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Abstract

Meta-analysis has increasingly been used to synthesise the environmental valuation literature, but only a few test the use of these analyses for benefit transfer. These are typically based on national studies only. However, meta-analyses of valuation studies across countries are a potentially powerful tool for benefit transfer, especially for environmental goods where the domestic literature is scarce. We test the reliability of such international meta-analytic transfers, and find that even under conditions of homogeneity in valuation methods, cultural and institutional conditions across countries, and a meta-analysis with large explanatory power, the transfer errors could still be large. Further, international meta-analytic transfers do not on average perform better than simple value transfers averaging over domestic studies. Thus, we question whether the use of meta-analysis for practical benefit transfer achieves reliability gains justifying the increased effort. However, more meta-analytic benefit transfer tests should be performed for other environmental goods and other countries before discarding international meta-analysis as a tool for benefit transfer.

Keywords: benefit transfer, environmental valuation, meta-analysis, forest.
JEL Classification: Q51, H41.

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Introduction

Meta-analysis (MA), "the study of studies", is now common in environmental economics and non-market valuation (Smith and Pattanayak, 2002). Since Smith and Karou's (1990) seminal study of recreational benefits, MA has been conducted for a wide range of environmental goods, from wetlands (Woodward and Wui, 2001) to visibility (Smith and Osborne, 1996). Common to all of these studies is the focus on research synthesis and hypothesis testing, rather than on the more interesting policy question of how MA can be used to improve benefit transfer (BT) practices. Meta-analytic benefit transfer (MA-BT) to unstudied sites ("policy sites") has only been cursory treated in the literature, typically a few pages add-ons at the end of lengthy MA papers, although authors emphasise its potential importance for future research and applications, for example in cost-benefit analysis (CBA) (see the special issue on benefit transfer in Ecological Economics 2006). While there is some knowledge of how unit value and value function-based BT from single studies perform (Rosenberger and Phipps, 2007), Bergstrom and Taylor (2006:359) point out that "before widespread application of MA-BT models, there is a need for additional MA-BT convergent validity tests across different types of natural resources and environmental commodities." Only a few studies have, to our knowledge, investigated the validity and reliability of MA-BT (Santos, 1998; Rosenberger and Loomis, 2000b; Shrestha and Loomis, 2001; 2003; Santos, 2007; Shrestha et al., 2007). Four of the studies, however, are based on the same large dataset of use values for different recreational activities in the USA[2], and are unable to cover the breadth of issues involved in more typical MA-

[2] A recreational database originally assembled for the US Forest Service maintained over 20-30 years, supplemented with additional data collected for the purpose of validity testing (see e.g. Rosenberger and Loomis (2001)).

BT exercises, i.e. limited datasets, complex goods with significant non-use values, different level of methodological heterogeneity and mix of international studies to mention a few. Santos (2007) is the only study attempting a comprehensive comparison of two versions of a domestic MA-BT with simple BT techniques often used in practice. Further, all the above studies can be said to under-appreciate the potential impacts on the MA-BT performance of model specifications, values of methodological variables (Johnston et al., 2006) and other choices the meta-analyst needs to make (Hoehn, 2006)[3].

This paper aims to investigate the validity and reliability of international MA-BT of Non-timber benefits (NTBs) based on a recently published MA of contingent valuation (CV) research in Fennoscandia (a term for Norway, Sweden and Finland) (Lindhjem, 2007). Compared to previous research on MA-BT, our paper adds several new and interesting dimensions: (i) a more systematic and diverse testing of different MA-BT models, including comparisons with simple BT techniques, (ii) the good we investigate is complex and has substantial non-use values related to biodiversity (rather than mainly use values), (iv) data from three European countries, which are similar culturally, economically, institutionally (e.g. everyman´s right to walk in private forests), and in the way the good is perceived and used, and (v) data are generally more homogenous methodologically since only CV studies are included. We investigate the transfer error (TE) of four different meta-regression model specifications, and use the best two models to compare MA-BT with simple unit value transfer techniques. A key question is whether the use of MA-BT achieves reliability gains justifying the increased effort.

[3] A alternative to the classical MA approach, not considered here, uses Bayesian modeling techniques to address some of these challenges, for example treatment of methodological variables and difference in availability of regressors across source studies (Moeltner et al., 2007)

As pointed out by Navrud and Ready (2007a:288): “Simple approaches should not be cast aside until we are confident that more complex approaches do perform better”.

Validity and reliability of meta-analytic benefit transfer

Underlying theory of MA-BT

The simple underlying indirect utility function for a change from Q_0 to Q_1 in the quality/quantity vector describing an environmental good available for individual *i* is:

$$V_i\,(p_i, I_i, Q_0) = V_i\,(p_i, I_i\text{-}WTP, Q_1) \qquad (1)$$

where P_i, I_i are a market price vector and income, respectively, and WTP is Willingness-to-Pay. Since indirect utility functions are homogenous of degree 0, identical individuals from two countries using different currencies will have the same real WTP only if they have the same real income and faces the same real prices. Thus the appropriate exchange rate to use for conversion is the purchasing power parity (PPP) (Ready and Navrud, 2006). Equation (1) solved for WTP, yields the bid-function that forms the (often implicit) basis for any MA-BT exercise. Following Bergstrom and Taylor (2006), we further assume what they call a “Weak structural utility theoretic” approach[4], i.e. that the underlying variables in the WTP-function is assumed to be derivable from some unknown utility function, but that flexibility is maintained to introduce explanatory variables, such as study characteristics, into the WTP model that do not necessarily follow from (1). This is the approach used in most previous MA-BT exercises (for example Rosenberger and Loomis 2000b, Shresta and Loomis 2003). We

[4] Bergstrom and Taylor (2006) suggest three main approaches (of which only the first two are recommended); (1) Strong structural utility theoretic approach; (2) Weak structural utility theoretic approach; (3) Non-structural utility theoretic approach.

specify a meta-model that captures *j* site characteristics X, *k* study or methodological characteristics M, *l* program characteristics P, and *q* socio-economic characteristics S. Mean WTP estimate (long term, per household in Norwegian Kroner 2005) *m* from study *s*, WTP_{ms}, can then be defined as:

$$WTP_{ms} = \beta_0 + \beta_X X^j_{ms} + \beta_M M^k_{ms} + \beta_P P^l_{ms} + \beta_S S^q_{ms} + e_{ms} + u_s \qquad (2)$$

Where, β_0, β are constant term and parameter vectors for the explanatory variables, and e_{ms} and u_s are random error terms for the measurement and study levels, respectively. MA-BT involves estimating (2) based on previous studies, and inserting values for X, P and S for the policy site under investigation, and choosing values for M (typically average of the meta-data, "best-practice" values or sample from a distribution – see e.g. Johnston et al (2006)). The meta-model has several potential advantages for BT, compared to unit value transfer or function transfer based on a single study[5]. MA utilizes information from several studies providing more rigorous measures of central tendency that are sensitive to the underlying distribution of the study values (Rosenberger and Loomis 2000b). Further, as specified in the model above, MA can control for study-specific choices of methodology, and finally it is possible to account for differences in site and programme characteristics between the policy site and the study sites in the meta-data, by setting these variable values equal to the policy site[6].

Convergent validity and reliability

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[5] The benefit transfer function from a single study is often specified as WTP_i=a + bX_{ij} +cY_{ik} + e_i, where WTP_i is willingness-to-pay of respondent i, X site/good characteristics (j), Y respondent characteristics (k), e_j random error, and the number of observations is equal to the number of respondents (Brouwer, 2000).

[6] This is provided that the policy site characteristics are represented in the meta-data. Otherwise the meta-model would be unsuitable for BT to that particular policy site.

If the process of BT is accurate, it can be used to inform decisions at a policy site, for example in a CBA framework. The focus to date has primarily been on the concept of validity, which requires that the values, or the value functions generated from the study site(s), be statistically identical to those estimated at the policy site (Navrud and Ready, 2007b). Further, the transferred estimate should be relatively invariant to various judgements by the analyst, for example choice of model specification (Rosenberger and Loomis 2000b). Most of the studies testing BT validity have used the same questionnaire for similar goods for different populations nationally and internationally, often resulting in high levels of TE (up to several hundred percent; see Rosenberger and Phipps (2007, table 1) for an overview of results). For MA-BT, such tests are scarce. For one thing, it is harder to define a yardstick value suitable for comparison with the transferred estimate. Rosenberger and Loomis (2000b) compare raw values from studies within their sample of recreation activities with the predictions from their national and regional MA-BT models, and calculate TE. Shresta and Loomis (2003; 2001) and Shresta et al (2007) follow a similar convergent validity approach, comparing their meta-model predictions based on the same dataset with the recreational values from a number of additional domestic and international studies, respectively (i.e. out-of-sample comparisons). More recently the BT validity testing has shifted focus somewhat to the concept of reliability, which requires that TE is small (but not necessarily zero). Santos (2007) compares the performance of MA-BT of landscape values to a site for which there exists a CV study to investigate convergent validity but also to assess the practical importance of TE for policy. Equivalence tests, which combine the concepts of statistical significance and policy significance into one test by defining an acceptable TE prior to the validity test, have been suggested (Kristofersson and Navrud, 2005). However, there is still no agreement on what the acceptable transfer errors should be for different policy applications, though levels of 20 and 40 percent have been suggested (Kristofersson and Navrud, 2007). Thus, the focus here is on measuring reliability in terms of TE and compare across model specifications and restrictions, and between alternative ways of conducting BT based on the same data. We define TE as

$$\mathrm{TE} = \frac{|\,\mathrm{WTP_E} - \mathrm{WTP_T}\,|}{\mathrm{WTP_T}}, \qquad (3)$$

where E = Estimated (predicted) value, T = True (observed) value[7]. Our procedure for measuring TE and checking reliability of BT is summarised in Table 1 below, and explained in the following. A first check of the transfer error for our meta-model specified in (2) is to compare the in-sample model prediction or forecast with the WTP observation and calculate TE for each observation and the overall Mean Absolute Percentage Error (MAPE) in our sample (Objective 1 in Table 1). Second, we estimate N-1 different MA-BT models, where for each run the WTP observation we shall predict is taken out, and calculate TE and MAPE again[8]. The TE can be expected to be larger than for the within-sample error above. We also characterize TE for different observations to discern patterns in the data. Brander et al (2006; In press) have suggested the within- and out-of sample TE calculation procedure for each observation as a first step to check reliability of the MA-BT model. Third, to more closely resemble an actual BT situation, we draw (randomly) a single WTP observation from each survey to represent a benchmark, unknown policy site value (Objective 2 in Table 1)[9]. We then

[7] It is important to note that this value, the benchmark value for comparison, for example as estimated by a single study, is only an estimate of the assumed, true underlying value and has its own measurement error.

[8] As pointed out by Brander et al (2006) this is similar to a jacknife resampling technique.

[9] This procedure, i.e. using internal WTP estimates as benchmark for "true" values, resembles how convergent validity considerations often are carried out in the (MA-)BT literature, e.g. starting with Loomis (1992).

use the other studies to transfer a best estimate to that "policy site" based on different BT techniques that are often used in practice[10]. Such techniques include a simple transfer of the mean WTP estimate from one study that has similar site and program characteristics, or the mean WTP averaged over several similar domestic or international study sites. We compare TE from these methods with the use of the two most promising MA-BT models, judged on the basis of lowest TE from the initial MAPE assessment above. Finally, in previous MA-BT convergent validity studies no systematic check on the impact of the choice of model specifications and model restrictions on TE have been carried out (Objective 3 in Table 1). There are many different types of meta-model specifications in use, and there is little guidance as to which to choose (linear, semi-log, double-log etc) (Johnston et al., 2005). Regarding restricted model versions, a model frequently used (though rarely convincingly justified[11]) in MA-BT is one where variables that are not significant at the 20 percent level are left out of the MA-BT model. To investigate the implications of this choice, we decided to test both a fully specified meta-model and a restricted version, like the one used for example in Rosenberger and Loomis (2000b).

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[10] In this case, all observations from the same survey from which a WTP estimate has been drawn to represent the policy site, are left out of the MA-BT model used for transfer.

[11] A principal reason put forward for this choice is that it is easier to use for practitioners, a reason that may not be valid today as a spread-sheet based BT tool would easily accommodate more variables without complicating the operation.

Table 1 Objectives and transfer error calculation procedures for validity and reliability check of MA-BT

Objective	**Transfer error estimation procedure**
1. Convergent validity of transfer estimates	
Analyse within-sample TE	Compare model predictions with observed WTP on the individual measurement level for all observations, and calculate Mean Absolute Percentage Error (MAPE).
Analyse out-of-sample TE	Compare N meta-model predictions and observed WTP for N-1of the meta-data for each prediction, and calculate Mean Absolute Percentage Error (MAPE).
2. Reliability comparison of different benefit transfer procedures	
Compare reliability of simple unit transfer techniques with MA-BT	Simple unit transfer techniques based on the most similar study, mean of similar domestic and international studies, are compared with MA-BT transfer. Single WTP observations from each study are drawn randomly as a benchmark, unknown true policy site value for TE calculation.
3. Robustness of transfer errors to methodological choices and meta-analysis scope	
Analyse TE across model specifications & restrictions	Two different model specifications (linear, and double-log) and two restricted models are used for transfer error calculations under 1. The two specifications with the lowest TE are used in 2.

Based on the Objectives 1.-.3 in Table 1, we will get a good check on convergent validity, reliability and robustness of MA-BT, and a comparison with other BT techniques. If the MA-BT approach through these procedures is found (or can be made) to be reliable enough for certain applications, specific WTP forecasts for different site and programme characteristics (for example a national forest protection plan for Norway) may be calculated for policy use (for example as attempted by Van Houtven et al (2006) for water quality policy).

Meta-data sources and regression results

A substantial stated preference literature of around 50 studies reporting from 30 surveys valuing NTBs has developed in Fennoscandia over the last 20 years. The studies typically ask for respondents' WTP for either full forest protection plans or for programmes introducing more environmentally and/or recreationally sensitive forestry practices – called multiple use forestry (MUF) (for example leaving old trees of importance for biodiversity, limiting clear-cutting, leaving broadleaf trees etc). The values from these studies can be interpreted as the WTP to obtain a positive change in at least one element in an attribute vector describing the forest environment, Q in the utility function (1), i.e. level of biodiversity, forest density, forest size, scenic beauty etc. A substantial portion of the stated WTP can be assumed to be non-use values. Based on a broad search for studies in the three countries we compiled a meta-dataset consisting of 72 observations, where 1-7 WTP estimates were gleaned from each study. All but one use the CV approach, and the number of studies is about evenly distributed between countries. To make WTP from different countries comparable, estimates from Sweden and Finland were converted to NOK at the year of the survey using annual average OECD PPP rates, and then adjusted to 2005 by use of the Norwegian consumer price index (CPI). For each WTP observation from a study, we coded explanatory variables according to the meta-model specified in (2) (see first column of Table 2). Of the variables, only the year is a continuous variable. The base format (all dummies equal to zero), is an in-person survey of a Norwegian national level forest protection program increasing Q, asking a dichotomous choice question in the spring/summer season, using a non-voluntary payment vehicle (e.g. tax), reporting long-term annual WTP per household. We chose long-term average annual WTP per household as the base format, coding other formats (such as per month, per year for a limited period etc) using dummies, since respondents' discount rates are not known. Preliminary analysis showed that the socio-economic variables, S (income, age and education level), did not have a significant effect on WTP, and were therefore excluded from the subsequent analysis. This is a very common result in MA (Rosenberger and Loomis, 2000b; Johnston et al., 2003; Johnston et al., 2005).

Programme (P) and site (X) characteristics variables try to capture the variation in the forest good valued and are of particular relevance for MA-BT. The size of the forest can a priori be assumed to capture an important dimension of the good. In preliminary analyses we used different measures of the size of the forest in hectares, as percentage of productive forestland in the country or as part of the whole land area, to capture this scope dimension. This analysis is conceptually difficult for several reasons. Some surveys ask WTP for national changes in forest practices, which basically would involve all forest areas in the country. Further, the dataset included both surveys of local and national protection or MUF plans, with high non-use values at the national level and higher degree of resource conflicts at the local level[12]. We did not find any significant increase in WTP with simple measures of forest size, which in our opinion is *not* evidence against valid stated preference research. The complexity of the good, the high share of non-use values, the relatively small changes proposed, the geographical dimensions, may just mean that the area of the forest is too crude a measure to capture people's sense of scope in a MA[13]. It may also be that two forest plans that only differ marginally in size, may be seen as no different in substance as long as people know for instance that a minimum of biodiversity is protected with both plans. The existing MA literature, with a few exceptions such as Smith and Osborne (1996), can be said to have under appreciated the potential conceptual and practical problems involved in capturing scope sensitivity across very heterogeneous international studies of complex goods such as wetlands – where WTP/hectare often is used uncritically as the variable explained

[12] Although there may also be a distance decay effect, i.e. that people value forests closer to where they live, higher.

[13] It is also fair to say that some of the studies had unclear and fuzzy scenario descriptions making it harder for people to judge differences between plans.

(Woodward and Wui, 2001; Brander et al., 2006)[14]. Instead, we included other dimensions of the good that may be important to people; geography (local, regional, national levels, country), primarily use, and type of plan (forest protection, multiple use forestry or a mix, urban forests). We also included a dummy for the season of the survey, checking if people display "season illusion".

[14] Recent CV studies have moved beyond the relatively simplistic ("bird count") scope debate following the Exxon Valdez disaster in the early 1990s, trying to probe deeper into the issue. See for example Bateman et al (2004) and Heberlein et al (2005).

Table 2 *Meta-regression model results with different model specifications*

	Full models		Restricted models (double log)	
Variable	**I. Linear**	**II. Double-log**	**III. Trimmed (one obs. excluded)**	**IV. Restricted (p<0.2 excluded)**
Intercept	1549.256*	4.140617**	1.943833**	1.72109***
	(875.5331)	(1.170449)	(.9250999)	(.5947163)
Choice experiment	192.6951	.3297439	.0964237	
	(378.0004)	(.2406569)	(.1543238)	
Open ended question	-1334.071**	-.495455	-.3484957	
	(594.0914)	(.3395935)	(.3404972)	
Open ended w/payment card	227.536	-.3608809	-.2795691	
	(382.0898)	(.2204971)	(.1720878)	
Voluntary payment vehicle	3799.7***	2.803627***	1.716961***	1.845446***
	(988.7608)	(.7711909)	(.5918412)	(.3816678)
Use/access payment vehicle	-2564.024***	-.3300177	.2968603	
	(424.8793)	(.4289763)	(.2882608)	
WTP stated other than long-term	183.4371	-.066285	.419128	
	(620.5135)	(.4875653)	(.4353331)	
Actual payment asked	-571.5364*	-2.099854***	-1.974489***	-1.715755***
	(320.3029)	(.1061977)	(.1579718)	(.3672467)
Individual, not household, WTP	1834.944***	1.295294***	1.58119***	1.410485***
	(471.8069)	(.2941284)	(.1762362)	(.1699934)
High response mail survey (65%)	-6477.973***	-4.986712***	-5.232499***	-4.10506***
	(1032.545)	(.7683036)	(.7537281)	(.6955875)
Medium response mail survey (50-65%)	-4864.702***	-4.270923***	-4.919064***	-3.735134***
	(1043.229)	(.9019158)	(.8583492)	(.7212115)
Low response mail survey (below 50%)	-2476.168**	-3.009995***	-4.18444***	-3.328119***
	(970.375)	(.9114381)	(.7654645)	(.6948911)
Unpublished study	-791.1643**	.0190386	.0845459	
	(320.2655)	(.3603327)	(.3065782)	
MSc Thesis	-1916.265**	-1.730453***	-1.299899***	-1.121121**
	(754.8593)	(.5586125)	(.3998584)	(.414038)
Multiple use forestry (MUF) program	765.1689**	.2771635	-.1541521	
	(320.39)	(.3163496)	(.2228194)	
Mix of protection and MUF	-1261.768	-.6688487	-.4740047	
	(808.1531)	(.5322865)	(.4339458)	
Percentage/hectare forest not given in survey	1276.517	1.279632**	1.246919***	1.168564***
	(934.0211)	(.525085)	(.4045801)	(.1942462)
Local forest	649.1225	-.4468539	-1.327387***	-1.088904***
	(536.0937)	(.4902242)	(.3227563)	(.1613885)
Regional forest	2350.52***	.821114*	.5384419	
	(746.4256)	(.471253)	(.6038565)	
Sweden	1111.561	2.147048**	3.889263***	3.370004***
	(822.4675)	(.9714438)	(.7683388)	(.5675196)
Finland	644.2306	2.131236*	2.254886***	1.932351***
	(1046.65)	(.6016583)	(.707515)	(.6072392)
Urban forest	-1551.158***	-.5718084	.1599182	
	(552.4695)	(.4513243)	(.3069824)	
Season (spring/summer)	-1879.212***	-.784065**	-.6893471**	-.447132**
	(496.1174)	(.313954)	(.2758698)	(.1828208)
WTP for avoiding a loss	627.9457	.5853566*	.1907735	
	(415.2456)	(.3072963)	(.1567345)	
Use primarily	451.9457	.0224779	-.3166314	
	(721.9776)	(.3540051)	(.2617096)	
Year/LnYear	130.3553	1.242805**	2.380679***	2.246495***
	(82.63281)	(.5555091)	(.2862772)	(.3421329)
Log likelihood χ^2	101.47***	121.56***		
R^2	0.756	0.815	0.886	0.814
N	72	72	71	71

Note: *p < 0.10, **p < 0.05, ***p < 0.01, Number of survey clusters for models = 27. All models are Huber-White robust estimations. Models estimated using STATA Version 9.1. Models I and II are identical with Models II and III in Lindhjem (2007).

The simplest approach to estimating the meta-model in (2), which has been used in several MA studies (Loomis and White, 1996; Rosenberger and Loomis, 2000b), is to treat all WTP observations as independent replications and hence assume that study level error is zero. A more advanced approach, and our preferred choice here, is to apply a Huber-White robust variance estimation procedure to adjust for potential heteroskedasticity and intercluster correlation[15] (Smith and Osborne 1996). Given this empirical framework, we choose four different models. The first two are linear and double-log specifications, while the third model is restricted in that one observation, which gave very high TE was left out[16]. The fourth model is a version of the third where we following Rosenberger and Loomis (2000b), retain only those variables that are significant at an 80 per cent level or better based on t-statistics. The regression results displayed in the second to fifth columns of Table 2 show that the models fit the data well, with adjusted R^2 between 75 and 90 percent. The models confirm several of our expectations about the methodological variables, for example related to open ended question formats, response rates of mail surveys, voluntary payment vehicles, actual payment etc (see Lindhjem (2007) for discussion). It is clear that methodological variables show a higher degree of significance than site and programme variables for explaining the variation in the data. This is a potential problem when using the meta-regressions for BT, and is common in the literature. Regarding the site and programme variables, the geographical variables in the model show that regional forests are valued

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[15] Some MA studies use multilevel models, but often find little improvement on the standard models applied here (for example Bateman and Jones (2003), Rosenberger and Loomis (2000b)). We therefore do not pursue this approach here.

[16] In preliminary analysis we also ran several alternative models, e.g. following Shresta and Loomis (2001), testing a trimming procedure of the data, leaving out WTP estimates larger or smaller than two standard deviations from the mean. This procedure did only marginally reduce the TE.

higher than national (the base case) though not statistically significant, while local forests have lower WTP. The resource use conflicts at local levels may explain the latter difference. Further, Sweden and Finland have significantly higher WTP in the last three models than observed in Norway, suggesting that even if economic, cultural and institutional conditions are similar across these countries, WTP can still be different. Urban forests are valued lower than other forests, which may indicate that non-use values of non-urban forests are important. WTP to avoid a loss is higher (though not significantly so) than WTP for a gain. WTP from users or related primarily to use is not statistically different than from a mixed group. Regarding type of programme, our results are somewhat puzzling. It seems that respondents value full protection lower than MUF, but higher than a mix between the two (though not significant through the four models). It is worth noting that in Model IV, the only site/programme description variables left are the local and country dimensions. Further, it also seems to be important to the stated WTP whether forest area and percentage have been explicitly mentioned in the survey. These results are of an exploratory kind, but shows at least that it is not immaterial to people whether it is question of full protection or just a change in existing forestry practices. Finally, the models show that the season variable is negative and highly significant, while the year of the survey influence WTP positively. The discussion of meta-regression results is not elaborated further here since our intention is to use the estimated equations for BT analysis (see Lindhjem (2007) for details).

Transfer error results and comparison of benefit transfer techniques

Within and out-of-sample Mean Average Percentage Error (MAPE)

The first step in our assessment of the suitability of MA-BT involves checking the Mean Average Percentage Error (MAPE) comparing the forecasts of our four regression models in Table 2 with WTP observations. This is first carried out within-sample (i.e. the models predict single observations in the sample) and then out-of-sample (i.e. N versions of each of the four models are run using N-1 of the data to predict the single out-of-sample observations). For each run, TE is calculated and averaged over all the observations into MAPE[17]. The results from these two exercises are given in Table 3.

Table 3 Mean Average Percentage Error for within-sample and out-of-sample runs of four MA-BT models

	Mean Average Percentage Error (MAPE) for different model specifications			
	Model I: Linear	**Model II: Dbl log**	**Model III: 1 obs. excl.**	**Model IV: p>0.2 excl.**
Within-sample				
Mean TE	135	52	39	52
Median TE	37	26	25	30
0 -25th percentile (obs 1-18)*	390	71	77	76
25 - 50th percentile (obs 19-36)	105	92	57	72
50 - 75th percentile (obs 37-54)	24	17	25	24
75 – 100th percentile (obs 55-71/2)	24	26	26	37
N-1 out of sample functions				
Mean and Median TE	266	222	62	63
Median TE	51	40	34	31
0 -25th percentile (obs 1-18)	770	202	110	109
25 - 50th percentile (obs 19-36)	213	592	70	53
50 - 75th percentile (obs 37-54)	38	27	20	35
75 – 100th percentile (obs 55-71/2)	42	67	50	54

Notes: *Percentiles calculates the transfer errors in four different segments of the data, when WTP is sorted in ascending order.

[17] The MA results for the double-log models allow one to calculate ln(WTP) for each observation, transformed into WTP using antilog. To account for econometric error we add standard deviation ($s^2/2$), which estimate varies when the sample changes, prior to transfomation of ln(WTP) (Johnston et al., 2006). An alternative, or supplement, for brevity not considered here would be to replace s^2 with the variance of the prediction (Goldberger, 1968).

The first point to note is the relatively low *median* MAPE for all models, varying from 25-51 percent. Further, it is expected that MAPE goes up when the observation we predict are left out of the data. When considering means, the linear models perform much worse than the double-log models having transfer errors between 135 and 266 percent. The double-log model II also has high MAPE, which is considerably reduced when leaving out an extreme observation and retained at the same level when the model is further restricted in that variables with p>0.2 are taken out. The predicted and observed values are plotted for out-of-sample Models II and IV for ascending order of WTP in Figures 1 and 2.

Figure 1 Plot of observed log WTP (lnwtp05) and predicted/transferred values (wtp_p) for Model II of out-of-sample

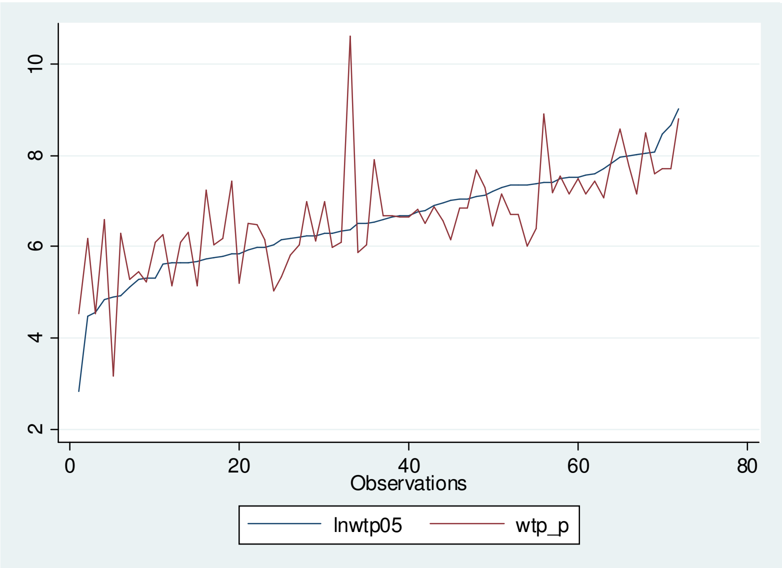
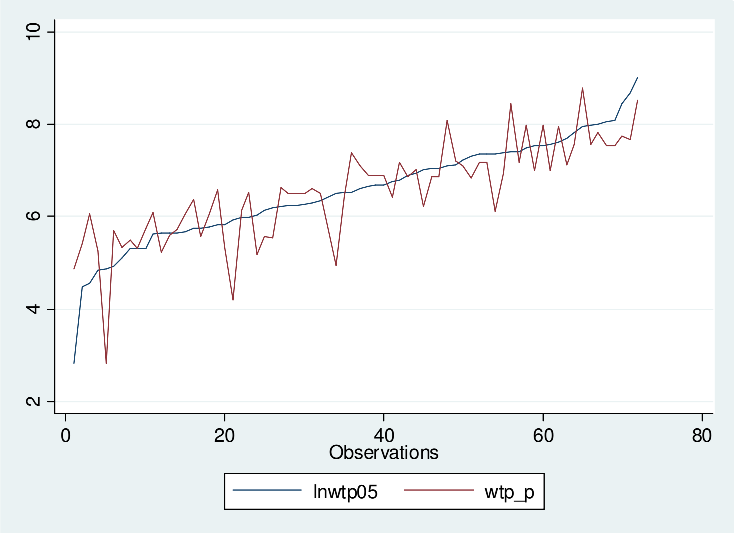


Figure 2 Plot of observed log WTP (lnwtp05) and predicted/transferred values (wtp_p) for Model IV out-of-sample



It can be seen from the figures firstly that the precision increases considerably using Model specification IV (or III) rather than II. Further, the figures show that TE is higher for lower values of WTP, a similar result to Brander (2006). Calculating MAPE for different percentiles of the data, as shown in Table 3, when WTP is sorted in ascending order, also clearly shows the error going down for higher WTP (though TE goes up again for the highest percentile)[18]. The predictions also seem to overshoot more often for lower WTP than for the ones above the median, which is an important consideration in making MA-BT conservative and err on the low side. The interpretation of TE for different levels of WTP is important also in terms of calculating a total welfare measure, i.e. summing WTP over the relevant population. For practical CBA it is the TE of the total welfare estimate that is important. If WTP per household from a local survey of a local protection plan is lower than a nationwide survey of a national plan (which is the case in our data), the overall TE for the welfare measures of both plans may "even out" in the aggregate.

The MAPE of around 60 percent we find for Models III and IV is comparable or somewhat lower than the only two studies we have seen conducting this exercise

[18] This is partly a result of the definition of TE, as the same absolute prediction error is higher in relative terms for low WTP values than for high.

(Brander et al 2006, 2007)[19]. Their meta-analyses have 72 and 201 observations, are based on more heterogeneous data, and use regression models with lower explanatory power. In their convergent validity tests of MA-BT Shresta and Loomis (2001; 2003) find average TE ranging from a low 28 percent to 88 percent, respectively. The within-sample test results of Rosenberger and Loomis (2000b) show mean TE ranging from 54 to 71 percent depending on whether a national or a region/activity specific model is used. The MAPE would be not directly comparable to TE from BT exercises based on single study situations.

TE for different BT techniques

Based on the first assessment above, we compare the two models with the lowest TE (i.e. Models III and IV) with simple BT techniques using a more realistic simulation of actual BT. If we were faced with a policy site without sufficient time and resources to do a primary study, we could use a study from the most similar site, use a mean from studies of similar domestic or international sites, or conduct an international MA-BT[20]. We compare these techniques in the following way. First we randomly draw one estimate from each of the 26 surveys included in the data, to represent a benchmark, "true" value for a policy site. All observations from this survey are then excluded when the remaining data is used for BT. We then calculate TE for each site, and calculate the overall mean and median TE for each BT technique[21]. The benchmark value has of

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[19] Brander et al (2006) also exclude an extreme observation from their model, so the most relevant model for comparison would be our model III.

[20] Most countries will not have enough domestic studies conduct an MA, and would have to base their MA-BT on a mix of domestic and international studies, like in the present study.

[21] We realise that a fuller test could include a bootstrap to calculate TE for many random draws of single "policy site" estimates, and not just one draw.

course its own error in measurement and is influenced by the survey methodology chosen. Nevertheless, a comparison of BT techniques for all sites represented by the data gives a valuable indication of the reliability and level of error that can be expected. Table 6 displays results. The second column is the value in Norwegian Kroner (2005) representing the unknown benchmark value for a site to be predicted. This value can be seen as a rough estimate of long-term household WTP for a forest protection or MUF plan at a policy site, defined by certain site and programme characteristics[22]. Column three displays the raw mean of WTP, regardless of site characteristics, for all observations in the data (except the benchmark study), representing an upper TE ceiling ("the worst you could do"). Column six displays the mean WTP for domestic surveys in the data that have the same site characteristics (the variables defining MUF, forest protection or a mix of the two, and local or national forests were used to assemble relevant value estimates).[23] Column seven is the mean WTP when observations with the same site characteristics from the other two countries were also included. For both these mean value transfers study characteristics are ignored. Expanding to include international studies would typically be done if there are no similar domestic studies or because the analyst believes a larger dataset will improve precision in BT. In contrast with the raw mean in column three, we picked the two values closest to the policy site value from the set of domestic or international studies that have the same site

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[22] We do not distinguish between different formats of WTP in terms of long-term vs lump-sum and individual vs household etc, but assume that the value at the site and the simple transfer estimates roughly represent long-term household WTP (and as the meta-regression results show many of these dummies were also insignificant in the analysis).

[23] Using the whole set of site characteristics, i.e. also urban, regional and primarily use value etc have the disadvantage that there often are no observations in the data with exact matching characteristics. A subset was therefore chosen.

characteristics (see columns four and five). This would not be possible in practice, but is a useful indication of the lower bound TE from choosing estimates from single, similar site studies ("the best you could do")[24]. Finally, the last two columns give the results from the use of the MA-BT models III and IV. Instead of setting the methodological dummy variables at average values, at 0.5 or at some best practice value as would have to be done in practical MA-BT (for example as investigated by Johnston et al (2006)), we set the values of these dummies to the same as for the benchmark study. This represents the lower bound TE for the MA-BT models. It would be unnecessary to introduce in our comparison the additional TE implied by the choice of methodological dummy values if the MA-BT models in our "best case" perform only marginally better than the simple BT techniques.

The last four rows in Table 4 sum up the mean and median TE for all BT techniques. First we ignore that some studies with matching policy site characteristics are not available (marked "na" in Table 4). Using the simplest of all techniques, just transferring raw mean WTP from the dataset of forest valuation studies would yield a mean TE of 217 percent. If it were possible to choose the closest value estimate with similar site characteristics, mean TE would be 62 percent if chosen from domestic studies and 71 percent if the set were expanded to include international studies. Taking means from domestic and the whole set of studies with similar site characteristics yields mean TE of 86 percent and 166 percent, respectively. Thus, expanding the dataset to include international studies in this case increases the TE substantially – close to "max"

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[24] We first tried to use an objective rule to choose a study or site estimate that would most closely resemble the policy site to mimic situation of simple BT . However, this is not straightforward as the set of studies with the full range of site and programme characteristics matching the policy site is often empty. In this case, secondary rules using a subset of the site characteristics need to be applied to end up with a unique, best estimate.

TE of 216 percent. In comparison, the two MA-BT models yield mean TE of 126 and 47 percent, a range that includes the TE from using mean from domestic studies. One reason why the MA-BT model IV gives a lower TE than model III is that simplified models often tend to give better predictions compared to fully specified models. Our BT testing procedure yields a lower number of observations for each model run, hence reinforcing his feature compared to the within and out-of-sample tests in the previous section. From comparing mean TE for all 26 sites, international MA-BT does not perform better on average than transferring mean WTP from domestic studies, though the best meta-model has lower TE. Considering the medians this conclusion is strengthened. It is clear from the data that the TE from using the simple BT techniques is pushed up by a number of high values compared with MA-BT. Medians of the best simple BT technique and MA-BT models are 41 percent and 37 per cent, respectively. Comparing TE from all 26 sites is not entirely satisfactory as there are missing values for some of the simple techniques, while the MA-BT predicts values for all sites. Limiting the set for comparison to the sites where transferred estimates are available across all BT techniques does not change the general picture, though MA-BT comes out a little more favourably in this case (see last two rows of Table 4).

Table 4 Comparison of transfer error between BT techniques. Norwegian kroner 2005 (Transfer Error)

Main reference	Site value to be pred-icted	Raw mean for all studies (-1)	Best similar domestic (D) or internat. (I) study		Mean of similar domestic (D) or international (I) studies				MA-BT models	
			D	D+I**	D		D+I		III	IV
					N	Mean	N	Mean		
Simensen and Wind (1990)	286	1225 (328%)	289 (1%)	289 (1%)	4	300 (5%)	14	756 (164%)	113 (60%)	272 (4%)
Hoen and Winther (1993)	340	1277 (275%)	na	1847 (443%)	0	na	6	3954 (1063%)	2367 (596%)	641 (88%)
Veisten et al (2004a; b)	1355	1193 (11%)	na	1638 (20%)	0	na	1	1638 (20%)	572 (57%)	1256 (7%)
Sandsbråten (1997)	277	1218 (339%)	286 (3%)	286 (3%)	4	351 (27%)	14	771 (178%)	1175 (323%)	416 (49%)
Leidal (1996)	3248	1109 (65%)	1567 (51%)	1567 (51%)	3	1047 (67%)	10	519 (84%)	1985 (38%)	2258 (30%)
Skagestad (1996)	96	1207 (1157%)	na	na	0	na	0	na	278 (188%)	353 (266%)
Veisten and Navrud (2006)	204	1247 (511%)	201 (1%)	201 (1%)	6	131 (13%)	27	1100 (439%)	128 (37%)	282 (38%)
Hoen and Veisten (1994)	324	1204 (271%)	311 (4%)	311 (4%)	5	329 (1%)	15	736 (127%)	239 (26%)	456 (40%)
Hoen and Veisten (1994)	311	1204 (287%)	324 (4%)	324 (4%)	5	332 (6%)	15	736 (136%)	755 (142%)	547 (75%)
Strand and Wahl (1997)	1567	1187 (24%)	2930 (86%)	2930 (86%)	4	2438 (55%)	11	1072 (31%)	479 (69%)	660 (57%)
Kniivilä (2004)	393	1210 (208%)	342 (12%)	342 (12%)	5	256 (34%)	12	1173 (198%)	486 (23%)	422 (7%)
Lehtonen et al (2003)	1534	1159 (24%)	1464 (4%)	1464 (4%)	12	791 (48%)	26	868 (43%)	1372 (10%)	1360 (11%)
Pouta (2003; 2004; 2005)	1137	1192 (4%)	1226 (7%)	1226 (7%)	13	992 (12%)	27	962 (15%)	1433 (25%)	976 (14%)
Pouta et al (2000; 2002)	1847	1173 (36%)	na	2838 (53%)	0	na	5	4058 (119%)	873 (52%)	1153 (37%)
Rekola and Pouta (2005)	126	1207 (857%)	734 (482%)	734 (482%)	6	957 (659%)	15	749 (494%)	227 (79%)	173 (36%)

Table 4 Continued

Siikamäki and Layton (2005)	531	1216 (129%)	512 (3%)	512 (3%)	14	1124 (111%)	28	1029 (93%)	1391 (161%)	652 (22%)
Mäntymaa et al (2002)	569	1230 (116%)	531 (6%)	531 (6%)	13	1191 (109%)	27	1058 (85%)	307 (46%)	651 (14%)
Tyrväinen & Väänänen (1998)	796	1237 (55%)	734 (7%)	734 (7%)	4	875 (4%)	13	691 (13%)	2462 (208%)	959 (20%)
Tyrväinen (2001)	284	1238 (335%)	342 (20%)	342 (20%)	3	312 (9%)	10	1374 (383%)	525 (84%)	277 (2%)
Bojö (1985)	372	1203 (223%)	na	393 (5%)	0	na	12	1175 (216%)	252 (32%)	218 (41%)
Bostedt and Mattson (1991)	2478	1173 (52%)	393 (5%)	540 (78%)	2	519 (79%)	15	592 (76%)	3755 (51%)	2138 (13%)
Mattson and Li (1993)	5843	1099 (81%)	8251 (41%)	8251 (41%)	2	5544 (5%)	5	3040 (47%)	1744 (70%)	2681 (54%)
Mattson and Li (1994)	2838	1067 (62%)	3020 (6%)	3020 (6%)	2	4432 (56%)	5	2595 (8%)	10487 (269%)	5560 (95%)
Kriström (1990a; b)	1853	1110 (40%)	590 (68%)	590 (68%)	1	590 (68%)	27	756 (59%)	5291 (185%)	2645 (42%)
Johansson (1989)	1638	1185 (27%)	na	1355 (17%)	0	na	3	1151 (29%)	3636 (121%)	3660 (123%)
Bostedt and Mattson (1995)	540	1211 (124%)	2478 (358%)	2478 (358%)	1	2478 (358%)	14	737 (36%)	2236 (313%)	688 (27%)
Mean TE		**217%**	**62%**	**71%**		**86%**		**166%**	**126%**	**47%**
Median		**120%**	**7%**	**12%**		**41%**		**85%**	**70%**	**37%**
Mean TE* (same obs.)		**196%**	**62%**	**62%**		**86%**		**136%**	**111%**	**33%**
Median* (same obs.)		**120%**	**7%**	**7%**		**41%**		**85%**	**70%**	**29%**

Notes: * For a few of the benchmark values to be predicted there were no estimates in the data with matching site characteristics, indicated by "na". Therefore, mean and median TE were also calculated only for those sites where WTP estimates were available across all BT techniques (i.e. na-values were excluded). ** If no domestic study with the right site characteristics was available, international studies were included

Plotting for the limited set the transferred estimates in ascending order of TE for the MA-BT model IV and the use of domestic mean, respectively, is instructive (see Figure 3).

Figure 3 Transfer errors for MA-BT model 4 and mean of similar domestic studies arranged in ascending order of TE for each BT technique, resepctively.

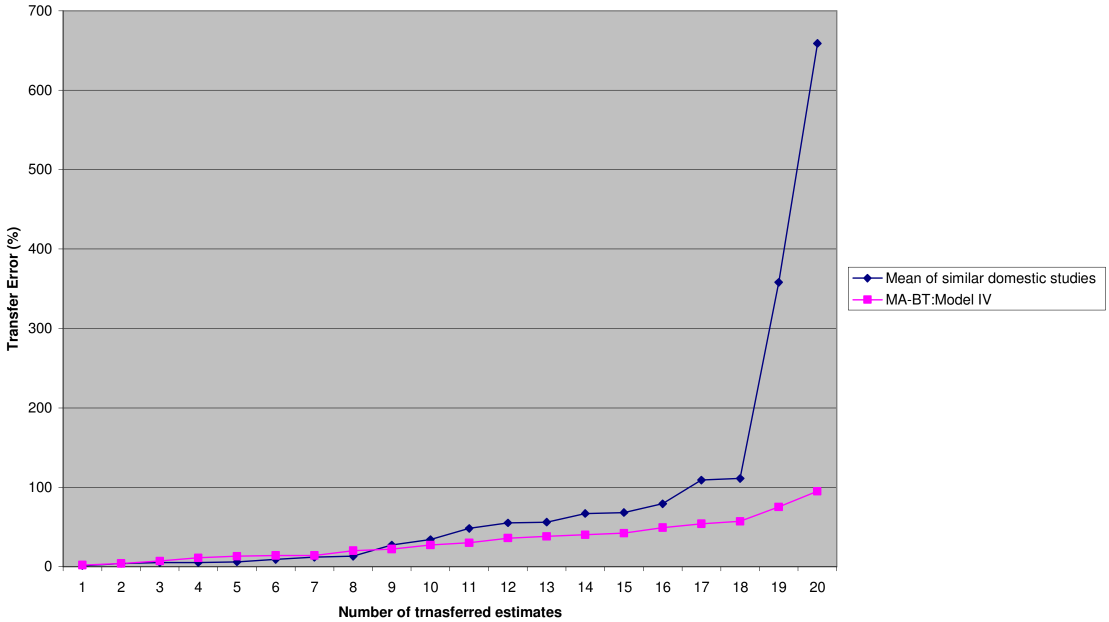

Figure 3 clearly shows that the better performance of MA-BT model IV over using domestic means overall, is largely due to a few very high TE values for the latter. 50 percent of the domestic mean transfers and 70 percent of the MA transfers have TE below 40 percent, while 40 percent of the transfers for both techniques have TE below 20 per cent. Excluding the two extreme transferred values from both sets of transferred estimates brings the mean and median TE for both techniques down to around 35 percent. We also compared whether BT would work better to certain countries. It seems that there is no consistent pattern, i.e. using an international MA-BT model does not yield systematically higher or lower TE between the three countries (nor do the other BT techniques). Due to the already limited dataset it was not possible to investigate whether a subset of the data matching the policy context better would improve the reliability of the MA-BT models. Santos (2007) investigates a subset of his meta-data and finds no improvement in MA-BT performance, though this result may not extend to our case. Another potentially relevant factor for our comparison that we were unable to investigate due to limited reporting in source studies, is the different level of uncertainty in WTP estimates. A richer BT test could use confidence intervals for the "true value" at the policy site as benchmark, as done by Santos (2007).

Concluding remarks

This paper has investigated the validity and reliability of international meta-analytic benefit transfer (MA-BT) based on a data set of stated preference surveys of forest protection and multiple use forestry plans from Norway, Sweden and Finland. The studies included in the meta-analysis (MA) are relatively homogenous in terms of valuation methodology and all three countries have similar cultural, institutional and economic conditions. We assess convergent validity of transfer estimates for within-sample and out-of-sample individual estimates, compare reliability of MA-BT with simpler transfer techniques frequently in use, and investigate the impact on transfer errors (TE) of different meta-regression specifications. The initial check of the convergent validity of within and out-of sample predictions of four meta-models show substantial variation in performance. The best models give median and mean TE of between 25-34 percent and 39-62 percent. The TE is lower for higher WTP estimates. Moving to the comparison of transfer techniques, MA-BT shows mean TE of between 47-126 percent (median 37-70 percent) depending on the model. A simple transfer based on the mean of domestic studies with similar site characteristics to the policy site yields a mean TE of 86 percent (median 41 per cent), as compared with 62 percent (median 7 percent) if a best study estimate could be chosen from a domestic study. Including also international studies in the simple mean transfer increases the TE substantially to 166 percent (median 85 percent). Finally, the meta-model specification and observations included have substantial impact on the TE. Despite the simple flavour of the BT exercise and the challenges of any convergent validity and reliability test based on the same dataset, our comparison of simple BT techniques with more advanced international MA-BT nevertheless shows interesting results. The best simple BT technique yields TE in the middle of the range of the two international MA-BT models. It is worth emphasising that in practical BT applications, the TE for the MA-BT models would increase since values of methodological characteristics would have to be set. Our results suggest that MA-BT may not always yield reliability gains over simple unit value techniques, as often claimed in the MA literature. However, more MA-BT tests should be performed for other environmental goods and other countries before discarding international MA as a tool for BT.

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