information revelation. Namely, before each market trading period opens buyers know the tax/levy competitiveness of each seller with each buyer. Buyers know the minimum difference in price (the levy/tax) each seller must achieve if they want to trade with a given buyer. This is similar to the finding in Cason and Plott, 2004, that disclosure of market strategies by one side of the market hurts the disclosing parties.

For regulators considering the implementation of a salinity levy this testbed confirms that an economic incentive based regulation does improve the environmental outcome. However, using an instrument like the salinity levy discussed in this paper, which creates barriers to trade into certain geographic regions or salinity impact zones, may not be the most cost effective method. Our results suggest that it may be better to introduce a salinity tax without trade barriers as environmental outcome is higher thereby requiring fewer public resources to be allocated towards salinity interception.

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VI. Appendix

Table 1: Salinity impact zones and river salinity concentration impacts

Impact division	River impact (EC) per 1,000 ML of water traded into the zone
Low Impact Zone 1 (LIZ 1)	0.02
Low Impact Zone 2 (LIZ 2)	0.05
Low Impact Zone 3 (LIZ 3)	0.1
Low Impact Zone 4 (LIZ 4)	0.2
High Impact Zone (HIZ)	0.6

Source: SRWA, 2002. Note: The reported river impacts are long-term averages - 30 years, noting the overall prediction over 100 years (MDBC, 2003).

Table 2: Salinity levy and salinity tax

	Salinity Levy[a] (per megalitre)					Salinity tax[b] (per megalitre)				
Water traded from	Water traded to					Water traded to				
	HIZ	LIZ 4	LIZ 3	LIZ 2	LIZ 1	HIZ	LIZ 4	LIZ 3	LIZ 2	LIZ 1
HIZ	0	0	0	0	0	0	0	0	0	0
LIZ 4	Can't trade	0	0	0	0	292	0	0	0	0
LIZ 3	Can't trade	130	0	0	0	422	130	0	0	0
LIZ 2	Can't trade	195	65	0	0	487	195	65	0	0
LIZ 1	Can't trade	234	104	39	0	526	234	104	39	0

[a] The salinity levies are taken from the field. These levies are reported by SRWA, 2002 and DNRE, 2001. [b] The salinity tax has been calculated for the purposes of this experiment.

Table 3: Value parameters and land-use distribution for Sunraysia.

Industry[b]	Value of irrigation water for 5 types of irrigation technology ($/ha/yr)[a]	Distribution of land-use across salinity impact zones - % of total area (Number of experimental subjects)[c]				
		HIZ total area 8808ha	LIZ 4 total area 5736ha	LIZ 3 total area 905ha	LIZ 2 total area 3927ha	LIZ 1 total area 7231ha
Wine grapes	762 - 1566	32% 2 subjects	32% 1 subject	2%	6%	20% 1 subject
Table Grapes	621 - 978	10%	1%	1% 1 subject	14%	3%
Dried Grapes	878 - 1800	39%	10%	2%	23% 2 subjects	27% 1 subject
Orchard + Citrus	794 - 1478	7%	43% 1 subject	32% 1 subject	8%	17%

[a] Represents a range for each industry across different irrigation technologies. Technologies are flood, pressurised mix, other pressure, overhead and drip. [b] The table does not include all land-uses in the region. We decided not to include field crops and vegetables, as these industries are low value relative to other industries included in the experiment. If subjects represented these industries in the experiment then they could not profitably trade if on the buyers side and would be able to sell all units at very low price relative to all other sellers if on the seller side. Some land-uses were undefined, indicating surveyors do not know what is produced on this land. Vacant land was also not included in the experiment. [c] Experimental subjects are distributed across industries to approximate land-use in the region.

Table 4: Experimental Design

Treatment	Features	Number of sessions
Treatment 1 – Water Market	Free trade of water, no salinity policy.	3 inexperienced
Treatment 2 – Water Market with Salinity Tax	Free trade of water, buyers pay salinity tax depending on location of seller and buyer.	4 experienced
Treatment 3 – Water Market with Salinity Levy	Trade barrier on water trade into the HIZ from all LIZ zones, buyers pay salinity tax in all other zones depending on location of seller and buyer.	4 experienced

Note: Inexperienced subjects may have participated in an experiment previously, but they had not participated in an experiment using this environment and software. Experienced subjects had participated in at least one Treatment 1 session. We chose to use (more) experienced subjects for Treatments 2 and 3 as the environment was more complicated than in Treatment 1.

Table 5: Model predictions

	Water market (Treatment 1)	Salinity tax (Treatment 2)	Salinity levy (Treatment 3)
Price	1009 - 978	1029 - 930	1029 - 930
Transaction quantity	8	7	7
Salinity impact (improvement in EC)	0.000011	0.000049	0.000049
Average seller Surplus	51.5	50.875	50.875
Average buyer surplus	91.875	93.875	93.875
Average total surplus (efficiency)	143.375	144.75	144.75

Table 6: Average Price, Quantities, Earnings and Salt Impact

	Average Price (per megalitre)				Average Quantity (megalitre)				Average Earnings (experimental dollars)				Average Salt Improvement (EC)		
Period	Treatment 1	Treatment 2	Treatment 3	Period	Treatment 1	Treatment 2	Treatment 3	Period	Treatment 1	Treatment 2	Treatment 3	Period	Treatment 1	Treatment 2	Treatment 3
1	1034	984	1002	1	9	7	6	1	102	92	126	1	-0.00003	0.000265	0.000705
2	1006	984	997	2	9	7	6	2	112	132	114	2	0.0004933	0.0005625	0.00066
3	1011	986	995	3	8	7	6	3	132	120	120	3	0.0003	0.0004325	0.000665
4	1001	1003	995	4	9	7	7	4	119	120	111	4	0.0001733	0.00091	0.000485
5	1008	982	998	5	8	8	7	5	141	122	134	5	0.0003933	0.000635	0.0005125
6	1003	982	996	6	9	8	8	6	124	117	119	6	0.0005333	0.0007175	0.0005825
7	999	984	1000	7	9	9	8	7	118	122	124	7	0.00044	0.00077	0.00044
8	996	987	988	8	9	8	8	8	132	111	127	8	0.0004167	0.000955	0.00077
9	999	992	996	9	7	9	8	9	143	116	132	9	0.0004067	0.0006625	0.0005325
10	998	988	996	10	9	10	9	10	127	130	138	10	0.0002767	0.00087	0.0002375
11	997	991	998	11	9	10	10	11	119	138	129	11	0.0003733	0.000665	0.000645
12	997	991	996	12	9	10	10	12	132	135	137	12	0.0004167	0.00051	0.0006475
13	997	993	995	13	8	10	10	13	121	132	146	13	0.00024	0.00075	0.0006425
14	997	994	993	14	8	8	10	14	136	121	133	14	0.0002433	0.000785	0.000615
Average price all periods	1003	989	996	Average quantity all periods	9	8	8	Average total earnings all periods	125.53	122.01	127.85	Average salt improve-ment all periods	0.00033	0.0006779	0.00058143

Table 7: Pair wise t-test and Wilcoxon ranksum test for average price, quantity, earnings and salt impact

	Average Price			Average Quantity			Average Earnings			Average Salt Improvement		
	Treatment 1	Treatment 2	Treatment 3	Treatment 1	Treatment 2	Treatment 3	Treatment 1	Treatment 2	Treatment 3	Treatment 1	Treatment 2	Treatment 3
Treatment 1		-2.14*	-1.09		-1.14	-2.70**		-0.34	0.47		8.94***	3.10**
		*-1.77**	*-0.71*		*-1.06*	*-1.77**		*-0.35*	*0.35*		*2.12***	*2.14***
Treatment 2			1.52			-2.11*			0.76			-1.44
			1.16			*-1.73**			*0.29*			*-1.16*
Treatment 3												

**** Significant at 1%, **Significant at 5%, *Significant at 10%.

Note: first estimate is the calculated t-statistic, *second estimate is the calculated z-statistic.* Note: pair-wise estimates use the vertical as the comparison base

Table 8: Pair wise variance ratio test for price, quantity and salt impact variability

	Standard deviation average price			Standard deviation average quantity			Standard deviation average salt improvement		
	Treatment 1	Treatment 2	Treatment 3	Treatment 1	Treatment 2	Treatment 3	Treatment 1	Treatment 2	Treatment 3
Treatment 1		0.53	0.38		0.44	0.53		0.55	4.58
Treatment 2			0.71			1.22			8.35
Treatment 3									

*** Significant at 1%, ** Significant at 5%, * Significant at 10%.

Note: the numbers reported are the relevant F statistics. None of the comparisons are statistically different. Note: pair-wise estimates use vertical as the comparison base

Table 9: Random effects estimation models for average transaction price

	Model 1	Model 1a	Model 2
constant	1005.896***	1005.6219***	994.331***
	(4.997)	(5.245)	(4.937)
	(201.32)	*(191.72)*	*(201.40)*
Period (t)	-0.372	-0.379	
	(0.249)	(0.251)	
	(-1.49)	*(-1.51)*	
1/t			37.769***
			(7.344)
			(5.14)
Treatment 2	-14.46***	-14.801**	
	(6.13)	(6.466)	
	(-2.36)	*(-2.29)*	
Treatment 3	-6.908	-7.153	
	(6.13)	(6.415)	
	(-1.13)	*(-1.11)*	
Salt improvement		991.1773	
		(3365.168)	
		(0.29)	
Treatment 2 x(1/t)			-49.388***
			(9.654)
			(-5.12)
Treatment 3 x (1/t)			-30.641***
			(9.654)
			(-3.17)
Treatment 2 x (t-1)/t			-3.893
			(6.531)
			(-0.60)
Treatment 3 x (t-1)/t			0.2722
			(6.531)
			(0.04)
No. of observations	154	154	154
Wald Chi-squared	7.87	7.54	34.72
Significance level	0.04	0.10	0.0000
R-squared (overall)	0.16	0.16	0.26

The first number in brackets is the standard error; the second number in brackets is the z-statistic.
*** Significant at 1%
** Significant at 5%
* Significant at 10%

Table 10: Random effects estimation models for average transaction quantity

	Model 1	Model 2
constant	8.607***	8.413***
	(0.219)	(0.218)
	(39.27)	*(38.59)*
Period (t)	-0.005	
	(0.018)	
	(-0.26)	
1/t		0.681
		(0.582)
		(1.17)
Treatment 2	-0.268	
	(0.226)	
	(-1.18)	
Treatment 3	-0.661***	
	(0.226)	
	(-2.92)	
Treatment 2 x(1/t)		-0.606
		(0.633)
		(-0.96)
Treatment 3 x (1/t)		-1.507**
		(0.633)
		(-2.38)
Treatment 2 x (t-1)/t		-0.165
		(0.288)
		(-0.57)
Treatment 3 x (t-1)/t		-0.405
		(0.288)
		(-1.40)
No. of observations	154	154
Wald Chi-squared	9.00	11.2
Significance level	0.02	0.04
R-squared (overall)	0.08	0.10

The first number in brackets is the standard error; the second number in brackets is the z-statistic.
*** Significant at 1%
** Significant at 5%
* Significant at 10%

Table 11: Random effects estimation models for average total earnings

	Model 1	Model 2
constant	113.639***	137.817***
	(0.7.104)	(8.112)
	(16.00)	*(16.99)*
Period (t)	1.352***	
	(0.402)	
	(3.36)	
1/t		-84.381**
		(40.713)
		(-2.07)
Treatment 2	-3.279	
	(7.926)	
	(-0.41)	
Treatment 3	2.498	
	(7.926)	
	(0.32)	
Treatment 2 x(1/t)		2.401
		(47.292)
		(0.05)
Treatment 3 x (1/t)		20.130
		(47.292)
		(0.43)
Treatment 2 x(t-1/t)		-4.160
		(10.732)
		(-0.39)
Treatment 3 x (t-1/t)		-0.238
		(10.732)
		(-0.02)
No. of observations	154	154
Wald Chi-squared	11.94	13.09
Significance level	0.007	0.02
R-squared (overall)	0.07	0.08

The first number in brackets is the standard error; the second number in brackets is the z-statistic.
*** Significant at 1%
** Significant at 5%
* Significant at 10%

Table 12: Random effects estimation models for average buyer earnings

	Model 1	Model 2

constant	44.810***	77.423***
	(8.738)	(9.44)
	(5.13)	*(8.20)*
period (t)	1.508***	
	(0.381)	
	(0.71)	
1/t		-136.122***
		(37.910)
		(-3.59)
Treatment 2	7.455	
	(10.520)	
	(0.71)	
Treatment 3	4.921	
	(10.520)	
	(0.47)	
Treatment 2 x(1/t)		66.304
		(44.670)
		(1.48)
Treatment 3 x (1/t)		56.304
		(44.670)
		(1.26)
Treatment 2 x(t-1/t)		-1.677
		(12.493)
		(-0.13)
Treatment 3 x (t-1/t)		-3.052
		(12.493)
		(-0.24)
No. of observations	154	154
Chi-squared	16.17	23.18
Significance level	0.0010	0.0003
R-squared (overall)	0.09	0.12

The first number in brackets is the standard error; the second number in brackets is the z-statistic.
*** Significant at 1%
** Significant at 5%
* Significant at 10%

Table 13: Random effects estimation models for average seller earnings

	Model 1	Model 2
constant	68.829***	60.393***
	(4.812)	(5.560)
	(14.29)	(10.86)
Period (t)	-0.156	
	(0.289)	
	(-0.54)	
1/t		51.741*
		(29.078)
		(1.78)
(t-1)/t		
Treatment 2	-10.734**	
	(5.235)	
	(-2.05)	
Treatment 3	-2.42	
	(5.235)	
	(-0.46)	
Treatment 2 x(1/t)		-63.903**
		(33.708)
		(-1.90)
Treatment 3 x (1/t)		-36.174
		(33.707)
		(-1.07)
Treatment 2 x(t-1/t)		-2.483
		(7.356)
		(-0.34)
Treatment 3 x (t-1/t)		2.814
		(7.356)
		(0.38)
No. of observations	154	154
Chi-squared	5.24	8.52
Significance level	0.15	0.13
R-squared (overall)	0.09	0.10

The first number in brackets is the standard error; the second number in brackets is the z-statistic.
*** Significant at 1%
** Significant at 5%
* Significant at 10%

Table 14: Random effects estimation models for salt improvement

	Model 1	Model 2
constant	0.0003***	0.0004***
	(0.00007)	(0.00007)
	(4.08)	(6.12)
Period (t)	7.28e-06	
	(6.20e-06)	
	(1.17)	
1/t		-0.0003
		(0.0002
		(-1.72)
Treatment 2	0.0003***	
	(0.00007)	
	(5.18)	
Treatment 3	0.0002***	
	(0.00007)	
	(3.73)	
Treatment 2 x(1/t)		0.0002
		(0.0002)
		(0.93)
Treatment 3 x (1/t)		0.0006***
		(0.0002)
		(3.06)
Treatment 2 x (t-1/t)		0.0004***
		(0.00009)
		(4.41)
Treatment 3 x (t-1/t)		0.0001
		(0.00009)
		(1.51)
No. of observations	154	154
Wald Chi-squared	29.06	41.85
Significance level	0.000	0.0000
R-squared (overall)	0.17	0.24

The first number in brackets is the standard error; the second number in brackets is the z-statistic.
*** Significant at 1%
** Significant at 5%
* Significant at 10%

Table 15: Average buy and sell offer price

	Treatment 1		Treatment 2		Treatment 3	
	Average Buy Offer	Average Sell Offer	Average Buy Offer	Average Sell Offer	Average Buy Offer	Average Sell Offer
All periods	947.836 (157.506) *(960)*	1048.771 (101.087) *(912)*	922.29 (174.284) *(1489)*	1073.571 (141.434) *(1325)*	943.051 (101.238) *(1491)*	1037.357 (89.177) *(1300)*
Last five periods	945.064 (185.511) *(324)*	1028.796 (65.257) *(264)*	931.755 (175.986) *(503)*	1051.717 (115.589) *(453)*	951.393 (78.547) *(501)*	1019.178 (47.068) *(456)*

Note: First number in brackets is the standard deviation, *second number in brackets is the number of observations*.

Table 16: Estimation models for buyer offers

	OLS regression		Random effects estimation	
	All periods	Last five periods	All periods	Last five periods
Constant	528.86***	485.005***	577.194***	516.85***
	(39.877)	(82.539)	(46.76)	(80.655)
	(13.26)	(5.88)	(12.34)	(6.41)
Period (t)	1.652**	-0.312	1.15*	-2.008
	(0.567)	(2.894)	(0.4602)	(2.28)
	(2.91)	(-0.11)	(2.5)	(-0.88)
Value	0.383***	0.444***	0.337***	0.437***
	(0.037)	(0.07)	(0.0358)	(0.063)
	(10.3)	(6.35)	(9.4)	(6.94)
Treatment 2	-19.984***	-8.44	-27.74	-30.04
	(5.968)	(10.55)	(36.396)	(46.35)
	(-3.35)	(-0.8)	(-0.76)	(-0.65)
Treatment 3	-0.168	8.73	2.681	5.889
	(5.96)	(10.538)	(36.36)	(46.33)
	(-0.03)	(0.83)	(0.07)	(0.13)
No. of observations	3940	1328	3940	1328
Wald Chi-squared/F-statistic	34.38	11.23	95.07	49.75
Significance level	0.000	0.000	0.000	0.0000
R-squared (overall)	0.03	0.03	0.03	0.03

The first number in brackets is the standard error; the second number in brackets is the t or z-statistic.
*** Significant at 1%
** Significant at 5%
* Significant at 10%

Table 17: Estimation models for seller offers

	OLS regression		Random effects estimation	
	All periods	Last five periods	All periods	Last five periods
Constant	892.28***	788.153***	831.98***	758.36***
	(28.29)	(44.413)	(29.176)	(41.75)
	(31.54)	(17.75)	(28.52)	(18.16)
Period (t)	-5.286***	0.833	-5.199***	0.978
	(0.46)	(1.68)	(0.422)	(1.51)
	(-11.46)	(0.5)	(-12.31)	(0.65)
Cost	0.215***	0.241***	0.275***	0.274***
	(0.029)	(0.039)	(0.028)	(0.036)
	(7.38)	(6.14)	(9.93)	(7.59)
Treatment 2	21.109***	18.066***	14.36	9.55
	(4.85)	(6.425)	(15.71)	(13.844)
	(4.35)	(2.81)	(0.91)	(0.69)
Treatment 3	-9.620**	-7.8	-10.31	-11.489
	(4.814)	(6.373)	(15.72)	(13.82)
	(-2.00)	(-1.22)	(-0.66)	(-0.83)
No. of observations	3537	1173	3537	1173
Wald Chi-squared/F-statistic	66.34	18.7	263.99	63.09
Significance level	0.000	0.0000	0.0000	0.0000
R-squared (overall)	0.07	0.06	0.07	0.06

The first number in brackets is the standard error; the second number in brackets is the z-statistic.
*** Significant at 1%
** Significant at 5%
* Significant at 10%

Figure 1: Market supply and demand

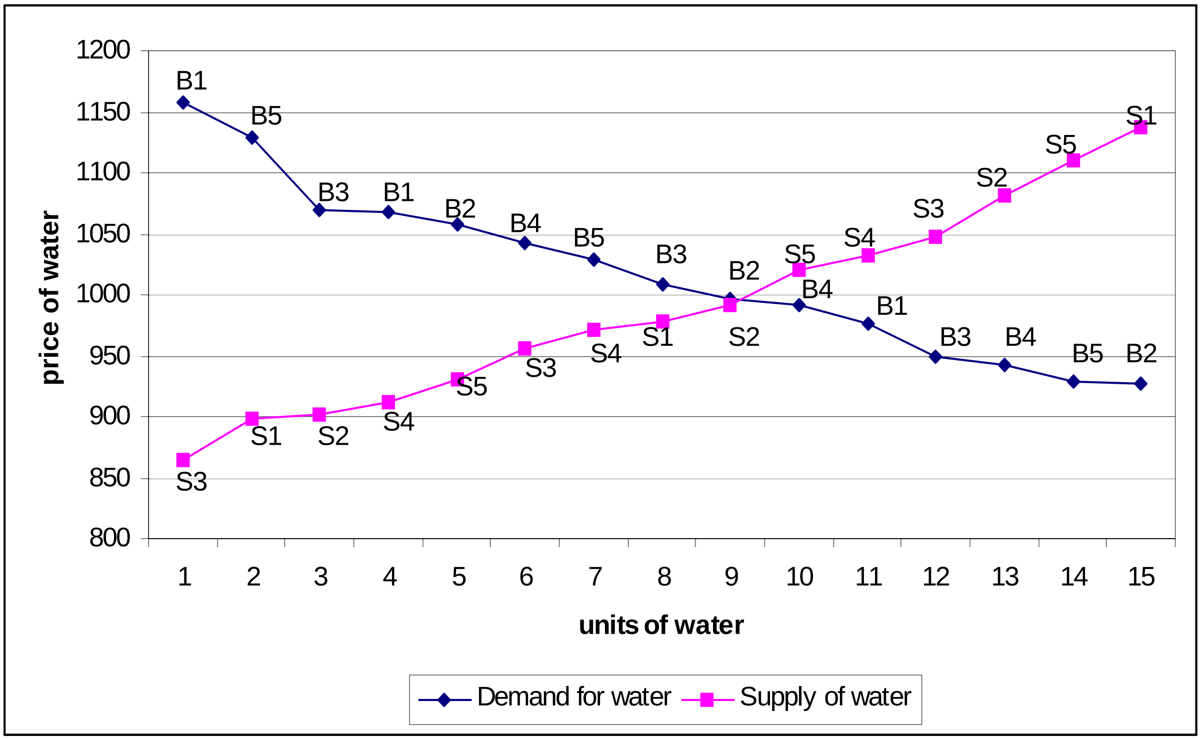


Figure 2: Average transaction price across all sessions for all treatments

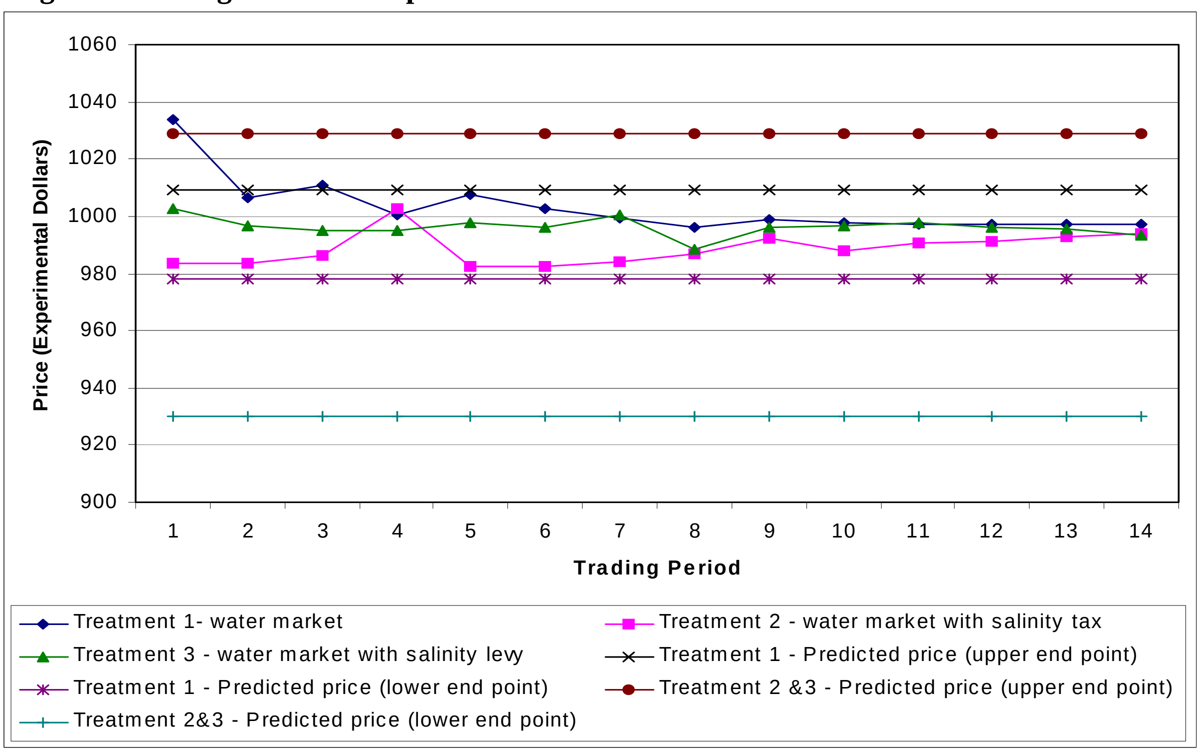


Figure 3: Average transaction quantity across all sessions for all treatments

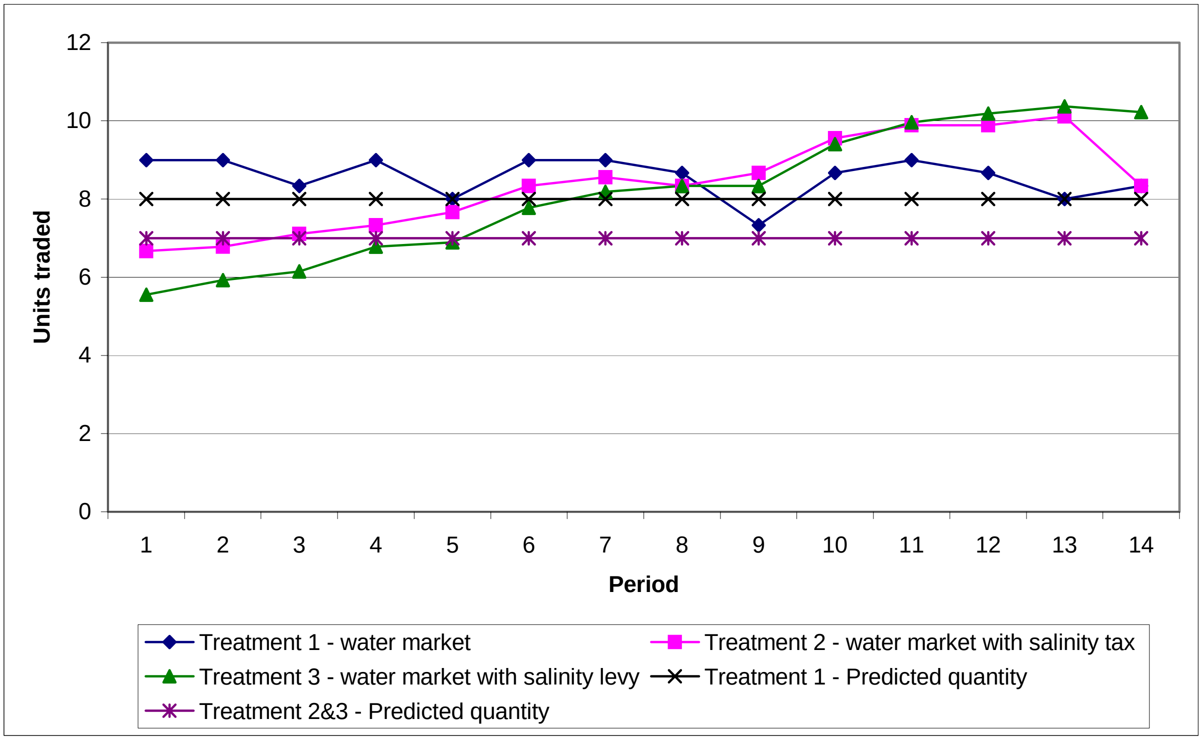


Figure 4: Average change in salinity across all sessions for all treatments

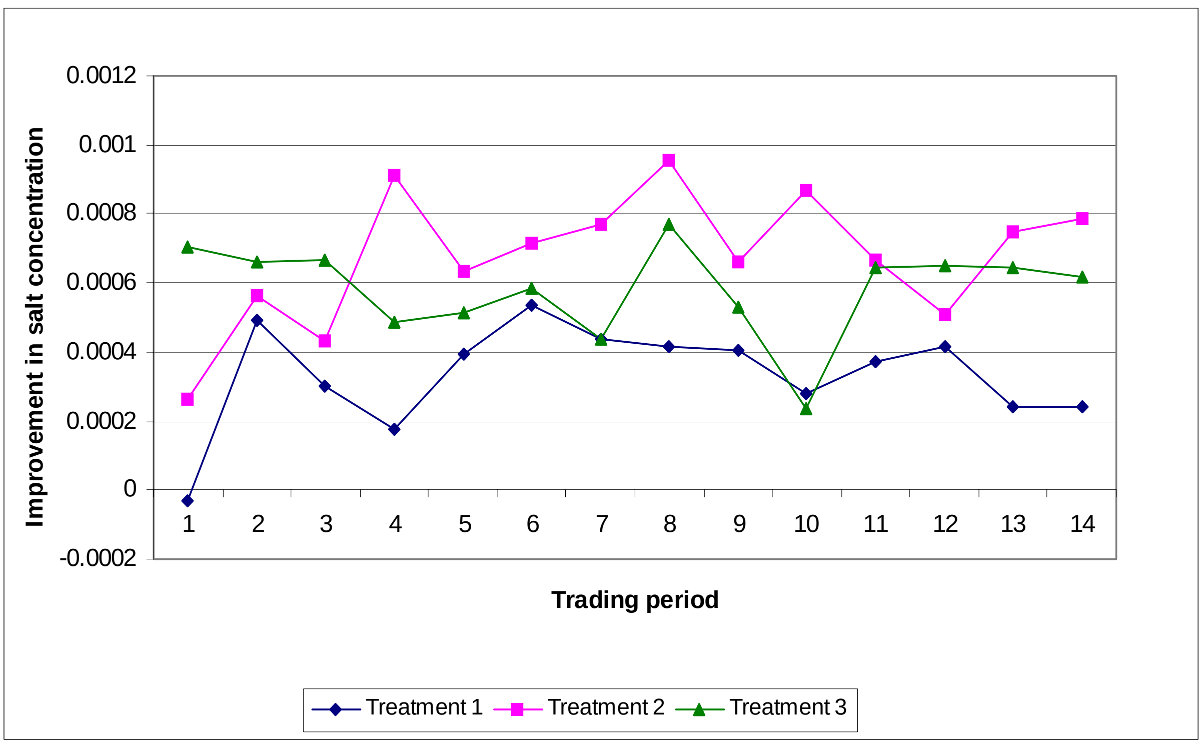


Figure 5: Average buyer surplus across all sessions for all treatments

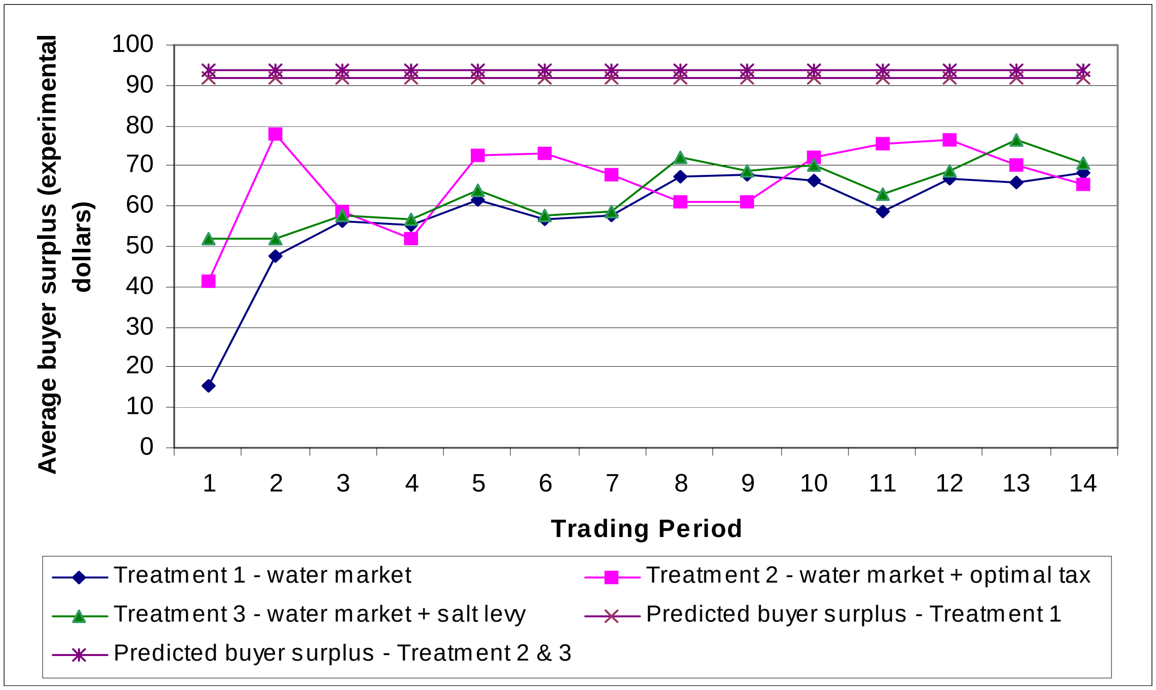


Figure 6: Average seller surplus across all sessions for all treatments

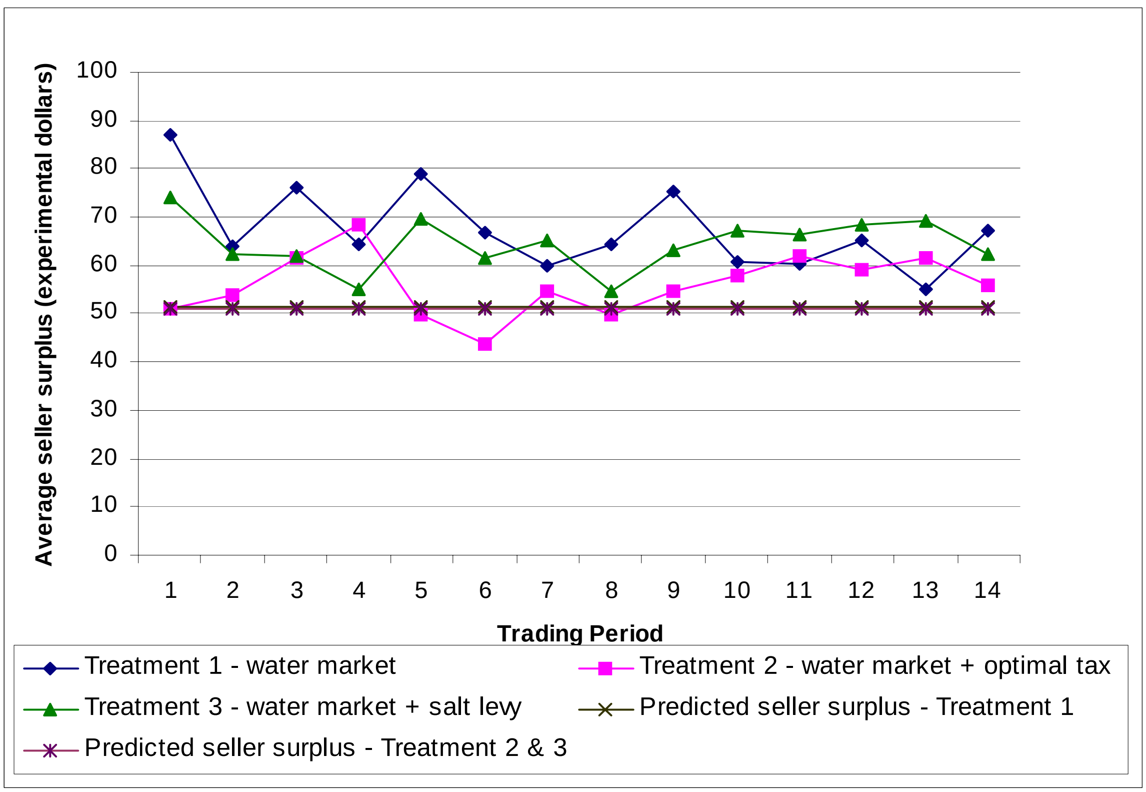
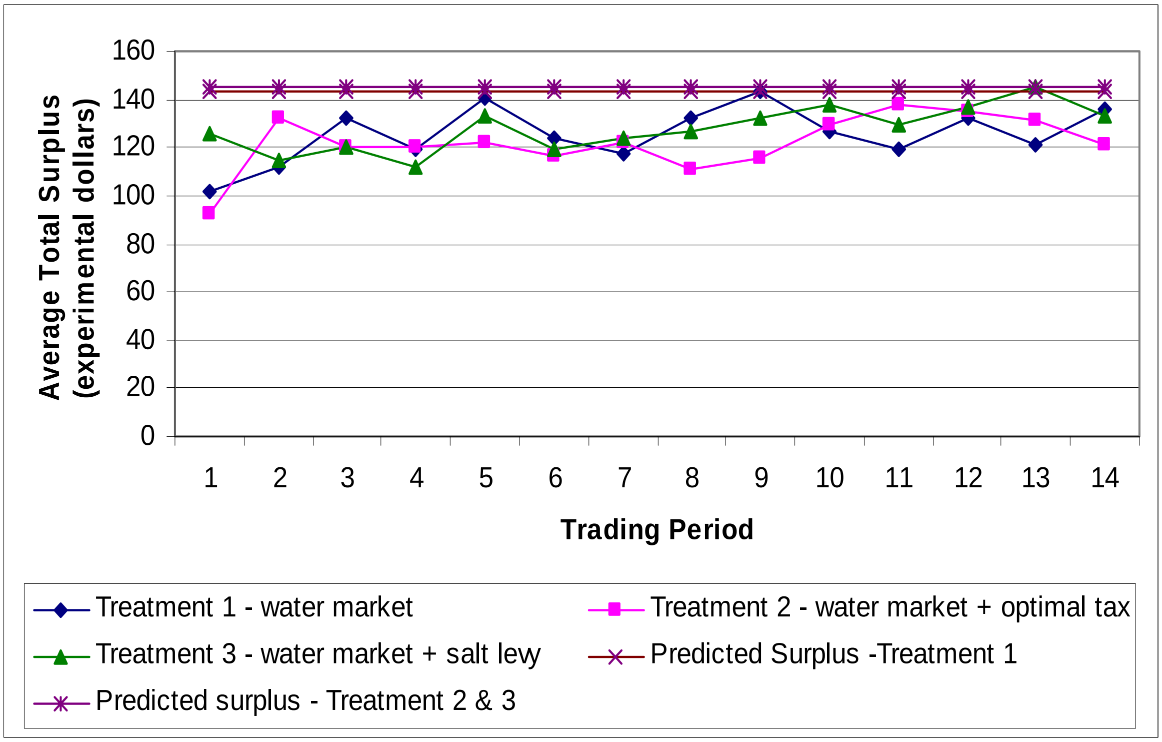


Figure 7: Average total surplus across sessions all treatments



Experiment instructions (Treatment 3, The Salinity Levy)

Buyer___________ Seller_____________

Introduction

This is an experiment in the economics of decision making. The instructions are simple and if you follow them carefully and make good decisions you will earn money that will be paid to you at the end of the experiment. All earnings on your computer screens are in Experimental Dollars. These Experimental Dollars will be converted to real Dollars at the end of the experiment, at a rate of ________Experimental Dollars = 1 real Dollar. Everyone starts with 2000 Experimental Dollars (which is worth ______real Dollars), and your additional earnings are added to this starting amount.

We are going to conduct a set of markets in which you will be a participant in a sequence of market trading periods. Attached to these instructions you will find a sheet labelled Personal Record Sheet, which will help you keep track of your earnings based on the decisions you might make. You are not to reveal this information to anyone. It is your own private information.

During each market period you will be free to buy or sell units as you choose. Buyers earn money from redeeming any purchased units at a known redemption amount from the experimenter. Sellers earn money from selling units that cost a known amount. You will be either a buyer or a seller in today's experiment, and you will remain in this role throughout the experiment. This experiment has five sellers and five buyers.

Buyers

Redemption is like a resale to the experimenter at a set price. If you are a buyer, your computer screen includes Redemption Values written on the left side—one value for each unit you might buy. See Figure 1.

- If you buy a unit during the trading period, your earnings from that unit are,
    - Earnings in experimental dollars = (redemption value) – (purchase price)
- When deciding to make an offer to buy or accept a seller's offer to sell it is important to take note of the sellers' taxes. Sellers' taxes are listed in the box in the middle of your screen. (You have also been given a hardcopy of the sellers' taxes.) If you buy from a seller with a tax you must pay the tax (all taxes accrue to the experimenter.)

If you buy a unit during the trading period from a seller with a tax, your earnings on that unit are,

- Earnings in experimental dollars = (redemption value) - (purchase price + tax)
- In some cases a buyer may not be allowed to buy from a given seller. If there is a seller that you are not allowed to buy from, then the seller will be displayed on the top left of your screen in red. If you can buy from all sellers then this information is not displayed on your screen.
- Your total earnings in experimental dollars will be updated immediately after a purchase on the left side of your computer screen, above your redemption values, and are labelled "Current Profits this Period".
- The number of units you have bought so far this period is shown on the left side of your computer screen above current profits. The yellow bar on the left side of your computer screen also highlights the unit you are currently trying to buy. Please pay careful attention to the number of units you have bought so far this period, since that information is useful when assessing the redemption value of the next unit you buy.
- Notice that if you pay more for a unit than its redemption value then you suffer a loss in earnings on that unit. If you are buying from a seller who has a tax and you pay more for a unit than its redemption value *minus* the tax then you will suffer a loss in earnings on that unit. If you do not buy any units in a period then your earnings are zero for that period. Your redemption values may be different from the values of other buyers.

Sellers
Sellers incur a per-unit cost when they sell a unit. If you are a seller, your computer screen displays your costs on the left side—one cost value for each unit you might sell. See Figure 2.

- If you sell a unit during the trading period, your earnings on that unit are,
    - Earnings in experimental dollars = (sale price of unit) – (cost of unit).
- Your total earnings will be updated immediately after a sale on the left side of your computer screen, above your costs, and are labelled Current Profits this Period.
- The number of units you have sold so far this period is shown on the left side of your computer screen above current profits. The yellow bar on the left side of your computer screen also highlights the unit you are currently trying to sell. Please pay careful attention to the number of units you have sold so far this period, since that information is useful when assessing the cost of the next unit you sell.
- Some sellers have taxes. You have been given a hardcopy of the taxes for each seller. If you have a tax, then a buyer will have to pay the tax if she wants to buy your unit. (All taxes accrue to the experimenter.)
- Some sellers are not allowed to sell to some buyers. If you cannot trade with a buyer, then that buyer is shown in red at the top left of your screen. If you can trade with all buyers then this information is not shown on your screen.
- Notice that if a unit costs more than the amount for which you sell it then you suffer a loss in earnings on that unit. If you do not sell any units in a period then your earnings are zero for that period. Importantly, You do not incur the cost of a unit unless you sell that unit. Your costs may be different from the costs of other sellers.

How to Buy and Sell
Each trading period will be open for trading for 5 minutes; the time left in the period is shown on the upper right of the trading screen.

- At any time during the period, any buyer is free to make an offer to buy a unit at a price they choose; likewise, any seller is free to make an offer to sell a unit at a price they choose.
- Also at any time during the period, any buyer is free to buy at the best offer price specified by a seller, and any seller is free to sell at the best offer price specified by the buyers.

You will enter offer prices and accept prices to execute transactions using your computer. Figure 1 shows the market trading screen seen by buyers. Figure 2 shows the market trading screen seen by sellers.

Buyers

- Submit offers to buy using the "Price" box in the upper centre of the screen, and then clicking on the "Make Offer" button (DO NOT PRESS ENTER, you must click the 'Make Offer' button to submit an offer).
- Choose which sellers you want to make the offer to by checking and unchecking the boxes next to the sellers' names. If all boxes are checked then you make the offer to all sellers. If you uncheck a box then the offer is not made to the unchecked seller(s).
- Buyers' offers to buy are displayed at the top right of your screen. All new offers to buy must be higher than the current highest buy offer.
- Buyers observe seller's offers to sell on the lower right part of their computer screen. Sellers' offers are displayed including any tax you must pay.
- An accept button will appear next to the seller's offer. If you want to accept this offer you click on the accept button next to the offer you want to accept.

- If a seller's offer is shown in red, then you cannot trade with this seller. No accept button will appear next this seller's offers.
- The previous trading prices in the current period are displayed in the "Sold Prices" list in the bottom centre of your computer screen.

Sellers

- Submit offer prices using the "Price" box in the lower centre of the screen, and then clicking on the "Make Offer" button (DO NOT PRESS ENTER, you must click the 'Make Offer' button to submit an offer).
- This offer price is immediately displayed on all traders' computers on the lower right part of the screen, labelled "Sellers' Offers."
- If you already have a Sell Offer displayed in the current period, then new sell offers you submit must be lower than your current sell offer. (Note. Your new offer price does not have to be lower than other sellers' best price, but it must be lower than your best offer price.)
- Sellers observe the Buy Offers made by buyers on the upper right part of their computer screen. They accept the best (that is, highest offer price) by simply clicking the "Accept" button on the right side of their computer screen. This results in an immediate trade at that price.
- Some sellers cannot trade with some buyers. If an offer price is from a buyer you cannot trade with the accept button will not appear on your screen.

Recording Rules

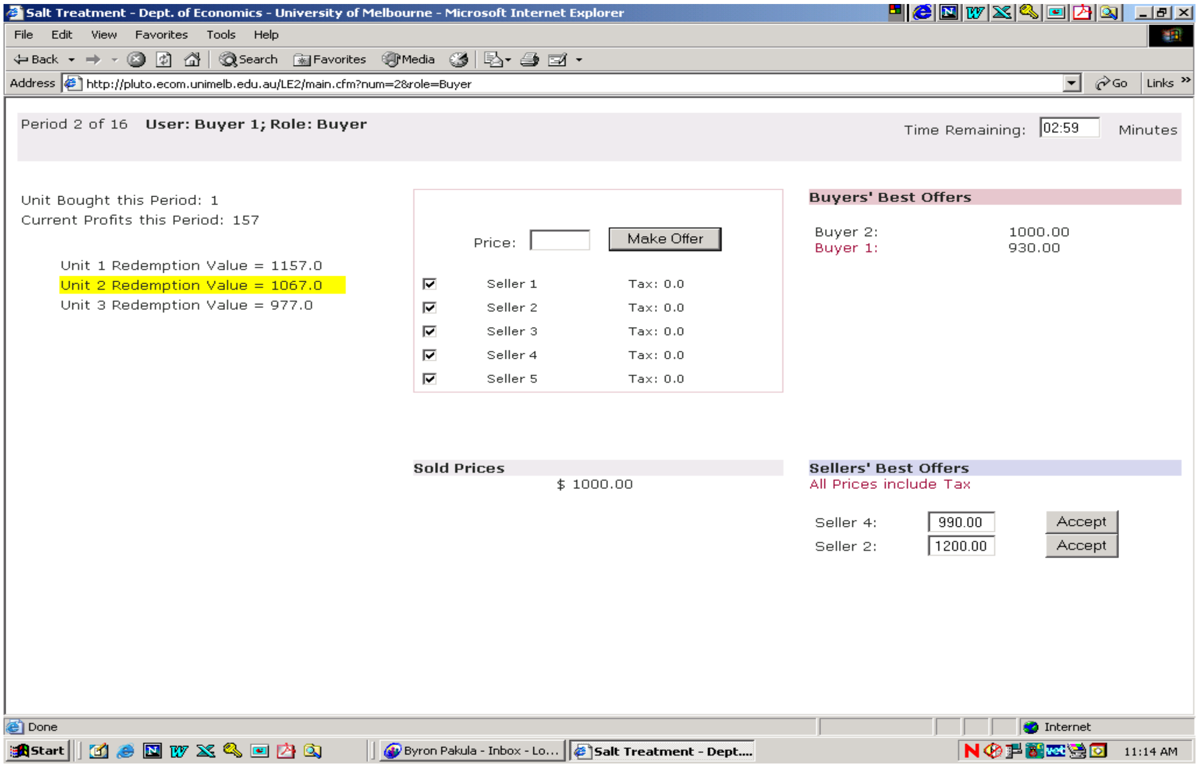
- Your end of period earnings (in experimental dollars) equal the sum of all profits (positive and negative) on the units that you trade.
- This total will be updated continuously on the left side of your computer screen, and it will be displayed briefly on a screen at the end of a trading period.
- At the end of a trading period you should write down these earnings in column (2) of your Personal Record Sheet. Keep track of your cumulative profits in column (3), by adding the column (2) period profit to the previous period's cumulative profit. At the end of the experiment you will divide your cumulative profit by the conversion rate to determine you total earnings in real Dollars.

Summary

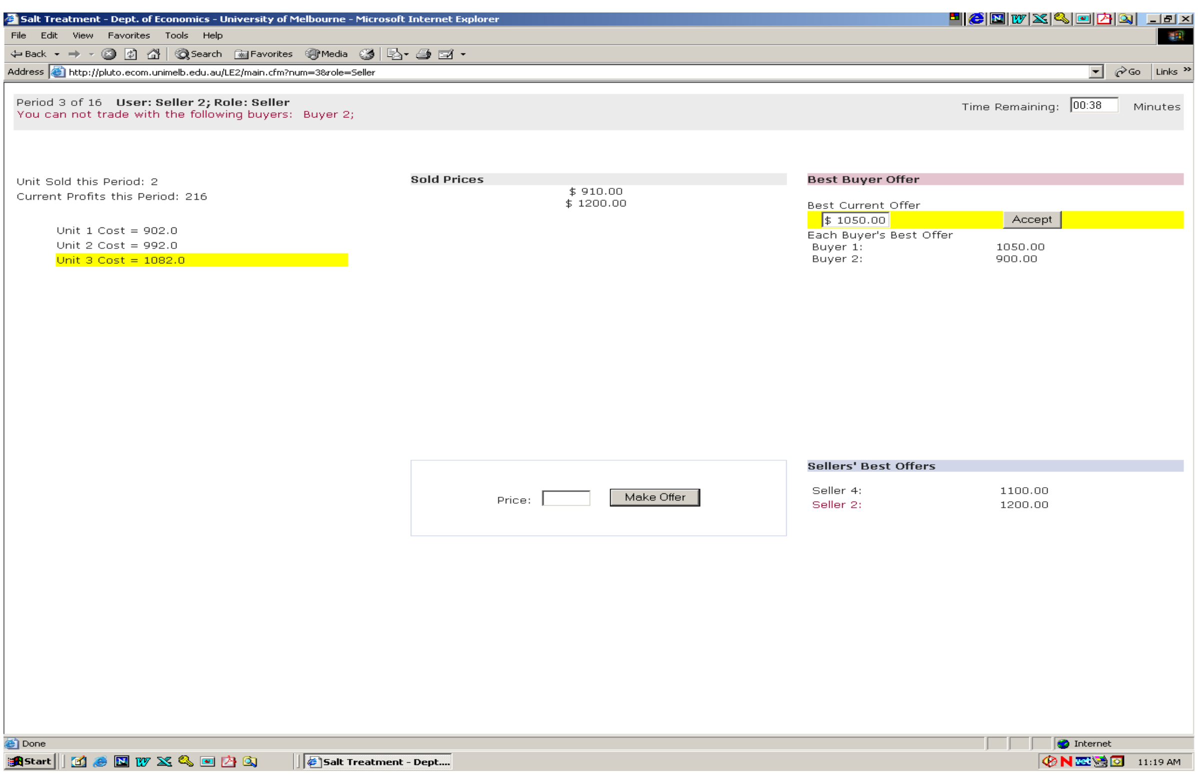
- Seller earnings on a sold item = sale price of item – cost of item
- Buyer earnings on a purchased item = redemption value of item – price paid for item
- Some sellers have taxes. If a buyer buys from a seller with a tax then the buyer earnings on the purchased item = redemption value – tax – price paid for item.
- At any time during the 5-minute trading period, buyers submit offers to buy or accept sellers' offers to sell an item. Sellers submit offers to sell or accept buyers' offers to buy an item.
- Some buyers cannot buy from some sellers.
- Some sellers cannot sell to some buyers
- New offers to buy must be higher than all existing offers to buy.
- A seller's new offer to sell must be lower than his/her existing offer to sell.
- Profits should be recorded on Record Sheets at the end of each period.

We will conduct _____training periods. Are there any questions now before we begin the experiment?

Example Buyer Screen



Example Seller Screen



Personal Record Sheet for Seller/Buyer Number _______________

Period Number (Column 1)	Profit this Period (displayed on your computer screen at the end of the period) (Column 2)	Cumulative Profit (all Periods) (Column 3)
0		Start at 2000
1	TRAINING	TRAINING
2	TRAINING	TRAINING
3		
4		
5		
6		
7		
8		
9		
10		
11		
12		
13		
14		
15		
16		
Conversion Rate:	Divide by:	
	Converted Total:	