School of Economics, Finance and Property

Investment Decisions Under An Emissions Intensity Target

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Declaration

To the best of my knowledge and belief this thesis contains no material previously published by any other person except where due acknowledgment has been made. This thesis contains no material which has been accepted for the award of any other degree or diploma in any university.

Human Ethics

The research presented and reported in this thesis was conducted in accordance with the National Health and Medical Research Council National Statement on Ethical Conduct in Human Research (2007) – updated March 2014. The proposed research study received human research ethics approval from the Curtin University Human Research Ethics Committee (EC00262), Approval Number # HRE2019-0201.

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Abstract

This thesis reports the results of a controlled laboratory experiment to test the effect of emissions price certainty on firm investments in clean technology when a regulator has set an intensity target. Participants acting as firms make a technology decision that determines the emissions intensity of their production. Emissions permits are generated by cleaner firms producing below this intensity target, and are required by dirtier firms producing above it. When permit prices are fixed, incentives are much clearer to firms and emissions much closer to predictions. As such, policymakers concerned with meeting specific reduction targets might be more inclined to fix prices when using an intensity target. In the Australian context, if taxes are politically unpopular, then employing a market mechanism instead could result in even greater emissions reductions. The reversibility of the technology choice is also varied to test for effects on different types of investments (where reversible decisions are a proxy for short-run investments, and irreversible ones a proxy for long-term investments). Reversibility tended to affect the pattern of decision making, with differences particularly marked when combined with the market mechanism. This might support evidence from elsewhere of a tendency to delay large, irreversible investment decisions when pricing is uncertain. The use of a market also introduced unexpected trading behaviour by participants, which could also have real-world implications for market efficiency.

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1 Introduction

As 2019 brought the hottest decade on human record to a close (WMO, 2019) and millions of hectares burned across Australia (Zhou, 2020), the 25th global climate change talks (COP25) concluded with a partial agreement that countries needed to be more ambitious if they were to meet the goals of the Paris Agreement (Harvey, 2019). A signatory to the Agreement, Australia has committed to reduce emissions 26-28% below 2005 levels by 2030 (Department of the Environment and Energy, 2018b). However, emissions, as tracked by the Australian Government, are rising, despite reductions achieved in the electricity sector, and the nation is not on track to achieve its Paris commitments given current projections (Department of the Environment and Energy, 2018a).

Achieving these targets will require policy intervention, but the question remains: what policy? In the broadest terms, mechanisms intended to reduce emissions can either employ price signals to motivate reductions or enforce quantitative limits. Some policies combine the two. The effectiveness of various policy options has been the subject of much debate, but may depend, to some extent at least, on context and application. Meeting long-term goals will require investment in low-emission technology and so, in devising policy, regulators must consider the way policy options interact with firm investment decisions. A principal concern is the impact of emissions pricing certainty on investments. The use of market-based pricing mechanisms creates cost uncertainties for firms with respect to their abatement efforts, compared to the use of a fixed tax. Will firms more readily invest in cleaner technology when emission prices are certain?

A secondary issue is understanding the way that policies interact with different sorts of investments. Investment in production technologies that require more permanent upgrades (long-term investments, as defined in this thesis) may respond differently to policy than investments that are more temporary in nature (short-term). Policy-makers with a particular objective in mind may wish to promote one or the other. The electricity sector is a common focus for climate change policy, since it not only accounts for the largest share of emissions (by sector), but cost-effective, low-emission technologies are already available (Finkel et al., 2017). As such, a failure to drive cuts here may mean that the overall costs of meeting reduction targets will be much higher as the burden falls to other sectors of the economy. The rapidly declining cost of renewable energy generation means the shift toward low-emission electricity is well underway. Wind energy is competitive with coal generators, and large-scale solar may not be far behind (Brinsmead et al., 2014). This is a sector very much characterised by large investments, generally long-term in nature. But generation capacity can be fuelled by either fossil fuels or renewables. The choice here will depend, at least in part, on the price signals created by emissions reduction policies. The prospect of higher emissions prices into the future is more likely to induce investment in cleaner options in the near term. Will these signals be stronger if the prices are fixed and known with certainty as compared to those determined via a market mechanism?

A particular challenge, also, is effectively managing this transition to cleaner energy to ensure reliability of supply. A number of policy measures have been introduced or proposed, including a carbon tax, trading scheme and intensity target. Current Federal measures include a Renewable Energy Target (RET), mandating a percentage of electricity generation to be derived from renewable sources and creating tradeable permits, and electricity generators are also subject to the Safeguard Mechanism, which mandates baseline emissions rates for the economy's largest emitters. Some State governments have emissions and/or renewable energy targets in place, as well as energy efficiency programs, renewable subsidies and such that may interact with Federal policies.

While questions remain about effectiveness, it is useful to be able to test the effect of different mechanisms before rollout. This thesis presents the design for a controlled laboratory experiment to test the effect of permit price certainty on investment decisions, and the interaction between policy settings and different types of investment. As well as being able to test the impact of altering one variable, and holding others constant, a key advantage of this sort of experiment is in being able to observe the behavioural response of real people. Sometimes theory is confirmed (Plott, 1983), sometimes it is confounded by the decisions of people, leading to the development of better theory (consider the development of prospect theory to explain violations of expected utility theory, for example Tversky & Kahneman (1992)). Indeed, it is possible to observe unexpected outcomes under this setting and even serious flaws. Classic examples of this include the exposure of biased price signals in US emissions permit auctions (Timothy N. Cason, 1995).

1.1 Emissions Intensity Targets

One policy option is to establish a market for tradeable emissions permits. Under the more familiar cap & trade scheme, total emissions can be limited by controlling the supply of the number of permits, and the price is determined by trading in a market. The basic economic intuition here is that emitters will either purchase permits to cover their emissions, or reduce these emissions (abate), depending on which is cheaper. In this way, trading in the permit market will, in theory, result in a permit price equivalent to the marginal abatement cost. This is the basic setup for a cap (the limit) and trade scheme.

One example of a cap & trade scheme, the EU emissions trading schemes (ETS), has been in place for some years now, and similar ETS exist elsewhere. Prior to more recent reforms, one problem with the EU ETS, however, was very low prices. Although total emissions were still controlled in the near-term, the concern was that prices may be too low to motivate longer-run investment in cleaner technologies (Brink et al., 2016). Theoretical outcomes in these markets are dependent on the regulator being able to accurately determine the number of permits required. Under a cap & trade scheme, prices can be low because either the supply of permits is set too high, or demand is too low (Marschinski & Lecocq, 2006). For the EU, the combination of a miscalculation of permit needs and the economic downturn after 2008 worked to push down emissions permit prices. The effect on prices can work the other way as well, if supply is set too low or the market is affected by a positive demand shock in the economy that also increases demand for permits. This possibility is of particular concern in the context of economic growth. An additional implication of the cap & trade permit market is that prices can be volatile, as supply cannot respond to changes in demand. This is one reason some advocate for taxes instead.

An alternative to setting a limit on the number of permits is to set a limit based on the rate of emissions. This is the basic idea behind the use of an intensity target, which essentially equates to a technology standard. Here, the regulator sets a limit on the volume of emissions per unit of output. These limits can vary by industry or project, depending on current standards and prospects for abatement, or may be set at the economy level (allowing the market to determine the most cost effective abatement). This intensity target in effect marks the ‘free’ level of production since firms producing at the technology standard are not required to surrender emissions permits. In practice, this equates to a production subsidy.

A key difference with the cap & trade scheme is that intensity targets allow for the total supply of permits to rise and fall in response to changes in demand. Permits are generated when output is produced at a lower intensity than the target level. So, firms employing relatively cleaner technologies receive emissions permits equivalent to the difference between the emissions intensity of their production and the technology standard. Conversely, firms with dirtier technologies produce more emissions per unit of output and must surrender permits (equivalent to the difference between the technology standard and the emissions intensity of their production) in order to comply with the scheme. These firms must then purchase permits from firms with cleaner technology. This means that permit supply is determined by the technology choices of firms that 'over-achieve' on the target and is unknown in advance, but flexible.

Instead of setting a limit on emissions, the regulator sets a target rate of emissions. Since supply is flexible, total emissions will not be known in advance, a key distinction between this system and a cap and trade regime. As the issue with greenhouse gas emissions is the atmospheric accumulation over time, rather than the flow rate at source, this variability in emissions is not necessarily a problem, though the objective will still be to reduce emissions overall over time. An advantage of this flexibility, also, is that prices of traded permits tend to be more stable due to the responsiveness of supply to changes in economic conditions (Kuik & Mulder, 2004). Similarly, intensity targets are often cited as better able to accommodate economic growth compared to cap & trade schemes (Fischer & Springborn, 2011; Pizer, 2005).

Having set an intensity target, the regulator can choose to either allow permits to be traded, with prices determined in a market (often referred to as a baseline & credit scheme), or fix the prices as a tax and subsidy (Almutairi & Elhedhli, 2014). In theory, outcomes may be the same either way, so long as the price is set appropriately. It is theoretically possible to achieve the same overall outcomes with either a cap & trade or intensity target scheme. In practice, however, uncertainty around demand, baseline emissions (based on which the target required to achieve policy goals is determined) and the cost of abatement means that this is far from certain in practice. Even when incentives are consistent across schemes, outcomes may vary in practice as decision makers respond differently to the regimes. And, because of the implicit production subsidy contained within the intensity target mechanism, even if the same emissions outcome is achieved, the market permit price will be higher under an intensity target compared to a comparable cap & trade scheme. (This assumes the case of a cap & trade scheme where all permits are purchased, whereas in practice permits may be allocated for free up to some limit, implying a comparable production subsidy. Similarly, taxes may be applied only beyond some level of production.) When comparing these two types of schemes, then, policymakers may consider the trade-offs to be made in terms of overall prices, as well as pricing volatility and control over total emissions.

1.2 Intensity Targets in Australia

Australia has considerable experience with the use of intensity targets, in a variety of mechanisms. NSW formerly had a baseline & credit scheme in the electricity sector. The Greenhouse Gas Reduction Scheme (GGAS) commenced in 2003 and was wound up in anticipation of the national ETS proposed by a previous government (and no longer planned) (Crossley, 2008). The NSW State government set an overall target for emissions on a per-capita basis, and individual emissions-intensity benchmarks were set for participants based on their contribution of electricity supply to end-customers. Participants, which included all electricity retailers, certain generators, and larger customers, were obliged to reduce emissions rates to this benchmark level, or purchase New South Wales Greenhouse Abatement Certificates (NGACs). The number of NGACs that had to be surrendered was calculated in line with regulated benchmarks and participants' annual sales (Crossley, 2008). Electricity retailers could adjust the emissions intensity of the electricity they sold (and alter the number of NGACs they had to purchase) by altering their portfolio of purchases from generators, engaging in energy efficiency activities or purchasing approved offsets (Passey et al., 2008). This created demand for abatement in the sector. The permits themselves were created when electricity generators undertook approved projects with emissions intensity levels below the calculated benchmark (Crossley, 2008).

The Renewable Energy Target (RET) utilises a quasi-intensity target in the form of a targeted proportion of electricity to be sourced from renewable generators. However, the resulting emissions intensity also depends on the remainder of the generation mix. Generators of renewable energy are able to create tradeable permits, which can be used by retailers to meet their reductions requirements (Clean Energy Regulator, 2018), and so the scheme functions somewhat like a baseline & credit market. There is currently no plan to extend the RET beyond 2020. The other key element of the Federal approach to reducing emissions is the Climate Solutions Fund (formerly the Emissions Reduction Fund), from which entities can be awarded contracts (to receive 'credits') for undertaking certain kinds of projects that will result in lower emissions (Department of the Environment and Energy, 2019). These credits can either be sold back to the Government at a pre-arranged rate, or traded with other entities. The

Safeguard Mechanism was also introduced in association with the Climate Solutions Fund, which is rather limited in terms of the scope of its impact in the economy. The Safeguard mandates baseline emissions rates for the economy's largest emitters, and is intended to ensure that any emissions reductions achieved by the Climate Solutions Fund are not offset by increases elsewhere.

If a mechanism incorporating an intensity target is preferred by policy makers, there are options to expand or adapt existing schemes. In theory, the Safeguard could be expanded into an emissions trading scheme, with cross-trading permitted across the RET (if it were continued) and the Climate Solutions Fund (which could function as a source of offsets to ensure adequate market volume). Additionally, or alternatively, a single intensity target could be applied across the electricity generation sector, avoiding the complication of determining individual baselines, as in the current Safeguard Mechanism. To aid in such a process, this project explores the effect of different pricing mechanisms used in conjunction with an intensity target. This research will also offer insight into any effect that irreversibility of the technology choice may make. In combination, this should offer guidance to regulators to better fine-tune policy to meet their specific objectives.

1.3 Investment in Clean Energy

Since reducing emissions in the electricity generation sector is both technologically feasible (ClimateWorks Australia et al., 2014) and likely to be relatively more cost effective compared to making reductions in other areas of the economy (Bruckner et al., 2014), it is a common focus for policy makers. It is notable, however, that the sector is characterised by long-lived capital investments, meaning that investment decisions here are both long-term and irreversible in nature, raising particular issues with respect to the nature of long-term price signals required to drive new investment in cleaner technology. Firms may react both to expected trajectories of emissions prices, as well as expectations about future policy and its stability over time (Climate Change Authority, 2016).

Managing an 'orderly transition' will mean ensuring policies promote appropriate long-term investments in new generation capacity to replace retiring fossil-fuel generators (Finkel et al., 2017). However, risk-averse managers may respond to uncertainty by avoiding making large, fixed investments if they have the option to make smaller, reversible investments instead (even if these are sub-optimal and increases operating costs in the long-run) Dorsey (2019). In the context of electricity generation technology, retrofitting carbon capture and storage technology may be more likely in the face of pricing uncertainty (Blyth et al., 2007), and investment in cleaner technologies may be likely to be delayed when pricing is uncertain, with these delays increasing with increases in the uncertainty (Fuss et al., 2009). This project will examine individual choices to detect differences when the technology decision is either reversible or irreversible. This is broadly consistent with the sorts of decisions managers may make when adopting new technologies. The setup here does not allow for directly choosing between different options, but can detect different effects under the two pricing mechanisms when the technology choice is either reversible or irreversible. It is also possible to test whether there are other differences, such as tending to pick higher intensity levels early on and taking longer to choose lower levels, which might suggest a tendency to delay when the technology choice is irreversible.

1.4 Methodology

One challenge associated with the use of empirical data for understanding the effectiveness of environmental policy is that it is difficult to disentangle causal effects due to the endogenous nature of policy choices (Cason & de Vries, 2005). Laboratory methods can overcome this limitation. They can also be used to testbed new institutional features before rollout, as well as make design improvements. This was demonstrated by Cason and Plott (1996) in the context of the US' sulphur dioxide markets, with significant contributions to the development of the auction pricing system. Within a simplified setting that contains the essential elements of the system under study, experiments can aid in the identification of the most influential factors of interest, as well as an opportunity to identify potential obstacles to full implementation of policy. Indeed, experimental methods have become commonplace in environmental economics, and there is a significant body of experimental research specifically addressing ETS dating back to Plott (1983), who first demonstrated market behaviour in the presence of an externality using an experiment (for reviews see Muller & Mestelman (1998); Sturm & Weimann (2006); Cason (2010), and Friesen & Gangadharan (2013)).

This thesis contributes new insights into the impact of the combination of investment irreversibility and permit price uncertainty in the context of an intensity target mechanism. It extends Thomas' (2016) study of firms making reversible decisions about emissions intensity under an intensity target institution. Unlike the Thomas thesis, rather than comparing outcomes in intensity target and cap & trade regimes, this study considers only intensity target schemes, but introduces a fixed-price institution, contrasting this with outcomes in the double auction permit market. Additionally, the reversibility of firm technology choices is varied, to introduce irreversibility into this context. To align participant incentives with outcomes, their total earnings depend on their performance as firms in the experiment. Additionally, the experiment is presented using neutral framing, where participants are firms making input and output decisions to earn revenue. There is no mention of emissions permits or intensity targets. This is intended to avoid individual bias on the topic affecting results, and ensure the setting is closer to that of a simple, profit-maximising firm.

A key benefit of using the controlled laboratory approach in this case is that marginal abatement costs (and therefore the expected equilibria) are known with certainty. Given the experimental setup here, if firms are profit maximisers there is no reason, in theory, to expect differences in the pricing mechanism to lead to different outcomes in terms of emissions or technology choices. However, permit-price uncertainty may influence firm technology decisions if factors such as risk or ambiguity attitudes are relevant. Laboratory experiments allow for the testing of effects by changing one factor and holding all else constant. And, even though there may not be a defined theoretical rationale for behavioural responses, the experimental approach provides an opportunity to observe these.

In this experimental setup, firms interact in an emissions permit market, with permits required for output production. The output market context in this experiment is intended to loosely simulate that of the electricity sector. Firms are assumed to be monopolists with homogeneous products, as might be the case for electricity generators serving distinct geographic areas. To better understand how pricing mechanisms affect firm incentives to invest in lower-emission technologies, and thus the impact on overall emissions, two institution types are considered: a market-based regime and a fixed-price regime. Each of these represents a highly stylised version of a possible policy setting, and the main test here is whether the intensity decisions and emissions outcomes differ between regimes.

At the outset, all firms make a technology choice equating to their emissions intensity level. This decision is made relative to a target, which determines whether they receive permits when they produce output (if producing below the target), or need to purchase permits before they produce output (if producing above the target). If they choose an intensity rate equal to the target, then they do not need permits to produce output, and nor do they receive any permits. In the market-based regime, the permit price is determined by trading between firms in a market, and so depends on this interaction as well as the intensity and output decisions of the firms themselves. It is these decisions that will determine supply and demand. In order to motivate trade in emission permits in the market case, there are two firm types, differentiated by their cost structure. Clean firms have a lower marginal cost of abatement. As will be discussed in Section 4.2, incentives within the experiment are such that in equilibrium clean firms generate permits by choosing intensity levels below the target while dirty firms choose intensities above the target and require permits in order to produce output.

In the fixed-price regime, firms know in advance exactly how much their earnings will be for producing each unit of output, rather than having to wait and see what the permit price is. Under the fixed-price case, firms no longer trade for permits, but either receive a subsidy or pay a tax depending on whether they produce above or below the intensity target (in the same way that permits are calculated in the market case). This permit price is set by the 'regulator'. Altering the institution type in this way tests the impact of permit pricing mechanisms. When permit prices are determined in a double auction market the price is dependent on the supply of permits, in turn, a product of the technology decisions of clean firms. In the fixed-price case, however, permit supply is unlimited and permit prices are known with certainty. Though the market and fixed-price treatments are set up so optimal intensity and output decisions are equivalent, permit price uncertainty may mean outcomes differ in practice.

To detect differences associated with the nature of the investment, the intensity choices are either reversible, and made freely each period, or constrained in one direction, as with irreversible decisions. This is intended to represent the difference between a short-term and long-term investment (for instance the difference between retrofitting existing plant to reduce emissions and building new, cleaner plant) and test the difference that pricing certainty may make to economic incentives with different types of investment. Here, there is particular interest in understanding any difference that irreversibility makes to the decision. Again, predicted optimal decisions remain the same, but there may be behavioural differences in the presence of uncertainty. Participants may delay investment in cleaner production when the decision is irreversible, for instance. This might suggest transition to cleaner technologies could be delayed. The study will also test whether the impact of irreversibility in the investment decision varies with emissions price uncertainty. The effect of any interaction here could be of particular interest to regulators with specific policy goals.

Some explanation for behavioural effects may be deduced from the risk and ambiguity attitudes of individual participants. Risk here is defined as known possible outcomes with known odds, while ambiguity refers to the case where the probabilities of outcomes are unknown. These are measured using standard testing methods. For risk, the Holt and Laury (2002) assessment is utilised. Ambiguity attitudes are measured with a similar test, using an adaptation of the Ellsburg urn task (Ellsberg, 2000) from Kőnig-Kersting and Trautmann (2016).

Both attitudes could affect decision making under uncertainty, and increased uncertainty may "accentuate the perception of risk" (Ghosh & Ray, 1997). Within the experimental environment, a key source of uncertainty is permit pricing in the market-based treatments. Thus, we might expect risk and ambiguity attitudes to affect intensity choices in the market treatments in a different way to the fixed-price ones. Individuals with less tolerance for risk may be more inclined to choose an intensity level at the intensity target, since this eliminates any effect of price uncertainty on their earnings. Individual reactions to the irreversibility of the intensity choice may also depend on these attitudes since an irreversible intensity choice cannot be undone. Those with a lower tolerance for risk or ambiguity may be less inclined to choose lower intensity levels in the irreversible treatments, or may take longer to do so. Conversely, those with greater tolerance, may make bolder decisions.

1.5 Main Findings

The study found that when emissions permit prices were fixed, emissions were higher and closer to the predicted level compared to when prices were determined in a market. Emissions also tended to be lower when the intensity choice was irreversible. Differences in early period decisions were also detected, with participants in the market treatment with irreversible intensity choices tending to wait longer before choosing lower intensity levels.

1.6 Significance

The results offer some useful guidance for policy makers intending to use an intensity target to reduce emissions. Overall, it may be that specific targets are more likely to be achieved if a fixed price is used. This has the advantage, also, of reducing uncertainty about future costs for firms, which may facilitate investment decision making, depending, of course, on the credibility of the policy itself. Conversely, if a policy makers' goal is to reduce emissions, then market-derived pricing may be a more effective tool. Given Australia's history with a carbon tax, this option might already be preferable by policymakers.

Emissions also tended to be lower in the irreversible treatments. Additionally, irreversibility affected the pattern of technology choices, with the clearest difference being that firms tended to choose higher intensity levels early on and took longer to reduce them in the irreversible treatments.

Though the present study does suggest reversibility may alter responses to incentives, the setup does not afford the opportunity to examine how firms might choose between different investment options under uncertainty. Another study that addresses this is one possible extension. In an industry context where it might be optimal to make large, fixed investments in cleaner technology, for instance, risk-averse managers may respond to emissions price uncertainty by instead making smaller, reversible investments (even if this increases costs in the long run). If firms elect to make smaller-scale, reversible amendments to their technologies, rather than making long-run investments, this could contribute to a general delay in the transition to cleaner output.

The results also offer insight into possible interactions between individual attitudes toward risk and ambiguity and decisions made at the firm level. It may not be reasonable to assume managers are all risk neutral, and risk attitudes could well be amplified in the context of uncertainty, which will affect investment decisions and economic outcomes. This is also relevant in the case of permit trading, where participants tended to engage in extra trading that was not required for the production of output.

2 Literature Review

2.1 Environmental Policy and Investment Decisions

While a number of papers have attempted to rank environmental policies by their effectiveness in promoting any investment (see Downing & White (1986), for an early example, also Requate (2005) for an update of his rankings), another line of literature considers the interaction of policy choice with firm decisions about the type of investment. Much of this builds on the work of Weitzman (1974), which compared the effectiveness of either price or quantity measures. His main finding was that the answer depended on the relative slopes of the marginal abatement cost and marginal damages curves. However, factors to consider include not only cost uncertainty, but also firm heterogeneity and information asymmetry, since firms would have better information about their cost structures than regulators setting policy (Krysiak & Oberauner, 2010). By adjusting abatement efforts, firms are also able to adjust their costs, but the extent of this flexibility is dependent on their upfront investment decision.

Investments might be either flexible, representing end-of-pipe technologies that could be operated flexibly, or fixed, such as long-lasting production facilities. Krysiak (2008) found that a tax induced more investment in flexible technology, leading to socially suboptimal choices as compared to tradeable-permit regimes. Storrosten (2014), assuming endogenous cost structures, reported similar results. Where the technology choice is endogenous, tradeable permits appear to have a comparative advantage compared to taxes in incentivising optimal investments.

Storrosten (2015) considered outcomes in terms of optimal investment in technology. Taxes reportedly encouraged more flexible abatement tech if and only if the covariance of stochastic costs and equilibrium permit price was sufficiently strong and positive, compared with the variance in consumer demand for the goods being produced. D'Amato & Dijkstra (2018) found that setting policy before firms invested meant that quotas created greater investment incentive than taxes. However, with asymmetric information, tradeable permits may lead to less than optimal adoption of new technology compared to the use of taxes (D'Amato & Dijkstra, 2015).

This suggests policymakers may need to consider different policy settings depending on the sort of investment they wish to induce. Given that conditions can be expected to vary by industry, policymakers may wish to tailor different mechanisms to suit different sectors, with specific investment goals in mind. In testing the effect of altering the reversibility of investment, this experiment will offer some guidance on the impacts on short-term and long-term investments in the context of an emissions intensity target.

2.2 Irreversible Investments Under Uncertainty

Large-scale fixed investments can be considered irreversible in that payoffs are calculated over relatively long time frames and costs are effectively 'sunk'. The standard NPV approach to valuing investment options implicitly assumes that investments are reversible, however, or that the firm has no choice about the timing of the investment (Dixit & Pinkyck, 1994). The irreversibility of some investments presents particular risks with respect to future product prices and operating costs, interest rates, and the cost and timing of investment (Pindyck, 1991). And, even where there may be some resale value on the assets, the investment itself is irreversible.

It is this interaction between the irreversibility of certain investments, uncertainty and choice over timing that fundamentally alters the way firms may approach the investment decision. In short, uncertainty resolves through time, and investments can be postponed (List & Haigh, 2010). From this perspective, the opportunity to invest (where there is some choice), represents an option that is, according to Dixit & Pinkyck (1994), analogous to a financial call option. Under this framework, the option value must be added to the investment value, and its loss considered an opportunity cost in the calculation (Pindyck, 1991). If we assume investors are risk-neutral, an increase in the variability of the present value of its benefits would increase the value of an investment opportunity and thus affect the optimal timing of that investment (McDonald & Siegel, 1986).

The alternative method of calculating investment value utilises real option theory. Under this method, the investment project is recognised as a portfolio of complex “real options”, that is, options on real assets (De Giovanni & Massabò, 2018). The stability and credibility of macro policies may prove crucial given the central importance of uncertainty in these calculations (Pindyck, 1991). While increasing price variance may increase the opportunity cost of investing ‘now’, uncertainty over the cost of investment in the future may mean firms either postpone or bring forward their investment plans (Pindyck, 1991). Where firms have downside flexibility (they can reduce production output), but it is costly, there is greater incentive to invest sooner (De Giovanni & Massabò, 2018). More generally, since investments are made upfront but payoffs are dependent on future transactions for which prices are uncertain, inefficiency in investment may be likely (Mailath et al., 2004).

In weighing its investment decision, firms must consider the trade-off between scale and flexibility to adapt to conditions as uncertainty is resolved (Dixit & Pinkyck, 1994). Firms have volume flexibility when they can profitably produce at volumes that differ from installed capacity in response to demand changes (De Giovanni & Massabò, 2018). Firms may have upside or downside flexibility (or both), and this flexibility may include the ability to halt production altogether. However, if adaptability comes at a cost, the choice of optimal capacity affects the possibility of suspending production, where this option may form part of the optimal solution with relatively low capacity choices, but might be too expensive where firms have selected relatively large capacities. If production cannot be ceased altogether, the trade-off with respect to reducing the risk associated with a future market crash and being able to exploit a boom requires a much smaller production capacity. For electricity generators, for example, the question of downside flexibility is relevant. In this case, cheaper adjustment costs motivate larger capacity investment (De Giovanni & Massabò, 2018), with a consequent reduction in utilisation rates.

Environmental policies that impose a price on emissions represent an additional source of uncertainty for firms. Real option theory suggests that, under uncertainty, the availability of tradeable permits that afford firms the right to emit may incentivise them to delay irreversible investments in abatement technology (Chao & Wilson, 1993). In effect, there is a degree of substitutability between emissions permits and investment in abatement. As such, the price of permits in a cap & trade scheme would reflect an option value, as well as the marginal abatement cost. However, when considering the choice between a tax and cap & trade scheme, the flexibility offered by tradeable permits might help to maintain incentives for firms making irreversible investments (Zhao, 2003). This is because of the tendency for permit prices to change in line with demand and supply in the permit market, whereas taxes are fixed.

The incentives driving investment under a mechanism based on an intensity target may differ, however, since the supply of permits can move with changes in economic conditions. This may, in turn, reduce the effect of demand uncertainty on investment decisions. Relative to a cap & trade scheme, the use of an intensity target may limit the downside risk of adjustment costs, where emissions are higher than predicted (and firms have under-invested), or opportunity costs where emissions are lower than expected (and firms have over-invested) (Wing et al., 2006). At the economy-level, Marschinski and Edenhofer (2010) determined that an intensity target may be better able to promote long-term, fixed investments in abatement technology, where such a scheme was able to reduce uncertainty about abatement costs.

Much of the literature compares cap & trade schemes and taxes, or cap & trade and intensity target trading schemes, but another area of interest is in comparing tradeable permit and tax regimes both based on an intensity target. In a comparison drawing on a case study derived from the cement industry and based on a social-welfare maximising model, Almutairi and Elhedhli (2014) determined that an intensity-target-based tax compared favourably to an intensity target scheme with tradeable permits (as well as other policies). Although, in principle, a scheme that allowed for trading would entail somewhat higher social welfare, under an intensity target scheme this trading might not actually occur due to uncertainty about permit pricing. In this instance, the scheme effectively transforms into one based on mandatory caps, as in the absence of trade for permits all firms must produce at the mandated technology level. Almutairi and Elhedhli (2014) also determined that overall emissions were lower under an intensity target tax compared to an intensity target trading scheme. These findings are obviously sensitive to the parameters used. There is scope for further research into the relative effectiveness of either market-derived or fixed prices under an intensity target regime. This experiment is designed to offer insight here, also.

2.3 Literature from Economics Experiments

As with the theoretical literature, much of the experimental work also considers cap & trade schemes. Ben-David et. al. (2000) investigated irreversible investment decisions under uncertainty in the context of a cap & trade ETS. Participants in the experiment were either sellers or buyers in a market for emissions permits, and uncertainty was introduced in terms of the volume and timing of a decrease in permit supply. The paper predicted that risk-averse net sellers of permits would abate less than was optimal (under-invest), while net buyers would abate more (over-invest), resulting in an under-utilisation of the permit market. The results showed no such effect, however. Instead, the authors theorised that this may have been because of the irreversibility of the investment, which motivated potential buyers of permits to abate less and rely on the permit market until the outcomes were revealed and the uncertainty resolved.

Alternatively, Betz and Gunnthorsdottir (2009) proposed that the unexpected outcomes from Ben-David et al. (2000) were because the baseline condition involved price uncertainty, also, so the treatment intended to model uncertainty around the number of permits only increased this uncertainty. As such, Betz and Gunnthorsdottir (2009) adapted this experimental design to compare outcomes in a baseline (spot) market and treatment (forward) market, where participants could trade current and future vintage permits. As per theoretical predictions, the results of this experiment indicated that net sellers over-invested and net buyers under-invested, making less use of the market.

A large number of experiments have extended this research into investment decisions in ETS, but most address the case of a cap & trade scheme with an absolute cap, and results may not necessarily be useful in predicting outcomes where an intensity target is used. In comparing intensity target and cap & trade schemes, the theoretical prediction is that output and emissions will be higher under an intensity target, for a given price (Boom & Dijkstra (2009); Dewees (2001); Kuik & Mulder (2004)). The principal reason for this difference is the subsidy paid to firms for production at emissions intensity levels up to the target (as described in Section 1).

Buckley, Mestelman, and Muller (2006) set out to determine differences in abatement behaviour under cap & trade and intensity target institutions, with subjects considering trade-offs between short-run production and long-run capacity decisions. For an electricity generator, this would be the difference between electricity actually supplied period to period, and investment decisions to determine the capacity of its generation plant (and also the nature of this generation). The technological decision here concerned the emissions intensity rate (or, analogously, the amount of abatement), which was reset at the end of each period. The treatment of the investment decision as a reversible choice about the emissions intensity might equate to marginal decisions around the purchase of offsets or other interventions to temporarily reduce emissions, for instance, but does not model firm choices around irreversible investments. The results provided support for theoretical predictions that output would be higher under intensity target schemes, as opposed to cap & trade ones.

More recently, Thomas (2016) extended the work of Buckley, Mestelman, and Muller (2006) to include demand uncertainty, based on earlier work by Stranlund, Murphy, and Spraggon (2014). Thomas also compared outcomes for cap & trade and intensity target ETS where participants made period-by-period intensity decisions, but faced uncertainty in output demand conditions. Though parameterised to equalise emissions across treatments, Thomas (2016) reported that emissions and output were consistently lower than predicted under the intensity target treatments. This may be attributable to risk attitudes and responses to uncertainty, though this was not apparent from Thomas' testing of risk attitudes. Alternatively, the difference may be due to the way participants engaged with the two institutional designs.

It is also possible that attitudes toward uncertainty may be more relevant than those toward risk. In this experimental design, risk pertains to the known probability of the revenue state being either 'high' or 'low', whereas the price of permits (as well as their supply in the intensity target ETS) is unknown and so the degree of uncertainty is ambiguous, rather than risky. One way this experiment extends on Thomas (2016) is a test for ambiguity attitudes to attempt to detect any relationship here. This experiment will offer further insight into investment incentives under intensity target regimes, testing the effect of uncertainty about emissions pricing by altering the pricing mechanism. This sort of comparison was also made by Jones and Vossler (2014), where uncertainty in a water-quality trading scheme experiment was introduced by altering abatement requirements in each period. Participants played a series of one-shot games, making irreversible investments to determine their technology level, and payoffs were determined by subsequent trading in permit markets. This experiment found that fixed prices were associated with more optimal investment decisions, as compared to the use of tradeable permits. In the base case for the experiment described in this thesis, participants face uncertainty about pricing and supply of permits. In the fixed-price treatments, this uncertainty is eliminated.

3 Theory

3.1 Theoretical Setting

In this experimental setting, I start with an emissions intensity target with tradeable permits, the price of which is determined in a market. This mirrors Thomas' (2016) experiment examining differences in outcomes between the intensity target and cap & trade ETS where intensity decisions were reversible.

Motivating this setup, the theoretical model presents the firm-level profit-maximisation problem. The regulator sets an intensity target, intended to achieve an emissions reduction goal. Firms, in turn, are assumed to be monopolists in their output markets, but compete for emissions permits in a double auction market. This broadly represents the case of geographically dispersed firms serving independent markets that must purchase emission permits from a centralised market. These permits are required to produce output. Firms must make an upfront decision about the intensity of their production, and permits are generated by those firms that produce output at an intensity level below the target. The number of permits generated or required thus depends on the difference between the firm's intensity choice and the target, as well as total output. Output demand is uncertain, represented as either a high or low demand setting, and unknown prior to making this intensity decision. When the investment decision is irreversible, firms are only able to move to cleaner production, and cannot go dirtier again.

This thesis also considers the case where the cost of permits is certain. In the fixed-price treatments, firms no longer trade for permits, but pay a tax on output produced at an intensity rate above the target, and receive a subsidy on production below this target. This intends to illustrate any differences in investment or production decisions that occur with the elimination of price (and supply) uncertainty that comes with the trading of permits.

There are two firm types differentiated by their marginal abatement cost (MAC) schedules. Clean firms have relatively flat MAC schedules. It is cheaper for these firms to abate emissions. Dirty firms have steeper MAC schedules, and so face higher costs to reduce emissions. Participants are presented with net marginal revenue schedules, exclusive of any permit costs. These reflect decreasing demand for output and constant marginal costs of production.

The setting necessarily abstracts from many real-world features in order to focus on the association between the incentives under consideration and outcomes of interest. As such, this experiment imposes full compliance, so that firms can only produce output if they have the necessary permits, and can only sell permits they have in hand. There are no penalties for failing to acquire the necessary permits to produce, because it is simply not possible to do this. Nor is it possible to produce at a different intensity level than is reported. No banking or borrowing of permits is permitted either, as, in theory at least, this is not necessary for optimisation. Bankruptcy is also prevented so participants can't leave the experiment owing the organisers money. Finally, it is assumed that the social planner sets an optimal intensity target that will achieve the policy goal.

3.2 A Theory of Intensity Targets

The theoretical model is based on the static stochastic emission permit trading model presented in Thomas (2016). This study was, in turn, based on work by Stranlund et. al. (2014), extended by borrowing from Buckley et. al. (2006) to incorporate emissions intensity decisions.

Though firms make decisions over time, a static model is appropriate. It is assumed the regulator sets the intensity target to realise a given level of expected total emissions. It is possible to show that permit markets clear each period (Section 3.3), with net demand for permits zero at equilibrium. It follows, also, that the firm's optimal intensity decision is unaffected by the demand state, and so will be the same in every period. As such, expected permit prices will be constant over all periods, warranting a static model.

The regulator sets an economy-wide emissions intensity target r^T which defines the targeted rate of emissions associated with each unit of output produced. The higher r is, the more emissions are generated with each unit of output produced. As such, a higher r is associated with dirtier production technology. Firms decide the emissions intensity at which they produce output, $r_{i_{t,u}}$ prior to the revelation of industry-wide stochastic shocks to demand $u_t \in [low, high]$. The probability of either output demand state occurring is equally likely in this setting. This means that expectations about demand for firm output in the future, as well as expectations about the price of emission permits, will affect decision making. When the intensity choice is irreversible $r_{t-1} - r_{i_{t,u}} \geq 0$, because firms can only either maintain the same intensity level or choose to invest in a cleaner technology with a lower emissions intensity.

Following the revelation of the demand state, firms choose the quantity of output $q_{i_{t,u}}$ to produce. Total emissions per firm are found by multiplying the emissions intensity rate chosen by the firm by the number of units of output produced, $e_{i_{t,u}} = r_{i_{t,u}} * q_{i_{t,u}}$. Industry output is $Q_{u_t} = \sum_{i=1}^{N} q_{i_{t,u}}$, and aggregate emissions are $\Omega_{u_t} = \sum_{i=1}^{N} r_{i_{t,u}} * q_{i_{t,u}}$. The regulator sets the intensity target with the intention of achieving some targeted level of emissions. No absolute cap on emissions is directly specified. In practice, the choice of emissions intensity target is based on the estimated marginal abatement costs for firms in an industry, and the actual results depend on the intensity and production decisions of firms.

The firm's cost of production is a linear homogeneous function of output and intensity, as per Thomas (2016):

$$C_{i_t} = C_{i_t}(q_{i_{t,u}} e_{i_{t,u}}) = q_{i_{t,u}} * C_{i_t}(1, r_{i_t}) \tag{1}$$

Unit cost is separated into unit capacity cost $c_{i_t}(r_{i_{t,u}})$, a positive and declining function of intensity, and unit variable cost w_{i_t}, a constant function of output. Total cost is:

$$C_{i_t} = c_{i_t}(r_{i_{t,u}}) * q_{i_{t,u}} + w_{i_t} * q_{i_{t,u}} \tag{2}$$

Marginal cost of output is $\frac{\partial C_{i_t}}{\partial q_{i_{t,u}}} = c_{i_t}(r_{i_{t,u}}) + w_{i_t}$ and marginal abatement cost is $\partial C_{i_t}/\partial r_{i_{t,u}} = c^{'}_{i_t}(r_{i_{t,u}})$.

Under the market setup, a firm's net demand for permits equates to the number of units of output multiplied by the difference between its chosen emissions intensity level and the mandated emissions intensity target, or $(r_{i_{t,u}} - r^T) * q_{i_{t,u}}$. Net demand will be negative if the firm chooses an intensity level below the mandated target, leading to the creation of permits that can be sold to firms producing at an intensity above the targeted level (which have a positive net demand). The price of each emission credit, P_u^M, is determined by the net supply and demand for permits overall (implemented in a double auction in the experiment). This theoretical equilibrium price is used as the tax (subsidy) rate in the fixed-price treatment leading to the same expected outcome in both the market and fixed-price treatments, as shown in Section 3.4.

Revenue is determined by the quantity of output produced by the firm and the price of output, which depends on demand for output. Output prices are higher in the high demand state and lower in the low demand state. Costs are determined by the choice of intensity and price of permits. The firm's profit maximisation problem is as follows:

$$\begin{aligned} max_{r_{i_{t,u}},q_{i,u}} E_u[\pi^M_{i_{t,u}}] = \\ E_u[P_{i_{t,u}}(q_{i_{t,u}}) * q_{i_{t,u}} - c_{i_t}(r_{i_{t,u}}) * q_{i_{t,u}} - w_{i_t} * q_{i_{t,u}} - P_u^M * q_{i_{t,u}} * (r_{i_{t,u}} - r^T)] \end{aligned} \tag{3}$$

Where E is the expected value operator, all possible demand states are known, and expectation is over all possible demand states.

In the irreversible case, firms can only reduce their intensity level, $r_{i_{t,u}} \leq r_{i_{t-1,u}}$. All production decisions are assumed to involve quantities which are non-negative, ie, $q_{i_{t,u}} \geq 0$.

From (3.2), the first order conditions for an interior solution defining the optimal choice of emissions intensity, output, and therefore emissions are:

$$-E_u[c_i^{'}(r^M_{i,t,u}] = E_u[P_u^M] \tag{4}$$

$$E_u[P'_{i,u}(q^M_{i,u}) * q^M_{i,u} + P_{i,u}(q^M_{i,u})] = E_u[c_i(r^M_{i,u}) + w_i + r^M_{i,u} * P^M_u - r^T * P^M_u] \quad (5)$$

Equation (4) solves for the optimum intensity level, illustrating the familiar equalisation of marginal abatement costs with expected permit prices. Equation (5) is the zero marginal profit condition that determines optimal output, in combination with the emissions intensity choice of the firm. In this equation marginal revenues (the left-hand side) are equal to marginal costs (the right-hand side). In addition to the marginal cost of production and the cost of permits purchased on the right-hand side of Equation (5), there is also a negative term denoting an effective production subsidy proportional to the emissions intensity target. Firms with intensities below the target face a reduction in costs because they generate permits which can then be sold in the permit market (or receive a subsidy in the fixed-price case).

3.3 Market Clearing in the Market for Permits

Market clearing in the emission permits market implies that net demand for permits in equilibrium is zero. Taking the optimal decisions of all firms in the market described in Equation (4), where clean firms produce below the target and dirty firms above, and summing for all firms implies that total emissions by all firms in the market are equal to the emissions that would be produced if all firms produced at the intensity target:

$$\sum_{i=1}^{N} r^M_{it,u} * q^M_{it,u} = \sum_{i=1}^{N} r^T * q^M_{it,u} \quad (6)$$

Firms make the intensity decision prior to the revelation of the demand state $u_t \in [low, high]$, and since this is determined by (4), it follows that $r^M_{it,u} = r^M_{it}$ regardless of output demand state. Equation (5) therefore reduces to the following:

$$-c_i(r^M_{i,u}) = -c_i(r^M_i) = E_u[P^M_u] = P^M \tag{7}$$

Equation (7) also implies that the optimal intensity level will be constant across periods. As such, firms are expected to optimise by selecting the optimal intensity level immediately, without adjustment over time, leading to the first hypothesis to be tested.

Hypothesis 1:

Intensity decisions are the same regardless of reversibility.

When the intensity choice is irreversible, in each period firms can either choose to remain at the same emissions intensity, or elect to move to a lower emissions intensity. They cannot move back to a higher level. However, regardless of whether this restriction is imposed, the optimal choices at equilibrium remain the same since the same intensity decisions are predicted across all periods. The experimental design permits the observation of possible behavioural responses to variations in reversibility, however.

Upon realisation of the demand state, firms make output decisions. In the low demand state, net marginal returns from production are lower than in the high state, and so firms produce less output. This means that when the demand state is low (high), clean firms produce fewer (more) units of output and supply fewer (more) emissions permits, while dirty firms also produce fewer (more) units of output and demand fewer (more) emissions permits.

Regardless of the demand state, in equilibrium, the production decisions of clean firms will lead to the generation of permit supply sufficient to satisfy the demand of dirty firms. As such, intertemporal optimisation over the experiment is not necessary and, therefore, no banking or borrowing of permits is required.

An additional implication of Equation (7) is that the expected price of permits will be the same across periods, also. This means that equation (5) becomes:

$$E_u[P'_{i,u}(q^M_{i,u}) * q^M_{i,u} + P_{i,u}(q^M_{i,u})] = c_i(r^M_i) + w_i + r^M_i * P^M - r^T * P^M \tag{8}$$

It is possible to show that the difference between the intensity level selected and the target is equivalent to marginal net benefit of output per dollar spent on emission permits.

$$r^M_i - r^T = \frac{E_u[P'_{i,u}(q^M_{i,u}) * q^M_{i,u} + P_{i,u}(q^M_{i,u})] - c_i(r^M_i) + w_i}{P^M} \tag{9}$$

As such, output production decisions depend on each firm's cost structure. Clean firms will have financial incentives to produce below the target, resulting in the supply of permits to the market. Dirty firms, however, will have a clear advantage in producing above the target, creating demand for permits.

3.4 No Market for Permits

Also consider the case in which instead of a market for emission permits, the price of permits is fixed and set by the regulator, applied in the form of a tax and subsidy, and there is no limit on the number of permits that can be bought or sold. Equation (4) becomes:

$$-E_u[c'_{i_t}(r_{i_t})] = P^F \tag{10}$$

With P^F set equal to P^M there is no difference in optimal marginal cost and therefore no change in the optimal emissions intensity choice for the firm. This leads to the second hypothesis to be tested by this experiment.

Hypothesis 2:

Firms make the same intensity decisions in the market and fixed-price treatments, where emission prices are set at the predicted equilibrium permit price ($P^F = P^M$).

Again, optimal decisions are equivalent in both settings. From the firm's perspective, however, the price signal may be more immediately apparent in the fixed-price case (as compared to the market-derived price) as this price is known with certainty. This may elicit different responses in practice. In a market setting, also, price convergence is likely to take some time.

4 Experiment

In order to test the hypotheses of this thesis, an appropriate experimental setup is established that includes participants acting as firms to make intensity and production decisions. As described by Wilde (1981), laboratory experiments create micro-economies that allow for the control and accurate measurement of variables in question. Here, the primary interest is in the interaction between pricing mechanisms and investment decisions, as represented by the intensity choice. The setup is intended to reveal impacts on investment patterns and emissions outcomes.

The experiment includes participants acting as firms. Framing is neutral, with participants invited to engage with the experiment as firms making input and output decisions in order to earn revenue. The use of more realistic framing, using terms such as "emissions permits" and "pollution", could alter induced values (Smith, 1976) and so lead to a loss of control as participants make decisions based on their feelings about these issues, rather than the explicit economic incentives that firms would be expected to respond to (Alekseev et al., 2017). It has been demonstrated that environmental framing could elicit different responses (Cason & Raymond, 2011), and avoiding explicit language is common in environmental economics experiments. Participants trade 'inputs', rather than emissions permits, and decide on an 'input ratio' (the number of inputs required to produce a unit of output), rather than an emissions intensity.

For similar reasons, payoffs are determined by the revenue earned in the experiment, aligning participant preferences within the context of the experiment and ensuring salience of rewards, by tying rewards to outcomes (Wilde, 1981). In the case of the market sessions where input (permit) prices are determined via an interactive market, outcomes will be determined, to some extent, by the actions of others, as well as participants themselves. In the fixed-price sessions, however, there is no permit market and so optimal participant decisions are independent of others' choices. Privacy around reward payments is also important to avoid participants making comparisons between themselves, which might impact on incentives if, for instance, participants value equity (Wilde, 1981). As such, payments for this experiment are kept confidential.

Participants make three key decisions in the experiment. They are informed of a free input ratio, equivalent to an emissions intensity target, and decide on an input ratio at which they will produce. The difference between their choice of ratio and this free level determines whether they receive inputs when they produce output, or need to purchase inputs in order to produce output. Participants also make decisions on the production of output and trading of inputs. Output is produced by clicking on a button, and participants interact in a double-action market to buy and sell inputs. They make revenue by producing output and selling inputs. Some participants must purchase inputs before they can produce output, while the others can produce first, then sell the inputs received.

4.1 Treatments

The experiment was programmed and conducted with the economics experiment software z-Tree (Fischbacher, 2007). The experiment is a 2x2 design, with two institution types and two investment types, as in Table 1. If there were 6 sessions of each treatment, to ensure a minimum number of data points for analysis, this would mean running 24 sessions in total. Due to delays and difficulties recruiting participants, however, only 3 sessions of each treatment have been run for this project.

Table 1: 2x2 Treatment Design

Pricing	Reversible Intensity Choice	Irreversible Intensity Choice
Market	1	2
Fixed-Price	3	4

4.2 Experimental Parameterisation

The experimental parameterisation that follows is based on that of Thomas (2016), with a few modifications. The target intensity used in this setup is 2.

4.2.1 Market Pricing

Participants start the experiment with an endowment of L$250 (lab dollars, to be exchanged for Australian dollars at a rate of L$85/AUD and L$60/AUD for clean-type and dirty-type firms, respectively) (the endowment was L$1,000 for the practice rounds). Different conversion rates are used to account for differences in profit outcomes between firm types due to the parameterisation of the experiment. (See Appendix A for more on payments.) Uncertainty about the demand state (revealed in the second stage) is defined by random shocks to marginal revenues, defined in Table 2 as either the Low or High state.

Table 2: Marginal Revenue Schedule, by Demand State

Output	Low	High
1	148	188
2	138	178
3	128	168
4	118	158
5	108	148

Costs for each firm type are determined based on the abatement cost function, defined as in Buckley et al. (2006):

$$c_i(r_{i,u}) = u_{0,i} + (u_{1,i} - u_{0,i})[\frac{r_{max} - r_{i,u}}{r_{max}}]^\alpha \tag{11}$$

Here $u_{0,i}$ and $u_{1,i}$ are firm-type-specific constants which define the bounds of the function, and α is a convexity parameter, the same for all firms. Intensity ($r_{i,u}$) can take the value 0-5.

In Thomas (2016), the baseline & credit case was parameterised to equalise the predicted emissions with those under the cap & trade case she was also considering. This meant setting the target emissions intensity ratio to 2 and generated a predicted price of L$28. In the interests of comparability, this experiment retains these settings.

As the equation has been converted into a discrete version, it is not a simple matter to solve for the equilibrium. Thomas (2016) used the difference in firm cost structures to identify a unique profit-maximising emissions intensity and output level for each firm type in each period.

The solution involves an iterative (and somewhat circular) process. One starting point is to solve for the optimal intensity level via the first order condition as in Equation (4) $-E_u[c_i^{'}(r_{i,t,u}^M] = E_u[P_u^M]$. Where the abatement cost function is as in Equation (11) $c_i(r_{i,u}) = u_{0,i} + (u_{1,i} - u_{0,i})[\frac{r_{max}-r_{i,u}}{r_{max}}]^\alpha$.

Taking the derivative of the abatement cost function with respect to r (intensity), using the values in the table below and setting the permit price at L$28 allows for the derivation of optimal intensity choices for each firm type, which are 1 for clean firms and 3 for dirty firms (rounded).

Table 3: Cost parameters for determining MAC

u0		u1		a	rmax
Clean	Dirty	Clean	Dirty		
88.5	74	182	380	3	5

The u0 value for clean firms has been altered slightly from Thomas (2016) (88.5 as opposed to 88). This does not alter equilibrium predictions, but allows for a clearer distinction to be made between net marginal revenue outcomes in the fixed-price treatments (rounded to the nearest integer, the net marginal revenue for clean firms producing at intensity 1 or 2 appear the same at a fixed price under the original parameterisation).

Conversely, it is also possible to start by determining the range of possible permit prices associated with each intensity level. For example, for clean firms, if $0.5 < r < 1.5$, then the intensity level is 1, rounded to the nearest integer. This equates to a price band of L\$27.70-L\$45. For dirty firms, if $2.5 < r < 3.5$ (equating to an intensity of 3), the price band is L\$16.70 to L\$45.50. Therefore, a price range of L\$27.70-L\$45 would mean both firm types produce at their predicted intensity levels. The lower bound is given by the permit price L\$27.70, since a lower price would mean clean firms are better off producing at the target intensity rather than an intensity level of 1. This would collapse the market as there would be no supply of permits.

A net marginal revenue schedule was generated using the equation $MR_u - c_i(r_{i,u})$ based on equation (2), where unit variable cost w_{i_t} was assumed to be zero. This continuous form was converted into a discrete version for the purposes of the experiment, with all values rounded to the nearest integer.

Net marginal revenue schedules are presented to each participant. The schedules differ by firm type, as detailed in Tables 4 & 5.

Table 4: Net Marginal Revenue for Clean Firms

Intensity (r)	0		1		2		3		4		5	
Output	Low	High	Low	High	Low	High	Low	High	Low	High	Low	High
1	-34	6	12	52	39	79	54	94	59	99	60	100
2	-54	-14	-8	32	19	59	34	74	39	79	40	80
3	-74	-34	-28	12	-1	39	14	54	19	59	20	60
4	-94	-54	-48	-8	-21	19	-6	34	-1	39	0	40
5	-114	-74	-68	-28	-41	-1	-26	14	-21	19	-20	20

Table 5: Net Marginal Revenue for Dirty firms

Intensity (r)	0		1		2		3		4		5	
Output	Low	High	Low	High	Low	High	Low	High	Low	High	Low	High
1	-232	-192	-83	-43	8	48	54	94	72	112	74	114
2	-252	-212	-103	-63	-12	28	34	74	52	92	54	94
3	-272	-232	-123	-83	-32	8	14	54	32	72	34	74
4	-292	-252	-143	-103	-52	-12	-6	34	12	52	14	54
5	-312	-272	-163	-123	-72	-32	-26	14	-8	32	-6	34

The change in net marginal revenue for a given output level across intensity levels for each firm type produces the intensity margin, graphed in Figure 1. Note that the predicted permit price intersects the curves at the predicted intensity level for each firm type. Thomas' reduced form (as here) incorporates the work of both Stranlund, Murphy, and Spraggon (2014) and Buckley, Mestelman, and Muller (2006).

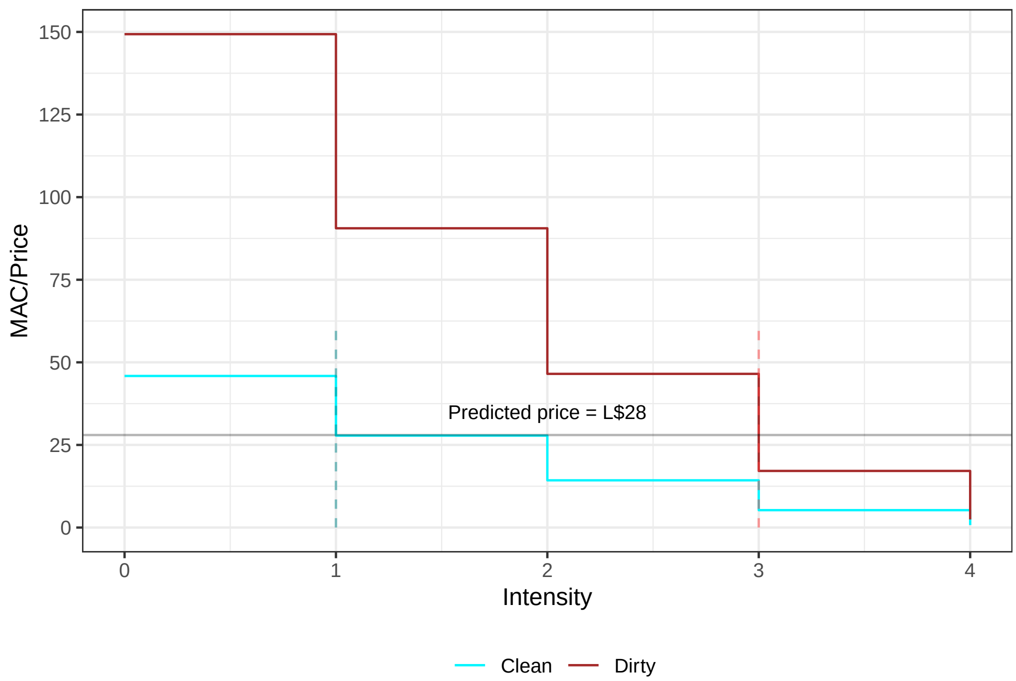

Figure 1: Intensity Margin by Firm Type. Steeper MAC Curve for Dirty Firms Reflects Higher Cost of Abatement Relative to Clean Firms.

If, for example, both firm types produce at the target intensity level of 2, clean firms can produce either 2 or 4 units of output (in the low or high demand state), earning L$58 and L$198 (with reference to Table 4). Dirty firms can produce 1 or 3 units to earn L$8 and L$84 (with reference to Table 5). However, in this case no firm would demand or supply permits, and so no market for permits would occur.

If there is to be trade, there must be some production above and below the target. Clean firms have a clear advantage to produce below the target (based on their net marketing revenue schedule), benefiting from the sale of permits. This creates market supply. Conversely, dirty firms have a financial incentive to produce above the target, creating demand for permits. The relative advantage of each firm type is apparent from Tables 4 and 5. For instance, the permit price would have to be at least L$43 for a dirty firm to produce even one unit of output in the high demand state at an intensity ratio of 1, whereas clean firms can earn a profit producing at this level even if the permit price were zero.

Where one firm is creating permit supply, this must be matched by demand in order for the market to clear. If a clean firm produces a unit of output at an intensity level of 1, creating 1 emissions credit (based on the difference between the intensity choice and the target rate of 2), then a dirty firm could purchase this permit and produce one unit of output at an intensity level of 3. If clean firms produce at an intensity level of 1, but dirty firms produce at an intensity level of 4, for instance, there will be excess demand in the market for permits, since dirty firms will need 2 permits per unit of output, but clean firms are only producing 1 permit per unit of output.

Assuming the permit price is L$28, the optimal intensity rate can be solved for as described above. For clean firms this gives an intensity level of 1.468 and for dirty firms 3.047. This corresponds to the predicted intensity levels of 1 and 3, when rounded. Working from the other direction, intensity choices of 1 and 3 imply possible price bands of L$27.70 to L$45 for clean firms and L$16.70 to L$45.50 for dirty firms. The iterative process continues to determine the output decisions and narrow down possible price bands.

From Tables 4 and 5, it is clear that clean firms will be willing to produce the first two units of output under a low demand state, if producing at an intensity of 1, at any permit price. They will also produce the third unit if the permit price is more than L$28 (so that they earn profit on this unit). However, dirty firms (at intensity level 3) will only be willing to produce a third unit of output in this case if the permit price is no more than L$14. Thus, this third unit will not be produced. Via a similar process it is evident that clean firms will produce a fourth unit in the high demand state so long as the permit price is more than L$8 and a fifth unit if it is more than L$28. Dirty firms will produce the fourth unit in the high-demand state if the permit price is no more than L$34, but will only produce the fifth if it is no more than L$14.

Taking all the above into account narrows the possible price band to L$27.70-28. This is because a price above L$28 would lead to an oversupply in the permit market, bringing prices down. A price below L$27.70 would see clean firms produce at an intensity level of 2 rather than 1, collapsing the market. Though technically there is a range of possible prices, L$28 is the predicted price, rounded to the nearest integer. At the predicted equilibrium, clean firms will produce either 2 or 4 units of output, depending on whether the demand state is low or high, and earn either L$60 or L$200. Dirty firms will also produce either 2 or 4 units, and earn L$32 or L$146.

4.2.2 Fixed-Price

For the fixed-price case, the essential set up is identical, where the price of permits is set at L$28, the predicted equilibrium permit price in the market setting. It is possible that clean firms may produce one extra unit in each of the low and high demand settings compared to the market case, however, as these have a zero net marginal revenue (given the L$28 permit price and assuming they produce at an intensity level of 1, as predicted). There is no financial incentive to produce the extra but, unlike in the market setting, there is no restriction on the generation of permits so some participants may choose to do this. This could potentially result in higher emissions compared to the market case.

The absence of uncertainty over the price may lead to differences in investment decisions, also. There may be a tendency to delay investment decisions in the face of abatement pricing uncertainty, particularly when making irreversible investment decisions (Ben-David et al., 2000). Conversely, in the absence of financial risk associated with uncertain permit prices, investment decisions may be closer to the optimum (Jones & Vossler, 2014). Reversibility of technology choices may also influence outcomes as participants may prefer to make incremental investments to test outcomes as they go.

4.3 Predictions and Additional Hypotheses

Based on predicted decisions by participants, dependent on their firm type and the demand state, the experiment predictions are presented below. Sessions ran for 12 periods, with equal numbers of high and low demand states (though the sequence of demand states varied somewhat). In line with the theoretical model presented earlier, there are no differences predicted between treatments.

Table 6: Experiment Predictions (for all Treatments)

Variable	Prediction (all periods)	Adj. Prediction
Average Output per Period per Participant	3	3
Total Output	288	192
Total Emissions	576	384
Mean Intensity - Clean	1	1
Mean Intensity - Dirty	3	3
Permit Price	28	28

Note:
Adjusted to exclude first 4 periods

Predictions are based on the demand states (whether high or low) and number of periods in the experiment, and as such are calculated after the fact. Typically, participants will take some time to get used to the experimental environment, and as such early periods may be characterised by learning effects as they make adjustments. For this reason, price and volume data from the market treatments can be examined to determine a truncated selection of periods to utilise for the analysis.

It is worth noting, however, that this period of adjustment is quite different in the irreversible case, as movement is constrained in one direction. Any difference in patterns of behaviour are of particular interest here, so cutting periods out of the analysis is not actually desirable, although comparing decisions in early versus later periods may offer some additional insights. For this purpose, the first 4 periods have been identified as 'early', where there appears to generally have been some settling in price movements after the 4th period. For a full description see Appendix B.

Truncation would not affect intensity or price predictions, but would reduce predictions for output and emissions, as in Table 6.

Two additional hypotheses extend from the above.

Hypothesis 3: *Emissions outcomes will be the same for all treatments.*

Since intensity decisions are predicted to be the same, based on the experimental setup then output decisions would also be expected to be the same. Taken together, this means that emissions outcomes would also be the same.

Hypothesis 4a: *Prices are the same in all treatments.*

Hypothesis 4b: *Prices in the market treatments converge to the predicted equilibrium.*

Since other outcomes are predicted to be the same regardless of reversibility, prices are not expected to vary across the two market treatments, and nor are they predicted to vary from the fixed-price treatments.

5 Experimental Procedure

Participants were recruited from the Curtin student body using the ORSEE recruitment server (Greiner, 2003). (See Appendix A for details.) In total, 96 participants were recruited to undertake 12 sessions (3 of each treatment). Of these participants, 51 were women, 43 men and 1 nonbinary (1 did not answer); 28 participants were in their first year of study, 26 in their second year, and 14 in their third year, with the rest either at higher year levels or staff, or choosing not to answer. Only two indicated that they had prior experience with economics experiments. Of our participants, 21 indicated that their main area of study was economics or finance. There were also 21 from health, 19 from other commerce majors, 13 from engineering, and the remainder from humanities, social sciences and 'other'.

Table 7: Participant Characteristics by Treatment Group

	Market		Fixed-Price	
Participants	Reversible	Irreversible	Reversible	Irreversible
Male	38%	46%	42%	54%
Economics	8%	21%	33%	25%
Health	25%	29%	17%	17%
Commerce	17%	17%	21%	25%
Engineering	13%	17%	17%	8%
First Year	12%	21%	12%	17%

This experiment received approval from the Research Ethics Office at Curtin, Approval no. HRE2019-0201. The research was supported by an Australian Government Research Training Program (RTP) Scholarship, and payments for participants

were funded by the Curtin Experimental Economics Lab (CEEL).

5.1 Setup

Each session ran with 16 participants, with two treatments run in the lab at the same time. At the outset, participants were assigned randomly to a computer station, which meant they were randomly assigned to a treatment and also to one of two firm types (ensuring 4 of each firm type), to control for possible selection bias (Friedman and Sunder, 1994).

In each period, high and low demand states were equally likely and determined by a random draw from a uniform distribution. This sequence was predetermined and programmed into z-Tree for each session so that there were the same number of high and low periods in each.

If participants are able to anticipate when the session will end, this could affect decision making. To avoid the possibility of end-game effects, random processes were also used to determine the number of periods. Participants were informed that sessions were programmed with 10 periods with certainty, but after that, every period had a 1/6 chance of being the last. This was determined by a random draw from [1,2,3,4,5,6], where drawing 1 would end the session. Because of the possibility of either very short or extremely long sessions eventuating if the software determined the number of periods, these schedules were also determined prior to the start of the sessions. Although the session lengths were pre-determined, they appeared random and were unknown to participants until the experiment had concluded. Each session ran with 12 periods.

Participants were asked to wait outside the lab and once enough had arrived, their names were signed off on the sign-up sheet and they were given an information form to read, and consent form to sign. They were asked whether they had any questions before proceeding. Any extra participants who arrived were also marked off the sheet and given a A$5 on-time show-up fee and dismissed. Others who did not arrive were marked as no-shows in ORSEE.

Before entry to the lab, participants took a card out of a box to determine which computer terminal they would sit at. This card was also used to match them up with their correct payment at the end of the session. Computer stations were each shielded with cardboard dividers to reduce the possibility of participants looking at each other's decisions. To make this even more unlikely (or unhelpful) treatments were also alternated across the computers in the lab, so that nobody was sitting directly next to anyone in the same treatment as themselves. On sitting at their assigned station, participants were asked to put on their headphones.

Prior to starting the experiment instructions, participants completed the Holt-Laury test and then the ambiguity-assessment task (see Appendix D), for which participants received additional payments at the end of the experiment.

A guided tutorial explaining how to use the experiment software and 6 practice periods followed. The tutorial was run with a series of narrated slides detailing instructions for the experiment delivered to each participant's computer station. The practice rounds included a very similar set up as would be seen in the real experiment, but with different numbers in the revenue tables (to avoid participants simply transferring strategies from the practice run to paid sessions). Participants were encouraged to try out different things to see what would happen. Participants were informed that the outcomes of these practice periods had no bearing on final payoffs.

At the end of the session, totals displayed on the screens of each participant were checked to ensure payments were correct as in the system. After this a short, optional demographic survey was run to collect some basic information about participants. Then participants were called up to the front of the room, one by one, to collect their payment in private.

5.2 Decision Process

Participants were informed when the paid experiment was to begin. In the first stage of each period, all participants chose the input ratio they would produce at. This would either be a reversible or irreversible decision, depending on the treatment group. A customised calculator feature was provided so that participants could test the impact of possible input prices on their revenues before moving to the next screen.

In the second stage, the demand state was revealed, and participants engaged in the input market to buy and sell inputs, and make production decisions. As per Figure 2, in the market treatments, the way this process unfolded was dependent on the actions of the different firm types. Firms that selected an input ratio below the free ratio level (predicted to be clean firms) needed to produce output (and thus generate inputs to be traded) before firms that selected a higher input ratio (predicted to be dirty firms) were also able to produce output. If not, there was no trade. However, those selecting intensity ratios above the target could enter bid prices for permits they wished to purchase. For fixed-price treatments, this was no longer the case since there was no requirement to generate inputs before they could be used (Figure 3).

Another calculator tool was available here in the market treatments. By entering an input price and clicking, participants could calculate their total earnings for each unit of output for this given price.

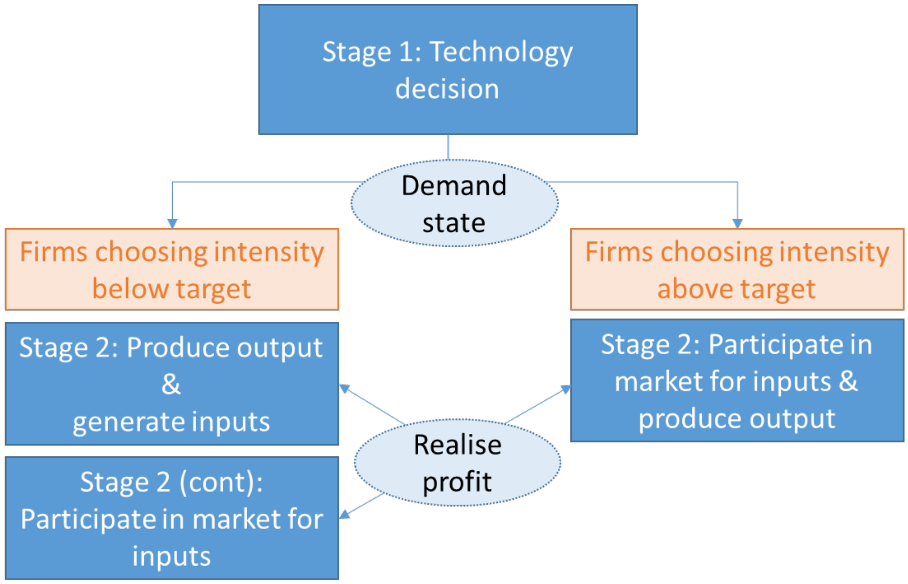

Figure 2: Decision Process for Participants in Market Treatments

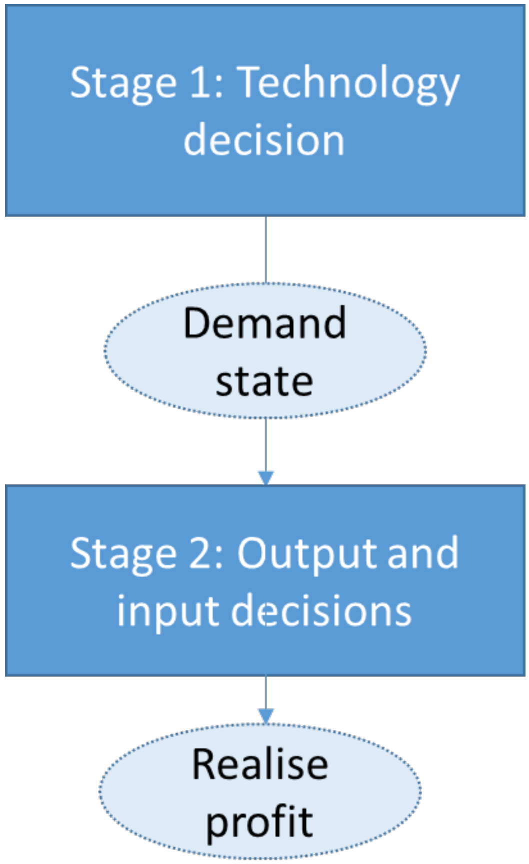

Figure 3: Decision Process for Participants in Fixed-Price Treatments

6 Main Results

Table 8: Summary Statistics for all Periods

Variable	Prediction	Fixed		Market	
		Reversible	Irreversible	Reversible	Irreversible
Intensity					
Clean	1	1.4*	1.5	1.6*	1.7*
		(0.07)	(0.08)	(0.1)	(0.11)
Dirty	3	2.9‡	2.1*†	2.2*	1.7*†
		(0.04)	(0.1)	(0.09)	(0.12)
Output					
Clean	36	41*‡	39‡	30*	24*
		(1.52)	(1.5)	(4)	(2.7)
Dirty	36	35‡	24*†‡	20*	9*†
		(1.66)	(3.78)	(2.39)	(2.78)
Total	288	304‡	253*†‡	201*	134*†
		(10.69)	(15.81)	(11.57)	(19.62)
Emissions					
Clean	36	58*‡	55*	40	36
		(4.39)	(7.94)	(6.29)	(5.89)
Dirty	108	100‡	67*†‡	47*	19*†
		(4.51)	(12.95)	(7.73)	(7.22)
Total	576	632*‡	489†‡	349*	220*†
		(17.07)	(52.22)	(30.17)	(29.76)

Note:
For all periods
Individual means for clean and dirty (96 obs)
Session totals for output and emissions results (3 obs)
Standard errors in parentheses
Two-tailed Wilcoxon and permutation tests used
* Significant difference from prediction
† Significant difference from reversible treatment
‡ Significant difference from market treatment

Table 8 provides an outline of results for all periods. Intensity choices for both firm types are significantly different from predictions (on two-tailed Wilcoxon and permutation tests) for both market and the irreversible fixed treatments. In the reversible fixed treatment, there was a significant difference detected for clean firms only on the permutation test, but there was no significant difference from the prediction for dirty firms.(Full results are shown in Appendix C.)

Intensity choices can be expected to influence output decisions and for both firm types these also differed significantly from predictions for the market treatments. And, as above, output decisions deviated from predictions for clean firms in the reversible fixed treatment, and dirty firms in the irreversible fixed treatment.

Emissions are the outcome of these decisions. Clean firms producing more output at higher intensities in the reversible fixed treatment, for instance, also produced more emissions than predicted. And dirty firms producing less output at a lower intensity emitted less than predicted in the irreversible fixed treatment.

Table 9: Summary Statistics for Periods 5-12

		Fixed		Market	
Variable	Prediction	Reversible	Irreversible	Reversible	Irreversible
Intensity					
Clean	1	1.3*	1.2	1.6*	1.4
		(0.07)	(0.09)	(0.13)	(0.11)
Dirty	3	2.9‡	1.9*†	2.2*	1.4*†
		(0.05)	(0.12)	(0.1)	(0.15)
Output					
Clean	24	27*‡	26*‡	21*	17*
		(1.08)	(1.41)	(2.81)	(1.91)
Dirty	24	22*‡	14*†‡	14*	5*†
		(1.16)	(3.11)	(1.92)	(2.09)
Total	192	198‡	161*†‡	139*	88*†
		(4.36)	(12.41)	(4.26)	(13.89)
Emissions					
Clean	24	35*	30	26	25
		(3.04)	(6.52)	(4.78)	(5.07)
Dirty	72	65‡	38*†‡	36*	11*†
		(3.39)	(9.28)	(6.11)	(5.32)
Total	384	400‡	273*†‡	249*	143*†
		(26)	(30.89)	(12.72)	(17.8)

Note:
Individual means for clean and dirty (96 obs)
Session totals for output and emissions results (3 obs)
Standard errors in parentheses
Two-tailed Wilcoxon and permutation tests used
* Significant difference from prediction
† Significant difference from reversible treatment
‡ Significant difference from market treatment

Excluding the first 4 periods, and allowing for some early adjustments to the experimental setting, reveals slightly different outcomes. Table 9 illustrates the same results when only periods 5-12 are included (as discussed in Section 4.3). Most discussion will pertain to these periods only, unless otherwise stated. (Full results are shown in Appendix C.)

Clean firm intensity choices were significantly different to predictions in the reversible treatments only (the difference in the irreversible market is no longer significant), while dirty firm choices differed in both market treatments and the irreversible fixed treatment (as when including all periods). For both firm types, in all cases, output is below predictions. This means that clean firms only 'overshoot' on predictions for emissions in the reversible fixed treatment.

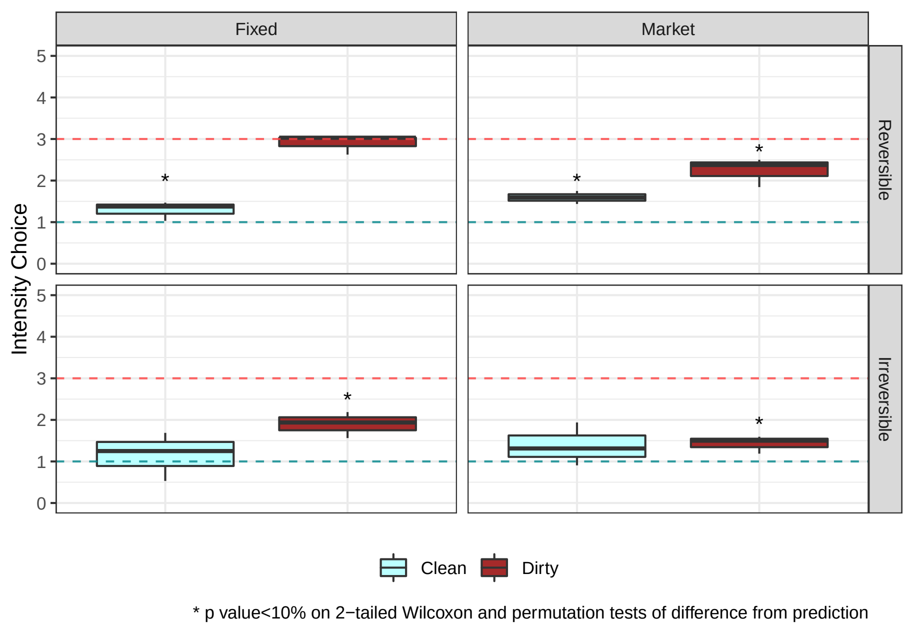

Figure 4: Mean Intensity Choices by Firm Type and Treatment (Periods 5-12). Predictions in Dashed Lines, Coloured by Firm Type.

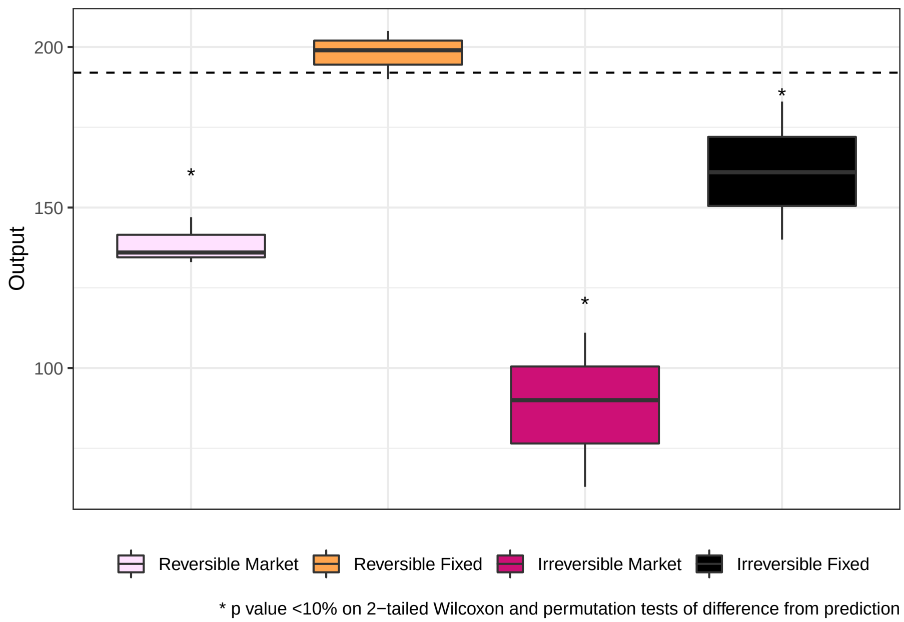

Figure 5: Total Output by Treatment (Periods 5-12). Prediction in Black Dashed Line.

The main outcome of interest, emissions, is determined by the intensity and output decisions of participants in the experiment. When pooled, total emissions outcomes differed from predictions for all treatments except the reversible fixed-price one, which was also the case for total output.

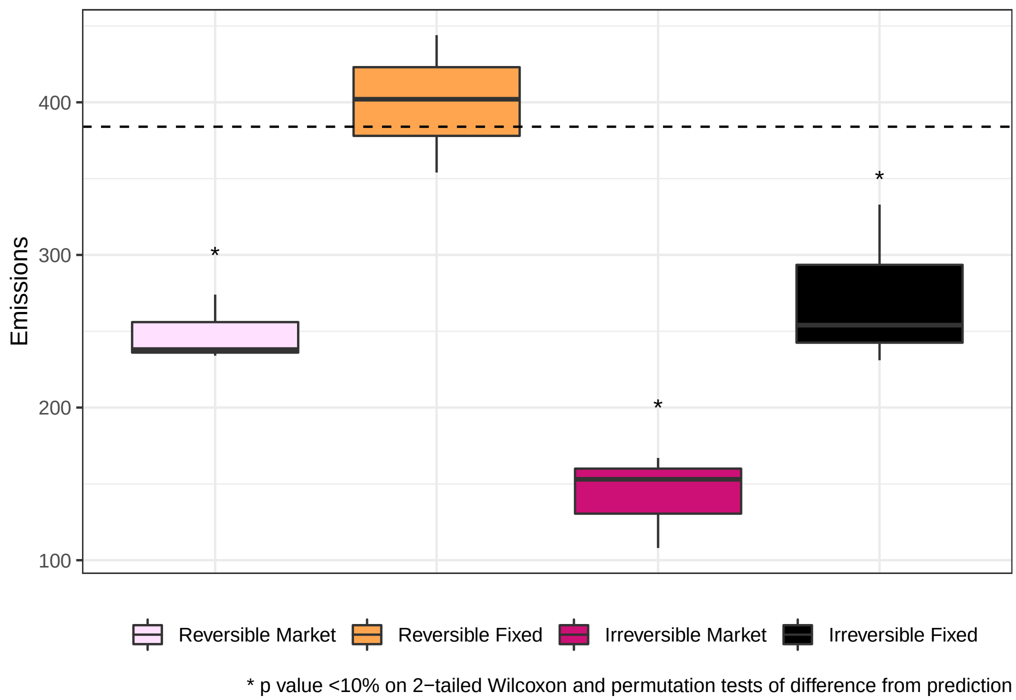

Figure 6: Total Emissions by Treatment (Periods 5-12). Prediction in Black Dashed Line.

6.1 Intensity Outcomes

Three sessions were run for each treatment, with 4 participants of each firm type in each session. It is possible to consider the data from the individual level, or at the session level. For some sessions, however, there appears to be a group effect in that participants' decisions may be influenced by others in the experiment (and indeed this sort of interaction is expected in the market treatments) (Figure 7). As such, testing proceeds based on session means (3 data points), rather than considering individual choices to test for treatment effects.

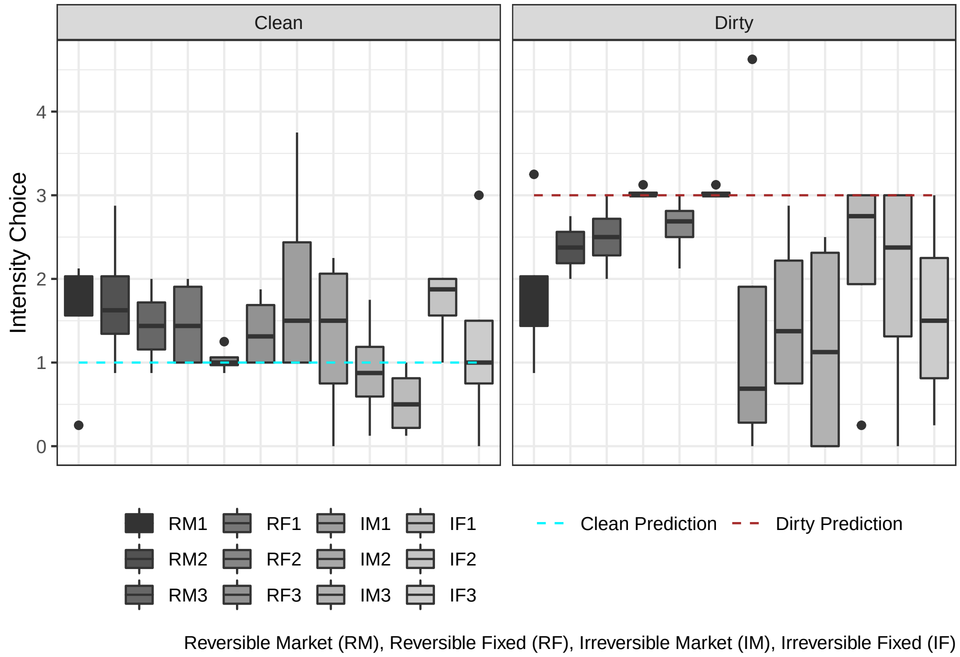

Figure 7: Average Intensity Choices. Boxplots Represent Mean Intensity Choices for 4 Individuals (of Each Firm Type) in Each Session, Grouped by Treatment

Hypothesis 1:

Intensity decisions are the same regardless of reversibility.

Intensity choices are not the same when comparing reversible and irreversible treatments and this hypothesis is rejected. Overall, intensity choices are lower when the decision is irreversible. When the market setting is also considered, choices differ only in the fixed-price case, however. In comparing firm types, dirty firm choices differ based on reversibility, but clean firms' do not.

From Figure 8, pooling all intensity choices together, where the predicted intensity choice is now 2, the difference between the reversible and irreversible case is clear. However, there is no significant difference detected when prices are determined in the market; the hypothesis is rejected only for the fixed-price treatments. This is confirmed by testing (see Appendix C).

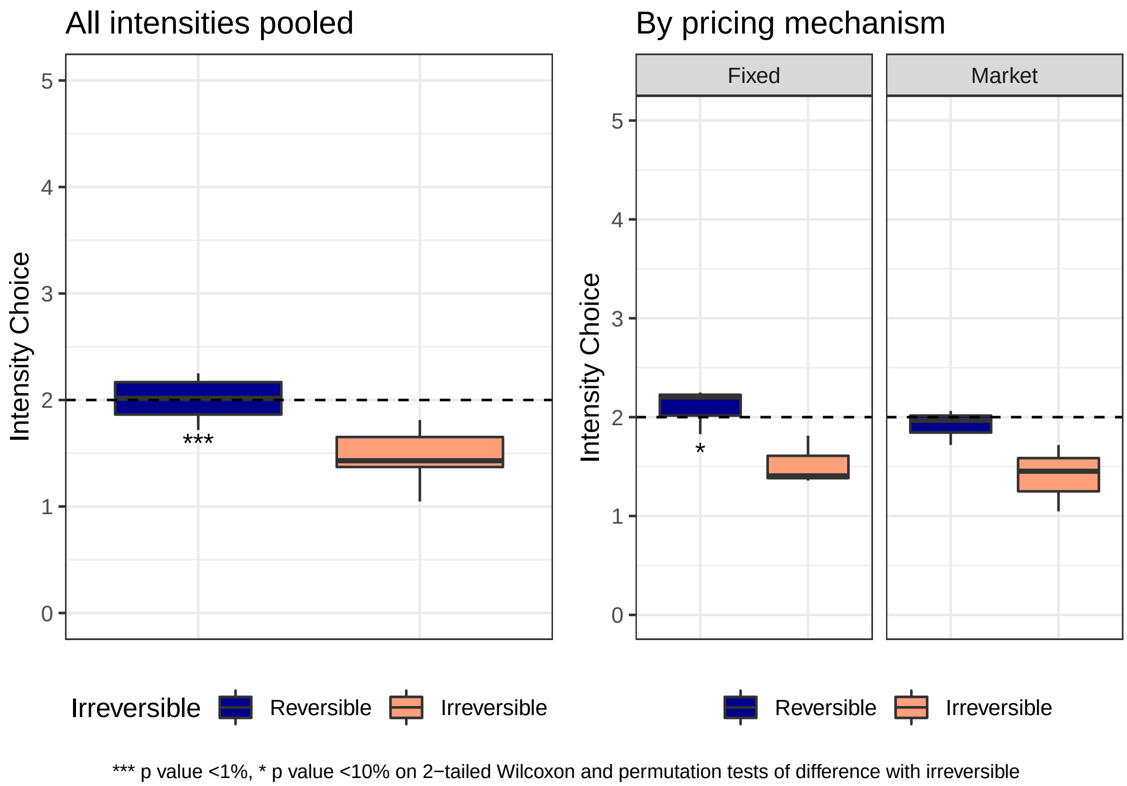

Figure 8: Irreversible vs Reversible Intensity Choices (Periods 5-12). All Intensities Pooled, and Compared by Pricing Mechanism. Dashed Line Represents Pooled Intensity Prediction.

By firm type (Figure 9), the hypothesis is rejected only for dirty firms, with no significant difference detected for clean firms' intensity choices on the basis of reversibility.

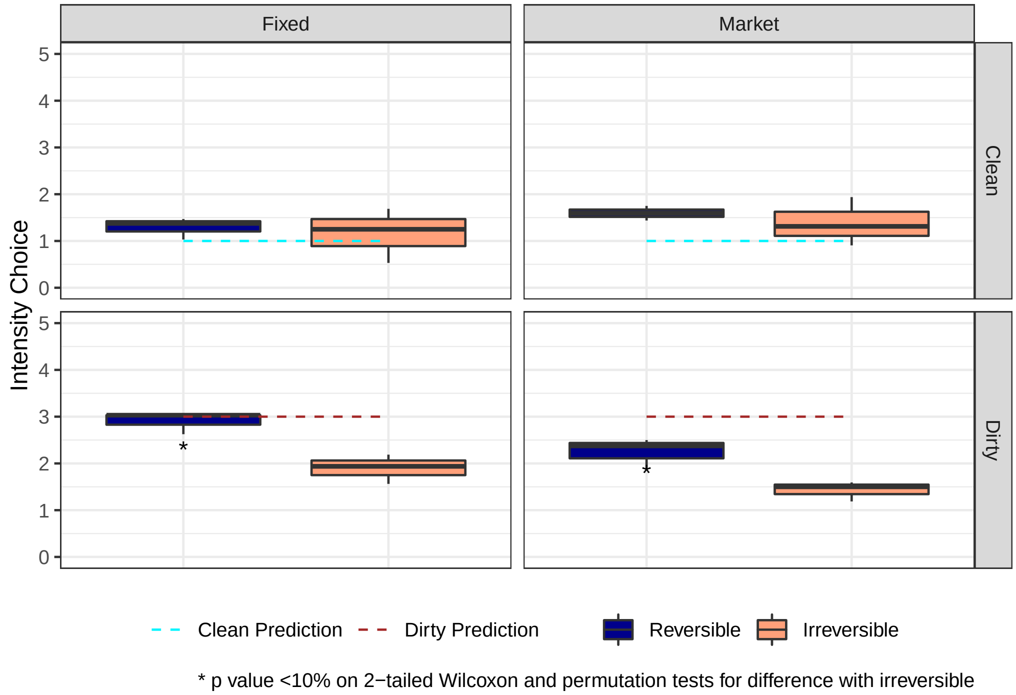

Figure 9: Mean Intensity Choices - Irreversible vs Reversible by Treatment and Firm Type (Periods 5-12). Dashed Lines Represent Predictions, Coloured by Firm Type.

When intensity choices over all periods are observed (Figure 10), it is apparent that the pattern of intensity decisions is also affected by reversibility. Given the constraint, the direction of choices can only be downward or flat in the irreversible case. However, it looks as though intensity choices start higher when they are irreversible, which might indicate some wariness about going too low.

The decline in intensity choices is steeper in the early periods in the market case, with clean firms approaching the predicted intensity level, but dirty firms undershooting, reducing scope for trade or output production. In the irreversible fixed-price treatment, dirty firms tended to converge toward the target intensity, where they did not need to purchase inputs to produce output. This divergence might be some indication of different learning effects for dirty firms in particular when the price is uncertain, relative to when it is fixed.

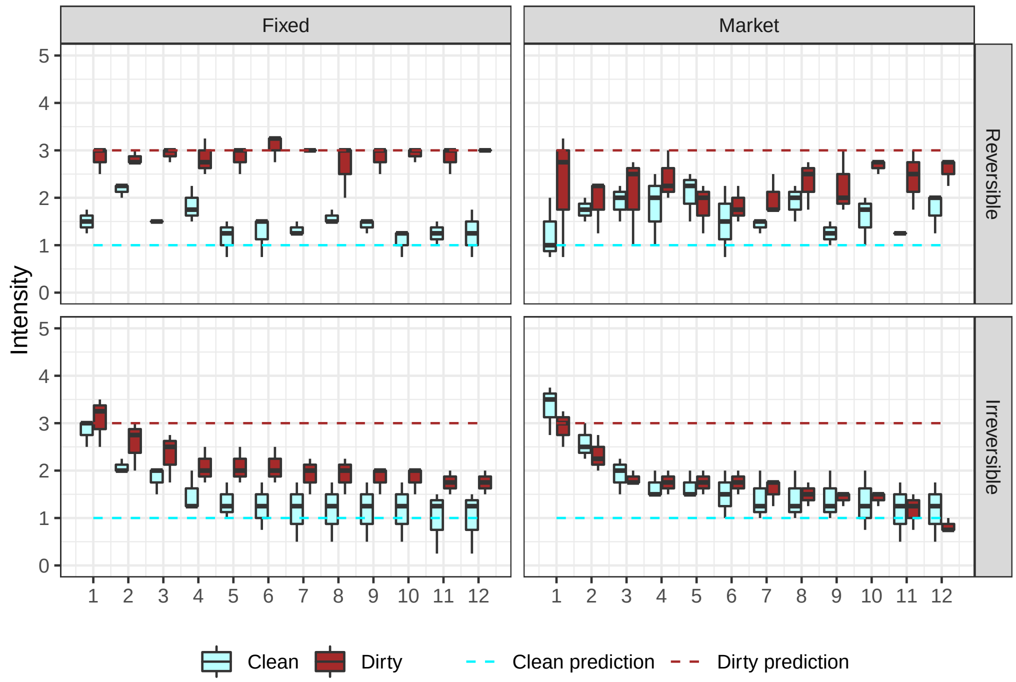

Figure 10: Intensity Choices by Period and by Treatment

Another way to consider the difference that irreversibility makes is to compare choices in early periods with those made later (Figure 11). Both firm types tended to choose higher intensities earlier on, though the degree of difference was less for the reversible treatments. For dirty firms, mean choices were very similar across reversible and irreversible treatments in the early periods, with the divergence appearing later on (mean choices don't appear to shift much in the reversible treatments). For clean firms, also, it looks like the downward movement in mean intensity choices is greater in the irreversible treatments, generally, which makes sense given the one-sided constraint, but might also be an indication that these movements are relatively delayed compared to the reversible treatments. For dirty firms, mean intensity choices increased somewhat in later periods for reversible treatments, closer to the prediction.

That they move so far downward in the irreversible treatments is an indication of the degree to which many ‘overshot’ their optimal intensity choice, however. From Figure 10, this pattern of decline in intensity choices in the irreversible treatments is very clear. Intensity choices tend to start higher, but end up lower, compared to the reversible treatments.

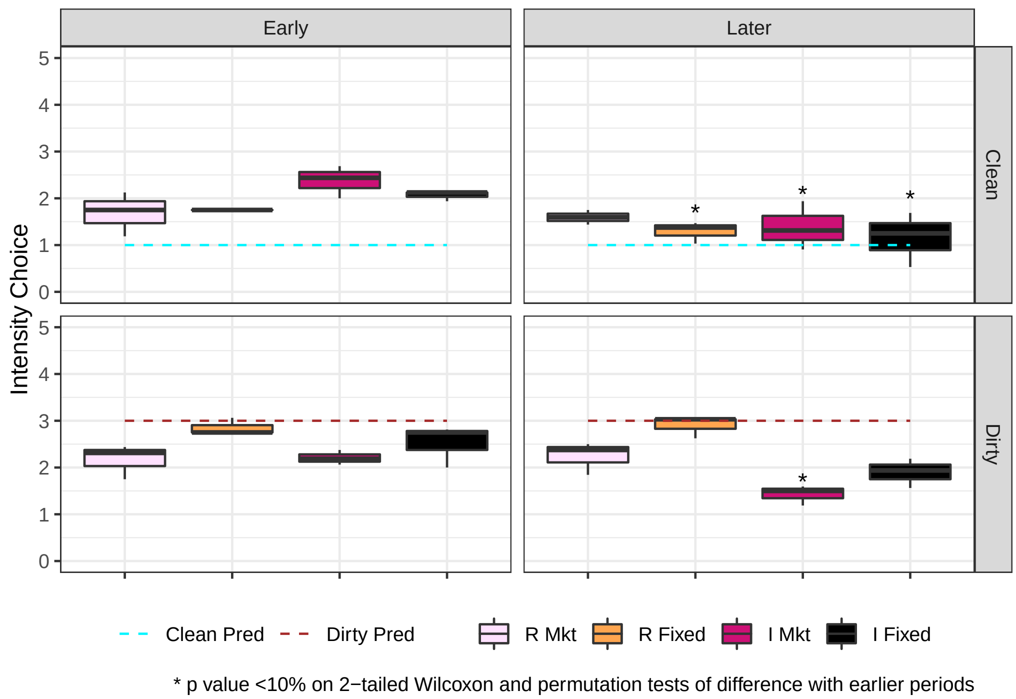

Figure 11: Intensity Choice Differences Between Early (1-4) and Later (5-12) Periods. Dashed Lines Represent Predicted Intensity by Firm Type.

Figure 12 presents the average timing of the first change in intensity (after the initial selection), by firm type and treatment. These means exclude those participants who did not change intensity at all (15 in total), and the minimum possible is 2 (second period). It seems clear that when the price was market-determined, irreversibility of the intensity choice meant participants tended to wait longer on average before changing their intensity after the initial selection. This also suggests that the response to the irreversibility of the intensity choice differed when the price was uncertain. By firm type, testing detects significant effects here in the fixed-case for clean firms (though in the other direction), and in the market case for dirty firms (see Appendix C.1). The boxplots do suggest there is a similar difference for clean firms in the market case, also. The early-period patterns of intensity choices vary with the interaction of reversibility and pricing uncertainty, and there may be some effect here of firm technology levels, also.

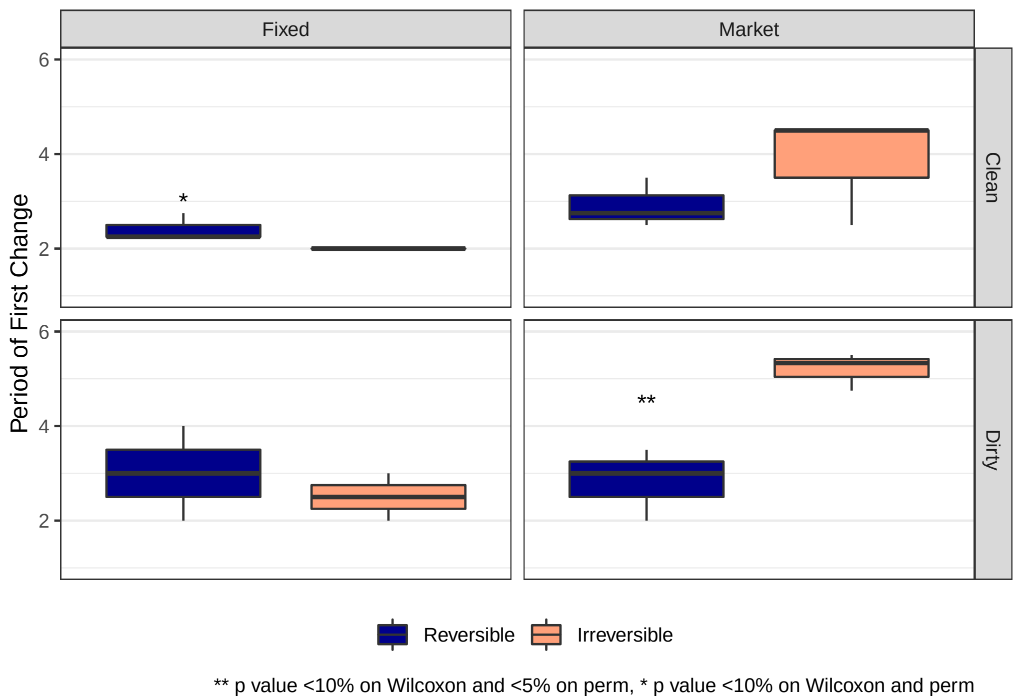

Figure 12: Period of First Change in Intensity After Initial Selection. Excludes Those Who Never Changed Their Intensity.

Irreversibility tends to lead to higher initial intensity choices and a delay in making changes to these choices compared to the reversible case. Reversibility has a greater impact on decisions by firms with dirtier technology, which tended to converge toward the target intensity level (and below predictions) in the irreversible fixed treatment, and undershoot even this level in the irreversible market treatment.

Hypothesis 2:

Firms make the same intensity decisions in the market and fixed-price treatments, where emission prices are set at the predicted equilibrium permit price ($P^F = P^M$).

It is not possible to reject this hypothesis outright. Intensity choices are only significantly different between market and fixed-price treatments in the reversible case and only for dirty firms.

There is no apparent difference between intensity choices on the basis of pricing mechanism, either when all intensity choices are pooled, or when grouped by reversibility (Figure 13).

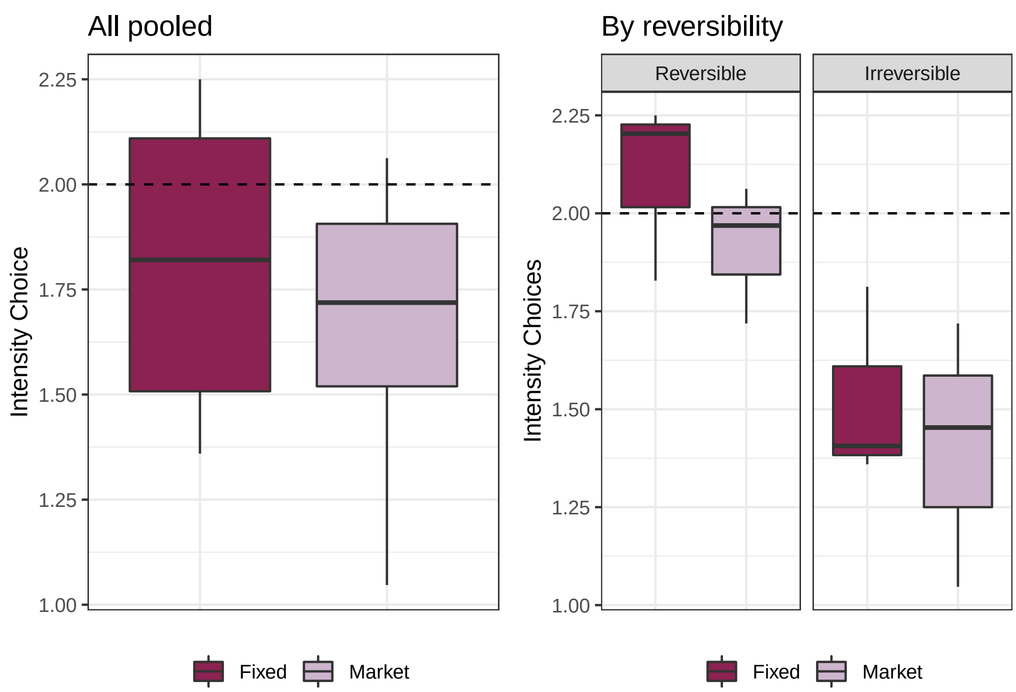

Figure 13: Market vs Fixed Intensity Choices for Both Firm Types (Periods 5-12) - All Pooled and by Reversibility. Dashed Line Represents Pooled Type and Reversibility Status Prediction.

Adding in firm type, the only significant difference between market and fixed-price treatments can be detected for dirty firms in the reversible case (Figure 14).

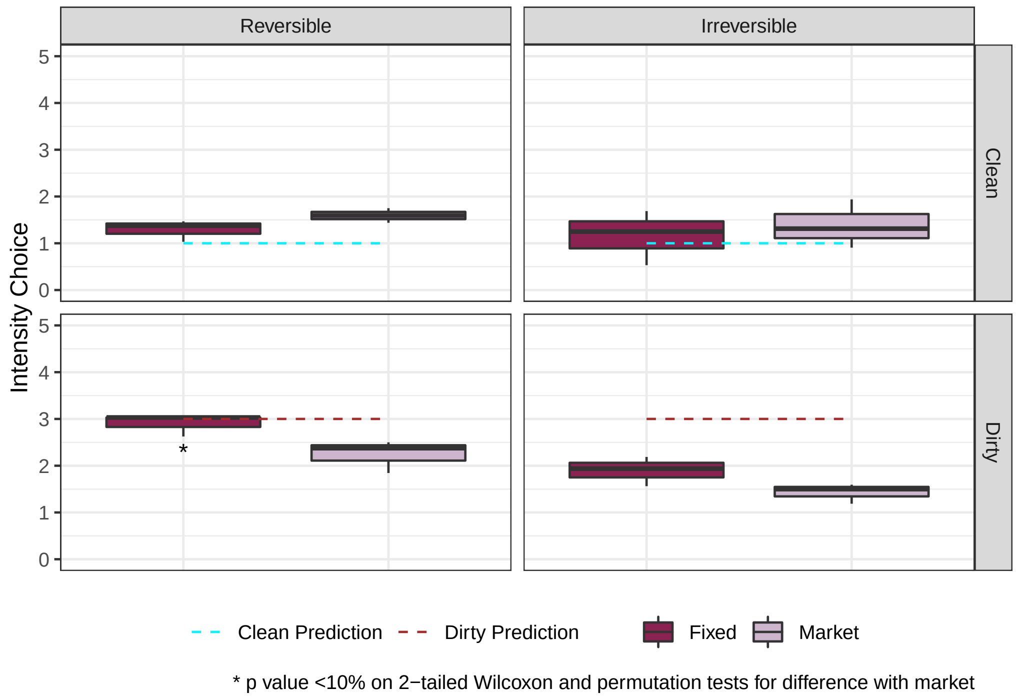

Figure 14: Mean Intensity Choices - Fixed vs Market Pricing by Treatment and Firm Type (Periods 5-12). Dashed Lines Represent Intensity Predictions, Coloured by Firm Type.

Generally, it seems that both firm types tended to choose intensity levels closer to their predicted levels in the fixed-price treatments, compared to the market ones. Testing reveals a significant difference only in the case of dirty firms in the reversible cases.

6.2 Output Outcomes

Participants made output decisions in each period, with predicted total output dependent on the overall number of either high or low states. (Note that although the number of high and low states was the same across all sessions, there were 2 sequences used, so when comparing sessions, the number of high or low demand states in a given period may vary.) However, intensity choices, as well as prices in the market treatments, would affect how much output they could produce. Additionally, some participants did not produce as much output as they could have.

Predicted output is the same for both firm types, however differences are evident here if analysed separately. In general terms, output is higher in the fixed-price treatments compared to the market ones, and in the reversible treatments compared to the irreversible ones, but overall output is lower for dirty firms compared to clean ones. Since dirty-type firms would be expected to produce at an intensity level above the target this might suggest they faced difficulties sourcing adequate supplies of permits. It may also be the case that if they were more likely to choose their intensities unwisely they were unable to produce much. All totals by firm type are significantly different from predictions.

Interestingly, while the reversibility of the intensity decision didn't apparently make much difference to the output decisions of clean firms in the fixed-price treatments, there was a clear effect for dirty firms (Figure 15). Irreversibility seems to make a difference for both firm types in the market treatments.

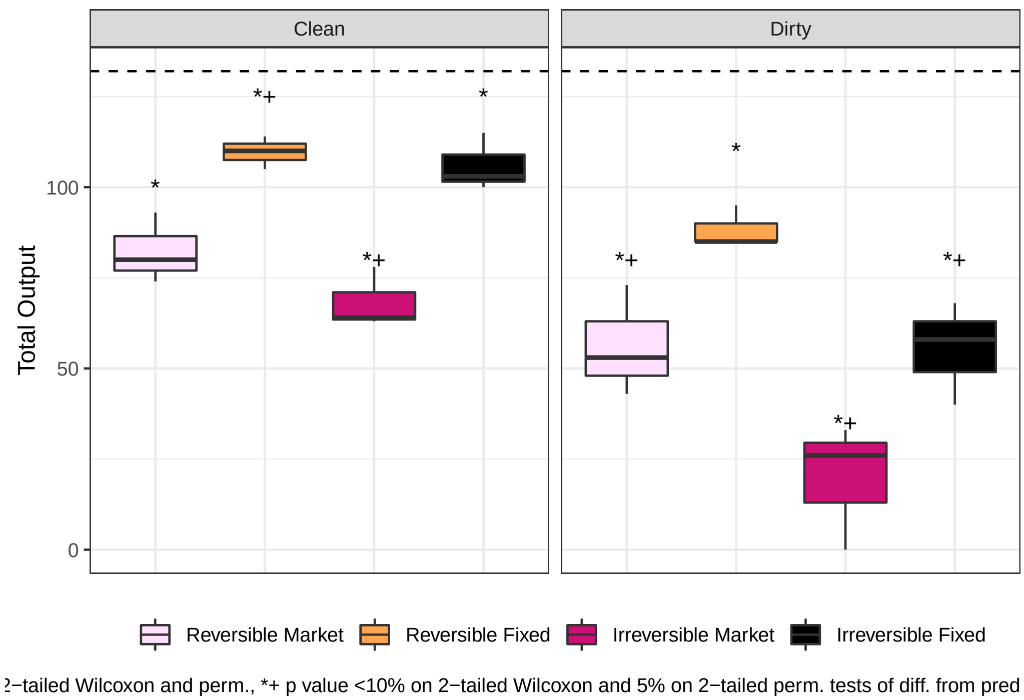

Figure 15: Total Output by Firm Type vs Predictions (Periods 5-12). Dashed Line Represents Prediction.

The difference in output between clean and dirty type firms is particularly large in the irreversible market price treatments, where clean firms, on average, produced around three times as much as dirty firms. A closer examination of participant-level data suggests that many participants assigned to the dirty firm type in the irreversible treatments chose intensity levels that were too low and then could not increase them again, reducing (or eliminating) their ability to produce output. This had a significant impact on overall production.

In the irreversible fixed-price treatment, also, clean firms produced, on average, about two-thirds more output than the dirty firms. The difference in the reversible market treatments was about 50%, but overall output levels were higher compared to the irreversible case. The difference in output was much smaller in the reversible fixed-price treatments, as might be expected since intensity decisions were also closer to predictions.

Note, also, that when prices were fixed, clean firms that chose the optimal predicted intensity may have been induced to produce an extra unit of production for zero net revenue, which might explain the inflated output.

Figure 16 presents per-period session total output for each treatment and is colour coded for the high and low output demand states. As expected, output is generally higher in the high demand state. However, the distinction is far less clear for market treatments, and there is a fair bit of overlap for the irreversible market treatment where the demand state appeared to have little impact. In affecting intensity choices, the market mechanism and reversibility both appear to have an effect on firms' responsiveness to demand shocks.

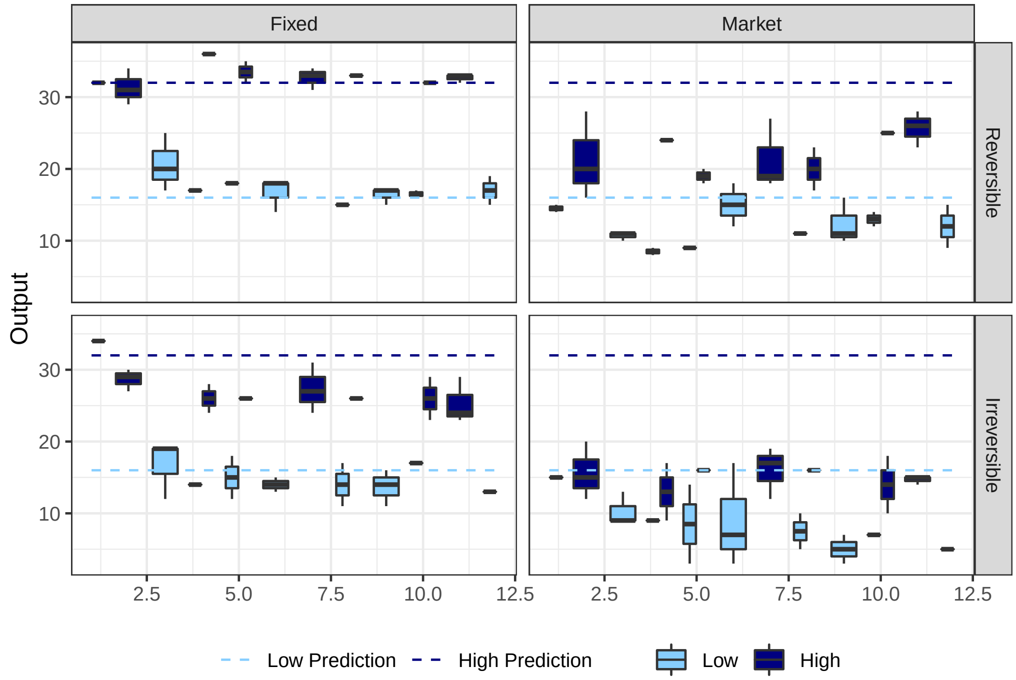

Figure 16: Per-Period Output by Treatment and Demand State. Dashed Lines Represent Predictions by Demand State. Total Output Expected to be Twice as High in High Demand State Compared to Low.

There were participants who produced no output at all for the entire experiment. Presumably this was a result of some failure to understand the experiment. Two were dirty-type firms and one clean, and two were in the irreversible market treatment and one in the reversible market treatment. They did purchase some permits, despite not producing any output. Given that this apparent lack of comprehension would be likely to affect the basis of their decision making, they have been excluded from some of the analysis (See Appendix F for further explanation).

6.3 Emissions Outcomes

Hypothesis 3: *Emissions outcomes will be the same for all treatments.*

Emissions are not the same for all treatments and differ from predictions except in the case of the reversible fixed-price treatment.

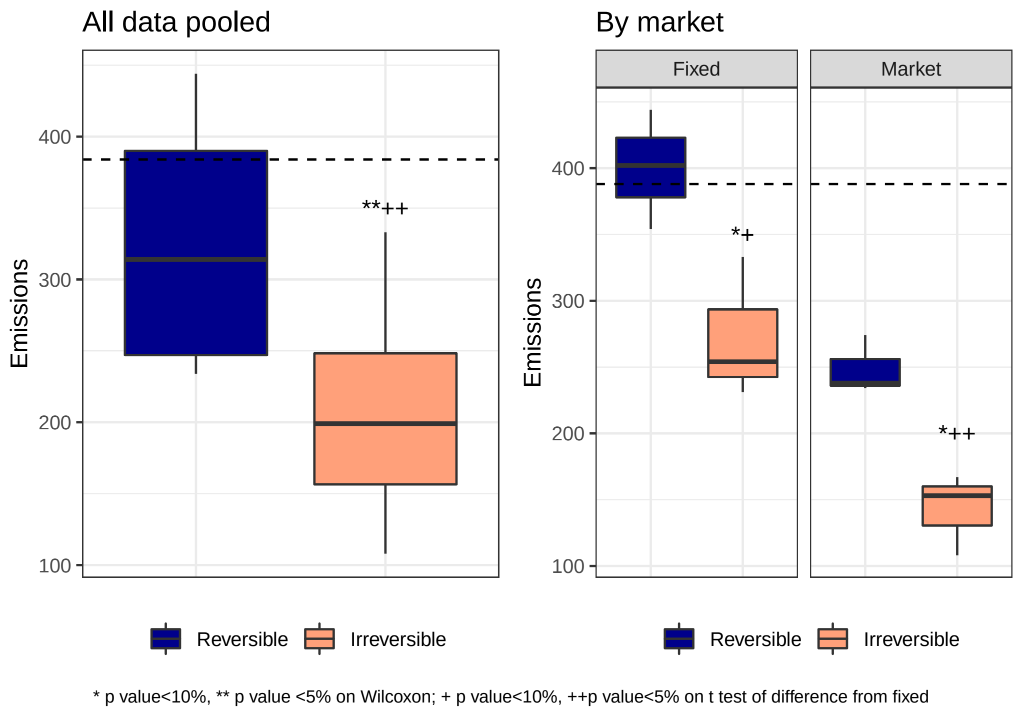

Figure 17: Total Emissions - Comparing Reversible and Irreversible (Periods 5-12) for Pooled Data and by Market Setting. Dashed Line Represents Pooled Prediction

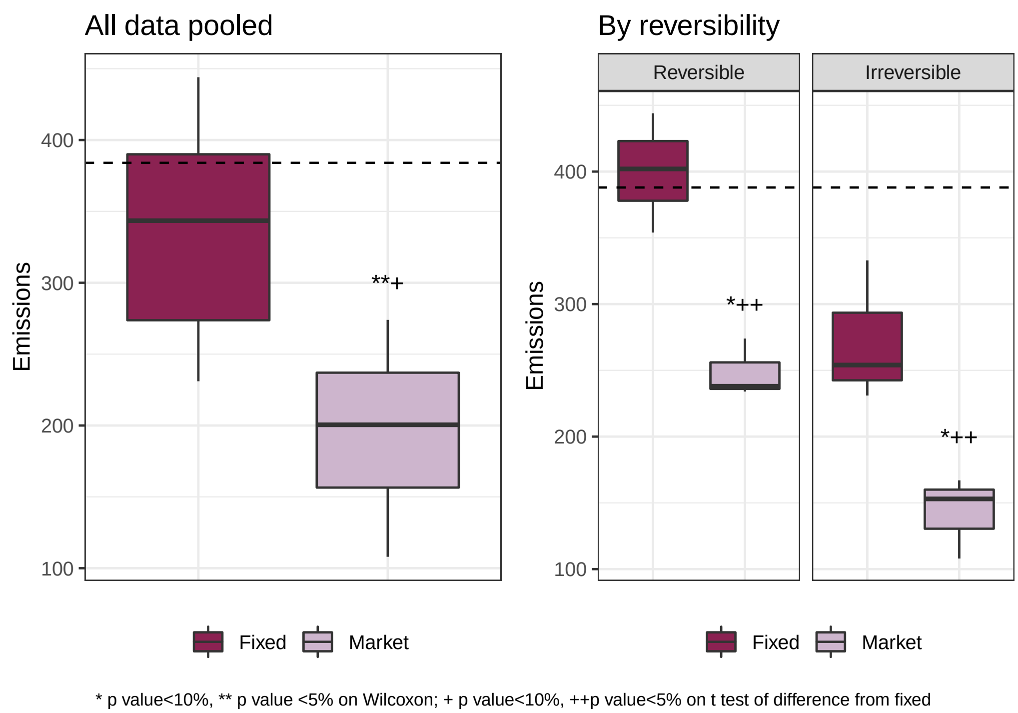

Figure 18: Total Emissions - Comparing Market and Fixed Pricing (Periods 5-12) for Pooled Data and by Reversibility. Dashed Line Represents Pooled Prediction.

Emissions outcomes follow from the intensity and output decisions made by participants. Higher intensity ratios and lower output in many cases tended to reduce emissions in the experiment. Total per-period emissions over the session are presented in Figure 19, plotted against the fitted values from a regression (with confidence intervals).

Emissions in the low demand state were predicted to be 32 per period. Testing of the coefficients could only reject the null hypothesis of no difference for the irreversible market treatment, where emissions were lower. However, only the coefficient for the reversible fixed-price treatment was significant.

When demand was high, emissions were predicted to be 64 per period. In this case, testing suggests there is a difference in all treatments except the irreversible fixed-price one. The coefficients for the two reversible treatments were significant. Emissions were higher than predicted in the high output demand state in the fixed-price treatments, but lower in the market treatments.

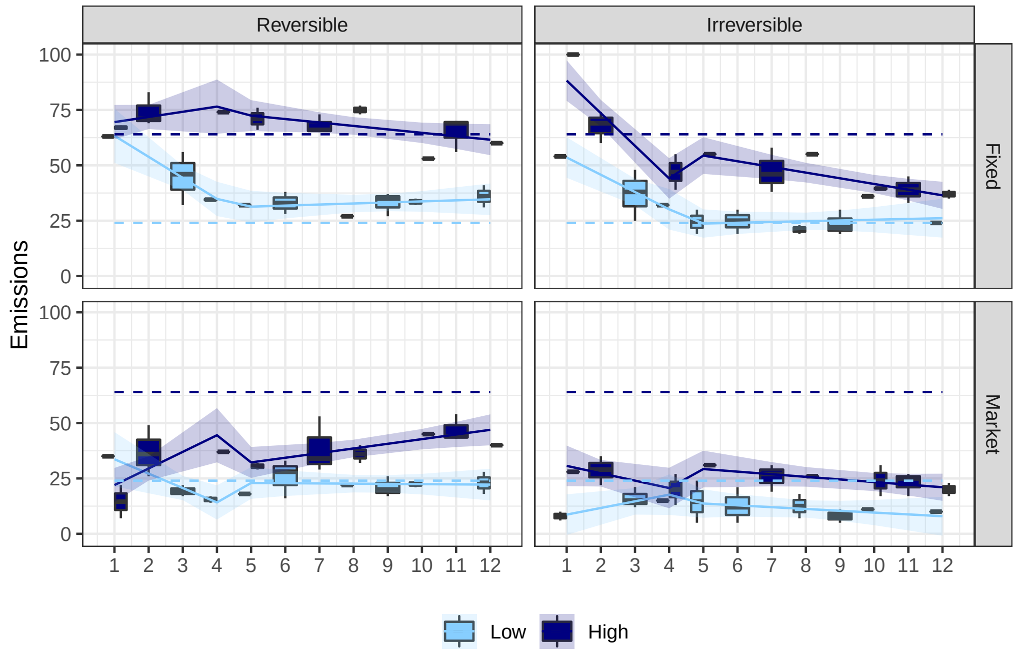

Figure 19: Emissions by Period and Demand State. Dashed Lines Represent Predictions by Demand State.

While the predicted difference in emissions per period between low and high state was 32, the estimate here was 51 for the base case. Testing of all the demand state coefficients found that there was a statistically significant difference from the prediction in the base case (periods 5-12, reversible fixed-price treatment) and the reversible market case. The response to high demand states differs between market and fixed-price treatments, with a larger effect detected in the fixed-price treatments. This suggests responsiveness to the business cycle may be affected by the pricing mechanism.

Emissions are higher in the early periods (1-4) of all treatments, but to a far lesser extent for the irreversible market treatment.

For reversible treatments, the trend in emissions is downward in the early periods and (slowly) increasing afterwards. This may well reflect adjustments made early on to intensity choices, and resulting output choices. In these treatments, participants were able to correct mistakes and the learning effect may well be more apparent compared to the irreversible case. In the early periods, emissions increased more rapidly in the high-demand state, but more slowly in the low demand state.

For irreversible treatments, the trend in emissions over time was generally downward, with the exception of the first 4 periods in the irreversible market case. This may be because of differences in intensity choices at the outset. If they started relatively higher, then output would likely be constrained and emissions rise as intensity choices declined.

The linear regression was run using total per-period emissions for each individual group. To account for inter-group heterogeneity, errors were clustered on the session and corrected using a Satterthwaite test to adjust degrees of freedom and CR2 estimators to account for the small sample sizes (as described in Pustejovsky and Tipton (2018)). Wild cluster bootstrap methods were also considered, as these are often effective with a small number of clusters (Cameron et al., 2008), but can fail when the regressor of interest is a binary categorical variable that takes the value of 0 for all values in a particular group, as when recording treatment effects (MacKinnon & Webb, 2018)).

To start, periods were treated as factors to capture mean session and individual effects in each, and testing revealed there were differences between period coefficients in some cases. However, an additional effect expected between periods depends on the demand state, where high or low demand would alter the levels of periods. Using period as a continuous variable interacted with state achieved similar outcomes as when treating periods as a factor. Additionally, to capture different time trends between treatments, market and irreversible dummies were also interacted, as well as another dummy to control for early period effects. Data analysis has already indicated apparent differences in outcomes between the first 4 periods and the rest of the session. Removing some interactions with the period variable were trialed, but the full version is a better fit. The full output is in Appendix G.1.

6.4 Prices for Market Treatments

Hypothesis 4: *a: Prices are the same in all treatments.*

b:Prices in the market treatments converge to the predicted equilibrium.

Hypothesis 4a is rejected if all periods are considered, but not if only periods 5-12 are used. Prices trend down over the session, with evidence of convergence toward the predicted level. As such, hypothesis 4b cannot be rejected either.

Mean prices for both treatments in periods 5-12 are presented in Figure 20. Test results are mixed, with no clear indication of a statistical difference from the prediction for either market treatment. (For details on tests, see Appendix C.)

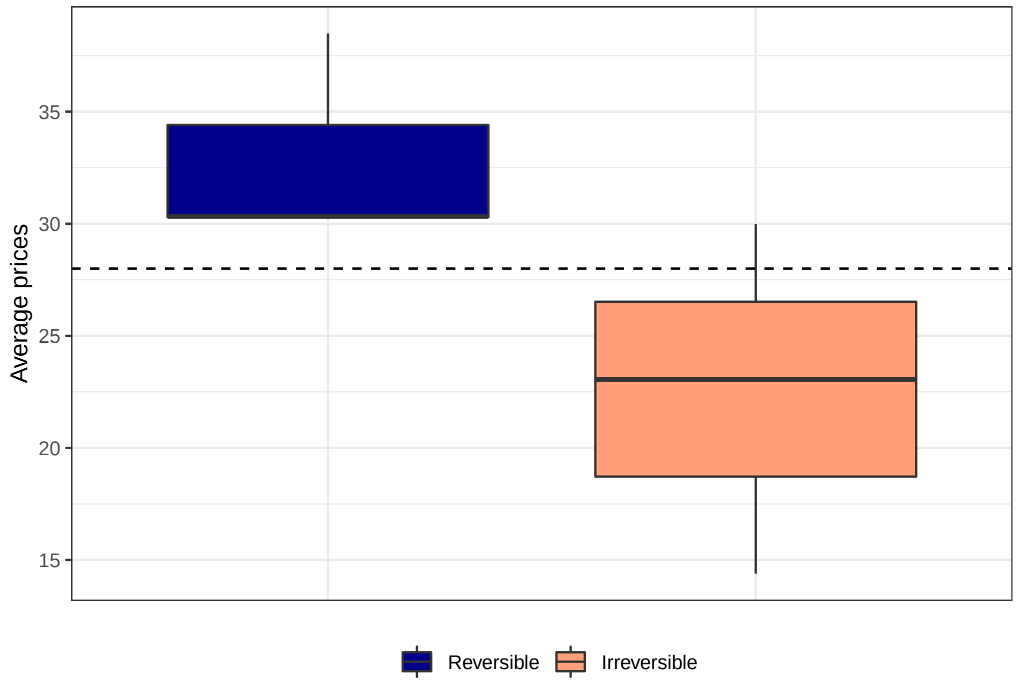

Figure 20: Avg Prices for Periods 5-12 vs Prediction in Dashed Line

Figure 21 illustrates the movement in prices over the course of the experiment for each of the market treatments, with individual sessions differentiated by colour. All trades are included here, rather than only the period mean, so the larger variations in early periods, with generally higher prices, are even clearer. There seems to be evidence of a convergence in prices over the course of the session, as well as decreasing variance in later periods.

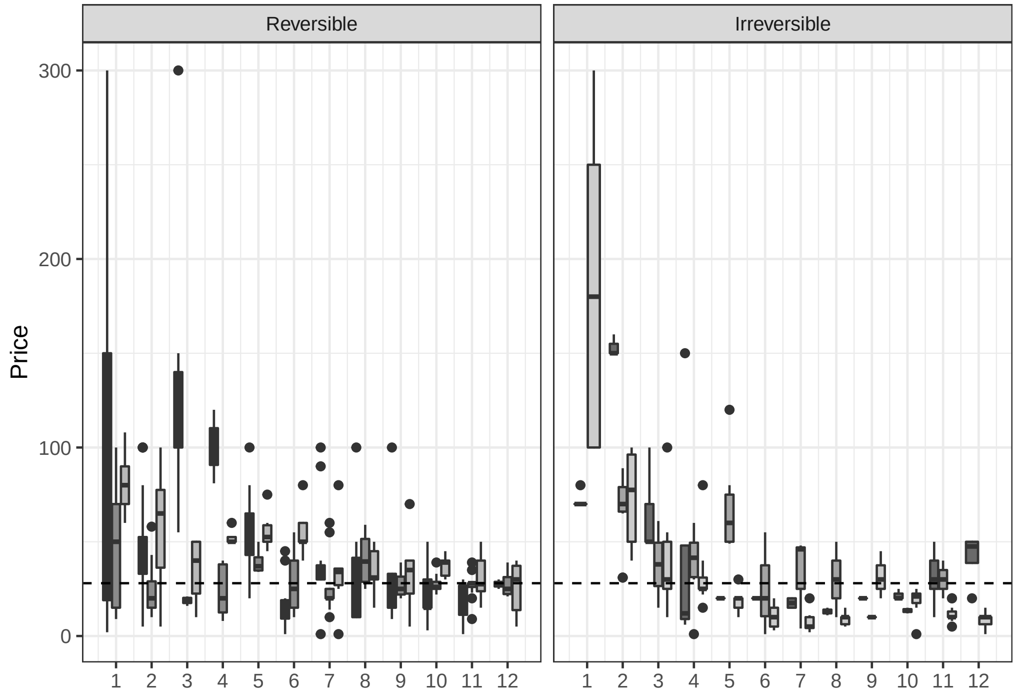

Figure 21: All Prices Each Period by Session. Predicted Price in Dashed Line.

Reversibility does not seem to have an impact on price outcomes, either, with test results again mixed, and there only appears to be a statistically significant difference between the early and later periods in the irreversible treatment (details of testing in Appendix C). This is in line with the differences in patterns in early periods noted already for intensity and output, where the reversibility of the intensity choice affects outcomes in different ways in the early periods compared to later on.

Another way to consider price convergence is in terms of the variance in prices over the period. From Figure 22, there certainly appears to be a decrease in variance after the first 4 periods in both market treatments, and testing confirms this is the case (Appendix C).

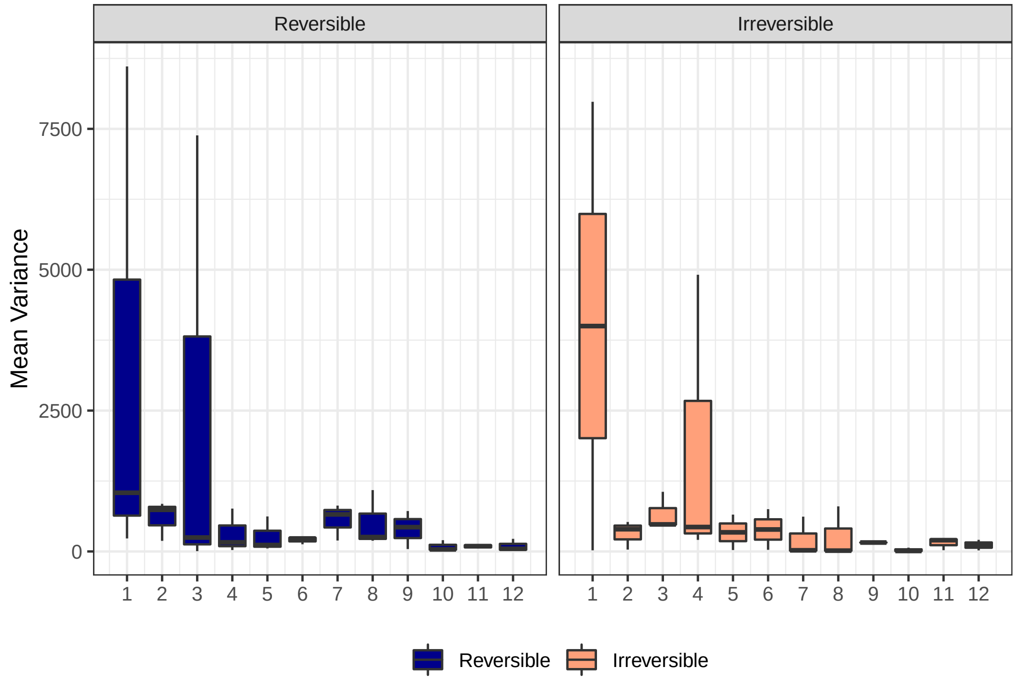

Figure 22: Session Average Permit Price Variance, by Period. Three Observations in Each Boxplot.

7 Additional Results - Individuals

One of the aims of this thesis is to investigate how personal preferences or characteristics might influence decision making in this context. A closer examination of individual decisions offers more insight into outcomes, where individual choices may reflect behavioural responses to the experimental setting.

7.1 Risk and Ambiguity Attitudes

Because intensity choices are made prior to knowing the output demand state, or the price of the emissions permits, individual risk or ambiguity attitudes could affect intensity choices. It may be that this impact will be more pronounced when intensity decisions are irreversible (Ben-David et al., 2000). Even with a reversible decision, Thomas (2016) noted that clean firms tended to choose intensity levels above the predicted level and dirty firms below, both closer to the target level. Ambiguity attitudes can be considered in a similar way, and may interact differently to risk with price uncertainty and the reversibility of the intensity decision. Both risk and ambiguity attitudes may affect decisions in the context of uncertainty Ghosh and Ray (1997).

For risk, a multiple price list method, established by Holt and Laury (2002), is utilised. Ambiguity attitudes are measured using an adaptation of the Ellsburg urn task (Ellsberg, 2000) from Kőnig-Kersting and Trautmann (2016). These scores give a relative measure of the individual's tolerance for either risk – known outcomes with known probabilities – or ambiguity, where probabilities for these outcomes are unknown.

7.1.1 Risk Attitudes

The score for the Holt-Laury test is calculated based on the number of safe choices made by participants (explained in Appendix D). As per expected utility theory, for a risk-neutral individual, we would expect a switch-over from the safe choice to the other option at question 5, when the expected payoff is higher for the 'B' option. Higher values here suggest some degree of risk-aversion, while lower values suggest risk-seeking behaviour.

The last choice is considered a comprehension check for participants as there is a 100% chance of winning either of the higher payoffs, and the alternate 'B' choice has a higher payoff for sure than the safe 'A' option, so there is no reason for anyone to choose 'A'. In total, 17 individuals chose the safe choice 'A' for the last question. These individuals are presumed to not have comprehended the exercise. One individual also chose all 'B' options, but it is entirely possible this is an expression of their preference for risk, rather than an error.

Exclusions can be made of those who selected the safe option in the last question of the Holt-Laury test (see Appendix F) since a lack of understanding of the task suggests the results are unlikely to reflect the individual's risk attitude. This sort of exclusion is common, though it doesn't always alter the results (Andersen et al., 2009; Anderson & Mellor, 2009; Dave et al., 2010; Holt & Laury, 2002a). In this case, there does appear to be a difference in mean outcomes between those who apparently did and did not comprehend the exercise (based on the answer to the final choice) (Appendix F.2.1).

It is also worth noting that 35 out of 96 participants recorded inconsistent preferences in the Holt-Laury test. That is, they switched more than once. The measure used here is robust to such switching, since it tallies the total number of safe choices.

However, there still may be questions as to the usefulness of this measure. The decision to exclude these results tends to vary depending on their number and impact on overall results. Eckel et. al. (2004), for example, encountered a smaller percentage of inconsistent responses when using a Holt-Laury test, but found no particular difference in the distribution and so did not exclude these responses.

It may be that switching between options reflects indifference on the part of the participants (Andersen et al., 2006). However, the possibility that inconsistencies reflect confusion on the part of participants does raise questions about their inclusion (Charness et al., 2013). In this case, there was quite a large proportion of these, but there is not apparently any particular difference between the groups (Appendix F.2.1).

Gender-based differences in risk attitudes have been noted elsewhere, though not universally (Eckel & Grossman, 2008; Fellner & Maciejovsky, 2007). Testing indicated no differences along gender lines, however, even when inconsistent responses and those who chose option A in the last question were also removed.

For comparison sake, the scale has been reversed for analysis, so a score of 0 now indicates a high aversion to risk, and a score of 10 a high tolerance for it.

7.1.2 Ambiguity Attitudes

Though the test used here also employs a list of binary lottery choices, values for this measure are calculated differently to those for the Holt-Laury test. These values represent a mid-point between the highest odds at which an individual would choose an uncertain bet and the lowest odds at which they would choose a known-odds bet, the so-called probability equivalent score (see Appendix D). The method means that scores range between a minimum possible score of 0.325, which represents high aversion to ambiguity, and a maximum of 0.675, which would indicate a high tolerance for ambiguity. A score of 0.5 suggests the individual has a neutral attitude toward ambiguity. Four individuals scored the lowest possible value here, and one had a score of higher than 0.6. To make interpretation slightly easier, the scores have been relabeled 0-14, with 0 indicating the case of the highest aversion to ambiguity and 14 the greatest tolerance.

The choices are presented out of order for this test, and again consistency is not necessary for the calculation of the score. In this case, only 11 out of 96 participants displayed inconsistent preferences by switching more than once.

There are fewer examples to draw on with respect to the treatment of inconsistent results in the ambiguity test. Dimmock et. al. (2013) included controls for inconsistent results, while König-Kersting and Trautman (2016) found that exclusions made no difference to results. Testing, however, suggests that there is a difference in results between those with consistent and inconsistent responses in this case, with consistent respondents tending to have a lower tolerance for ambiguity (mean of 6.2 compared to 7.3) (see Appendix F.2.2).

Some prior studies have found evidence of differences in ambiguity attitudes based on gender (Brachinger et al., 2000). Testing of these results suggests participants who identified themselves as female tended to have a greater tolerance for ambiguity according to this measure, compared to those who identified themselves as male. This remained the case even when inconsistent results were excluded (Appendix F.2.2).

7.1.3 Comparing Risk and Ambiguity

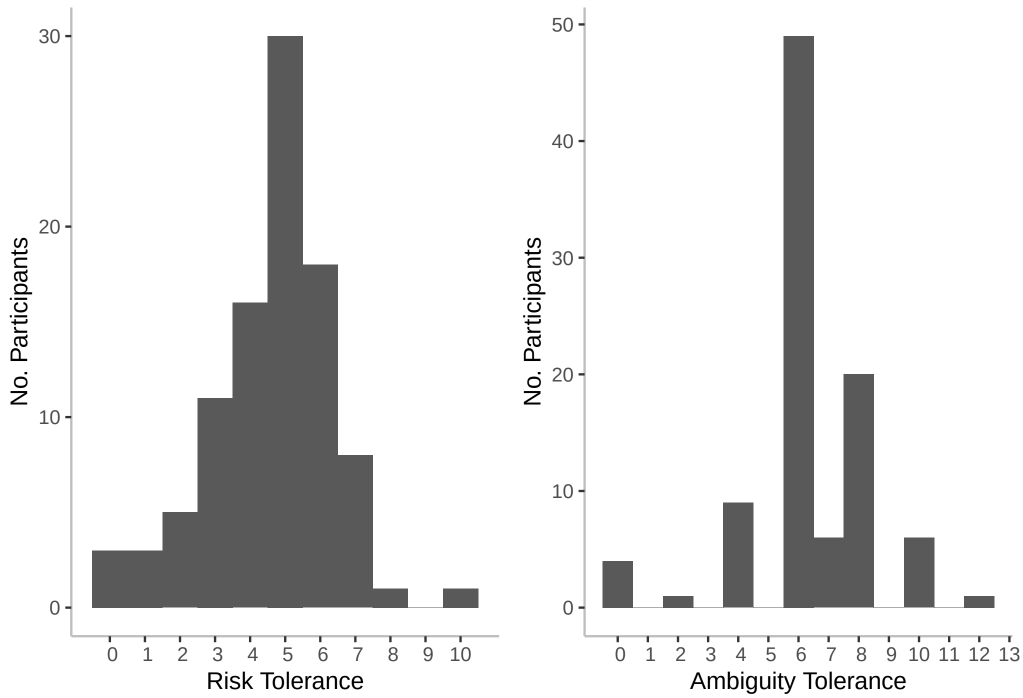

Figure 23: Distribution of Risk and Ambiguity Attitudes. Risk Tolerance Centred Around Level 5, Ambiguity Tolerance Centred Around Level 6.

The results for the risk and ambiguity tests are compared to see if there is a correlation between them. It is not necessarily the case that there would be a strong correlation, if any (Di Mauro & Maffioletti, 2004).

Plotting the measures against each other (Figure 24), there is no apparent correlation between the two scores.

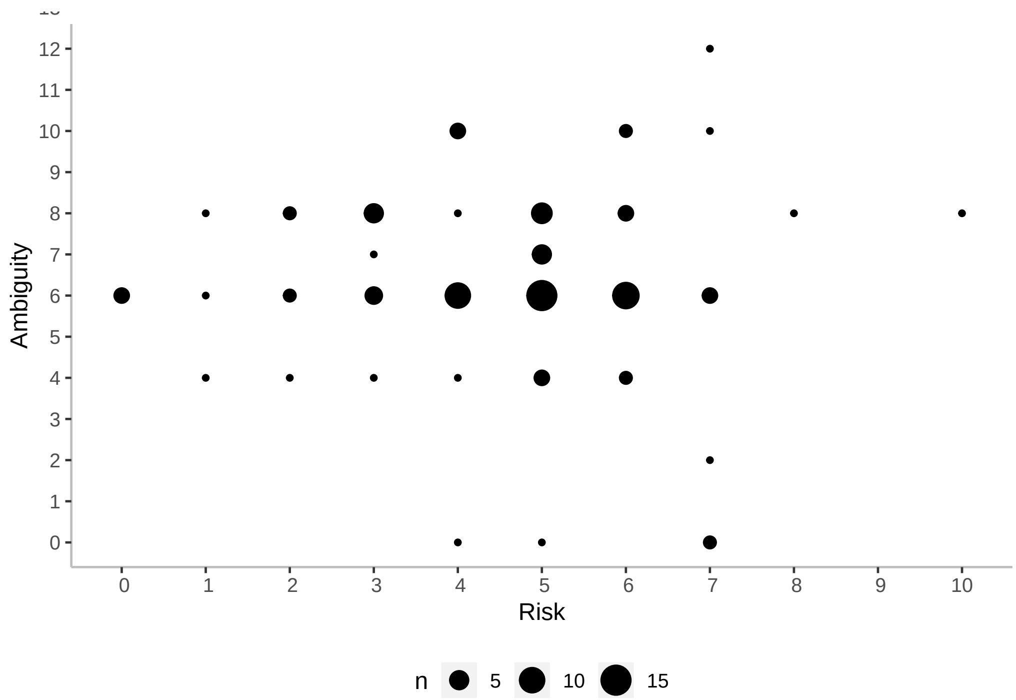

Figure 24: Risk vs Ambiguity Attitudes. Observations Centred Around Risk Level of 4-6 and Ambiguity Level of 6. Size of the Point Relates to the Number of Observations.

In Table 10, a Kendall tau test is used to test for a correlation between the paired results of individuals' Holt-Laury and ambiguity tests. The p-values suggest the null hypothesis that there is no correlation cannot be rejected. As noted above, there were some inconsistencies in participant responses and results may vary with their exclusion. In the first instance, the 17 participants who selected the safe option in the last choice in the Holt-Laury test are excluded, since this is considered a measure of participant comprehension. The process is repeated to exclude inconsistent responses on the Holt-Laury and ambiguity tests (reducing the number of participants included to 51), with no evidence of a correlation here either. This provides some assurance that there is no correlation between these results.

Table 10: Test of Relationship Between Risk and Ambiguity Results

test	tau	p-value
HL vs Amb	-0.19	0.85
Excl. Q10 test	-0.07	0.94
Excl. inconsistencies and Q10	-0.20	0.84

The three individuals with no output could also be excluded (see Appendix F). This is because we are comparing decisions under the assumption that participants understand the experiment, where failing to produce any output can be considered a revealed indicator of incomprehension. Tests do not suggest there is a difference between risk or ambiguity attitudes for these individuals compared to the rest of the group (Appendix F.2.1 and F.2.2).

7.2 Uncertainty in the Experimental Setting

When considering individual decisions in the experiment, the effect of risk and ambiguity in the context of uncertainty is of interest. In the market treatments, the permit price is uncertain. Individuals may interact with this uncertainty in different ways, possibly determined by their relative tolerance for risk or ambiguity. Firms could produce output at the target level without having to purchase permits or relying on the permit price for final revenue. As such, this can be regarded as a sort of safe choice. One way to measure individual responses to uncertainty in this experimental setting is to calculate the average distance between their intensity choices and the target intensity level. From Figure 25, most intensity choices are within 1 step from the target.

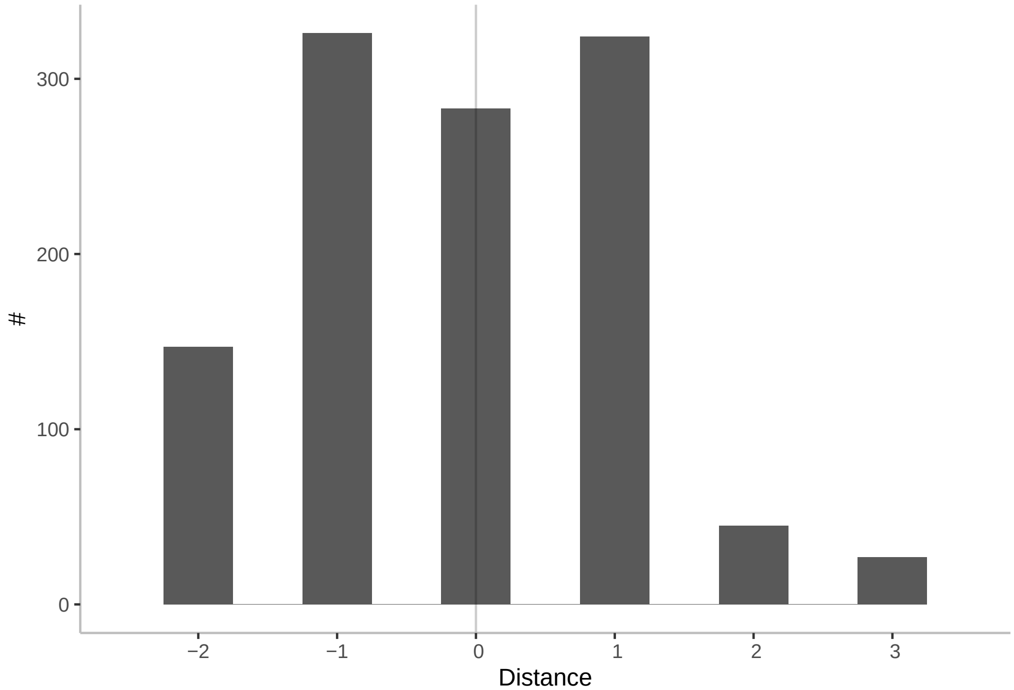

Figure 25: Distance from Target Intensity Level of All Intensity Choices (All Periods)

While the average distance from the target is negative for clean firms in all treatments, as expected for this firm type (Figure 26), dirty firms fall below the target in the irreversible treatments. Testing suggests that there is some difference in the group means when excluding those who didn't produce output (Appendix F.2.3).

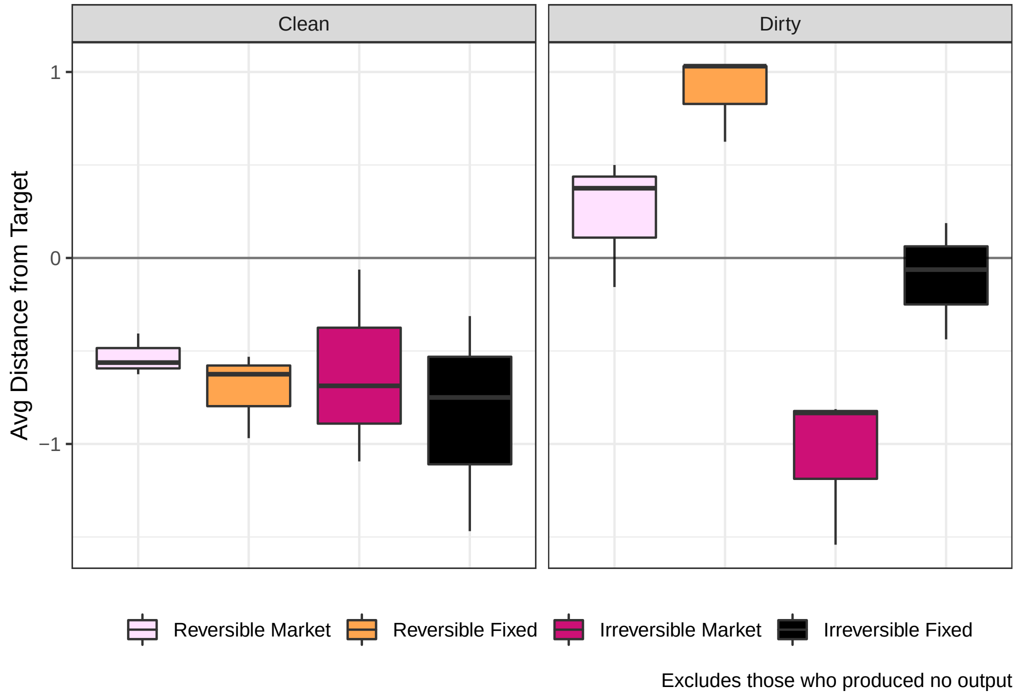

Figure 26: Average Distance From Intensity Target by Treatment and Firm Type (Periods 5-12)

When individual average distances from the target are plotted (Figure 27), only dirty firms in the reversible fixed treatment seem to have mean intensity choices at a clear distance to the target. Of interest, the range of values in the irreversible cases appears greater than in the reversible ones. Perhaps this reflects some divergence in strategy for those in irreversible treatments, where participants might either be hesitant to reduce their intensity, or act too 'boldly' and go too low, then get stuck there.

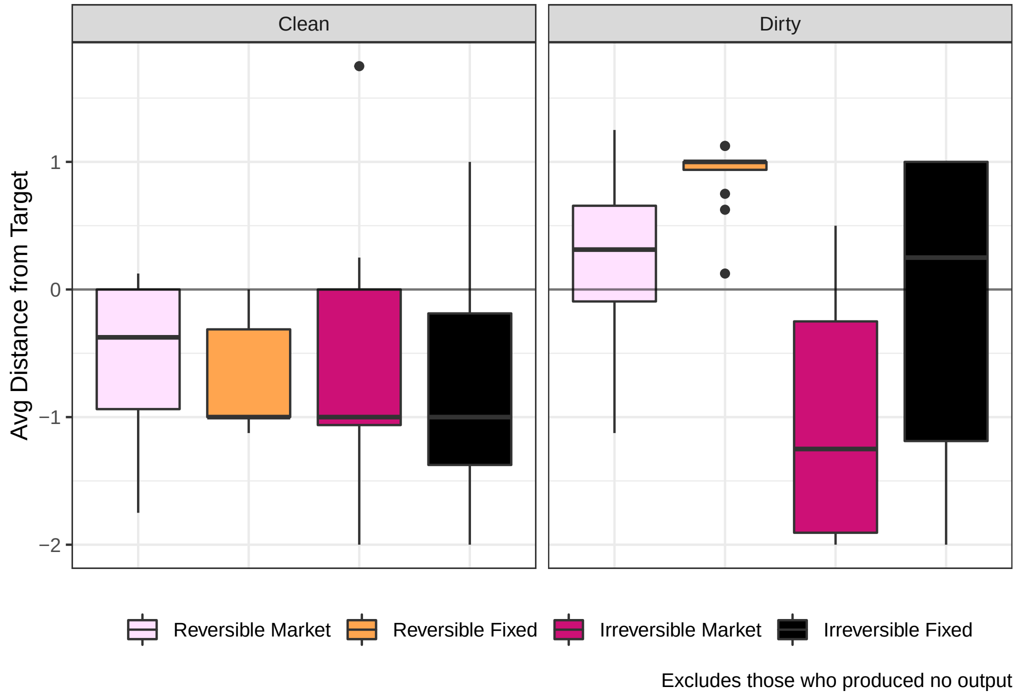

Figure 27: Individual Average Distance From Target (Periods 5-12)

Since we are interested in a subject's decisions relative to the target, we can compute this as the mean of the absolute distance of the individual's intensity choices from the target.

7.2.1 Risk and Ambiguity and Distance from the Target

Of key interest is the possibility of a relationship between these attitudes and the mean absolute distance from the target of individuals' intensity choices. In Thomas (2016) the absolute value of the distance between the mean of each subject's intensity decisions and the target was taken as a measure of 'closeness'. And a correlation between this and risk aversion determined in a Holt-Laury assessment was calculated using Kendall's tau statistic. The mean of these correlation values was then tested using a Wilcoxon test of the difference from 0. Thomas (2016) did not detect any statistically significant difference in this way.

Repeating Thomas' exercise (and noting the small sample size), there is no evidence to suggest the correlation between the two is any different from zero (Table 11). The same is true for tests on a correlation with ambiguity (Table 12).

Table 11: Correlation Between Risk Attitudes and Mean Distance from Target

Treatment	Mean tau	Wilcoxon
Reversible Market	-0.32	1.00
Reversible Fixed	-0.17	1.00
Irreversible Market	0.93	0.50
Irreversible Fixed	0.44	0.25

Note:
Excl. HL non-comprehension and zero producers

Table 12: Correlation Between Ambiguity Attitudes and Mean Distance from Target

Treatment	Mean tau	Wilcoxon
Reversible Market	-0.40	0.25
Reversible Fixed	-0.62	0.75
Irreversible Market	0.41	1.00
Irreversible Fixed	-0.19	0.25

Note:
Excl. inconsistent results and zero producers

Risk and ambiguity scores are discrete variables, however it is not an intuitive interpretation to ascribe effects only to certain scores in these test. Other studies using similar measures have tended to treat them as continuous variables (Anderson & Mellor, 2008; Fellner & Maciejovsky, 2007; Fellner-Röhling & Krügel, 2014; Picone et al., 2004; van der Pol et al., 2017). Testing in the early phases of building a regression model, however, detected statistically significant differences between some of the levels. To retain the possibility of detecting different effects at different levels, but make the interpretation more intuitive, the variables were regrouped into a 3-level factor, based on scores representing aversion (below the mid-point), neutrality, and loving (above the mid-point).

However, some groups (by firm type and treatment and risk/ambiguity attitude) either have few or no values. This problem was only worsened when some of the data was excluded. Figure 28 presents mean distance from the target by risk attitudes, firm type and treatment, and Figure 29 does the same for ambiguity attitudes (with exclusions applied). It is evident that some groups include no individuals, while quite a few include only 1. This is a particular problem when single values represent outliers.

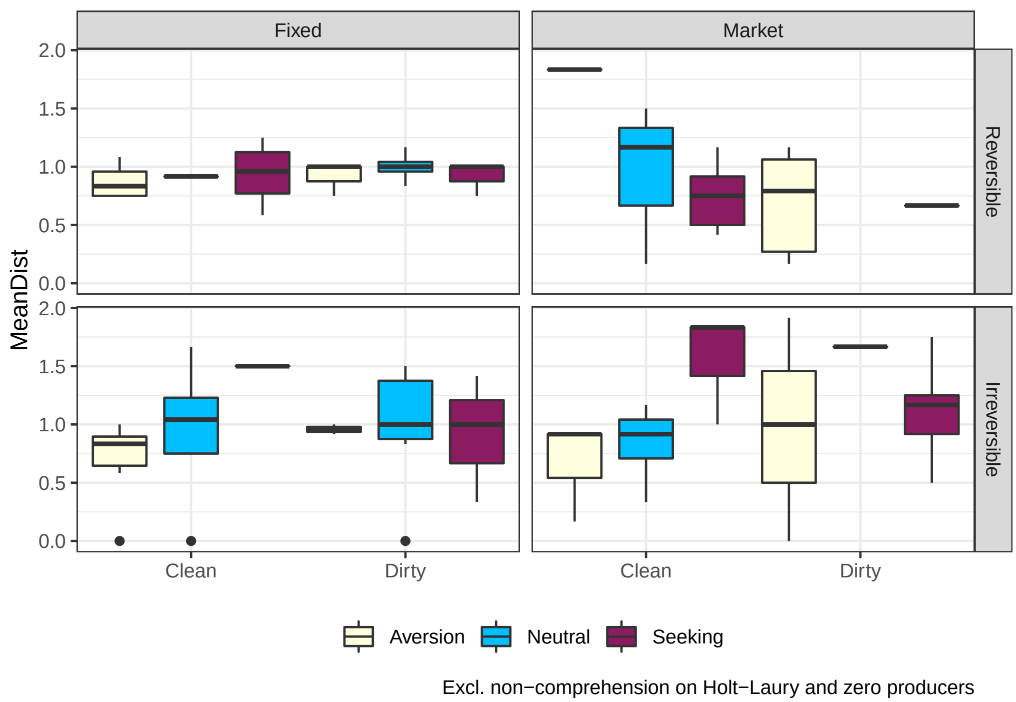

Figure 28: Risk Levels and Mean Absolute Distance from Intensity Target. Individual Mean Values Used.

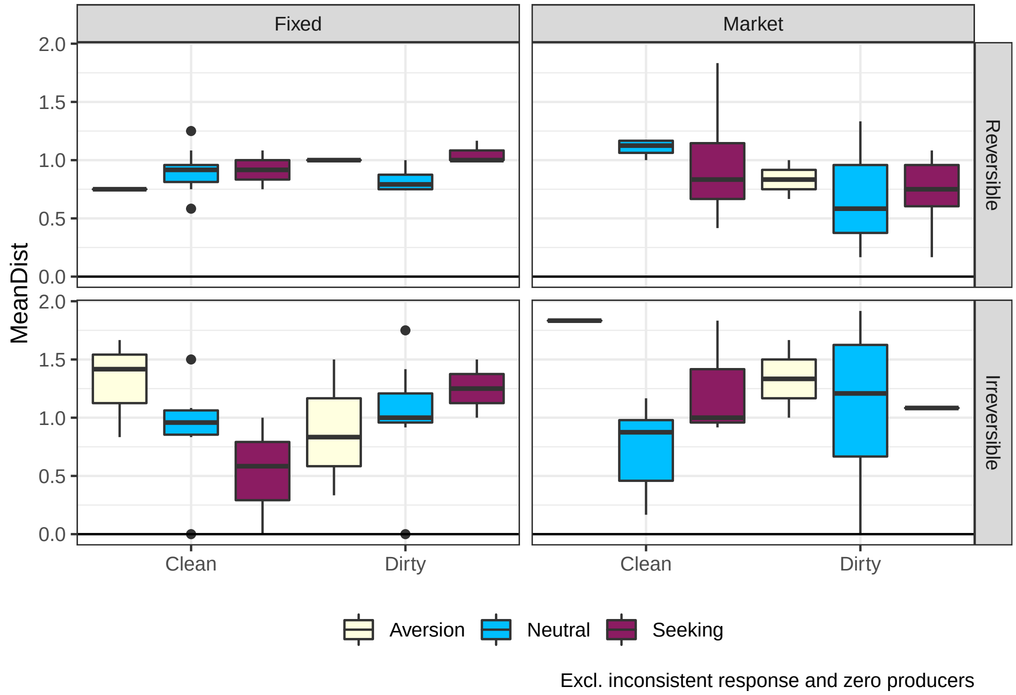

Figure 29: Ambiguity Levels and Mean Absolute Distance from Intensity Target. Individual Mean Values Used.

A linear regression was attempted to assess the relationship between the mean of the absolute distance from the target of individuals' intensity choices on the measures for risk and ambiguity collected here, as well as a number of dummy variables for treatment and firm type. Nothing of particular significance was discovered, however, so this analysis was excluded.

It is possible that, in the irreversible treatments, at least, the constraint on decision making may have obscured the relationship between risk and ambiguity attitudes and decisions. This is because of a tendency to move toward the target but then away from it again. However, it may also be that participants did not necessarily interpret choosing an intensity level at the target as a relative reduction in uncertainty, and thus it is not a good measure of this tendency anyway.

7.2.2 Determinants of Individual Intensity Choices

In general terms, intensity choices are expected to be made with reference to the firm's revenue table and expected permit pricing (in market treatments) to maximise earnings.

In the market treatments, participants would be expected to adjust their intensity choices in response to price signals. This would mean choosing lower intensity levels when permit prices are high, in anticipation of earning more this way by either producing below the target and selling permits, or reducing the number of permits required to produce output. Conversely, firms would respond to lower prices by choosing higher intensities. If the initial response to higher prices led to an increase in supply, then, all else being equal, prices would be expected to decrease again.

This process of adjustment would be quite different when the intensity choice is irreversible. If high permit prices early on induced firms to choose low intensity choices, they would be unable to respond to a later decline in prices as supply increased. This could be particularly bad for firms with dirtier technology that have chosen an intensity level that is too low, as they would not be able to produce output.

Charting these variables does not reveal any apparent relationship here, however. (Refer to Appendix E.1 for charts.)

It is also possible that attitudes toward risk or uncertainty might affect decisions. However, charting intensity choices through the session versus measured risk and ambiguity attitudes doesn't reveal any obvious relationship here, either (see Appendix E.2 and E.3). Testing did suggest there may be some degree of correlation between ambiguity results and intensity choices for clean firms (see Appendix C).

A linear regression was used to test for possible relationships between these variables and individual intensity decisions for each treatment. The intensity choice itself is a discrete variable. However, it is an interval scale and our interest here is in the relative change - either higher or lower - rather than the specific levels, or the propensity to move between certain levels. As such, treating intensity as a continuous variable seems reasonable.

Individual intensity choices over the session are utilised. Testing indicated that, when treated as a factor, all levels for the period variable were the same and replacing this with a continuous variable produced very similar results to capture trend effects over the session. A particular interest here was detecting specific differences between firm types, as well as between early and later periods (particularly in the irreversible treatments). A dummy was used to control for effects in the first 4 periods, and others were added to control for firm type, and pricing mechanism and reversibility.

As discussed in Section 7.2.1, there were problems using the risk and ambiguity results in a regression. There appeared to be more problems with the ambiguity results with respect to missing values, so adding risk attitudes alone was attempted. This appeared to greatly improve the fit of the model, however some of the estimates suggested unrealistically large effects, particularly once any other variables were included.

As a result of this process of elimination, risk and ambiguity attitudes were left out of the regression. For reference sake, Figures 30 and 31 present mean intensity choices by risk and ambiguity levels in each of the treatments, for both firm types. Testing suggests there is a difference between mean intensity choices by ambiguity levels for dirty firms in the reversible market only (Appendix C.1).

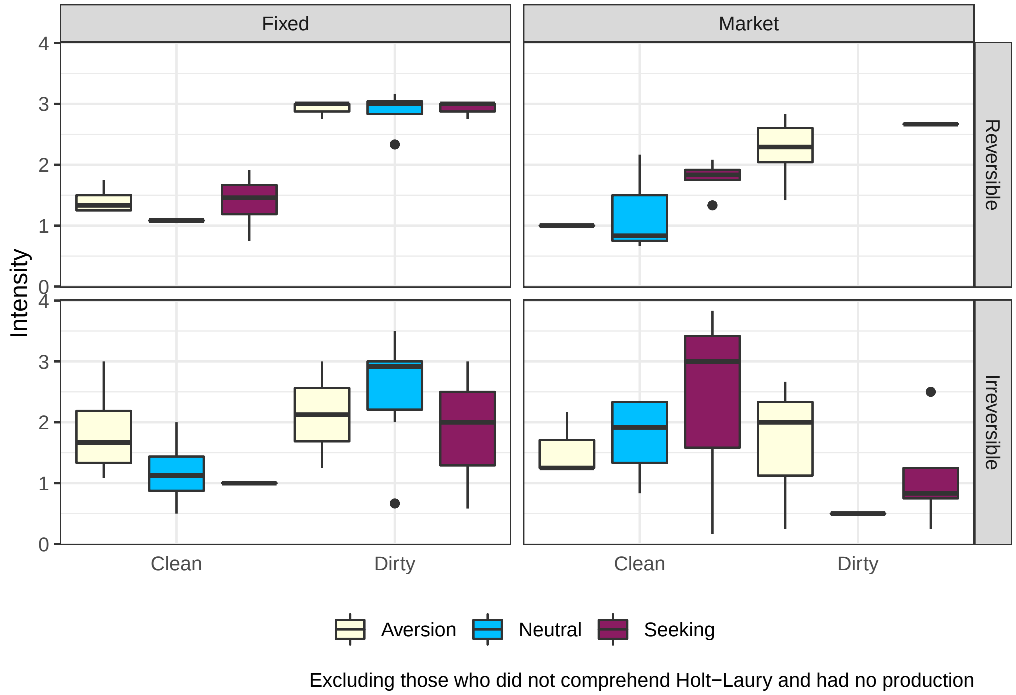

Figure 30: Risk Levels and Mean Intensity Choices. Colour Coded for Risk Tolerance. A Rising or Falling Trend Across Categories Would Indicate Relationship.

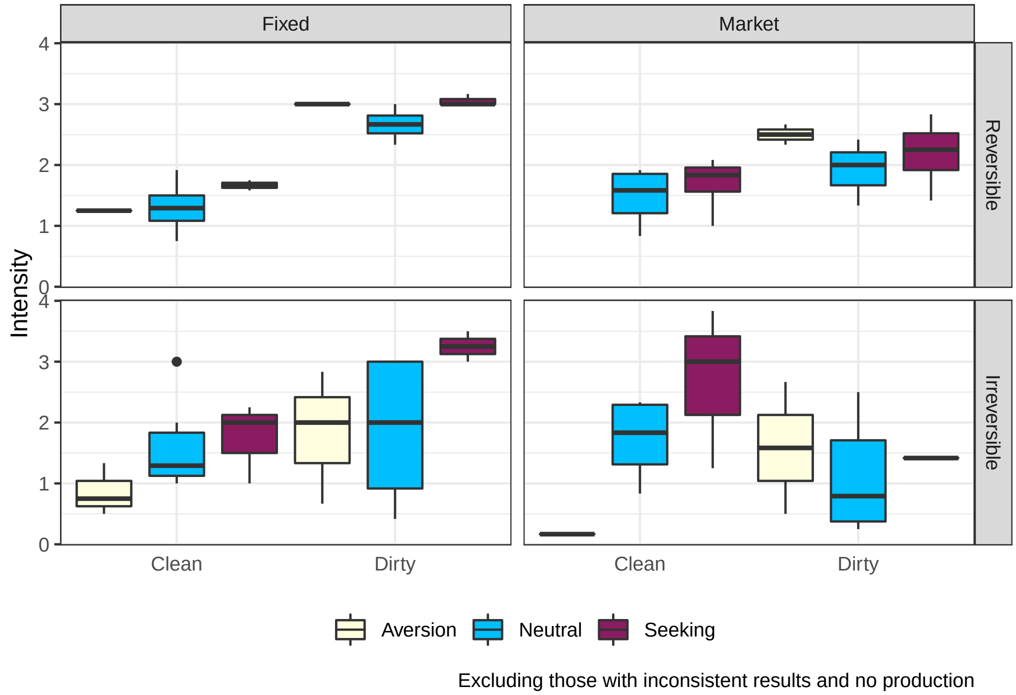

Figure 31: Ambiguity Levels and Mean Intensity Choices. Colour Coded for Ambiguity Tolerance. A Rising or Falling Trend Across Categories Would Indicate Relationship.

The final model (in Appendix G.2) includes gender, major and use of the intensity calculator tool. This button allowed participants to test the effect on their total earnings of an intensity selection and given permit price and was intended to assist with their decision making. In determining the final model to be used, as well as considering overall fit, another consideration was whether coefficients made sense. There is a limited range of possible values for intensity (0-5), so models that produced coefficients indicating results outside this range could be excluded.

Errors are clustered on the individual level and cluster-robust standard errors were used with a Satterthwaite correction, as discussed previously. Exclusions to the data set were also applied, as per the above discussion (Section 7.1.3).

It is expected that dirty firms would choose intensities 2 levels higher than clean firms, however testing confirmed the difference from 2 was statistically significant in all cases.

The additional explanatory variables added to the model are intended to shed some light on the decision making by participants. Use of the intensity test button was not significant following the corrections, but suggests that increased use is associated with a decrease in intensity (this did not change when an interaction with reversibility was trialled). The impact of this on the firm would depend on the firm type, though no significant effects were detected when interacted. A lower intensity might bring a dirty firm down toward the target, or a clean firm down toward a more optimal intensity choice. It could also mean either firm chose an intensity level that was too low. Use of the test button might be greater when the participant intends to change their intensity anyway.

Men apparently chose higher intensities than women. It could be that this indirectly reflects the effect of differing attitudes to risk or ambiguity. Although there was no difference detected along gender lines for risk attitudes, there was for ambiguity. The other factor that apparently had an effect on intensity choices was study background. Compared to commerce majors, students in the humanities, social sciences and health tended to choose higher intensity levels. The implications of these differences obviously also depend on the firm type, but adding interactions by firm type didn't produce any significant coefficients.

One factor that might relate study background to choices in the experiment is maths ability. This was not tested for, but is one possible reason why students from different schools might have quite different results. Controlling for maths ability has been found to alter outcomes in risk assessments, for example (Dave et al., 2010).

7.3 Trading in the Permit Market

Based on their revenue schedules and predicted pricing, dirty firms were expected to purchase permits to produce output and not sell any, whereas clean firms were expected to sell permits after producing output and not buy any. However, there was a lot of extra trading through all of the sessions (almost all participants engaged in this at least once).

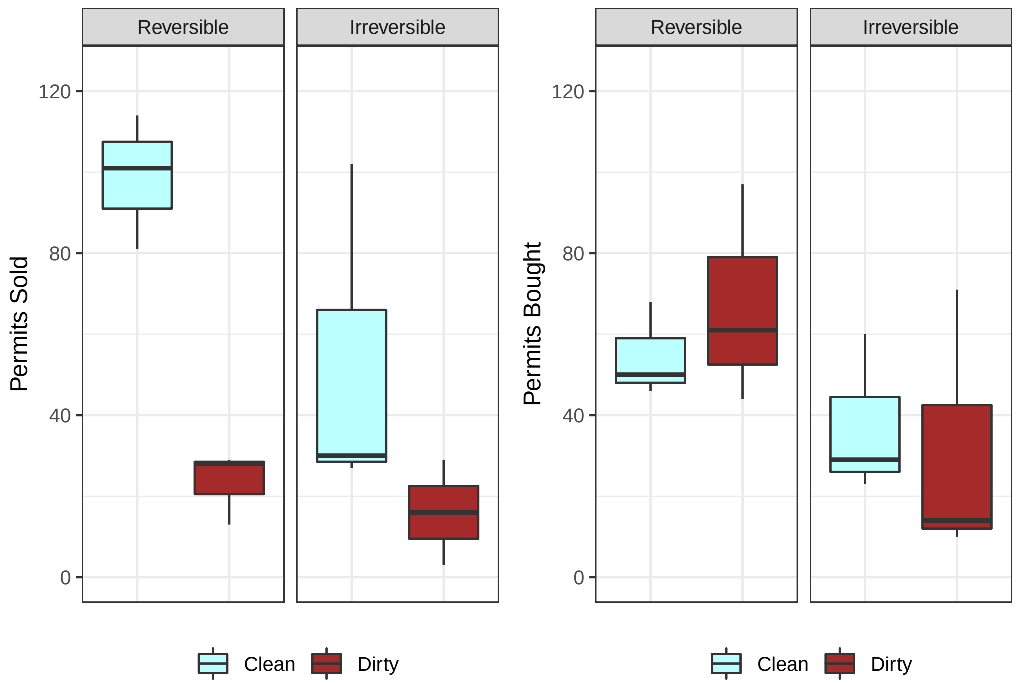

Figure 32: Total Trading by Firm Type in Permit Market (All Periods). Clean Firms were Expected to Only Sell Permits and Dirty Firms were Expected to Only Buy Permits.

Overall, clean firms accounted for a large proportion of total trading, both as the buyer and seller of permits. And, looking specifically at the market treatments, while total purchases of permits didn't differ dramatically by firm type, though only dirty firms were expected to buy permits, clean firms did account for far more sales than dirty ones. That clean firms would sell permits was expected, but overall this outcome suggests that clean firms were more active in the permit market.

Permit prices appear to have been affected by the firm type engaging in the trade, also. Testing (Appendix C) confirms that prices tended to be higher when clean firms were buying and lower when clean firms were selling. One possible reason for this difference may stem from the firms' respective revenue schedules. Clean firms could more readily produce output without purchasing permits first (they could make money producing below the target even if the permit price was zero). This may have meant they were relatively less sensitive to pricing in the permit market.

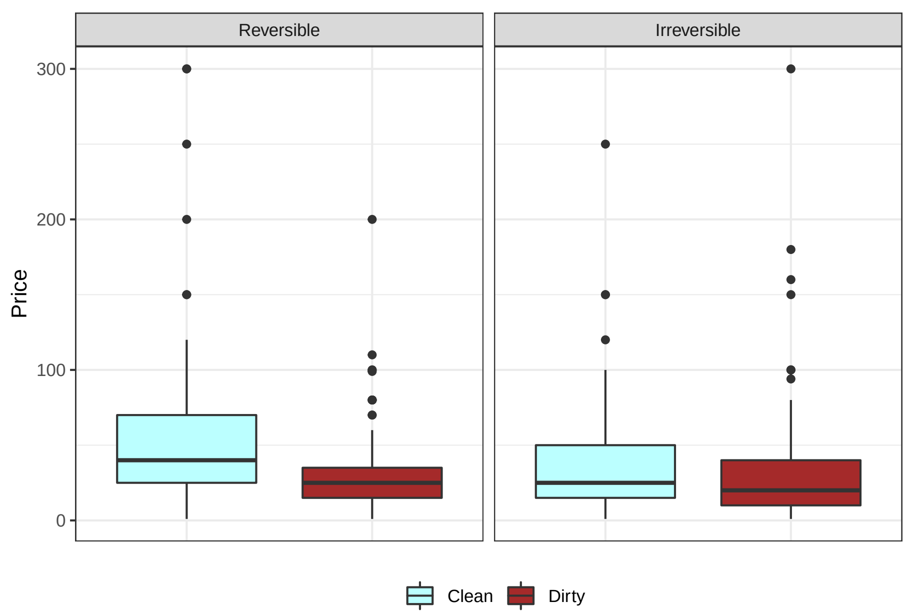

Figure 33: Buying Prices by Firm Type and Treatment. Clean Firms Tend to Pay More.

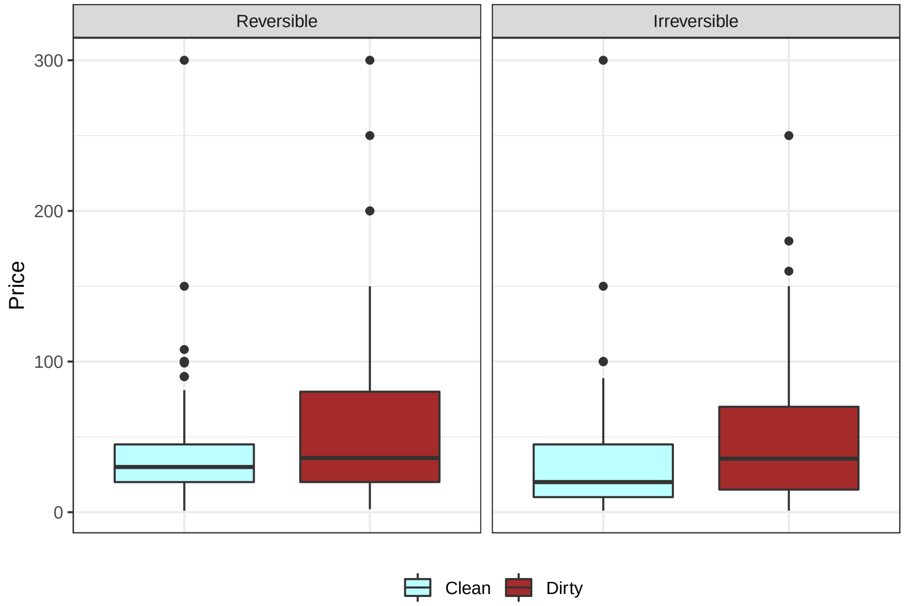

Figure 34: Selling Prices by Firm Type and Treatment. Dirty Firms Tend to Receive More.

In contrast, dirty firms would have to purchase permits first in order to produce output (their revenue schedules were such that producing below the target would look quite unappealing unless they were expecting permit prices to be very high). This entailed taking a loss upfront before being able to realise earnings. Perhaps, as a consequence, dirty firms were more likely to consider the final earnings associated with their transaction before purchasing. Of course, dirty firms could also consider producing output at the target intensity level, where permit prices were irrelevant.

It is possible to detect instances of permit purchases in excess of what would be expected given the individual's intensity and output decisions for a given period. These extras actually accounted for a relatively large proportion of total trading, as well as a large amount of the unsold permits that were left over in periods.

In the fixed-price treatments, since permits could be freely purchased so long as participants had sufficient funds, and sold so long as they had sufficient holdings, any additional transactions are likely to be a sign of boredom in the experimental setting, or errors (clicking too many times and having to undo the error, for example). There was no way to employ this strategy to increase earnings when prices were fixed. In market treatments, however, there may have been an intention to profit from the extra trading.

Figure 35 illustrates the number of extra permit purchases through the session. State does not seem to have a clear effect, and it may be that since firms were producing more in the high demand state the increased demand for permits for this purpose might tend to reduce extra trades.

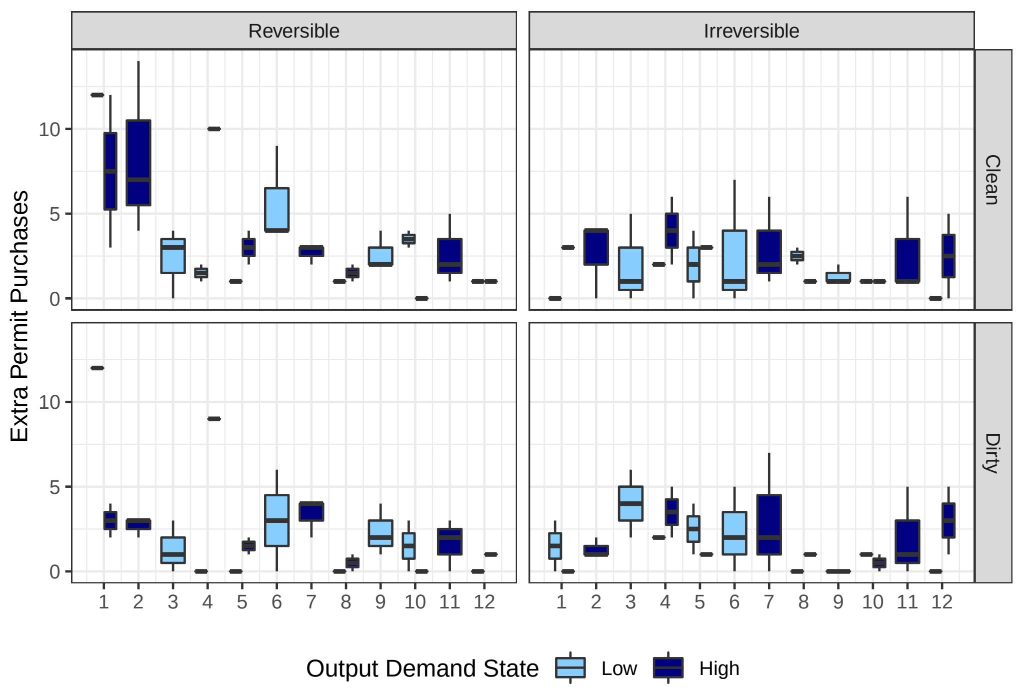

Figure 35: Total Extra Permit Purchases Per Period in Market Treatments by Firm Type. Demand State Doesn't Make Much Difference.

The volume of extra purchases appears to decline over the session in the reversible treatment, particularly for clean firms, whereas the trend is flatter overall in the irreversible treatment.

Presumably, the intention of this permit ‘flipping’ was to make a profit. However, given that prices are generally observed to decline through the period (and there is no banking of permits), the scope to profit from flipping permits was limited. It is possible that some of this behaviour might reflect difficulties understanding the experiment (bearing in mind the 3 participants who produced no output at all but did buy permits).

7.3.1 Extra Trading and Profits

Assuming that participants are acting as profit-maximising firms, they would not be expected to do things, or at least not keep on doing them, if they didn’t profit as a consequence. Overall, profits tended to rise throughout the experiment in market treatments (Figure 36), but there were individual exceptions.

It was expected that clean firms would earn more than dirty ones, because of differences in their revenue schedules. This is corrected in the final payoffs via a different conversion rate to Australian dollars. It looks as though profits for some participants, and dirty firms in particular, were quite flat to declining over the session. A number also ran out of money entirely. This appeared to be more common in the irreversible treatments.

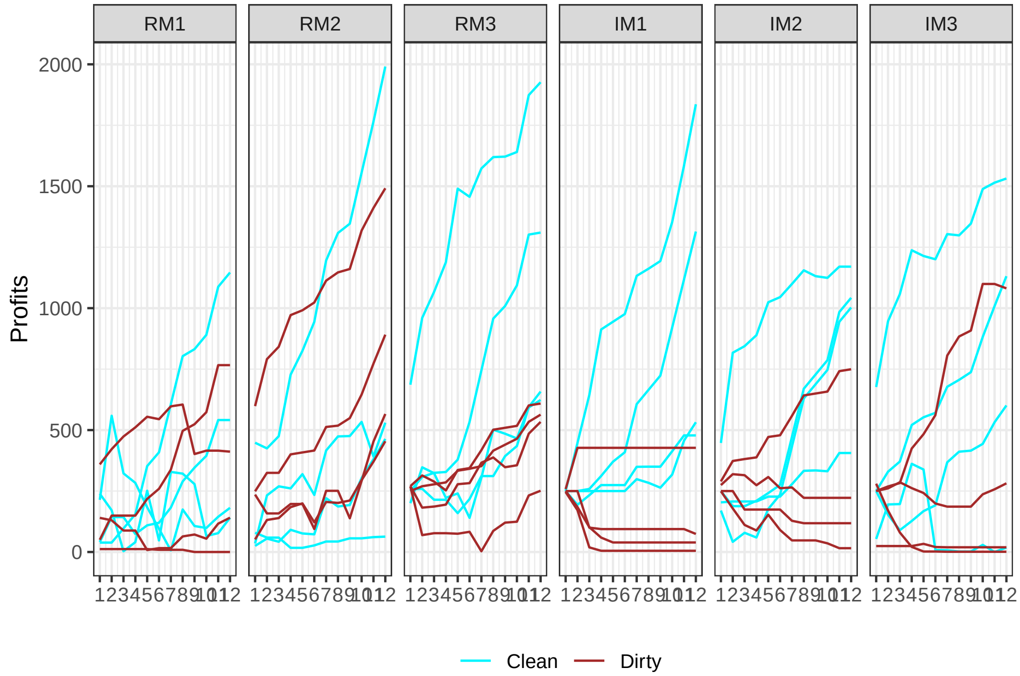

Figure 36: Total Lab Dollar Earnings by Period (Market Treatments). Clean Firms Expected to Earn More (Later Adjusted for Final AUD Earnings)

To get an idea of the impact of extra trading activity on profits in the experiment and responses by participants, profits were regressed on the number of unnecessary permits purchased in each period. The period is used to capture trend effects across the experiment, and interacted with firm-type and treatment dummies to capture differences there, also. The demand state does not appear to be relevant here, nor does there appear to be a significant difference in early versus later periods in terms of the trend. Earnings are expected to be driven by output, which is included here and interacted with the absolute difference of the individual's intensity choice from their predicted optimum. This variable accounts for differences in the predicted intensity level by firm type, and offers a reasonable guide to deviation from optimal decision making. The full output of the model building is in Appendix G.3. Once again, cluster-robust errors are used, with errors clustered at the individual level, with Satterthwaite corrections.

To this base model was added the number of extra, unnecessary permit purchases, interacted with firm type and market type to detect differences between each of these groups. Use of the test button on the market screen was also found to be a factor, along with the participants' majors. These offered more information still when interacted with the number of extra purchases.

For clean firms, each additional purchase of unnecessary permits reduced profits by about L$70 in the reversible market and L$190 in the irreversible market. For dirty firms, each additional purchase reduced profits by around L$35 in the reversible market, but increased profits by about L$35 in the irreversible market. Only the coefficients for the irreversible treatments were significant, however. It seems the effect of this extra trading varies enormously by firm type. This might have something to do with the prices at which firms bought and sold at, with clean firms tending to pay more and receive less when trading permits. Perhaps, also, it indicates a behavioural difference driven by the firms' differing net revenue schedules, where dirty firms face upfront losses in order to produce output that might make them more price conscious (even if they are making unnecessary purchases).

Use of the test button on the market screen increased profits, with every additional click lifting earnings by around L$24. This might be because this calculator tool was designed to be used in the context of output decisions, and so using the button could be predictive of someone less inclined to make unnecessary purchases in the first place. In general terms, the button may just be helpful in assisting with decision making, thus improving profits.

The participants' study background was also found to be relevant here. Figure 37 presents the effect on profits of different majors on their own, and when an additional extra purchase is made. These base figures represent the difference in profits versus a comparable commerce major (i.e., the change in the intercept). Of interest here is the differential effect of this extra trading activity, based on major (this represents the change in profit with an extra purchase relative to the change for a commerce student). This might reflect differences in maths ability - success here hinges on selling at higher prices than when buying - or even the speed of their response time.

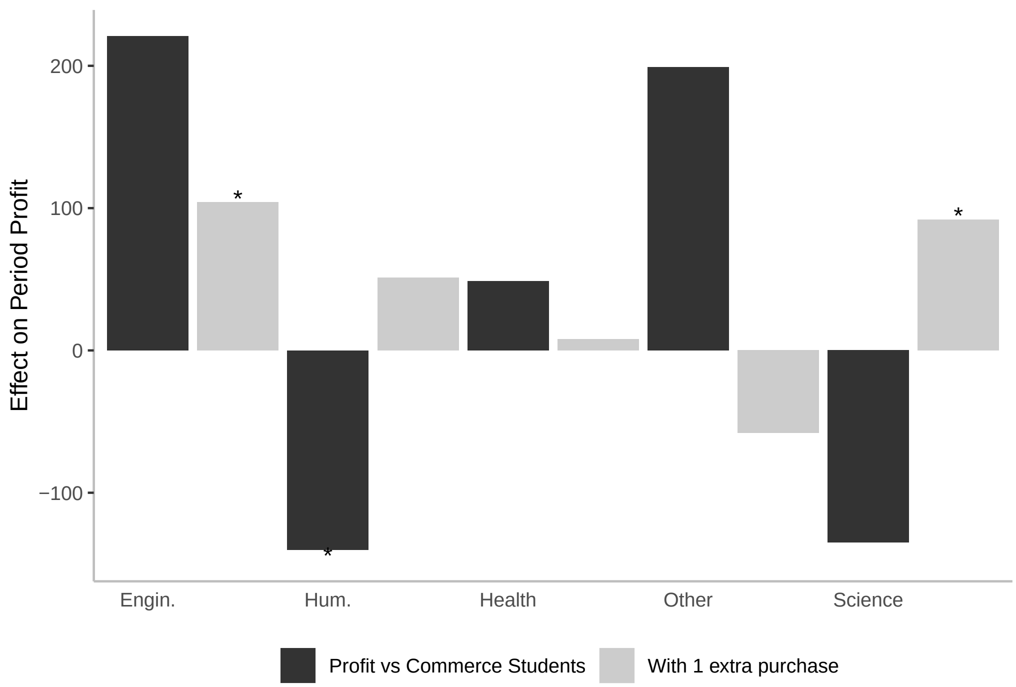

Figure 37: Regression Results of Fixed Effect of Majors on Per-Period Profits and Effect of Purchasing Extra Permit by Major.

Extra trading appears to have benefited some participants and hurt others. Aside from firm type, reversibility of the intensity choice may have some bearing here on the impact, as well as other factors pertaining to the individuals in question, including their study background.

7.3.2 Risk & Ambiguity and Extra Trading

It seems possible that there could be some relationship between risk or ambiguity attitudes and this extra trading activity in the market treatments. Based on testing (see Appendix F.2.4 and Figure 38), there are indicators that there may be a linear relationship between risk attitudes and the total amount of extra permits purchased in both market treatments.

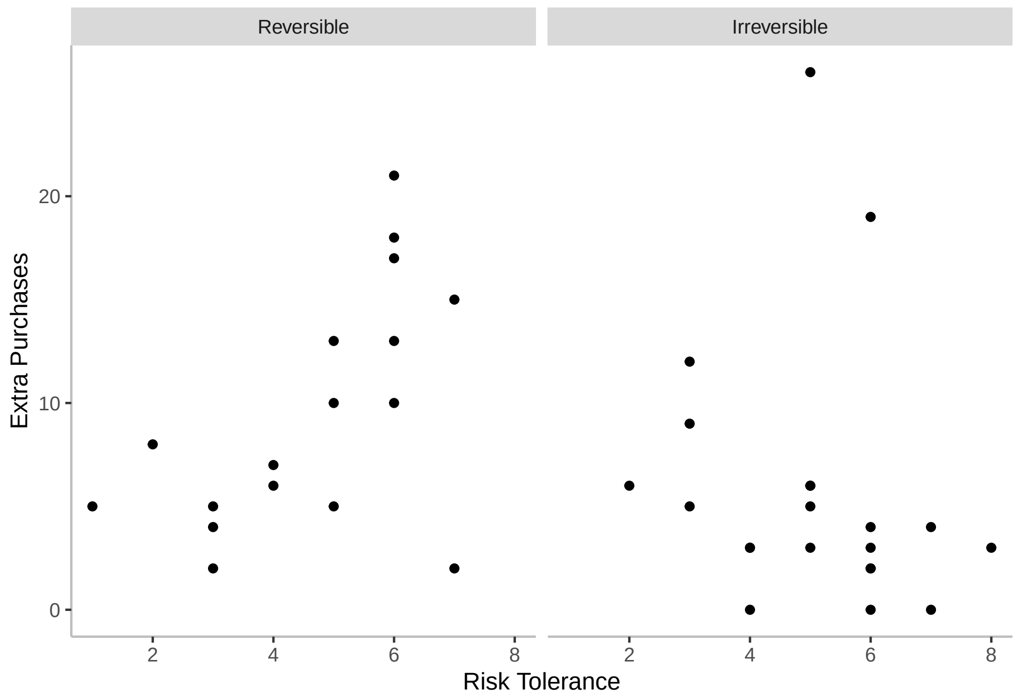

Figure 38: Risk Tolerance and Extra Purchases

To test this further, a linear regression is used. Individual period totals of unnecessary permit purchases are regressed on risk attitudes (as a 3-level factor, as discussed in Section 7.2.1), with controls added for reversibility of the intensity choice and firm type. As already noted, there could be some issues here with small sample size in the way risk and ambiguity results spread across different groups. In this case, the outlier effects do not seem so serious as when using intensity data. Again, however, results are better when using risk alone. Output from the iterative model building process is included in Appendix G.4. (A regression on all data, no exclusions, is also included for comparison.)

This model considers only the market treatments and includes controls for reversibility and firm type. Testing also indicated time effects across periods, with differences between high and low demand states and early periods and later, so controls for these are also included to capture this.

Once again, errors were clustered at the individual level, with cluster-robust errors and Satterthwaite corrections.

Risk attitudes seem to have some bearing on decisions here. Figure 39 presents the effect of rising risk tolerance, relative to the comparable case for a risk averse individual. In most cases the effect is positive, however, being risk seeking is not necessarily associated with more extra purchases relative to the risk neutral case. (Note that there are no estimates for risk-neutral dirty firms in the irreversible market.) The relationship between risk and extra purchases is not clear, however two of the coefficients were significant. Risk-neutral clean firms purchase more permits than risk-averse ones. This adds some weight to the idea that this behaviour might be driven by propensity to take risks, since it is far from a given that the individual might make a profit from the activity.

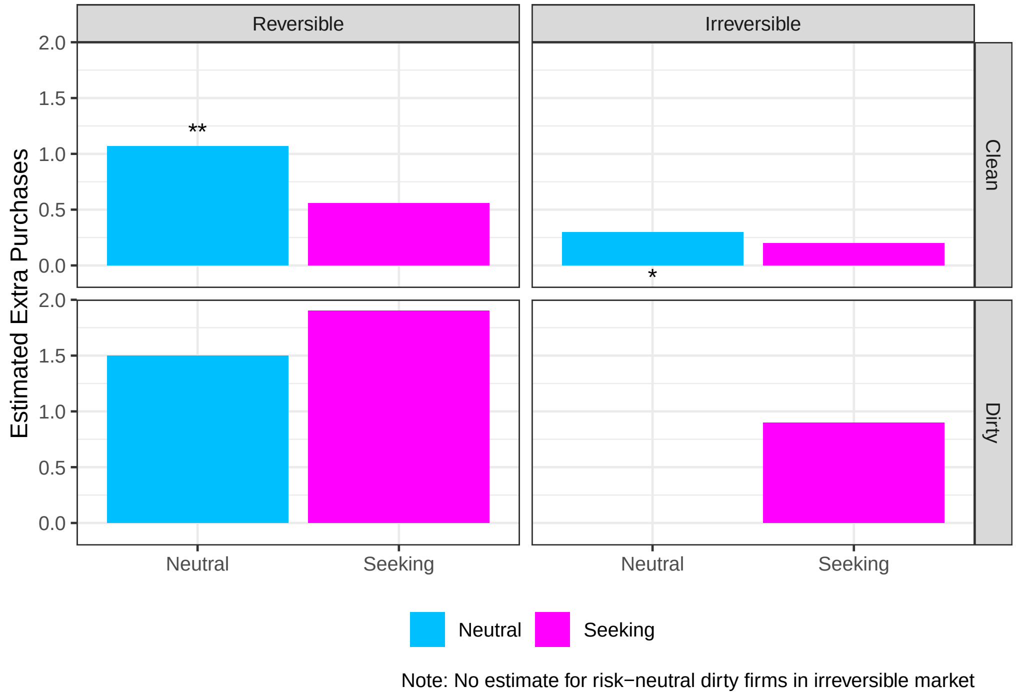

Figure 39: Estimated Effect of Risk Tolerance on Number of Unnecessary Purchases in Market Treatments, Relative to Comparable Risk-Averse Individual

Study background appears to be relevant here, and when interacted with risk attitudes, more detail is available on this relationship. Relative to risk-averse commerce students, there is a tendency for participants from other schools to make more extra purchases, with the exception of engineering students. Although a higher tolerance for risk was generally associated with more extra purchases, it was not necessarily the case that being risk seeking had a greater effect than being risk neutral. Only coefficients for risk-neutral science students and students from ‘other’ majors were significant, and for other majors, the number of extra unnecessary purchases relative to risk-averse commerce students appeared to decrease with rising risk tolerance.

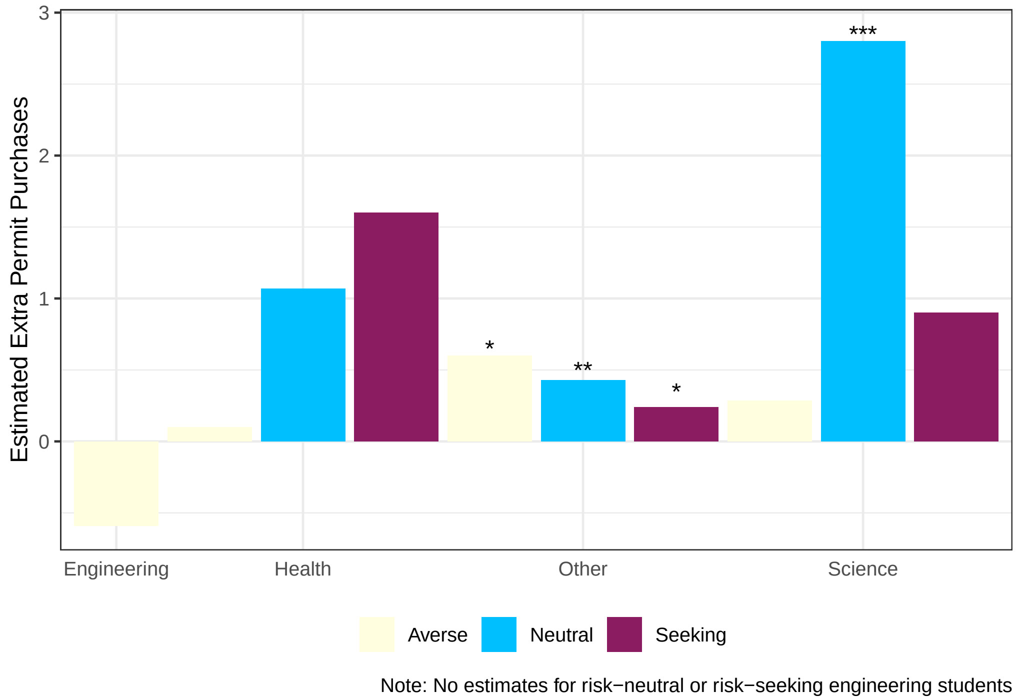

Figure 40: Estimated Effect of Risk Attitudes on Extra Unnecessary Permit Purchases by Major. Relative to Risk-Averse Commerce Major.

Although significant effects were not detected in the regression, it is possible there might be some relationship here between risk and ambiguity attitudes and use of the market test button (Appendix C.1). The negative sign of the coefficient suggests there were fewer extra purchases the more the test button on the market screen was used, a relationship also suggested by Figure 41. This suggests that this particular calculator tool may have been of use to some participants. It was intended to help determine the net earnings when producing output, so those using it were perhaps more inclined to trade permits in order to produce output, rather than for its own sake.

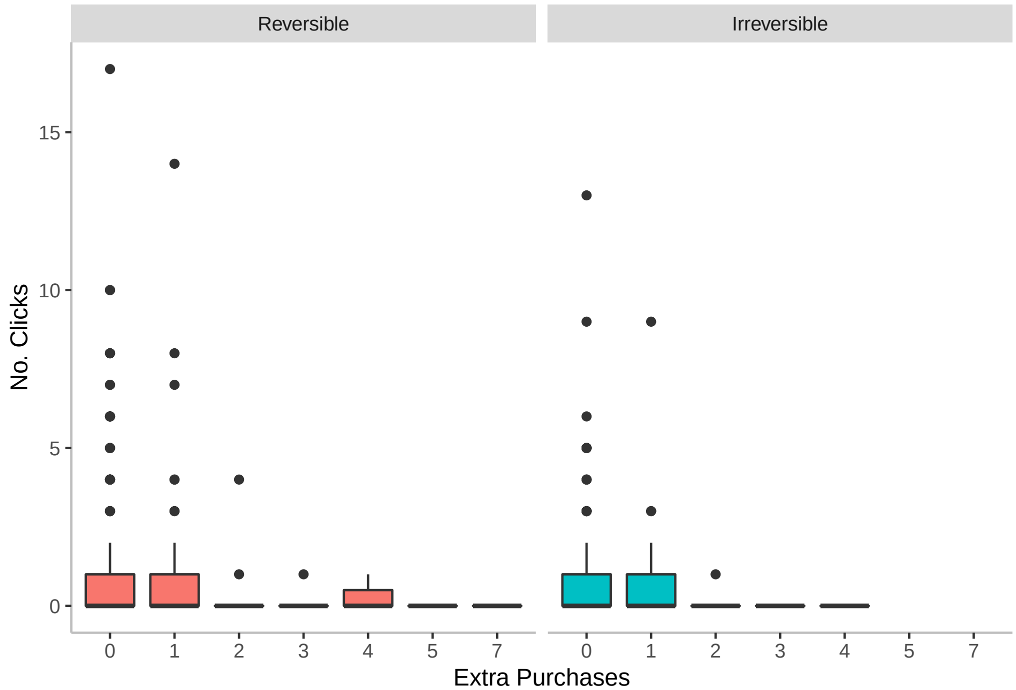

Figure 41: Extra Purchases vs Use of Market Test Button

Perhaps greater risk tolerance might be associated with reduced use of this tool if participants feel comfortable trying things without checking outcomes first, for instance. Conversely, increased risk tolerance might also mean greater willingness to seek out extra information for decision making (Turner et al., 2006). This might then feed indirectly into outcomes such as this extra trading activity.

Although extra permit trading activity was widespread across the market sessions, regression results suggest that personal attributes including risk attitudes and study background may at least partly explain the differences, though the effects may vary somewhat between individuals.

8 Discussion and Limitations

Final emissions outcomes are naturally of interest to policymakers with reduction goals in mind. In the context of an intensity target, this thesis examines the effect on emissions of the use of a market mechanism to determine permit pricing, as well as the reversibility of the technology choice. With the setup here, as described in the theory section, intensity choices and emissions outcomes were expected to be identical in all treatments. However, differences were found. Although the small number of sessions run limits the size of the data set for testing, results offer evidence of treatment effects when comparing pricing mechanisms and reversibility of the intensity choice. There appears to be some interaction between these two factors and also with the firm's existing technology. In some cases, effects are only detected for firms with dirtier production technology.

8.1 Reversible vs Irreversible Intensity Choice

Overall, irreversibility leads to lower intensity levels, lower output and lower emissions. Reversibility of the intensity choice affects early-period decisions in particular, with firms tending to start with higher intensities and waiting longer before lowering them in the irreversible case. The effect of reversibility varies with pricing mechanism and firm type, with the strongest evidence of a difference in intensity decisions for firms with dirtier technology.

Reversibility affects decision making in the first 4 periods versus the rest of the experiment. The difference here is greater when the market mechanism is used. One reason for this may be because of a tendency to delay changing intensity after the initial selection in these treatments. This suggests a behavioural response by participants when confronted by the constraint on intensity choice where they tend to choose higher intensity levels to begin with and then wait longer (2-3 periods) before changing this selection.

Despite the delay in changing intensity, mean choices tended to be lower in the irreversible treatments compared to the reversible ones. This is, to some extent, likely due to the nature of the constraint here, where some participants overshot and could not correct their mistake. Arguably, this mirrors the real-world situation where firms must make large, irreversible investments. In this experiment, however, the revenue schedules are fixed such that dirty types are better off producing above the target and clean firms below, whereas in the real world, firms can make investments in cleaner technology to alter their cost structures.

Lower intensity choices also tended to mean lower output, and thus lower emissions overall for the irreversible treatments. Reversibility made an even greater difference when market pricing was used. As such, the combination of uncertain permit prices and an irreversible intensity choice had an additional effect on emissions outcomes.

What this suggests is that the impact of emissions reduction policies may differ depending on the nature of a firm's production technology and investment decisions. This is of particular importance if a policy is to be applied across industries (which may have different investment patterns). If policymaker goals require large irreversible investments in certain, high-emission industries, for instance, then one policy may be more effective than another in motivating this. This experiment does not test this question directly, though it does offer some evidence that reversibility may lead to different responses to incentives. When faced with an irreversible rather than reversible choice, participants made decisions in different ways, which could be interpreted as wariness about making intensity choices. An additional effect was the tendency to choose lower intensities over the course of the experiment, particularly in the case of dirty firms.

Another possibility worthy of further investigation, not tested here, is that risk-averse managers may respond to uncertainty by avoiding making large, fixed investments if they have the option to make smaller, reversible investments instead (even if these are sub-optimal and increases costs in the long-run). This is the sort of effect that Dorsey (2019) attempted to measure, in the context of uncertain policy, that created uncertainty about pricing. Similarly, and also in the context of electricity generation technology, retrofitting carbon capture and storage technology may be more likely in the face of emissions pricing uncertainty (Blyth et al., 2007), and investment in cleaner technologies may be likely to be delayed when pricing is uncertain, with these delays increasing with increases in the uncertainty (Fuss et al., 2009). While this experiment offers no insight here, since participants were not able to choose between reversible or fixed investments, differences were noted, including a tendency to delay changing intensity in the irreversible treatments compared to the reversible ones. And this tendency to delay was more notable where pricing was uncertain, suggesting avenues for further investigation.

8.2 Market vs Fixed Permit Prices

The permit pricing mechanism had a clear effect on intensity choices and emissions outcomes. Emissions were higher and closer to predictions when permit prices were fixed, and significantly lower when prices were market determined.

Overall, intensity choices differed based on the pricing mechanism, but the scale of the difference was greater when the intensity choice was reversible. What this tends to mean is that firms choose intensities closer to the prediction when prices are fixed and incentives clear. In this case, dirty firms got closer to predictions than clean ones. One possible reason for this is due to the clean firms' revenue schedule. In the fixed-price treatments, firms know precisely how much they will earn at a given intensity level as the price of permits is known. In theory, they ought to be able to choose correctly each time. For dirty firms, the difference between the optimum choice and another is much more obvious than for clean firms, however. Clean firms see only a L$1 difference between choosing the predicted intensity and choosing the target intensity for each unit of output produced, meaning the incentives may be less distinct.

Intensity choices closer to the prediction also mean output tends to be closer to the prediction and, thus, emissions are, also. Although testing suggests that emissions outcomes for all treatments bar the reversible fixed-price treatment were different to predictions, the fixed-price treatments were generally closer. Emissions in market treatments were considerably lower.

If a key objective is to achieve a set emissions outcome, it may be that a fixed-price policy would be more effective. Of course, this assumes that the regulator is able to set the price appropriately, which can be expected to be difficult in the presence of information asymmetry. This experimental setting abstracts away from such uncertainty, as it does from the possibility of regulators having to change pricing over time to adjust to new information. This might reduce the certainty of taxes and their relative appeal to firms making planning decisions.

Alternatively, market pricing might be preferable if the priority is to lower emissions.

8.3 Risk Seeking and Loss Aversion in the Permit Market

When permit prices are determined in a market, participants become traders. Trading volume was unexpectedly high and often did not reflect an underlying need for permits to produce output. Firms' technology level affected the price at which they bought and sold permits, reflecting the impact of their differing revenue schedules. Prices were observed to converge over the session as a whole, and there was no significant difference detected between reversible and irreversible treatments in terms of this convergence. The results of the risk and ambiguity assessments completed at the start of the session and some basic demographic data collected offer some possible insight into behaviour here.

In order to try and assess behaviour under uncertainty in this experiment, an attempt was made to analyse intensity decisions relative to the target intensity. The mean absolute distance between intensity choices and the intensity target was taken as one measure of uncertainty avoidance in the experimental setting. This is because production at the target would mean earnings were not dependent at all on permit prices. It was theorised that participants seeking to reduce uncertainty in the market treatments, where prices were uncertain, might tend to choose to produce at the target level. If so, then some relationship might be found between risk and/or ambiguity attitudes and the size of this gap.

While no relationship was found, this could well be due to issues with the small sample size, or because the measure of avoidance used is not an appropriate measure of how participants perceived uncertainty in the experiment.

There may also be issues with using these measures of risk and ambiguity attitudes. Prior testing of risk and ambiguity attitudes demonstrates that results tend to be domain specific (Armantier & Treich, 2016; Michael L Platt & Scott A Huettel, 2008; Nosić & Weber, 2010; Weber et al., 2002) and sensitive to methodology and framing (Fellner & Maciejovsky, 2007; Michael L Platt & Scott A Huettel, 2008; Weber, 2007). For this reason, results are not necessarily comparable across studies. Similarly, this may mean that even if risk attitudes collected are accurate, they may only reflect attitudes for the particular context in question. The way participants respond to binary lottery choices may be quite different to the way they respond to the experimental setting, which might reduce the usefulness of these measures and mean that the absence of correlation does not actually mean that risk attitudes are not relevant. Additionally, some studies have measured both attitudes and perceptions of risk, finding that differences in these perceptions were important determinants of decision making (Michael L Platt & Scott A Huettel, 2008; Weber et al., 2002; Weber, 2007). Such perceptions were not examined here, but may have varied between individual participants in ways that affected outcomes in the experiment, also.

A relationship between risk attitudes and trading activity was detected, however. Rather than trading permits purely in the context of producing output, many participants bought unnecessary permits. There is no clear reason for this. To some extent, these results might support findings by Fellner and Maciejovsky (2007), that greater risk tolerance is associated with increased market activity (bids and asks submitted in their market experiment). In this experiment, we might interpret such speculative behaviour as similar to trading activity in an asset market. Similarly, Durand (2013) noted an association between increased trading activity and a measure of risk tolerance, where the number of trades was taken as one possible proxy measure for overconfidence in a market context.

To some extent, higher levels of risk tolerance were associated with an increase in the number of extra permits purchased, but only coefficients for clean firms were significant. Participants' study background was apparently also relevant, and interactions with risk attitudes suggest differential effects here, also.

Risk attitudes matter, but they matter in different ways to different people. While a greater tolerance for risk was generally associated with more unnecessary purchases compared to the risk-averse case, it was not necessarily true that risk-seeking individuals bought more than risk-neutral ones. The difference by study background might reflect different characteristics affecting how participants fared in the experiment, also. Maths ability, for instance, could affect intensity choices, as well as the decision to purchase permits. Perhaps, also, it affected their perception of the risks involved, or the way they interacted with the experiment itself.

The inclination to engage in extra trading could also affect the way the market works. On the one hand, if volume is otherwise low, speculative trade could assist with increasing volume and improving price discovery. However, large amounts of speculative trading could affect efficiency by moving prices beyond levels justified by supply and demand fundamentals, which in theory would reflect marginal abatement cost at equilibrium (Lucia et al., 2015).

Prices in the permit market were also affected by the underlying technology of the firms making the trade. Clean firms tended to buy for higher prices and sell for lower ones, compared to dirty firms. This may be explained by the differing attitudes toward gains and losses, as per prospect theory (Barberis, 2012). Clean firms could earn revenue from producing output and then realise the gain from selling the permit. Meanwhile, dirty firms had to take a loss upfront before they could produce output, either with negative revenue from output production below the target, or by purchasing a permit before producing output above the target. They could only avoid this by producing at the target intensity. Even if the expected net earnings are identical, from their respective reference points, firms may respond differently to their situation.

This meant that clean firms' profits suffered for every extra purchase they made, presumably because of their relative price insensitivity, while dirty firms tended to do better. The effect of differing attitudes toward gains and losses may be something to consider when implementing policy with an intensity target, depending on how permits are allocated and how pricing is determined.

8.4 Small Sample Limitation

As only a small number of sessions have been run, some care must be taken in interpreting results at the treatment level.

Even under the best circumstances, experiments of this nature typically suffer from small sample sizes. A prospective power analysis can provide guidance for the experimental design in terms of the number of sessions that ideally ought to be run (and thus the number of participants to be recruited). Based on an arbitrary 5% significance level and 80% power, if the experiment is looking to detect a "small" effect (Cohen's d of 0.30) this would require a sample of 175 for a two-tailed test or 138 for a one-tailed test Cohen (2013). This would mean recruiting around 2,000 participants, which is clearly infeasible. The only way tests are likely to have reasonable statistical power with only 6 data points (as was originally proposed) is if testing for very large effects (even at higher significance levels). Here, there are just 3 data points when considering treatment effects.

The occurrence of low power to detect a meaningful difference is a common issue in experiments. Confidence in the findings is increased when a number of tests point to the same result. More obvious treatment effects may be revealed via the "IntraOcular

Trauma" test Friedman & Sunder (1994), where differences can be clear in a graph. With more data points, basic statistical tests on the differences between treatments would have some validity. Boxplots of the key outcomes in the experiment in many cases illustrate clear differences due to treatment effects, but statistical testing may not be valid.

Individual data offers more to work with, though there are still small sample effects evident when considering groupings by firm type and treatment.

8.5 No Choice Between Short and Long-Term Investments

In this experimental setup the nature of the investment decision is enforced, so we can infer nothing about the sorts of outcomes that might occur if firms had a choice between different types of investment. Such an extension might allow even greater insight into the differing impact of pricing mechanisms on investment decisions.

Additionally, although some delay in changing intensity was observed in the irreversible market treatment, there was no opportunity under this setup to observe investment delay, per se, as participants had to choose an intensity level at the outset of every period. An alternate setup that allowed for participants to put off investment might also offer useful insight into investment behaviour under different pricing mechanisms.

9 Conclusion

This thesis reported on an experiment conducted to inform emissions reduction policy employing an emissions intensity target. The research focused specifically on the impact on emissions outcomes of altering the pricing mechanism and reversibility of the firm's technology choice.

The market mechanism resulted in much lower emissions compared to fixed pricing. The result was clear and held regardless of reversibility. As such, policymakers concerned with meeting specific targets might be more inclined to fix prices if using an intensity target. In the Australian context, where the use of a tax may be politically unpopular, however, opting for a market mechanism instead could well result in greater emissions reductions.

The reversibility of the intensity choice tended to affect the pattern of decision making. The difference between early and later periods was particularly marked when combined with the market mechanism, and might support theorising about a tendency to delay large, irreversible investment decisions when pricing is uncertain. Emissions outcomes overall were lower when the intensity decision was irreversible. There appeared to be a behavioural response to the irreversibility. This was particularly apparent in the fixed-price case where all the same information was available to participants, but whereas they were generally able to choose optimum intensities in the reversible case, they did not in the irreversible treatment. The effect seemed stronger for firms with dirty technology.

For policymakers, this suggests that it may not be reasonable to expect the same outcomes from one policy for different industries where the nature of investment decisions varies, and could be an argument for employing different mechanisms to incentivise different types of investments.

9.1 Significance

There are few experimental studies examining emissions intensity target schemes. This thesis lays the groundwork for a better understanding of how different pricing mechanisms affect outcomes under an intensity target. It also offers some insight into how these mechanisms may interact with different sorts of investments made by firms, which might be either short-term or long-term in nature. This will be of particular interest to policymakers intending to utilise an intensity target to meet emissions reduction goals across the economy.

9.2 Future Work

Further work should explore the impact of allowing firms to choose between short-term and long-term investments and the interaction of these choices with permit-pricing mechanisms. This would also afford an opportunity to observe convergence patterns toward cleaner technologies under different policy settings. Additionally, it might be possible to directly observe delays in making longer-term investments in this way.

Another study could reexamine the underlying assumptions of the output market. What difference would it make to the permit market if output markets were also competitive? There would also be scope here to determine the effect of feedback from output markets with changes in permit pricing. Similarly, research could explore the impact of market power in one or both of these markets. Market power might well be an issue in electricity markets, for instance, where large 'gentailers' tend to dominate, still, in the Eastern states. With some degree of power in the wholesale and retail markets, such operators may also exert a strong influence in permit markets. More work in this area in the context of intensity targets would be illuminating.

Another line of research would investigate the trading activity observed in the permit market in more detail. While prices in the market treatments tended to be higher early on in the period and then trend down, a fairly large amount of speculative trading was observed, despite limited opportunities to benefit from this activity in the absence of banking. An obvious question concerns the effect this had on prices and volatility in the market, which this study is unable to determine. One test here might be to see the effect of restricting speculative activity. Would the market have worked better, or might this have hampered price discovery? Further testing could also include the addition of non-compliance traders, that is, individuals that did not need to purchase any permits, but did so for other reasons. These could be traders looking to profit from the transactions, or perhaps non-market actors looking to influence prices (environmentalists might want to increase them, for instance).

A second question concerns the propensity to engage in this extra trading. Are people generally inclined to treat any market mechanism as an opportunity to buy and sell, irrespective of the context? In this case, certain individual characteristics appear to have influenced this activity. With more data, it would be possible to further explore the influence of different personal characteristics on this activity.

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A ORSEE

ORSEE maintains a depersonalised database of participants to be recruited via electronic communications. Students are invited to register as potential participants online. They can then be invited to participate in experiments via email. Any information they receive in the invite can be adapted to maintain confidentiality about the nature of the experiment, and participants are informed of the details collected and privacy rules. The system also tracks no-shows, both for the experiment at hand, as well as for future reference (potential participants may be excluded for repeated prior no-shows).

Using ORSEE, subject pools can by adjusted according to the needs of the experimenter.

The possibility of issues in the selection process may raise some concerns about external validity. This is because of the narrow composition of the subject pool and the fact that subjects are self-selecting (Exadaktylos et al., 2013). It may seem more sensible, when testing economic theory, to use either representative or professional samples. The use of such samples, however, entails a number of disadvantages (Frechette, 2016). Costs may be a particular issue where the opportunity cost of participation may be quite high (relative to students), and availability can be a concern. Both these factors may undermine replicability. Also, although it may seem more natural to use professional managers in this sort of experiment, the necessary abstraction of the experimental environment may pose particular challenges in regards to their ability to engage.

Reassuringly, surveys on the literature suggest that results are generally consistent across subject pools, increasing confidence in the use of student participants

(Exadaktylos et al., 2013; Frechette, 2016). If concerns remain, it may be possible to improve the robustness of the experimental design by running sessions across different locations or different subject pools (by segmenting students by discipline area, for example). This may reveal whether there is, in fact, any effect associated with the choice of participants.

A.1 Participant payments

Payments to participants include a small on-time turn-up fee (A$5). They also receive an additional payment for the two exercises described in Appendix D (~A$5). These payments are determined by a random process to first select which of the lottery choices the payment will be based on, and then another to determine the outcome of that lottery.

Experiment rewards are set with reference to the opportunity cost of participant time to ensure dominance of this factor, so that changes in utility arise from rewards, not some other factor (Friedman & Sunder, 1994). With an estimated run time of about two hours, the exchange rate from lab dollars is set to try and ensure participants playing both firm types have the potential to receive similar payments of about A$40 (which is roughly equivalent to the current minimum wage paid for two hours of work).

The exchange rates for each participant type were determined by calculating the maximum predicted earnings (with an equal number of high and low demand states) over 12 periods, then adjusting this to result in a payment of around A$30. In this way, a participant who turns up, completes the exercises at the start of the session and makes optimal decisions throughout would earn about A$40.

B Prices and Trading Data

Based on the movements in prices and volume in the market treatments run, and noting as ever that few sessions were run, there appears to be some 'settling' of behaviour after the 4th period. The spread in prices for each period narrows considerably after this point (Figure 42. The line graphs (Figure 43) illustrate price movements over the experiment for individual sessions, by way of comparison. To a certain extent, particularly high prices early on lead to several participants running out of money, pushing down prices in later periods.

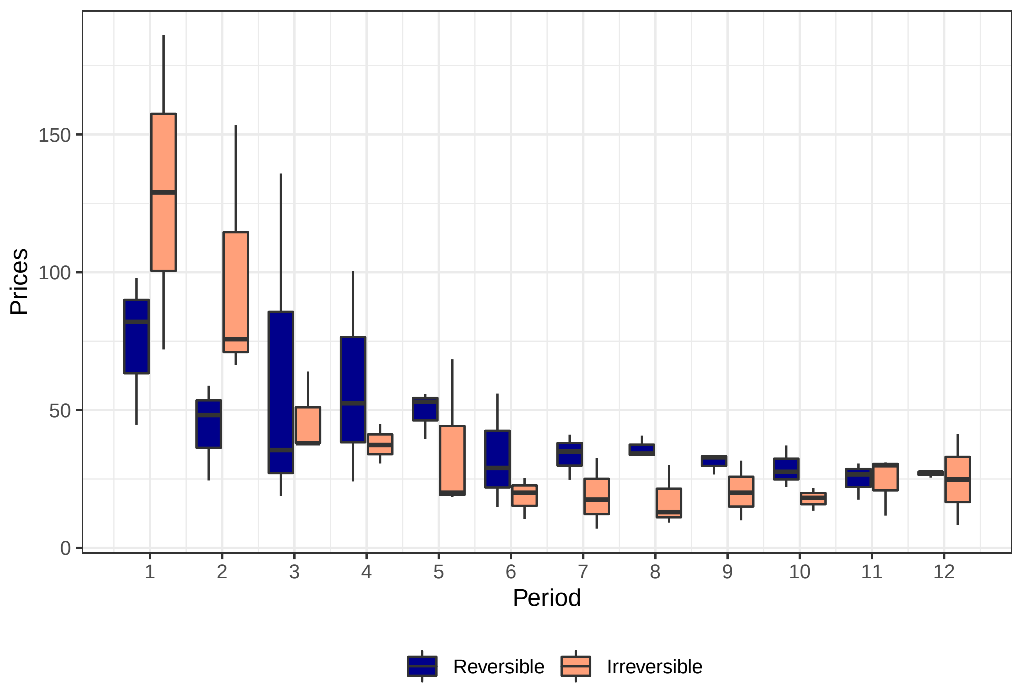


Figure 42: Prices by Period

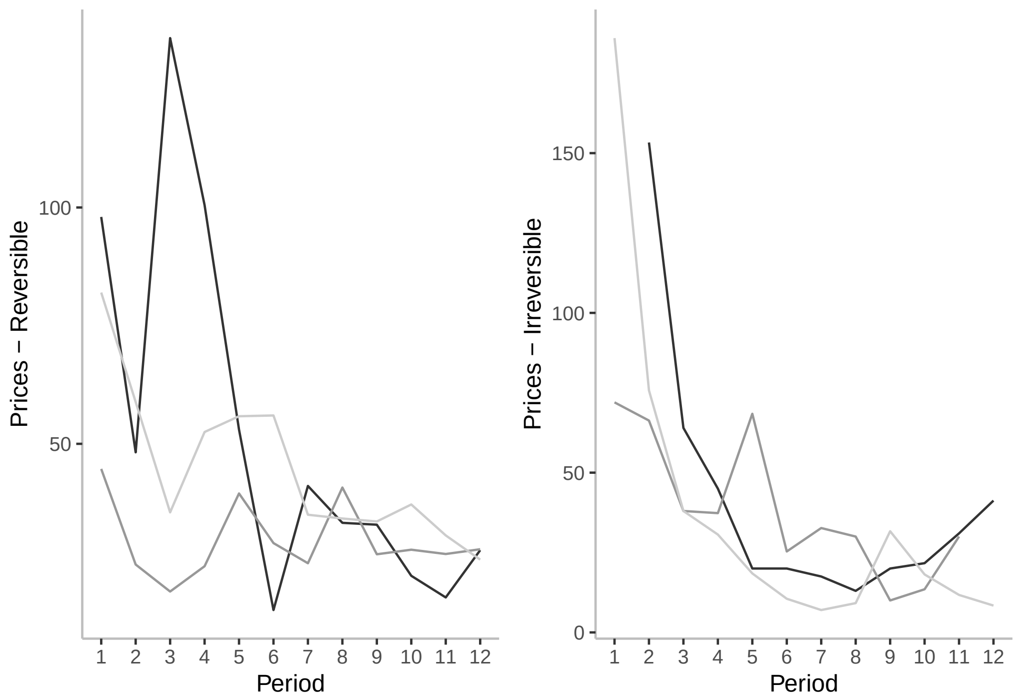


Figure 43: Prices by Period (session period averages shown separately)

Trading volume (Figure 44) declines somewhat after the first few periods, but remains more variable over time compared to prices. Volume over the experiment tended to be choppier in the reversible treatments (Figure 45).

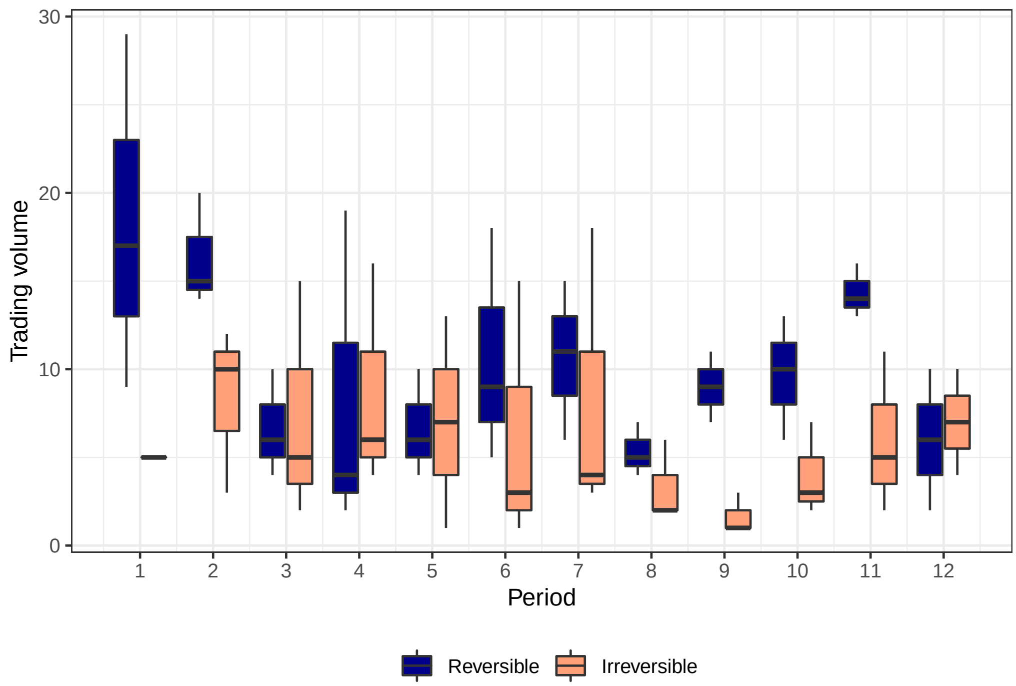


Figure 44: Trading Volume by Period

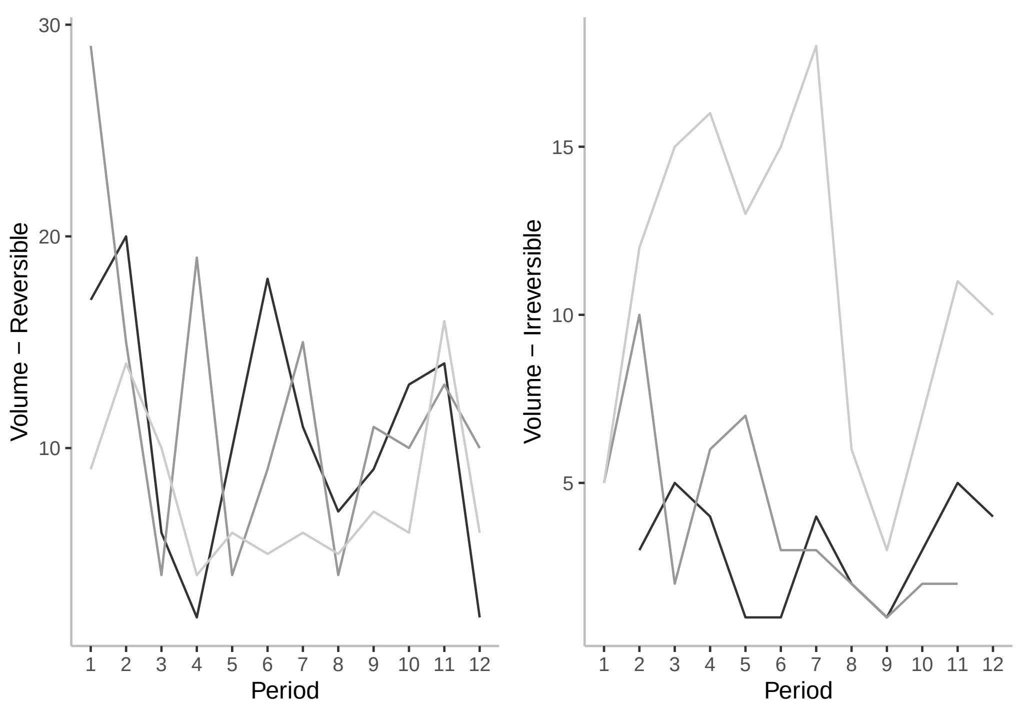


Figure 45: Volume by Period (session average shown separately)

C Testing of results

Table 13: Test Results for Mean Intensity Results by Firm Type and Treatment

	Clean			Dirty		
Test	Mean	Wilcoxon	Perm	Mean	Wilcoxon	Perm
RM vs Pred	1.625	0.064	0.026	2.215	0.064	0.047
RF vs Pred	1.444	0.064	0.038	2.882	0.643	0.296
IM vs Pred	1.715	0.064	0.072	1.688	0.059	0.027
IF vs Pred	1.458	0.197	0.126	2.104	0.064	0.042
RMvsRF	NA	0.121	0.122	NA	0.081	0.066
RMvsIM	NA	0.663	0.709	NA	0.077	0.090
RFvsIF	NA	1.000	0.952	NA	0.081	0.055
IMvsIF	NA	0.663	0.453	NA	0.376	0.131
RM 1-4 vs Pred	1.688	0.064	0.080	2.167	0.064	0.046
RF 1-4 vs Pred	1.750	0.047	0.025	2.854	0.637	0.200
IM 1-4 vs Pred	2.375	0.064	0.032	2.208	0.064	0.029
IF 1-4 vs Pred	2.062	0.059	0.026	2.521	0.064	0.131
RM 5-12 vs Pred	1.594	0.064	0.032	2.240	0.064	0.048
RF 5-12 vs Pred	1.292	0.064	0.098	2.896	0.637	0.422
IM 5-12 vs Pred	1.385	0.643	0.227	1.427	0.064	0.027
IF 5-12 vs Pred	1.156	0.643	0.614	1.896	0.064	0.034
RM1-4 vs 5-12	NA	0.825	0.719	NA	0.663	0.782
RF1-4 vs 5-12	NA	0.064	0.053	NA	1.000	0.787
IM1-4 vs 5-12	NA	0.081	0.071	NA	0.081	0.037
IF1-4 vs 5-12	NA	0.077	0.075	NA	0.190	0.117
5-12 RMvsRF	NA	0.190	0.126	NA	0.077	0.072
5-12 RMvsIM	NA	0.663	0.480	NA	0.081	0.053
5-12 RFvsIF	NA	1.000	0.681	NA	0.077	0.042
5-12 IMvsIF	NA	0.663	0.582	NA	0.190	0.102

Note:
p values reported from Wilcoxon and permutation tests
Two-tailed tests of difference from prediction or between treatments (as indicated)
Mean values for intensity outcomes reported in Mean column
RM=Reversible Market, RF=Reversible Fixed
IM=Irreversible Market, IF=Irreversible Fixed
1-4 refers to periods 1-4 and 5-12 to periods 5-12
1,152 observations in total for all periods; 144 by firm type for each treatment
384 observations in total for 1-4; 48 by firm type for each treatment
768 observations in total for 5-12; 96 by firm type for each treatment

Table 14: Total Output and Emissions by Firm Type with Results of Tests vs Predictions and Between Treatments

	Output (Units)						Emissions (Output x Intensity)					
	Clean			Dirty			Clean			Dirty		
Test	Mean	Wilcoxon	Perm	Mean	Wilcoxon	Perm	Mean	Wilcoxon	Perm	Mean	Wilcoxon	Perm
RM vs Pred	30	0.064	0.035	20	0.064	0.043	40	0.643	0.234	47	0.064	0.034
RF vs Pred	41	0.064	0.038	35	0.643	0.441	58	0.064	0.027	100	0.064	0.172
IM vs Pred	24	0.064	0.042	9	0.064	0.029	36	0.643	0.628	19	0.064	0.027
IF vs Pred	39	0.064	0.108	24	0.064	0.041	55	0.064	0.097	67	0.064	0.046
RM vs RF	NA	0.081	0.031	NA	0.081	0.051	NA	0.081	0.038	NA	0.081	0.040
RM vs IM	NA	0.190	0.103	NA	0.081	0.094	NA	0.663	0.308	NA	0.081	0.091
RF vs IF	NA	0.383	0.317	NA	0.081	0.053	NA	1.000	0.770	NA	0.081	0.065
IM vs IF	NA	0.081	0.039	NA	0.081	0.050	NA	0.081	0.102	NA	0.081	0.047
1-4 RM vs Pred	10	0.064	0.037	6	0.064	0.046	14	0.064	0.212	11	0.064	0.037
1-4 RF vs Pred	14	0.064	0.046	13	0.643	0.671	23	0.064	0.033	36	0.643	0.909
1-4 IM vs Pred	7	0.064	0.057	4	0.064	0.033	12	1.000	0.886	8	0.064	0.027
1-4 IF vs Pred	13	0.064	0.135	10	0.064	0.104	25	0.064	0.026	29	0.643	0.184
5-12 RM vs Pred	21	0.064	0.084	14	0.064	0.041	26	0.643	0.407	36	0.064	0.033
5-12 RF vs Pred	27	0.064	0.037	22	0.059	0.092	35	0.064	0.052	65	0.064	0.114
5-12 IM vs Pred	17	0.064	0.035	5	0.064	0.031	25	0.643	0.698	11	0.064	0.028
5-12 IF vs Pred	26	0.064	0.099	14	0.064	0.038	30	0.643	0.442	38	0.064	0.037
5-12 RM vs RF	NA	0.081	0.042	NA	0.077	0.054	NA	0.190	0.091	NA	0.081	0.042
5-12 RM vs IM	NA	0.190	0.125	NA	0.081	0.071	NA	1.000	0.689	NA	0.081	0.061
5-12 RF vs IF	NA	0.663	0.463	NA	0.077	0.049	NA	1.000	0.540	NA	0.081	0.050
5-12 IM vs IF	NA	0.081	0.035	NA	0.081	0.070	NA	0.663	0.508	NA	0.081	0.062

Note:

p values reported from Wilcoxon and permutation tests

Two-tailed tests of difference from prediction or between treatments (as indicated)

Mean for individual session totals reported in Mean column

RM=Reversible Market, RF=Reversible Fixed

IM=Irreversible Market, IF=Irreversible Fixed

1-4 refers to periods 1-4 and 5-12 to periods 5-12

3 observations for each treatment by firm type

Table 15: Total Output, Emissions, Mean Permit Prices with Results of Tests vs Predictions and Between Treatments

	Output			Emissions			Prices		
Test	Mean	Wilcoxon	Perm	Mean	Wilcoxon	Perm	Mean	Wilcoxon	Perm
RM vs Pred	201	0.064	0.031	349	0.064	0.031	42	0.064	0.103
RF vs Pred	304	0.643	0.180	632	0.064	0.056	NA	NA	NA
IM vs Pred	134	0.064	0.030	220	0.064	0.027	39	0.064	0.028
IF vs Pred	253	0.064	0.096	489	0.643	0.151	NA	NA	NA
RM vs RF	NA	0.081	0.033	NA	0.081	0.030	NA	NA	NA
RM vs IM	NA	0.081	0.064	NA	0.081	0.062	NA	0.663	0.586
RF vs IF	NA	0.081	0.073	NA	0.081	0.076	NA	NA	NA
IM vs IF	NA	0.081	0.039	NA	0.081	0.041	NA	NA	NA
1-4 RM vs Pred	63	0.064	0.043	100	0.064	0.038	60	0.064	0.155
1-4 RF vs Pred	106	0.643	0.262	232	0.064	0.134	NA	NA	NA
1-4 IM vs Pred	46	0.064	0.030	78	0.064	0.029	74	0.064	0.042
1-4 IF vs Pred	91	0.643	0.282	216	0.643	0.283	NA	NA	NA
5-12 RM vs Pred	139	0.064	0.027	249	0.064	0.028	33	0.064	0.130
5-12 RF vs Pred	198	0.643	0.205	400	0.643	0.511	NA	NA	NA
5-12 IM vs Pred	88	0.064	0.031	143	0.064	0.027	22	0.643	0.042
5-12 IF vs Pred	161	0.064	0.082	273	0.064	0.051	NA	NA	NA
5-12 RM vs RF	NA	0.081	0.028	NA	0.081	0.037	NA	NA	NA
5-12 RM vs IM	NA	0.081	0.052	NA	0.081	0.039	NA	0.081	0.114
5-12 RF vs IF	NA	0.081	0.069	NA	0.081	0.059	NA	NA	NA
5-12 IF vs IM	NA	0.081	0.046	NA	0.081	0.050	NA	NA	NA
RM 1-4 vs 5-12	NA	NA	NA	NA	NA	NA	NA	0.663	0.204
RF 1-4 vs 5-12	NA	NA	NA	NA	NA	NA	NA	NA	NA
IM 1-4 vs 5-12	NA	NA	NA	NA	NA	NA	NA	0.081	0.041
IF 1-4 vs 5-12	NA	NA	NA	NA	NA	NA	NA	NA	NA

Note:
p values reported from Wilcoxon and permutation tests
Two-tailed tests of difference from prediction or between treatments (as indicated)
3 observations for each treatment for total output and emissions
36 observations by treatment for average prices
Mean values for total output and emissions by session in Mean column
RM=Reversible Market, RF=Reversible Fixed
IM=Irreversible Market, IF=Irreversible Fixed
1-4 refers to periods 1-4 and 5-12 to periods 5-12

Table 16: Effect of Irreversibility and Market on Intensity Choices

Test	All	RMvIM	IFvRF	IFvIM	RFvRM
Irreversibility (Wilcoxon)	0.010	0.121	0.081	NA	NA
Irreversibility (t)	0.004	0.103	0.045	NA	NA
Market (Wilcoxon)	0.575	NA	NA	1.00	0.383
Market (t)	0.507	NA	NA	0.65	0.356

Note:
p values reported of two-tailed Wilcoxon and t tests between groups
All: Pooled results, differentiated by reversibility/market only (as indicated)
1,152 observations in total, 288 for each treatment
RM=Reversible Market, RF=Reversible Fixed
IM=Irreversible Market, IF=Irreversible Fixed

Table 17: Effect of Irreversibility and Market on Emissions

Test	All	RMvIM	IFvRF	IFvIM	RFvRM
Irreversibility (Wilcoxon)	0.045	0.081	0.081	NA	NA
Irreversibility (t)	0.039	0.011	0.036	NA	NA
Market (Wilcoxon)	0.031	NA	NA	0.081	0.081
Market (t)	0.008	NA	NA	0.032	0.015

Note:
p values reported of two-tailed Wilcoxon and t tests between groups
All: Pooled results, differentiated by reversibility/market only (as indicated)
3 observations for each treatment
Emissions calculated as output produced multiplied by intensity choice
RM=Reversible Market, RF=Reversible Fixed
IM=Irreversible Market, IF=Irreversible Fixed

Table 18: Difference in Price Variance between Periods 1-4 and Periods 5-12

Treatment	Mean (1-4)	Mean (5-12)	Wilcoxon	Perm
Reversible	1686.207	275.526	0.104	0.028
Irreversible	1501.226	202.202	0.016	0.037

Note:
p values reported of two-tailed Wilcoxon and permutation tests of difference between mean variance for early (1-4) and later (5-12) periods
36 observations in total for each treatment

C.1 Tests on Individual Results

Table 19: Main Outcomes and Tests of Differences by Gender and Major, Correlation with Risk and Ambiguity

	Output		Emissions		Clean-Intensity		Dirty-Intensity	
Test	t.test	Wilcoxon	t.test	Wilcoxon	t.test	Wilcoxon	t.test	Wilcoxon
Male	0.272	0.342	0.082	0.049	0.024	0.041	0.168	0.238
Major (KW)	0.388	NA	0.076	NA	0.643	NA	0.095	NA
Risk (corr)	0.392	NA	0.678	NA	0.571	NA	0.985	NA
Amb(corr)	0.742	NA	0.388	NA	0.044	NA	0.230	NA

Note:
p values reported for:
Two-tailed Wilcoxon and t tests of differences between means for men and women
Kruskal-Wallis tests used for differences between majors
Kendall tau used for correlations with risk and ambiguity
96 observations in total
94 observations for gender (51 female, 43 male); excludes two NA

Table 20: Comparison of Buying and Selling Prices by Firm Type

	Buying				Selling			
	Mean Price		Clean Higher		Mean Price		Clean Lower	
Test	Clean	Dirty	Wilcoxon	t-test	Clean	Dirty	Wilcoxon	t-test
All	47.13	30.69	0	0.00	34.34	55.07	0.00	0.00
Reversible	52.35	29.66	0	0.00	35.93	56.33	0.01	0.00
Irreversible	39.50	32.87	0	0.12	31.40	53.23	0.01	0.01

Note:
p values reported of one-sided Wilcoxon and t tests:
Clean buyers pay more than dirty buyers
Clean sellers receive less than dirty sellers
366 observations for Reversible treatment
207 observations for Irreversible treatment

Table 21: Do Men Choose Higher Values?

	Intensity								Output			
	Clean				Dirty				Total			
Test	Male Avg	Female Avg	t-test	Wilcoxon	Male Avg	Female Avg	t-test	Wilcoxon	Male Avg	Female Avg	t-test	Wilcoxon
Rev Market	1.8	1.5	0.121	0.155	2.4	2.1	0.079	0.055	1.907	2.211	0.934	0.956
Rev Fixed	1.5	1.4	0.401	0.422	3.0	2.8	0.002	0.007	3.129	3.222	0.729	0.744
Irrev Market	2.5	1.2	0.000	0.000	2.3	1.3	0.000	0.000	1.675	1.038	0.000	0.000
Irrev Fixed	1.6	1.1	0.001	0.000	2.2	2.0	0.255	0.477	2.833	2.394	0.007	0.013

Note:

p values reported for one-sided Wilcoxon and t tests that men choose higher intensity and output

94 observations in total (51 female, 43 male); excludes two NA

Table 22: Tests of Between-Treatment Differences in Time Taken to Change Intensity

	All		Clean		Dirty	
Test	Wilcoxon	Perm	Wilcoxon	Perm	Wilcoxon	Perm
Reversible Market vs Reversible Fixed	0.628	0.651	0.261	0.188	1.000	0.799
Reversible Market vs Irreversible Market	0.036	0.017	0.500	0.235	0.081	0.039
Reversible Fixed vs Irreversible Fixed	0.182	0.198	0.059	0.081	0.653	0.419
Irreversible Market vs Irreversible Fixed	0.009	0.006	0.059	0.071	0.081	0.031

Note:

p values reported for two-tailed Wilcoxon and permutation tests of differences in means between groups

Measure is period when first change in intensity occurs, after initial selection

3 observations for mean of each treatment by firm type

Means exclude those who didn't change intensity at all

Table 23: Tests of Differences by Risk and Ambiguity for Key Variables, by Treatment

	Mean Dist from Target				Mean Intensity				Total Extra Purchases			
	Risk		Ambiguity		Risk		Ambiguity		Risk		Ambiguity	
Test	Clean	Dirty	Clean	Dirty	Clean	Dirty	Clean	Dirty	Clean	Dirty	Clean	Dirty
Reversible Fixed	0.258	1.000	0.245	0.857	0.449	0.449	0.468	0.430	0.561	0.130	0.468	0.101
Reversible Market	0.883	0.628	0.625	0.089	0.502	0.890	0.409	0.091	NA	NA	NA	NA
Irreversible Fixed	0.094	0.739	0.169	0.925	0.637	0.662	0.165	0.739	0.159	0.532	0.361	0.400
Irreversible Market	0.193	0.914	0.255	0.587	0.139	0.655	0.236	0.170	NA	NA	NA	NA

Note:

p values reported from Kruskal-Wallis tests

Risk excluding non-comprehension on Holt-Laury and zero producers (76 obs)

Ambiguity excluding inconsistent responses and zero producers (82 obs)

Risk and ambiguity categorised into 3-level factor: averse, neutral, seeking

Market treatments only considered for extra permit purchases (35 risk and 38 amb observations

D Risk and Uncertainty Attitudes

D.1 Holt-Laury Test

The Holt-Laury test (Holt & Laury, 2002b) is one commonly used way to determine individual risk preferences. Participants are presented with 10 binary lottery choices. Option A choices are "safe" in that the difference between each payoff is smaller than the difference between payoffs in Option B. The odds of winning the larger payoff in each bet increases as the participant moves down the page. The odds of winning in the last bet are 100%, and this question can serve as a test of whether participants understand the test itself. The number of safe choices gives a measure of the participant's risk attitude. Alternatively, it is possible to calculate a switch point. In theory, respondents are expected to be consistent, that is, not switch more than once, but using the number of safe choices allows for inconsistent preferences to be included.

D.2 Ellsberg-Urn Task

Here, two possible methods for conducting this test are presented (Dimmock et al., 2013; König-Kersting & Trautmann, 2016). Based on the Ellsberg Urn Task (Ellsberg, 2000), the test involves eliciting preferences between a risky prospect, with known odds, and another with unknown odds. This is usually undertaken using different coloured balls in an 'urn'. In the first iteration, participants are offered the choice between an urn with a 50/50 split of two different coloured balls, or an urn with the same total number of balls, but no information about the distribution of colours. They win a set prize if a ball of the selected colour is drawn from the urn of their choice.

Dimmock, Kouwenberg, Mitchell and Peijnenburg (2013) allowed for participants to choose either of the urns or to be indifferent. Thus, in this test choosing indifference indicated the participant was ambiguity neutral. Choosing the risky urn suggested they were ambiguity averse, and choosing the urn with unknown odds meant they were ambiguity seeking.

In Dimmock, Kouwenberg, Mitchell and Peijnenburg (2013), this test was followed up with a series of choices, presented one at a time. Participants that chose the risky urn in stage one were offered lower odds in the second round. If they still chose the risky option, they would be offered even lower odds in the third round. This process continued until participants switched or selected indifference between the options. A similar process followed for those that initially selected the unknown odds. In this instance they were offered progressively higher odds on the risky prospect. The end point, either very high or very low odds, or the point at which they selected indifference, was taken as their "matching probability", a measure of their relative ambiguity preference.

The test used in this experiment is adapted from Kőnig-Kersting and Trautmann (2016), where participants are offered a range of choices at once (similar to the setup for the Holt-Laury test). To simplify the experimental setting (where numerous other tasks are also being undertaken), only the 2-colour task is utilised. Kőnig-Kersting and Trautmann (2016) had a low-odds version, also, where there were 10 different coloured balls), where participants choose between a risky prospect and an ambiguous bet involving 'balls' of two different colours. A range of probabilities for the risky option are presented to participants. The order in Kőnig-Kersting and Trautmann (2016) was deliberately not ascending/descending to make "preference consistency requirements much less salient” than in the case of ascending order lists.

The outcome of all the decisions can be used for the determination of a probability equivalent score for the ambiguous bet. As in Kőnig-Kersting and Trautmann (2016), this is calculated as the mid-point between the lowest probability at which the participant prefers the risky prospect, and the highest probability for which they choose the ambiguous bet.

If, for example, the lowest odds at which the participant has chosen the risky prospect is 0.45 and the highest odds at which they have selected the ambiguous prospect is 0.4, this would result in a probability equivalent of 0.425. The higher the value of this measure, the greater the individual’s aversion to uncertainty.

In the case that a participant chose all risky options or all ambiguous options, the upper and lower bounds were determined by calculating the mid-point between 0.35 (the lowest odds of the risky bets) and 0.3, and between 0.65 (the highest odds of the risky bets) and 0.7.

D.3 Testing Procedure

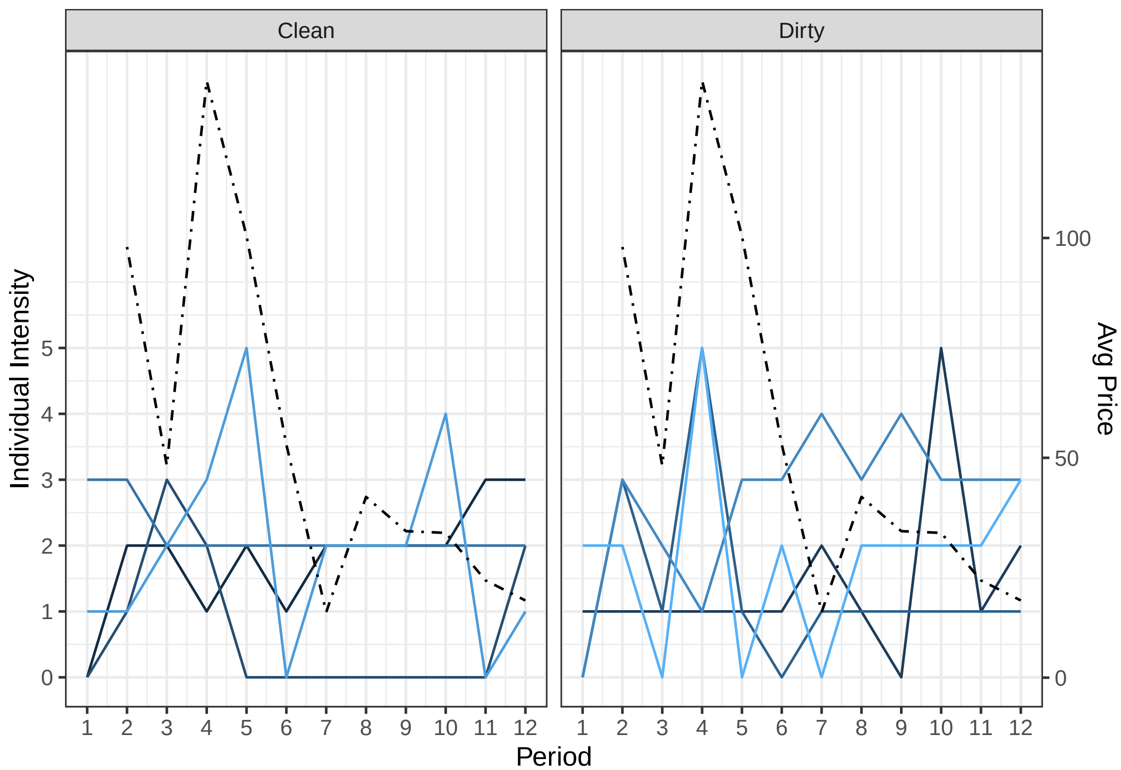
There are a few options in regards to when these tests are run. From the participants’ perspective, running a risk test at the start of the session might frame the experiment as a test of risk attitudes, which could bias results. Conversely, testing everyone after the experiment might mean that the results of the lottery experiment are affected by how participants feel about how they have performed. There may be a similar issue with the ambiguity test. To better control for these possible effects, the sequence could be alternated between pre- and post-assessment. This would expand the number of treatment groups by a multiple of two (for each test), as it would be necessary to run two versions of each, one starting with the lottery experiment and one ending with it.

Alternatively, one of the assessments could be run prior to the experiment and one after. If nothing else, this could break up the session a little.

E Individual Decision Making

E.1 Individual Intensity Choices and Prices

In the market treatments, participants may be expected to respond to permit prices when making intensity choices. Since intensity is selected at the start, charting the prior-period average price against intensity choices for each period might reveal patterns of behaviour if there are any. High prices might be expected to induce lower intensity levels, as participants seek to earn more from permit sales or pay less for permits. Lower prices might be expected to have the opposite effect. There is no particular sign here of a relationship, however.



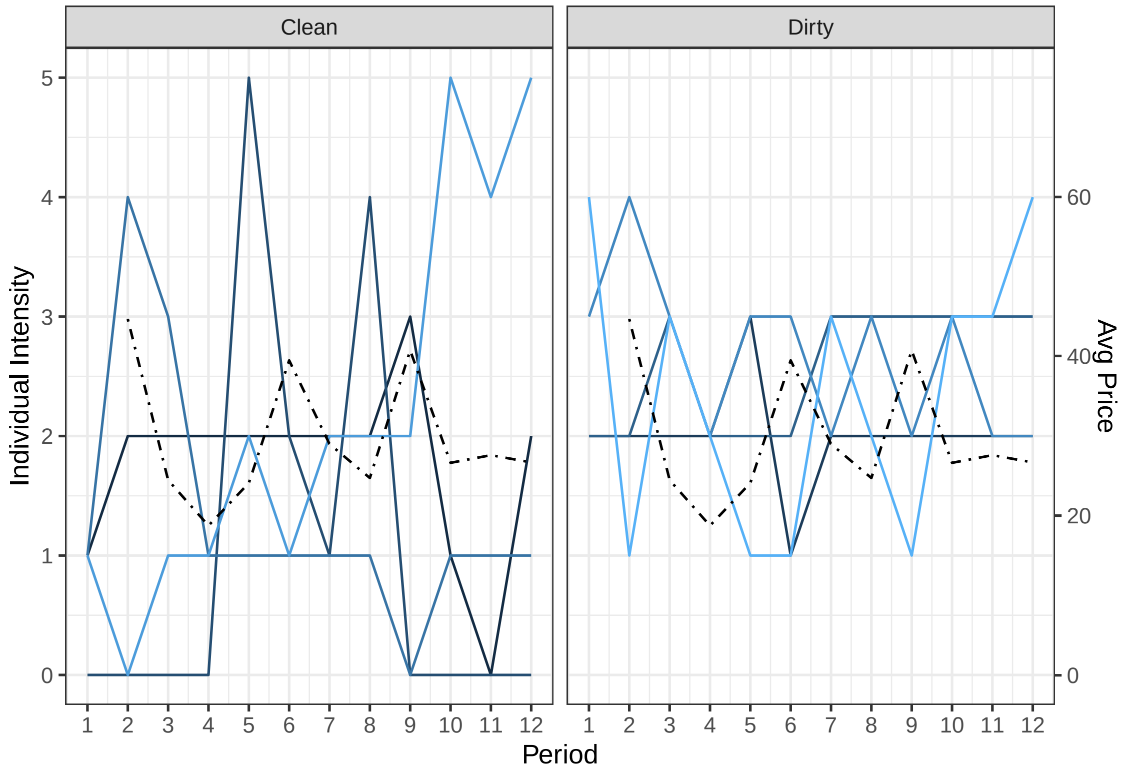


Figure 46: Individual Intensity Choices for Reversible Market over the Period, with Prices in Dashed Line (RHS axis)

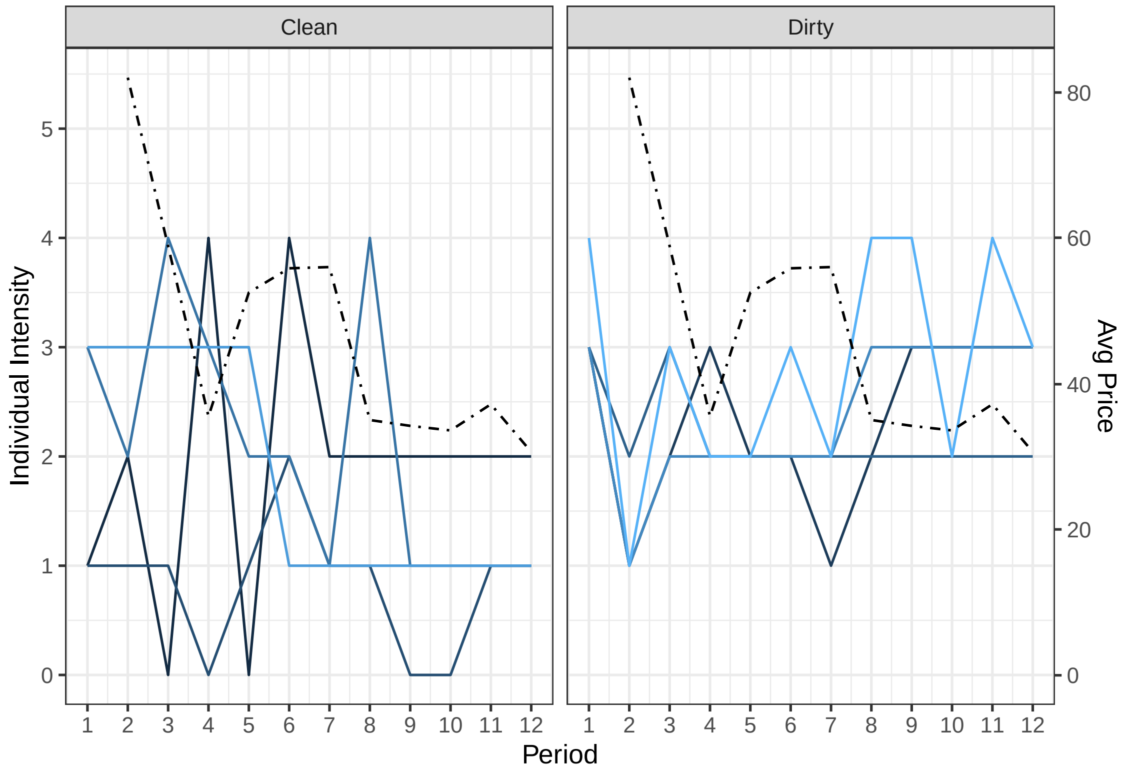


Figure 47: Individual Intensity Choices for Reversible Market over the Period, with Prices in Dashed Line (RHS axis)

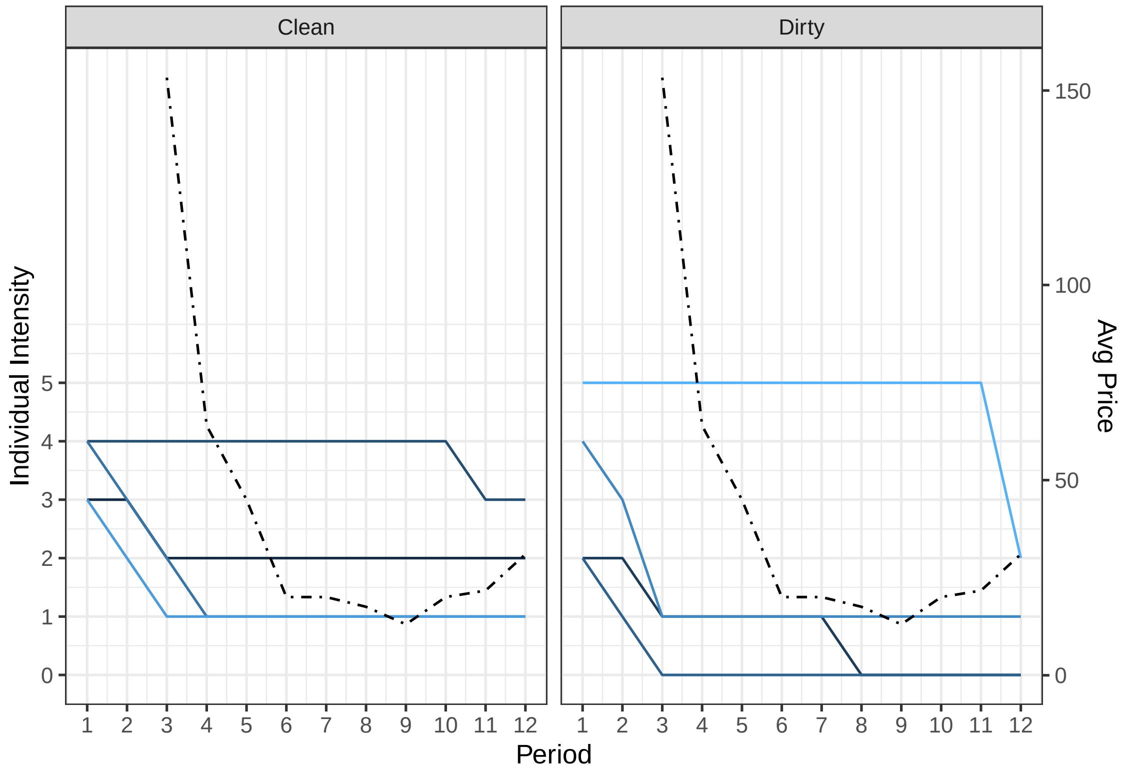


Figure 48: Individual Intensity Choices for Irreversible Market over the Period, with Prices in Dashed Line (RHS axis)

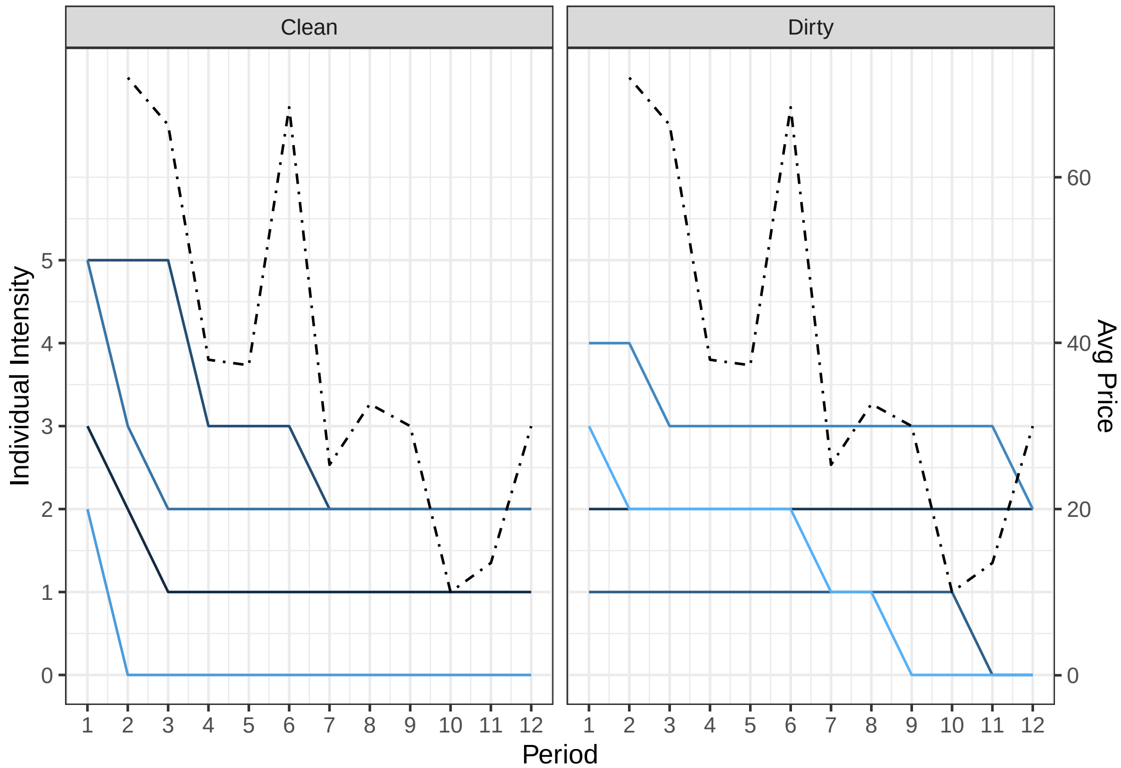


Figure 49: Individual Intensity Choices for Irreversible Market over the Period, with Prices in Dashed Line (RHS axis)

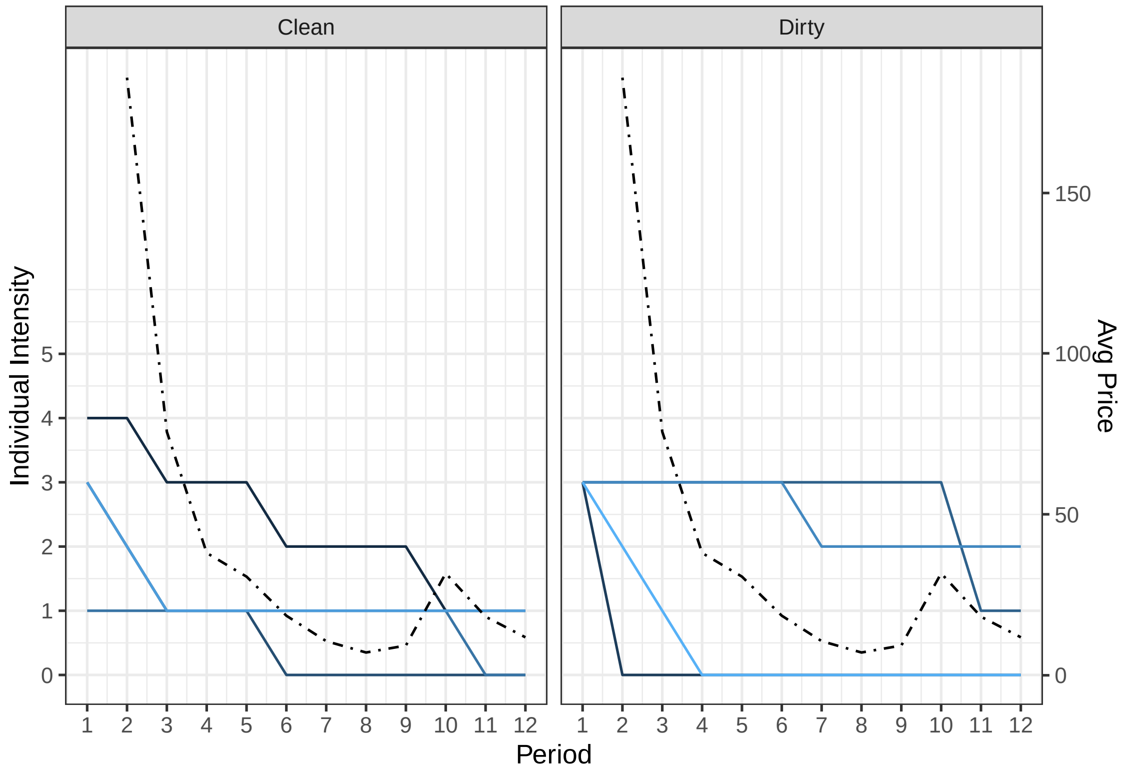


Figure 50: Individual Intensity Choices for Irreversible Market over the Period, with Prices in Dashed Line (RHS axis)

E.2 Intensity Choices & Risk Attitudes

In more general terms, we can also look for patterns in intensity decisions versus the individual's uncertainty and risk attitudes, as measured here.

Figures 51 to 54 depict intensity choices by firm type for each individual (by treatment). The colours relate to the individual's score on the Holt-Laury test, coded by relative tolerance for risk.

If risk attitudes are affecting decisions, then we would expect to see risk-averse individuals moving toward the target of 2, and risk seekers moving away. We would also expect differences between market and fixed treatments, since pricing uncertainty is no longer a factor in the fixed-price treatments. There don't appear to be any particular patterns here, however.

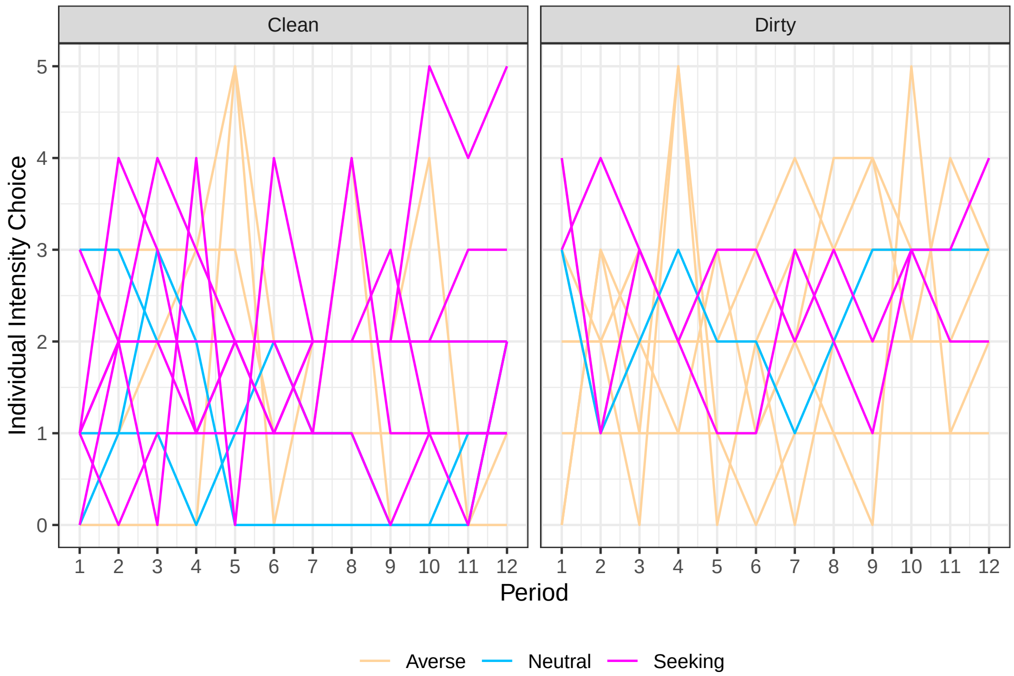


Figure 51: Individual Intensity Choices - Reversible Market. Colours Indicate Risk Tolerance. If Risk Affects Intensity Decision, Might Expect Risk-Averse Individuals to Tend to Choose the Target (2)

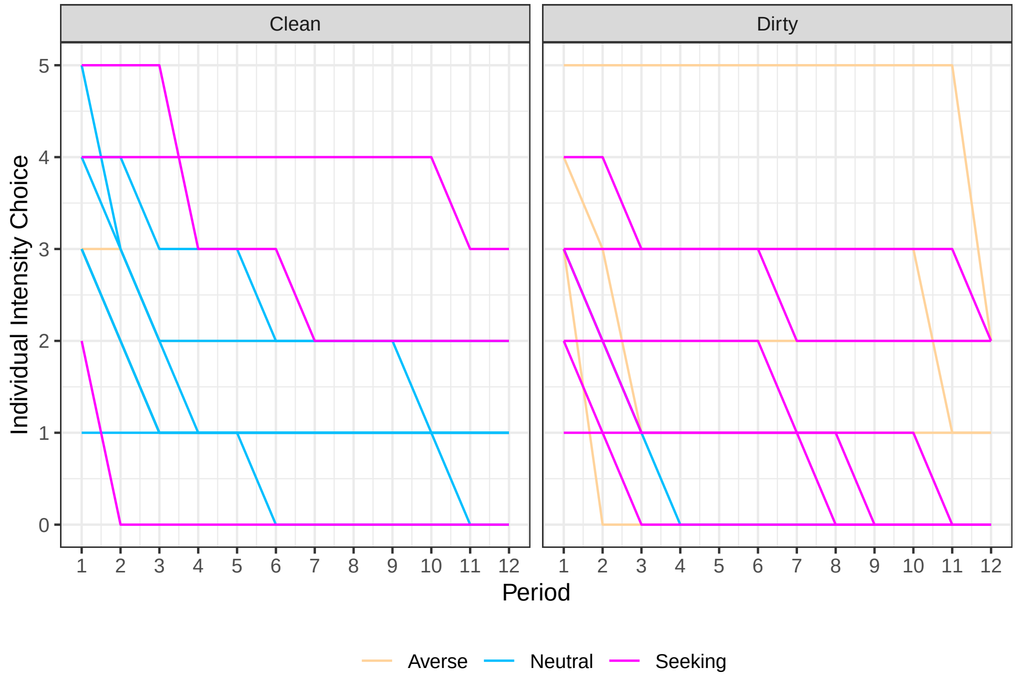


Figure 52: Individual Intensity Choices - Ireversible Market. Colours Indicate Risk Tolerance. If Risk Affects Intensity Decision, Might Expect Risk-Averse Individuals to Tend to Choose the Target (2)

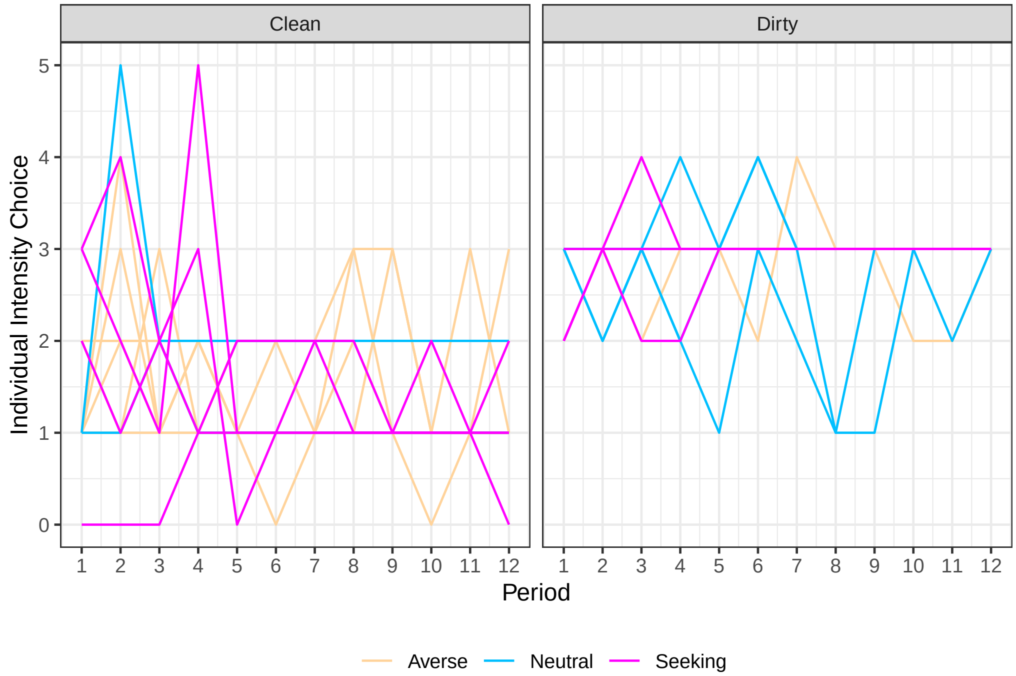


Figure 53: Individual Intensity Choices - Reversible Fixed. Colours Indicate Risk Tolerance. If Risk Affects Intensity Decision, Might Expect Risk-Averse Individuals to Tend to Choose the Target (2)

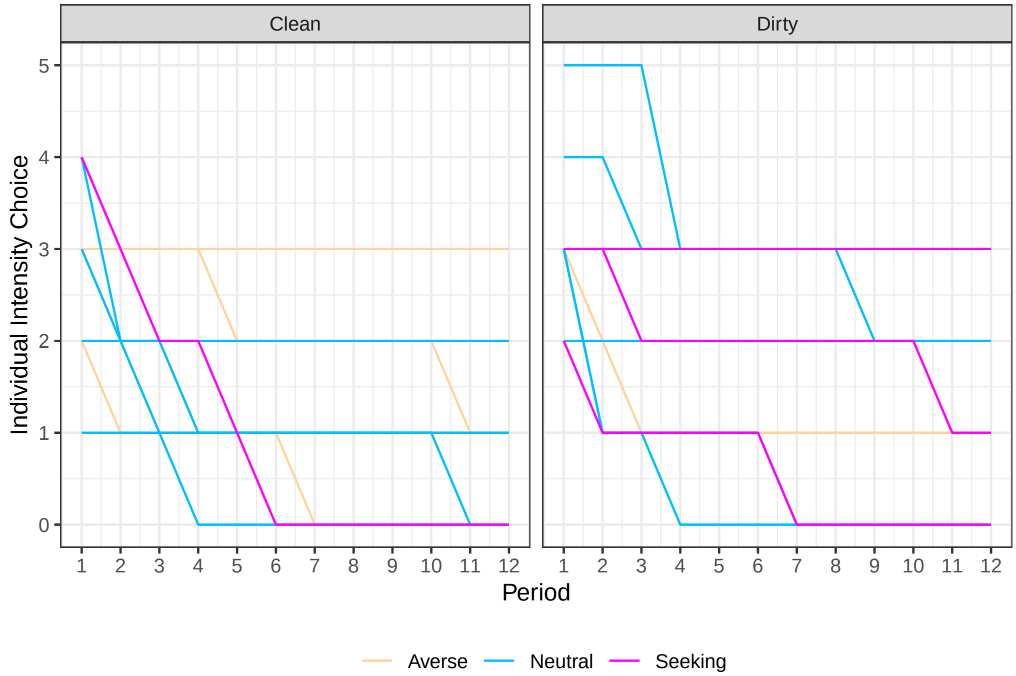


Figure 54: Individual Intensity Choices - Irreversible Fixed. Colours Indicate Risk Tolerance. If Risk Affects Intensity Decision, Might Expect Risk-Averse Individuals to Tend to Choose the Target (2)

E.3 Intensity Choices & Ambiguity Attitudes

Figures 55 to 58 illustrate intensity choices by firm type for each individual, where the colours relate to their score on the ambiguity test.

If participants' attitudes relate to their decisions, then we would expect to see risk-averse individuals tending to move toward the target of 2, and risk seekers tending to move away from it. We also expect differences between reversible and irreversible treatments, and differences between market and fixed treatments, since pricing uncertainty is no longer a factor in the fixed-price treatments. There is no particular evidence of any pattern here.

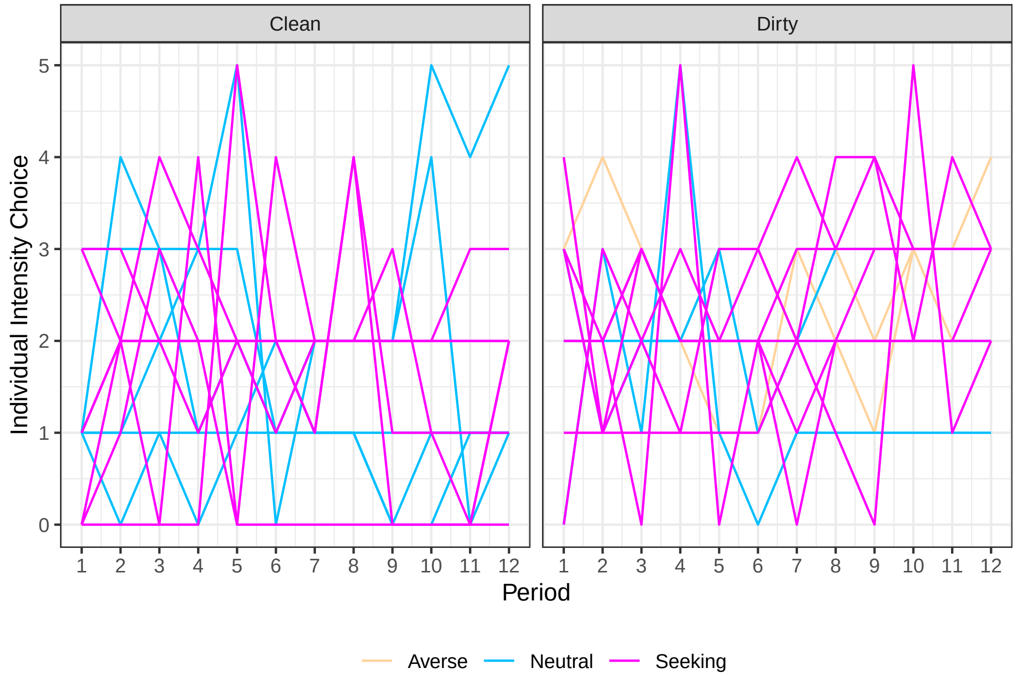


Figure 55: Individual Intensity Choices - Reversible Market. Colours Indicate Ambiguity Tolerance. If Ambiguity Affects Intensity Decision, Might Expect Ambiguity-Averse Individuals to Tend to Choose the Target (2)

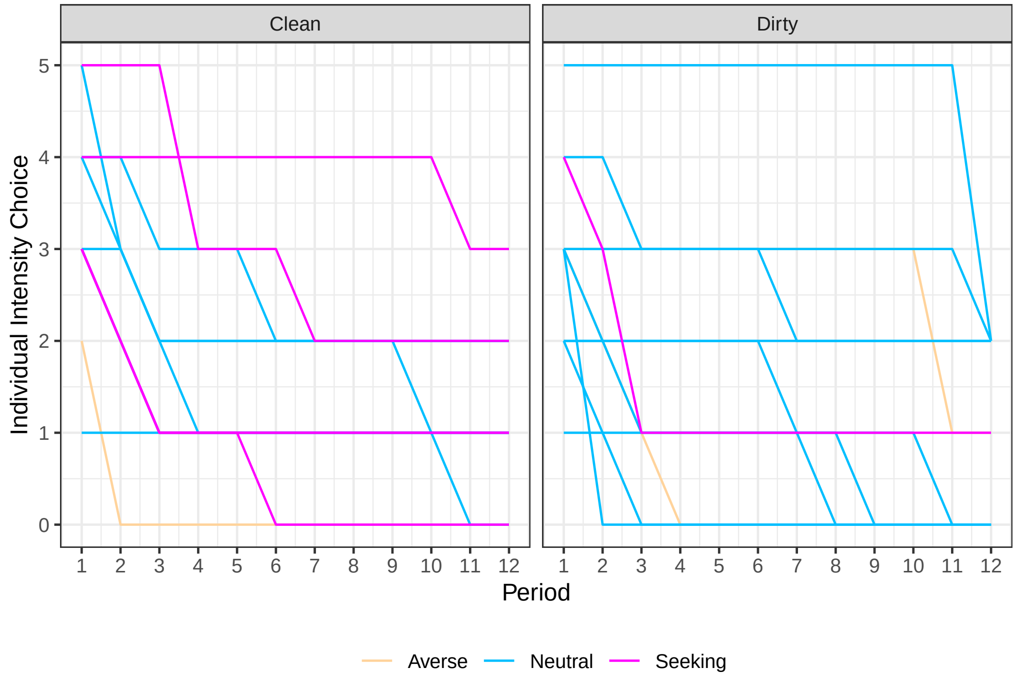


Figure 56: Individual Intensity Choices - Irreversible Market. Colours Indicate Ambiguity Tolerance. If Ambiguity Affects Intensity Decision, Might Expect Ambiguity-Averse Individuals to Tend to Choose the Target (2)

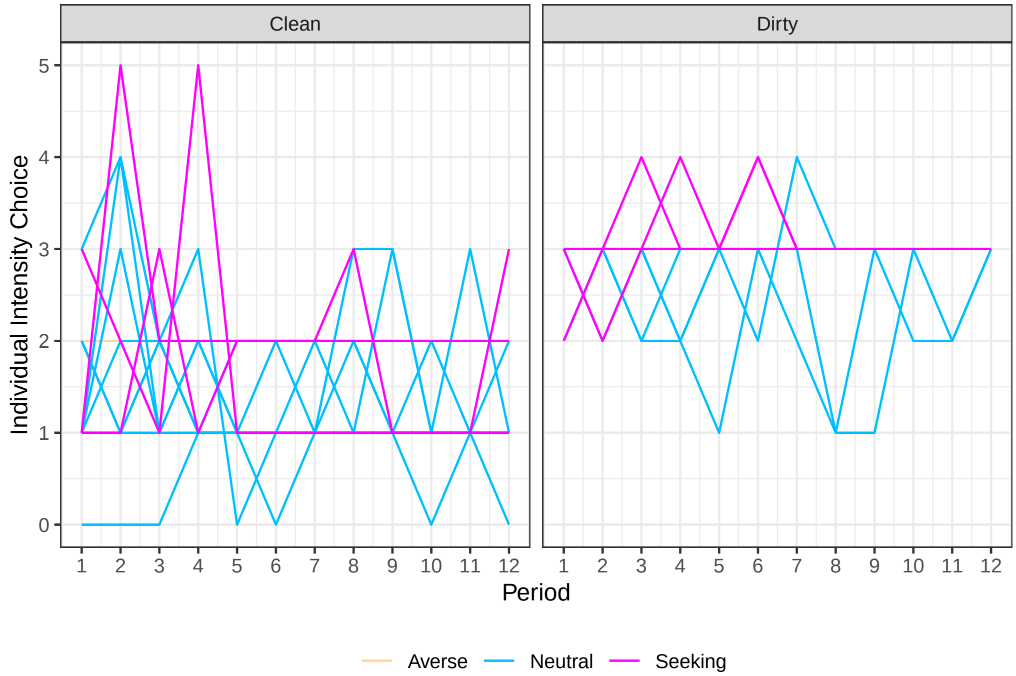


Figure 57: Individual Intensity Choices - Reversible Fixed. Colours Indicate Ambiguity Tolerance. If Ambiguity Affects Intensity Decision, Might Expect Ambiguity-Averse Individuals to Tend to Choose the Target (2)

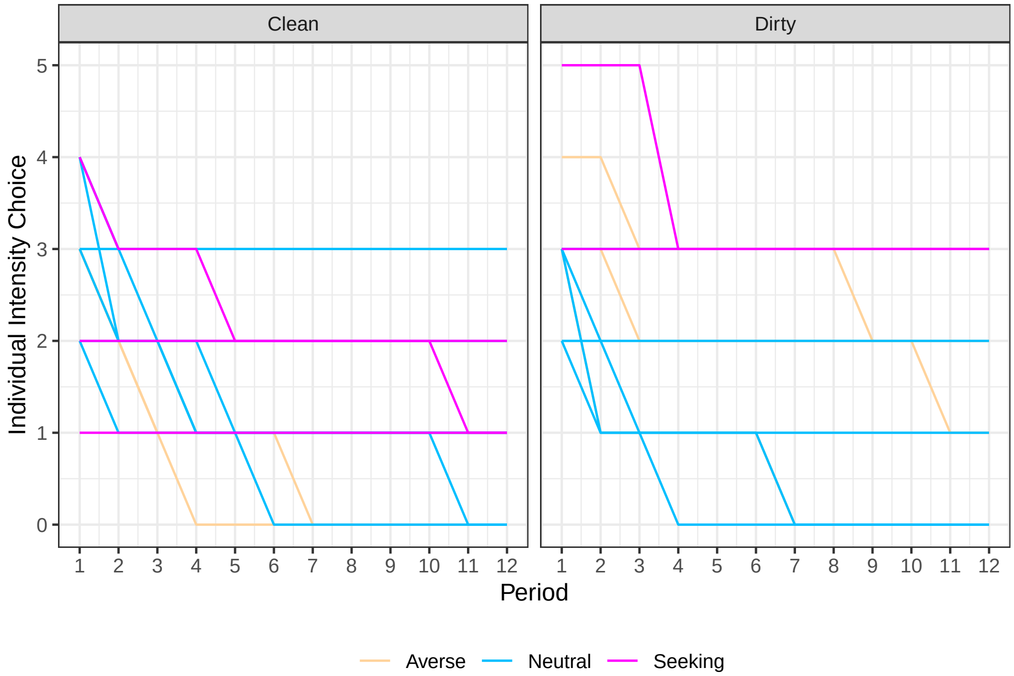


Figure 58: Individual Intensity Choices - Irreversible Fixed. Colours Indicate Ambiguity Tolerance. If Ambiguity Affects Intensity Decision, Might Expect Ambiguity-Averse Individuals to Tend to Choose the Target (2)

F Exclusions

Data was adjusted in two ways. For general, individual-level results, the participants who did not produce output were removed in some cases. The behaviour by these participants suggests that they either did not understand the experiment at all or were not engaging in good faith (this seems much less likely). Quite a number of other participants made errors or questionable decisions, but otherwise appeared to be engaging with the experiment as intended. As such, these particular participants stood out. In most instances, their exclusion has little impact. At the treatment level, there is no significant effect on intensity choices (an alternative presented below) and prices (where they cannot be excluded). Individually, they also appear in the permit trading data and risk analysis.

F.1 Intensity results with exclusions

Results for intensity choices in treatments compared to predictions are unchanged by the exclusion of those who did not produce output. Tests of between-treatment effects also detect similar results.

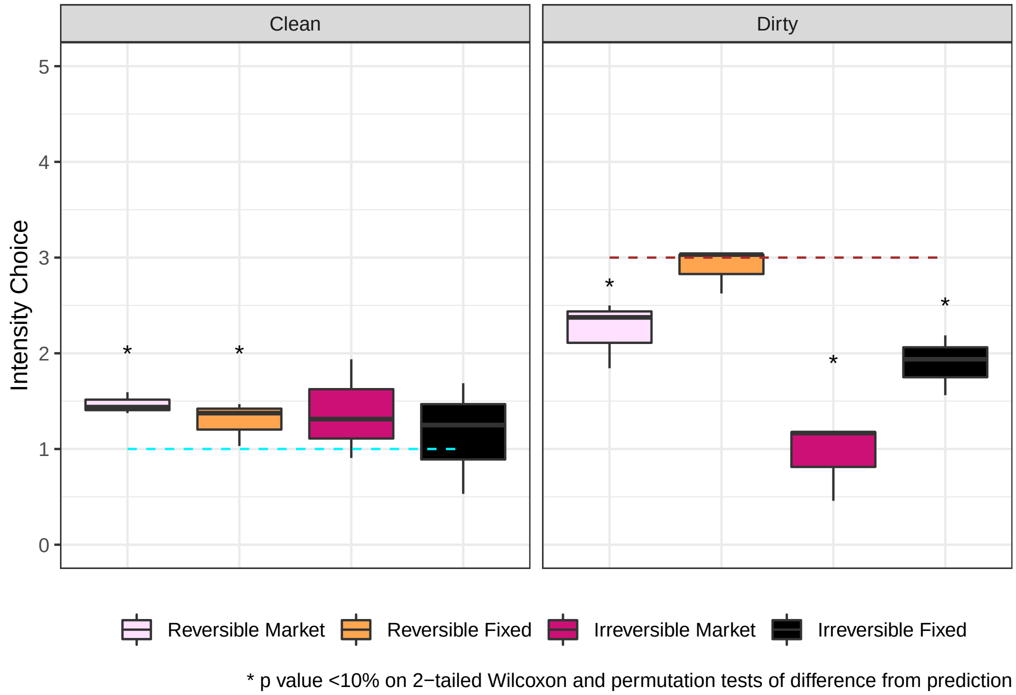


Figure 59: Average Intensity Choices (periods 5-12). Results Run Again Excluding those who did not Produce Output, but no Difference Detected. Dashed Lines Represent Predictions by Firm Type

F.2 Exclusions from Risk & Ambiguity

The second group of exclusions relates to Section 7.1. Test results are in Appendix F.2.6.

F.2.1 Risk Results

It is useful to test the sensitivity of results to exclusions. In the first instance, removing inconsistent results doesn't appear to make any particular difference. However, there does appear to be a difference between those who chose the safe option in the last question and everyone else. This provides some further support for the exclusion of these individual results when considering risk attitudes.

Exclusion of the participants who did not produce output doesn't appear to make any difference, and no difference along gender lines was detected, either for the full sample or with exclusions.

F.2.2 Ambiguity Results

For ambiguity test results, inconsistent results can also be removed, and testing suggests ambiguity tolerance is lower for the consistent group compared to the inconsistent one. There does not appear to be any particular difference in results with the exclusion of those who did not produce output.

Gender does appear to make a difference here, with Wilcoxon test results suggesting that women's ambiguity tolerance was greater than men's (significant at 5% for both the full sample and with inconsistent results excluded).

F.2.3 Mean Distance from Target

Correlation tests suggest there is a relationship between risk attitudes and the mean absolute distance from the target in the irreversible fixed-price treatment, when those who did not comprehend the test are excluded (see Appendix F.2.6). There does not appear to be any relationship with ambiguity results.

There is a significant difference in the measure for mean distance when those who do not produce output are excluded.

F.2.4 Extra Purchases

There appears to be a positive relationship between risk tolerance and total unnecessary permit purchases in both market treatments (see Appendix F.2.6). However, there appears to be no such correlation with ambiguity attitudes.

Nor does there appear to be any difference along gender lines.

F.2.5 Use of the Test Buttons

There does not appear to be any relationship between use of the intensity test button and either risk or ambiguity attitudes. There does, however, appear to be a positive correlation between use of the market test button and both risk and ambiguity attitudes (Appendix F.2.6).

F.2.6 Tests of Exclusions

Table 24: Is there a Correlation Between Mean Distance from the Target and Risk and Ambiguity?

Test	tau	p value
Risk & Distance	1.287	0.198
Risk (excl. Q10)	1.631	0.103
Ambiguity & Distance	-0.896	0.371
Amb (ex inconsistent)	-0.965	0.335

Note:

p values reported from Kendall tau test of correlation

Table 25: Correlation Between Mean Distance from the Target and Risk and Ambiguity by Treatment

	Rev Mkt		Rev Fixed		Irrev Mkt		Irrev Fixed	
Test	tau	p value	tau	p value	tau	p value	tau	p value
Risk & Distance	0.329	0.742	0.365	0.715	1.024	0.306	1.495	0.135
Risk (excl. Q10)	0.723	0.469	0.219	0.826	1.057	0.291	1.951	0.051
Ambiguity & Distance	-0.545	0.585	0.948	0.343	-0.261	0.794	-0.582	0.560
Amb (ex inconsistent)	-0.446	0.656	0.866	0.387	-0.548	0.584	-0.582	0.560

Note:

p values reported from Kendall tau test of correlation

Exclusions: For comprehension on risk test, and for consistency on ambiguity test

96 obs. total

79 excluding for comprehension on risk test

85 excluding for consistency on ambiguity test

Table 26: Is there a Correlation Between Extra Purchases and Risk and Ambiguity?

test	tau	p-value
Risk & Extra	0.072	0.348
Risk (excl. Q10)	0.029	0.733
Ambiguity & Extra	0.072	0.367
Amb (ex inconsistent)	0.046	0.595

Note:

p values reported from Kendall tau test of correlation

Exclusions for comprehension in risk test

Exclusions for consistency in ambiguity test

Market treatments only considered

48 observations total; with exclusions: 38 for ambiguity, 35 for risk

Table 27: Correlation Between Extra Purchases and Risk and Ambiguity by Market Treatment

	Rev Market		Irrev Market	
Test	tau	p value	tau	p value
Risk & Extra	0.402	0.010	-0.215	0.181
Risk (excl. Q10)	0.452	0.018	-0.315	0.069
Ambiguity & Extra	0.046	0.775	-0.064	0.705
Amb (ex inconsistent)	0.018	0.918	-0.115	0.532

Note:

p values reported from Kendall tau test of correlation

Exclusions for comprehension in risk test

Exclusions for consistency in ambiguity test

Market treatments only considered

48 observations total; with exclusions: 38 for ambiguity, 35 for risk

Table 28: Is there a Correlation Between Use of Market Test Button and Risk and Ambiguity?

test	tau	p-value
Risk & Test Button	0.134	0.109
Risk (excl. Q10)	0.177	0.056
Ambiguity & Test Button	0.167	0.054
Amb (ex inconsistent)	0.174	0.061

Note:

p values reported from Kendall tau test of correlation

Exclusions for comprehension in risk test

Exclusions for consistency in ambiguity test

Market treatments only considered

48 observations total; with exclusions: 38 for ambiguity, 35 for risk

Table 29: Is there a Correlation Between Use of Intensity Test Button and Risk and Ambiguity?

test	tau	p-value
Risk & Test Button	0.070	0.350
Risk (excl. Q10)	0.014	0.869
Ambiguity & Test Button	-0.061	0.438
Amb (ex inconsistent)	-0.033	0.690

Note:

p values reported from Kendall tau test of correlation

Exclusions for comprehension in risk test

Exclusions for consistency in ambiguity test

96 observations total; with exclusions: 76 for risk and 85 for ambiguity

Table 30: Tests on Individual Variables

	Mean Distance		Extra		Mkt Test Button		Int Test Button		Risk		Ambiguity	
Test	Wilcoxon	Perm	Wilcoxon	Perm	Wilcoxon	Perm	Wilcoxon	Perm	Wilcoxon	Perm	Wilcoxon	Perm
Comprehension	0.908	0.790	0.754	0.587	0.880	0.502	0.329	0.616	0.024	0.006	0.551	0.968
Consistent(A)	0.579	0.537	0.201	0.733	0.572	0.891	0.730	0.708	0.247	0.090	0.010	0.101
Consistent(H)	0.605	0.852	0.082	0.791	0.936	0.452	0.369	0.607	0.358	0.765	0.173	0.184
Production	0.054	0.008	0.619	0.846	0.684	0.557	0.354	0.401	0.576	0.694	0.524	0.790
Male	0.452	0.417	0.243	0.352	0.975	0.832	0.254	0.268	0.941	0.831	0.094	0.097
Major (KW)	0.415	NA	0.789	NA	0.519	NA	0.605	NA	0.740	NA	0.325	NA

Note:

p values reported of two-tailed Wilcoxon and permutation tests in difference of means, Kruskal-Wallis test used to test by major

Mean Distance = Individual mean absolute distance from target of intensity choices

Extra = Individual total unnecessary permit purchases

Mkt/Int Test = Total individual market and intensity test button clicks

Comprehension - Compares those who comprehended the Holt-Laury and those who didn't (76 vs 20 obs)

Consistent - Compares consistent and inconsistent results for A-ambiguity (85 vs 11 obs), H-Holt-Laury (61 vs 35 obs)

Production - Compares those who produced output and those who didn't (3 obs for zero producers)

Male - Compares men and women (94 observations total)

G Full Regression Output

G.1 Emissions

Table 31: Total Emissions per Session per Period

	Final
Intercept	28.959*** (0.961)
Period	0.475 (0.277)
High Demand	51.249** (10.348)
Early Periods	43.874** (7.584)
Market	−5.470 (2.693)
Irreversible	−6.962 (7.160)
Period:High	−2.029 (1.353)
Period:Early	−9.975** (1.242)
High:Early	−56.915** (12.557)
Period:Market	−0.582 (0.365)
High:Market	−53.004** (15.182)
Early:Market	−27.197** (8.463)
Period:Irrev	−0.131 (0.898)
High:Irrev	−6.026 (16.268)
Early:Irrev	−4.538 (9.929)
Market:Irrev	1.231 (16.122)
Period:High:Early	13.863** (1.575)
Period:High:Mkt	4.236* (1.771)
Period:Early:Mkt	3.582 (1.776)
High:Early:Mkt	33.004 (23.049)
Period:High:Irrev	−0.880 (1.929)
Period:Early:Irrev	1.831 (2.305)
High:Early:Irrev	53.359** (13.301)
Period:Mkt:Irrev	−0.581 (1.674)
High:Mkt:Irrev	25.136 (21.547)
Early:Mkt:Irrev	−24.231 (16.019)
Period:High:Early:Mkt	−2.069 (5.631)
Period:High:Early:Irrev	−17.887** (5.558)
Period:High:Mkt:Irrev	−1.683 (2.414)
Period:Early:Mkt:Irrev	8.381** (3.125)
High:Early:Mkt:Irrev	−18.469 (24.053)
Period:High:Early:Mkt:Irrev	0.116 (8.161)
Observations	144
R^2	0.905
Adjusted R^2	0.878
Residual Std. Error	6.764 (df = 112)
F Statistic	34.255*** (df = 31; 112)

Note: *p<0.1; **p<0.05; ***p<0.01
Robust SE clustered on session with Satterthwaite correction

G.2 Individual Intensity Choices

Table 32: Intensity Choices per Period

	(1)	(2)
Intercept	1.400*** (0.156)	1.388*** (0.194)
Dirty	1.437*** (0.134)	1.500*** (0.144)
Irreversible	0.203 (0.287)	0.591 (0.385)
Market	−0.099 (0.430)	−0.208 (0.486)
Period	−0.003 (0.011)	−0.006 (0.013)
Early Periods	0.131 (0.221)	0.090 (0.278)
Risk Neutral		−0.333* (0.118)
Risk Seeking		−0.021 (0.273)
Dirty:Irrev	−0.792* (0.405)	−1.208 (0.935)
Dirty:Mkt	−0.847*** (0.247)	−0.250 (0.254)
Irrev:Mkt	0.691 (0.628)	0.287 (0.699)
Period:Early	0.024 (0.058)	0.042 (0.073)
Irrev:Period	−0.044** (0.018)	−0.039* (0.020)
Irrev:Early	1.172*** (0.360)	1.079** (0.401)
Mkt:Period	0.041 (0.049)	−0.014 (0.052)
Mkt:Early	−0.123 (0.514)	−0.380 (0.605)
Dirty:Risk N		0.292 (0.234)
Dirty:Risk S		0.021 (0.297)
Irrev:Risk N		−0.313 (0.446)
Irrev:Risk S		−0.813* (0.403)
Mkt:Risk N		0.556 (0.489)
Mkt:Risk S		0.804** (0.300)
Dirty:Irrev:Mkt	0.174 (0.674)	0.042 (1.237)
Irrev:Period:Early	−0.352*** (0.089)	−0.334*** (0.100)
Mkt:Period:Early	0.068 (0.117)	0.008 (0.123)
Irrev:Mkt:Period	−0.085 (0.056)	−0.035 (0.060)
Irrev:Mkt:Early	0.138 (0.705)	0.436 (0.798)
Dirty:Irrev:Risk N		0.729 (1.086)
Dirty:Irrev:Risk S		0.549 (1.197)
Dirty:Mkt:Risk N		−2.354 (1.367)
Dirty:Mkt:Risk S		−0.387 (0.384)
Irrev:Mkt:Risk N		0.285 (0.806)
Irrev:Mkt:Risk S		0.807 (1.226)
Irrev:Mkt:Period:Early	−0.132 (0.164)	−0.081 (0.175)
Dirty:Irrev:Mkt:Risk S		−1.482 (1.866)
Observations	1,152	912
R^2	0.236	0.368
Adjusted R^2	0.223	0.344
Residual Std. Error	1.054 (df = 1132)	0.931 (df = 877)
F Statistic	18.409*** (df = 19; 1132)	15.034*** (df = 34; 877)

Note: *p<0.1; **p<0.05; ***p<0.01
(1) Base case, (2) Include risk factors with exclusions
Exclusions for comprehension on Holt-Laury and zero producers
Cluster-robust SE with Satterthwaite corrections.

Table 33: Intensity Choices per Period (Cont...)

	(3)	(4)	(5)	Final
Intercept	1.532*** (0.229)	1.347*** (0.271)	1.507*** (0.242)	1.174*** (0.275)
Dirty	1.431*** (0.135)	1.569*** (0.137)	1.438*** (0.139)	1.383*** (0.187)
Irreversible	0.172 (0.290)	0.180 (0.297)	0.163 (0.296)	0.064 (0.297)
Market	−0.177 (0.441)	−0.022 (0.462)	−0.192 (0.425)	−0.223 (0.507)
Period	−0.009 (0.014)	−0.014 (0.014)	−0.008 (0.014)	−0.010 (0.015)
Early Periods	0.157 (0.227)	−0.784 (0.874)	0.506 (0.487)	0.192 (0.238)
Intensity Test	−0.018 (0.018)	−0.011 (0.023)	−0.015 (0.020)	−0.026 (0.018)
Profit Change		0.001* (0.001)		
Lagged sd Prices			0.002 (0.003)	
Male				0.639*** (0.180)
Humanities				0.617* (0.275)
Science				0.383 (0.264)
Engineering				−0.046 (0.294)
Social Sciences				1.381*** (0.319)
Health				0.420* (0.230)
Other				−0.567 (0.477)
Dirty:Irrev	−0.787* (0.410)	−0.790* (0.433)	−0.759* (0.437)	−0.714* (0.379)
Dirty:Mkt	−0.853*** (0.248)	−0.952*** (0.261)	−0.893*** (0.250)	−0.859** (0.349)
Irrev:Mkt	0.700 (0.625)	0.637 (0.641)	0.439 (0.643)	0.922 (0.646)
Period:Early	0.016 (0.060)	0.267 (0.258)	−0.089 (0.145)	−0.007 (0.059)
Irrev:Period	−0.044** (0.018)	−0.040** (0.017)	−0.044** (0.018)	−0.046** (0.019)
Irrev:Early	1.206*** (0.362)	1.314 (0.943)	0.486 (0.561)	1.200*** (0.372)
Mkt:Period	0.043 (0.049)	0.042 (0.050)	0.044 (0.049)	0.043 (0.050)
Mkt:Early	−0.145 (0.515)	0.536 (1.522)	−0.624 (0.744)	−0.178 (0.517)
Dirty:Irrev:Mkt	0.184 (0.675)	0.287 (0.709)	0.256 (0.687)	0.166 (0.594)
Irrev:Period:Early	−0.357*** (0.089)	−0.368 (0.270)	−0.142 (0.159)	−0.342*** (0.090)
Mkt:Period:Early	0.077 (0.116)	−0.102 (0.437)	0.211 (0.205)	0.099 (0.115)
Irrev:Mkt:Period	−0.084 (0.056)	−0.085 (0.056)	−0.062 (0.060)	−0.079 (0.056)
Irrev:Mkt:Early	0.162 (0.704)	−0.962 (1.663)	0.389 (0.908)	0.299 (0.707)
Irrev:Mkt:Period:Early	−0.142 (0.163)	0.157 (0.466)	−0.171 (0.248)	−0.190 (0.161)
Observations	1,152	960	1,008	1,128
R^2	0.238	0.220	0.217	0.334
Adjusted R^2	0.224	0.203	0.201	0.318
Residual Std. Error	1.054 (df = 1131)	1.048 (df = 938)	1.039 (df = 986)	0.990 (df = 1100)
F Statistic	17.651*** (df = 20; 1131)	12.631*** (df = 21; 938)	13.041*** (df = 21; 986)	20.437*** (df = 27; 1100)

Note: *p<0.1; **p<0.05; ***p<0.01
(3) Add change in prior period profit
(4) Add intensity test button
(5) Add lagged sd of prices
Final: (3) plus gender and majors
Cluster-robust SE with Satterthwaite corrections.

G.3 Effect of Extra Permit Purchases on Profits

Table 34: Per Period Effect of Extra Permit Purchases on Profits

	(1)	(2)	(3)
Intercept	40.892 (74.460)	85.048 (92.803)	153.253 (100.402)
Irreversible	158.656* (77.460)	147.919* (80.468)	177.775* (93.809)
Dirty	166.941** (79.648)	146.688 (87.512)	22.119 (101.192)
Period	50.713*** (12.369)	53.514*** (13.115)	47.215*** (12.990)
Abs Difference from Predicted Intensity	−45.924* (24.621)	−55.951* (29.871)	−53.320* (30.323)
Output	97.683*** (21.667)	85.172*** (22.339)	91.345*** (21.199)
Extra Permits Purchased			−50.714** (20.722)
Dirty:Irrev	−158.009 (109.371)	−162.793 (117.274)	−147.683 (143.419)
Irrev:Period	−6.578 (16.947)	−9.673 (17.460)	−5.175 (16.306)
Dirty:Period	−36.156** (15.593)	−38.434** (16.237)	−29.207 (17.612)
Output:Abs Difference	−54.113*** (15.872)	−51.381*** (16.429)	−52.302*** (16.112)
Irrev:Extra			−136.637* (61.948)
Dirty:Extra			98.002 (56.019)
Irrev:Dirty:Period	0.657 (22.539)	8.397 (23.938)	1.043 (24.585)
Irrev:Dirty:Extra			92.397 (95.397)
Observations	576	540	540
R^2	0.391	0.386	0.444
Adjusted R^2	0.381	0.374	0.430
Residual Std. Error	308.175 (df = 565)	313.703 (df = 529)	299.434 (df = 525)
F Statistic	36.329*** (df = 10; 565)	33.187*** (df = 10; 529)	29.991*** (df = 14; 525)

Note: *p<0.1; **p<0.05; ***p<0.01
(1) Base, (2) With exclusions for production
(3) Add extra permit purchases
Cluster-robust SE with Satterthwaite corrections.

Table 35: Effect of Extra Permit Purchases on Profits

	(4)	(5)	(Final)
Intercept	137.353 (101.251)	76.079 (117.238)	99.978 (119.743)
Irreversible	135.139 (105.355)	180.768 (126.158)	165.558 (128.270)
Dirty	2.201 (107.994)	98.684 (111.439)	100.451 (113.034)
Period	47.370*** (12.600)	48.613*** (13.350)	49.153*** (13.498)
Abs Difference from Predicted Intensity	−61.316* (31.423)	−72.220** (31.311)	−67.351** (30.325)
Output	90.673*** (21.465)	87.792*** (20.916)	89.221*** (20.438)
Extra Permits Purchased	−46.718* (20.604)	−35.843 (21.169)	−71.081 (39.205)
Market Test Button	24.589** (10.290)	22.657* (11.512)	23.909* (11.758)
Humanities		−118.238 (70.206)	−139.233* (70.769)
Science		−69.475 (92.257)	−135.647 (103.423)
Engineering		265.199 (151.969)	221.051 (151.933)
Health		51.493 (110.770)	48.466 (117.883)
Other		185.411 (147.320)	199.036 (162.820)
Dirty:Irrev	−86.589 (156.119)	−284.088* (159.285)	−282.937* (158.067)
Irrev:Period	−0.847 (15.827)	−3.260 (16.424)	−4.685 (16.911)
Dirty:Period	−27.632 (17.597)	−30.164* (16.831)	−30.520* (16.484)
Output:Abs Difference	−51.530*** (15.828)	−42.643** (16.708)	−46.466*** (16.382)
Irrev:Extra	−131.245* (61.607)	−119.560 (66.992)	−120.007* (54.869)
Dirty:Extra	100.981 (58.445)	64.773 (36.305)	39.593 (38.852)
Extra: Humanities			50.584 (33.355)
Extra:Science			91.677* (46.394)
Extra:Engineering			103.660* (44.530)
Extra:Health			7.874 (38.005)
Extra:Other			−58.295 (85.134)
Irrev:Dirty:Period	−3.368 (24.705)	3.108 (23.917)	3.119 (24.306)
Irrev:Dirty:Extra	88.103 (97.430)	124.874 (85.940)	192.485** (85.513)
Observations	540	540	540
R^2	0.461	0.513	0.526
Adjusted R^2	0.446	0.494	0.503
Residual Std. Error	295.199 (df = 524)	281.893 (df = 519)	279.548 (df = 514)
F Statistic	29.879*** (df = 15; 524)	27.356*** (df = 20; 519)	22.803*** (df = 25; 514)

Note: *p<0.1; **p<0.05; ***p<0.01
(5) Add majors, Final: Add interactions with majors
Cluster-robust SE with Satterthwaite corrections

G.4 Extra Permit Purchases

Table 36: Per Period Effect of Risk Attitudes on Extra Permit Purchases

	(1)	(2)
Intercept	1.455*** (0.471)	0.658* (0.351)
Period	−0.081** (0.039)	−0.053 (0.042)
High Demand	−0.438 (0.490)	−0.055 (0.551)
Irreversible	−0.303 (0.650)	0.183 (0.521)
Early	2.343** (1.003)	2.208 (1.225)
Dirty	−0.389* (0.203)	0.048 (0.146)
Risk Neutral		0.452 (0.255)
Risk Seeking		0.588 (0.364)
Period:High	0.041 (0.049)	0.0003 (0.057)
Period:Irrev	−0.015 (0.055)	−0.026 (0.062)
High:Irrev	−0.073 (0.689)	−0.173 (0.774)
Period:Early	−0.857*** (0.271)	−0.769* (0.342)
High:Early	−2.353* (1.187)	−1.876 (1.517)
Early:Irrev	−3.371*** (1.090)	−3.135** (1.319)
Dirty:Irrev	0.306 (0.307)	0.202 (0.351)
Irrev:Risk N		0.045 (0.564)
Irrev:Risk S		−0.769 (0.442)
Dirty:Risk N		−0.594 (0.555)
Dirty:Risk S		0.281 (0.466)
Period:High:Irrev	0.050 (0.078)	0.055 (0.089)
Period:High:Early	1.251*** (0.416)	1.029 (0.574)
Period:Irrev:Early	1.128*** (0.288)	1.004** (0.357)
High:Irrev:Early	2.906** (1.321)	2.257 (1.648)
Irrev:Dirty:Risk S		−0.214 (0.633)
Period:High:Irrev:Early		−1.086* (0.595)
Period:High:Irrev:Early	−1.316*** (0.438)	
Observations	576	420
R^2	0.190	0.220
Adjusted R^2	0.165	0.173
Residual Std. Error	0.964 (df = 558)	0.955 (df = 395)
F Statistic	7.690*** (df = 17; 558)	4.655*** (df = 24; 395)

Note: *p<0.1; **p<0.05; ***p<0.01
(1) Base
(2) Base plus risk, exclusions for comprehension and zero production
Cluster-robust SE with Satterthwaite corrections.

Table 37: Per Period Effect of Risk on Extra Permit Purchases (cont)

	(Final)
Intercept	0.620 (0.459)
Period	−0.088** (0.041)
High Demand	−0.545 (0.487)
Irreversible	0.216 (0.597)
Early	2.175* (1.129)
Risk Neutral	1.069** (0.347)
Risk Seeking	0.555 (0.588)
Dirty	0.369 (0.324)
Market Test Button	−0.051 (0.028)
Science	0.285 (0.295)
Engineering	−0.589 (0.490)
Health	0.091 (0.085)
Other	0.630* (0.098)
Period:High	0.057 (0.051)
Period:Irrev	0.015 (0.060)
High:Irrev	0.422 (0.779)
Period:Early	−0.818** (0.300)
High:Early	−1.840 (1.414)
Irrev:Early	−3.081** (1.225)
Irrev:Risk N	−1.020* (0.426)
Irrev:Risk S	−0.580 (0.617)
Dirty:Risk N	0.045 (0.254)
Dirty:Risk S	0.994 (0.321)
Dirty:Irrev	−0.353 (0.409)
Science:Risk N	1.432*** (0.319)
Science:Risk S	0.027 (0.584)
Health:Risk N	−0.102 (0.154)
Health:Risk S	0.964 (0.559)
Other:Risk N	−1.239*** (0.137)
Other:Risk S	−0.918** (0.139)
Period:High:Irrev	−0.013 (0.092)
Period:High:Early	1.143** (0.499)
Period:Irrev:Early	1.083*** (0.324)
High:Irrev:Early	2.367 (1.583)
Irrev:Dirty:Risk S	−0.652 (0.596)
Period:High:Irrev:Early	−1.289** (0.542)
Observations	420
R^2	0.362
Adjusted R^2	0.304
Residual Std. Error	0.876 (df = 384)
F Statistic	6.225*** (df = 35; 384)
Note:	*p<0.1; **p<0.05; ***p<0.01
	Final: Market test button and major
	Cluster-robust SE with Satterthwaite corrections.

Table 38: Extra Permit Purchases and Risk (No Exclusions)

	(No Exclusions)
Intercept	1.354* (0.760)
Period	−0.094** (0.045)
High Demand	−0.599 (0.586)
Irreversible	−0.390 (0.830)
Early	2.304** (0.965)
Risk Neutral	0.627 (0.536)
Risk Seeking	0.817 (0.779)
Dirty	−0.176 (0.474)
Market Test Button	−0.036 (0.032)
Humanities	−1.459** (0.510)
Science	−0.193 (0.308)
Engineering	−0.150 (0.160)
Health	0.195 (0.398)
Other	0.602* (0.081)
Period:High	0.058 (0.059)
Period:Irrev	0.0004 (0.059)
High:Irrev	0.122 (0.779)
Period:Early	−0.865*** (0.249)
High:Early	−2.322* (1.153)
Irrev:Early	−3.312*** (1.045)
Irrev:Risk N	−0.246 (0.605)
Irrev:Risk S	−0.863 (0.812)
Dirty:Risk N	−2.026** (0.552)
Dirty:Risk S	0.767 (0.686)
Dirty:Irrev	0.209 (0.538)
Science:Risk N	1.579*** (0.473)
Science:Risk S	−0.420 (0.667)
Engineering:Risk S	−0.636 (0.506)
Health:Risk N	−0.627 (0.520)
Health:Risk S	−0.223 (0.710)
Other:Risk N	−1.403** (0.240)
Other:Risk S	−0.839** (0.119)
Period:High:Irrev	0.027 (0.088)
Period:High:Early	1.278*** (0.378)
Period:Irrev:Early	1.148*** (0.274)
High:Irrev:Early	2.959** (1.298)
Irrev:Dirty:Risk N	2.145** (0.680)
Irrev:Dirty:Risk S	−0.394 (0.845)
Period:High:Irrev:Early	−1.389*** (0.420)
Observations	576
R^2	0.316
Adjusted R^2	0.268
Residual Std. Error	0.903 (df = 537)
F Statistic	6.539*** (df = 38; 537)
Note:	*p<0.1; **p<0.05; ***p<0.01
	With all data for comparison with final model
	Cluster-robust SE with Satterthwaite corrections.

H Instructions

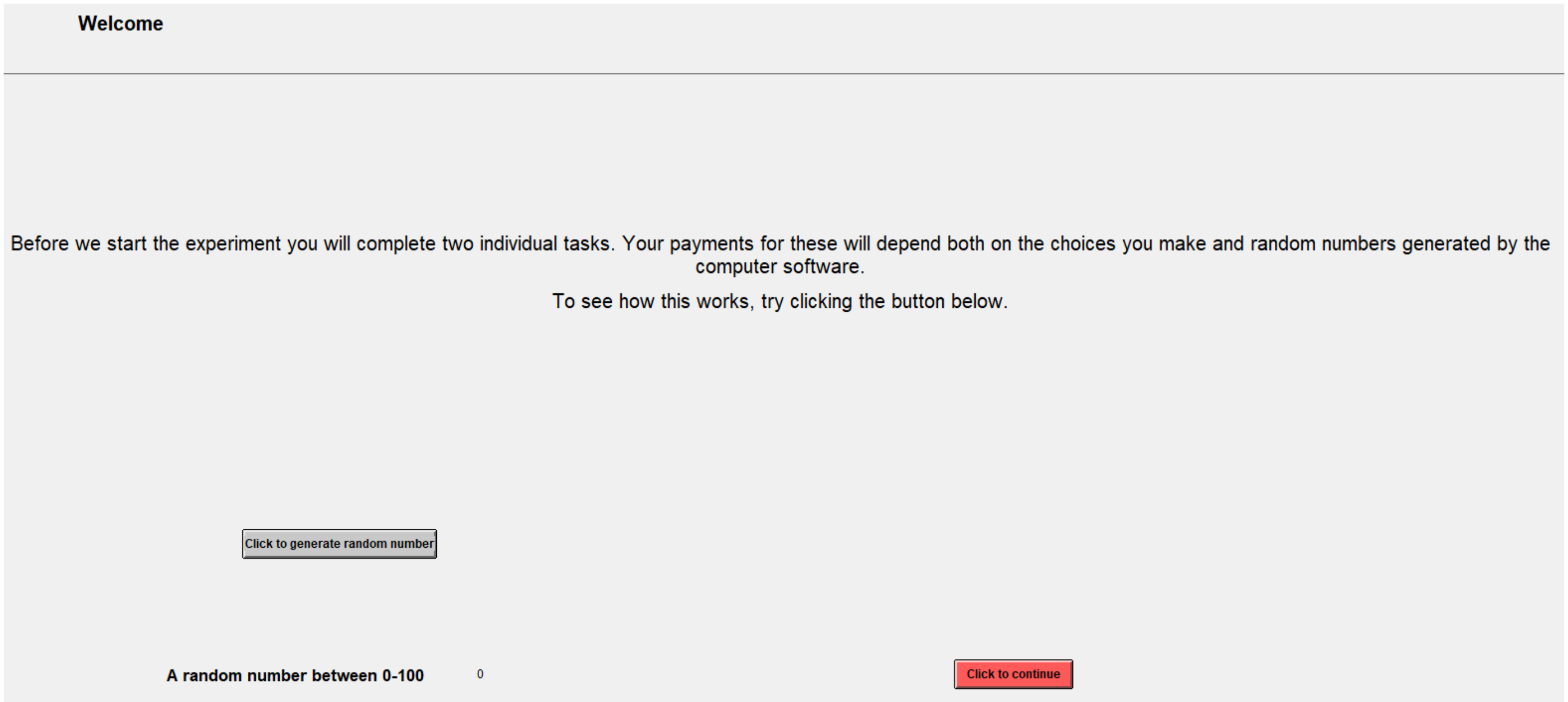
Presented below are screenshots and instructions from the reversible market treatment, as an example of those presented to participants.

Welcome

Introduction to Today's Session

Welcome to the Curtin Experimental Economics Lab. In today's experiment you will have the opportunity to earn money that will be paid to you in cash at the end of today's session.

Ready!


Welcome
Before we start the experiment you will complete two individual tasks. Your payments for these will depend both on the choices you make and random numbers generated by the computer software.
To see how this works, try clicking the button below.
Click to generate random number
A random number between 0-100
0
Click to continue

Individual Task

A				B		
Probability 1, payoff 1	:	Probability 2, payoff 2	Choice	Probability 1, payoff 1	:	Probability 2, payoff 2
1/10, $2.00	:	9/10, $1.60	A B	1/10, $3.85	:	9/10, $0.10
2/10, $2.00	:	8/10, $1.60	A B	2/10, $3.85	:	8/10, $0.10
3/10, $2.00	:	7/10, $1.60	A B	3/10, $3.85	:	7/10, $0.10
4/10, $2.00	:	6/10, $1.60	A B	4/10, $3.85	:	6/10, $0.10
5/10, $2.00	:	5/10, $1.60	A B	5/10, $3.85	:	5/10, $0.10
6/10, $2.00	:	4/10, $1.60	A B	6/10, $3.85	:	4/10, $0.10
7/10, $2.00	:	3/10, $1.60	A B	7/10, $3.85	:	3/10, $0.10
8/10, $2.00	:	2/10, $1.60	A B	8/10, $3.85	:	2/10, $0.10
9/10, $2.00	:	1/10, $1.60	A B	9/10, $3.85	:	1/10, $0.10
10/10, $2.00	:	0/10, $1.60	A B	10/10, $3.85	:	0/10, $0.10

Individual Task

In this individual task we will ask you to make 10 decisions. Each decision is between option A and option B.

Even though you will make 10 decisions, only one will affect your earnings. You will not know in advance which one will be used as this will be decided by a random process. After you make your 10 choices between option A and option B, the computer will pick two random numbers between 1 and 10. There is an equal likelihood that any number between 1 and 10 will be selected, so it will be like the computer is rolling 2 dice that each have 10 sides. The first random number will be used to select one of the 10 decisions, which will be used to determine your payment for this task. The second random number will be used to determine what your payoff will be for the option you chose (either A or B) for the particular decision selected.

In the first decision on the screen, option A pays $2.00 if the random number rolled is 1, and it pays $1.60 if the number is between 2 and 10. Option B pays $3.85 if the random number is 1, and it pays $0.10 if the random number is between 2 and 10. The other decisions are similar, except that as you move down the screen, the chances of the higher payoff for each option increases. In fact, for decision 10 in the bottom row, the second random number will not be needed since each option pays the highest payoff for sure, so your choice here is between earning $2.00 or $3.85.

To summarise, you will make 10 choices; for each decision row you will choose between option A and option B. You may choose

option A for some decision rows and option B for other rows, and you may make them in any order. When you are finished and click the “Done” button at the bottom of the screen, the computer will choose a random number between 1 and 10 to pick which of the 10 decisions to use to calculate your payment. Then the computer will choose a second random number between 1 and 10 to determine your payoff. You will not find out the results of these random numbers until the end of today’s session after the experiment has ended.
Now you may begin by clicking either A or B to make our selections.
Please do not talk with anyone while you are doing this; raise your hand if you have any questions.

Individual Task 2

Option A	Choice	Option B
Choice 1: **50%** chance of winning $2, 50% chance of winning nothing	A ◯ ◯ B	OR **??%** chance of winning $2, ?? chance of winning nothing
Choice 2: **35%** chance of winning $2, 65% chance of winning nothing	A ◯ ◯ B	OR **??%** chance of winning $2, ?? chance of winning nothing
Choice 3: **65%** chance of winning $2, 35% chance of winning nothing	A ◯ ◯ B	OR **??%** chance of winning $2, ?? chance of winning nothing
Choice 4: **40%** chance of winning $2, 60% chance of winning nothing	A ◯ ◯ B	OR **??%** chance of winning $2, ?? chance of winning nothing
Choice 5: **60%** chance of winning $2, 40% chance of winning nothing	A ◯ ◯ B	OR **??%** chance of winning $2, ?? chance of winning nothing
Choice 6: **45%** chance of winning $2, 55% chance of winning nothing	A ◯ ◯ B	OR **??%** chance of winning $2, ?? chance of winning nothing
Choice 7: **55%** chance of winning $2, 45% chance of winning nothing	A ◯ ◯ B	OR **??%** chance of winning $2, ?? chance of winning nothing

Individual Task 2

In this task you are asked to choose between 2 options. Each option represents a bet that would give you a chance of winning $2.00 or winning nothing. Notice that option A tells you the chance of winning, while option B does not.

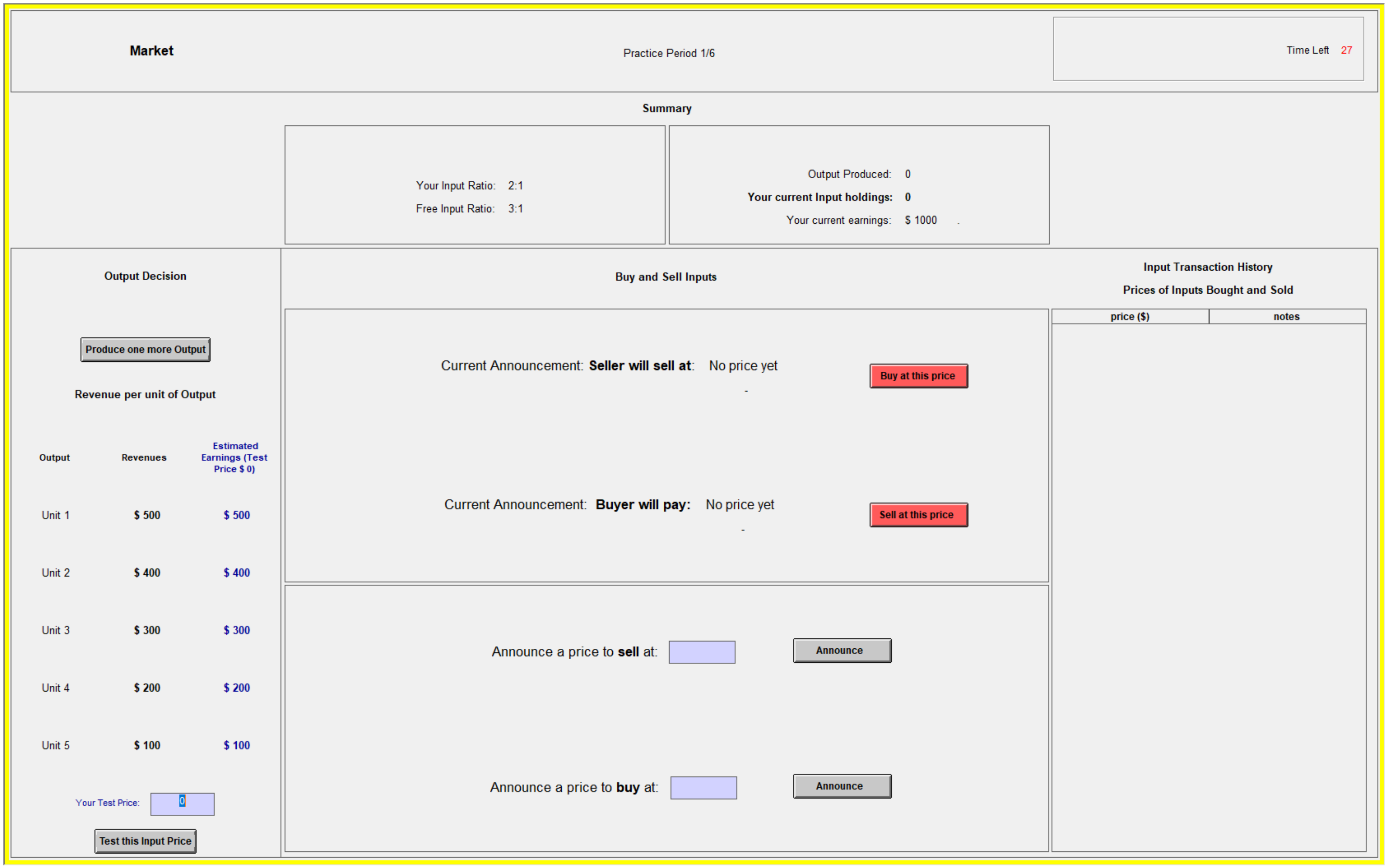
Think of it this way. Imagine you have two bags each containing 100 balls, some red and some black. You win if you draw a red ball out of the bag, but lose if you draw a black ball. In one bag, we know how many black and red balls there are. For instance, you might know that there are 50 red and 50 black balls. In the other bag you don't know how many red and black balls there are. Option A represents the first bag, where you know how many red and black balls there are, and option B represents the second bag. Which option would you prefer if you had a choice between these two bags?

Your earnings from this task will be determined in a similar way to the first task. First, the computer will randomly select a number between 1 and 7 to select one of the choices. This will determine which choice your payoff is calculated on. Second, it will generate more random numbers to calculate your payoff. For instance, if the first number selected is 1, then your earnings will be based on the first choice. If you chose option A for choice 1, your chance of winning is 50%. The computer will generate a random number between 0-100 and if this number is 50 or less, then you win, and if this number is 51 or higher, then you lose. If you choose option B, the process is very similar. The computer will first generate a random number between 0-100 to signify the chance of winning. For instance, if the computer generates 60, then the chance of winning is 60%. Then, if the second random number generated is 60 or less, then you win. If it is 61 or higher, then you lose.

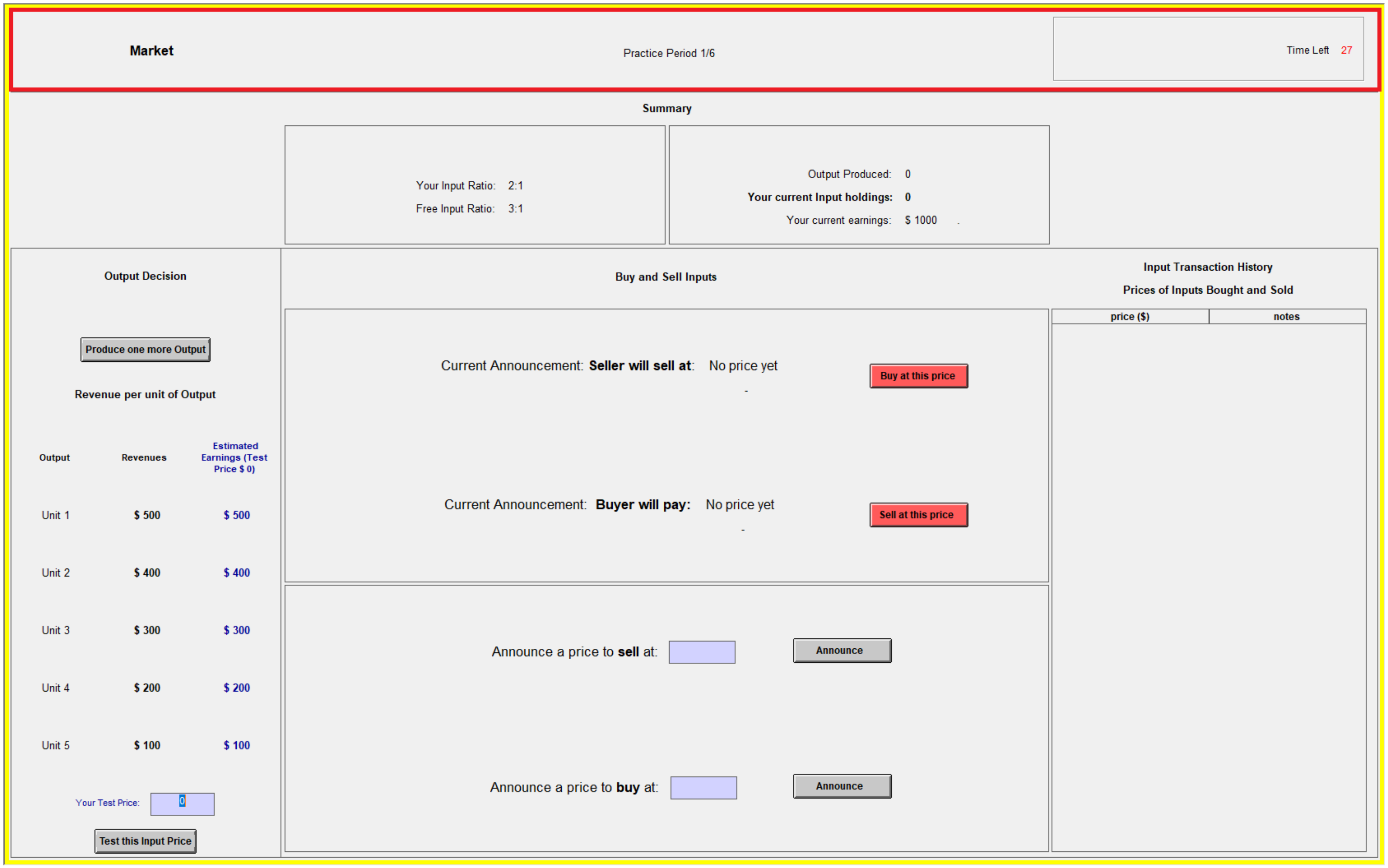
When you are finished, click on the Done button to generate your random numbers and calculate your earnings and continue. You will be informed of your earnings for this task at the end of the experiment.

Once everyone has finished the task we will start the experiment.

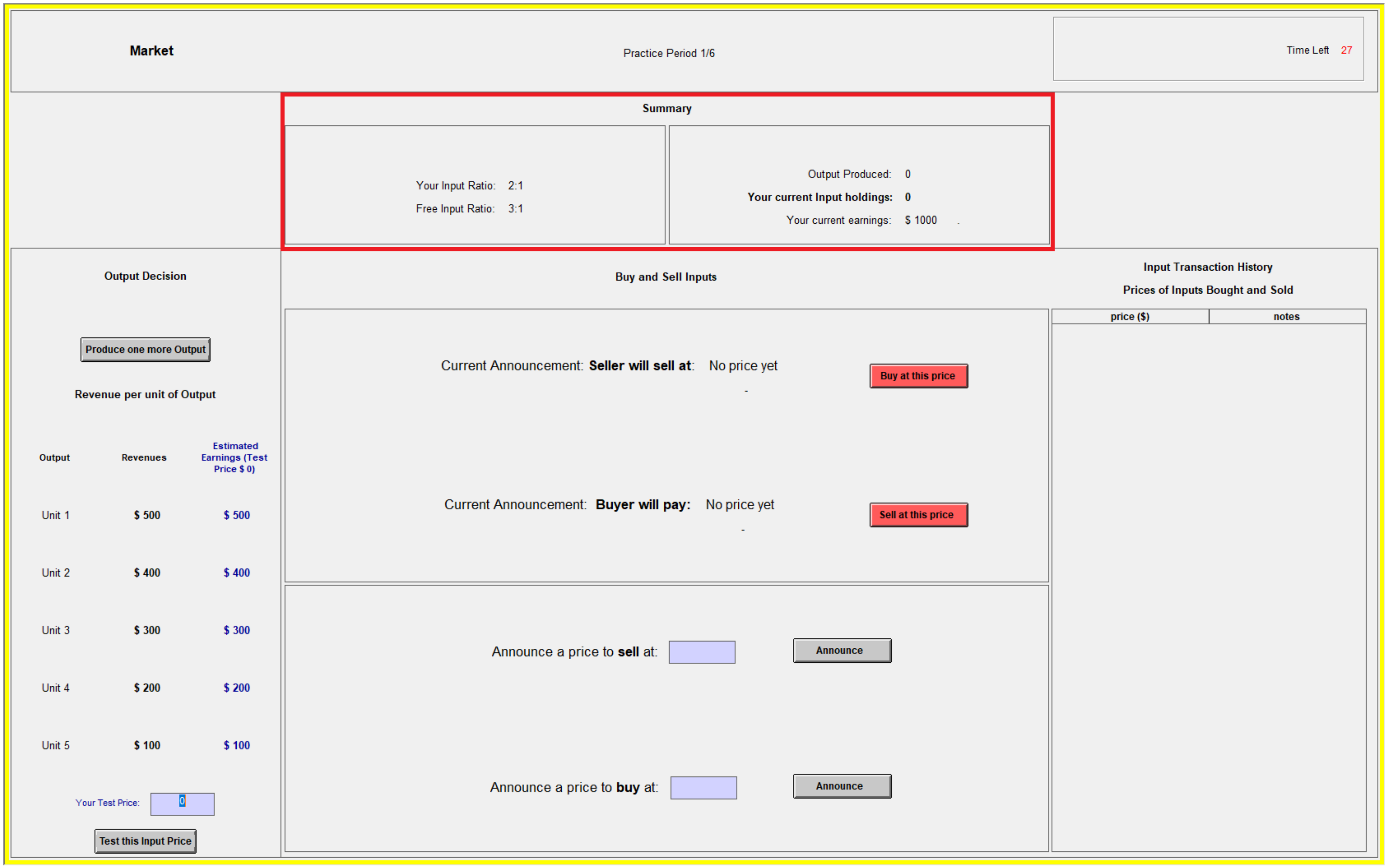
A slideshow with recorded instructions is used to introduce the market screen, followed by 3 practice rounds using the market screen.



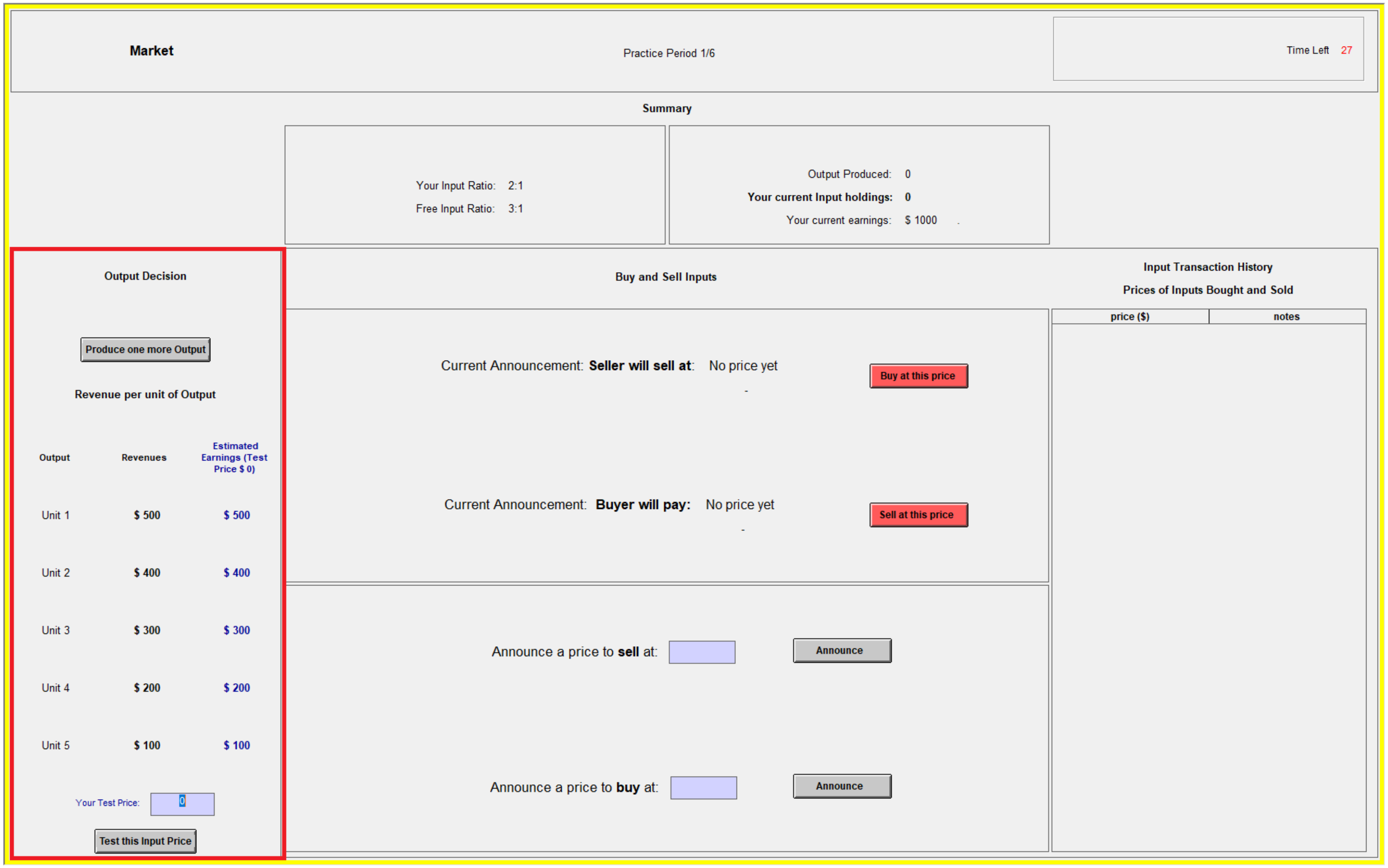
This is an example of the market screen, where you will produce output and trade inputs. We begin by quickly introducing each element of the screen.



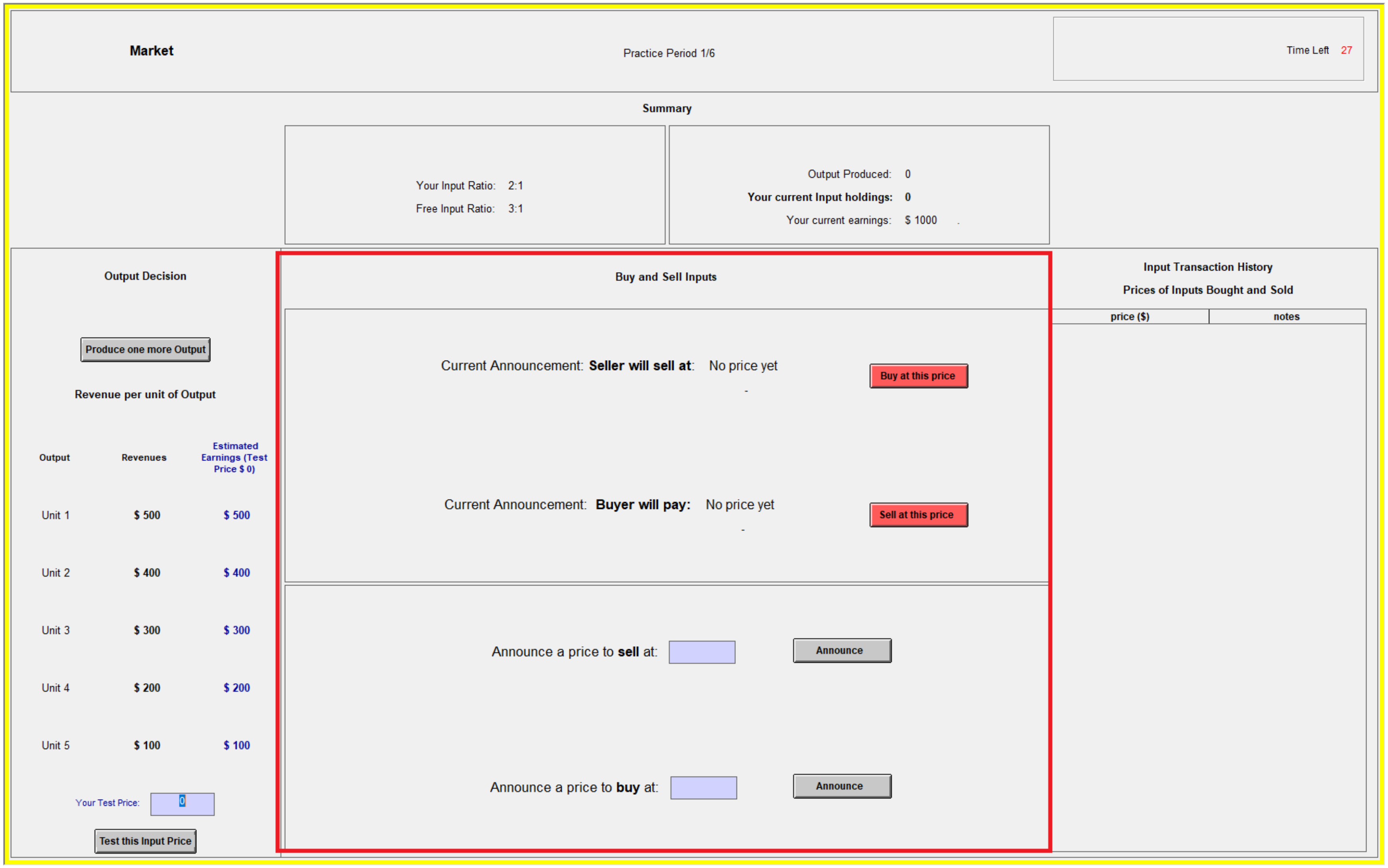
At the top of the screen is information about the period number, and time remaining till the end of the period. In each practice period you will have 3 minutes to complete trading and output decisions.



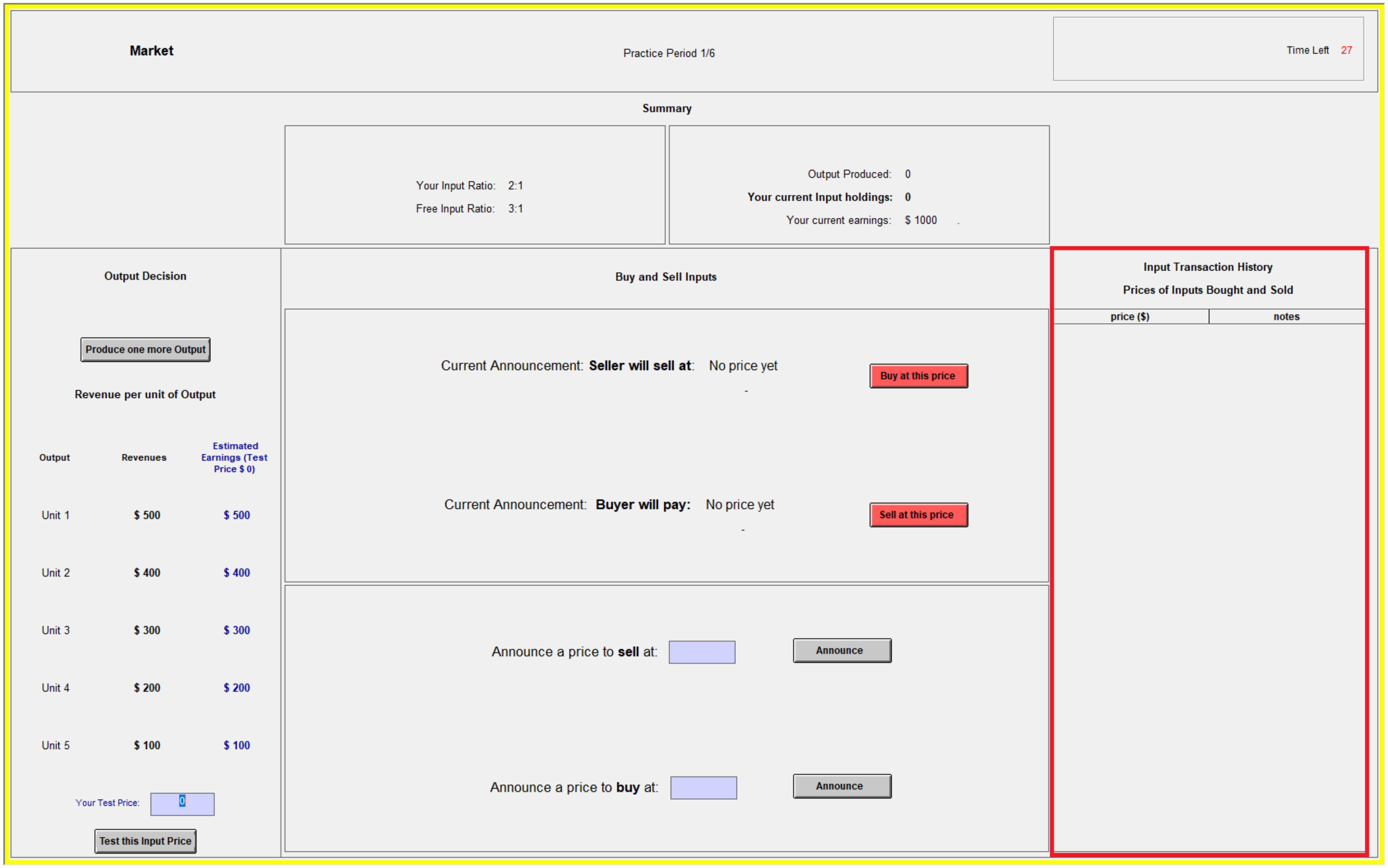
The summary section at the top of the screen gives an overview of your current situation. Here you can see your current input ratio is set at 2:1 and the free input ratio is 3:1. There is also an overview of output produced this period, your current input holdings and total earnings.



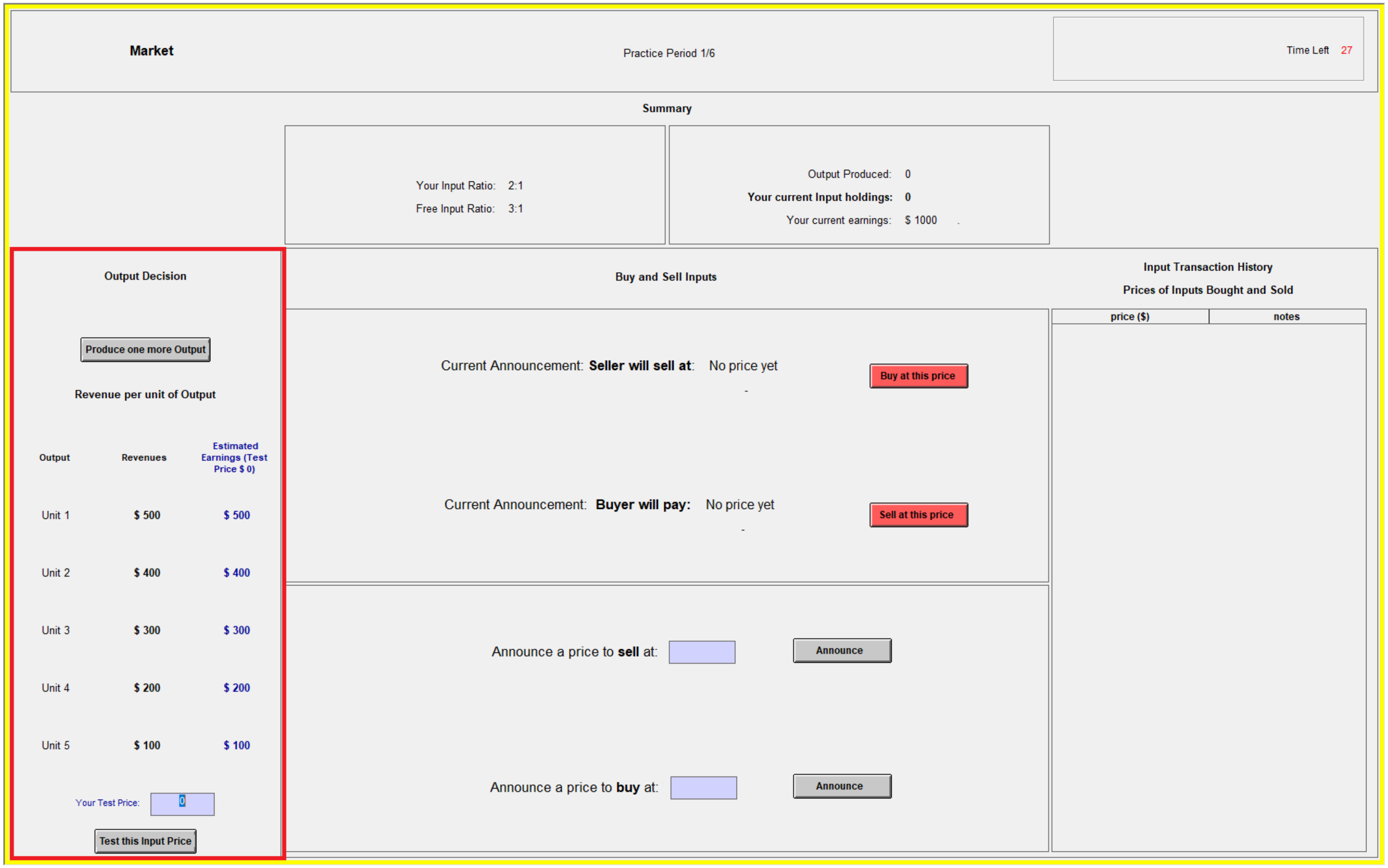
To the left of the screen is the output decision box. This gives you information on revenue and is where you produce output.



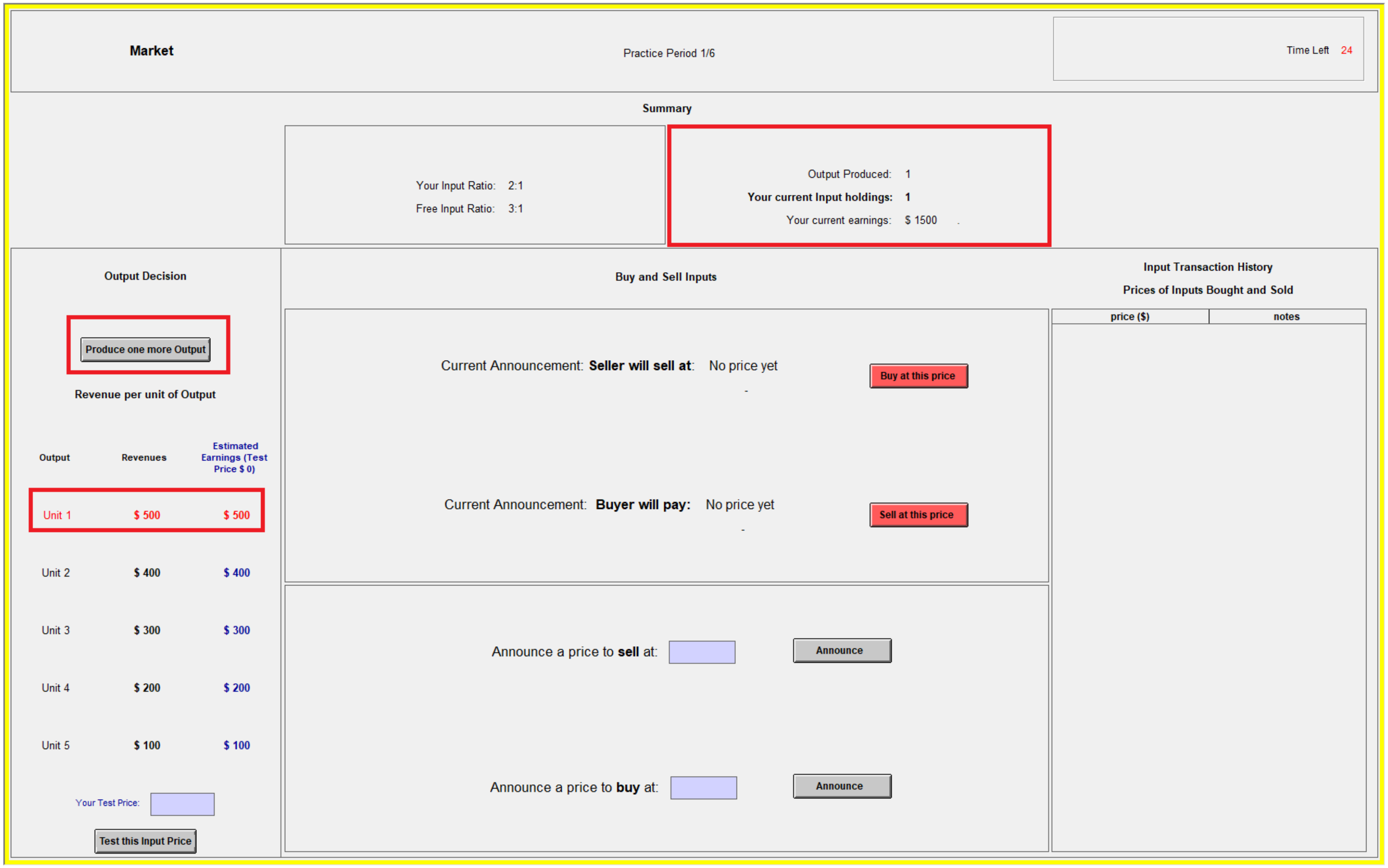
In the middle of the screen is the input trading box, where you will buy and sell inputs with other participants.



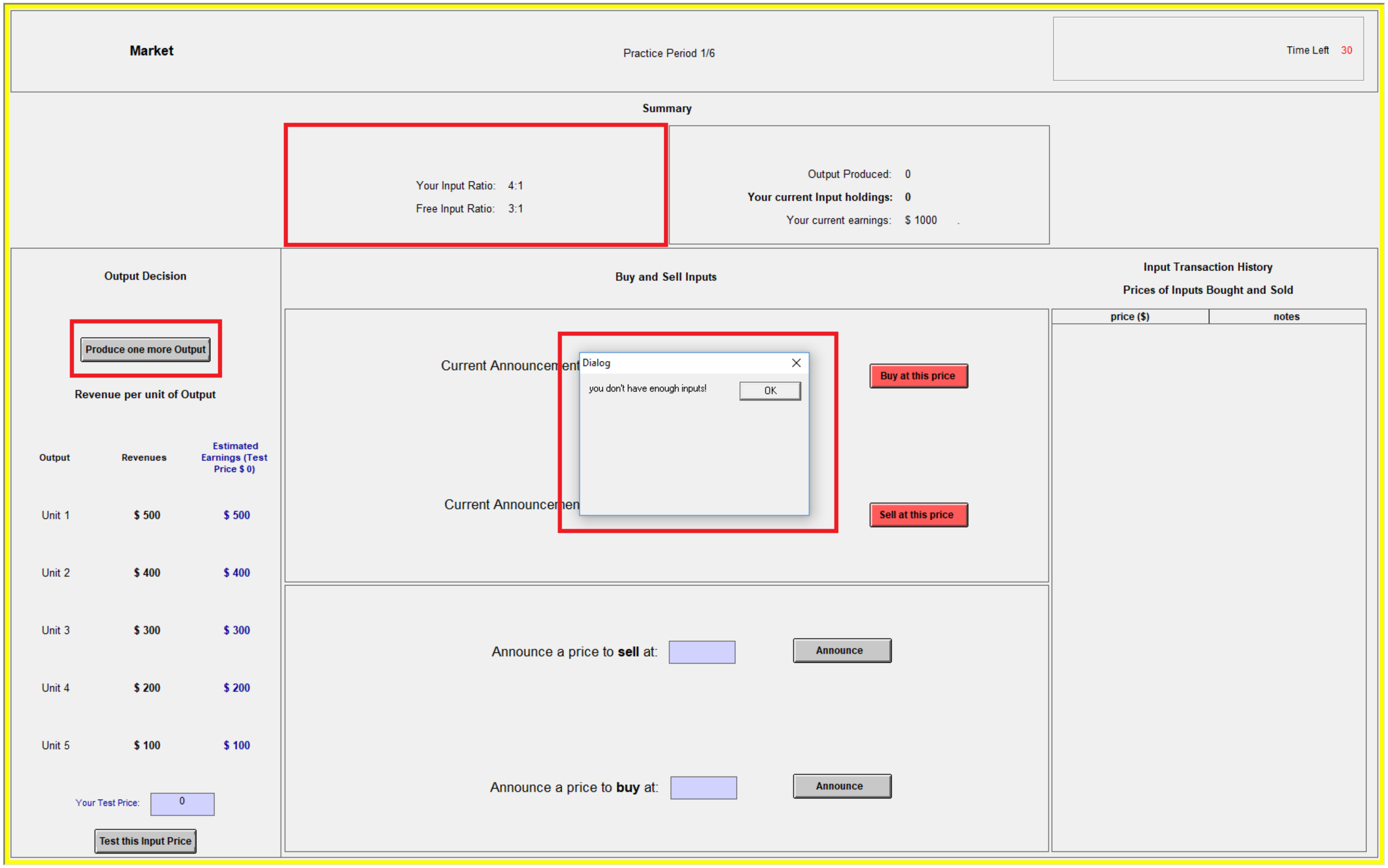
To the right of the screen is the input transaction history box. This records information about input trades during the period.



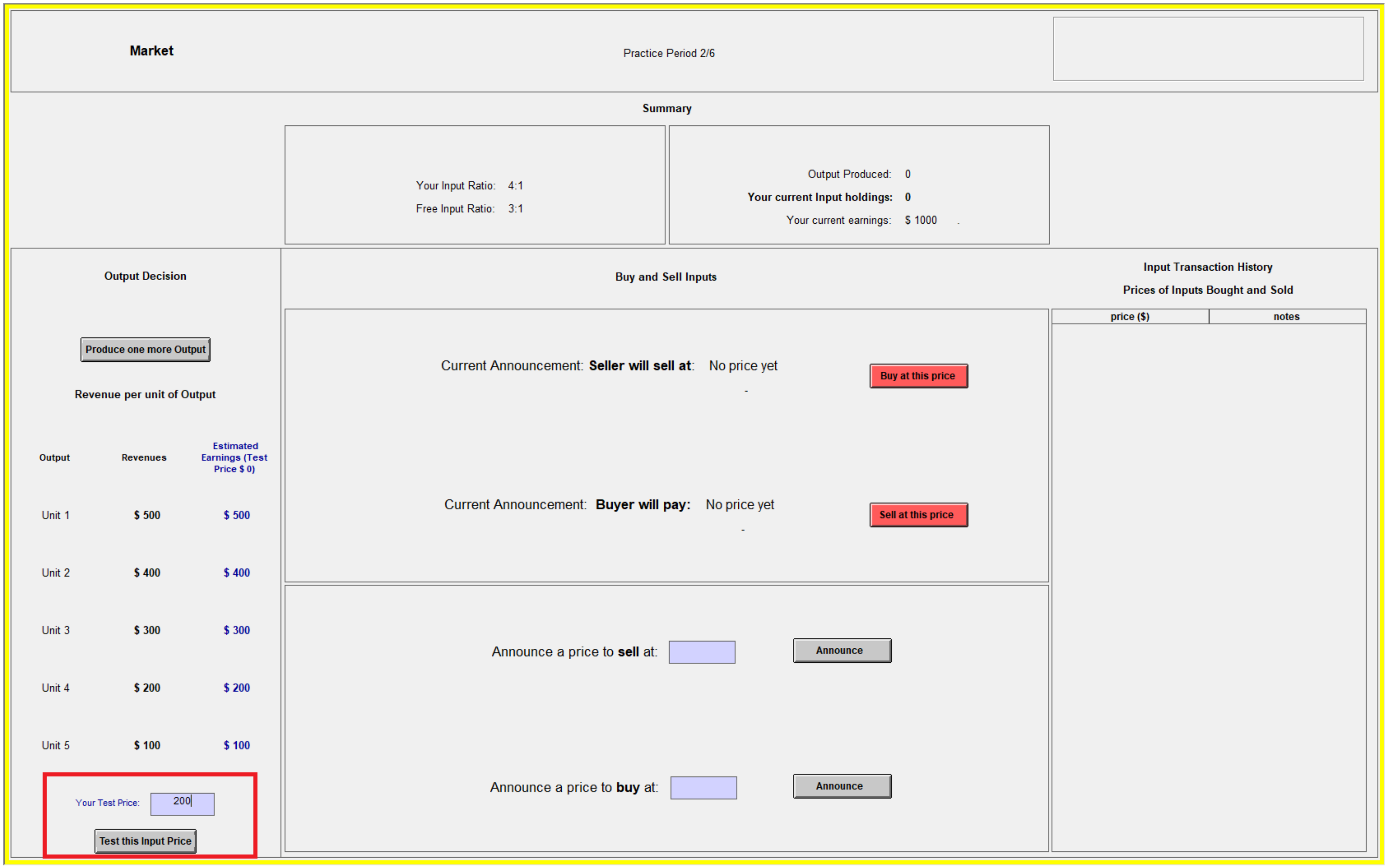
Looking at the output decision box again. For every unit of output you produce, you can earn lab dollar revenues shown in the table. You can see that the revenue earned from each unit produced decreases with each additional unit. Here, the first unit you produce will earn you $500, the second $400, and so on. This table is used for every unit of output you produce in the period. The table is reset at the end of the period. If you want to produce a unit, you can click on “Produce one more Output”. You need to be sure that you have enough inputs in your holdings (see top of screen) before you can produce an output.



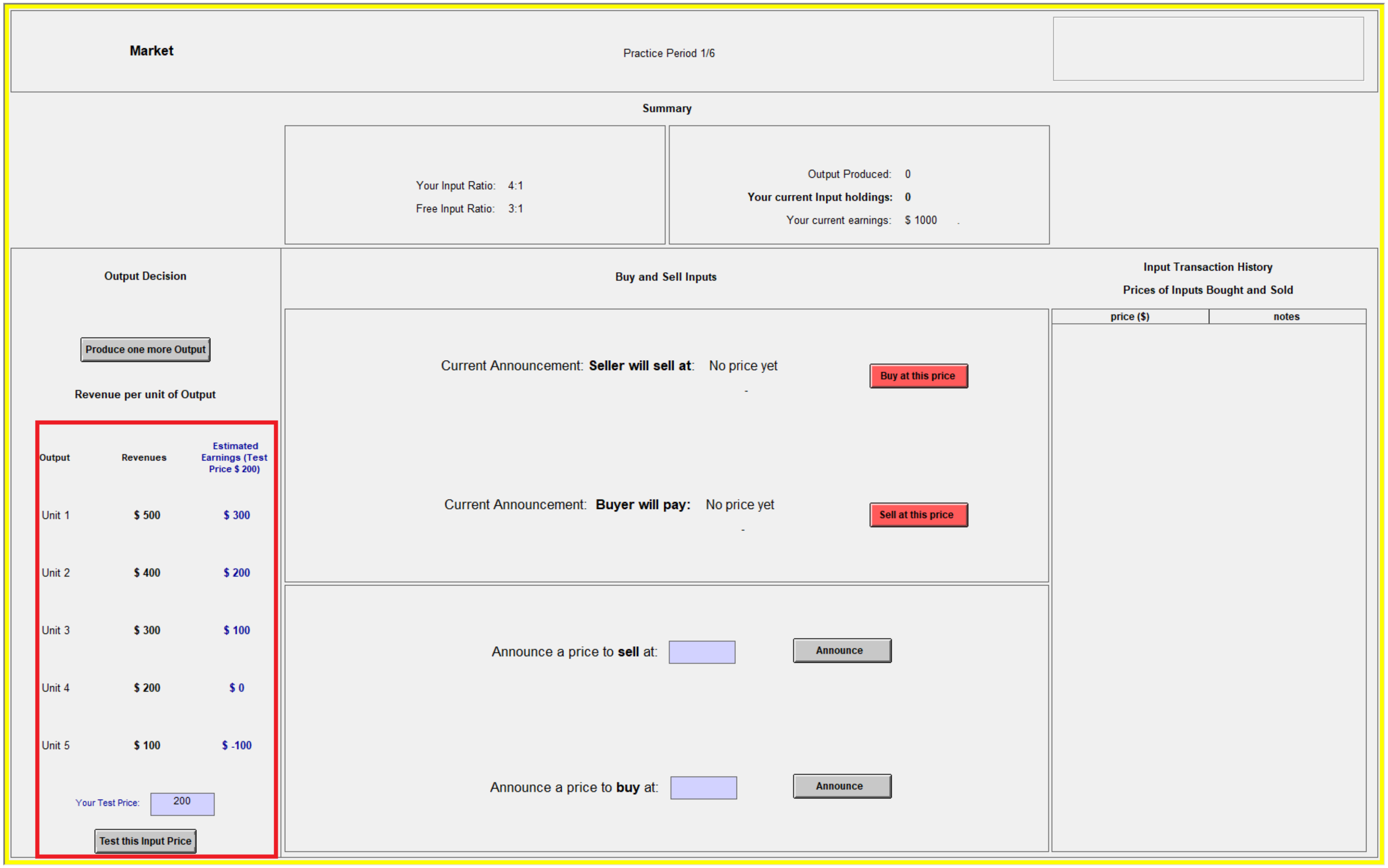
As soon as you click on "produce one more output" the top row of the revenue table turns red, signifying that you have produced your first unit of output in this period, earning revenue of $500. The summary box will automatically update. Note the changes in your input holdings and total earnings. Because in this example the input ratio is 2:1 and the free ratio is 3:1, producing one unit of output means you receive one input.



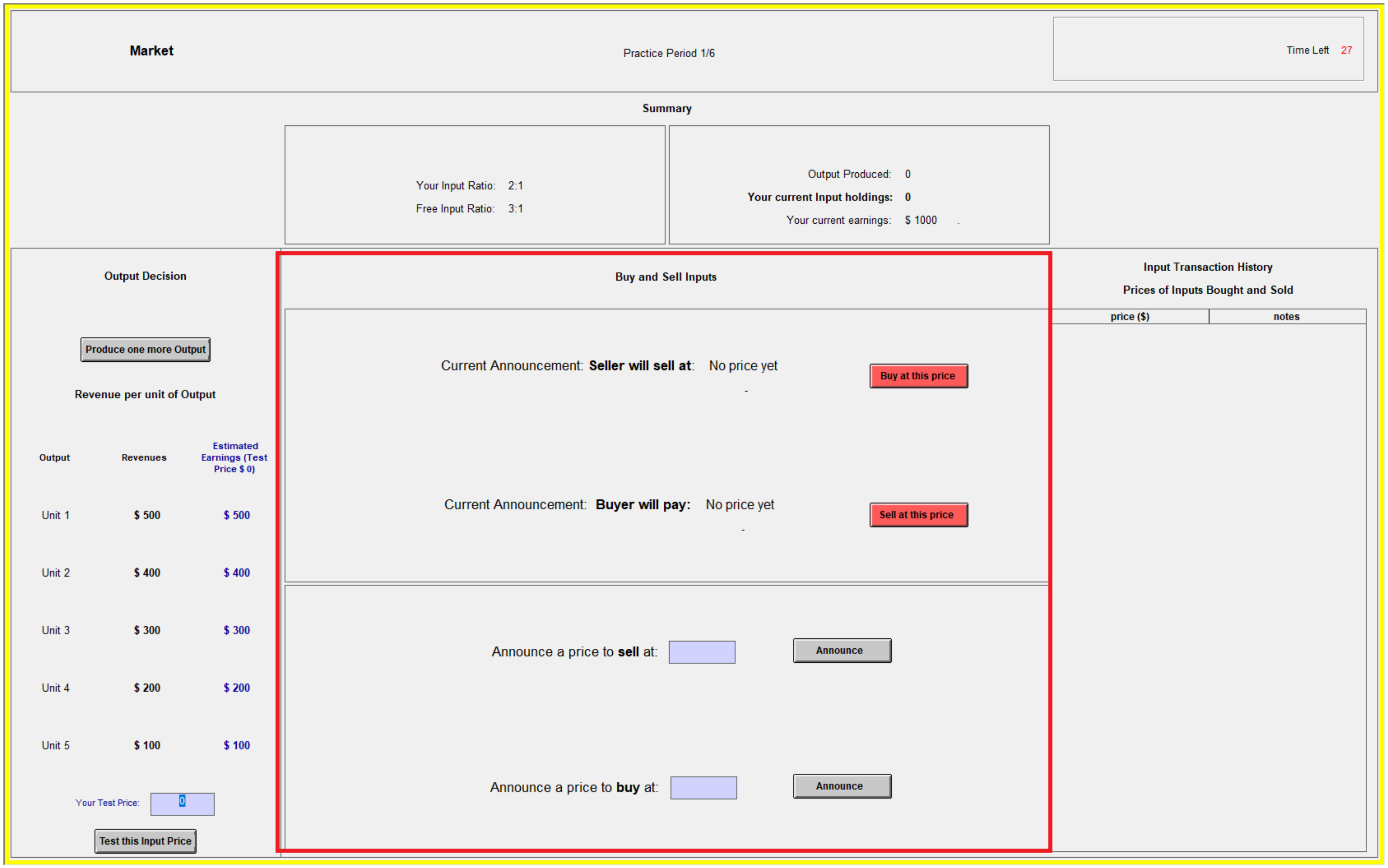
You may need to buy inputs before you can produce output. In this example, you have an input ratio of 4:1 and a free input ratio of 3:1. That means you need an input in order to produce an output. But, as you can see at the top of the screen, your current input holdings are zero. This means that when you click on Produce one more Output, you will get an error message that tells you you don't have enough inputs.



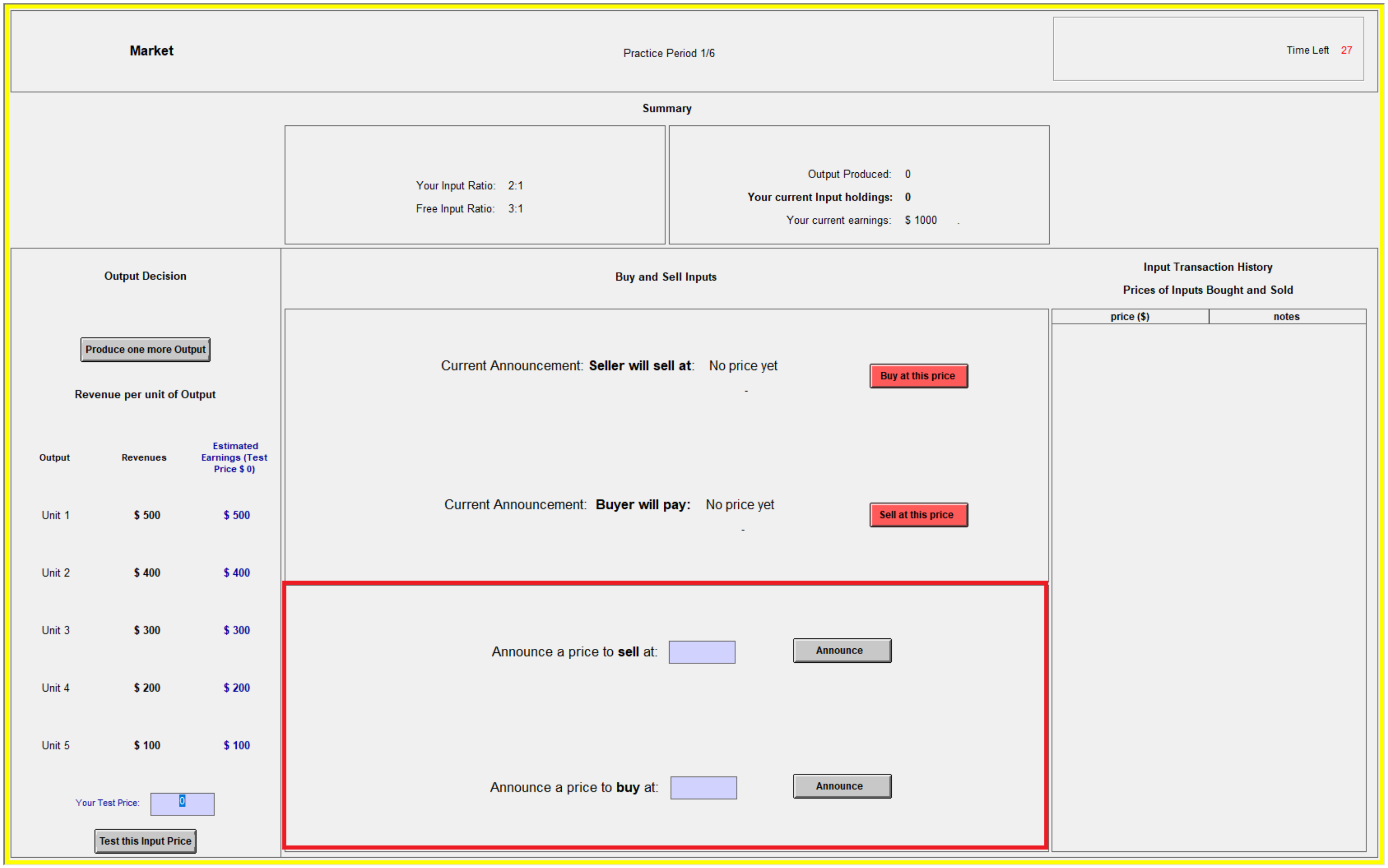
Notice that the output decision box also contains an area where you can test input prices. You can use the calculator to test the impact on your earnings of different input prices. If you type in a price for inputs and click test the input price, the estimated earnings column will update accordingly. Let's see what the estimated earnings would be for someone with an input ratio of 4:1 if the input price was $200.



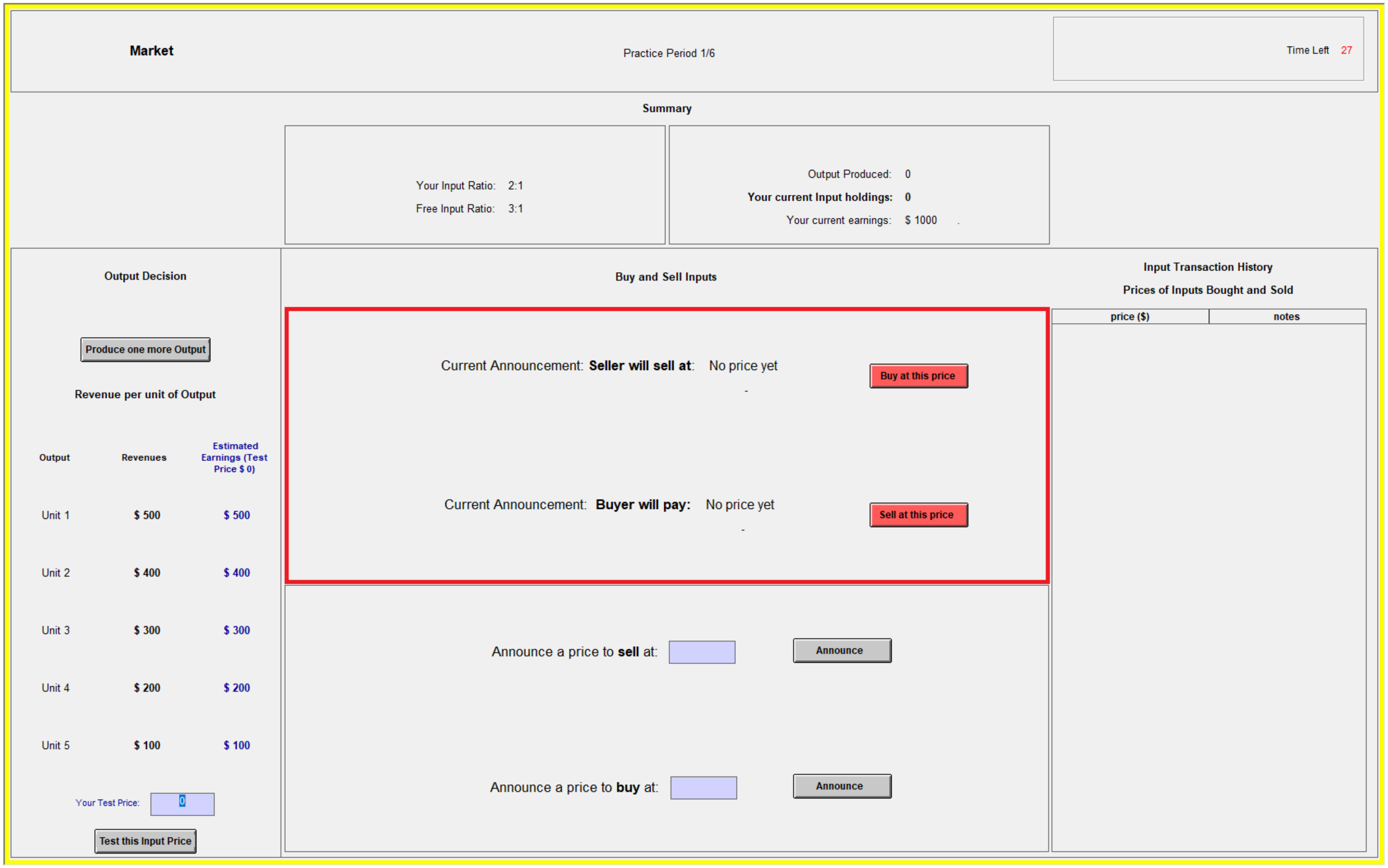
Once we click Test this Input Price, the estimated earnings column updates. Because you have an input ratio of 4:1 here, the revenue decreases by the cost of the inputs. This is because you need to purchase an input in order to produce an output. If you had an input ratio of 2:1, your estimated earnings would increase by the price of the input, as you would receive an input when you produced an output, and could then sell the input. In summary, because inputs are bought and sold, your total revenue earned by producing outputs will be higher or lower than stated in the revenue table. The difference will depend on the price of the inputs.



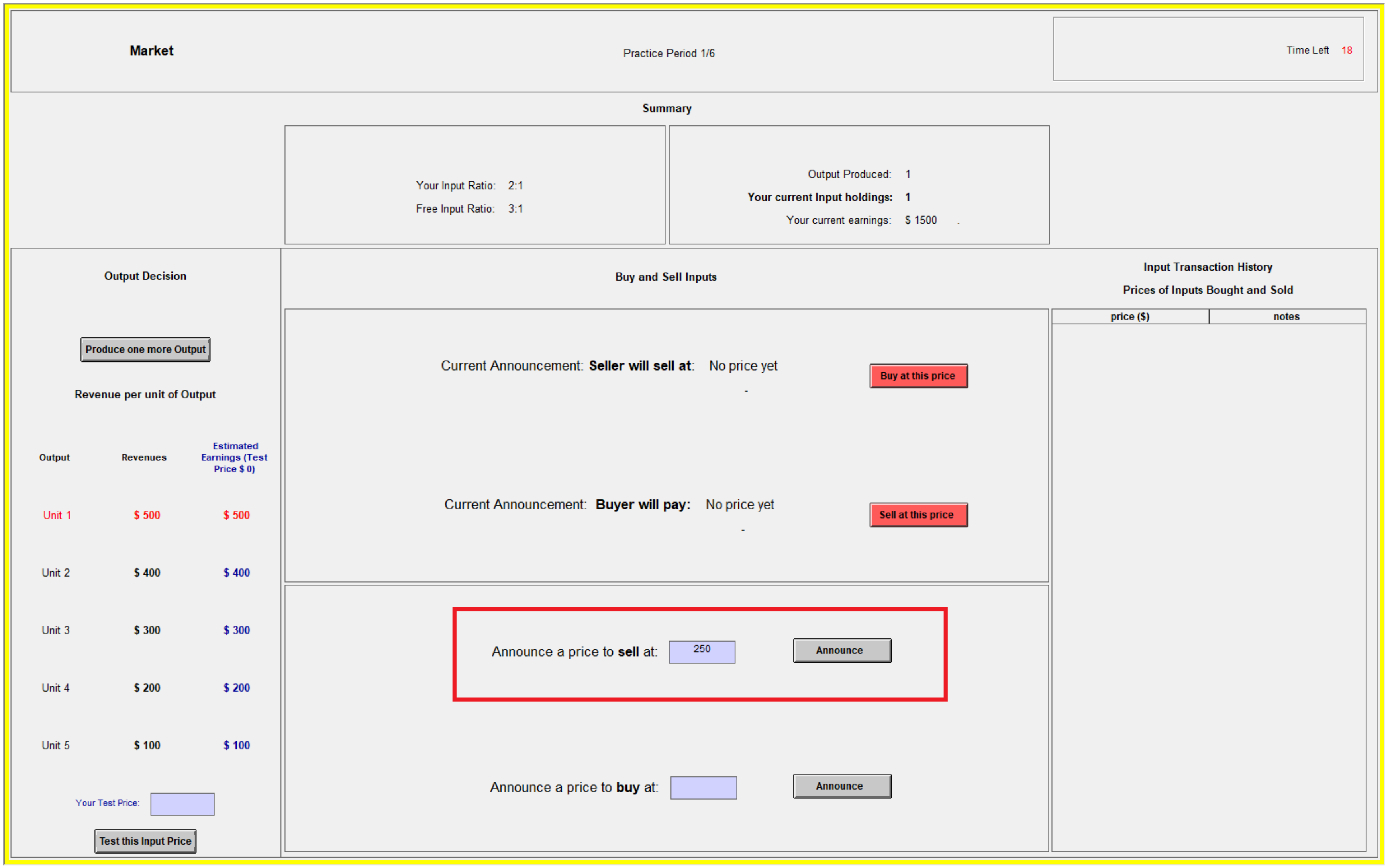
You will be buying and selling inputs in this experiment. Inputs can be bought to increase your output production, or inputs can be sold to increase your earnings. The input trading box is where you can buy and sell inputs, and find information about current prices for inputs. You will be trading inputs with the other participants in today's experiment. Note that you cannot save inputs for future periods – use them or lose them. You can buy and sell as many inputs as you like during each period. However, you can only buy or sell one at a time. Notice that the Buy and Sell inputs box is separated into two sections. The bottom section is where you can announce a price you want to buy an input for, or a price you wish to sell an input for. The top section is where you can see the currently announced buy and sell prices, and also where you can choose to buy or sell at these prices by clicking the buttons.



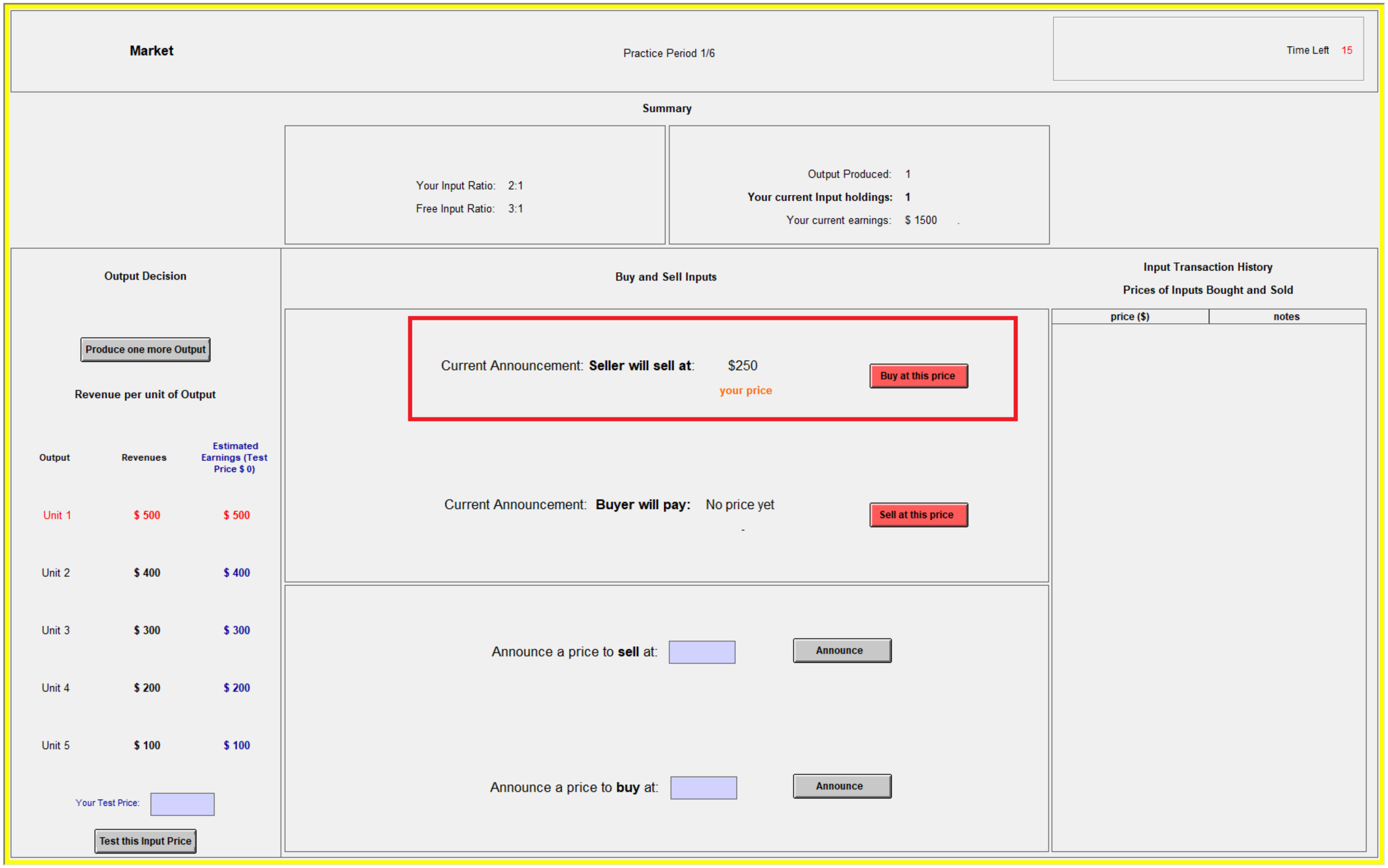
In the lower part of the trading box is where you can announce prices to buy or sell inputs at. Buyers announce prices they're willing to buy at. These offers are binding so that if another person wishes to sell at this announced price, then the seller can complete the sale by clicking on "Sell at this price". Any person wishing to sell an input can also announce the price they're willing to sell at. And if someone wants to buy at this price, then they can complete the purchase by clicking on "Buy at this price". The market only allows for one buying price announcement and one selling price announcement at a time. If someone else wants to make a new buying announcement, for example, they must enter a higher price than the current one. Similarly, if someone wants to make a new selling price announcement, they must enter a lower price than the current one. To summarise, buying price announcements can only increase from the currently announced price, and selling price announcements can only decrease from the currently announced price.



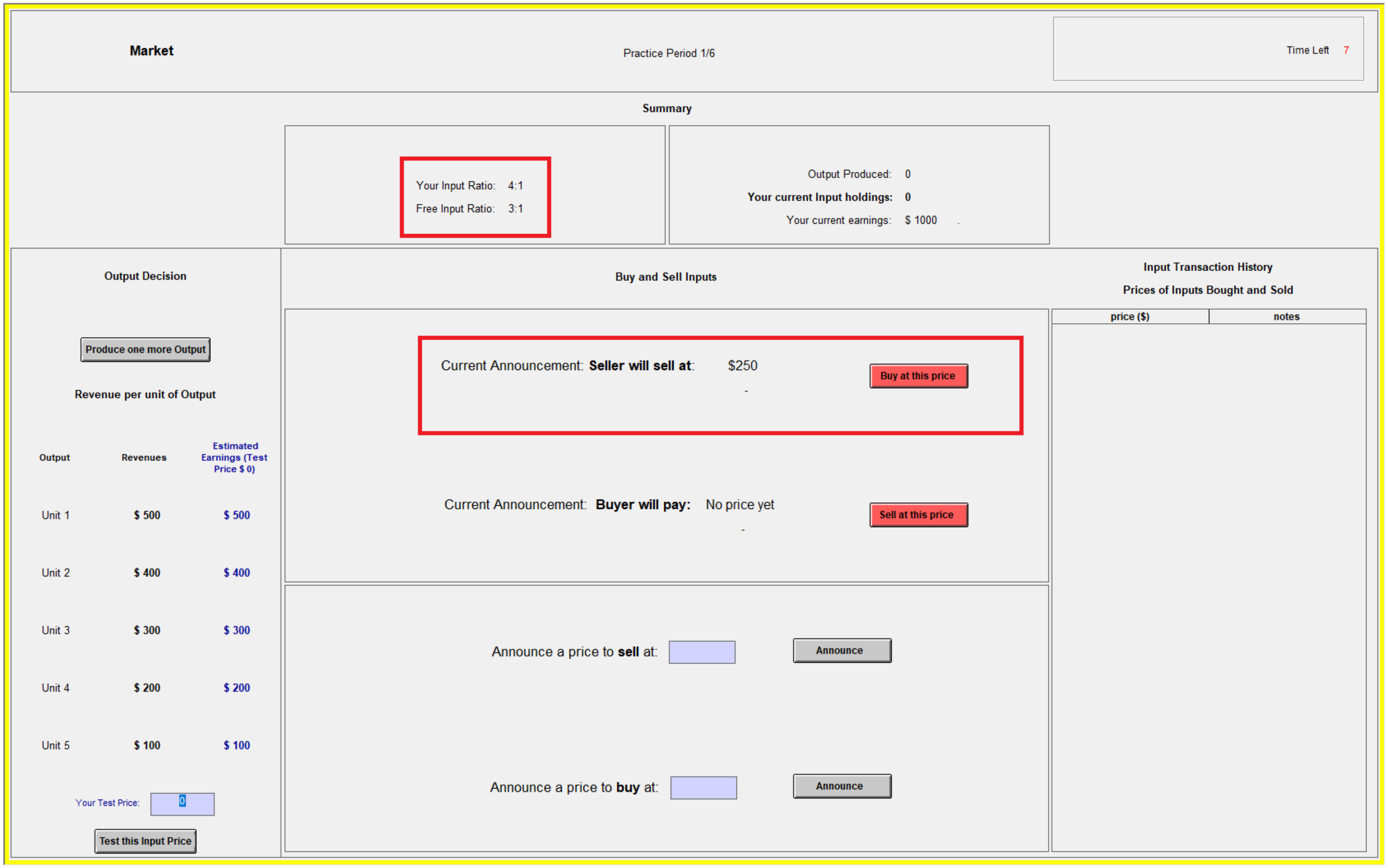
When someone sees an announced price that appeals to them, they can buy or sell at the price announced by clicking on the appropriate button. The transaction will then take place. If you are buying an input, you will see your input holdings immediately increase by one and your earnings decrease by the agreed upon price. If you are selling an input, you will see your input holdings decrease by one and your earnings increase by the agreed upon price. As soon as the input transaction has been completed the buy and sell announcements will be reset.



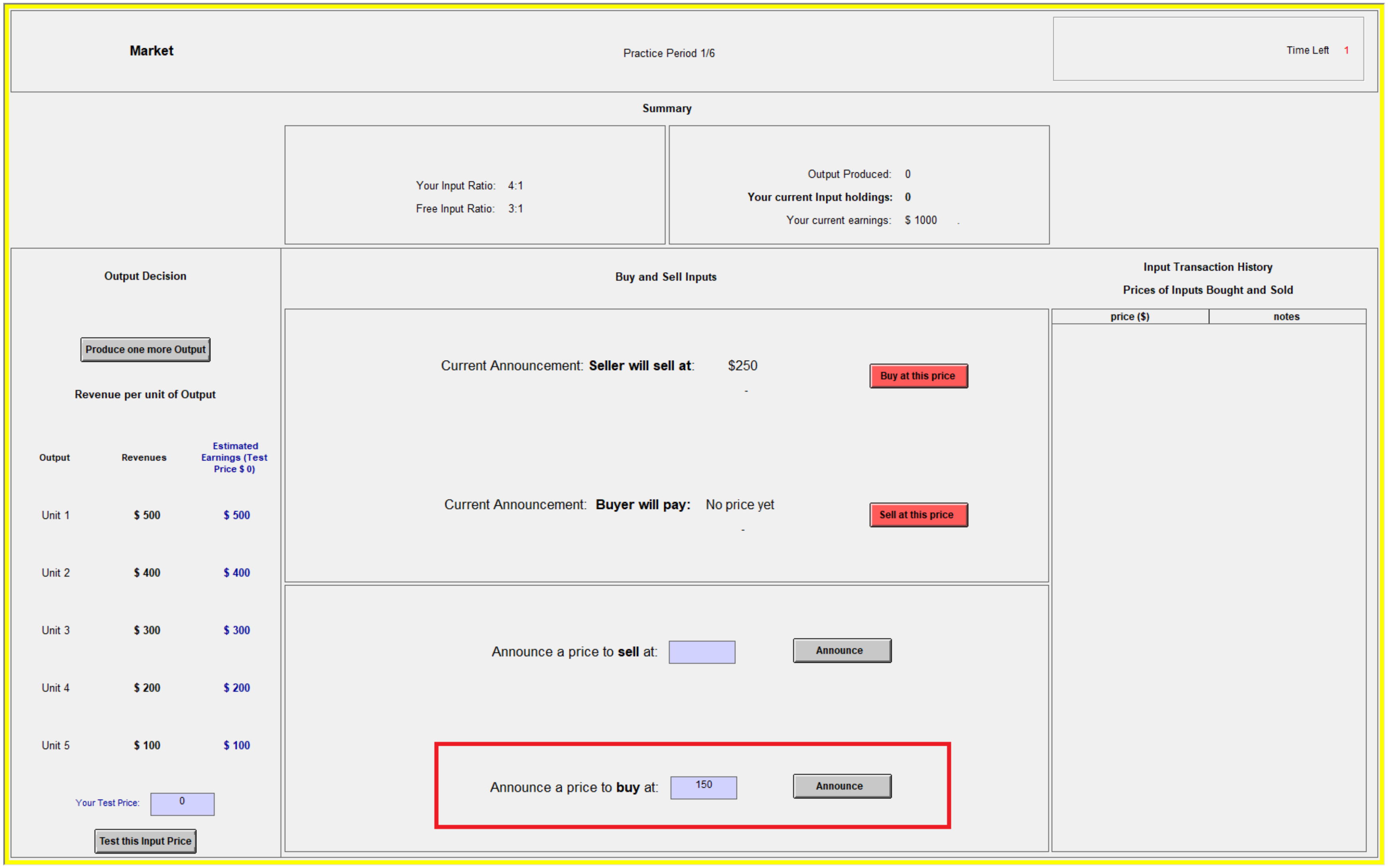
Suppose, for example, that you have an input that you wish to sell. You can announce a price you are willing to sell at by entering the number in the lower part of the screen, as shown here. In this example, you wish to sell your input for \$250.



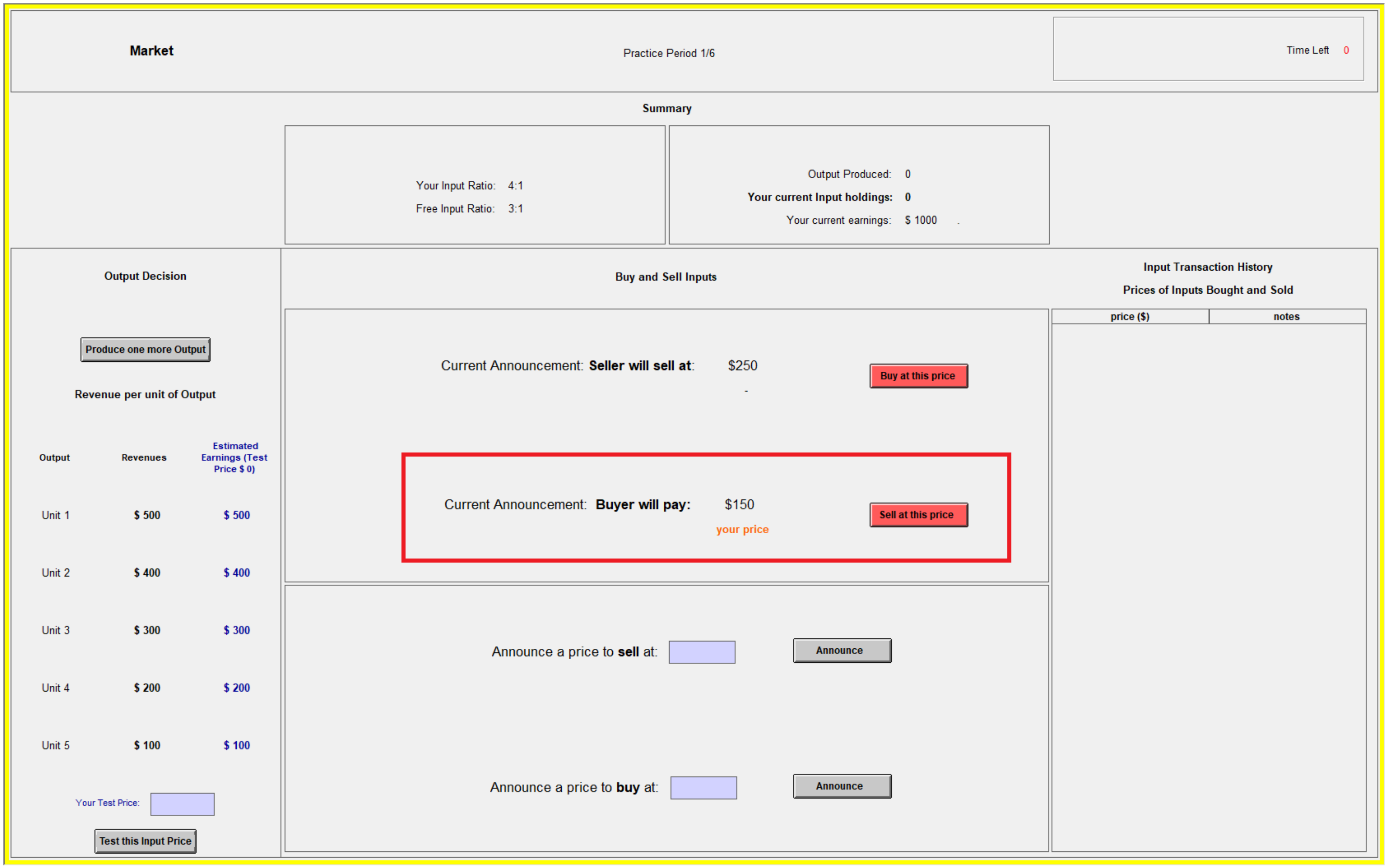
By clicking on Announce, your price will be displayed in the upper part of the trading screen, and other participants will be able to see it. If the price is yours, it will say "your price". This is just for your information. No-one else will know who posted the price. Now that you've announced your selling price, you can wait to see if someone else will accept this price and buy, or announce a new, lower selling price. Simply announcing your preferred selling price does not guarantee that you will sell an input at this price. Someone else will have to accept the price. Also, once you have clicked on "Announce" you cannot take back your announced price, even if you change your mind (so check what you've typed before clicking "Announce"). Note, also, that if you want to announce a price to sell at, you must have at least one input in your holdings. You cannot offer to sell an input if you don't have any.



Now, let's suppose you have an input ratio of 4:1. You cannot produce output without first purchasing inputs. On the screen, there is a current announcement for a selling price – you can buy for $250.

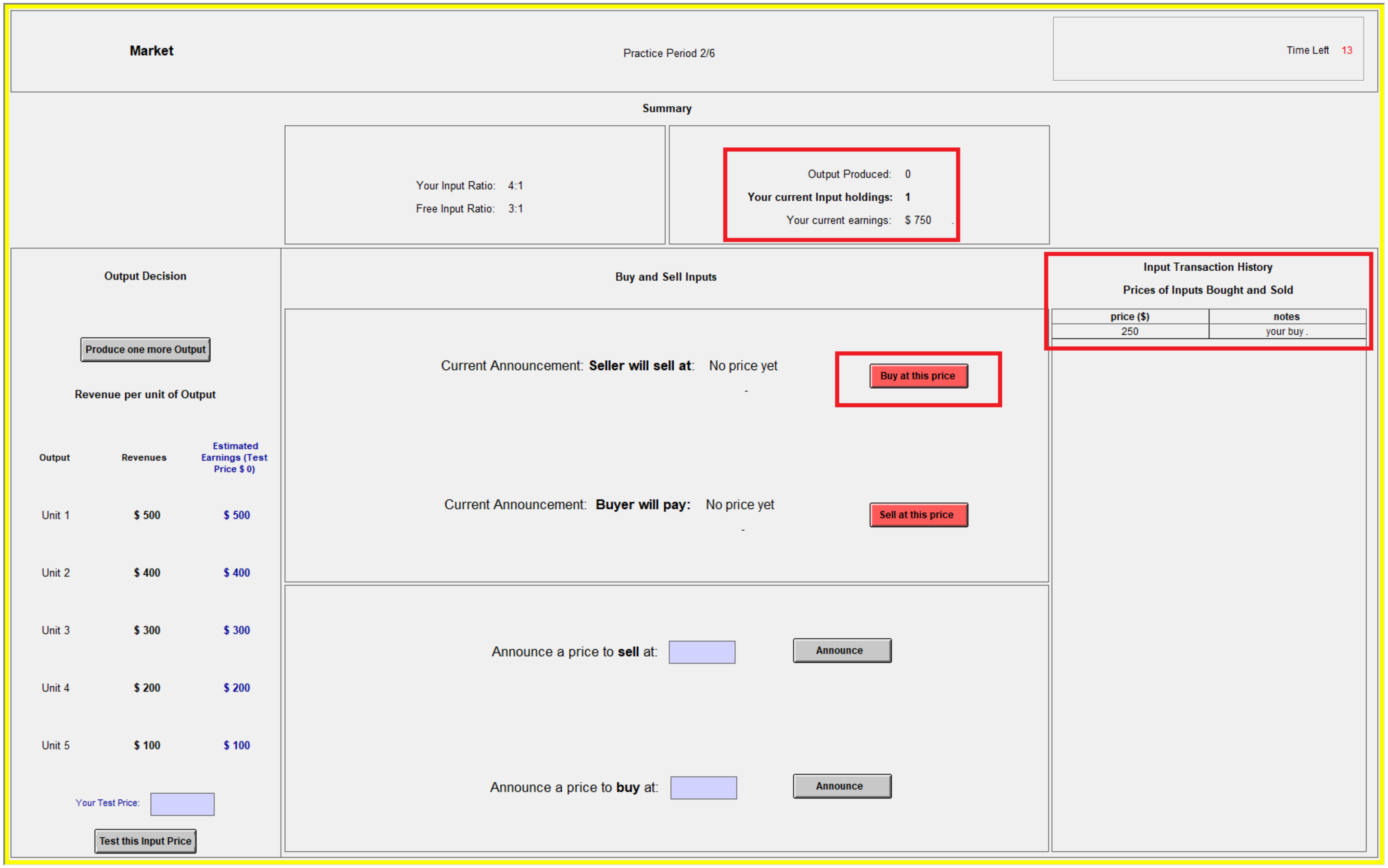


Suppose you'd rather spend less. You can announce a price to buy at of $150. This is done in the same way as announcing a sell price, but using the other input area and button.



Once you click on announce, the price you are willing to buy at is displayed in the current announcement section of the screen.

Note again, that it tells you that this is your price. You can wait and see if someone else will accept your price and sell an input to you for \$150. Or you can accept another sell price that has been announced.



Let's suppose you accept the $250 announcement by another player. If you click on buy at this price, your input holdings and earnings will automatically update. The transaction history will also update, showing that an input has been traded at a price of $250. It also notes that this was your buy (but others do not see this information).

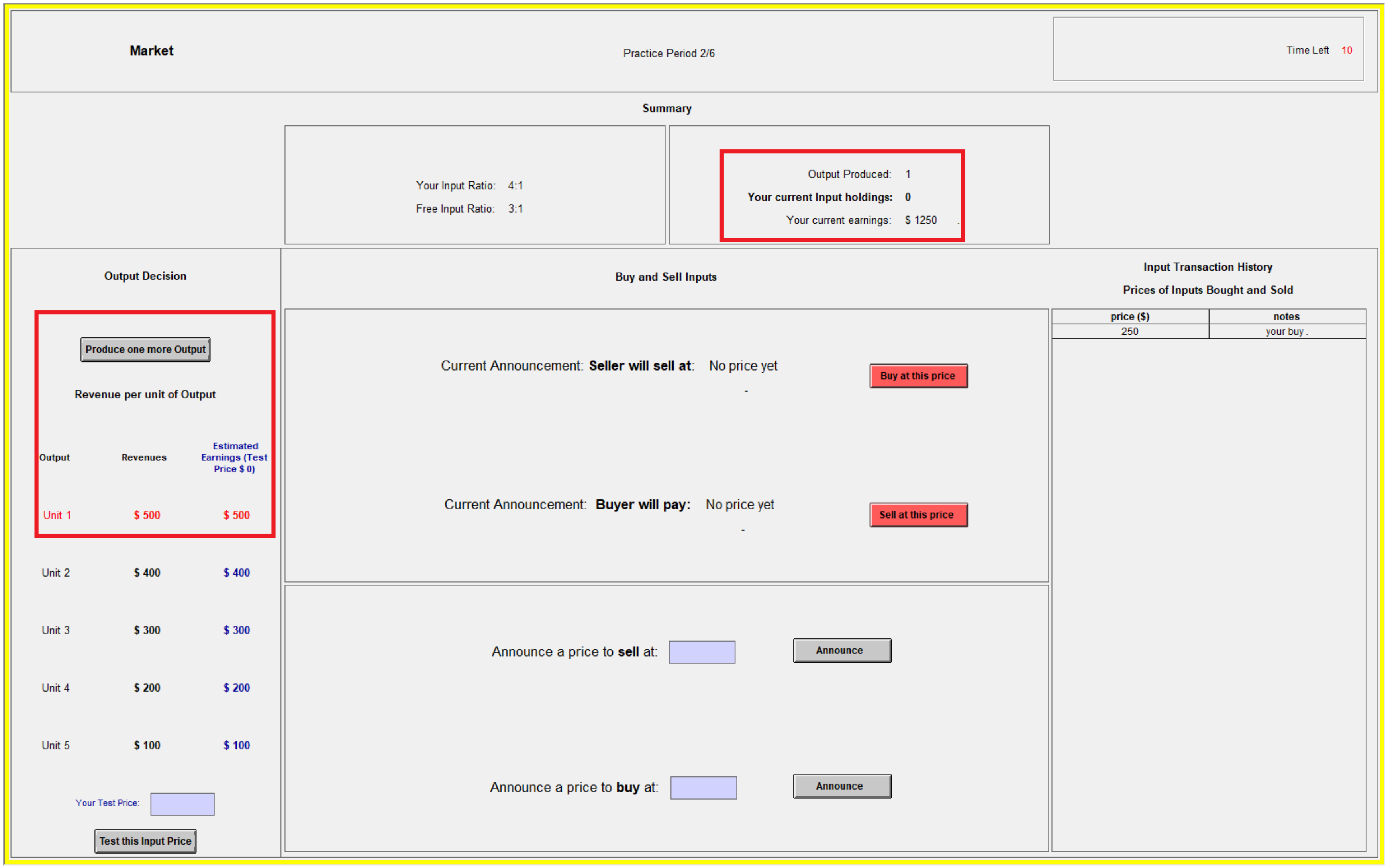
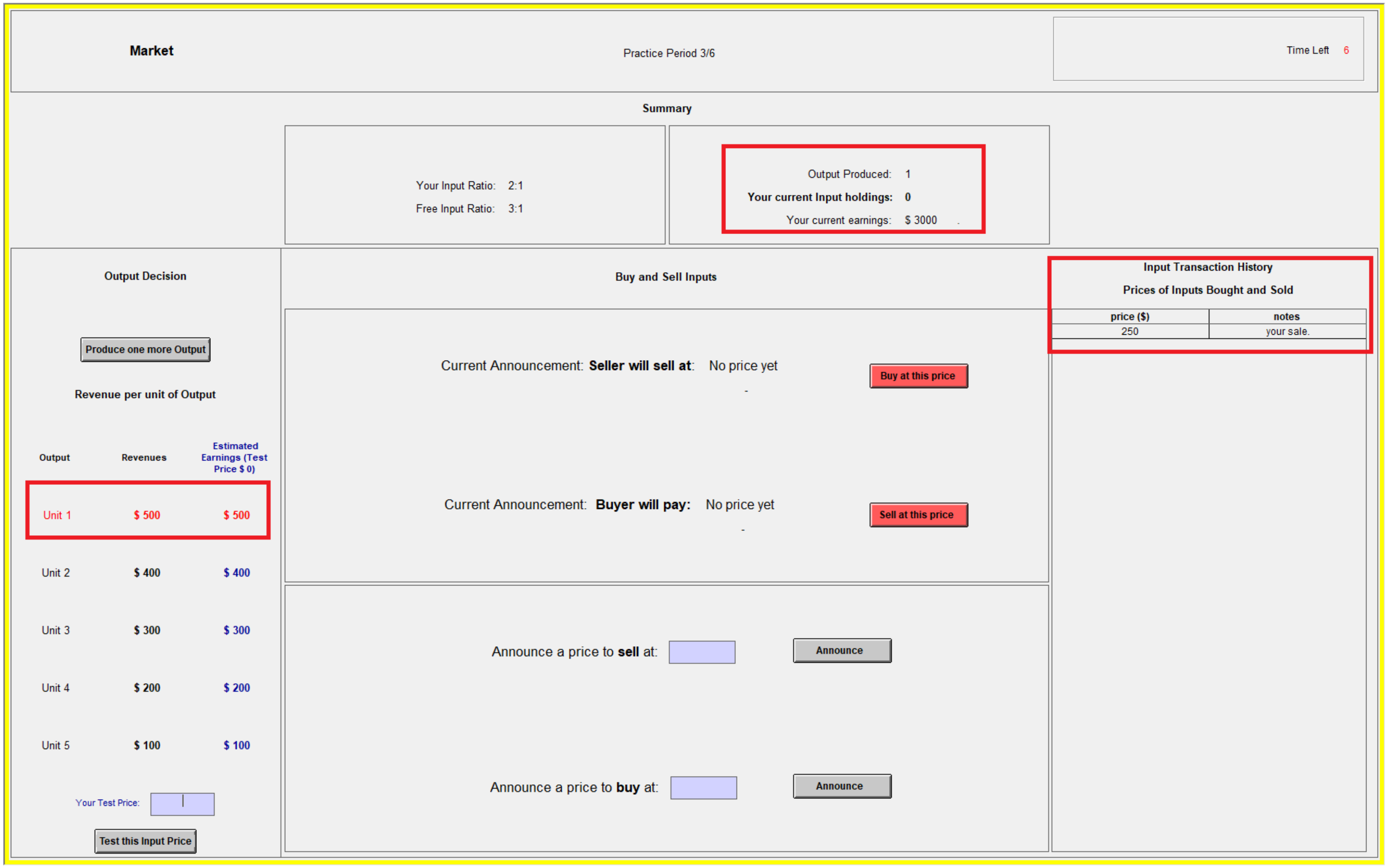
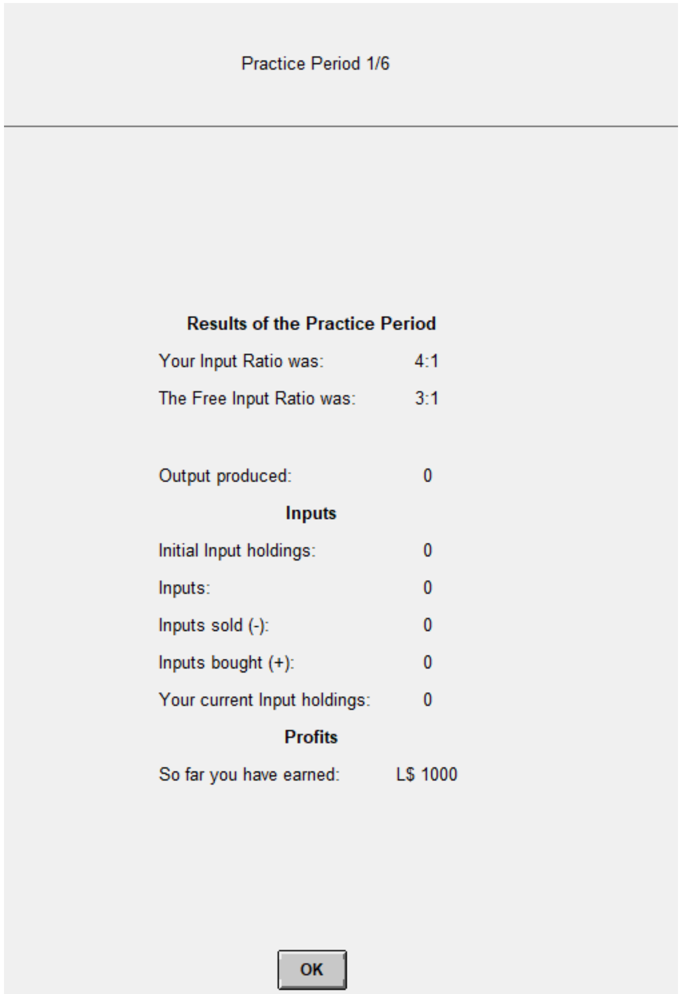


Figure 60: Market Screen

Now that you have an input, you can produce an output. By clicking on produce one more output, your summary information automatically updates again. Your output produced increases to 1, your input holdings fall to 0 and your earnings increase by the revenue earned.



For the seller, the updated screen, with transaction history, looks like this. Input holdings are now reduced by one, earnings increase by the sale price, and the transaction history records the transaction as the seller's sale, but this information is only available to the seller.



At the end of each period, you will see a period summary screen. This provides you an overview of how you are doing in the experiment so far. Take some time to review this information as you may find it useful. After a few moments, this screen will disappear and you will start the next period of the experiment. Now we will begin the 6 practice periods, before we get started on the paid experiment. Feel free to explore and try different things out as the earnings you make in these practice periods will not affect the cash you earn in today's experiment.

The Input Ratio and Revenues

Before we get started on the paid experiment, let's talk a little more about how input ratios work. In this experiment, you'll be making decisions about what input ratio you will be producing at. You can choose a ratio of between 0:1 and 5:1. You will make this choice at the start of each period.

If you choose an input ratio higher than the free input ratio, then you will need inputs in order to produce output. For example, if the free input ratio is 3:1 and you choose an input ratio of 5:1, then you will need 2 inputs to produce 1 unit of output.

If you choose an input ratio lower than the free input ratio, then you will receive inputs when you produce an output. For example, if the free input ratio is 2:1 and you choose an input ratio of 0:1, then you will receive 2 inputs when you produce 1 unit of output.

You can see that the number of inputs you need to produce output (or receive when you produce output) depends on the difference between the free input ratio and your chosen input ratio.

If you choose the Input Ratio 4:1 and the Free Input Ratio is 2:1, in total how many Inputs will you use or receive when producing a unit of Output?

- receive 1 unit
- use 1 unit
- receive 2 units
- use 2 units
- receive 3 units
- use 3 units
- receive 4 units
- use 4 units

High and Low Revenues

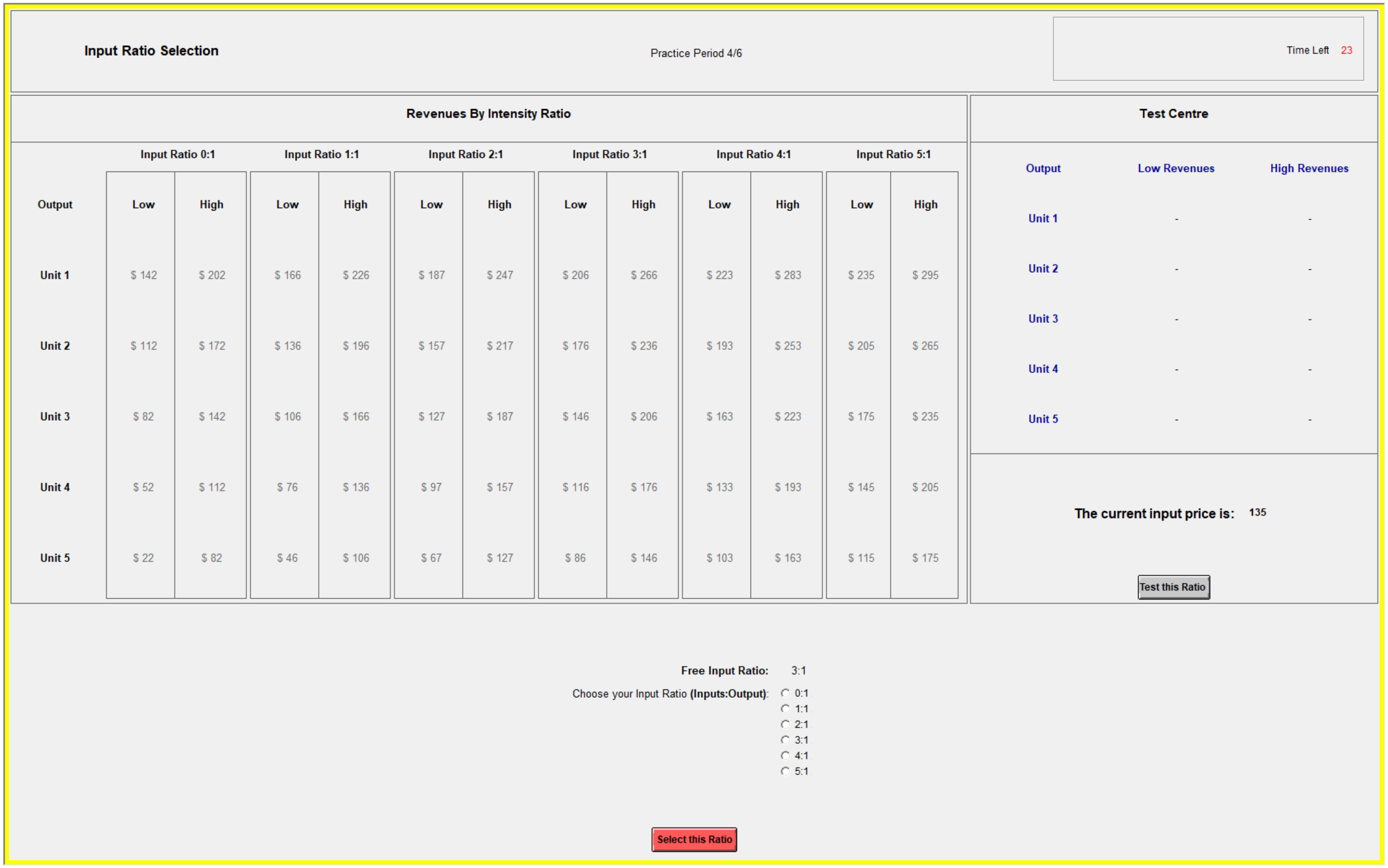
For the rest of the experiment there will be two revenue levels - high and low. Revenues you can earn per unit produced will vary depending on the input ratio you choose, and whether revenues are high or low. Each period, after you have chosen your input ratio and before you have produced output or traded inputs, the computer will randomly decide whether revenues in that period will be high or low. There is a 50:50 chance of either revenue level being chosen by the computer.

When revenues are high, each output produced will earn $60 more than when revenues are low for all participants in the experiment.

Once everyone has selected their input ratio at the start of the period, everyone will be able to produce output and buy or sell inputs, just as we explained earlier.

The next slides will show you how you can choose your input ratio.

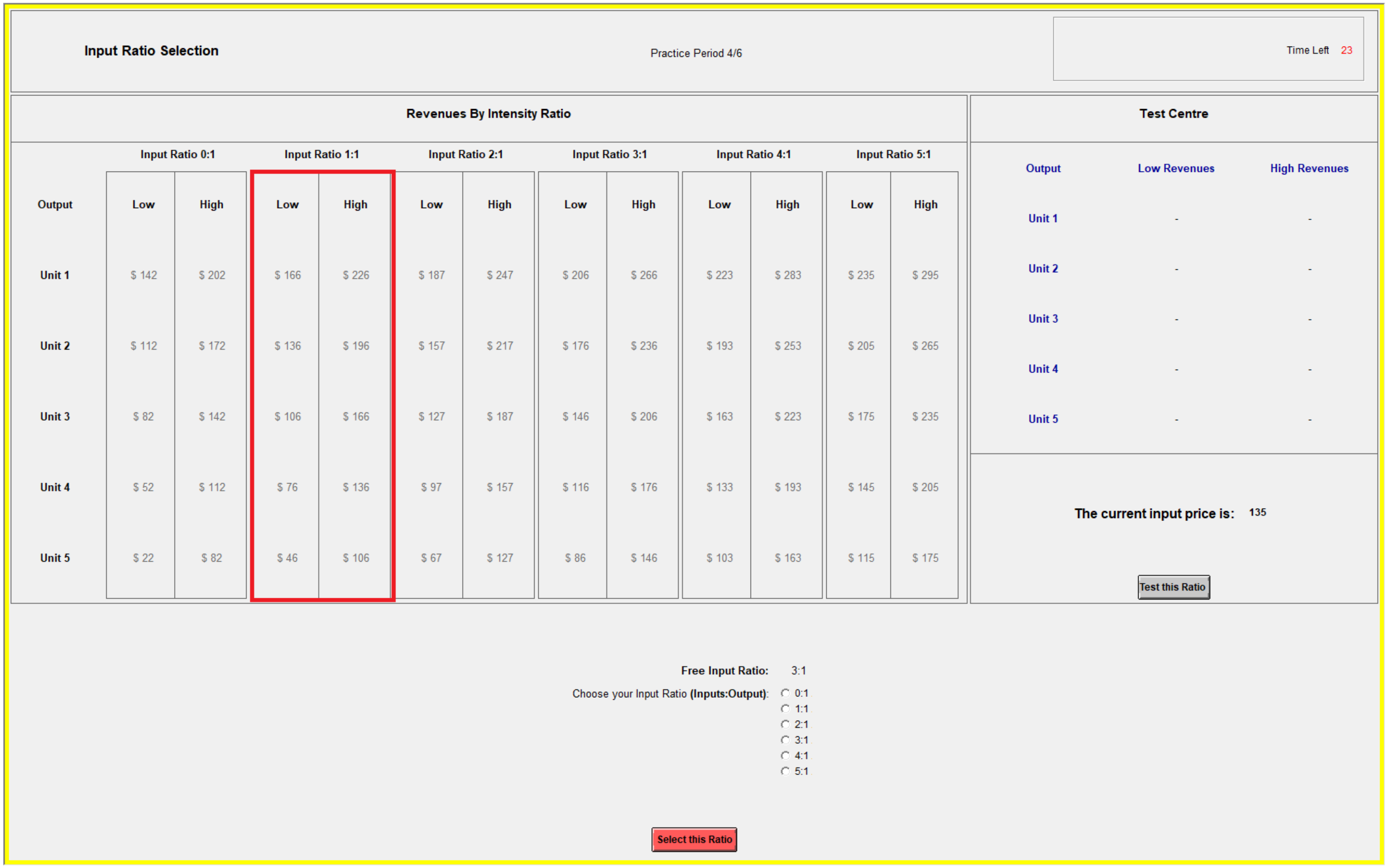
A slideshow with recorded instructions was also used to introduce the intensity decision screen, followed by 3 more practice rounds, incorporating the intensity choice and output screen.



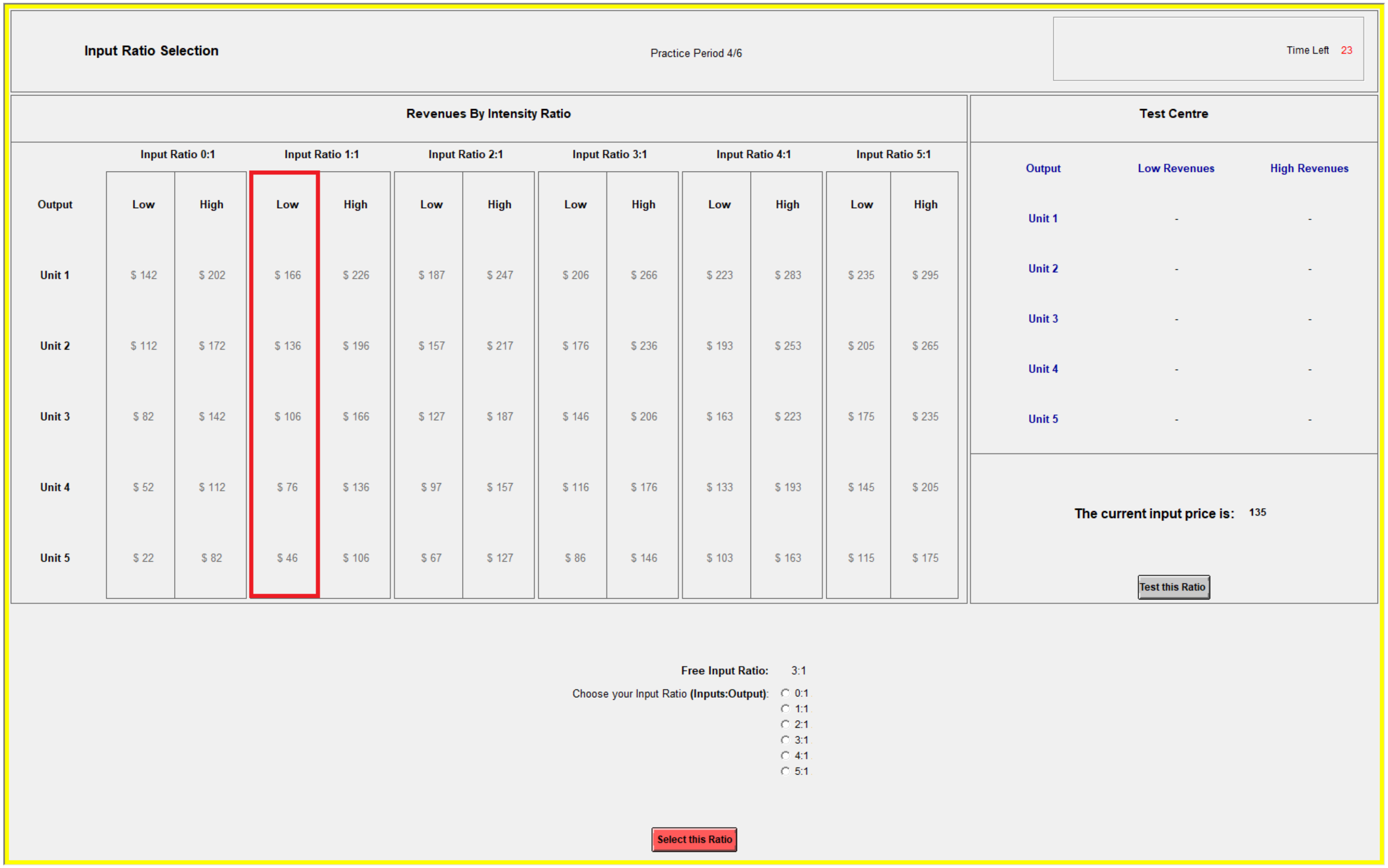
Output	Input Ratio 0:1 Low	Input Ratio 0:1 High	Input Ratio 1:1 Low	Input Ratio 1:1 High	Input Ratio 2:1 Low	Input Ratio 2:1 High	Input Ratio 3:1 Low	Input Ratio 3:1 High	Input Ratio 4:1 Low	Input Ratio 4:1 High	Input Ratio 5:1 Low	Input Ratio 5:1 High
Unit 1	$ 142	$ 202	$ 166	$ 226	$ 187	$ 247	$ 206	$ 266	$ 223	$ 283	$ 235	$ 295
Unit 2	$ 112	$ 172	$ 136	$ 196	$ 157	$ 217	$ 176	$ 236	$ 193	$ 253	$ 205	$ 265
Unit 3	$ 82	$ 142	$ 106	$ 166	$ 127	$ 187	$ 146	$ 206	$ 163	$ 223	$ 175	$ 235
Unit 4	$ 52	$ 112	$ 76	$ 136	$ 97	$ 157	$ 116	$ 176	$ 133	$ 193	$ 145	$ 205
Unit 5	$ 22	$ 82	$ 46	$ 106	$ 67	$ 127	$ 86	$ 146	$ 103	$ 163	$ 115	$ 175

Output	Low Revenues	High Revenues
Unit 1	-	-
Unit 2	-	-
Unit 3	-	-
Unit 4	-	-
Unit 5	-	-

You will see this screen at the start of the period. This is where you make your input ratio selection. The table shows you the revenue you can earn for each unit of output you produce, given a particular input ratio selection. For any input ratio, and any output unit, notice that the high revenue is always $60 higher than the low revenue. As your input ratio rises, the revenue you can earn from each unit of output also increases. This means higher input ratios result in higher revenues per unit of output. However, remember that higher input ratios will mean you have to buy inputs in order to produce outputs and earn these revenues. Meanwhile, lower input ratios will mean you receive inputs that you can then sell to earn extra revenue. The input costs are not shown here, as they will depend on the buying and selling prices during the experiment.



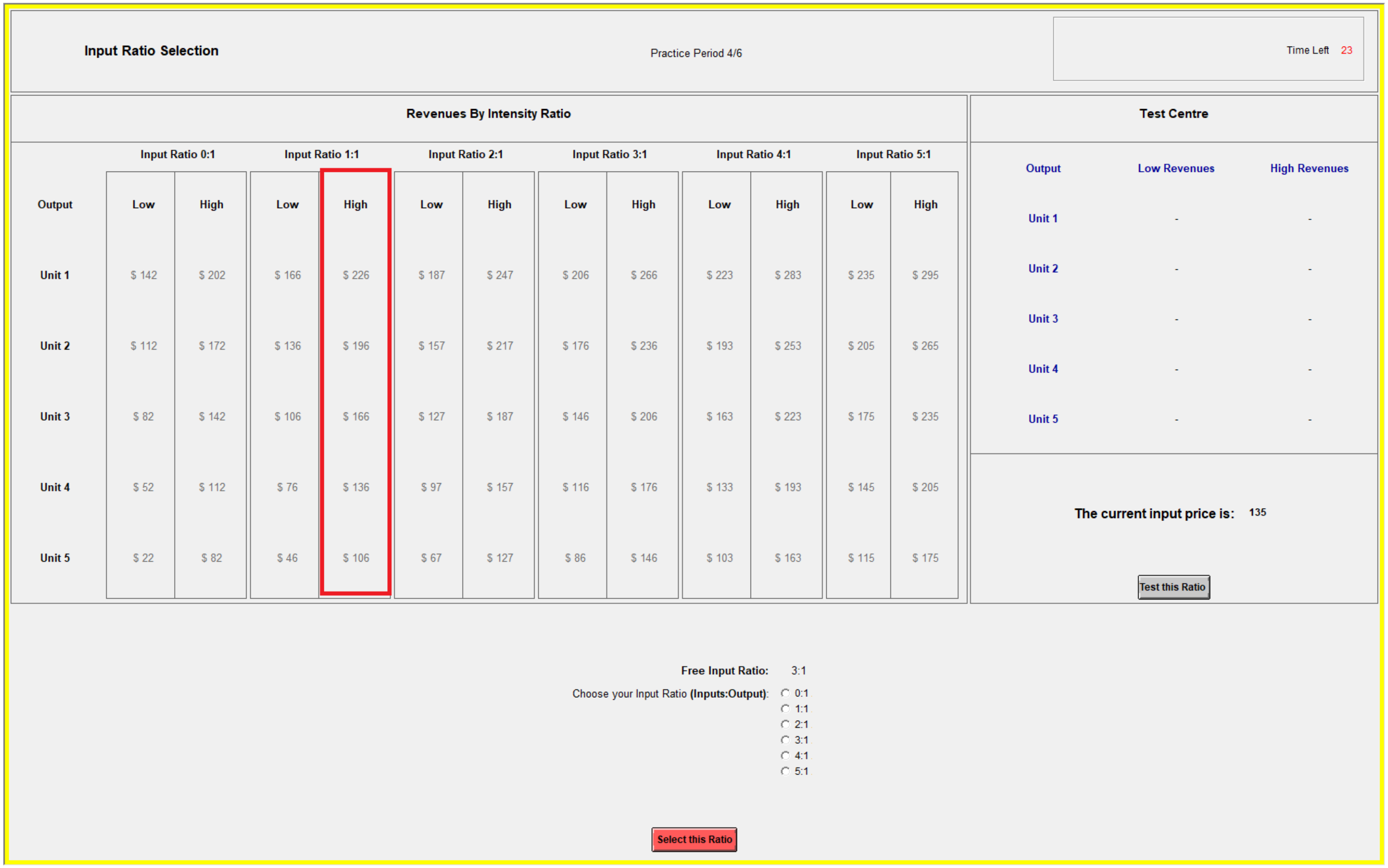
Let's take a look at the high and low revenues if you choose an input ratio of 1:1.



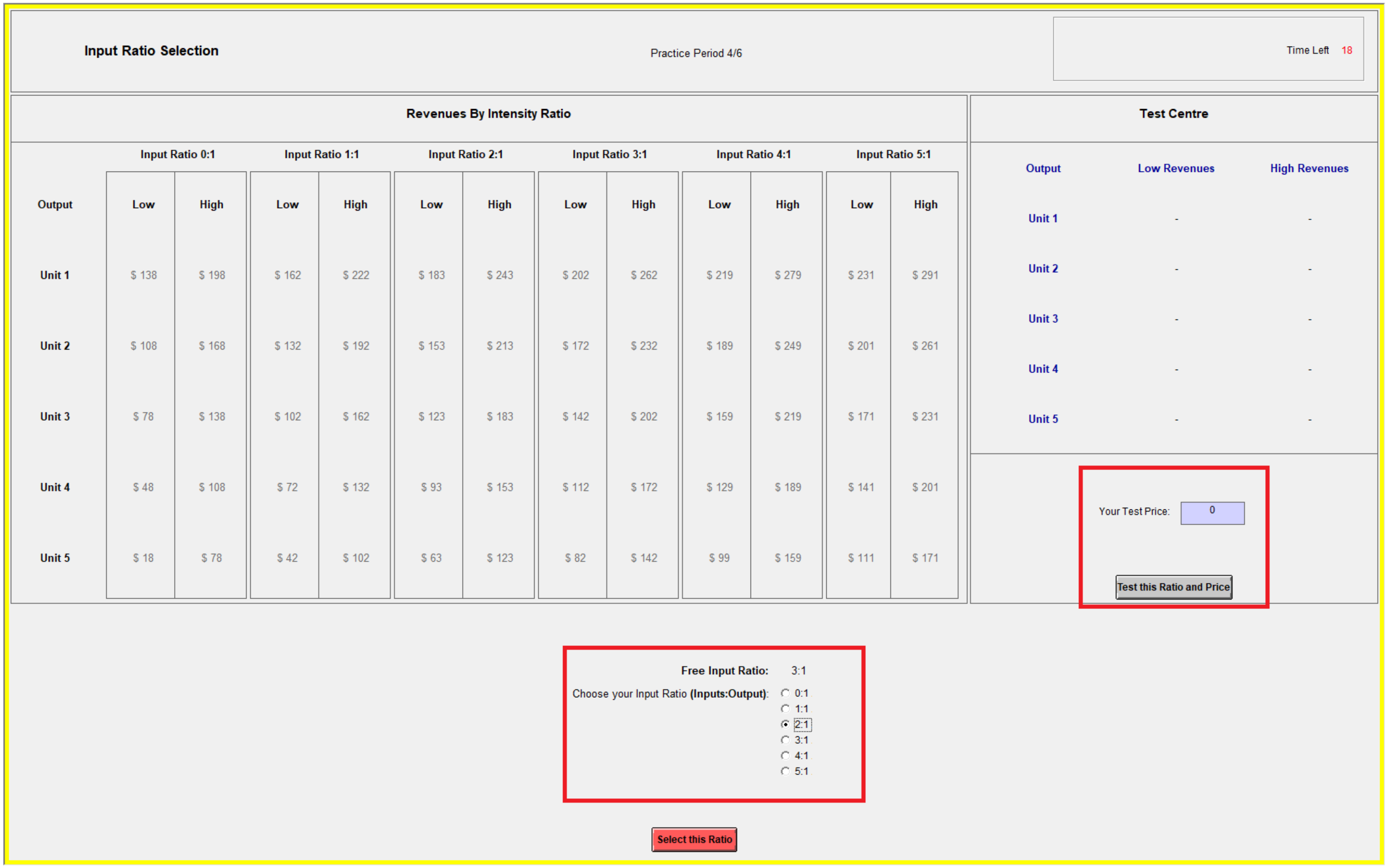
	Input Ratio 0:1		Input Ratio 1:1		Input Ratio 2:1		Input Ratio 3:1		Input Ratio 4:1		Input Ratio 5:1	
Output	Low	High	Low	High	Low	High	Low	High	Low	High	Low	High
Unit 1	$ 142	$ 202	$ 166	$ 226	$ 187	$ 247	$ 206	$ 266	$ 223	$ 283	$ 235	$ 295
Unit 2	$ 112	$ 172	$ 136	$ 196	$ 157	$ 217	$ 176	$ 236	$ 193	$ 253	$ 205	$ 265
Unit 3	$ 82	$ 142	$ 106	$ 166	$ 127	$ 187	$ 146	$ 206	$ 163	$ 223	$ 175	$ 235
Unit 4	$ 52	$ 112	$ 76	$ 136	$ 97	$ 157	$ 116	$ 176	$ 133	$ 193	$ 145	$ 205
Unit 5	$ 22	$ 82	$ 46	$ 106	$ 67	$ 127	$ 86	$ 146	$ 103	$ 163	$ 115	$ 175

Output	Low Revenues	High Revenues
Unit 1	-	-
Unit 2	-	-
Unit 3	-	-
Unit 4	-	-
Unit 5	-	-

If revenues are low, then you would earn $66 for the first unit of output produced, $36 for the second, and so on.

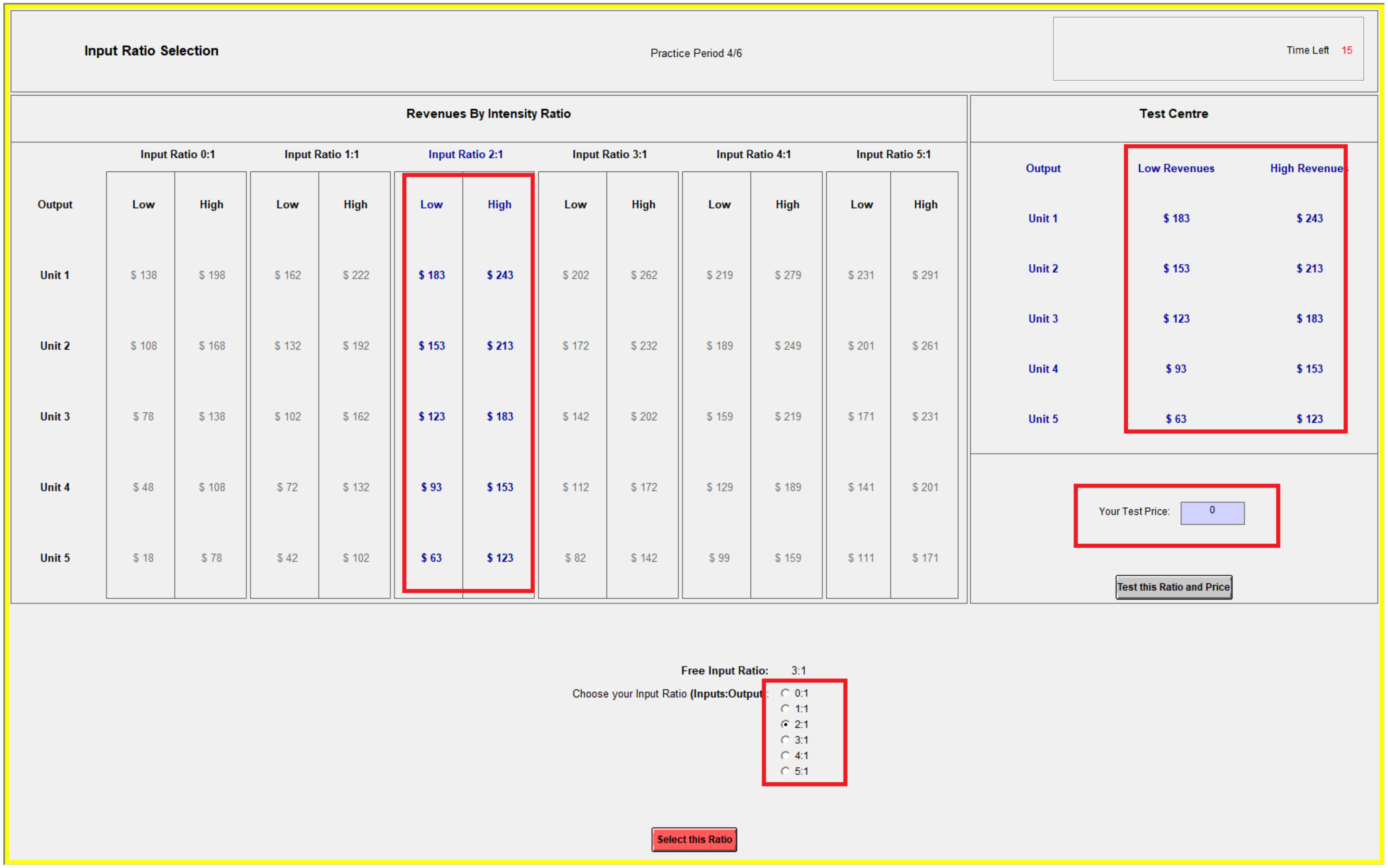


And if revenues were high, you would earn $126 for the first unit of output produced, $96 for the second, and so on.

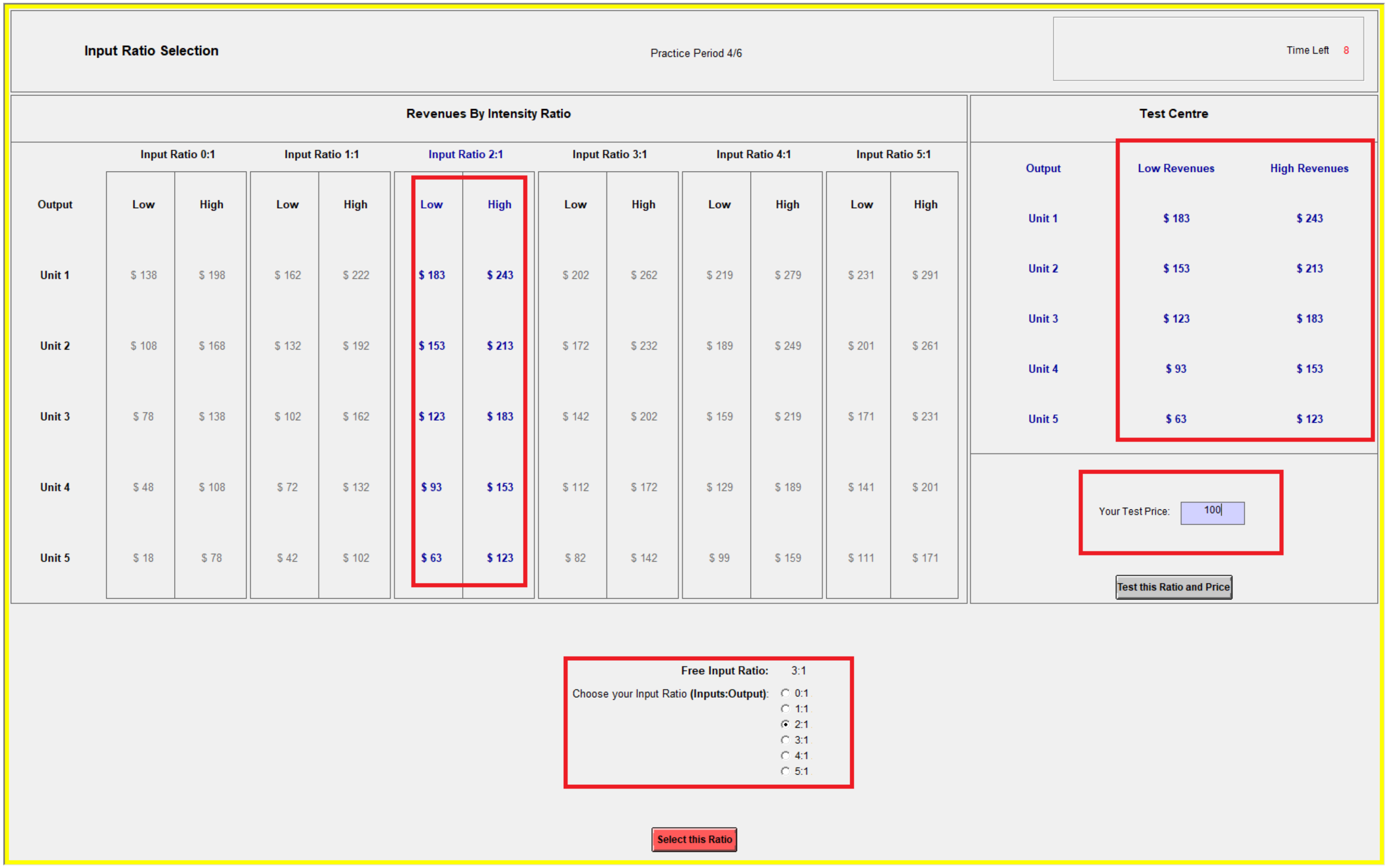


	Input Ratio 0:1		Input Ratio 1:1		Input Ratio 2:1		Input Ratio 3:1		Input Ratio 4:1		Input Ratio 5:1	
Output	Low	High	Low	High	Low	High	Low	High	Low	High	Low	High
Unit 1	$ 138	$ 198	$ 162	$ 222	$ 183	$ 243	$ 202	$ 262	$ 219	$ 279	$ 231	$ 291
Unit 2	$ 108	$ 168	$ 132	$ 192	$ 153	$ 213	$ 172	$ 232	$ 189	$ 249	$ 201	$ 261
Unit 3	$ 78	$ 138	$ 102	$ 162	$ 123	$ 183	$ 142	$ 202	$ 159	$ 219	$ 171	$ 231
Unit 4	$ 48	$ 108	$ 72	$ 132	$ 93	$ 153	$ 112	$ 172	$ 129	$ 189	$ 141	$ 201
Unit 5	$ 18	$ 78	$ 42	$ 102	$ 63	$ 123	$ 82	$ 142	$ 99	$ 159	$ 111	$ 171

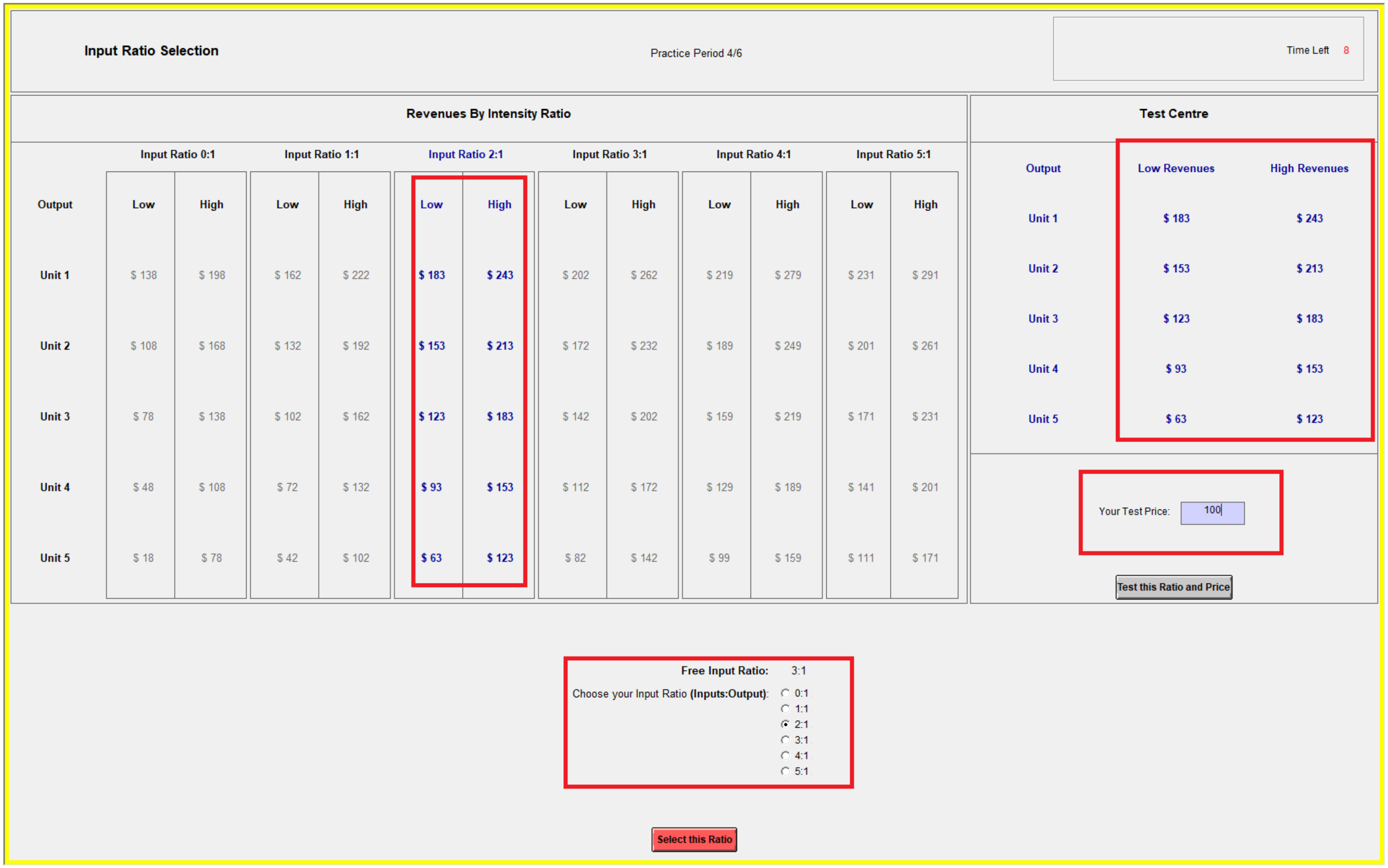
Your final earnings will depend on the price of inputs, as well as your choice of input ratio. Before you select your input ratio, you can try testing input prices to see what your total revenues would be under either the high or low settings. In the test centre, to the right of the screen, you can test a possible input price for the input ratio indicated at the bottom of the screen. Click on a particular input ratio, but don't yet click on "Select this ratio". When you enter a possible input price in the test box, the table presented will calculate the total revenues including this input price. This will allow you to see what your earnings would be like at different input ratios with different possible input prices. Let's see what happens when we select an input ratio of 2:1, when the free input ratio is 3:1.



When the test price is $0, the table in the test centre will display the high and low revenues for your chosen input ratio, just as you see in the Revenues table. Here, for example, if the free input ratio is 3:1 and you select an input ratio of 2:1, then you would earn \$87 for the first unit if revenues are low, then \$57 for the second, and so on. Or, if the revenues were high, you would earn \$147 for the first unit, and then \$117 for the second and so on.



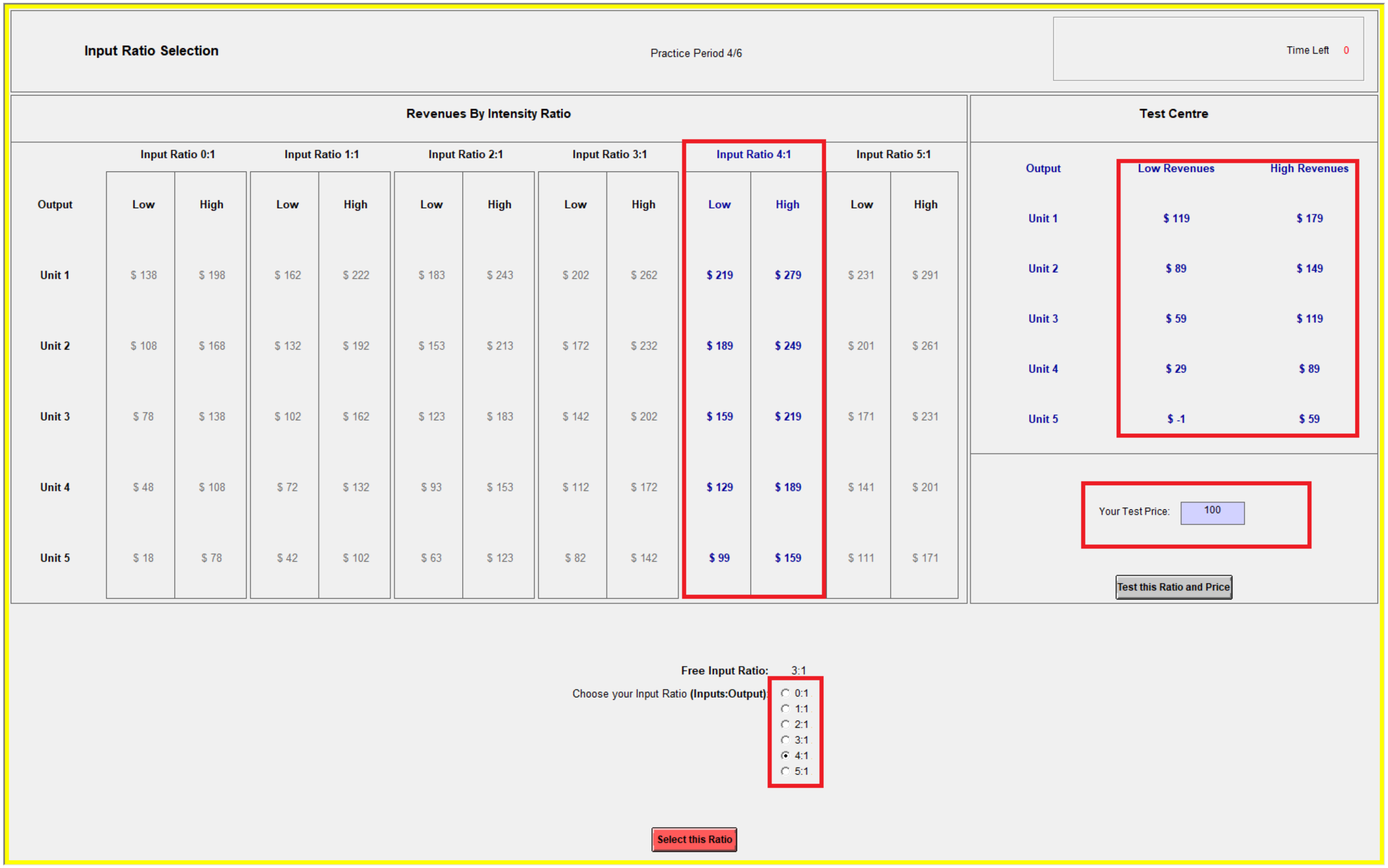
If you enter a test price for inputs, you can see what effect that input price would have on your earnings. For example, suppose that input prices have been around \$100 in recent periods, so you want to see what your earnings would be like if the input price was \$100. You can enter this amount and click on "Test this ratio and price". This will update the revenues columns in the test centre.



	Input Ratio 0:1		Input Ratio 1:1		Input Ratio 2:1		Input Ratio 3:1		Input Ratio 4:1		Input Ratio 5:1	
Output	Low	High	Low	High	Low	High	Low	High	Low	High	Low	High
Unit 1	$ 138	$ 198	$ 162	$ 222	$ 183	$ 243	$ 202	$ 262	$ 219	$ 279	$ 231	$ 291
Unit 2	$ 108	$ 168	$ 132	$ 192	$ 153	$ 213	$ 172	$ 232	$ 189	$ 249	$ 201	$ 261
Unit 3	$ 78	$ 138	$ 102	$ 162	$ 123	$ 183	$ 142	$ 202	$ 159	$ 219	$ 171	$ 231
Unit 4	$ 48	$ 108	$ 72	$ 132	$ 93	$ 153	$ 112	$ 172	$ 129	$ 189	$ 141	$ 201
Unit 5	$ 18	$ 78	$ 42	$ 102	$ 63	$ 123	$ 82	$ 142	$ 99	$ 159	$ 111	$ 171

Output	Low Revenues	High Revenues
Unit 1	$ 183	$ 243
Unit 2	$ 153	$ 213
Unit 3	$ 123	$ 183
Unit 4	$ 93	$ 153
Unit 5	$ 63	$ 123

Because you have selected an input ratio of 2:1, you will generate an input each time you produce a unit of output. So the revenues in the test centre are higher as the calculator adds in the test price for the inputs that you produce and can sell. This means that, if revenues are low, you would earn a total of $187 for the first unit produced when the input price is $100. If the revenues are high, total earnings for your first unit of output produced would be $247, if the input price is $100.



Compare this to the case where you choose an input ratio of 4:1. Now your estimated revenues are lower in the test centre because you have to purchase an input in order to produce your output. If the input price is \$100 again and revenues are low, you would have to spend \$100 to buy an input, then earn \$123 for your first unit of output produced, which means your total earnings would only be \$23. If revenues are high, you would end up earning \$83 for your first unit of output produced, after paying \$100 for an input. As you can see, your choice of input ratio and the input price can have a big impact on your total earnings.

Paid Periods

Now that you have completed the practice periods, we are ready to begin the paid periods of the experiment. The experiment will reset again now and you will be given a one-off endowment of L$250 to get started. Your revenues from producing output will be different from those you saw in the practice periods, and other participants may have different revenue values than you do. For all of the paid periods everyone in the experiment will have a Free Input Ratio of 2:1.

For the first 7 Paid periods the Market will last 3 minutes (180 seconds), after that the Market will last 2 minutes (120 seconds).

The experiment will have a minimum of 10 paid periods. After the tenth period there is a 1 in 6 chance that the experiment will end. If the experiment does not end we will continue for another paid period. At the end of the 11th paid period (if there is one) there will again be a 1 in 6 chance that the experiment will end and hence a 5 in 6 chance that the experiment will continue for another paid period. This process has been programmed into the experiment, and will repeat at the end of each additional period until the experiment ends. Therefore the experiment could end at the end of the 10th period, but it is more likely that the experiment will last longer than that.

End of Experiment

After the last period ends the computer will show you the lab dollar earnings that you accumulated from the first paid period to the last paid period and the conversion to Australian dollars. You will also be shown the results of the individual tasks you completed at the beginning of today's session. When you click the 'OK' button you will be asked to complete a few optional demographic questions. Your earnings today will not be revealed to other participants so shortly after completing the demographic questions you will be called one by one to privately collect your cash. No communication between participants is allowed at any time during the experiment. Please raise your hand at any time if you have any questions.

The first paid period of the experiment will begin after everyone has pressed the 'Ready!' button below.

Figure 61: Notice to Start Paid Periods