Lecture 10 (AO, May 7)

Environmental Policy in the Central European Context

Time: Thursdays 3 p.m. – 6 p.m.
Location: at CERGE-EI, Room # 10

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Undergraduate Program in Central European Studies

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WEEK: TOPICS: DATE: INSTRUCTOR
1 Introduction (history/outline) Feb 19 JK/AO
2 Market failures: externalities, tragedy of the commons, enforcement as public good, also, (rise and fall) of the environmental Kuznets Curve February 26 AO
3 Interventionalist solutions to the Externality problem – Pigouvian taxes and standards and charges, also environmental labeling and incomplete consumer information in laboratory markets March 5 JK
4 Interventionalist solutions to the Externality problem – Marketable pollution permits March 12 AO
5 Non-Interventionalist solutions to the Externality problem – The Coasian solution March 19 JK
6 Non-interventionalist solutions to the Externality problem – Self-regulation March 26 AO
7 *Mid-term exam April 2*
8 (L7) Environmental Policy in the Czech Republic – History and current issues April 9 JK
9 (L8) Environmental Policy in the EU – History and current problems April 23
(April 16 falls into Semester break and on Easter Holiday) AO
10 (L9) Environmental Policy in the world context – History and Current problems April 30 JK/AO
11 (L10) Contingent valuation and related issues May 7 AO
12 (L11) To be determined by the interests of the class (DISCUSS!)
Final exam: to be determined (according to schedule May 18 – 21)

Also, please remember to grade articles that you read so far …

Overdue worksheets will come tomorrow …

Update on political developments in the Czech Republic

As always, taken from www.radio.cz

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May 6th, 2009: Senate vote seals Czech parliamentary approval for EU's Lisbon treaty
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The Czech Senate has voted to ratify the European Union's Lisbon treaty, two and a half months after it was approved by the lower house. The motion was passed when 54 out of 79 senators present raised their hands for Lisbon ratification in Wednesday afternoon's vote. It was effectively decided by members of the Civic Democratic Party, which had previously been opposed to Lisbon: twelve of the party's senators voted in favour of its ratification.
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May 6th, 2009: Czech president says he will wait to put signature to ratification if senators send Lisbon to court
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The Czech president, Vaclav Klaus, now has to put his signature to ratification in order for it to come into effect. A group of euro-sceptic Civic Democrat senators have proposed sending the treaty to the Czech Constitutional Court; Mr Klaus said after Wednesday's vote that if they do so, he will not put his signature to Lisbon until the court rules on whether the document is in line with the Czech constitution. A request last year from Civic Democrat senators for the court to consider the treaty put the matter on ice for some months and is one reason the Czech Republic is among the last states to deal with its ratification. President Klaus also said he would not sign ratification until Lisbon has been approved by Ireland. He reiterated his view that Ireland's no vote in a referendum last year rendered the treaty a dead document.

[06.05.2009] - Current Affairs - Chris Johnstone

Caretaker Prime Minister Jan Fischer's government line-up gets cool response

Six weeks after the fall of the centre-right coalition, Czechs have been told the line-up of their new caretaker government. The last minute haggling over its composition has fuelled scepticism about its short-lived mission but it nonetheless faces some tough tasks.

Incoming Prime Minister Jan Fischer on Tuesday read out the list of his 16 ministers who should pilot the Czech Republic until early elections in October. The new government should begin work on Friday after being sworn in by President Václav Klaus.

Snapshots of the new line-up staring out of some of Wednesday's papers include a few experienced old hands, such as long time budget minister Eduard Janota - promoted to the top job at the finance minister - or experienced diplomat Jan Kohout – the new foreign minister – as well as quite a few relatively unknowns. Only one minister from the outgoing centre-right coalition, Michael Kocáb at human rights, has made the transition from the old to new government.

Media coverage of the new government appears to have been influenced by the last minute haggling between the two main parties and Greens over who should go where and Mr Fischer's own comments when presenting his line up. The incoming prime minister admitted that posts had been filled as a result of compromises and that compromise was not a

dirty word. He spelled out as well that this was not a government of experts that could govern without worrying about a parliamentary majority and that he would be in frequent contact with party leaders.

Wednesday's edition of the Lidové noviny newspaper described the new government as "submissive." It also quoted criticism from the Christian Democrats about how the two main parties, the Civic Democrats and Social Democrats, will be able to agree on what line the government should take once they enter the closing stages of what promises to be a fairly brutal parliamentary electoral campaign.

The business daily, Hospodářské noviny, pointed out to the high number of ex-communists in the government, four including Mr Fischer himself, and the heavy behind the scenes lobbying of major business interests over the Cabinet's composition.

In spite of the new government's complicated birth, doubts and misgivings about its parentage – it still faces some tough tasks. One of the biggest will be framing a new budget for 2010 against the background of soaring government spending and lower income due to the economic crisis.

Budget framing old timer, Mr Janota, says he wants to impose a 10 percent across the board cut in all ministerial budgets. To those who doubt whether the caretaker government could push that sort of tough measure though – it is pointed out that the politicians may prefer a temporary government to take such an unpopular step before they get back into power.

Review:

United Nations Framework Convention on Climate Change

http://unfccc.int/2860.php

Negotiations in 2009 leading to Copenhagen

2009 is a crucial year in the international effort to address climate change, culminating in the United Nations Climate Change Conference in **Copenhagen, 7-18 December.** In 2007, Parties agreed to shape an ambitious and effective international response to climate change, to be agreed at Copenhagen.

The Ad Hoc Working Group on Further Commitments for Annex I Parties under the Kyoto Protocol (AWG-KP) and the Ad Hoc Working Group on Long-term Cooperative Action under the Convention (AWG-LCA) will operate in full negotiating mode in 2009 to advance work towards meeting their respective mandates. <snip>

Kyoto Protocol (and some of its mechanisms)

What taxes can do and not do (OECD policy brief) …

More on allowances trading in the CR/Europe …

- Emissions trading within EU

- Emissions trading within CR

BASIC TERMS AND FACTS

Permission – a decree issued by the Ministry of Environment of CR (MECR) which

1) permits emission of GHG to the extent of allocated allowances
2) sets the conditions for assessment, declaration and verification of emissions

Procedure:

1. Apply for permission
2. Permission issued by the MECR, contains conditions for assessment, specification of methodology and frequency of assessment; conditions for declaration and public disclosure of emissions
3. Administrator of the facility assesses and declares emissions, declaration must be submitted to the MECR by Feb 28 of the next calendar year
4. Administrator of the facility is responsible for verification of emissions by the authorized person (which must be accredited, later on that…); a certificate must be submitted to the MECR by March 31 (if not, MECR informs the administrator of the national registry for allowances trading who then does not transfer new allowances before certification)

Trading:
For each trading period, MECR, together with the Ministry of Industry and Trade, prepares a proposal of the National Allocation Plan (NAP) in which the total number of allowances and also their distribution to individual facilities is specified for given trading period. <snip>

By April 30, every facility is responsible for removing from trading the number of allowances that corresponds to their emissions in the preceding calendar year.

MECR and the Czech inspection of Environment are the governing bodies

MECR is responsible for

- state supervision (to make sure the law is obeyed)
- permission issuance and changes approvals
- certifies persons authorized to verify emissions
- submits NAP proposal
- decides about issuance and assignment of additional allowances
- assigns number of allowances to new facilities (emerging after NAP approval)
- discloses the blacklist of facilities that have not complied with their obligations (to report)
- coordinates adoption and implementation of European law
- deals with offences

Czech inspection of Environment

- controls compliance with obligations and fulfillment of conditions set in permission(s)
- controls compliance with obligations related to certification of authorization to verify emission
- sets corrective measures
- deals with offences such as:
    - operation without permission or in conflict with permission (fine up to CZK 5mln)
    - failure to report change of conditions that may require change of permission (fine up to CZK 0.5 mln)
    - failure to report change of facility data/background information (fine up to CZK 0.1mln)
    - reports the data in conflict with permission or with law (fine up to CZK 2mln)
    - failure to remove corresponding number of allowances from trading (fine: in the 1st reporting period EUR 40, in the 2nd rp EUR 100 for each ton of CO2 equivalent not removed)
      (fines are collected and enforced by the competent customs office; 70% go to the state environmental fund, 30% to corresponding district)

Types of facilities involved
<snip>

Verification of emissions:

Emission verification is based on the emission report prepared by the facility. Reliability, credibility and precision of the data is checked (data about the activity, measurements, computations, choice of emission factors, used measurement methodology).

Verification is performed by an authorized person (auditor). Full access to the facility and to the data must be allowed. Auditor issues a report about verification for the facility.

Whole process of assessment, declaration and verification of emission is in detail described in corresponding gvmt regulations (procedure, methodology for measurement and computation; precision level, declaration, administration of the data, control mechanisms to ensure quality of the data, methodological directives for the authorized person, etc…)

Certification of auditors:

Candidate must get a license/certificate from the Czech institute of accreditation (ČIA – www.cia.cz). CIA's team of referees evaluates auditors' "specialized skills" and methods of verification. They check ex-ante competence and equipment, as well as "on-site" performance (so-called "witness audit"). The certificate is issued for the period of 3 years, with yearly "on-site' checks.

<snip>

Assignment of allowances (facility level)

- There are 394 facilities in the CR registered to the EU ETS system. 303 of those emitted in 2005-2006 less than 50 thousand tons of CO2 – small facilities. Altogether, small facilities produced 4.6% of total 2005 emissions and 4.4% of total 2006 emissions.
- Remaining 95 facilities produced 95.4% (95.6%) of all emissions in 2004 (2006) – large facilities. It was also shown that between 2005 and 2006 for large emitters the trading is very efficient tool for reduction; it motivates facilities to search and implement saving measures and increases the use of renewable energy sources.
- Small facilities are more complicated, smaller diversity of production (and customers), lower elasticity of energetic efficiency of their production, higher transaction and administration cost together with limited yield from trading, limited capital reserves (to use for modernization); their emissions are more volatile => more difficult to predict
  - ⇨ therefore the classification for the purpose of assignment of allowances is based on size of the facility, not on the field
  - ⇨ For small facilities - average (small) 2005-2006 emissions +7%
  - ⇨ For large facilities - average (large) 2005-2006 emissions +1.279%
  - ⇨ for facilities with large year-on-year deviation (more than 20%) – upwards correction

Schleich, Rogge, & Betz, Incentives for energy efficiency in the EU Emissions Trading Scheme (ETS) (31 pages)

EU ETS applies to installations in the energy and industry sectors … (but not non-ET sectors like household, tertiary, and transport); it is the world's largest emissions trading system and the first international trading system; it covers around 12,000 large greenhouse gas emitting installations in the energy and industry sectors … it covers about 50% of Europe's CO2 emissions and 40% of the total greenhouse gas emissions; … : companies are issued – essentially for free and historic precedent -- emission allowances that they can trade. Essentially, they have to make sure to have enough allowances as production warrants, intertemporally smoothed.,

Authors

- analyze the National Allocation Plans (NAPs) of the 27 EU member states (MS) for phases 1 (2005 – 7),

2 (2008 – 12, coincides with the Kyoto commitment period), and 3 (2013 – 20)

- find that the price and cost effects for improvements in carbon and energy efficiency will be stronger in phase 2 than in phase 1 (but only because the European Commission reduced "substantially" the number of allowances to be allocated by the MSs)

- find that domestic efficiency improvements in the targeted sectors may remain limited since companies can make substantial use of credits from the Kyoto Mechanisms

"Because ET systems allow for static and dynamic efficiency, they are often considered to be superior to other types of regulation [such as taxes]." (p. 2)

- static eff: if all participants face the same marginal abatement cost, overall reduction costs are minimized
- dynamic eff: incentives to adopt/develop more energy- and carbon-efficient technologies

So much for the theory …

"More stringent ET budgets will lead to higher prices for European Union Allowances (EUAs) and thus greater incentives to improve energy and carbon efficiency, ceteris paribus. In the first phase, these incentives were low since the ET budgets turn out to be rather lenient, resulting in low prices for EUAs." (p. 3)

"For phase 1, verified emissions data revealed that very few countries allocated EUAs in 2005 below the actual 2005 emission levels of the ET sector (Austria, Greece, Italy, Ireland, Spain, and the UK). As a consequence of this surplus allowance, in May 2006, prices for EUAs plummeted from around Euro26/EUA to around Euro10/EUA and to well below Euro1/EUA towards the end of the first trading period. According to Kettner et al. (2007), the surplus for 2005 amounts to about 100 million EUA. Since the emission level in the absence of the EU ETS cannot be determined (it is counterfactual), the real extent of possible overallocation cannot be determined." (p. 4) Ellerman and Buchner (2008) suggest that "a substantial part of the surplus may have resulted from abatement activities." (p. 4) In other words, the ETS as such might have spurred innovations/abatement activities, and hence was more effective since trading prices seem to suggest.

(That may be so – see analogue of self-regulatory organizations trying to stave of regulatory action.)

“Nevertheless, the surplus of EUAs and the correspondingly low price provided little additional incentive to improve energy and carbon efficiency in phase 1. Together with high uncertainty about governments’ commitment to long-term targets, this meant that firms were not strongly motivated to develop energy-efficient and low-carbon technologies and service in phase 1 (Montgomery 2005).” (p. 4)

“For phase 2, therefore, the Commission developed its own criterion, based on 2005 verified emissions data, economic growth, and carbon intensity trends. Applying this criterion has led the EC to require budget cuts in all but four of the assessed plans (Denmark, France, the UK, Slovenia). … The EC reduced the total cap by 10.4% … In absolute terms, the budget adjustments were highest for Poland and Germany. In percentage terms, the budgets of Latvia (~56%), Estonia (~48%), and Lithuania (~47%) were reduced the most. … The required budget cuts were much higher for the new EU-12 MSs (… -25.4%) than for the EU-15 MSs (… -4.1%).” (pp. 4 – 5)

“The European Commission not only adjusted the ET budgets but also set a maximum amount of credits from Kyoto Mechanisms that companies may use to cover their emissions. … Whether companies will purchase these credits crucially depends on their costs relative to the price for EUAs which in turn depends on the stringency of the ET budgets.“ (p. 5)

“In conclusion, the price and cost incentives to improve energy and carbon efficiency are likely to be substantially higher in phase 2 than they were in phase 1. However, the need for domestic emission reductions via improved energy and carbon efficiency in the energy and industry sectors may still be rather low. This is revealed by comparing the maximum amount of credits from the Kyoto Mechanisms that companies may use to cover their emissions under the EU ETS with the reduction requirements implied by the three criteria.” (p. 7)

The conclusions

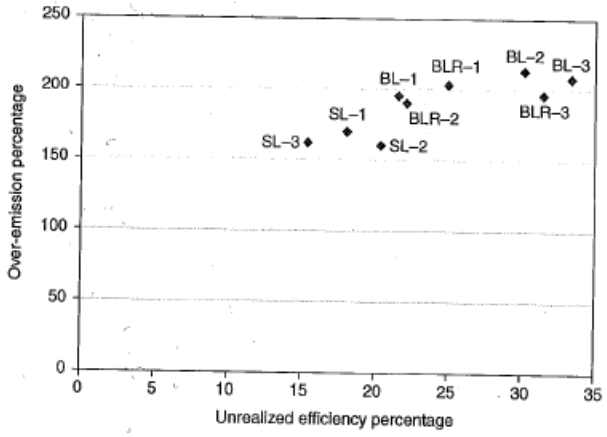
“Our analysis at the macrolevel suggests that, on average, ET budgets for phase 2 are about 12.8% lower than historical emissions in 2005, 12.9% lower than the budgets in phase 1 (2005-2007), and 15.7% lower than projected emissions in 2010. Thus the ET budgets for phase 2 are much stricter than for phase 1. … Prices for EUAs for phase 2 of currently around Euro20 support this view. … The tighter budgets for phase 2 are primarily the outcome of the EC’s decision to substantially cut the ET budgets in the notified NAPs [a centralized setting of the cap, AO] rather than the result of MS’ efforts to curtail greenhouse gas emissions using the EU ETS. … according to the EC’s proposal for phase 3, the future EU ETS will no longer require NAPs. Instead, there will be an EU-wide cap which corresponds to a reduction of 21% in 2020 compared to 1990 emission levels (or 14% compared to 2005 levels), … “ (p. 15)

“Phase 3 is scheduled to last for 8 years (2013-2020) rather than five. … Since longer phases better match companies’ investment cycles and reduce uncertainty about the profitability of new investments, they are likely to increase the diffusion and development of carbon- and energy-efficient technologies. Longer phases, however, also limit the system’s flexibility to react to unexpected developments, such as technological breakthroughs, sudden changes in climate policy, or improved knowledge about the causes and effects of climate change. … “ (p. 16)

“to sum up, the incentives for carbon and energy efficiency generated through the EU ETS have significantly improved at the macrolevel, but only slightly at the microlevel between phase 1 and phase 2. The EC’s proposal for phase 3 implies increased incentives for carbon and energy efficiency, in particular at the microlevel. … “ (p. 17)

Godby & Shogren, *Caveat emptor (buyer beware)* Kyoto – Comparing buyer and seller liability in carbon emission trading

- Kyoto Protocol requires that leading industrialized countries reduce their GHG emissions by an average of 5 percent below 1990 levels by 2008-12.
- ET allows regulated emitters to buy emission reduction efforts from other emitters
- The effectiveness of global trading depends on the rules of enforcement and sanctions for nations that shirk on their emission commitments.
- Domestic trading programs in the United States and elsewhere have relied on strong enforcement and sanctioning frameworks to ensure market compliance but that can not be relied on automatically in international contexts (see Phase 2 notified voluntary restrictions within the EU ETS)
- The Kyoto Treaty ignores enforcement (although it really is the main challenge)
- The critical issue: Who should be held responsible for overselling permits beyond quotas – the seller or the buyer country?
    - Weak under-compliance penalties and ineffective monitoring methods create the incentive for selling nations to oversell permits (and shirk on their emission reduction commitments)
    - An advantage of seller liability: there is only one price because for the buyer it does not matter where the licence to pollute comes from.
    - An advantage of buyer liability: buyers would have an incentive to ensure emission compliance through various means (including reputational enforecemment, collaterals of various kinds, etc.) Inevitably that would mean that we now talk about different prices because various risk factors would play a role in the determination of the price. (On the positive side, monitoring and enforcement costs could be dramatically reduced.) “The working hypothesis is that buyer liability leads to greater climate protection, as markets from to capture the gains from trade and reputations work to police market behavior.” (p. 49)
- The authors test experimentally the comparative advantages of three liability rules: seller, buyer, and buyer and refund (seller non-performance inflicts sanctions on buyers only, while sellers forfeit any permit revenues, say by way of escrow accounts [bonds].)
- Sneak preview: They find that “buyer liability under relatively weak international enforcement leads to the worst possible outcome – less climate protection at greater costs.” (p. 49) This result is robust to various robustness checks.
- The experimental details:
    - Stylized Kyoto emission market double auction experiment in which liability rules are the treatment
    - There is no calibration, the experiment is a “concept experiment”

- Results (somewhat surprising and contradicting intuition, especially as regards the BLR treatment):
    - o
    - 

    - o *Figure 3.2* Efficiency and emission outcomes by treatment.
    - o "Promoting a *caveat emptor* liability rule backfired in our experiment on both economic and environmental criteria. Holding the subjects that represented high-emission buyer nations responsible for climate shirking rather than holding the relatively poorer low-emission seller nation subjects responsible resulted in average emission levels exceeding those observed under seller liability by nearly 34 to 40 percent. The imposition of an escrow-like refund system did not alter this result; and neither did the introduction of tighter enforcement or conditions that could create stronger seller reputations. Our findings support the notion that buyer liability in global emission trading might lead to less climate protection at greater cost." (pp. 73-74)

On to new stuff:

- The Duhem-Quine problem:

„experimental results always present a joint test of the theory (however well articulated, formally) that motivated the test, and all the things you had to do to implement the test." (Smith EE 2002, p. 98) … the so-called auxiliary hypotheses …

- Why economists use financial incentives:

widespread belief among economists that they reduce performance variability

widespread belief among economists that increasing stakes tends to move experimental data closer to the theoretical prediction

The evidence seems to support that claim.

- Smith & Walker (1993) study
- Camerer & Hogarth (1999) study
- Hertwig & Ortmann (2001) studies
- Harrison & Rutstroem (1999/2008) study

Chapter 81

EXPERIMENTAL EVIDENCE ON THE EXISTENCE OF HYPOTHETICAL BIAS IN VALUE ELICITATION METHODS

GLENN W. HARRISON and E. ELISABET RUTSTRÖM

Handbook of Experimental Economics Results, Volume 1
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Table 1
Summary of findings of hypothetical bias

Study	Key features of bias comparison	Hypothetical bias	Statistically significant?
BH		−46%	no
JLJ	Conservative interpretation of CVM responses vs real; largest bias reported.	−43%	no
MSC	Smallest bias found at highest risk treatment.	−25%	?
Bohm	Comparison with auction institution.	0%	no
JLJ	Conservative interpretation of CVM responses vs real; smallest bias reported.	0%	no
SM		3%	no
DFG	Excluding team 2 and outlier identified in study.	8%	no
Bohm	Comparison of means; smallest bias reported.	16%	yes
JLJ	Standard interpretation of CVM responses vs real; smallest bias for lowest price.	19%	yes
Bohm	Comparison of medians; smallest bias reported.	25%	?
Griffin et al.	Comparison of proportion of "yes" responses.	29%	?
KMD	Private good.	30%	yes
DP	Means, excluding non-respondents.	35%	?
Bohm	Comparison of means; highest bias reported.	40%	yes
Frykblom	Dichotomous choice responses.	56%	yes
DFG	Comparison of "raw" means.	58%	no
BHK	Comparison of means.	60%	yes
Frykblom	Open-ended responses.	60%	yes
CEHM		67%	yes
DFG	Estimated demand functions; comparison of means; smallest bias reported.	69%	no
KMP	Public good.	100%	yes
Bohm	Comparison of medians; largest bias reported.	100%	?
MSC	Largest bias reported at lowest risk.	120%	?
CHR	Juicers; within-subjects.	163%	yes
CHR	Calculators; between-subjects.	163%	yes
BHK	Comparison of medians.	176%	?
DP	Comparison of means, assuming non-respondents have WTP = $0.	203%	?
Neill et al.	Painting; comparison of means.	290%	yes
DFG	Estimated demand functions; comparison of means; largest bias reported.	351%	no
Neill et al.	Painting; comparison of medians.	400%	?
JLJ	Standard interpretation of CVM responses vs real; highest bias reported.	701%	yes
CHR	Chocolates; within-subjects comparisons.	873%	yes
SS	Comparison of means.	2017%	?

(continued on next page)

Table 1
(continued)

Study	Key features of bias comparison	Hypothetical bias	Statistically significant?
Neill et al.	Map; HVA vs RVA; comparison of means.	2400%	yes
Neill et al.	Map; CVM vs RVA; comparison of means.	2600%	yes

Notes: The acronym for each study is provided in the References; bias measured as hypothetical minus real over real when positive, and real minus hypothetical over hypothetical when negative; statistical significance is determined at a one-sided 10% level, and a question mark indicates that there was insufficient information in the study to allow a simple determination.

[A little experiment:]

Choice between CzK 100 for sure and a lottery (CzK 0,.9;CzK 1,000,.1) whose expected value is CzK 100. Hypothetical and real.

Murphy and Stevens (M&S), Contingent Valuation (CV), Hypothetical Bias, and Environmental Economics (2004)

CV – contentious debate, recall litigation following the 1989 Exxon Valdez oil spill in the Prince William Sound

Contention stems from fact that surveys are hypothetical in both the payment for and provision of the good in question (which may be public but could also be private): “We do not know whether what an individual says she *would* do in the hypothetical setting matches what she *will* do when actually given the opportunity to do so.” (p. 180)

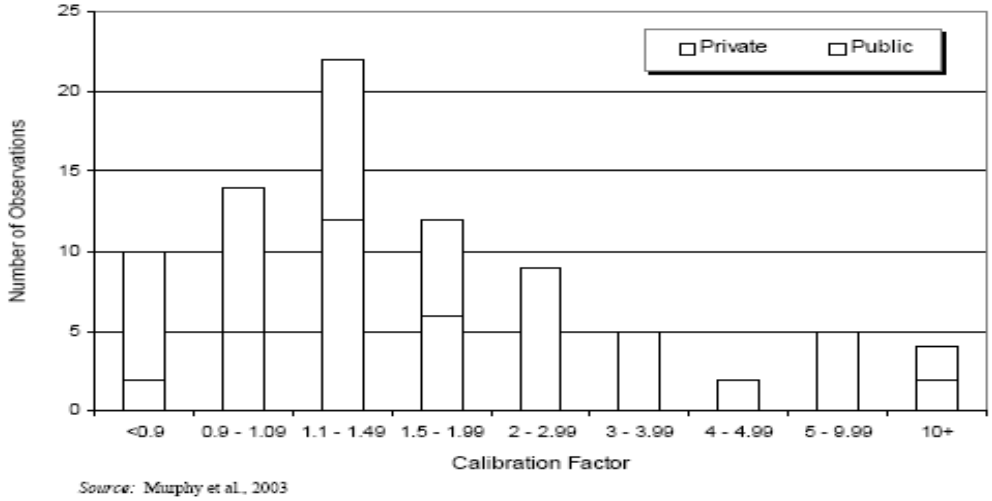
The usual concern (that seems to be borne out in many studies) that people will over-state their WTP: “As a matter of logic, if you do not have to pay for the good but a higher verbal willingness to pay (WTP) response increases the chance of its provision, then verbalize away to increase your expected utility. Similarly, if your verbal statement of a higher willingness to accept (WTA) compensation might reduce the risk that the commodity will be taken away from you verbalize as high an amount as you want.” (Harrison and Rutstroem ~ 1999/2008, p. 752)

Experiments to the rescue? Why because “unlike a typical field CV survey, a carefully designed experiment can include both hypothetical and real payment scenarios. By comparing outcomes in these two settings, one can make some inferences about the existence of hypothetical bias, its causes, and ways to mitigate its effects.” (p. 183) -> “induced values” rather than “homegrown values” (“most valuation experiments … do not use induced values”, p. 185), calibration

M&S mention the Champ et al. study on WTP for Grand Canyon road removal; one set of participants was asked a hypothetical contingent valuation donation question, another set was asked for actual contributions for road removal. “Both surveys made clear that the project was the only source of funding for the program. The estimated mean WTP in the hypothetical treatment ($46 - $89) was significantly greater than the mean actual contributions ($9).” (p. 185) It is the real payment control treatment that normally does not exist in survey situations.

“Generally, it is the body of experimental evidence, rather than a single study that allows us to draw more reliable conclusions about what we do and do not know.” (p. 183)

Here are the results of one of two meta-studies (actually this one updating and correcting the earlier one), based on 83 observations from 23 studies:



Source: Murphy et al., 2003

Figure 1. Distribution of calibration factors by type of good (where calibration factor = hypothetical value/actual value)

- Findings:
  - The mean hypothetical value about 2.5 to 3 times greater than actual values
  - The median hypothetical value about 1.5 (smaller because of outliers to the right)
  - About one half of the valuation experiments use private goods, the other half use public goods; see p. 187.
  - Hypothetical bias seems to exist for private and public goods (p. 184, Fig. 1) although there is no agreement whether indeed hypothetical bias is lower for private goods as List & Gallet (2001) claim, M&S (2004, p. 184) argue that the evidence is mixed but note that "as shown by Figure 1, private goods, in fact have a larger share of calibration factors which exceed 1.5 (54% vs. 36%). However, it is worth noting that most of the very large calibration factors (> 5) are from public good experiments." (p. 188)
    - Public goods is what we are really interested in (but …)
    - Private goods, on the other hand, are familiar in a narrow and broad sense, and -- because of their very nature -- are not afflicted by biases due to free-riding

So, what to do now that we know that hypothetical bias is a problem but that the problem can't be captured easily in one point estimate?

- NOAA (National Oceanic and Atmospheric Administration) panel of experts: we need better ways to "calibrate" the hypothetical bias
- Blackburn, Harrison, and Rutstroem (1994): here you are ! (they created a statistical bias function using socioeconomic characteristics [from an earlier experiment, Cummnings, Harrison, and Rutstroem] to estimate the extent to which people overstate their value.)
- Unfortunately, that is also not completely solving the problem because people may have different degrees of uncertainty about their valuation, hypothetical bias might be sensitive to the specific good being investigated (not just the type of good), and so on.
- So, people have been searching for other calibration techniques such as "cheap talk" (pp. 186 – 7) . Here, essentially, you alert participants to the likely existence of hypothetical bias and hope that they create, so to speak their own bias function: "According to the premise behind this technique, simply making respondents aware of hypothetical bias, regardless of underlying causes, is sufficient to eliminate it." (p. 187) Unfortunately, "the evidence about cheap talk's robustness is mixed .. " (p. 187) Even the length of a script seems to play a role.

It really would help to know why hypothetical bias exist in the first place? What causes it? What are its determinants?

- "The experimental valuation literature seems to have focused more on developing better CV instruments ad calibration techniques without directly addressing the underlying sources of the bias." (p. 187)
- The argument that Harrison and Rutstroem made (see above; a positive response to the valuation question increases the probability of provision of a public good) does clearly not apply to private goods, so it cannot be the whole story.
- Nor do respondent uncertainty or ambivalence theory (don't bother)

Here is the authors' conclusion:

Let's take stock of what we do and do not know. It has been well-established that hypothetical values exceed actual values, and calibration techniques have had some success in aligning these values. Less is known about the underlying causes of these differences in values. The inconsequential nature of the CV survey cannot alone explain these differences; it might explain greater variability in hypothetical responses, but not higher mean and median responses. Therefore, there must be an interaction of the hypothetical decision with other factor(s). Expressing positive sentiment toward the good, or an attempt to increase the likelihood of the good's provision, is certainly possible in a public goods setting, but this fails to explain why hypothetical bias also exists with private goods since these factors are not applicable. There is some evidence that respondents may be uncertain, but the causes of this uncertainty and its implications for valuation are not well understood. Ultimately, hypothetical bias is likely to be individual-specific and a composite of a variety of factors. Experiments can play a key role in developing a better understanding of what causes hypothetical bias and can assist researchers in designing CV instruments which incorporate these considerations.

Alternative explanations?

Harrison, Harstad, and Rutstroem (2004)

- Values elicited in the lab may be censored by the prevailing market price (obviously, this issue pertains for the most part to private goods)
- An individual might truthfully report her maximum WTP in a hypothetical setting but censor her actual WTP at a (perceived) market price: "A rational subject will not agree to obtain the same commodity in an experiment at a price that he perceives can be beaten outside the lab with sufficiently high probability." (p. 124)

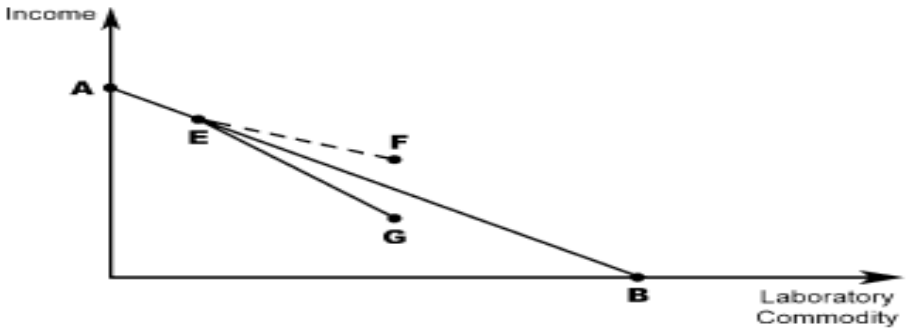


Figure 1. Frictionless field market.

- V = a homegrown value for the commodity we are interested in

- Dichotomous choice question: “Would you be willing to buy one unit of the commodity at a price X?” (The “normatively correct” answer would be “yes”, if V > X, or “no” if V < X.)
- Take bridge-toll tokens as example … in that case, the rational response is “no” if X > P, and “yes” if X < P.
- Figure 1:
    - E and A are initial endowment points (A is endowment point if participant has not consumed the commodity in the field) …
    - X < P -> EF (expands the budget set) -> accept
    - X > P -> EG (contracts the budget set) -> reject (even if V > X)
    - In both cases, the field price acts to censor the private values. “Hence, in this ideal frictionless market a dichotomous choice question *elicits no information whatsoever* about a rational subject’s value.” (p. 126)
- Essentially the same it true if we get rid of the no-friction assumption (although things do get a bit more complicated):

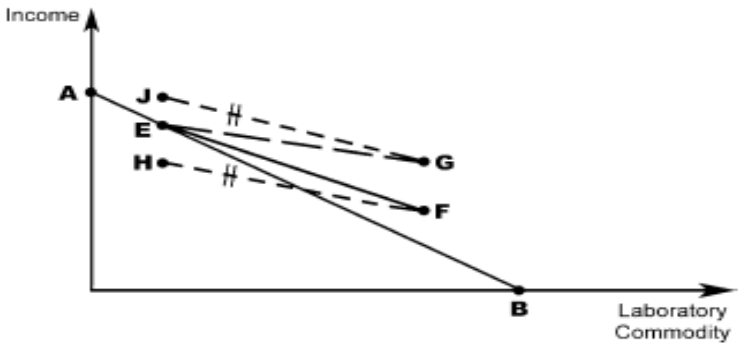


Figure 2. Resale below purchase price.

- Figure 2:
    - E and A as before
    - Changed assumptions:
        - Allow quantity responses between 0 and M
        - Resale price, R, less than field price, P.
        - Still no transaction cost
    - X > P -> EG (contracts the budget set) -> reject (even if V > X)
    - X < P -> EF (expands the budget set) -> accept
        - Two cases here

- R < X < P : participant can move all the way out to F (buying M units) but will not necessarily do so.
    - reselling would move her down FH, which would make her worse off
    - so, here no censoring
- X < R < P : participant moves all the way out to G (buying again M units) because …
    - Reselling then would move her down GJ but leave her in any case better off
    - So, here again censoring

- "To conclude on the case where the field resale value [R] is less than the field purchase price [P], then, a rational subject's response reflects *his private valuation accurately* only when the laboratory prices are set *below* the field purchase price yet *above* the field resale price." (p. 127)

- of course, the situation is even trickier since there is typically not one price but many, so a participant might have quite diffuse priors and therefore might search for cues (such as stated values by other subjects); experimentally, this implies that one has to make sure that participants are not being influenced through value elicitation; that's the issue of affiliated beliefs.

- The importance of these considerations is shown by way of a reanalysis of the Hoffman et al. (1993) field experiment … (which shall not interest us here since the basic problem of values elicited in the lab possibly being censored by the prevailing market price is the major issue that needs to be understood)

So, some final words then on WTP and WTA and the alleged divergence of the two …

List, Neoclassical Theory Versus Prospect Theory: Evidence from the Marketplace (Econometrica 72.2, 615 - 625)

What's the behavioral pattern called endowment effect?

And why are endowment effect and prospect theory synonyms? (See fn 2)

What would be a typical study showing the endowment effect? (Knetsch 1989)

- Most CU students keep what they were given (mugs, chocolate, etc.)

TABLE 1

SELECTED CHARACTERISTICS OF PARTICIPANTS

	Dealers Mean (Std. Dev.)	Nondealers Mean (Std. Dev.)	Nondealers Mean (Std. Dev.)
Trading intensity	11.81	4.94	6.88
	(10.9)	(6.58)	(6.39)
Yrs. of market experience	9.88	7.15	7.21
	(9.79)	(9.83)	(8.03)
Income	4.15	4.10	4.18
	(1.75)	(1.69)	(1.81)
Age	36.55	34.54	37.04
	(13.1)	(14.41)	(14.1)
Gender (% male)	.94	.85	.82
	(.24)	(.35)	(.39)
Education	3.54	3.44	3.54
	(1.40)	(1.33)	(1.54)
Sample Sizes:			
Private			
Treatment E_{candybar}	30	31	—
Treatment E_{both}	32	30	—
Treatment E_{neither}	35	33	—
Treatment E_{mug}	32	30	—
Public			
Treatment E_{candybar}	—	—	33
Treatment E_{both}	—	—	28
Treatment E_{neither}	—	—	29
Treatment E_{mug}	—	—	35

Notes: 1. Trading intensity represents the number of trades made in a typical month. 2. Yrs. of market experience denotes years that the subject has been active in the market. 3. Income denotes categorical variable (1–8): (1) Less than $10,000, (2) $10,000 to $19,999, (3) $20,000 to $29,999, (4) $30,000 to $39,999, (5) $40,000 to $49,999, (6) $50,000 to $74,999, (7) $75,000 to $99,999, (8) $100,000 or over. 4. Age denotes actual age in years. 5. Gender denotes categorical variable: 0 if female, 1 if male. 6. Education denotes categorical variable (1–6): (1) Eighth grade or less, (2) High School, (3) 2-Year College, (4) Other Post-High School, (5) 4-Year College, (6) Graduate School Education. 7. "Private" and "Public" sample sizes denote the number of subjects in Experiments 1A and 1B, respectively.

TABLE II

SUMMARY OF EXPERIMENTAL RAW DATA

	Number of Subjects Choosing Candy Bar	Number of Subjects Choosing Mug	Pearson χ^2
Panel A. Nondealers (Private)			
Treatment E_{candybar}	25 (81%)	6 (19%)	19.21 (3 df)
Treatment E_{both}	18 (60%)	12 (40%)	
Treatment E_{neither}	15 (45%)	18 (55%)	
Treatment E_{mug}	7 (23%)	23 (77%)	
Panel B. Nondealers (Public)			
Treatment E_{candybar}	29 (88%)	4 (12%)	34.79 (3 df)
Treatment E_{both}	16 (57%)	12 (43%)	
Treatment E_{neither}	17 (59%)	12 (41%)	
Treatment E_{mug}	6 (17%)	29 (83%)	
Panel C. Dealers (Private)			
Treatment E_{candybar}	14 (47%)	16 (53%)	.54 (3 df)
Treatment E_{both}	14 (44%)	18 (56%)	
Treatment E_{neither}	18 (51%)	17 (49%)	
Treatment E_{mug}	14 (44%)	18 (56%)	
	Preferred Exchange		p-Value for Fisher's Exact Test
Panel D. Trading Rates			
Pooled nondealers ($n = 129$)	.18 (.38)		$< .01$
Inexperienced consumers (< 6 trades monthly; $n = 74$)	.08 (.27)		$< .01$
Experienced consumers (≥ 6 trades monthly; $n = 55$)	.31 (.47)		$< .01$
Intense consumers (≥ 12 trades monthly; $n = 16$)	.56 (.51)		.64
Pooled dealers ($n = 62$)	.48 (.50)		.80

Notes: 1. The Pearson chi-square tests in Panels A–C are distributed with 3 degrees of freedom and each have a null hypothesis of Hicksian preferences. 2. Data in Panel D are pooled from Treatments E_{candybar} and E_{mug}. For nondealers, data from "public" and "private" are pooled. Standard deviations are in parentheses. 3. Experienced consumers are those consumers who trade 6 or more times per month (6 is roughly the mean level of monthly trades). Intense consumers trade 12 or more times per month (12 is roughly the mean plus one standard deviation). 4. Fisher's exact test in Panel D has a null hypothesis of no endowment effect.

Here is what Plott & Zeiler say in their (2005) article about List's study:

[4] For example, List (2003, 2004) finds that choice asymmetries differ across subject pools, and attributes this difference to variation in experience. In particular, he posits two theories, which depend on whether subjects are choosing between unique goods or "everyday consumable goods." In the case of unique goods, he theorizes that "experienced subjects are more certain of their preferences" and thus "[l]esser-experienced agents may keep their endowed good simply to avoid making embarrassing mistakes" (List 2004, 617). In the case of everyday consumable goods, he suggests that experience makes the subject more likely to view traded endowments as opportunity costs rather than losses (List 2004, 624). The results reported in the present paper suggest other possible explanations for observed asymmetries in his experiments. For example List's procedures allow for experimenter involvement in the choice of the endowed good, which, according to our results, can signal relative quality. Thus, our results, together with List's results, can be interpreted as suggesting that behavior was driven by the combination of experience with trading together with reactions to subtle signals of relative quality. If experienced subjects are more confident in their own abilities to assess quality (no matter what the good), they might rely less on signals to update their beliefs about the quality of goods. In addition, a host of alternative theories can be derived from the fact that inexperience is known to operate along several different channels (e.g., see Plott 1996), any one of which could lead to asymmetric choices. While List controlled several variables we identified as important, others left uncontrolled are potential explanations of observed choices.

One can formulate other candidate theories, based on various features of List's experiments, to explain his observations. For example, to understand the actual effect of experience on the propensity to resist giving up endowments, it seems important to rule out selection effects. It could be that those with higher levels of confidence in their own ability to judge the quality of goods trade more relative to those who have lower confidence levels. Therefore, the act of trading might do little to change the perception of the good in the eyes of the owner. Conducting experiments in the field makes it difficult, if not impossible, to control for selection effects. In the lab, however, subjects can be randomly assigned to different groups, some of whom gain experience during the experiment and some of whom do not. In general, the lab offers virtually unlimited opportunities to control for a multitude of variables with the goal of identifying the theory that best explains the data.

Plott and Zeiler (2005) [PZ 2005], The Willingness to Pay-Willingness to Accept Gap, the „Endowment Effect“, Subject Misconceptions, and Experimental Procedures for Eliciting Valuations (American Economic Review 95.3, 530 - 545)

p. 530 -1, fn. 1: „ ... For this reason, WTP-WTA gaps have come to be called ‚endowment effects‘. We refer to this explanation of gaps as ‚endowment effect theory‘ to denote that the terminology is not simply a label for a particular phenomenon, but rather implies a theoretical explanation of the observed phenomenon.“

What did PZ2005 try to accomplish (and how did they go about it)?

- To correct „an incorrect interpretation of experimental results“ (p. 531):
  „The endowment effect and loss aversion has been one of the most robust findings of the psychology of decision making – people commonly value losses much more than commensurate gains.“ (Knetsch et al. 2001; see also Rachlinski & Jourden and Horowitz et al quotations on same page)

- To design an experiment that completely controls for „subject misconceptions“ (a „close cousin“ to subject confusion): „If we design an experiment that completely controls for subject misconceptions as implicitly defined by the literature (i.e. an experiment that includes every procedure used in previous experiments to control for misconceptions), will we observe a WTP-WTA gap?“ (p. 532)

  - replicate experiments that typically produce the WTP-WTA gap, to establish benchmark
  - „conduct experiments in which subject misconceptions are completely controlled by incorporating the union of procedures found in the literature.“ (p. 532)

It is shown that these experimental controls (the five enumerated in the top row of Table 1) together eliminate the WTP-WTA gap: „When a full set of controls is implemented, the gap is not observed.“ (PZ2005, 542)

“Shaded cells [in Table 1] indicate that the experimenters used a theoretically incentive-compatible mechanism to elicit valuations.” (p. 534)

TABLE 1—SUMMARY OF THE LITERATURE BY EXPERIMENTAL DESIGN

	Result reported	Optimal responses explained	Practice rounds performed	Valuations elicited using incentive compatible mechanism	Valuations elicited using market environment with some incentives	Gap measured directly using valuations
Knetsch and Sinden (1984; test 1)	gap					
Knetsch and Sinden (1984; test 3)	gap					
Corsey et al. (1987; part 1)	gap					
Corsey et al. (1987; part 2)	gap					
Brookshire and Corsey (1987; exp 1)	gap					
Singh (1991; test 1 before learning)	gap					
Dubourg et al. (1994)	gap					
Brookshire and Corsey (1987; exp 2)	gap					
Knetsch (1989; test 1)	gap				binary choice	
Bateman et al. (2001)	gap				binary choice	
Shogren et al. (1994; stage 1, round 1)	gap				Vickrey	
Boyce et al. (1992)	gap		hypothetical		BDM	
Knetsch (1989; test 2)	gap		hypothetical		BDM	
Morrison (1997; part 2)	gap		random/pooled		BDM	
Shogren et al. (1994; stage 2)	gap		random/pooled		Vickrey	
Bateman (et al. (1997; exp 2)	gap		random/pooled		BDM	
Bateman et al. (1997; exp 1)	gap		random/pooled		BDM	
Knetsch and Sinden (1984; test 2)	gap					
Brookshire and Corsey (1987; exp 3)	gap				Smith auction	
Kahneman et al. (1990; exp 6 & 7)	gap	incorrectly suggested			sealed bid	
Franciosi et al. (1996; exp 1)	gap	incorrectly suggested			sealed bid	
Kahneman et al. (1990; exp 1 & 2)	gap	incorrectly suggested	random		sealed bid	
Kahneman et al. (1990; exp 4)	gap	incorrectly suggested	random/pooled		sealed bid	
Kahneman et al. (1990; exp 3)	gap	incorrectly suggested	pooled		sealed bid	
Loewenstein and Issacharoff (1994; exp 1)	gap	suggested			BDM	
Kahneman et al. (1990; exp 5)	gap	suggested	hypothetical		BDM	
Shogren et al. (2001; BDM)	gap	suggested	random/pooled		BDM	
Knetsch and Sinden (1984; test 4)	no gap					
Singh (1991; test 2 before learning)	no gap					
Singh (1991; tests 1 and 2 after learning)	no gap				DA	
Shogren et al. (1994; no available substitute)	no gap		random/pooled		Vickrey	
Corsey et al. (1987; part 3)	no gap				Vickrey	
Morrison (1997; part 1)	no gap		random/pooled		BDM	
Shogren et al. (1994; stage 1, rounds 2–5)	no gap		random/pooled		Vickrey	
Shogren et al. (1994; available substitute)	no gap		random/pooled		Vickrey	
Arien et al. (2002)	no gap		random/pooled		binary choice	
Shogren et al. (2001; Vickrey)	no gap		random/pooled		Vickrey	
Loewenstein and Issacharoff (1984; exp 2)	no gap	suggested			BDM	
Harless (1989)	no gap		pooled		Vickrey	

Notes: Optimal response explained: If blank, no explanation was provided. "Incorrectly suggested" indicates that the experimenter used a non-incentive-compatible elicitation mechanism but told subjects that revealing true valuations was the optimal strategy. "Suggested" indicates that the experimenter correctly advised subjects that the optimal strategy called for truthful valuation revelation. If shaded, the experimenter provided a detailed explanation of the optimal response.

Practice rounds performed: If blank, then no practice rounds were performed. "Hypothetical" indicates that practice rounds were not paid. "Random" indicates that randomly selected rounds were paid. "Pooled" indicates that the measurement of the gap includes valuations measured in the first round (without experience) and valuations measured in later rounds (after experience).

Valuations elicited using incentive-compatible mechanism: If blank, then non-incentive-compatible mechanism used to elicit valuations. If shaded, then incentive-compatible mechanism used to elicit valuations.

Valuations elicited using market environment with some incentives: If blank, elicitation was not conducted in a market environment. If shaded, then elicitation was conducted in a market environment with some incentives. The type of market environment is indicated for each experiment.

Gap measured directly using valuations: If blank, then gap measured using number of trades relative to predicted number of trades. If shaded, then gap measured using mean or median of actual WTP and WTA responses.

After this (suggestive) literature survey, PZ 2005 go out and

- replicate the KKT design and results (pp. 535 – 536, in particular Table 2)
- implement the Plott-Zeiler procedures (pp. 536 – 542, in particular Tables 3 and 4)

What are the replication results?

TABLE 2—INDIVIDUAL SUBJECT DATA AND SUMMARY STATISTICS FROM KKT REPLICATION

Treatment	Individual responses (in U.S. dollars)	Mean	Median	Std. dev.
WTP (n = 29)	0, 0, 0, 0, 0.50, 0.50, 0.50, 0.50, 0.50, 1, 1, 1, 1, 1, 1.50 2, 2, 2, 2, 2, 2.50, 2.50, 2.50, 3, 3, 3.50, 4.50, 5, 5	1.74	1.50	1.46
WTA (n = 29)	0, 1.50, 2, 2, 2.50, 2.50, 3, 3.50, 3.50, 3.50, 3.50, 3.50, 4, 4.50 4.50, 5.50, 5.50, 5.50, 6, 6, 6, 6.50, 7, 7, 7, 7.50, 7.50, 7.50, 8.50	4.72	4.50	2.17

What are the PZ procedures?

“We designed the procedures explicitly to control for concerns identified in the literature. In particular, Table 1 suggests that a gap is observed less often when an incentive-compatible mechanism is used to elicit valuations, and training and paid practice rounds are provided. Table 1 also reveals the absence of a particular and important set of procedures. Our analysis of the literature reveals that no one experiment designed to study WTP-WTA gaps implements a complete set of controls: an incentive-compatible elicitation device, training, paid practice, and anonymity. We fill this void with our experiment design.” (PZ 2005, pp. 536 – 7)

- incentive compatible elicitation device (BDM)

[9] The BDM mechanism pits each seller and buyer against a random bid. All sellers stating bids lower than the random bid sell the good, but receive an amount of money equal to the random bid. All buyers stating bids higher than the random bid buy the good, but pay an amount of money equal to the random bid. Sellers who bid higher than the random bid, and buyers who bid lower than the random bid, do not transact.

- training in understanding the mechanism used to elicit valuations
- (paid) practice rounds (two)
- anonymity

Results?

TABLE 4—INDIVIDUAL SUBJECT DATA AND SUMMARY STATISTICS

Experiment	Treatment	Individual responses (in U.S. dollars)	Mean	Median	Std. dev.
Experiment 1: (USC/practice)	WTP ($n = 15$)	0, 1, 1.62, 3.50, 4, 4, 4.17, 5, 6, 6, 6.50, 8, 8.75, 9.50, 10	5.20	5.00	3.04
	WTA ($n = 16$)	0, 0.01, 3, 3.75, 3.75, 3.75, 5, 5, 5, 6, 6, 6, 7, 11, 12, 13.75	5.69	5.00	3.83
Experiment 2: (USC/no practice)	WTP ($n = 12$)	1, 2, 3.50, 5, 5, 5, 8, 8.50, 9, 11.50, 13, 23	7.88	6.50	6.00
	WTA ($n = 14$)	0.50, 1, 2, 2.50, 2.50, 4.50, 4.50, 5.70, 6.25, 8, 8, 8.95, 12, 13.50	5.71	5.10	4.00
Experiment 3: (PCC/practice)	WTP ($n = 9$)	2.50, 5.85, 6, 7.50, 8, 8.50, 8.50, 8.78. 10	7.29	8.00	2.23
	WTA ($n = 8$)	3, 3, 3.50, 3.50, 5, 5, 7.50, 10	5.06	4.25	2.50
Pooled data	WTP ($n = 36$)		6.62	6.00	4.20
	WTA ($n = 38$)		5.56	5.00	3.58

Notes: Experiments 1 and 3 used the BDM mechanism to elicit responses and employed paid practice, training, and anonymity. Experiment 2 used the BDM mechanism to elicit responses and employed training and anonymity (without paid practice rounds).

„Interpreting gaps as support for endowment effect theory [which holds that the utility function has a ‚kink' in which gains are evaluated differently than losses] is problematic. The mere observation of the phenomenon does not support loss aversion ... That the phenomenon can be turned on and off while holding the good constant supports a strong rejection of the claim that WTP-WTA gaps support a particular theory of preferences posited by PT." (PZ 2005, 542)

Plott and Zeiler (2007) [PZ 2007], Exchange Asymmetries Incorrectly Interpreted as Evidence of Endowment Effect Theory and Prospect Theory? (American Economic Review 97.4., 1449 - 1466)

PZ2007 is an attempt to further investigate the factors that contribute to the appearance of the WTP-WTA gap (and therefore to the alleged existence of the endowment effect). As to PZ (2005) they conclude: „The data support the conclusion that observed WTA-WTP gaps are caused by subject misconceptions resulting from the use of special mechanisms required to elicit valuations. The results suggest that endowment effect theory cannot explain data from that class of experiments," (p. 1450)

Why "endowment effect theory" [which denotes a theoretical explanation and distinguishes it from the observed phenomenon]?

Because the nature and causes of "endowment effects" [or, the exchange asymmetries] are hypothesized to be explainable by prospect theory: specifically it's a particular aspect of prospect theory - loss aversion associated with an endowment – that is hypothesized to lead to asymmetries in valuations and exchange behavior. "To say that an observed phenomenon demonstrates an 'endowment effect' does not simply denote that an asymmetry was observed; rather, use of the label implies that a very special form of preferences causes the asymmetry." (p. 1449) "Endowment effect theory holds that the utility function includes a 'kink,' which leads to differing evaluations of gains and losses. … Endowment effect theory should not be confused with the theories about the potential role of ownership in the creation of features of goods that hold special values, such as sentimental value, emotional attachment, familiarity, etc. ["attachment theory"]" (p. 1453)

A second body of evidence (also going back to the Knetsch 1989 study) avoids complex elicitation procedures that were source of the subject misconceptions that were the object of PZ (2005); thus the PZ (2005) explanation does not seem of relevance to this second body of evidence.

Target of PZ2007 are four facets of experimental procedures:

A. Method and Language Used to Endow Subjects

B. Suggestions of Relative Value
C. Location of Endowed Good at Time of Choice
D. Public Revelation of Choices

Ad A. It might matter to subjects whether they know that subject is chosen randomly or by the experimenter (conversational implicature!)

For example, the message, “X is yours. You own X. I am giving X to you.” might be perceived by subjects as indicating that X is a gift from the experimenter (although that might not have been what the experimenter intended to communicate. Likewise the subject might imply that the good has some special value since the experimenter gave it to the subject.

Hence, first procedure: “I’m giving you X. It is a gift. You own it. It is yours.”

Hence, second procedure: Subject told X determined by flip of a coin before the start of the experiment. [Asymmetry in first case but not second]

Ad B. “Experimenters, intent on emphasizing entitlement, possibly establish more than a reference point by inadvertently signaling that the endowed good is more valuable than the alternate good.” (p. 1455)

Hence, second procedure - 1: “These X’s are yours.”

Hence, second procedure - 2: “Please circle the item you wish to take home with you.” {Available options were: “X”, “Y”, “I DON’T CARE”

For corresponding first procedures see p. 1455

Ad C. …

Hence, first procedure: endowed good placed immediately in front of the subjects

Hence, second procedure: alternate good, rather than endowed good, within reach of subjects

Ad D: … information cascades, subjects interpreting the choices of others as signals of value?

Hence, first procedure: public choices

Hence, second procedure: private choices

Two key treatments (Procedures):

- baseline
- full set of procedural controls

The results?

TABLE 1—DESIGN FEATURES AND CONTINUUM OF RESULTS
(From no asymmetry to statistically significant asymmetry)

	Full set of procedural controls	Loss emphasis test	Standard procedures	Transaction costs test	Baseline procedures
Endowed good immediately in front of subject at time of choice	NO	YES	YES	NO	YES
Experimenter chose and gave OR randomly assigned which good to endow	RANDOMLY ASSIGNED	RANDOMLY ASSIGNED	EXPERIMENTER CHOSE AND GAVE	EXPERIMENTER CHOSE AND GAVE	EXPERIMENTER CHOSE AND GAVE
Experimenter purposefully and repeatedly emphasized ownership	NO	NO	YES	YES	YES
Choices made by public show of hands OR use of private forms	FORMS	FORMS	HANDS (TRADE ENDOWED GOOD)	FORMS	HANDS (KEEP ENDOWED GOOD)
(# mug owners, # pen owners)	(69, 70)	(44, 43)	(44, 52)	(53, 48)	(64, 65)
(# mug owners who chose mugs, # pen owners who chose mugs)	(37, 47)	(36, 32)	(34, 32)	(38, 24)	(54, 18)
(Percent mug owners who chose mugs, percent pen owners who chose mugs)	(54 percent, 67 percent) diff = −13 percent	(82 percent, 74 percent) diff = 8 percent	(77 percent, 62 percent) diff = 15 percent	(72 percent, 50 percent) diff = 22 percent	(84 percent, 28 percent) diff = 56 percent
Result[†]	$p = 0.94$[‡]	$p = 0.18$	$p = 0.06$	$p = 0.01$	$p = 0.00$

Notes. Overall, we observed a general preference for the mug. Of the 618 subjects that participated (including pilots), 398 (or 64 percent) chose mugs. This is statistically significantly greater than 50 percent ($p = 0.00$). This general mug preference, however, does not affect our results as we measured asymmetries by comparing the percentage of mug owners who chose mugs and the percentage of alternate good owners who chose mugs. This measurement controls for the overall mug preference.

[†] Results are from two-sample tests of equality of proportions (null hypothesis: proportions are equal; alternate hypothesis: percent mug owners who chose mugs > percent of pen owners who chose mugs).

[‡] If we use an alternate hypothesis of H_a: percent of mugs owners who chose mugs < percent of pen owners who chose mugs, the p value is equal to 0.06. This (weakly) supports a hypothesis that a "reverse" asymmetry exists.

Key result: While there is a statistically significant exchange asymmetry for the "Baseline procedures" treatment, there is none (or, even a reverse) exchange asymmetry for the "Full set of procedural controls" treatment.

"These results, taken together, support the conjecture that exchange asymmetries result from classical preference theories working through the experimental procedures and cannot be explained by endowment effect theory or prospect theory." (p. 1460)

The additional treatments between "Full set of procedural controls" and "Baseline procedures" are meant to identify the relative importance of the various facets of the experimental procedures (A – D) … i.e.. which specific procedures exhibit what influence over choices.

"In sum, … while procedures clearly have a strong influence on the asymmetry of choices, any inferences about how particular features of the procedures affect choices are only conjectures at this stage. … One thing is clear, however: our results demonstrate that endowment effect theory cannot explain observed asymmetries." (p. 1462)