Düsseldorfer Institut für Wettbewerbsökonomie

DISCUSSION PAPER

No 100

Do Short-Term Laboratory Experiments Provide Valid Descriptions of Long-Term Economic Interactions? A Study of Cournot Markets

Hans-Theo Normann,
Till Requate,
Israel Waichman

July 2013

HEINRICH HEINE UNIVERSITÄT DÜSSELDORF

d|u|p düsseldorf university press

IMPRINT

DICE DISCUSSION PAPER

Published by

düsseldorf university press (dup) on behalf of
Heinrich-Heine-Universität Düsseldorf, Faculty of Economics,
Düsseldorf Institute for Competition Economics (DICE), Universitätsstraße 1,
40225 Düsseldorf, Germany
www.dice.hhu.de

Editor:

Prof. Dr. Hans-Theo Normann
Düsseldorf Institute for Competition Economics (DICE)
Phone: +49(0) 211-81-15125, e-mail: normann@dice.hhu.de

DICE DISCUSSION PAPER

ISSN 2190-9938 (online) – ISBN 978-3-86304-099-4

Do Short-Term Laboratory Experiments Provide Valid Descriptions of Long-Term Economic Interactions? A Study of Cournot Markets*

Hans-Theo Normann† Till Requate‡ Israel Waichman§

July 2013

Abstract

One key problem regarding the external validity of laboratory experiments is their duration: while economic interactions out in the field are often lengthy processes, typical lab experiments only last for an hour or two. To address this problem for the case of both symmetric and asymmetric Cournot duopoly, we conduct internet treatments lasting more than a month. Subjects make the same number of decisions as in the short-term counterparts, but they decide once a day. We compare these treatments to corresponding standard laboratory treatments and also to short-term internet treatments lasting one hour. We do not observe differences in behavior between the short- and long-term in the symmetric treatments, and only a small difference in the asymmetric treatments. We overall conclude that behavior is not considerably different between the short- and long-term.

JEL Classification: L13, C93, C72, D43, D21

Keywords: internet experiment, Cournot oligopoly, long-term interactions, methodology, internet vs. laboratory experiment

---

*We are grateful to the editor and two anonymous referees for useful comments and suggestions. We are thankful to Dennis Nissen for programming the experiment in Java and also to Markus Karde and Silke Werner for research assistance.

†Duesseldorf Institute for Competition Economics (DICE), University of Duesseldorf, Germany. Email: normann@dice.hhu.de

‡Department of Economics, University of Kiel, Olshausenstr. 40, 24118 Kiel, Germany. Email: requate@economics.uni-kiel.de

§Corresponding author: Department of Economics, University of Heidelberg, Bergheimerstr. 20, 69115, Heidelberg, Germany, Email: waichman@eco.uni-heidelberg.de

1 Introduction

Many economic interactions in the field are long term in nature, whereas typical laboratory experiments only last for an hour or two. For instance, competition between firms may go on for months or even years, and the duration of labor-market interactions or vertical firm relations is frequently measured in decades. By contrast, experiments are usually short. Inspecting the duration of experiments published in the journal *Experimental Economics* between 2006 and 2010 (issues 9-13), we find that the average duration of an experiment is 68.09 minutes, with a minimum duration of 20 minutes and a maximum of 180 minutes.

The discrepancy between the duration of field and lab interactions has given rise to concerns about the external validity of laboratory experiments.[1] In this respect, Gneezy and List (2006) find that in two controlled field experiments the impact of an unexpected increase in wages had only a short-term effect. After about three hours, subjects adjusted their behavior and the effect vanished. This finding is particularly interesting since there are hardly any economics experiments lasting more than three hours. Moreover, in long-term interactions individuals have more time to analyze the situation and to reflect their decisions.

It is therefore surprising that, except for Baik et al. (1999) and Oechssler et al. (2008), no laboratory experiment has been designed to test for the impact of long-term interaction on performance.[2] Conducting a two-player "endogenous-timing" tournament experiment, Baik et al. (1999) find that more time to think led to more rational behavior. In a "mini ultimatum game" Oechssler et al.

[1]Levitt and List (2007) argue that external validity is crucial regarding the relevance of laboratory experiments, whereas Falk and Heckman (2009) and Camerer (2011) contend that external validity is not central. Either way, it is important to know whether long-term interactions constitute a more appropriate way of analyzing economic behavior and whether short-term lab interactions are restrictive.

[2]A notable early exception where duration differs appreciably from the usual length is Hong and Plott (1982). Here subjects agreed to participate in four three hours experimental sessions. Another example is Isaac et al. (1994) who employed a long-term procedure for testing a large group-size effect in a public-good experiment. Finally, in Andreoni and Blanchard (2006), subjects made decisions once a day for a 10 days period over the internet. In these experiments, however, no comparison to a short-term lab experiment was made.

(2008) gave responders the opportunity to revise their decisions (whether or not to reject an offer) after 24 hours. They observed fewer rejections when stakes (represented by lotteries) were high, but they found no differences in rejections under standard ultimatum-game stakes.

Related to the duration issue is the number of repetitions of the stage game in the laboratory. In fact, there is evidence suggesting that the larger the number of rounds in an experiment is, the more subjects will engage in cooperation (see Normann and Wallace, 2012, and the references therein). Recently, Friedman and Oprea (2011) found almost 90 percent cooperation in a prisoner's dilemma with (nearly) continuous time. While this shows that the frequency of interaction may be significant, it does not answer the question of how duration itself affects the course of play in experiments; after all, even these high interaction-frequency experiments do not last longer than the typical lab experiment. Moreover, it is important to investigate whether short- or long-term interactions in experiments are a potentially more appropriate way of testing theories. In theory, the only thing that matters is the distinction between a finite and an infinite number of repetitions, but nothing is said about the time span of these interactions.

The present study investigates the impact of duration in symmetric and asymmetric Cournot duopolies. In particular, we conduct long-term treatments via the internet, and compare them with the corresponding short-term internet counterparts and the standard lab treatments. There are several reasons why we have chosen that framework. First, when one envisages a situation involving long-term economic interactions, competition between firms is among the first examples that come to mind. Second, this game is essentially a social dilemma, and its results could be generalized to apply to many other economic situations. Third, the Cournot game has been experimentally investigated in the laboratory dozens of times, and the results are fairly robust and well-established. Fourth, testing the impact of long-term interaction is especially promising in such a design since there is much room for enhancing collusion. More precisely, while in symmetric duopolies players cooperate to some extent, the average output is still closer to the Cournot-Nash than to the joint-payoff maximizing output (see, e.g., Huck et al., 2004). In asymmetric duopolies players typically fail to cooperate.[3]

One possible explanation for why behavior differs between short- and long-term time spans is that subjects are more prone to "hot" emotional states in short-term settings than in long-term interactions.[4] Researchers in neuroscience find that participation in the ultimatum bargaining game activates regions in the brain known to be involved in negative emotions (e.g., Sanfey et al., 2003; Koenigs and Tranel, 2007, etc.), and this is also true of cooperation experiments (see Rilling et al., 2002, Rilling et al., 2004). These negative emotions (or more generally "visceral factors") "often propel behavior in directions that are different from that dictated by a weighing of the long-term costs and benefits of disparate actions " (Loewenstein, 2000, p.426). For that reason, a cooling-off period is often used in negotiations. Hence, it may be easier to cooperate in the absence of immediate negative emotions such as anger, fear, or a feeling of injustice. Moreover, the possibility to think about the experiment for a few days (and maybe consult others) could impact on cooperation as subjects may realize that there are long-term benefits from collusion.

On the other hand, in the short term subjects may be more prone to punishing defectors (an action conflicting with their self-interest) than in the long run. An example for such an action is "road rage" a short-sighted response on the road that is not in the interest of the angry driver (Loewenstein, 2000). As a result,

[3] In fact, aggregate output may even be above the static Nash level (Mason et al., 1992; Mason and Phillips, 1997).

[4] The idea that different psychological mechanisms are at work in the short term ("hot" emotional states) and in the long run ("cold" emotional states) has been firmly established (e.g., Loewenstein and Schkade, 1999; Loewenstein, 2000, 2005, etc).

emotional behavior in the short term may induce cooperation even in the short run.

Due to the ex-ante ambiguity regarding the impact of a long-term procedure on behavior, we have a two-tailed hypothesis: an experiment that is conducted over a long time span will yield output decisions that are different from the standard short-term laboratory procedures.

The overall findings in our Cournot market treatments are as follows. In the long-term symmetric internet treatments behavior does not differ from the kind observed in their respective short-term lab and short-term internet counterparts. We also find no crucial differences between the two short-term treatments in the lab and via the internet. In the asymmetric case we observe that, due to the behavior of high-cost firms, in the short-term internet treatment subjects play significantly more competitively than in the long-term internet treatment. These difference are, however, quite small in magnitude. Therefore, our overall conclusion is that, in terms of experimental duration, at least in Cournot duopolies outcomes in short-term laboratory settings do not differ from longer term environments.

2 Related Literature

This study compares games of repeated interaction in the short run and in the long run. Such comparisons were, hitherto, only made for tournaments and one-shot games. See Baik et al. (1999) and Oechssler et al. (2008), respectively, as discussed in the introduction.

As an aside to our main research question, we also compare standard laboratory treatments to variants run over the internet. Interestingly, such comparisons have been done mainly on one-shot games and seldom on games of repeated interactions. In what follows, we review repeated-interaction studies which make a comparison between behavior in the lab and via the internet.

Andreoni and Blanchard (2006) conducted a variant of the ultimatum game over the internet. In their study, student participants made one decision a day for a period of 10 sequential days. The authors find that in the internet treatment participants behave remarkably similarly to comparable lab treatments. Egas and Riedl (2008) conducted random-matching public good game treatments (mainly with punishment) over the internet. They find that the behavior in their internet experiment is qualitatively comparable with that of students from comparable laboratory experiments. Chesney et al. (2009) also conducted several fixed-matching experiments (public good game, minimum effort game, and guessing game) in the second life (SL) virtual world and compared their results to those from the literature conducted in the lab.[5] In the public good game, they find that SL subjects contribute slightly more than lab students. In the minimum effort game, they find a higher mean behavior than in previous studies, while in the guessing game they observe a higher average guess than in previous studies. Nevertheless, all these differences are rather small. Suri and Watts (2011) conducted different variations of public-good games arranged on one of five network topologies and find their results to be overall in line with a related study by Cassar (2007).

To the best of our knowledge the only experimental study that tests for differences between behavior in the lab vs. via the internet using the same randomized subject pool in repeated interactions is Schmelz and Ziegelmeyer (2012). These authors study a principle-agent game where the principle chooses enforcement and the agent chooses an effort level. They find that negative reactions to enforcement are larger on average in the laboratory than over the internet, but these differences vanishes over time.

To sum up, the overall picture is that behavior in internet experiments of

[5]Chesney et al. (2009) also conducted one-shot ultimatum- and dictator-game treatments.

repeated interactions corresponds closely to their laboratory counterparts. This observation contrasts with most findings in one-shot interaction studies comparing behavior in the lab and internet environments (Bellemare and Kröger, 2007, Charness et al., 2007, Fiedler and Haruvy, 2009, Füllbrunn et al., 2011, and Hergueux and Jacquemet, 2012).[6]

3 Experimental Design

Following the design by Huck et al. (2004) we conducted six treatments (summarized in Table 1 and outlined in detail below) all of them implementing a simple Cournot duopoly game.

- Table 1 about here -

In all treatments, subjects represented firms producing a homogeneous good. Firms face the following linear demand function:

$$P(Q) = max\left\{100 - Q, 0\right\},$$

where $Q = q_1 + q_2$ is the total quantity produced in the market. In the symmetric treatments the cost function for each firm is given by

$$C(q) = q.$$

Under this setting, the (unique) Cournot-Nash equilibrium is given by $(q_1^N, q_2^N) = (33, 33)$ so that industry output is $Q_s^N = 66$. Moreover, the joint-payoff maximizing quantity is $Q_s^{JPM} = 49.5$, while the competitive (Walrasian) equilibrium quantity is $Q_s^C = 99$.

[6]Notably, Horton et al. (2011) find no difference in behavior in a prisoner's dilemma game between lab and internet treatments.

The asymmetric treatments are identical to their symmetric counterparts except that the two firms' costs differ and are given by

$$C_1(q) = q$$
$$C_2(q) = 7.5q.$$

In this case, the Cournot-Nash equilibrium is given by $(q_1^N, q_2^N) = (35.16, 28.66)$, so that industry output is now $Q_{as}^N = 63.83$. Note that, when playing equilibrium actions, the profit ratio between high- and low-cost firms is exactly 1.5.

In both the symmetric and asymmetric treatments, subjects could select integer quantities between 0 and 100 (we allowed integer outputs only so as to prevent subjects from signaling their identity by using decimals). Subjects were allowed to use a profit calculator to test the potential impact of various own and other firm's decisions before committing to the payoff relevant output decision. The profit calculator reduces dependency on a subject's cognitive abilities. Using this device ensures that differences in performance are not due to lack of time for calculations.[7] After each round, the subjects were given information about their own choice, the other firm's choice, and their payoffs in the current and previous rounds of the game. Each treatment lasted exactly 25 rounds, and the number of rounds were made common knowledge in the instructions.

We deliberately decided against conducting a long-term treatment in the laboratory where subjects enter the lab once a day, because it does not add further control in comparison with our long-term internet treatment. Even worse, the fact that subjects repeatedly meet in the lab may facilitate collusion. On the other hand, short-term lab experiments may not be entirely comparable

[7] We did not include a 'best response' button in the calculator as this may bias behavior toward playing this strategy (Requate and Waichman, 2011). This latter study also shows that in Cournot experiments the alternative method for presenting information, a "payoff matrix", produces indistinguishable results to a payoff calculator.

to a short-term internet experiment. Subjects participating in a short-term internet experiment often do not read the instructions beforehand, invest less time in the task, or make decisions faster than in a respective lab experiment (Anderhub et al., 2001, Schulte-Mecklenbeck and Huber, 2003, and Hergueux and Jacquemet, 2012, respectively). In this respect, and since the long-term internet treatment allows subjects to log in at that time of the day when they can concentrate on the experiment (as they would in the laboratory), a direct comparison between a short-term lab and a long-term internet experiment may be more appropriate for testing for the "pure duration effect" than a chain of comparisons (short-term lab vs. short-term internet vs. long-term internet).[8]

4 Procedures

Designing a long-term experiment poses various methodological issues. The main challenge is to conduct the experiment in a way that parallels the standard laboratory procedure as closely as possible, while at the same time dealing efficiently with the problem of subjects tending to opt out when required to take repeated decisions over a relatively long time span. In fact, we are not aware of any other long-term experiment involving as many as 25 repeated choices.

In the following we describe the procedure we chose to offset the tendency of subjects to skip decision rounds, while at the same time displaying sufficient comparability with standard short-term experiments. We recruited a total of 296 students from different departments of Kiel University to participate in the experiment in three cohorts. Subjects of the first two cohorts were randomly divided into two sub-groups, one participating in a standard 60-minute lab

[8]As pointed out by a referee, there are, however, aspects that cannot be controlled when directly comparing short-term lab with long-term internet treatments. For example, subjects in the lab may have a high tendency to engage in some activity (e.g., Lei et al., 2001), which may be different if subjects participate from home.

experiment (referred to as short-term treatment), the other participating in an internet version of the otherwise identical experiment with a duration of 25 working days (long-term treatment). A third cohort of 82 subjects was later recruited to participate in the short-term internet treatments.

During the recruitment phase, subjects of cohorts 1 and 2 were informed that a random lottery would determine whether they would participate in the short-term lab or in the long-term internet treatment. After random assignments, subjects were informed via email as to which group they had been assigned to and were asked to confirm their participation. The first cohort participated in the symmetric-, the second cohort in the asymmetric-cost treatment.

The experiment was programmed in Java with identical programs and frames for both the short- and the long-term treatments. Long-term experiments involve several methodological challenges, not least of which is that subjects may opt out of the experiment. Previous experiments lasting more than one week (Isaac et al., 1994, Andreoni and Blanchard, 2006) have used default rules to specify an action when a subject did not make a choice. This approach might not only distort the results, it could in fact change the game (by changing the number of active players in a group). The challenge is to limit the impact of the default rule.[9] Our design addresses this challenge in three different ways. First, we reduce the cost of participation by programming an experiment that can be operated from any computer, tablet, or smart phone, virtually anywhere (and in the long-term internet treatment) at any time. Second, we only allow two (non-subsequent) default decisions per subject. Third, we monitor whether subjects are making their decisions and we notify them by email, text messages, and even phone calls that we will terminate their participation if they do not make the requisite decisions.

[9] For instance, (Isaac et al., 1994, Figure 10, p.18) observe much higher default rates ranging between 20% and 55% in rounds 6 to 10 of their public-good experiment. Andreoni and Blanchard (2006) observe 12.5% and 8% defaults in the first and second sessions, respectively.

To ensure comparability between the short- and long-term versions, we followed the same procedure in all treatments. For example, in the short-term lab treatment we did not read the instructions aloud or answer any questions publicly. In addition, as it is not possible to pay subjects immediately after the last round in the long-term treatments (as is typically done in the lab), subjects could collect their earnings in both the short- and long-term treatments as of one day after completing the respective experiment.[10] In the symmetric-cost treatments average payoffs were €13.51, €11.63, and €27.30 in the short-term lab, short-term internet, and long-term internet treatments, respectively. In the asymmetric-cost treatments, payoffs were €14.23, €13.01, and €28.35 for the low-cost and €10.11, €10.39, and €20.58 for the high-cost firms in the short-term lab, short-term internet, and long-term internet treatments, respectively.[11]

Short-term lab procedure

In the case of the short-term laboratory treatments, the subjects were provided with individual usernames and passwords once they had entered the computer laboratory. They had 10 minutes to read the instructions. Questions could only be asked privately, and subjects could collect their earnings in cash as of the following working day (as in the long-term treatments).

Long-term internet procedure

On the first day of the long-term internet treatments, the subjects were provided with individual usernames and passwords. It was only possible to log in to the experiment on a working day (Monday to Friday), when subjects could log in

[10] Delay in payment in the long-term treatments may affect subjects' behavior in terms of the discount rate as well as their beliefs regarding the certainty of payment. By delaying the payment in the short-term treatments, we control for the latter. Moreover, a one day delay in payment reduces the impact of the discount rate if a subject's discounting is hyperbolic (Frederick et al., 2002).

[11] To promote saliency, the exchange rate in the long-term treatments (25 working days) was twice as high as in the short-term treatments (lasting approximately one hour).

to the system as often as they wanted. Once subjects were logged in, they could read the instructions, use the profit calculator, observe the outputs of the previous rounds, and make their output decisions once a day (between midnight and 11.59pm). Feedback on the outcome of the previous round was available from midnight on the following working day. Subjects who had not entered a quantity by 7pm were sent a reminder by email, followed by another reminder later via a cell phone text message. In the whole period of 25 working days, the subjects were allowed to skip a total of two decisions (as long as they were not successive). Subjects were dismissed from the experiment in two cases: (i) if they did not enter an output on the first day of the experiment;[12] ii) if they failed to enter more than two decisions.[13] In those cases where subjects did not make decisions, the output decision from the previous round was registered. Questions in the long-term treatments could be asked via email or phone.

Short-term internet procedure

The main challenge in conducting the short-term internet treatments was to ensure that all subjects logged in precisely at the time of the experiment and stayed online during the whole experiment. Another difficulty was that in the laboratory the experimenter can check the compatibility of the hardware and software before the start of the experiment (including maintaining a stable connection between the control computer and its clients), but in online experiments various technical problems could occur (e.g., problems with the program, computer, internet connection). This was not so crucial in long-term procedures as subjects had enough time to find a solution (they could contact the experimenters, log in from a different device, change internet connection, etc.). In

[12]Four and two subjects were excluded after not entering an output on the first day of the symmetric and asymmetric treatments, respectively.

[13]In the symmetric long-term treatment, three pairs were excluded from the experiment after 19, 22, and 23 days, respectively, and in the asymmetric treatment, two pairs were dismissed after 23 and 25(!) days.

short-term internet procedures the experimenter may be forced to dismiss a subject at the beginning or even during the experiment since, as in standard laboratory procedures, the next round starts only once all subjects have made their decisions.[14]

Consequently, we developed a three-part procedure to overcome these difficulties: First, all our recruits were asked to provide their cell phone numbers. We also provided subjects with three telephone numbers for technical support or questions. Second, on the morning of the experiment, we sent subjects their respective user names and passwords, but they could not log in to the experiment until the experiment started (because in short-term experiments, in contrast to long-terms experiments, subjects are not supposed to read the instructions before the start of the experiment). Finally, to make sure that all subjects logged in on time, we sent them a reminder by text message about an hour prior to the start of the experiment.

5 Results

This section centers on the issue of whether there are significant differences between the short- and the long-term treatments regarding both market (aggregate) performance and individual behavior. We follow the standard procedure by first investigating the output levels in each market averaged over time. Then we take a closer look at individual firm behavior. At the end of the section we report the default rates in the different treatments.

5.1 Aggregate behavior

Table 2 presents the summary statistics of the quantities selected in each market averaged over rounds 3 to 23 (in order to exclude possible beginning and end

[14]Three pairs were dismissed from both the symmetric and asymmetric short-term internet treatments.

effects), and over rounds 11 to 23. We report averages across and medians in rounds 3 to 23 (in order to exclude possible beginning and end effects) and across rounds 11 to 23 (to allow for learning).[15] We note that averages are close to but below the (static) Nash prediction in the symmetric treatments. In the asymmetric treatments, aggregate output is also close to the Nash level but here it is above Nash. We conclude that our results are consistent with previous Cournot experiments.

We illustrate our results using two figures. Figure 1 introduces a box plot of the output of each of the different duopoly markets averaged over these rounds (11-23). This figure indicates that the median behavior is close to the Cournot-Nash equilibrium in all (symmetric and asymmetric) treatments. Notably, in the symmetric treatments outputs are not dispersed around the Cournot-Nash output but are skewed toward the collusive output. By contrast, in the asymmetric treatments output either dispersed around the Nash output (in the long-term internet treatment), or skewed toward competitive outcome (in the short-term lab and internet treatments). Figure 2 illustrates the evolution of mean quantities per round throughout the course of the sessions. Inspection of Figure 2(a) suggests that there is no considerable difference in behavior among the symmetric treatments in the second-half of the experiment.[16] In the asymmetric treatments, it seems that average outputs in short-term internet treatments are slightly above the corresponding short-term lab and long-term internet treatments.

We now move toward analyzing and testing for treatment effects. Table 3

[15]For the rest of the analysis, we focus on data from rounds 11-23 to account for behavior after a possible adjustment period where subjects learn to play the experiment, but without the end effect in the last two rounds. It is especially justified as, in the short-term internet procedures, it may be that subjects do not read the instructions carefully or make decisions without focusing on the experiment ("as they may be busy with other things at the same time such as watching television and eating", see Charness et al., 2007, p.101).

[16]Once regressing the market quantities on "round" we find that the time coefficient is insignificant ($p > 0.10$) in all treatments except for the symmetric short-term internet treatment. However, even in this treatment the "round" coefficient becomes insignificant by the fourth round.

presents p-values of the pairwise statistical tests between the treatments. We start with the symmetric treatments, then follow the asymmetric treatments.

- Table 2 about here -

- Table 3 about here -

- Figure 1 about here -

- Figure 2 about here -

5.1.1 Symmetric treatments

Table 2(a) and Figure 1(a) indicate that the median quantities across markets in the three treatments are not substantially different from each other. Indeed, using a two-sided robust rank-order test (F-P test, following Fligner and Policello, 1981),[17] we do not find any significant differences between any of the treatments when comparing the markets averaged over rounds 11 to 23 pairwise, counting each market as one observation.[18] Moreover, Figure 1(a) indicates that the aggregate outputs in the short-term treatments are more dispersed than the outputs in the long-term internet treatment. However, these differences are not significant as the F-P tests show. Figure 2(a), too, does not indicate differences for the symmetric treatments.

Following Potters and Suetens (2009), we also looked at attempts (successful or not) to maximize joint payoffs. A pair of strategies is defined as joint-payoff maximizing (JPM) when both subjects choose an output of 24 or 25 (since the

[17] The robust rank-order test is less sensitive to changes in sample sizes and distributional assumptions than the Wilcoxon-Mann-Whitney test (see Feltovich, 2003).

[18] Overall, there are 15, 19, and 30 independent markets in the short-term lab, short-term internet, and long-term internet treatments, respectively.

exact JPM individual output is 24.75). We find that 26%, 21%, and 30% of the subjects made such choices in the short-term lab, short-term internet and long-term internet treatments, respectively. These differences are not significant ($p = 0.93$ using a Fisher exact test).

Result 1 [Symmetric firms]: *Aggregate performances in the short- and long-term treatments are indistinguishable.*

5.1.2 Asymmetric treatments

Table 2(a) and Figures 1(b) and 2(b) suggest that in the asymmetric-cost short-term treatments subjects play, on average, slightly more competitively than predicted by the asymmetric Cournot model, while in the corresponding long-term treatment quantities spread around the Cournot levels. Testing for differences in pairwise comparisons (using F-P tests), we only find a significant difference between the short- and the long-term internet treatment. Additionally, Figure 2(b) indicates that aggregate quantities do not change over time. This is essentially the same as in the symmetric treatment.

Result 2a [Asymmetric firms]: *The short-term internet treatment is slightly more competitive than in the long-term internet treatment. However, we do not find a significant difference when comparing short-term lab and long-term internet treatments, or when comparing short-term lab and internet treatments.*

Table 4 presents the summary statistics of the quantities chosen by high- and low-cost firms in the asymmetric treatments. Figure 3 shows the evolution over time of the average quantities chosen by the high- and low-cost firms in the different treatments. F-P tests indicate no significant differences for the low-cost firms. As to the high-cost firms, we find a significant difference between for both the short-term treatments and the long-term internet treatment. We observe no difference between the short-term lab and short-term internet treatments. We note that these difference are not substantial, though. We can hence formulate the following result:

- Table 4 about here -

- Figure 3 about here -

Result 2b [Asymmetric firms]: *We do not observe significant differences between the treatments regarding output decisions of low-cost firms. However, high-cost firms select significantly lower outputs in the long-term than in the short-term treatments.*

This last result indicates that the added time in the long-term treatment induces high-cost firms to play less competitively than in the short-term. It suggests that in interactions between heterogeneous parties a “cooling- off period” between rounds may result in outputs that are closer to the static Cournot-Nash predictions than when interactions take place within a short continuous process.

5.2 Individual behavior

Now we look at the individual strategies. Taking into account the fact that subjects were provided with a profit calculator and also received information about aggregate output selected in the previous round, they could easily calculate their best response to their competitors' outputs, assuming that the competitors do not retract their decision. Formally, the best response is defined as $r_i^t(\sum_{j\neq i} q_j^{t-1}) = \arg\max_{q_i}\{(P(q_i + \sum_{j\neq i} q_j^{t-1}) - c_i)q_i\}$. Besides ‘best response’, a subject could also play an ‘imitation’ strategy, which means that a subject tries to select the same output as its competitor. Accordingly, and following Huck et al. (1999), we estimate the following 'output adjustment' model using OLS (with robust standard errors at the group level):

$$
\begin{aligned}
q_i^t - q_i^{t-1} &= \beta_0 + \beta_1(r_i^{t-1} - q_i^{t-1}) + \beta_2(q_j^{t-1} - q_i^{t-1}) \\
&\quad + \beta_3 \left[(r_i^{t-1} - q_i^{t-1}) \times (Long\_term)\right] + \\
&\quad + \beta_4 \left[(q_j^{t-1} - q_i^{t-1}) \times (Long\_term)\right] \\
&\quad + \beta_5 \left[(r_i^{t-1} - q_i^{t-1}) \times (Lab)\right] + \\
&\quad + \beta_6 \left[(q_j^{t-1} - q_i^{t-1}) \times (Lab)\right] \\
&\quad + \beta_7\, Lab + \beta_8\, (Long\_term) + \beta_9\, JPM \\
&\quad + \beta_{10}\, Spiteful + \beta_{11}\, (Low\_cost) + \beta_{12}\, Male \\
&\quad + \sum_t \gamma_t Round_t
\end{aligned}
$$

where q_i^t is the output selected by subject i in round t, the variable r_i^{t-1} denotes subject i's best response to the output in the previous round selected by the competitor, and q_j^{t-1} denotes the output in the previous round selected by the competitor. Hence, $(r_i^{t-1} - q_i^{t-1})$ and $(q_j^{t-1} - q_i^{t-1})$ indicate best response and imitation output adjustments, respectively.

Lab and *Long-term* are dummy variables denoting short-term laboratory and long-term internet treatments, respectively. Furthermore, the dummy variable *JPM* indicates that a competitor's output is equal to either 24 or 25 at time $t-1$. It is only used in the symmetric treatments estimations, as there is no clear collusive focal point in the asymmetric treatments. *Spiteful* is a dummy variable denoting any rival's output decision in the interval [50,100]. We define an action as "spiteful" if it hurts both the competitor and also the initiator.[19]

[19] A possible reason for acting spitefully may be that subjects are concerned about relative payoffs. One is willing to hurt oneself when others are hurt even more (see Hamilton, 1970, and Bosch-Doménech and Vriend, 2003 in the context of Cournot oligopoly). Indeed, this conjecture is supported by the data. We find that 75%, and 65% of the spiteful decisions in the short-term lab and long-term internet treatments, respectively, result in an output of 50

In other words, a quantity from this interval is never a best response to any positive output selected by the competitor. We added dummies for JPM and spiteful outputs since one hypothesis is that "hot" emotions may more strongly affect behavior in the short-term than in the long-term treatment. Particularly noteworthy are the responses to "collusive" (JPM) and "spiteful" actions taken by the respective competitors. The dummy *Low_cost* in the asymmetric treatment estimations indicates when a subject is a low-cost type (with a marginal cost of 1), and *Male* is a dummy for sex (0 = female, 1 = male). Finally, we added dummy variables for each round. Note that for subjects playing strictly myopic best response strategies ('imitation'), we should have $\beta_1 = 1$ ($\beta_2 = 1$), while the other coefficients should be equal to zero. The specifications and estimation results are given in Table 5.

- Table 5 about here -

Table 5 indicates several interesting findings. First, in line with Huck et al. (1999), a (myopic) best response strategy and imitation explain output adjustment in the symmetric and asymmetric treatments. Moreover, in the symmetric treatments, the JPM output chosen by the competitor in the previous round decreases own output in the current round. This indicates that subjects reciprocate such a gesture (instead of trying to take advantage of the situation by choosing the best response quantity of 37). Under asymmetry, we find that the coefficient on the interaction between 'imitation' and 'long-term' has an effect that runs the opposite to the general impact of imitation. Thus, the impact of imitation in the asymmetric long-term treatment is reduced. This explains why the aggregate outputs in the long-term treatment are dispersed closer to the Nash equilibrium than in the more competitive short-term treat-

units (but only 23% of the spiteful decisions in the short-term internet treatments). At this output the payoff of the competitor has to be smaller or equal to that of the initiator.

ments. The interaction coefficients between 'imitation' and 'lab' and between 'best response' and 'lab', respectively, also have effects that run the opposite to the general impact of imitation and best response. The fact that the interaction coefficient between 'best response' and 'lab' is negative (and significant), while this coefficient between 'best response' and 'long-term' is insignificant could explain why outputs are dispersed closer around the Cournot-Nash outcome in the long-term internet than in the short-term lab treatment (see Figure 1(b)). We also find that output adjustment is increasing when a subject is of a low-cost type. This makes sense as the equilibrium output of a low-cost firm is larger than of a high-cost firm. We summarize this observations as the following result.

Result 3 [Individual output strategies]: *Output adjustment is affected by both best response and imitation strategies in both symmetric and asymmetric treatments. In the symmetric treatment, we find that if one player chooses the JPM output, the other player will follow by further reducing output. In the asymmetric treatment, we find that low-cost subjects increase their outputs over time, while in the long term output adjustment is made less of imitation and more of best response.*

Finally, we report on the default rates in the long-term internet experiment.[20] This is important in order to validate our experimental procedure and findings. We observe a total of 42 and 65 default decisions (2.8%, and 3.6% of all output decisions) in the long-term symmetric and asymmetric treatments, respectively. These numbers are far below the possible maximum of two defaults per subjects (8% of all decisions). Using a Fisher exact test we do not find a significant differ-

[20] As pointed out by a referee, the mere existence of a particular default rule may influence the behavior of subjects in the game. However, in our experiment we only allowed for very few default decisions per subject (two decisions). Moreover, as our default rule was specified neither with respect to the equilibrium predictions nor to the benchmark outputs (it is the subject's output in the previous round), it is not likely to bias behavior toward the equilibrium or benchmark outputs.

ence in default rates in the different working days ($p = 0.53$).[21] We also do not observe a time trend (neither increasing nor decreasing over time) in the number of defaults.[22] In addition, we do not observe that output varies according to the weekday in the symmetric and asymmetric treatment, respectively.[23]

6 Conclusions

Do short-term lab experiments provide valid descriptions of long-term economic interactions? To address this methodologically important question we compare standard Cournot competition laboratory treatments with 25 rounds lasting one hour to treatments with the same number of rounds but lasting for 25 working days. As an intermediate treatment, we also conduct short-term internet treatments with the same number of rounds and duration as in the lab.

Our ex-ante conjecture was that output decisions in the long term may differ from those in the short term mainly because the influence of immediate negative emotions is reduced. However, in the symmetric treatments, we did not find any significant differences in overall performance measured by industry output between the short-term lab, short-term internet, and long-term internet treatments. Although we discovered some significant differences between the asymmetric treatments, these are overall quite small in magnitude. Our findings can thus be considered good news for research in experimental economics since they indicate that short-term laboratory settings, at least with respect to Cournot competition, reflect the behavior in long-term repeated relationships quite well.

[21] We observe that on Thursdays of the long-term symmetric (asymmetric) treatment there are significantly more (less) defaults than on the other days. We observe no difference between any of the other days in each treatment, respectively.

[22] We checked this by regressing the variable "round" on "default" and also comparing defaults in rounds 3-10 and 11-23 using an F-P test.

[23] Using a one-way Anova to compare the mean quantities according to the day, we do not find differences ($p = 0.63$ and $p = 0.48$ in the symmetric and asymmetric treatments, respectively.)

Of course this experiment can only be considered as a first step to investigating whether short-term laboratory experiments adequately reflect behavior in long-term relationships. The outcomes in Cournot experiments are by and large clustered around the static equilibrium predictions. It would be desirable to investigate whether our findings on short- vs. long-term procedures generalize to other types of economic relationships, particularly to experiments where behavior is likely to deviate from the game theoretic predictions (e.g., public good game).[24]

References

Anderhub, V., Müller, R., and Schmidt, C. (2001). Design and evaluation of an economic experiment via the internet. *Journal of Economic Behavior and Organization*, 46:227–247.

Andreoni, J. and Blanchard, E. (2006). Testing subgame perfection apart from fairness in ultimatum games. *Experimental Economics*, 9:307–321.

Baik, K. H., Cherry, T., Kroll, S., and Shogren, J. (1999). Endogenous timing in a gaming tournament. *Theory and Decision*, 47(1):1–21.

Bellemare, C. and Kröger, S. (2007). On representative social capital. *European Economic Review*, 51:183–202.

Bosch-Doménech, A. and Vriend, N. J. (2003). Imitation of successful behavior in Cournot markets. *The Economic Journal*, 113:495–524.

Camerer, C. F. (2011). The promise and success of lab field generalizability in experimental economics: A critical reply to Levitt and List. mimeo.

[24]Regarding this last point, Andreoni and Blanchard (2006) who conducted a 10-day ultimatum game experiment concluded that "subjects in our game behaved remarkably similarly to participants of 10-period games played in a single lab sitting." [p.320] This is particularly interesting since it is well established that behavior in an ultimatum game considerably diverges from the subgame perfect equilibrium.

Cassar, A. (2007). Coordination and cooperation in local, random and small world networks: Experimental evidence. *Games and Economic Behavior*, 58:209–230.

Charness, G., Haruvy, E., and Sonsino, D. (2007). Social distance and reciprocity: An internet experiment. *Journal of Economic Behavior and Organization*, 63:88–103.

Chesney, T., Chuah, S.-H., and Hoffmann, R. (2009). Virtual world experimentation: An exploratory study. *Journal of Economic Behavior and Organization*, 72:618–635.

Egas, M. and Riedl, A. (2008). The economics of altruistic punishment and the maintenance of cooperation. *Proceedings of the Royal Society B*, 275:871–878.

Falk, A. and Heckman, J. J. (2009). Lab experiments are a major source of knowledge in the social sciences. *Science*, 326:535–538.

Feltovich, N. (2003). Nonparametric tests of differences in medians: Comparison of the Wilcoxon-Mann-Whitney and robust rank-order tests. *Experimental Economics*, 6:273–297.

Fiedler, M. and Haruvy, E. (2009). The lab versus the virtual lab and virtual field–an experimental investigation of trust games with communication. *Journal of Economic Behavior and Organization*, 72(2):716–724.

Fligner, M. A. and Policello, G. E. (1981). Robust rank procedures for the Behrens-Fisher problem. *Journal of the American Statistical Association*, 76:162–168.

Frederick, S., Loewenstein, G., and O'Donoghue, T. (2002). Time discounting and time preference: A critical review. *Journal of Economic Literature*, 40:351–401.

Friedman, D. and Oprea, R. (2011). A continuous dilemma. *American Economic Review*.

Füllbrunn, S., Richwien, K., and Sadrieh, A. (2011). Trust and trustworthiness in anonymous virtual worlds. *Journal of Media Economics*, 24:48–63.

Gneezy, U. and List, J. A. (2006). Putting behavioral economics to work: Testing for gift exchange in labor markets using field experiments. *Econometrica*, 74:1365–1384.

Hamilton, W. D. (1970). Selfish and spiteful behavior in an evolutionary model. *Nature*, 228:1218–1225.

Hergueux, J. and Jacquemet, N. (2012). Social preferences in the online laboratory : A randomized experiment. CES working papers 2012.70.

Hong, J. T. and Plott, C. R. (1982). Rate filing policies for inland water transportation: An experimental approach. *Bell Journal of Economics*, 13:1–19.

Horton, J. J., Rand, D. G., and Zeckhauser, R. J. (2011). The online laboratory: conducting experiments in a real labor market. *Experimental Economics*, 14:399–425.

Huck, S., Normann, H.-T., and Oechssler, J. (1999). Learning in Cournot oligopoly - an experiment. *The Economic Journal*, 109:80–95.

Huck, S., Normann, H.-T., and Oechssler, J. (2004). Two are few and four are many: Number effects in experimental oligopolies. *Journal of Economic Behavior and Organization*, 53:435–446.

Isaac, R. M., Walker, J. M., and Williams, A. W. (1994). Group size and the voluntary provision of public goods : Experimental evidence utilizing large groups. *Journal of Public Economics*, 54:1–36.

Koenigs, M. and Tranel, D. (2007). Irrational economic decision making after ventromedial prefrontal damage: Evidence from the ultimatum game. *Journal of Neuroscience*, 27:951–956.

Lei, V., Noussair, C. N., and Plott, C. R. (2001). Nonspeculative bubbles in experimental asset markets: Lack of common knowledge of rationality vs. actual irrationality. *Econometrica*, 69:831–859.

Levitt, S. and List, J. A. (2007). What do laboratory experiments measuring social preferences reveal about the real world. *Journal of Economic Perspectives*, 21:153–174.

Loewenstein, G. (2000). Emotions in economic theory and economic behavior. *American Economic Review*, 90:426–432.

Loewenstein, G. (2005). Hot cold empathy gaps and medical decision making. *Health Psychology*, 24:S49–S56.

Loewenstein, G. and Schkade, D. (1999). Wouldnt it be nice? Predicting future feelings. In Kahneman, D., Diener, E., and Schwarz, N., editors, *Well - Being: The Foundations of Hedonic Psychology*, pages 85–105. Russell Sage Foundation Press, New York.

Mason, C. F. and Phillips, O. R. (1997). Information and cost asymmetry in experimental duopoly markets. *Review of Economics and Statistics*, 79:290–299.

Mason, C. F., Phillips, O. R., and Nowell, C. (1992). Duopoly behavior in asymmetric markets: An experimental evaluation. *Review of Economics and Statistics*, 74:662–670.

Normann, H.-T. and Wallace, B. (2012). The impact of the termination rule in a prisoners dilemma experiment. *International Journal of Game Theory*, 41:707–718.

Oechssler, J., Roider, A., and Schmitz, P. (2008). Cooling-off in negotiations - does it work? Technical Report 6807.

Potters, J. and Suetens, S. (2009). Cooperation in experimental games of strategic complements and substitutes. *Review of Economic Studies*, 76:1125–1147.

Requate, T. and Waichman, I. (2011). "A profit table or a profit calculator?" A note on the design of Cournot oligopoly experiments. *Experimental Economics*, 14:36–46.

Rilling, J. K., Gutman, D. A., Zeh, T. R., Pagnoni, G., Berns, G. S., and D, K. C. (2002). A neural basis for social cooperation. *Neuron*, 35:395–405.

Rilling, J. K., Sanfey, A. G., Aronson, J. A., Nystrom, L. E., and Cohen, J. D. (2004). Opposing BOLD responses to reciprocated and unreciprocated altruism in putative reward pathways. *Brain Imaging*, 15:2539–2543.

Sanfey, A. G., Rilling, J. K., Aronson, J. A., Nystrom, L. E., and Cohen, J. D. (2003). The neural basis of economic decision making in the ultimatum game. *Science*, 300:1755–1757.

Schmelz, K. and Ziegelmeyer, A. (2012). Hierarchical distance and control aversion: Evidence from the internet and the laboratory. Working paper.

Schulte-Mecklenbeck, M. and Huber, O. (2003). Information search in the laboratory and on the web: With or without an experimenter. *Behavior Research Methods, Instruments, and Computers*, 2:227–235.

Suri, S. and Watts, D. J. (2011). Cooperation and contagion in web-based, networked public goods experiments. *PLoS ONE*, 6:e16836.

	Symmetric design	Asymmetric design
Short-term lab	30 subjects (70% males)	36 subjects (48% males)
Short-term internet	38 subjects (63% males)	32 subjects (53% males)
Long-term internet	60 subjects (63% males)	72 subjects (51% males)

Table 1: The different treatments (data refers to subjects who completed the experiment).

	Short-term lab		Short-term internet		Long-term internet	
	Q_{3-23}	Q_{11-23}	Q_{3-23}	Q_{11-23}	Q_{3-23}	Q_{11-23}
Mean	63.31	63.53	65.76	64.12	63.21	63.15
Median	65.19	65.53	65.71	66.30	64.40	65.07
Std. Dev.	8.51	8.90	12.00	13.40	7.96	8.63

(a) Mean quantities in the symmetric treatments (Benchmark outputs: $Q_s^N = 66$, $Q_s^{JPM} = 49.5$, $Q_s^C = 99$)

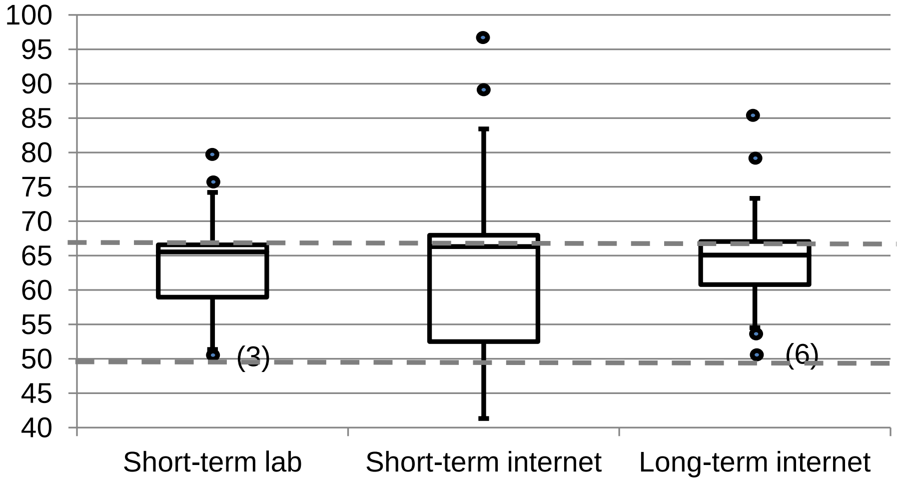
	Short-term lab		Short-term internet		Long-term internet	
	Q_{3-23}	Q_{11-23}	Q_{3-23}	Q_{11-23}	Q_{3-23}	Q_{11-23}
Mean	66.28	65.87	66.41	66.34	63.88	64.13
Median	65.26	64.15	66.66	66.96	63.78	64.00
Std. Dev	5.10	5.97	6.19	6.14	3.93	4.21

(b) Mean quantities in the asymmetric treatments (Cournot output: $Q_{as}^N = 63.83$)

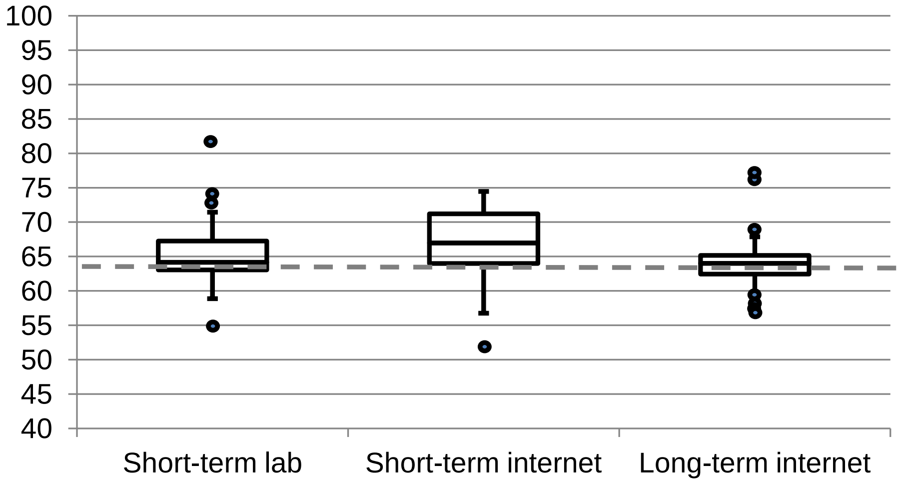
Table 2: Summary statistics of the market quantities averaged over the middle 21 rounds and over rounds 11-23.

	Median	Short-term lab	Short-term internet
Symmetric treatments (all firms) results:			
Short-term lab	65.53	-	-
Short-term internet	66.30	0.78	-
Long-term internet	65.07	0.75	0.71
Asymmetric treatments (all firms) results:			
Short-term lab	64.15	-	-
Short-term internet	66.96	0.28	-
Long-term internet	64.00	0.55	0.05
Asymmetric treatments (low-cost firms) results:			
Short-term lab	33.73	-	-
Short-term internet	33.51	0.79	-
Long-term internet	34.53	0.77	0.66
Asymmetric treatments (high-cost firms) results:			
Short-term lab	30.61	-	-
Short-term internet	32.76	0.24	-
Long-term internet	29.42	0.06	0.01

Table 3: P-values of F-P test comparing between each pair of treatments (averaged over rounds 11 to 23).



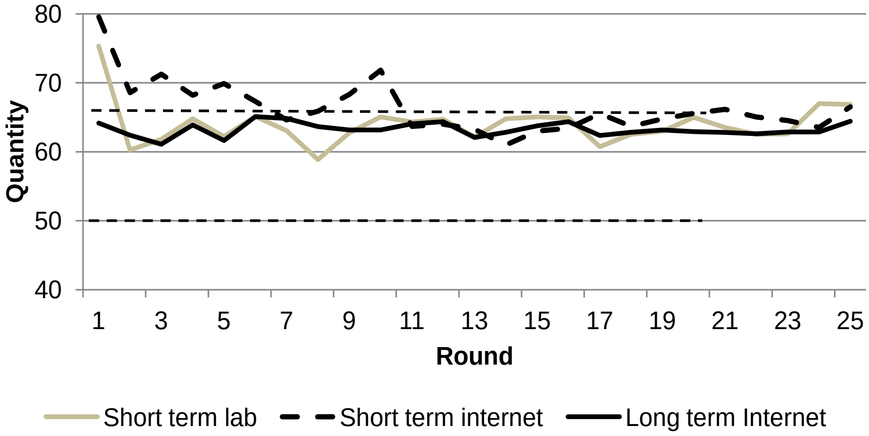
(a) Symmetric treatments



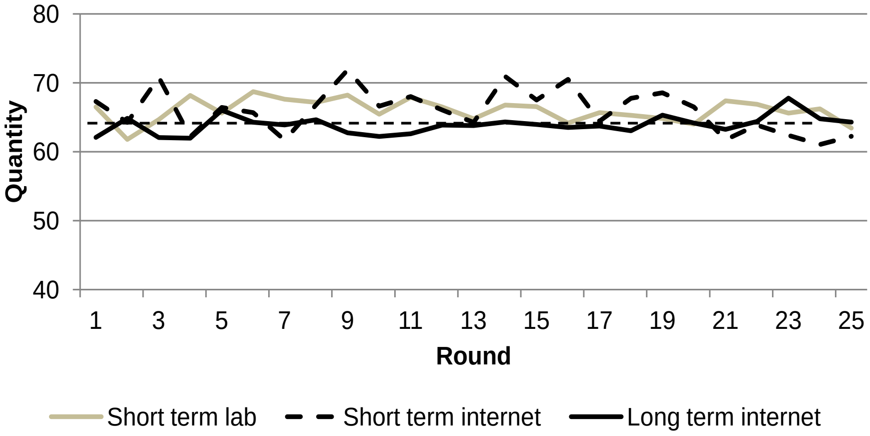
(b) Asymmetric treatments

The closed boxes are constructed from first and third quartiles, while the second quartile, the median is the middle line in each box. The whiskers mark the lower and upper adjacent limits. These limits are equal to maximum 1.5 times the difference between the third and first quartiles (in case of no outliers they are bounded by the minimum or maximum output values). Finally, the dots illustrate outliers. In Figure 1(a), the upper dashed reference line denotes the Cournot-Nash quantity, while the lower dashed line denotes the joint-payoff maximizing quantity. In Figure 1(b) the dashed reference line indicates the Cournot-Nash quantity.

Figure 1: Box plot of the average quantities (over rounds 11-23) of the different markets.



(a) Symmetric treatments



(b) Asymmetric treatments

The dashed horizontal lines denote the Cournot-Nash quantity in both treatments (the lower dashed line in Figure 2(a) denotes the joint-payoff maximizing quantity).

Figure 2: Mean quantities across markets for each round (a total of 25 rounds).

	Short-term lab		Short-term internet		Long-term internet	
	Q_{3-23}	Q_{11-23}	Q_{3-23}	Q_{11-23}	Q_{3-23}	Q_{11-23}
Mean	35.05	34.74	32.80	33.61	33.91	33.98
Median	34.04	33.73	32.28	33.53	34.16	34.53
Std. Dev	4.99	4.82	4.13	3.91	4.96	5.59

(a) Mean quantities of the *low-cost firms* (Cournot output: $q^N_{Low} = 35.16$)

	Short-term lab		Short-term internet		Long-term internet	
	Q_{3-23}	Q_{11-23}	Q_{3-23}	Q_{11-23}	Q_{3-23}	Q_{11-23}
Mean	31.23	31.12	33.60	32.73	29.96	30.14
Median	30.83	30.61	34.09	32.76	29.57	29.42
Std. Dev	3.10	3.14	4.08	4.65	4.03	4.08

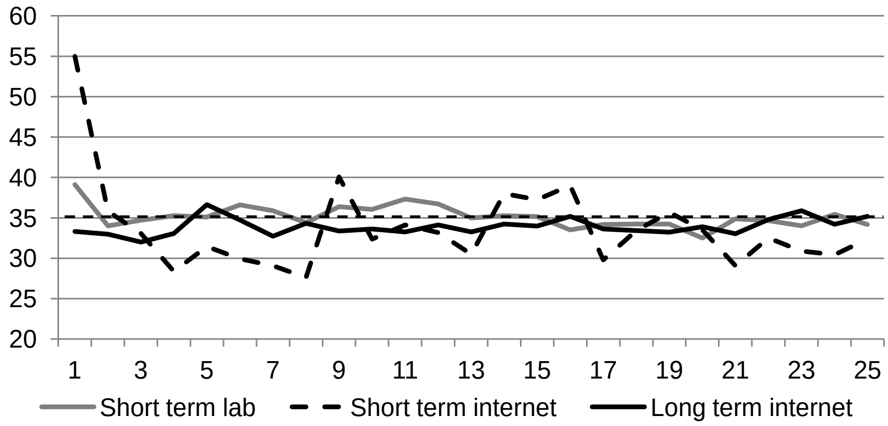
(b) Mean quantities of the *high-cost firms* (Cournot output: $q^N_{High} = 28.66$)

Table 4: Summary statistics of the quantities selected in the different asymmetric treatments averaged over the middle 21 rounds (rounds 3-23) and over rounds 11-23.

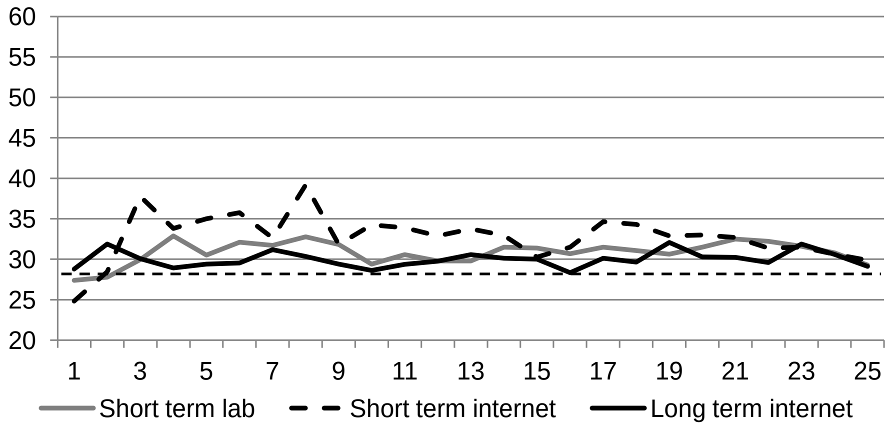
	Symmetric treatments		Asymmetric treatments	
Variable	Model 1	Model 2	Model 3	Model 4
Best response	0.34*** (0.05)	0.35*** (0.03)	0.54*** (0.05)	0.50*** (0.04)
Imitation	0.19* (0.11)	0.19** (0.08)	0.24*** (0.02)	0.12*** (0.03)
Best response×Long_term	0.01 (0.07)	-	-0.02 (0.06)	-
Imitation×Long_term	0.06 (0.11)	-	-0.18*** (0.05)	-
Best response×Lab	0.06 (0.07)	-	-0.18** (0.07)	-
Imitation×Lab	-0.05 (0.13)	-	-0.16*** (0.05)	-
Lab	-0.32 (0.66)	-0.26 (0.65)	0.03 (0.62)	0.05 (0.64)
Long_term	-0.29 (0.62)	-0.31 (0.61)	-0.69 (0.61)	-0.65 (0.58)
JPM	-3.25*** (0.49)	-3.14*** (0.48)	-	-
Spiteful	0.67 (2.10)	0.53 (2.23)	1.57 (2.16)	1.76 (2.16)
Low_cost	-	-	1.02** (0.46)	1.44*** (0.51)
Male	-0.24 (0.43)	-0.21 (0.49)	-0.31 (0.39)	-0.24 (0.41)
Round dummies	Insignificant	Insignificant	Insignificant	Insignificant
Cons	0.16 (0.92)	0.14 (0.94)	-0.31 (0.71)	-0.54 (0.67)
Obs	3072	3072	3360	3360
F-test	43.70***	16.67***	49.86***	38.48***
R^2	0.32	0.31	0.36	0.34

The table presents the estimation coefficients (std. err. are given in parenthesis) of the different estimations. As a benchmark for treatment variable we use the short-term internet treatment, and as a benchmark for round we used round 10. All round dummies are insignificant except for one in the asymmetric treatments (round 24). The signs *, **, and *** denote significance equal to (or less than) the 10%, 5%, and 1% levels.

Table 5: Estimation results of the symmetric and asymmetric treatment models.



(a) Low-cost firms



(b) High-cost firms

The dashed horizontal lines denote the Cournot-Nash asymmetric quantity for each firm type (low and high-cost).

Figure 3: Mean quantities across markets for each round (a total of 25 rounds).

A Translation of the Instructions:

Welcome to our experiment. Please read these instructions carefully! You can earn some money in this experiment depending on your decisions and on the decisions of the others.

If something is unclear, please contact the experimenters. You can either call them or send them an email [cell phone number and email of an experimenter and a research assistant were provided here].

- All participants receive the same instructions.
- You will stay anonymous to us and to the other participants during and after the experiment.
- In this experiment you represent a firm. There is one other firm that produces and sells the same product in the market. You will be matched with the other firm during the whole experiment.
- Each working day, all firms have to make one decision, namely what quantity they wish to produce. You can produce any integer quantity ranging between 0 to 100. The decision should be made once every 24 hours each working day (Monday-Friday). The experiment will continue for exactly 25 working days. The experiment starts on Thursday, June 23, 2011, and will end on Wednesday, July 27, 2011. In the instructions we refer to each working day (from 12.00am to 12.00pm) as one "period".
- In case you forgot to make your output decision in one period, the system will automatically submit your output decision from the last period. However, if you do not make decisions for two subsequent working days or for a total of three working days, you will be excluded from the experiment and you will not get paid.

- In case you do not make your decision by 7.00pm on a working day, you will receive a reminder email.
- You can make your decision from everywhere, conditional on having internet access (including smart phones).
- During the experiment all payoffs are expressed in ECU (experimental currency units). Each participant starts with an initial amount of 500 ECU.
- After the experiment we will exchange your payoff into €. The exchange rate will be 1000 ECU/€ where 1000 ECU equals 1 €.
- [**Symmetric treatment only:**] The costs of production are 1 ECU per unit (this holds for all firms).
- [**Asymmetric treatment only:**] The two firms in the market incur different costs of production. One firm **incurs** a cost of 1 ECU per unit while the other incurs a cost of 7.5 ECU per unit.
- [**Asymmetric treatment only:**] The production cost of your firm will appear on your computer screen. The other firm in the market will have the respective other cost.
- The following important rule holds: The larger the total (aggregate) quantity produced by the two firms, the lower the market price. Moreover, the price will be zero from a certain amount of total output upwards.
- Your profit per unit of output will be the difference between the market price and the production cost. Note that you will incur a loss if the market price is below the unit costs. Your profit per period will thus be equal to the profit per unit multiplied by the number of units you sell.

- During the experiment you can use a 'profit calculator' before you decide on the quantity to produce. You enter your quantity and a (hypothetical) quantity of the other firm, then the 'profit calculator' calculates your profit [**Asymmetric treatment only:** and the profit of the other firm].

- The demand and cost functions do not change over the 25 periods.

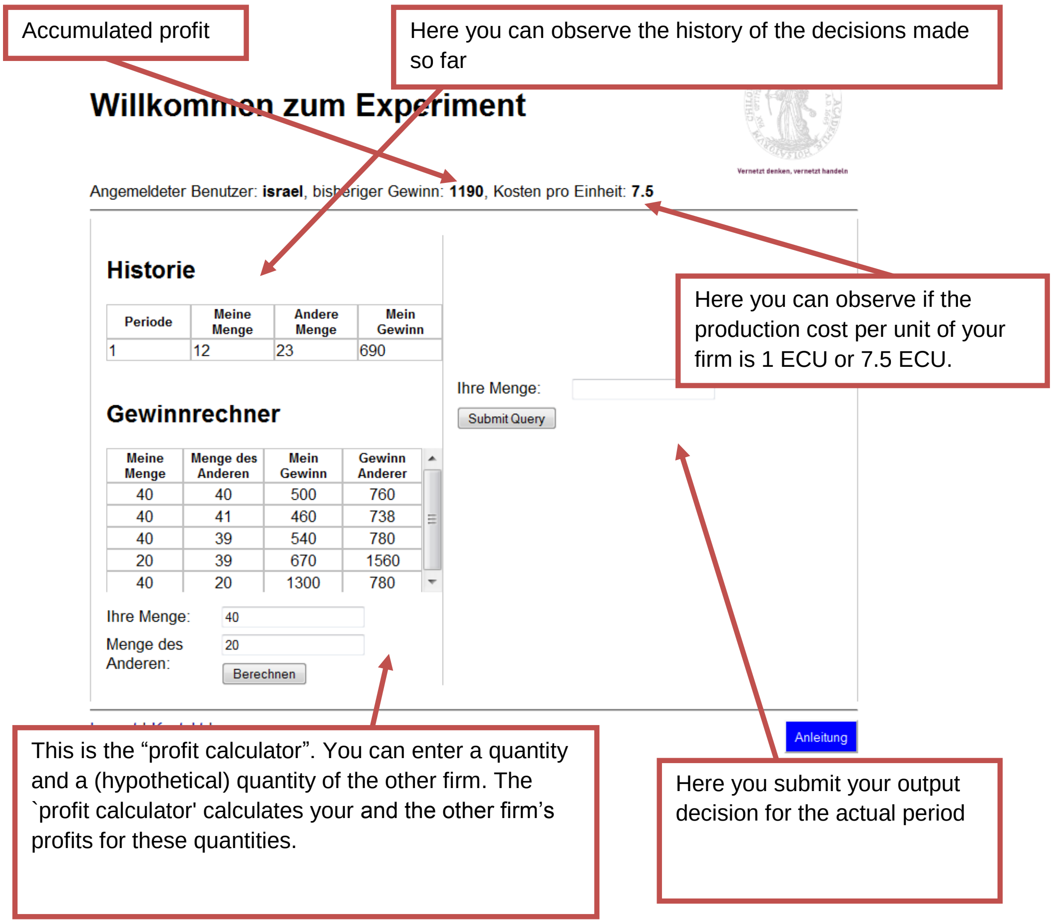
- In each period, yours and the other firms' output decision will be registered, the corresponding price will be determined and the profits will be calculated.

- After each period (on each working day from 12.00am) you will receive the following information: The quantities produced by you and by the other firm, your profit in the last period, [**Asymmetric treatment only:** the profit of the other firm], and your accumulated profit up to this period.

- The total profit in the experiment will be the total amount earned in the 25 periods of the experiment (including the initial amount of 500 ECU).

- At the end of the last period you will receive a confirmation of your total earnings in ECU during the experiment. We will add up your profits and calculate your money reward in €. You can pick up your earnings in cash one working day after the last period, i.e., starting on Thursday, July 28, 2011, at [address was provided].

- Note that other participants will not observe how much you have earned, and you will not observe how much others have earned.



Here is an example of a computer screen [the asymmetric treatment]

B Computer screens

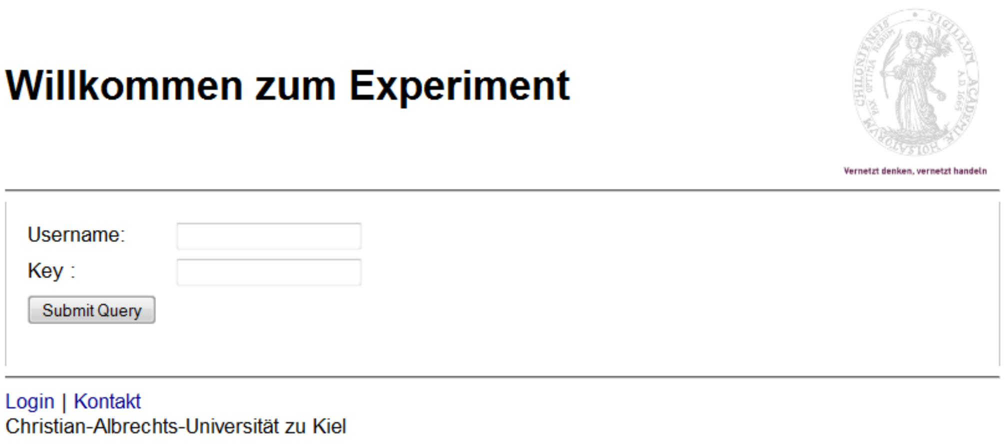


Figure B.1: The login screen

Figure B.2: The decision screen for the symmetric treatments

Figure B.3: The decision screen for the asymmetric treatments

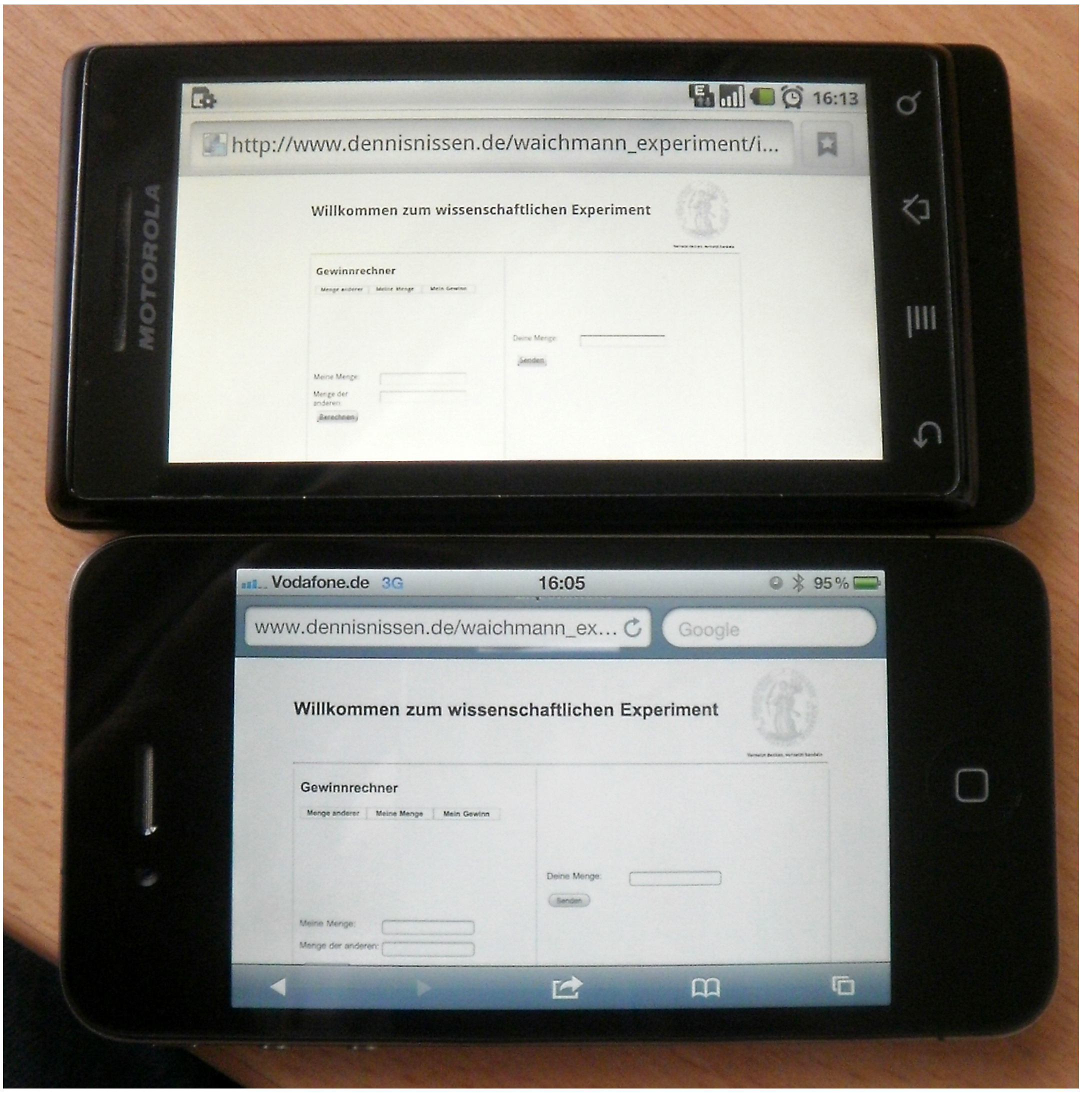


Figure B.4: The experiment could even be operated from smart-phones.

PREVIOUS DISCUSSION PAPERS

100 Normann, Hans-Theo, Requate, Till and Waichman, Israel, Do Short-Term Laboratory Experiments Provide Valid Descriptions of Long-Term Economic Interactions? A Study of Cournot Markets, July 2013.
Forthcoming in: Experimental Economics.

99 Dertwinkel-Kalt, Markus, Haucap, Justus and Wey, Christian, Input Price Discrimination (Bans), Entry and Welfare, June 2013.

98 Aguzzoni, Luca, Argentesi, Elena, Ciari, Lorenzo, Duso, Tomaso and Tognoni, Massimo, Ex-post Merger Evaluation in the UK Retail Market for Books, June 2013.

97 Caprice, Stéphane and von Schlippenbach, Vanessa, One-Stop Shopping as a Cause of Slotting Fees: A Rent-Shifting Mechanism, May 2012.
Published in: Journal of Economics and Management Strategy, 22 (2013), pp. 468-487.

96 Wenzel, Tobias, Independent Service Operators in ATM Markets, June 2013.
Forthcoming in: Scottish Journal of Political Economy.

95 Coublucq, Daniel, Econometric Analysis of Productivity with Measurement Error: Empirical Application to the US Railroad Industry, June 2013.

94 Coublucq, Daniel, Demand Estimation with Selection Bias: A Dynamic Game Approach with an Application to the US Railroad Industry, June 2013.

93 Baumann, Florian and Friehe, Tim, Status Concerns as a Motive for Crime?, April 2013.

92 Jeitschko, Thomas D. and Zhang, Nanyun, Adverse Effects of Patent Pooling on Product Development and Commercialization, April 2013.

91 Baumann, Florian and Friehe, Tim, Private Protection Against Crime when Property Value is Private Information, April 2013.
Published in: International Review of Law and Economics, 35 (2013), pp. 73-79.

90 Baumann, Florian and Friehe, Tim, Cheap Talk About the Detection Probability, April 2013.
Forthcoming in: International Game Theory Review.

89 Pagel, Beatrice and Wey, Christian, How to Counter Union Power? Equilibrium Mergers in International Oligopoly, April 2013.

88 Jovanovic, Dragan, Mergers, Managerial Incentives, and Efficiencies, April 2013.

87 Heimeshoff, Ulrich and Klein Gordon J., Bargaining Power and Local Heroes, March 2013.

86 Bertschek, Irene, Cerquera, Daniel and Klein, Gordon J., More Bits – More Bucks? Measuring the Impact of Broadband Internet on Firm Performance, February 2013.
Forthcoming in: Information Economics and Policy.

85 Rasch, Alexander and Wenzel, Tobias, Piracy in a Two-Sided Software Market, February 2013.
Published in: Journal of Economic Behavior & Organization, 88 (2013), pp. 78-89.

84 Bataille, Marc and Steinmetz, Alexander, Intermodal Competition on Some Routes in Transportation Networks: The Case of Inter Urban Buses and Railways, January 2013.

83 Haucap, Justus and Heimeshoff, Ulrich, Google, Facebook, Amazon, eBay: Is the Internet Driving Competition or Market Monopolization?, January 2013.
Forthcoming in: International Economics and Economic Policy.

82 Regner, Tobias and Riener, Gerhard, Voluntary Payments, Privacy and Social Pressure on the Internet: A Natural Field Experiment, December 2012.

81 Dertwinkel-Kalt, Markus and Wey, Christian, The Effects of Remedies on Merger Activity in Oligopoly, December 2012.

80 Baumann, Florian and Friehe, Tim, Optimal Damages Multipliers in Oligopolistic Markets, December 2012.

79 Duso, Tomaso, Röller, Lars-Hendrik and Seldeslachts, Jo, Collusion through Joint R&D: An Empirical Assessment, December 2012.
Forthcoming in: The Review of Economics and Statistics.

78 Baumann, Florian and Heine, Klaus, Innovation, Tort Law, and Competition, December 2012.
Forthcoming in: Journal of Institutional and Theoretical Economics.

77 Coenen, Michael and Jovanovic, Dragan, Investment Behavior in a Constrained Dictator Game, November 2012.

76 Gu, Yiquan and Wenzel, Tobias, Strategic Obfuscation and Consumer Protection Policy in Financial Markets: Theory and Experimental Evidence, November 2012.

75 Haucap, Justus, Heimeshoff, Ulrich and Jovanovic, Dragan, Competition in Germany’s Minute Reserve Power Market: An Econometric Analysis, November 2012.
Forthcoming in: The Energy Journal.

74 Normann, Hans-Theo, Rösch, Jürgen and Schultz, Luis Manuel, Do Buyer Groups Facilitate Collusion?, November 2012.

73 Riener, Gerhard and Wiederhold, Simon, Heterogeneous Treatment Effects in Groups, November 2012.

72 Berlemann, Michael and Haucap, Justus, Which Factors Drive the Decision to Boycott and Opt Out of Research Rankings? A Note, November 2012.

71 Muck, Johannes and Heimeshoff, Ulrich, First Mover Advantages in Mobile Telecommunications: Evidence from OECD Countries, October 2012.

70 Karaçuka, Mehmet, Çatik, A. Nazif and Haucap, Justus, Consumer Choice and Local Network Effects in Mobile Telecommunications in Turkey, October 2012.
Published in: Telecommunications Policy, 37 (2013), pp. 334-344.

69 Clemens, Georg and Rau, Holger A., Rebels without a Clue? Experimental Evidence on Partial Cartels, April 2013 (First Version October 2012).

68 Regner, Tobias and Riener, Gerhard, Motivational Cherry Picking, September 2012.

67 Fonseca, Miguel A. and Normann, Hans-Theo, Excess Capacity and Pricing in Bertrand-Edgeworth Markets: Experimental Evidence, September 2012.
Forthcoming in: Journal of Institutional and Theoretical Economics.

66 Riener, Gerhard and Wiederhold, Simon, Team Building and Hidden Costs of Control, September 2012.

65 Fonseca, Miguel A. and Normann, Hans-Theo, Explicit vs. Tacit Collusion – The Impact of Communication in Oligopoly Experiments, August 2012.
Published in: European Economic Review, 56 (2012), pp. 1759-1772.

64 Jovanovic, Dragan and Wey, Christian, An Equilibrium Analysis of Efficiency Gains from Mergers, July 2012.

63 Dewenter, Ralf, Jaschinski, Thomas and Kuchinke, Björn A., Hospital Market Concentration and Discrimination of Patients, July 2012.

62 Von Schlippenbach, Vanessa and Teichmann, Isabel, The Strategic Use of Private Quality Standards in Food Supply Chains, May 2012.
Published in: American Journal of Agricultural Economics, 94 (2012), pp. 1189-1201.

61 Sapi, Geza, Bargaining, Vertical Mergers and Entry, July 2012.

60 Jentzsch, Nicola, Sapi, Geza and Suleymanova, Irina, Targeted Pricing and Customer Data Sharing Among Rivals, July 2012.
Published in: International Journal of Industrial Organization, 31 (2013), pp. 131-144.

59 Lambarraa, Fatima and Riener, Gerhard, On the Norms of Charitable Giving in Islam: A Field Experiment, June 2012.

58 Duso, Tomaso, Gugler, Klaus and Szücs, Florian, An Empirical Assessment of the 2004 EU Merger Policy Reform, June 2012.
Forthcoming in: Economic Journal.

57 Dewenter, Ralf and Heimeshoff, Ulrich, More Ads, More Revs? Is there a Media Bias in the Likelihood to be Reviewed?, June 2012.

56 Böckers, Veit, Heimeshoff, Ulrich and Müller Andrea, Pull-Forward Effects in the German Car Scrappage Scheme: A Time Series Approach, June 2012.

55 Kellner, Christian and Riener, Gerhard, The Effect of Ambiguity Aversion on Reward Scheme Choice, June 2012.

54 De Silva, Dakshina G., Kosmopoulou, Georgia, Pagel, Beatrice and Peeters, Ronald, The Impact of Timing on Bidding Behavior in Procurement Auctions of Contracts with Private Costs, June 2012.
Published in: Review of Industrial Organization, 41 (2013), pp.321-343.

53 Benndorf, Volker and Rau, Holger A., Competition in the Workplace: An Experimental Investigation, May 2012.

52 Haucap, Justus and Klein, Gordon J., How Regulation Affects Network and Service Quality in Related Markets, May 2012.
Published in: Economics Letters, 117 (2012), pp. 521-524.

51 Dewenter, Ralf and Heimeshoff, Ulrich, Less Pain at the Pump? The Effects of Regulatory Interventions in Retail Gasoline Markets, May 2012.

50 Böckers, Veit and Heimeshoff, Ulrich, The Extent of European Power Markets, April 2012.

49 Barth, Anne-Kathrin and Heimeshoff, Ulrich, How Large is the Magnitude of Fixed-Mobile Call Substitution? - Empirical Evidence from 16 European Countries, April 2012.

48 Herr, Annika and Suppliet, Moritz, Pharmaceutical Prices under Regulation: Tiered Co-payments and Reference Pricing in Germany, April 2012.

47 Haucap, Justus and Müller, Hans Christian, The Effects of Gasoline Price Regulations: Experimental Evidence, April 2012.

46 Stühmeier, Torben, Roaming and Investments in the Mobile Internet Market, March 2012.
Published in: Telecommunications Policy, 36 (2012), pp. 595-607.

45 Graf, Julia, The Effects of Rebate Contracts on the Health Care System, March 2012,
Forthcoming in: The European Journal of Health Economics.

44 Pagel, Beatrice and Wey, Christian, Unionization Structures in International Oligopoly, February 2012.
Published in: Labour: Review of Labour Economics and Industrial Relations, 27 (2013), pp. 1-17.

43 Gu, Yiquan and Wenzel, Tobias, Price-Dependent Demand in Spatial Models, January 2012.
Published in: B. E. Journal of Economic Analysis & Policy,12 (2012), Article 6.

42 Barth, Anne-Kathrin and Heimeshoff, Ulrich, Does the Growth of Mobile Markets Cause the Demise of Fixed Networks? – Evidence from the European Union, January 2012.

41 Stühmeier, Torben and Wenzel, Tobias, Regulating Advertising in the Presence of Public Service Broadcasting, January 2012.
Published in: Review of Network Economics, 11, 2 (2012), Article 1.

40 Müller, Hans Christian, Forecast Errors in Undisclosed Management Sales Forecasts: The Disappearance of the Overoptimism Bias, December 2011.

39 Gu, Yiquan and Wenzel, Tobias, Transparency, Entry, and Productivity, November 2011.
Published in: Economics Letters, 115 (2012), pp. 7-10.

38 Christin, Clémence, Entry Deterrence Through Cooperative R&D Over-Investment, November 2011.
Forthcoming in: Louvain Economic Review.

37 Haucap, Justus, Herr, Annika and Frank, Björn, In Vino Veritas: Theory and Evidence on Social Drinking, November 2011.
In a modified version forthcoming in: European Journal of Law and Economics.

36 Barth, Anne-Kathrin and Graf, Julia, Irrationality Rings! – Experimental Evidence on Mobile Tariff Choices, November 2011.

35 Jeitschko, Thomas D. and Normann, Hans-Theo, Signaling in Deterministic and Stochastic Settings, November 2011.
Published in: Journal of Economic Behavior and Organization, 82 (2012), pp.39-55.

34 Christin, Cémence, Nicolai, Jean-Philippe and Pouyet, Jerome, The Role of Abatement Technologies for Allocating Free Allowances, October 2011.

33 Keser, Claudia, Suleymanova, Irina and Wey, Christian, Technology Adoption in Markets with Network Effects: Theory and Experimental Evidence, October 2011.
Published in: Information Economics and Policy, 24 (2012), pp. 262-276.

32 Çatik, A. Nazif and Karaçuka, Mehmet, The Bank Lending Channel in Turkey: Has it Changed after the Low Inflation Regime?, September 2011.
Published in: Applied Economics Letters, 19 (2012), pp. 1237-1242.

31 Hauck, Achim, Neyer, Ulrike and Vieten, Thomas, Reestablishing Stability and Avoiding a Credit Crunch: Comparing Different Bad Bank Schemes, August 2011.

30 Suleymanova, Irina and Wey, Christian, Bertrand Competition in Markets with Network Effects and Switching Costs, August 2011.
Published in: B. E. Journal of Economic Analysis & Policy, 11 (2011), Article 56.

29 Stühmeier, Torben, Access Regulation with Asymmetric Termination Costs, July 2011.
Published in: Journal of Regulatory Economics, 43 (2013), pp. 60-89.

28 Dewenter, Ralf, Haucap, Justus and Wenzel, Tobias, On File Sharing with Indirect Network Effects Between Concert Ticket Sales and Music Recordings, July 2011.
Published in: Journal of Media Economics, 25 (2012), pp. 168-178.

27 Von Schlippenbach, Vanessa and Wey, Christian, One-Stop Shopping Behavior, Buyer Power, and Upstream Merger Incentives, June 2011.

26 Balsmeier, Benjamin, Buchwald, Achim and Peters, Heiko, Outside Board Memberships of CEOs: Expertise or Entrenchment?, June 2011.

25 Clougherty, Joseph A. and Duso, Tomaso, Using Rival Effects to Identify Synergies and Improve Merger Typologies, June 2011.
Published in: Strategic Organization, 9 (2011), pp. 310-335.

24 Heinz, Matthias, Juranek, Steffen and Rau, Holger A., Do Women Behave More Reciprocally than Men? Gender Differences in Real Effort Dictator Games, June 2011.
Published in: Journal of Economic Behavior and Organization, 83 (2012), pp. 105-110.

23 Sapi, Geza and Suleymanova, Irina, Technology Licensing by Advertising Supported Media Platforms: An Application to Internet Search Engines, June 2011.
Published in: B. E. Journal of Economic Analysis & Policy, 11 (2011), Article 37.

22 Buccirossi, Paolo, Ciari, Lorenzo, Duso, Tomaso, Spagnolo Giancarlo and Vitale, Cristiana, Competition Policy and Productivity Growth: An Empirical Assessment, May 2011.
Forthcoming in: The Review of Economics and Statistics.

21 Karaçuka, Mehmet and Çatik, A. Nazif, A Spatial Approach to Measure Productivity Spillovers of Foreign Affiliated Firms in Turkish Manufacturing Industries, May 2011.
Published in: The Journal of Developing Areas, 46 (2012), pp. 65-83.

20 Çatik, A. Nazif and Karaçuka, Mehmet, A Comparative Analysis of Alternative Univariate Time Series Models in Forecasting Turkish Inflation, May 2011.
Published in: Journal of Business Economics and Management, 13 (2012), pp. 275-293.

19 Normann, Hans-Theo and Wallace, Brian, The Impact of the Termination Rule on Cooperation in a Prisoner's Dilemma Experiment, May 2011.
Published in: International Journal of Game Theory, 41 (2012), pp. 707-718.

18 Baake, Pio and von Schlippenbach, Vanessa, Distortions in Vertical Relations, April 2011.
Published in: Journal of Economics, 103 (2011), pp. 149-169.

17 Haucap, Justus and Schwalbe, Ulrich, Economic Principles of State Aid Control, April 2011.
Forthcoming in: F. Montag & F. J. Säcker (eds.), European State Aid Law: Article by Article Commentary, Beck: München 2012.

16 Haucap, Justus and Heimeshoff, Ulrich, Consumer Behavior towards On-net/Off-net Price Differentiation, January 2011.
Published in: Telecommunication Policy, 35 (2011), pp. 325-332.

15 Duso, Tomaso, Gugler, Klaus and Yurtoglu, Burcin B., How Effective is European Merger Control? January 2011.
Published in: European Economic Review, 55 (2011), pp. 980-1006.

14 Haigner, Stefan D., Jenewein, Stefan, Müller, Hans Christian and Wakolbinger, Florian, The First shall be Last: Serial Position Effects in the Case Contestants evaluate Each Other, December 2010.
Published in: Economics Bulletin, 30 (2010), pp. 3170-3176.

13 Suleymanova, Irina and Wey, Christian, On the Role of Consumer Expectations in Markets with Network Effects, November 2010.
Published in: Journal of Economics, 105 (2012), pp. 101-127.

12 Haucap, Justus, Heimeshoff, Ulrich and Karaçuka, Mehmet, Competition in the Turkish Mobile Telecommunications Market: Price Elasticities and Network Substitution, November 2010.
Published in: Telecommunications Policy, 35 (2011), pp. 202-210.

11 Dewenter, Ralf, Haucap, Justus and Wenzel, Tobias, Semi-Collusion in Media Markets, November 2010.
Published in: International Review of Law and Economics, 31 (2011), pp. 92-98.

10 Dewenter, Ralf and Kruse, Jörn, Calling Party Pays or Receiving Party Pays? The Diffusion of Mobile Telephony with Endogenous Regulation, October 2010.
Published in: Information Economics and Policy, 23 (2011), pp. 107-117.

09 Hauck, Achim and Neyer, Ulrike, The Euro Area Interbank Market and the Liquidity Management of the Eurosystem in the Financial Crisis, September 2010.

08 Haucap, Justus, Heimeshoff, Ulrich and Schultz, Luis Manuel, Legal and Illegal Cartels in Germany between 1958 and 2004, September 2010.
Published in: H. J. Ramser & M. Stadler (eds.), Marktmacht. Wirtschaftswissenschaftliches Seminar Ottobeuren, Volume 39, Mohr Siebeck: Tübingen 2010, pp. 71-94.

07 Herr, Annika, Quality and Welfare in a Mixed Duopoly with Regulated Prices: The Case of a Public and a Private Hospital, September 2010.
Published in: German Economic Review, 12 (2011), pp. 422-437.

06 Blanco, Mariana, Engelmann, Dirk and Normann, Hans-Theo, A Within-Subject Analysis of Other-Regarding Preferences, September 2010.
Published in: Games and Economic Behavior, 72 (2011), pp. 321-338.

05 Normann, Hans-Theo, Vertical Mergers, Foreclosure and Raising Rivals' Costs – Experimental Evidence, September 2010.
Published in: The Journal of Industrial Economics, 59 (2011), pp. 506-527.

04 Gu, Yiquan and Wenzel, Tobias, Transparency, Price-Dependent Demand and Product Variety, September 2010.
Published in: Economics Letters, 110 (2011), pp. 216-219.

03 Wenzel, Tobias, Deregulation of Shopping Hours: The Impact on Independent Retailers and Chain Stores, September 2010.
Published in: Scandinavian Journal of Economics, 113 (2011), pp. 145-166.

02 Stühmeier, Torben and Wenzel, Tobias, Getting Beer During Commercials: Adverse Effects of Ad-Avoidance, September 2010.
Published in: Information Economics and Policy, 23 (2011), pp. 98-106.

01 Inderst, Roman and Wey, Christian, Countervailing Power and Dynamic Efficiency, September 2010.
Published in: Journal of the European Economic Association, 9 (2011), pp. 702-720.

Heinrich-Heine-University of Düsseldorf

Düsseldorf Institute for Competition Economics (DICE)

Universitätsstraße 1 _ 40225 Düsseldorf
www.dice.hhu.de

ISSN 2190-9938 (online)
ISBN 978-3-86304-099-4