Loaded DICE: Refining the Meta-analysis Approach to Calibrating Climate Damage Functions

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Abstract

Climate change is one of the preeminent policy issues of our day, and the social cost of carbon (SCC) is one of the foremost tools for determining the socially optimal policy response. The SCC is estimated using Integrated Assessment Models (IAMs), of which Nordhaus' DICE is the oldest and one of the best respected. These numerical models capture the various steps in the climate and economic processes that translate a marginal unit of CO_2 emissions into economic damage. While accuracy at each of these steps is necessary to precisely estimate the SCC, correct calibrating the climate damage function, which translates a temperature change into a percentage change in GDP, is critical. Calibration of the damage function determines which climate damages are included and excluded from the cost of carbon. Traditionally, Nordhaus calibrated the DICE damage function using a global damage estimate calculated by aggregating a series of region-sector specific damage estimates (Nordhaus and Boyer, 2000; Nordhaus, 2008). However, in DICE-2013, Nordhaus moved to calibrating the DICE damage function using a meta-analysis at the global scale (Nordhaus and Sztorc, 2013). This paper critiques this meta-analysis approach as it is currently applied and re-estimates the DICE-2013 damage function using up-to-date meta-analysis techniques to more accurately reflect climate damages and the uncertainty underlying them. This paper finds that DICE-2013 damage function significantly under-estimates climate damages by a factor of two to three.

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Peter H. Howard and Thomas Sterner

Climate change is one of the preeminent policy issues of our day, and the social cost of carbon (SCC) is one of the foremost tools for determining the socially optimal policy response. As of 2008, the federal government must include the SCC in all federal cost-benefit analyses. The U.S. government's published estimates developed by the 2010 and 2013 Interagency Working Groups (IWGs) rely on three Integrated Assessment Models (IAMs): DICE, FUND, and PAGE. These numerical models capture the various steps in the climate and economic processes that translate a marginal unit of CO_2 emissions into an economic damage. While accuracy at each of these steps is necessary to precisely estimate the SCC, calibrating the climate damage function, which translate a temperature increase above the preindustrial level into a percentage change in GDP, correctly is critical. Calibration of the damage function determines which climate damages are included and excluded from the cost of carbon. Historically, developers calibrated IAMs using the enumerative strategy – whereby they aggregated region-sector specific damage estimates to determine the global damage (as a % of GDP) for a particular temperature increase.

In DICE-2013, the most recent version of DICE, Nordhaus calibrated the DICE damage function using a meta-analysis technique instead of using the more common enumerative strategy that he helped to pioneer. Of the three IAMs used by the IWG, Nordhaus' DICE is the first and one of the best respected. Traditionally, Nordhaus calibrated the DICE damage function using a global damage estimate calculated by aggregating a series of region-sector specific damage estimates (Nordhaus and Boyer, 2000; Nordhaus, 2008); this technique was utilized in the previous three versions of DICE: DICE-1999, DICE-2007, and DICE-2010. More recently in DICE-2013, Nordhaus utilized a meta-analysis approach instead. Drawing on global damage estimates from thirteen studies surveyed in Tol (2009), Nordhaus regressed climate damages on temperature change using ordinary least squares after rescaling the damage estimates upwards by 25% to account for missing damages (Nordhaus and Sztorc, 2013).

The estimation of the DICE-2013 damage function falls short in several respects. First, it relies on a data set assembled by Tol (2009), and later updated in Tol (2013), which does not meet the standards set in the meta-analysis literature. This potentially results in biased estimates due to selection bias and publication bias. Second, Nordhaus and Sztorc (2013) do not utilize the standard meta-analysis techniques developed in the literature. In fact, the authors utilize a simple OLS regression of global climate damages (as measured as a % of GDP) on average surface temperature. However, one of the leading studies - Nelson and Kennedy (2009) - argues that estimating "a simple OLS model…is clearly inadequate" when conducting a meta-analysis due to heteroskedasticity, dependence of errors, and other issues. Together, these shortcomings imply that the DICE-2013 damage function can easily be improved.

The goal of this paper is to improve the meta-analysis technique as applied to estimating a climate damage function in DICE. This is important for two reasons. First, given the leadership status placed on DICE by other economists and the prominence of Nordhaus in the field, it is important to scrutinize such a significant change in his modeling strategy. For example, the DICE-1999 damage function, along with FUND, helped inform Hope's choice of damage function parameters for both PAGE02 and PAGE09 (Hope, 2006; Hope, 2011; Howard, 2014). Second, the 2010 Interagency Working Group on the Social Cost of Carbon (2010 IWG) and the 2013 Working Group (2013 IWG) utilized the 2007 and 2010 versions of DICE, respectively, in their calculation of the U.S. social cost of carbon. Given the U.S. government's intent to update their estimates every two to three years based on the latest economic and scientific results, it is important to assess significant changes to DICE, FUND, or PAGE before their use in determining U.S. policy.

The results of this assessment should not be taken as a rejection of the DICE model. In fact, the DICE model is probably the most rigorously analyzed integrated assessment model in the literature, and it has stood the test of time thus far. Instead, this paper is an attempt to build upon and improve an important economic model of climate damages. With this aim in mind, I attempt to re-estimate the DICE-2013 damage function with a larger dataset and up-to-date estimation techniques. By doing so, I demonstrate how the IWG should re-estimate the DICE-2013 damage function if it chooses to use DICE-2013 in its updated social cost of carbon estimates. In particular, the damage function I estimate, unlike the current DICE-2013 damage function, conforms to the standards laid out by the EPA for meta-analysis (EPA, 2006).

In this paper, we focus on improving the estimation of the DICE-2013 damage function, and then improving it. First, we review the meta-analysis literature, including a summary of previous meta-analyses of the total cost of climate change and how these meta-analyses fall short of the current state of the art meta-analysis techniques. Second, we describe the creation of the datasets used in this paper. Third, we present the methods utilized in this paper to conduct its meta-analysis of willingness to pay to avoid non-catastrophic climate damages. Fourth, we follow this section up with a discussion of our results, including the derivation of a new DICE-2013 damage function whose magnitude represents a threefold increase from the Nordhaus and Sztorc (2013) model. Finally, we conclude with a summary of important results, and a discussion of potential directions for future work to improve the estimation of climate damage functions for IAMs.

Literature Review

A meta-analysis is a statistical method for combining multiple estimates (in this case multiple damage estimates) across studies into a new estimate (in this case a new damage function). Typically, the analyst collects a series of primary studies in economics by various authors that employ differing “study designs, model specifications, and econometric techniques,” and then regress a common summary statistic or effect size on the characteristics of the study (Nelson and Kennedy, 2009); regressors are typically specified as binary dummies (Nelson and Kennedy, 2009). In general these regressions attempt to explain the heterogeneity in study estimates, which exist for factual reasons (i.e., study context and characteristics of the object of the study – income, location, or time period) and methodological reasons (i.e., differing methodological approaches - assumptions, methods, models, and data). When the sample size is sufficient, an alternative to binary regressors is to conduct an analysis on more homogenous subsamples.

Generally, the meta-analysis regression is

$$\textbf{(1)}\ \hat{Y} = f(P, R) + \varepsilon$$

where *Y* is the effect size, *P* is the factual causes of observed heterogeneity in the effect size, *R* is the methodological causes of the observed heterogeneity in the effect, and ε is the error term. This error term can be subdivided into unobservables (μ) and measurement errors (e) from studies, such that

$$\textbf{(1')}\ \hat{Y} = f(P, R) + \mu + e$$

where it is often assumed that $\mu \sim N\left(0, \sigma_\mu^2\right)$ and $e \sim N(0, \sigma_e^2)$;[1] in many cases, σ_e^2 is reported in studies and σ_μ^2 has to be calculated (Rhodes, 2012). Often analysts assume a linear function, such that

$$\hat{Y} = \beta_0 + P\beta_1 + R\beta_2 + \mu + e = M\beta + \mu + e \text{ and } \mu = \alpha_1 Z + \alpha_2 X$$

where *Z* and *X* are the unobserved factual and methodological causes of the unobserved heterogeneity of the effect size.

The typical goals of conducting a meta-analysis are: research synthesis (obtain a mean value), hypothesis testing (explain heterogeneity in the estimated effect and/or publication bias), and benefit transfer (provide in-sample and out-of sample) predictions. In our case, the goal of the meta-analysis is estimating a damage function; this is a combination of in and out of sample prediction (Nelson and Kennedy, 2009; Smith and Pattanayak, 2002).

Current Guidelines for meta-analyses

As meta-analyses are becoming increasingly more common, several groups have developed guidelines for meta-analyses.[2] Though not a set of standards per se, the EPA assembled a working group in 2006 on estimating the value of a statistical life that was highly critical of

[1] While the normal error terms are often assumed, they are not necessary. Instead, we can assume more generally that $\mu \sim \left(0, \sigma_\mu^2\right)$ and $e \sim (0, \sigma_e^2)$.

[2] This has been necessary do to the inconsistent application of methods, and the frequency of studies failing to meet “minimum” standards of quality.

current meta-analyses. Though they focused on meta-analyses with regards to the value of a statistical life, their critique of methods is often cited as a minimum set of standards. Similar critiques are leveled by Stanley et al (2013), in which authors lay out the Journal of Economic Survey's minimum guidelines for met-analyses, and Nelson and Kennedy (2009), which reviews the econometric issues that arise in meta-analysis and develop a set of best practices. The three studies lay out similar sets of best practices for meta-analyses.

First, analysts must be transparent with respect to how they developed their data set. To start, the analyst should clearly define the problem to be tested, and clearly define the effect size to be studied.

Second, analysts should clearly lay out *a priori* their search protocols and study selection criteria, including their quality criteria. Specifically, Nelson and Kennedy (2009) request that analysts provide a clear statement defining the search protocol (who searched and what databases and the protocols they utilized) and study select rules; this is necessary to avoid publication bias and selection bias introduced by the analysts. This includes making sure that all primary studies measure the same effect,[3] and discussing any necessary adjustment of the dependent variable. While testing for publication bias is recommended in meta-analysis studies, these tests cannot be applied in the case of climate damages to a lack of standard error estimates.

Third, authors should clearly define their coding methods, including who read and coded the studies for the meta-analysis (at least two analysts must code the data),[4] when they did it, and what they coded and how; this should include how missing data was addressed. The analyst should also provide an explanation and summary of all relevant variables, including a discussion of which coefficients are most likely to be sensitive due to the small number of studies addressing the issue.

Fourth, the various studies layout slightly different data collection requirements, i.e. a minimum set of coding variables. Universally, the three studies agree that analysts should collect data on: the effect size, variance (standard deviation, standard error, and confidence intervals) when available, sample size when available, and time period and location of study (or data developed). Stanley et al (2013) further recommend controlling for model and/or methods utilized in study,[5] dummy variables for the omission of relevant variables, the type (and potentially source) of publication (journal, working paper, book, report, etc.), and dataset utilized.

[3] In terms of collecting data, the analyst must ensure that the dependent variable measures the same concept across all of the studies included in the meta-analysis; this may be an issue in climate change where analysts have done a poor job of clarifying whether damages estimates are relative to the pre-industrial or current period and a combination of willingness to pay and willingness to accept estimates have been employed.
[4] At a minimum, a second researcher must randomly double check a significant share of the coding.
[5] Similarly, the EPA (2006) recommends collecting data on key control variables in underlying studies.

Fifth, analysts should transparently address multiple damage estimates per study. The EPA (2006) suggests selecting one estimate per study.[6] Alternatively, analysts can combine damage estimating using variances as weights. If sufficient degrees of freedom are an issue, the analyst can also utilize panel data methods to address the resulting dependence.

Sixth, in term of meta-analysis regression methods, the guidelines all require a reporting of the estimation strategy utilized, and specifically a reporting of methods used to control for common meta-analysis issues: heteroskedasticity and dependent errors. Both of these econometric issues result in consistent and inefficient coefficient estimates, and biased estimates of the standard errors. Analysts must address these econometric issues to obtain unbiased estimates of the standard errors and also efficient estimates of the coefficients' mean and standard errors; efficiency is important due to the sample size associated with most meta-analyses.

Heteroskedasticity often arises in meta-analyses because of significant unobserved heterogeneity of the effect size, i.e. μ. While frequent solutions for heteroskedasticity are robust standard error calculations, e.g. Huber-White standard errors, or specifying more homogenous sub-populations upon which to conduct the meta-analyses, alternative solutions are often recommended in meta-analyses due to sample size that results in the asymptotic properties of estimators not applying. Specifically, the EPA (2006) and Nelson and Kennedy (2009) recommend controlling for heteroskedasticity by weighing estimates by their variance using either a fixed-effect-size or random-effect-size estimators (i.e. weighted least squares estimators). This is not possible in terms of the social cost of carbon because it would give undue influence on estimates that failed to include standard errors or other measures of uncertainty. Alternatively, the Davidson and MacKinnon adjustment can be utilized instead of Huber-White standard errors due to its adjustment for small sample size.

Dependence of the error terms in meta-analyses arises because of correlations in the underlying damage estimates. This can occur for several reasons, including: (1) multiple analysts utilize the same dataset; (2) a team of researchers contributes multiple estimates, and these estimates each contain their "perceptional bias" (Rhodes, 2012); (3) similar unobserved characteristics across studies; (4) similar observed characteristics across studies; (5) common adjustments across studies; and (6) an meta-analyst uses multiple estimates from the same study. While the last three causes can be addressed by carefully choosing the appropriate independent variables (i.e. regressors) and selecting one estimate per study, it is often necessary to still control for dependencies in the data due to the three first causes. These dependences, i.e. correlations, often result in the clustering of error terms. Assuming that groups are clustered by group *j*, the model becomes,

[6] The EPA (2006) suggests that the inclusion of multiple estimates from a study is statistically valid if these estimates are independent.

$$Y_{ij} = M_{ij}\beta + \mu_j + e_{ij}.$$

where μ_j, ν_k, e_{ij}, are assumed to be independent and identically distributed error terms. In this case, Nelson and Kennedy (2009) suggest a panel estimator: random-effects (RE) estimator or fixed-effects (FE) estimator. If instead, the errors are clustered around group *j* and group *k*, the model becomes,

$$Y_{ijk} = M_{ijk}\beta + \mu_j + \nu_k + e_{ijk}$$

where μ_j, ν_k, and e_{ijk} are assumed to independent and identically distributed error terms. In this case, Nelson and Kennedy (2009) suggest a hierarchical model. At a minimum, cluster robust standard errors should be calculated.[7]

Seventh, analyst should choose their final meta-regression by using specification tests. Authors should clearly provide all specification tests (omitted variables, functional form, and multicollinearity) and diagnostic tests (heteroskedasticity, outliers, and dependence). This should include tests for (1) data heterogeneity and heteroskedasticity using the Q test for homogeneity, (2) dependence of error term (i.e. correlation of effect sizes) using the I-test, (3) outliers using sensitivity analysis and effect size distributions, (4) omitted variable bias, and (5) publication bias using funnel plots and funnel asymmetry tests. Several tests can also be utilized to choose the correct estimator. If a weighted least squares estimator is chosen, the analyst can use a Cochrane's Q statistic to choose between the FES and RES estimators. If instead a panel data estimator is chosen, the analyst can use a Hausman test to choose between the fixed-effects and random-effects models.

Finally analysts should be transparent with regards to the robustness of their analysis. This includes conducting sensitivity analysis with respect to data points (to test for outliers), relevant variables (omitted variable bias), regression models (specification error), and sub-populations (heteroskedasticity).[8] Furthermore, if panel data methods are utilized, multiple levels of stratification, i.e., grouping, should be utilized to test for sensitivity of results to cluster choice.

[7] Nelson and Kennedy (2009) recommend generalized least squares (GLS) to ordinary least squares (OLS) when correlation is an issue because (1) GLS results in more sensible unbiased estimates than OLS, and (2) GLS accounts for heteroskedasticity. Often feasible Specifically, Nelson and Kennedy focus on subsets of GLS: hierarchical groups, panel methods (fixed effects and random effects), and clusters regressions. The latter two types of estimators are also special cases of the mixed linear model – a panel data method where cluster *j* is now the individual and damage estimates within cluster *j* are time periods. A mixed linear model allows for the variance of Y_{ijk} to vary with observed variables. More generally, the specification of the mixed linear model is

$$Y_{ij} = M_{ij}\beta + Z_{ij}\mu_j + e_{ij}$$

where $\mu_j \sim N(0, \Sigma_\mu)$, $e_{ij} \sim N(0, \sigma_e^2)$, and the variance and covariances in matrix Σ_μ are referred to as random parameters; while normality is not required for either error term, they must be independent and identically distributed. This model nests the pooled OLS model ($Z_{ij} = 0$), the random effects model $(Z_{ij} = 1)$, the random-coefficients model $(Z_{ij} = W_{ij})$, and the hierarchal model (Cameron and Trivedi, 2010, p. 305).

[8] Stanley et al (2013) emphasizes the need for sensitivity analyses of all relevant variables included in the meta-analysis.

This transparency also applies to clearly representing the level of uncertainty in estimate, including graphical representations of data, the residuals from the resulting estimates, and confidence intervals.

Global Climate Damage Meta-analyses

In addition to Nordhaus and Sztorc (2013), there are a couple of additional global climate damage meta-analyses: Tol (2009) and Tol (2013). Of these analyses, Tol (2009) and Nordhaus and Sztorc (2013) utilize the Tol (2009) dataset, while Tol (2013) utilizes an updated dataset. Few to none of the recommendations specified in the previous sub-section are followed by Nordhaus and Sztorc (2013) or Tol (2009; 2013).

Datasets. Tol (2009) develops the first datasets of global climate damage estimates; see table 1. When constructing the dataset, Tol (2009) did not follow the meta-analysis guidelines discussed above. He did not report his search protocols, study selection rules, or coding methods; nor did he ensure that all studies measure the same effect or that only one estimate was drawn per study. Instead, using an unspecified protocol, he assembled a dataset of 14 global climate damage estimates for 1 °C, 2.5 °C, and 3 °C increases in global mean surface temperature (relative to both the pre-industrial temperature and current temperature depending on the study) from 13 studies; this number falls short of the minimum number required for a meta-analysis according to Field (2001). Of these estimates, eight were derived using the enumerative approach, five using the statistical approach, and one using the survey approach; see the Appendix for a full discussion of these estimation approaches and their relative strengths. All of the studies are from 2006 or earlier, and a little less than half of them are from the 1990s. Of these estimates, there are several citation errors: Nordhaus (1994b)[9], Hope (2006),[10] and Nordhaus (2006).[11] See Table 1a.

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[9] Tol (2009) and Tol (2013) misquote the damage estimate in Nordhaus (1994b) as a 4.8% decline in GDP for a 3 °C increase; instead he should utilize the mean impact under Scenario A of a 3.6% decline in GDP for a 3 °C increase in GDP. This error is confirmed in Pearce et al (2006).

[10] Tol (2009) misquotes the damage estimate in Hope (2006). Given that only one region, Russian and Eastern Europe, experiences a positive effect from climate change, the assertion by Tol (2009) that a doubling of CO2 emissions increases GDP worldwide according to Hope (2006) is incorrect. Therefore, Nordhaus and Sztorc (2009) citation of a 0.9% increase in GDP (and a 1.125% increase with the non-market adjustment) for a 2.5 degree Celsius increase in temperature is incorrect. Instead, using mean damage estimates and 2100 GDP weights, we find a 1.38% decline in worldwide GDP assuming no adaptation, a 0.97% GDP decline assuming adaptation up to 2 °C, and a -0.86% GDP decline assuming adaptation up to 2.5 °C. This mistake is confirmed in Tol (2013) and in personal correspondence with Chris Hope.

[11] Nordhaus (2006) may estimate climate damages for a three degree Celsius increase in average land surface temperature, rather than average global surface temperature. In scenarios CC1 and CC2 of both papers, he assumes “a mean surface temperature change of 3.0 Celsius averaged over all terrestrial grid cells in the sample,” given an economic base year of 1990 and a base period of 1961-1990 for climate data. We assume this implies a 3 degree increase in land surface air temperature (LSAT) or global mean surface temperature, and thus a 2.07 °C increase in global mean surface temperature. This adjustment used numbers calculated from the IPCC (2013), but

Many of these estimates are highly related to one another. All of the fall in one of three camps: (1) Nordhaus and Mendelson at Yale, (2) Fankhauser, Maddison, Tol, and Rehdanz affiliated with University College of London, and (3 Hope at Cambridge University. Additionally, seven out of eight of the estimates are drawn from earlier versions of DICE, FUND, and PAGE.[12] Finally, many of the later estimates represent updates or modification of earlier estimates: Hope (2006) is based on estimates of Tol (2002) and Nordhaus and Boyer (2000); Plambeck and Hope (1996) is based on estimates of Tol (1995) and Fankhauser (1995),[13] and Nordhaus and Yang (1996) is based on the damages estimate from Nordhaus (1994a).

Tol (2013) constructs the second dataset by expanding the Tol (2009) from 14 damage estimates from 13 studies to 20 damage estimates from 17 studies. Like Tol (2009), the study does not follow the guidelines developed for meta-analyses. While the new dataset includes three damage estimates drawn from two studies that apply the general equilibrium approach (see Appendix for more), the other two studies are updates of previous estimates. Furthermore, while Tol (2013) corrects the citation error for Hope (2006), it maintains the citation error Nordhaus (1994b). See Table 1b.

Meta-analyses

Using ordinary least squares and equal weighting of studies, Tol (2009) regresses climate damages (measured as a percentage of GDP where 0.01 represents 1% of GDP) on the increase in global average surface temperature above pre-industrial levels (T) and this measurement of temperature change squared (T^2) on assuming that the constant term equals zero; we believe that Tol assumes homoscedasticity. The resulting damage equation is

$$D = -0.0246 * T + 0.011 * T^2$$

where all terms are significant at the 5% statistical level; 51% of variation is explained (i.e. R-squared equals 0.51);[14] a negative (positive) number implies that a temperature increase will

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corresponds to results ratios between global land temperature and global surface temperature in https://www.ncdc.noaa.gov/sotc/.

[12] Nordhaus (1994a), Nordhaus and Yang (1996), and Nordhaus and Boyer (2000) represent estimates of climate damages for various versions of DICE/RICE. Tol (1995) and Tol (2002) represent estimates of climate damages for the FUND model. Finally, Plambeck and Hope (1996) and Hope (2006) are two damage estimates for the PAGE model.

[13] Tol (2009) incorrectly asserts that Hope (2006) is based on estimates of Tol and Fankhauser. This is in fact Plambeck and Hope (1996).

[14] According to Tol (2009), he estimates this curve with fourteen data points. Given that there are only thirteen studies, this requires one study to be counted twice; this is Mendelsohn, Schlesinger, and Williams (2000) for whom Tol (2009) reports two damage estimates. If instead, we utilize only the thirteen estimates used by Nordhaus and Sztorc (2013), the resulting damage equation is

$$D = -0.0218 * T + 0.010 * T^2$$

have a positive (negative) effect on GDP. From this regression, Tol (2009) notes that (1) the effect of climate change at 2.5 degree Celsius will be relatively small, and (2) there will be some initial benefits from climate change up to a 2.24 degrees Celsius increase.[15,16]

Tol (2009) and Tol (2011) conduct sensitivity analysis over these results in several ways. First, he re-estimates the curve omitting each observation and each pair of observations. While Tol finds that no observation or any two pair of observations significantly affect the damage function, he finds that the two pairs of observations at 1 °C and 3 °C have the most substantial effects. Second, after converting confidence intervals to positive and negative standard deviations, Tol regresses temperature on the two resulting sets of five standard deviation estimates; this is the most accurate representation of uncertainty according to Tol, and he finds the potential for significant climate impacts. Tol (2012) similarly finds the potential for significant climate damages.

Also using the Tol (2009), Nordhaus and Sztorc (2013) estimates the DICE-2013 damage function - the first major update of the DICE model since the 2007 version. Like Tol (2009), Nordhaus and Sztorc (2013) fit an OLS damage function to the data assuming homoscedastic error terms. However, Nordhaus and Sztorc (2013) make several different assumptions than Tol (2009). First, Nordhaus and Sztorc (2013) drop one of one of the estimates from Mendelsohn, Schlesinger, and Williams (2000), such that there are 13 estimates from 13 studies; see Table 1a and Figure 1a for a full list of data points utilized by Nordhaus and Sztorc (2013). Second, Nordhaus and Sztorc (2013) multiply these damage estimates by 25 percent to account for damages of climate impacts omitted from the underlying studies cited by Tol (2009).[17] However, this adjustment is insufficient to include a catastrophic damage estimate equal in magnitude to DICE-2007; adjustment of 62% at 2.5 °C and 87% 6 °C would be necessary. Third, as in 2007, Nordhaus assumes that there are no initial benefits from climate change by setting the coefficient corresponding to the linear temperature term equal to zero.

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where all terms are significant at the 10% significance level (only the second term is significant at the 5% level statistical level) and 60% of variation is explained.

[15] Tol (2009) attributes the initial benefits of climate change to the CO_2 fertilization effect and that temperate countries, which produce the majority of the world's wealth, initially benefit from climate change.

[16] Again using Tol (2009), Tol (2012) using a vote counting procedure to estimate the probability distribution function, which actually represents the degree of belief, of the impacts of climate change. Again, Tol finds strong evidence of initial benefits and medium and long-term negative impacts for higher temperature increases. Net negative impacts are reasonable certain at a 3 °C increase.

[17] Specifically, Nordhaus and Sztorc (2013) state that "current studies generally omit several important factors (the economic value of losses from biodiversity, ocean acidification, and political reactions), extreme events (sea-level rise, changes in ocean circulation, and accelerated climate change), impacts that are inherently difficult to model (catastrophic events and very long term warming), and uncertainty (of virtually all components from economic growth to damages)." There are many additional damage impacts omitted from these underlying estimates; see Howard (2014).

The resulting DICE-2013 damage function is

$$D = 0.00267 * T^2$$

where a positive number implies that a temperature increase will negatively affect GDP.[18] See figures 2a and 2b for a comparison of DICE-2013 and Tol (2009) damage functions; the DICE-2013 damage function is strictly lower than the DICE-2007 damage function. Furthermore, they find higher climate damages than Tol (2009) up until 3 degrees Celsius, and lower damages thereafter.

Using an expanded dataset of 20 damage estimates from 17 studies, Tol (2013) estimates the damage function using ordinary least squares, standard bootstrap,[19] kernel regression constrained to go through the origin, and smoothed bootstrap.[20] For the parametric (i.e. non-kernel) regressions, he tests multiple functional forms drawn from the literature:

- $D = \alpha_0 T + \alpha_1 T^2$ (Nordhaus and Boyer, 2000; Tol, 2009)
- $D = \alpha_1 T^2$ (Nordhaus, 2008)
- $D = \alpha_1 T^2 + \alpha_2 T^6$ (Weitzman, 2012)
- $D = \alpha_3 T^{1.3}$ (Hope, 2008)
- $D = \alpha_4 \left[e^{\frac{2}{4.33} T} - 1 \right]$ (Karp, 2003; van der Poeg and Withagen, 2012)

where *T is* global mean surface temperature and *D* is climate damages measured as a percentage of GDP. Of the regressions run by Tol (2013), the kernel regression best fits the data; the kern regression follows a quadralinear path, i.e. $D = \beta_1 T + \beta_2 T^2 + \beta_3 T^3 + \beta_4 T^4$. While none of the parametric specifications from the literature fit the data particularly well, Weitzman's specification fits the data best according to its log-likelihood value followed by Tol's specification.

Tol (2013) also finds that the damage estimate in Maddison and Rehdanz (2011), a study unavailable in Tol (2009), is an outlier. Of the Tol (2013) studies, Maddison and Rehdanz (2011) is the only damage estimate based on willingness to accept.[21] In the empirical literature,

[18] Re-estimating this regression, we find that the resulting damage equation is

$$D = 0.002664 * T^2$$

where the coefficient is significant at the 99% significance level and 48% of variation is explained.

[19] In the standard bootstrap, Tol estimates draws 20 pseudo-observations with replacement, run ordinary least squares, and repeat 10,000 times.

[20] The standard regression relies on the false assumption that these twenty pseudo-observations are the only possible climate impacts. The smoothed bootstrapping method avoids this assumption by drawing the 20 pseudo-observations with replacement from the kernel regression,

[21] Technically, Maddison and Rehdanz (2011) measures compensating surplus, which is a combination of willingness to pay for those nations that prefer climate change and willingness to accept for those made worse off by climate change. Given that more nations experience damages in their dataset (59%) and they find an overall

willingness to accept estimates are often larger in magnitude than willingness to pay estimates (Tol, 2009). In addition to income effects, loss aversion, and agency effects (Tol, 2009), willingness to accept estimates can exceed willingness to pay estimates because the magnitude of the payment is not limited by income. Given that society already benefits from a stable climate, willingness to accept estimates are potentially the more appropriate estimate in terms of climate damages.

Shortcomings of meta-analyses

As asserted earlier, Nordhaus and Sztorc (2013) falls short of meeting many of the guidelines specified in the meta-analysis literature. These shortcomings are with respect to many of the key issues raised by the guidelines. First, the Tol (2009) dataset, which Nordhaus and Sztorc (2013) utilizes with only slight modification, fails to meet the transparency requirements with respect to data assembly specified by the meta-analysis. In other words, Tol fails to conduct a systematic review of his search and selection methods. Second, Tol (2009) fails to prove that these studies measure the same effect. By failing to control for factual and methodological causes of observed heterogeneity in the effect size other than temperature or to adjust the effect size to account for differences, Nordhaus and Sztorc (2013), like Tol (2009), compare apples and oranges – i.e. studies that measure fundamentally different study effects. Third, Nordhaus and Sztorc (2013) follow Tol (2009) in utilizing OLS – an inadequate estimator according to all meta-analysis guidelines discussed above. Finally, unlike Tol (2009), Nordhaus and Sztorc (2013) fail to conduct any post-estimation analysis – including sensitivity analysis. Tol (2013) suffers from similar shortcomings.

Though not address in the meta-analysis literature, there is the possibility that the error terms are not normally distribution in the case of climate damage estimates. Specifically, the uncertainty surrounding the climate problem (future emissions, the climate sensitivity parameter, omitted damages, and catastrophic damages) makes the damages and error terms right skewed.[22] While Tol (2009) find some evidence of right skewed error terms in the standard errors of some studies, he mainly argues that negative climate surprises are more likely than positive climate surprises given the ease at which they come to mind. Therefore, the analyst should test for whether normality holds.[23] If the estimates are skewed, the problems that arise in a small sample are similar to that of dependence and heteroskedasticity – the coefficient

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negative effect on welfare (do to willingness to accept estimates that exceed 100% of income), I refer to this estimate as a willingness to accept estimate.

[22] While the error term can potentially be skewed for multiple reasons, in the case of this paper, it cannot be due to the asymmetry of the climate sensitivity parameter because the meta-analysis estimates control for temperature nor due to catastrophic damages because following estimates capture only non-catastrophic impacts.

[23] Using the omnibus test, we find that the normality does not hold for Tol (2009) at the 10% significance level. In particular, we reject the null hypothesis that the data is symmetric at the 5% significance level, and find that it is skewed (Cameron and Trivedi, 2010, p. 102).

estimates are consistent and not efficient and the standard error estimates are potentially biased. A potential solution in STATA is the use of the mixed linear model, which allows for no structure to be placed on the time-invariance component of the error if an unstructured covariance matrix is chosen.[24]

Data

This section will discuss the methods used to construct this study's data for a meta-analysis of global climate damage estimates; specifically, *global willingness to pay to avoid non-catastrophic impacts of climate change* as measured as % of global GDP. This paper reviews the *a priori* search, study selection, and estimate selection criteria, as well as how we coded data. See Table 1c for the data for this paper.

Constructing dataset

All data points in Tol (2013) are selected for inclusion in this study. The data points are corrected for citation errors, i.e. Nordhaus (1994b) and Nordhaus (2006). We then added the latest damage estimates for FUND, PAGE, and G-ECON. Regardless of whether it was due to his search criteria or his selection criteria, Tol (2009) clearly omitted the latest climate damage estimates from DICE, FUND, PAGE, and G-ECO. At the time of the publication of Tol (2009), DICE-2007 (Nordhaus, 2008a), FUND 3.3, and updated estimates for G-ECON were available. Of these, Tol (2013) only included DICE-2007.

This was followed by a search on Google Scholar and Econlit and a re-reading of sources in Tol (2013) for high damage estimates.[25] In this search, I identified several sources of global climate damage estimates for high temperatures: Tol (1994b) and Weitzman (2012) via Ackerman and Stanton (2012). In our search for global damage estimates due to medium and high temperature increases, we found an additional low temperature damage estimates in Ackerman and Stanton (2012), which is based on an estimate from Hanemann (2008). More work is necessary in the future to identify new damage estimates, particularly unpublished estimates, especially since this is the primary method to eliminate publication bias in this context.[26] We will leave this for future work.[27]

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[24] In STATA, the mixed linear model allows for no structure to be placed on the time-invariance component of the error if an unstructured covariance matrix is chosen.

[25] The search terms were: "percentage of GDP", "climate change", global; "percent of GDP", "climate change", global; "percent GDP" , "climate change", global; "% of GDP" , "climate change", global; "% GDP" , "climate change", global; "Climate change" "world output"; "Estimated impact of global warming on world output"; "climate change" "economic impact" global – Econlit only; and "climate change" "global impact" – Econlit only.

[26] While testing for publication bias would be ideal, it is not possible in this context because many of the studies in Tol (2013) and this dataset do not provide standard errors. Thus, the Egger and Begg test statistics for publication bias cannot be constructed.

[27] In particular, alternative IAMs should be analyzed for how they calibrated their damage estimate. Also, various studies that analyze different model structures should be analyzed.

From the studies that we have assembled, we have the following criteria for including studies. First, the data estimate should be a willingness to pay estimate, and not a willingness to accept estimate. The one willingness to accept estimate cited in Tol (2013), i.e. Rehdanz and Maddison (2011), should be dropped unless the estimate can be adjusted; we drop the data point.[28] Second, cross sectional studies at the national level should potentially be dropped due to bias, as discussed earlier; this includes Maddison (2003), Rehdanz and Maddison (2005), Rehdanz and Maddison (2011), and one estimate from Mendelsohn, Schlesinger, and Williams (2000). Last, studies that rely on author discretion to cap damage damages at a particular level should excluded. The only estimate that potentially violates this criterion is Nordhaus (1994a), which is based on the arbitrary assumption that total U.S. climate damages will equal 1% of GDP for a 3 °C increase in global average surface temperature above. Due to the large number of estimates that would need to be dropped and the sample size in this study, we will not drop cross-sectional studies or Nordhaus (1994a) – the latter of which just barely meets our *a priori* quality specification. Instead we conduct a sensitivity analysis to their inclusion.

Following the EPA (2006) recommendation, we include only one estimate per study, unless multiple estimates are based on different methods. In other words, we select one estimate if multiple estimates were derived using the identical model. We choose the estimate that utilizes the business as usual scenario (A2), the most climate information available (i.e. temperature and precipitation changes), and GDP weights. Therefore, we recalculate the damage estimates from Rehdanz and Maddison (2005) Maddison and Rehdanz (2011), and Nordhaus (2006) cited in Tol (2013) using GDP projects from Columbia (http://ciesin.columbia.edu/datasets/downscaled/). Studies that have multiple estimates still included are: Nordhaus (1994b), Mendelsohn et al (2000), and Roson and van der Mensbrugghe (2012).[29] Sensitivity analysis over the choice to include multiple damage estimates is conducted.

Finally, I remove the catastrophic impact, from the studies that account for them, such that all study effects are the non-catastrophic climate impacts. First, I adjusted the Nordhaus and Boyer (2000) and Nordhaus (2008a), i.e. the DICE-1999 and DICE-2007, damage estimates to exclude catastrophic impacts. This is easy to do since both studies utilize the enumerative approach, and explicitly specify the magnitude of the catastrophic impacts. Second, I calculate the Hope (2002) and Hope (2009), i.e. PAGE2002 and PAGE2009, damage estimates excluding catastrophic impacts.

In summary, our data set includes 26 damage estimates from 23 studies. Of these 26 estimates, 12 utilize the enumerative approach (all for low temperature increases), 6 utilize the statistical

[28] Future work will test the effect of adding this data point, and adjust it to measure willingness to pay.

[29] In the latter case, i.e. Rehdanz and Maddison (2005), it is concern that the high damage estimate is not an independent estimate, but rather an extrapolation of the lower damage estimate using ENVISAGE - the computer general equilibrium model. While this may seem reasonable to include, this raises the problem that infinite damage estimate predictions could be included based on extrapolation from any IAM.

approach (all are for low temperature increases and 4 are cross-sectional analyses at the country scale), 2 utilize surveys (1 is for low temperature increases), 3 utilize the general equilibrium approach (2 are for low temperature increases), and 2 are scientific based (none are for low temperature increases). Only one of these studies represents a willingness to accept estimate, i.e. Maddison and Rehdanz (2011), and it is derived using a cross-sectional analysis at the country scale; this study is dropped bringing out total number of damage estimates to 25. Furthermore, ten of the estimates; authors are associated with the Yale Group, and seven with the University College of London. Of these estimates, DICE is the most represented model with 4 estimates, followed by FUND, PAGE, and CRED with 3. Two damage estimates are drawn from G-ECON, ICES, surveys by Nordhaus (1994b), and happiness studies by Maddison and Rehdanz.

Coding data

Using the 26 studies, Peter Howard coded the data. Some of that data was reviewed by Christo S. Tarazi, an undergraduate research assistant. Most of the studies are also discussed in Tol (2009) and Tol (2013), which provide a supporting opinion for much of the coding decisions.

We code multiple damage and temperature variables. The damage variables are *damage* - equals non-catastrophic damages – and *D_new* – the actual damage estimate cited in the paper including a mix of non-catastrophic and catastrophic impacts. The temperature variables are global average mean surface temperature increase in degrees Celsius and its squared value, i.e. *T_new* and *T2_new*; temperature must be controlled for in order to make the damage estimates comparable.[30] Like previous meta-analyses, temperature is included as the sole factual causes of observed heterogeneity. The idea behind this decision is not that temperature is the sole climate-related driver of impacts, but that many of the other climate drivers of impacts, such as an increase in storm intensity and precipitation change, are strongly correlated with temperature change.

The remaining choice of variables to code is based on assessment of the methodological causes of the observed heterogeneity in the study effect and potential causes of dependent errors. We identify nine possible methodological causes of study effect heterogeneity, and capture these causes using fourteen variables. First, *current* is an indicator variable equal to one if the temperature increase is relative to the current period, instead of the pre-industrial period. While the DICE damage function assumes that the climate damage function is relative to the pre-industrial temperature, many of the studies cited in Tol (2009) and Tol (2013) differ in whether they measure the temperature increase relative to the pre-industrial or current

[30] Some of the temperature variables are corrected from Tol (2009) and Tol (2013), specifically Nordhaus (2006) and Nordhaus (2008b) based on the belief that Nordhaus estimated the effect of a 3 °C increase in land temperature, which represents a smaller increase in global average mean surface temperature, as discussed earlier.

temperatures.[31] In other words, the various studies cited in Tol (2009) differ in their base year. Given that the damage function is increasing at an increasing rate, failing to include this dummy variable biases the coefficient on temperature upwards.

Second, *market* is an indicator variable if the estimate only accounts for market damages and *nonmarket* is an indicator variable if the estimate only account for non-market damages; *omit* is an indicator variable that equal one if the estimate captures only market or non-market impacts. Only estimates derived using the statistical approach suffer from this shortcomings. However, the sign corresponding to these variables is difficult to predict because estimates derived using other approaches (enumerative, general equilibrium, and survey) may fail to include damages or capture adaptation measured using the statistical approach.

Third, *cross* is an indicator variable equal to one if the statistical estimation approach relied on cross-sectional data at the national scale. Cross-sectional studies at the national or regional (above-national) scales are potentially bias because it can be difficult, if not impossible, to separate non-climatic factors at the national and regional scales from climatic factors. Therefore, climate damage estimates may, and likely do, suffer from omitted variable bias. Of the statistical studies, only Nordhaus (2006) and Nordhaus (2008b) conduct analysis at a sub-regional scale to avoid this complication.

Fourth, *Year* is the publication year of the article or book in the case of published material, and the release date of unpublished material, i.e. grey literature, and *Time* is the number of years since 1994, i.e. the first year that a study in the dataset was published, and *Time2* is *Time* squared. Time variables should be included because Tol (2009) finds that climate damage estimates decline over time; he finds that estimates decline by 0.23% of GDP per year over the 12 years that span the thirteen studies. This potentially could be due to multiple reasons: (1) the failure of early damage studies to recognize climate benefits, (2) the underestimation of humans ability to adapt by earlier studies, (3) the potential bias of statistical studies (discussed above) which are become more common in the latter period, and (4) the failure of Tol to account for temperature.[32] Regardless, including time and time squares can potentially improve the estimate.

Fifth, *WTA* is an indicator variable equal to one if the estimate corresponds to willingness to accept. If willingness to pay and willingness to accept estimates are combined, the inclusion of a willingness to accept indicator variable is necessary because willingness to accept is often larger in magnitude than willingness to pay.

[31] Eight of the thirteen studies estimate climate damages from an increase of temperature above the pre-industrial temperature. The other five studies analyze the effect of an increase in temperature relative to the current temperature.

[32] If we include a temperature in the regression (and correct citation errors in the data), the time variable is still negative but no longer significant. However, following Tol, this result is dependent on including an intercept.

Sixth, *arbitrary* is an indicator variable equal to one if the estimate potentially violates the non-arbitrary estimate requirement described in the previous section.

Seventh, *pub_type* is the type of publication the estimate was drawn from, and *peer* was whether the resulting estimate was peer-reviewed in either that publication or another.[33] Including these variables can potentially capture publication bias. However, this variable is not included in our study because all of the damage estimates were eventually peer reviewed.

Eighth, *product* is an indicator variable equal to one if the model allows climate change to affect productivity. Modeling economic growth can significantly increase the magnitude of damages. However, only general equilibrium models, i.e. ICES and ENVISAGE, have modeled productivity declines.

Finally, *cat* is an indicator variable that equals one if the study's original damage estimate accounts for catastrophic impacts. Like productivity, including catastrophic impacts can significantly increase total damage estimates.

See Table 1c for a summary and for the predicted sign of effect of these variables.

We identify four possible variables to cluster standard errors around. First, *Method* is categorical variable for estimation approach; see the Appendix. Second, *primary_model* is a categorical variable for the type model utilized in the study. Where possible, the integrated assessment that utilized the estimate was chosen as the primary model; the version of the model is ignored. Third, *primary_author* is the primary author of the study. Last, *Group* is a categorical variable for group of authors: Yale, University College of London, or other. These groups are chosen based on the idea that damage estimates made for the same model and by the same author, institutions, or methods are likely correlated.

Data Groups

The data are broken up into three varying subsets of these 26 estimates. The first group of estimates is the damage estimates cited in Tol (2009). The second group of estimates is damage estimates in the expanded dataset corresponding to a low temperature increase (i.e. a 3.2 °C increase or below). The final group is the full dataset, which includes all low and high temperature damage estimates.

Method

This section will discuss the methods used in this paper to conduct a meta-analysis of global climate damage estimates. Specifically, this paper will examine *global willingness to pay to avoid non-catastrophic impacts of climate change* as measured as % of global GDP. The

[33] We drew 19 estimates from academic journal articles, 4 from books, 2 from technical reports, and 1 from a working paper.

estimation will be broken up into four differing analyses: (1) a basic meta-analysis using only temperature square; (2) a complex meta-analysis of global climate damage estimates; (3) a re-estimation of the DICE-2013 damage function using the more advanced meta-analyses techniques, and (4) sensitivity analysis. Estimating the damage function differs slightly from the complex meta-analyses in that it requires that climate damage equal zero when temperature is at the pre-industrial level. This places several restrictions on the model, including that there is no intercept and that the methodological variables, such as time trends and omitted damage indicator, must be interacted with temperature-squared.

As discussed earlier, heteroskedasticity and dependence are the two main issues to address along with non-normal error terms. Each of the following sections will aim to address these issues using slightly different methods. Due to the speculative nature of impact estimates corresponding to high temperature increases, each section will utilize two sets of estimates to conduct to the analyses: (1) damage estimates corresponding to low temperature increases (i.e. 3.2 °C or below), and (2) the full dataset combining damage estimates corresponding to low and high temperature increases.

Previous meta-analysis techniques

For each of the data groups and the Tol (2009) dataset, the original Nordhaus and Sztorc (2013) analysis is conducted whereby we regress on temperature squared using OLS. However, in addition to calculating the standard homoscedastic standard errors as in Nordhaus and Sztorc (2013), we also calculate heteroskedasticity robust standard errors, i.e. Huber-White and Davidson-MacKinnon, and cluster robust standard error.

To determine at what scale to cluster standard errors, we estimate multiple measures of within-cluster correlation and calculate the Breusch-Pagan test of independence for each of the four cluster groups identified. See Table 4 for measures of within cluster correlation for the four possible clustering levels: author group, primary author, estimation method, and primary model. Using all data points in the study, we find little evidence that damage estimates are correlated within author groups; this is confirmed when we test for independence using the Breusch-Pagan test of independence. However, if we look at primary author instead, we find strong evidence of dependence using all three measurements. Similarly, we find strong evidence of dependence at the method and primary model scales. Using only damage estimates corresponding to low temperature increases (3.2 °C or below), we find less evidence of dependence. While we still reject independence at the primary author and primary model levels, we now fail to reject dependence at both the author group and method cluster levels.

Based on these results, we focus our clustering at the primary-author and primary-method levels in the basic, complex, and damage meta-regressions.[34]

Complex meta-analysis

The complex meta-analysis extends this analysis in two ways. First, OLS and panel (fixed effects, random effects, and hierarchical) estimates are utilized with Huber-White standard errors to address heteroskedasticity and dependence of error terms. These meta-regressions run the above specifications with an intercept and no interaction terms; these regressions are not meant for prediction. Due to the small sample size, the analysis is conducted with two sets of variables: (1) the full set of relevant variables, and (2) a reduced set of variables determined by the significance of coefficients in the previous regression. Using the theoretically important variables discussed above, the relevant variables are determined using correlation-coefficients; variables with correlations below 10% are dropped from all meta-analyses.

Table 3a displays correlation coefficients for all theoretically important variables using the full dataset. Only a handful of variables are strongly correlated with climate damages. Clearly, temperature and temperature squared are highly correlated with climate damages. As theorized, damage estimates for temperature increases relative to the current period are strongly positively correlated with climate damages. Also as theorized, omitted damages, driven by omitted market damages, are strongly negatively correlated with climate damages. Additionally, time is strongly positively correlated with damages. Given that this is the opposite result found by Tol (2009), this indicates the potential need for linear and quadratic time variables. Finally, cross-section estimates at the national scales appear to be biased downwards. All other variables are weakly correlated (below 10%) with damages.

Table 3b displays correlation coefficients using data points for low temperature increases only. The key differences to note are that (1) non-market damages estimates are negatively correlated with damages, and (2) current period is no longer correlated with damages.

Based on these results, *T2_new*, *current*, *omit*, *cross*, *Time*, and *Time2* are included in the complex meta-analysis and damage function regressions. Given that both *Market* and *Non-market* are negatively correlated with damages, these variables are combined into the *omit* variable to preserve degrees of freedom. *cat* is not included in this analysis because for the main specifications, catastrophic impacts are removed from the estimates.

Estimating a new damage function

[34] In addition to failing to reject the null hypothesis at the author group level using the data corresponding to low temperature, we also decide to not utilize the estimation approach (i.e. *Method*) to cluster standard errors because the primary model captures method-author specific effect making this measurement somewhat redundant. Sensitivity analysis will be included with respect this decision.

To re-estimate the DICE-2013 damage function, several specific changes are necessary to the above analysis. First, it is necessary to restrict the damage function such that there are no climate damages for a zero degree temperature increase. Second, we must adjust the damage estimates to account for omitted damages. Third, we must include catastrophic impacts. Together, these three adjustments result in a new damage function.

Damage function restriction. Because the goal of the meta-analysis in this sub-section, i.e. the re-estimation of the DICE-2013 damage function, is for in and out of sample prediction, it is necessary to restriction the damage function in order to be consistent with reality: there are no economic damages from zero degree Celsius increase above pre-industrial temperatures. For this to hold, we restrict the meta-regression to have no constant. Additionally, we interact the methodological variables in our regression with temperature squared; these methodological variables are only included as interaction terms.

Add 25%. Nordhaus and Sztorc (2013) multiply the damage estimates cited in Tol (2009) by 1.25 to account for omitted climate impacts. According to Nordhaus and Sztorc (2013), these omitted damages include: the loss of biodiversity, ocean acidification, political reactions to climate change, environmental tipping points, damages for high temperatures, and uncertainty. In addition to these damages, there are many other climate impacts systematically omitted from climate damage estimates, including slower economic growth due to labor and total factor productivity declines, forced migration, and violence (Howard, 2014; IPCC, 2014). The magnitude of the adjustment is arbitrary according to the authors.

Instead of multiply the damage estimates by 1.25, we add a 25% adjustment in order to avoid increasing in magnitude both omitted benefits and omitted damages. As currently constructed, the 25% adjustment increases the short-run benefits in those studies, such as Tol (2002) and FUND 3.6, that find net climate benefits for low temperature increased. Considering that Nordhaus and Sztorc (2013) do not list any short-term benefits in their list of significant missing damages and omitted climate benefits should be more than canceled out by omitted damages,[35] it seems that a multiplicative adjustment is unjustified. Instead we add a 25% adjustment. The exception made is the Weitzman estimates because they are estimated using a scientific technique that likely captures these omitted damages more generally and the estimate for a 12 °C estimate is already at the upper limit of willingness to pay.

[35] Tol (2009) argues that smaller missing damages can be ignored because their omission is balanced out by missing benefits. According to Tol (2009), these minor missing damages include: increased strength of extratropical storms and tropical storms, sea level rise on the salinization of groundwater and redesigning water systems, and higher water temperatures on the costs of cooling energy plants. There are also several minor missing benefits: higher winds speeds will decrease the costs of renewable energies, less ice will decrease shipping costs and make some previously unavailable minerals available, and higher temperatures will lower clothing costs and decrease traffic delays due to snow and ice.

Catastrophic Impacts. We include catastrophic damages in addition to the 25% adjustment discussed above because this adjustment is insufficient to cover both omitted non-catastrophic impacts and omitted catastrophic impacts. Using the 25% adjustment, Nordhaus and Sztorc (2013) implicitly assume omitted damages account for a 0.34% decline in GDP for a 2.5 °C increase and 1.94% decline in GDP for a 6 °C increase.[36] In contrast, catastrophic damages in DICE-1999 are 1.02% at 2.5 °C and 6.94% °C according to Nordhaus and Boyer (2000), while they are 1.16% at 2.5 °C and 4.72% at 6 °C in DICE-2007.[37] To achieve the levels of catastrophic damages observed in DICE-1999, Nordhaus and Sztorc (2013) would have to have chosen a missing damage adjustment of between 77% and 91%. Similarly, to achieve the catastrophic damages observed in DICE-2007, the authors would need to have chosen a missing damage adjustment of between 62% and 87%. Therefore, if we believe that the certainty equivalent measure of catastrophic damages are anywhere near the scale proposed in these earlier versions of DICE, the 25% arbitrary increase by Nordhaus is nowhere near sufficient to account for the potential of catastrophic impacts, let alone the other omitted damages. In addition to adding the 25% adjustment, an adjustment of 75% is made to account for catastrophic impacts equivalent to DICE-2007. Again, the only exception made is the Weitzman estimates because (1) it is unclear whether the Weitzman damage estimate corresponding to a 6 °C increase includes or excludes catastrophic impacts, and (2) the Weitzman damage estimate corresponding to a 12 °C increase is already at the upper limit of willingness to pay of 100% of GDP.[38]

Standard errors. As for all of the parameters in the DICE-2013 model, Nordhaus and Sztorc (2013) model damages as certain. In addition to Tol (2009) demonstrating the large uncertainties surrounding the damage estimates in his meta-analysis, the 95% confidence interval for the Nordhaus and Sztorc (2013) estimates imply a range of damage estimates from 0.58% to 2.75% of GDP for a 2.5 degree Celsius increase. Furthermore, Tol (2009) underscores that this standard measurement of uncertainty understates the level of uncertainty due to the large number of omitted damages. Therefore, when discussing the results, emphasis is placed on the underlying uncertainty.

Sensitivity analyses

Currently, we redo the analysis for the regression corresponding to all data (i.e. low and high temperatures) to account for catastrophic impacts. This test is necessary due to the difficulty of

[36] Without making a 25% adjustment, the Nordhaus and Sztorc (2013) damage function would have been $D = 0.00213 * T^2$, which implies damage estimates of 1.33% of GDP at 2.5 °C and 7.67% of GDP at 6 °C.

[37] The lower catastrophic damages in DICE-2007 compared to DICE-1999 result from Nordhaus no longer accounting for risk aversion when calculating catastrophic damages.

[38] Due this lack of adjustment, it could potentially be argued that the complex regression estimates provided in this paper for all data points (low and high temperatures) are invalid. However, the corresponding analysis for low temperature is valid. To address this concern, sensitivity analysis is conducted.

removing catastrophic impacts from the Weitzman estimates. We test the sensitivity of the results by including an indicator variable for whether the damage estimate includes catastrophic impacts, i.e. *cat*. This latter sensitivity analysis requires the use of the unadjusted damage estimate (*D_new)* as the endogenous variable, which potentially includes catastrophic impacts. Thus, the endogenous variable represents a mix of non-catastrophic and total (i.e. non-catastrophic and catastrophic) climate impacts.

While not currently conducted, many additional runs will be completed to check the robustness of the current results. First, we will re-run the preferred specification dropping each observation once. This covers dropping the effect of dropping Nordhaus (1994a), which was identified as potentially not meeting the quality standards of this paper. Second, we will test the effect of dropping studies that potentially are affected by cross-sectional bias. Third, we will re-estimate the preferred specification after selecting one estimate from each of the studies than including multiple estimates: Nordhaus (1994b), Mendelsohn et al (2000), and Roson and van der Mensbrugghe (2012). Fourth, sensitivity analysis will be conducted with respect to the functional form of the temperature variable. Three additional functional forms will be fit: linear and quadratic temperature variables, the Weitzman function, and the quintile temperature variables; see above. Fifth, we currently adjust the G-ECON temperature data assuming that Nordhaus (2006; 2008b) estimates climate damage for a 3°C increase in land surface temperature, and we will re-estimate the preferred specification after adjusting the temperature variable corresponding to these studies to a 3°C increase in global mean surface temperature. Sixth, we will test the sensitivity of the results to the inclusion of the willingness to pay estimate from Maddison and Rehdanz (2011). Seventh, in addition to the sensitivity analysis conducted above, we vary the omitted damage adjustment term from 25% to 0% and 50% and the catastrophic adjustment term from 75% to 50% and 100%. Last, even after adjusting for heteroskedasticity and dependence, the uncertainty underlying the damage estimates is under-estimated because many of the point estimates of global damages are merely the central estimate of the studies from which they are drawn. Therefore, we will bootstrap our standard errors to correctly account for the uncertainty underlying the point estimates.

Results

This section discusses the results from three sets of regressions discussed above. The section demonstrates that selection bias is potentially a more significant issue that heterogeneity and dependent errors with respect to the omission of climate damage estimates corresponding to high temperatures. It also demonstrates that controlling for heterogeneity in study effects due to differing estimation methodologies and strategies is key, particularly with respect to the reference period and whether only market or non-market impact are included. Furthermore, it demonstrates that the fixed-effect regression is potentially the preferred panel method, and that the inclusion of fixed effects decreases the coefficient corresponding to temperature – at least using the small sample available. Finally, it demonstrates that the DICE-2013 damage function potentially underestimates climate damages, particularly due to its omission of catastrophic impacts.

Basic Results

To start, we re-estimate the simple regression from Nordhaus and Sztorc (2013) without the 25% adjustment using four datasets: the original Tol (2009) dataset - see column (1) in Table 5; the Tol (2009) dataset corrected for citation errors – see column (2) in Table 5; our expanded dataset for low temperature increases only – see column (3) in Table 5; and our expanded dataset for low and high temperature increases – see column (4) in Table 5. For each estimate, we calculate four standard errors: homoscedastic standard errors, the Davidson and MacKinnon adjustment for heteroskedasticity; clustered standard errors at the primary author level, and clustered standard errors at the primary model level.

Re-estimating the DICE-2013 damage function after correcting for the errors in the data discussed above does not significantly change the damage function with respect to Nordhaus and Sztorc (2013). This is due to the multiple errors in Tol (2009) canceling out. While heteroskedasticity is not present, hence the heteroskedasticity corrected standard errors are almost identical, clustering errors at the primary author and primary model levels significantly decreases the resulting standard errors. Similar results hold when re-estimating the DICE-2013 damage function using the expanded dataset for low temperatures except that the standard errors do not decline as significantly with clustering, and in fact increase when clustering at the primary-model level.

When we re-estimate the DICE-2013 damage function using the fully expanded dataset (i.e. including damage estimates for low and high temperature increases), the damage function significantly differs from the Nordhaus and Sztorc (2013) damage function, and increases by a magnitude of three. Furthermore, heteroskedasticity is found to be present, such that standard errors more than double when calculating Davidson-MacKinnon standard errors and increases by 50% when using clustered standard errors. These preliminary results indicate that while dependence of errors is an issue for all groupings of the data, heteroskedasticity appears to only be an issue when damage estimates corresponding to high temperature increases are included. While controlling for both of these econometric issues is important, it also appears that selection bias, particularly with respect to the inclusion of high temperate damage estimates, may be more of a problem in estimating the DICE-2013 damage function than using the correct econometric methodologies in estimating the DICE damage function.

Additionally, while the error distributions are skewed for the Tol (2009) dataset, we fail to reject the null of hypothesis that the error terms are non-skewed for the new datasets. More importantly, we fail to reject the null hypothesis that the error terms is normally distributed (i.e. "total").

Complex Regressions

Using the low temperature increase and all data, we re-conduct the meta-analyses using OLS, fixed effects, random effects, and hierarchical models. For each of the models, we calculate cluster robust standard errors, and we do not impose normality for the hierarchical models. To start, we use these estimators with the full set of the pre-selected variables (using the correlation matrices above) - *T2_new*, *current*, *omit*, *cross*, *Time*, and *Time2* - and the pre-selected clusters (using the Breusch-Pagan test of independence) - primary author and primary model. We then re-estimate the models with the consistently significant variables, including the intercept term.

<u>Low temperature data.</u> Table 6a displays the full set of regressions for the data points corresponding to the low temperature increases. The only relatively consistently significant variables across the specifications are *T2_new*, *current*, *Time*, and *Time2*; *T2_new* and *current* switch alternate in significance based on the specification. Each of these variables has the theoretically correct sign. A test of whether fixed or random effects models are more appropriate is not possible because the random effects model collapses to a pooled model in both cases; hence, the coefficients corresponding to the OLS, random effects, and hierarchical models are equivalent. Re-estimating these fixed-effects, random-effects, and hierarchical models with only the significant variables produces similar results; see Table 6b. While one of the models again collapse to pooled regressions (i.e. models 3 and 4), the random effects model clustered at the primary author scale does not collapse; in this case, we reject the null hypothesis that the random effects model is consistent.

The clear result is that damage estimates at low temperatures do suffer from a bias resulting from the inconsistent use of pre-industrial time period as a reference point, damage estimates follow a quadratic form over time (i.e. declined as noted by Tol (2009) and then have increased again), and that damages increase with temperature squared.

The fixed effect specification of model (3), i.e. primary-model fixed effects are included, is the preferred specification.[39] However, the magnitudes of the coefficients between model (2), i.e. primary-author fixed effects are included, and model (3) are similar, particularly with respect to temperature squared. While the fixed-effect specification does not address non-normal error distributions, we fail to reject the null hypothesis that the errors are symmetric and that the errors are normally distributed.

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[39] Given that fixed effect models are consistent, the rejection of the random effects model, and the frequent collapse of the random effects models, the fixed effect models are preferred over the random or hierarchical models. Model (3) is chosen as the preferred specification given its higher R-squared and pseudo-likelihood value.

All data. Table 7a displays the full set of regressions for all data points, including damages from high temperature increases. Due to the effect of outlier estimates from Weitzman, the damage function is now only a significant function of *T2_new*. As before, the random effects models collapsed to pooled regressions. Re-estimating these fixed-effects, random-effects, and hierarchical models with the only significant variable and the intercept produces similar results; see Table 6b. While one of the random effects models again collapses to a pooled regression, the other – clustered at the primary model level – does not collapse; again, we reject the null hypothesis that the random effects model is consistent. The key results from these regressions is that including high damage estimates based on the scientific limits of human, i.e. Weitzman, dramatically increases damage estimates and drives the results. Also, methodologically speaking, the use of panel methods, specifically fixed effects, reduces the magnitude of the temperature coefficient.

The fixed effect specification of model (3) again is the preferred specification.[40] Again, the magnitudes of coefficients are almost identical between model specification (2) and model specification (3). Again, we fail to reject the null hypothesis that the errors are symmetric and that the errors are normally distributed.

Damage Function

Using the low temperature increase and all data, we re-conduct the meta-analyses using OLS, fixed effects, random effects, and hierarchical models setting the coefficient corresponding to the constant equal to zero; the endogenous variable is damages adjusted to account for omitted non-catastrophic and catastrophic impacts, i.e. *dam_25_75*. For each of the models, we calculate cluster robust standard errors, and we do not impose normality for the random effect or hierarchical models. To start, we use these estimators with the full set of the pre-selected variables - *T2_new*, *current*, *omit*, *cross*, *Time*, and *Time2* – interacted with temperature squared, and we again include fixed and random effects using the pre-selected clusters - primary author, primary model, and method approach levels. We then re-estimate the models with the consistently significant variables.

Low temperature data. Table 8a displays the full set of regressions for the data points corresponding to the low temperature increases. As in the case of the complex regression, the only consistently signed variables across the specifications include *T2_new*, *current*, *Time*, and *Time2*; each of these variables has the theoretically correct sign. While re-estimating these fixed-effects, random-effects, and hierarchical models with only the significant variables, produces similar results, the indicator variable accounting for whether damages are relative to current temperatures is no longer significant; see Table 8b.

[40] This decision is based on an identical reasoning as specified in footnote 67.

As before, model (3) is the preferred specification.[41] While normally the random effects model would be chosen for out of sample prediction, the fixed effects model is selected because the random effects model again collapses to a pool specification. Again, the magnitudes of coefficients are almost identical between the fixed effect specifications of model (2) and model (3). Calculating the updated total damage function coefficient for temperature squared relative to pre-industrial temperatures (*current*=0 and *Time*=18) using the preferred specification, we find that the coefficient corresponding to temperature squared is now 0.0055; this is over double the size of the DICE-2013 damage function coefficient.

We reject the null hypothesis that the error term is non-skewed. However, we fail to reject the null that the error terms are normally distributed; this lends support to the use of this current model. Alternatively, if we utilize the original version of this specification in Table 8a which is not characterized by skewedness, we find that the coefficient on the damage function increases to 0.0067.

<u>All data.</u> Table 9a displays the full set of regressions for all data points, including damages from high temperature increases. Unlike earlier, the damage function is now a significant function of *omit* in addition to *T2_new*. Re-estimating these fixed-effects, random-effects, and hierarchical models with the only significant variables produces similar results; see Table 9b.

Just as before, the fixed effect specification of model (3) is the preferred specification. Again, the magnitudes of coefficients are similar between model specification (2) and model specification (3), though they differ slightly for the temperature variables. Calculating the updated total damage function coefficient for temperature squared relative to pre-industrial temperatures (*omit*=0 and *Time*=18) using the preferred specification, we find that the coefficient corresponding to temperature squared is now 0.0061; this is similar to the previous result estimated with low temperatures only. Because the Weitzman damage estimates were not adjusted for omitted or catastrophic impacts, the robustness of the damage estimate indicates that these estimates may in fact be consistent with the low temperature impact estimates.

Sensitivity Analysis - Alternative Specification for Meta-Regressions using All Data

We may be concerned that our adjustments by 25% and 75% for omitted and catastrophic impacts are driving the results. We can test the sensitivity of our results by including an indicator variable for whether the damage estimate includes catastrophic impacts, i.e. *cat*. For these sensitivity tests, we utilize damage estimates that do not adjust Nordhaus and Boyer (2000), Nordhaus (2008), and Hanemann (2013) to eliminate catastrophic impacts, i.e. we

[41] Given that fixed effect models are consistent, the rejection of the random effects model, and the frequent collapse of the random effects models, the fixed effect models are preferred over the random or hierarchical models. Model (3) is chosen as the preferred specification given its higher R-squared and pseudo-likelihood value.

utilize the damage variable *D_new* instead of *damage* or *d_25_75.* In these sensitivity analysis regressions, we assume that the scientific estimates derived by Weitzman and used in CRED 1.4 include catastrophic impacts. Furthermore, we do not make our 25% and 75% adjustments to the measurement of damages.

We re-estimate the complex meta-analysis and the damage function using all data points, i.e. tables 7b and 9b respectively, including the indicator variable for catastrophic impacts, i.e. *cat*. In the latter set of regressions, we include *cat* interacted with temperature squared. In the complex meta-analysis regression, we find that the temperature squared and catastrophic impacts significantly increase damage estimates as expected; see table 10a. The fixed effect specification of model (3) is again the preferred specification.

Also re-estimating the DICE-2013 damage function with catastrophic impacts, we again find that the inclusion catastrophic impacts significantly increase impacts; however, the omitted damage indicator variable is no longer consistently significant. Again, the fixed effect regression of specification (3) is the preferred model specification. Using this specification, we find a temperature squared coefficient equal to 0.0062 without adjusting for omitted damages. With an adjustment upwards of the non-catastrophic impact estimates, i.e. $D_{non-cat} = B_1 + B_4 * 18 + B_5 18^2 = 0.0014$, by 25%, we find that the coefficient corresponding to temperature squared in the DICE-2013 damage function is now 0.0066; this is nearly triple the size of the DICE-2013 damage function coefficient. However, it is close to the previous estimates of 0.0055 and 0.0061 presented earlier in this paper.

Thus, the results of this paper indicate that that the central DICE-2013 damage function should be approximately equal to

$$D = 0.006 * {T_{atmosphere}}^2.$$

This new damage function will significantly increase the SCC estimate from DICE-2013. However, future work will need to determine an appropriate measure of uncertainty surrounding this estimate accounting for: uncertainty in the underlying damage estimates, uncertainty in the model specification, and uncertainty of the resulting meta-analysis estimates.

Additional Sensitivity Analyses – Future Work

Future work will conduct the sensitivity analyses outlined in previous section. Particular focus on the accurate calculation of damage estimate variance is necessary, including how to account for non-normal error terms.[42]

Conclusion

[42] The Cameron & Trivedi's decomposition of IM-test was run for all OLS specifications in Table 6-10, and no evidence of non-normal skewness or kurtosis was found.

This paper's primary goal is to improve the technique used for estimating the DICE-2013 damage function. In particular, this paper aims to apply the latest techniques for meta-analysis developed in the three recent sets of standards developed for conducting meta-analysis to improve the DICE-2013 damage function. This includes the transparent assembly of a dataset to avoid selection and publication bias, and the adjustment of damages and inclusion of control variables to ensure that the same effect is analyzed across all studies. Finally, estimators that control for heteroskedasticity and dependence are necessary for consistent estimates, particularly given the small sample size. This analysis is critically important because (1) DICE is the pre-eminent integrated assessment model used to analyze the economics of climate change, and (2) the DICE model, along with FUND and PAGE, is used to calculate the U.S. social cost of carbon. With regards to this latter point, the paper demonstrates how DICE-2013 damage function should be re-estimated to meet the standards set by the EPA (2006) for meta-analysis.

Several key findings result from this analysis. First, this paper demonstrates that the adjustments of damage estimates are necessary to ensure that climate impact meta-analyses are not comparing apples and oranges. In particular, specific focus must be paid to the inclusion of catastrophic impacts in some studies, such that the meta-analyses focus on non-catastrophic impacts exclusively.

Second, this analysis provides evidence of heterogeneity treatment across studies with respect the reference period, i.e. the pre-industrial versus the current period, and with respect to the inclusion of market or nonmarket damages. However, the statistical significance of the impacts of these different modeling decisions and estimation methods on climate damage estimates is not robust across all specifications. With regards to the reference period, the significance of this effect may be inconsistent due the (1) second order importance of this effect, particularly at the low temperature increases commonly observed, and (2) the small sample size of this study. With regards to omitted impacts, the indicator variable for omitted impacts may have an inconsistent significance because (1) whether the author omits market and non-market impacts, which are combined here, effects the magnitude of the climate impact estimate, and/or (2) the studies that only capture market or non-market climate impacts, which all correspond to statistical studies, may capture market and non-market impacts omitted by other estimation approaches, primarily the enumerative approach. With respect to this latter point, while many, if not all, of these damage estimates are missing the cost of some climate impacts, the climate impacts missing from a particular estimate depends greatly on the estimation strategy employed by the corresponding author.

Third, while Tol (2009) finds evidence of the magnitude of damage estimates declining over time, we find evidence of this decline reversing in recent years. Fourth, the previous damage estimates in Tol (2009) and Nordhaus and Sztorc (2013) suffer from selection bias due to the omission of many climate impact studies. While Tol (2013) corrects for many impact omissions, the authors still omit critical impact estimates corresponding to medium and high temperature increases. As a result, the current impact estimates are biased downwards. Future work is necessary to refine damage estimates corresponding to high temperatures using this scientific approach given their potential importance in determining the shape of the damage function.

Last, this paper re-estimates the DICE-2013 damage function using panel methods and heteroskedasticity-robust standard errors. Using fixed effects at the primary model scale, this study produces three estimates of the DICE damage function for non-catastrophic and catastrophic impacts. Including catastrophic impacts, we find that the new damage function is approximately three times the magnitude of the damage function included in Nordhaus and Sztorc (2013). Future work is still necessary to further test the robustness of this damage function estimate and to estimate the corresponding uncertainty underlying these estimates.

While this paper focuses on the improving the meta-analysis used to estimate the DICE-2013 damage function, we should emphasize the shortcomings of this technique. First, there are few data points of the cost of climate change at the global scale, and few of these estimates are truly independent. As a result, these damage estimates overly rely on a handful of models estimated by only a small group of economics. In addition to issues of dependence, this creates an incentive to include all available impact estimates, including incomparable damage estimates and estimates that are out-of-date and potentially biased. Additionally, many of the estimators rely on asymptotic theory, and as a consequence are biased due to the small sample size. By expanding the dataset, we hoped to address these shortcomings along with selection bias.

Second, it is difficult to determine what damages are actually included in the resulting damage function due to its reliance on multiple estimates that capture different impacts. In particular, what does it mean for a fraction of the studies included in a meta-analysis to account for a particular impact, such as the effect of climate change on vector-borne disease? A solution is to control for these differences using indicator variables. However, given the small sample size discussed above, it is impossible to control for all of the differences between climate impact estimates. Thus, if the meta-analysis is omitting methodological variables correlated with temperature, the resulting estimates may be biased. The use of model specific fixed effects and an adjustment for omitted impacts potentially addresses this shortcoming.

Third, sampling and publication bias are extremely difficult to address, particularly because climate damage estimation is not a large field and there are few unpublished global damages estimates easily found. Therefore, it is difficult to trust that the sample is representative of the true population. As a consequence, it is difficult to trust out-of-sample prediction upon which Nordhaus and Sztorc (2013) and this paper rely. To address this shortcoming we have attempted to expand the dataset, but further efforts to expand the set of climate damage estimates are necessary.

Fourth, the following damage curve represents a willingness to pay damage curve. However, a willingness to accept curve, which will imply higher damages per temperature increase, is more appropriate. In some ways, the current climate is "owned" by the current generation and they are being asked to accept a future climate, which in most cases, is less desirable. As a consequence, willingness to accept is potentially a more appropriate welfare measurement. As a result, the current damage function likely underestimates the social cost of climate change, and thus the resulting SCC. Few willingness to accept estimates exist in the literature and future work must expand this set of estimates. At a minimum, a ratio between willingness to pay and willingness to accept estimates must be determined to adjust the current willingness to pay damage function.

Last, Tol (2009) argues that these studies should not be treated as time-series data, and cautions that any analysis attempting to estimate a damage function should be interpreted cautiously.[43] In fact, he only uses his meta-analysis to illustrate that there are slight initial benefits from climate change followed by significant future damages. However, this is exactly what Nordhaus and Sztorc (2013) and we set out to do; as do Tol (2012) and Tol (2013). We are sympathetic to those who question whether such an analysis is worthwhile given that this analysis is potentially methodologically unjustifiable. However, we believe that if analysts in the field of climate change damage choose to embrace meta-analysis technique, we should at least ensure that that the climate damage studies meet the minimum guidelines set by the meta-analysis field.

An alternative to a meta-analysis a global scale is to conduct meta-analyses at the sector level where a sufficient number of studies are available. For example, there are a multitude of agricultural studies on the costs of climate change, and a meta-analysis to estimate a regional-agricultural or global-agricultural damage function would be possible. Another alternative, laid out by Kopp, Hsiang, and Oppenheimer (2013) is to develop an infrastructure that uses statistical (for example, Bayesian) methods to update damage functions as new estimates become available. Future work should aim to improve global climate damage estimates (the top down approach) and region-sector climate damage estimates (a bottom up approach), and utilize meta-analyses to determine whether these approaches are converging in magnitude.

[43] Specifically, he states that "Of course, it is something of a stretch to interpret the results of these different studies as if they were a time series of how climate change will affect the economy over time, and so [a meta-analysis of this type] should be interpreted more as an interesting calculation than as hard analysis."

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Table 1a. Damage Studies Cited in Tol (2009) and Nordhaus and Sztorc (2013)

Study	**Temperature Increase (Degrees Celsius)**	**Damage -Tol (2009)**	**Damage - Nordhaus and Sztorc (2013)**	**Corrected Temperature**	**Corrected Damage - Tol (2009)**
Nordhaus (1994a)	3	1.30%	1.63%	3	1.30%
Nordhaus (1994b)	3	4.80%	6.00%	3	**3.60%**
Fankhauser (1995)	2.5	1.40%	1.75%	2.5	1.40%
Tol (1995)	2.5	1.90%	2.38%	2.5	1.90%
Nordhaus and Yang (1996)	2.5	1.70%	2.13%	2.5	1.70%
Plambeck and Hope (1996)	2.5	2.50%	3.13%	2.5	2.50%
Mendelsohn, Schlesinger, and Williams (2000)	2.5	0.00%	0.00%	2.5	0.00%
		-0.10%	-		-0.10%
Nordhaus and Boyer (2000)	2.5	1.50%	1.88%	2.5	1.50%
Tol (2002)	1	-2.30%	-2.88%	1	-2.30%
Maddison (2003)	2.5	0.10%	0.13%	2.5	0.10%
Rehdanz and Maddison (2005)	1	0.40%	0.50%	**1.024**	**0.29%**
Hope (2006)	2.5	-0.90%	-1.13%	2.5	**0.86%**
Nordhaus (2006)	2.5	0.90%	1.13%	**2.07**	1.05%

Table 1b. Damage Studies Cited in Tol (2013)

Study	**Temperature Increase (Degrees Celsius)**	**Damage -Tol (2009)**	**Corrected Temperature**	**Corrected Damage - Tol (2009)**
Nordhaus (1994a)	3	1.30%	3	1.30%
Nordhaus (1994b)	3	4.80%	3	**3.60%**
Fankhauser (1995)	2.5	1.40%	2.5	1.40%
Tol (1995)	2.5	1.90%	2.5	1.90%
Nordhaus and Yang (1996)	2.5	1.70%	2.5	1.70%
Plambeck and Hope (1996)	2.5	2.50%	2.5	2.50%
Mendelsohn, Schlesinger, and Williams (2000)	2.5	0.00%	2.5	0.00%
		-0.10%		-0.10%
Nordhaus and Boyer (2000)	2.5	1.50%	2.5	1.50%
Tol (2002)	1	-2.30%	1	-2.30%
Maddison (2003)	2.5	0.10%	2.5	0.10%
Rehdanz and Maddison (2005)	1	0.40%	**1.024**	**0.29%**
Hope (2006)	2.5	0.90%	2.5	**0.86%**
Nordhaus (2006)	2.5	0.90%	**2.07**	1.05%
New Damage Estimates				
Nordhaus (2008a)	3	2.50%	2.5	1.77%

Rehdanz and Maddison (2011)	3.2	11.50%	3.2	17.23%
Bosello et al. (2012)	1.92	0.50%	1.92	0.50%
Roson and van der Mensbrugghe (2012)	2.3	1.80%	2.3	1.80%
	4.9	4.60%	4.9	4.60%

Table 1c. Data

obs_estimate	damage	D_new	dam_25_75	T_new	T2_new	current	market	nonmarket	omit	cross	Year	Time	WTA	arbitrary	product	Method	primary_model	primary_author	Groups	pub_type	peer
1	0.013	0.013	0.026	3	9	0	0	0	0	0	1994	0	0	1	0	enumerative	DICE	Nordhaus	1	Book	Yes
2	0.036	0.036	0.072	3	9	0	0	0	0	0	1994	0	0	0	0	Survey	Survey	Nordhaus	1	Journal	Yes
3	0.014	0.014	0.028	2.5	6.25	0	0	0	0	0	1995	1	0	0	0	enumerative	Fuankhauser	Fankhauser	2	Book	Yes
4	0.019	0.019	0.038	2.5	6.25	1	0	0	0	0	1995	1	0	0	0	enumerative	FUND	Tol	2	Journal	Yes
5	0.017	0.017	0.034	2.5	6.25	0	0	0	0	0	1996	2	0	0	0	enumerative	DICE	Nordhaus	1	Journal	Yes
6	0.025	0.025	0.05	2.5	6.25	1	0	0	0	0	1996	2	0	0	0	enumerative	PAGE	Hope	3	Journal	Yes
7	0	0	0	2.5	6.25	0	1	0	1	1	2000	6	0	0	0	statistical	Mendelsohn	Mendelsohn	1	Journal	Yes
8	0.0048	0.015	0.0096	2.5	6.25	0	0	0	0	0	2000	6	0	0	0	enumerative	DICE	Nordhaus	1	Book	Yes
9	-0.023	-0.023	0	1	1	1	0	0	0	0	2002	8	0	0	0	enumerative	FUND	Tol	2	Journal	Yes
10	0.001	0.001	0.002	2.5	6.25	1	0	1	1	1	2003	9	0	0	0	statistical	Production	Maddison	2	Journal	Yes
11	0.0029	0.0029	0.0058	1.024	1.048576	1	0	1	1	1	2005	11	0	0	0	statistical	Happiness	Maddison	2	Journal	Yes
12	0.0086	0.0086	0.0172	2.5	6.25	0	0	0	0	0	2006	12	0	0	0	enumerative	PAGE	Hope	3	Journal	Yes
13	0.0105	0.0105	0.021	2.07	4.2849	0	1	0	1	0	2006	12	0	0	0	statistical	G-ECON	Nordhaus	1	Journal	Yes
14	0.011245	0.011245	0.0224899	3	9	0	0	0	0	0	2009	15	0	0	0	enumerative	PAGE	Hope	3	Working Paper	Yes
15	0.0061	0.0177	0.0122	2.5	6.25	0	0	0	0	0	2008	14	0	0	0	enumerative	DICE	Nordhaus	1	Book	Yes
16	-0.0142	-0.0142	0	1	1	0	0	0	0	0	2012	18	0	0	0	enumerative	FUND	Tol	2	Technical Report	Yes
17	0.067	0.067	0.134	6	36	0	0	0	0	0	1994	0	0	0	0	Survey	Survey	Nordhaus	1	Journal	Yes
18	0.5	0.5	1	6	36	1	0	0	0	0	2012	18	0	0	0	Science	CRED	Ackerman	3	Journal	Yes
19	0.99	0.99	1.98	12	144	1	0	0	0	0	2012	18	0	0	0	Science	CRED	Ackerman	3	Journal	Yes
20	0.0318	0.042	0.0636	2.5	6.25	0	0	0	0	0	2012	18	0	0	0	enumerative	CRED	Ackerman	3	Technical Report	Yes
21	0.005	0.005	0.01	1.92	3.6864	0	1	0	1	0	2012	18	0	0	1	CGE	ICES	Bosello	3	Journal	Yes
22	0.018	0.018	0.036	2.3	5.29	1	1	0	1	0	2012	18	0	0	1	CGE	ENVISAGE	Roson	3	Journal	Yes
23	0.046	0.046	0.092	4.9	24.01	1	1	0	1	0	2012	18	0	0	1	CGE	ENVISAGE	Roson	3	Journal	Yes
24	0.003	0.003	0.006	2.07	4.2849	0	1	0	1	0	2008	14	0	0	0	statistical	G-ECON	Nordhaus	1	Journal	Yes
25	0.1722887	0.1722887	0.3445774	3.2	10.24	1	0	1	1	1	2011	17	1	0	0	statistical	Happiness	Maddison	2	Journal	Yes
26	-0.001	-0.001	0	2.5	6.25	0	1	0	1	0	2000	6	0	0	0	enumerative	Mendelsohn	Mendelsohn	1	Journal	Yes

Table 2. Data Summary

Variable	Obs	Mean	Std. Dev.	Min	Max	Predict
Damage	26	0.07554	0.211985	-0.023	0.99	NA
D_new	26	0.076771	0.211641	-0.023	0.99	NA
dam_25_75	26	0.154018	0.422731	0	1.98	NA
T_new	26	3.076308	2.195809	1	12	-
T2_new	26	14.0998	27.99608	1	144	+
Current	26	0.384615	0.496139	0	1	+
Market	26	0.269231	0.452344	0	1	-
nonmarket	26	0.115385	0.325813	0	1	-
Omit	26	0.384615	0.496139	0	1	-
Cross	26	0.153846	0.367947	0	1	?
Year	26	2004.077	6.945059	1994	2012	+
Time	26	10.07692	6.945059	0	18	+
WTA	26	0.038462	0.196116	0	1	+
Arbitrary	26	0.038462	0.196116	0	1	-
Product	26	0.115385	0.325813	0	1	+

Table 3a. Correlation coefficients - all data

	damage	T2_new	T_new	pre_idr	current	Time	omit	market	cross	product	arbitrary	nonmarket
damage	1											
T2_new	0.9481	1										
T_new	0.9102	0.9609	1									
pre_idr	-0.3687	-0.3032	-0.2688	1								
current	0.3687	0.3032	0.2688	-1	1							
Time	0.3161	0.2326	0.1722	-0.1814	0.1814	1						
omit	-0.221	-0.1993	-0.2223	-0.1319	0.1319	0.2917	1					
market	-0.1774	-0.1456	-0.1314	0.0965	-0.0965	0.3066	0.8315	1				
cross	-0.1231	-0.1285	-0.1788	-0.2359	0.2359	-0.0615	0.4924	0.0439	1			
product	-0.0852	-0.043	-0.0053	-0.2359	0.2359	0.4453	0.4924	0.5922	-0.1364	1		
arbitrary	-0.0567	-0.0383	-0.0066	0.1531	-0.1531	-0.2942	-0.1531	-0.1273	-0.0754	-0.0754	1	
nonmarket	-0.0974	-0.1117	-0.1759	-0.3932	0.3932	0.0087	0.3932	-0.1839	0.7985	-0.1089	-0.0602	1

Table 3b. Correlation coefficient - low temperature data

	damage	T_new	T2_new	Time	omit	cross	nonmar~t	market	current	pre_idr	arbitrary	product
damage	1											
T_new	0.679	1										
T2_new	0.663	0.9873	1									
Time	-0.2612	-0.3975	-0.4313	1								
omit	-0.2394	-0.228	-0.2996	0.3278	1							
cross	-0.2358	-0.1903	-0.1831	-0.0275	0.5204	1						
nonmarket	-0.1715	-0.2879	-0.2687	0.0462	0.4136	0.7947	1					
market	-0.1459	-0.058	-0.1475	0.3223	0.8062	0.043	-0.2052	1				
current	-0.0873	-0.3353	-0.3302	-0.0924	0.155	0.3443	0.513	-0.1667	1			
pre_idr	0.0873	0.3353	0.3302	0.0924	-0.155	-0.3443	-0.513	0.1667	-1	1		
arbitrary	0.0674	0.2756	0.3388	-0.3201	-0.1754	-0.0913	-0.0725	-0.1414	-0.1414	0.1414	1	
product	0.0612	-0.0946	-0.1495	0.4547	0.4136	-0.1325	-0.1053	0.513	0.1539	-0.1539	-0.0725	1

Table 4a. Test of dependence - all data

Group		
Intra-class correlation for author groups	0.04172669	
Correlation for adjoining method	0.02123163	
Breusch-Pagan LM test of independence	chi2(3) = 2.509	Pr = 0.4737
Author		
Intra-class correlation for author groups	0.42455132	
Correlation for adjoining method	0.02526504	
Breusch-Pagan LM test of independence	chi2(28) = 56.000	Pr = 0.0013
Method		
Intra-class correlation for author groups	0.92770677	
Correlation for adjoining method	0.98486002	
Breusch-Pagan LM test of independence	chi2(10) = 20.000	Pr = 0.0293
Model		
Intra-class correlation for author groups	0.3721171	
Correlation for adjoining method	0.90744753	
Breusch-Pagan LM test of independence	chi2(36) = 72.000	Pr = 0.0003

Table 4b. Test of dependence - low temperature data

Group		
Intra-class correlation for author groups	0.117616	
Correlation for adjoining method	-0.10238377	
Breusch-Pagan LM test of independence	chi2(3) = 5.138	Pr = 0.1620
Author		
Intra-class correlation for author groups	0.20628843	
Correlation for adjoining method	0.4612997	
Breusch-Pagan LM test of independence	chi2(15) = 30.000	Pr = 0.0119
Method		
Intra-class correlation for author groups	-	
Correlation for adjoining method	-0.03431927	
Breusch-Pagan LM test of independence	chi2(3) = 2.810	Pr = 0.4219
Model		
Intra-class correlation for author groups	1.19E-01	
Correlation for adjoining method	-0.40194432	
Breusch-Pagan LM test of independence	chi2(15) = 30.000	Pr = 0.0119

Table 5. Linear Regressions

VARIABLES	(1) D_orig	(2) Damage	(3) damage	(4) damage
T2	0.00213***			
	(0.000640)			
T2_new		0.00207***	0.00195***	0.00671***
OLS SE		(0.000512)	(0.000412)	(0.000470)
Davidson-MacKinnon SE		(0.000522)	(0.000409)	(0.000987)
Cluster SE - author		(0.000328)	(0.000359)	(0.000654)
Cluster SE - model		(0.000215)	(0.000468)	(0.000625)
Observations	13	13	21	25
R2	0.480	0.576	0.528	0.895
Adjusted R-squared	0.437	0.541	0.505	0.890
Liklihood	37.08	40.24	64.85	30.20
F-statistic	11.08	16.32	22.39	204.0
Prob>F	0.00601	0.00164	0.000128	0
Cameron & Trivedi's decomposition of IM-test: p-values				
heteroskedasticity	0.2381	0.1606	0.1415	0.0001
Skewness	0.0851	0.065	0.1098	0.1505
Kurtosis	0.8753	0.9925	0.6069	0.1507
Total	0.21	0.1326	0.1507	0.0001

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6a. Complex meta-analysis - Panel regression techniques using data corresponding to low temperature increases

Model	(1)	(2)		(3)		(4)
VARIABLES	damage	damage	damage	damage	damage	damage
T2_new	0.00377**	0.00189	0.00377**	0.00194	0.00377**	0.00377**
	(0.00139)	(0.00184)	(0.00188)	(0.00144)	(0.00168)	(0.00157)
current	0.00540	0.00904**	0.00540*	0.0138***	0.00540	0.00540**
	(0.00453)	(0.00350)	(0.00327)	(0.00207)	(0.00378)	(0.00274)
omit	0.00207	-0.00251	0.00207	-0.0189**	0.00207	0.00207
	(0.00565)	(0.0125)	(0.00734)	(0.00607)	(0.00773)	(0.00614)
cross	-0.00102	0.00100***	-0.00102	0.00157	-0.00102	-0.00102
	(0.00904)	(0)	(0.00694)	(0.00424)	(0.00862)	(0.00581)
Time	-0.00315*	-0.00364**	-0.00315***	-0.00370**	-0.00315**	-0.00315***
	(0.00150)	(0.00104)	(0.000777)	(0.00125)	(0.00124)	(0.000650)
Time2	0.000175*	0.000194**	0.000175***	0.000208**	0.000175**	0.000175***
	(8.16e-05)	(5.24e-05)	(4.75e-05)	(7.27e-05)	(7.04e-05)	(3.97e-05)
Constant	-0.00695	0.00605	-0.00695	0.00940	-0.00695	-0.00695
	(0.0113)	(0.0122)	(0.0144)	(0.00803)	(0.0143)	(0.0120)
Observations	21	21	21	21	21	21
R-squared	0.619	0.583		0.749		
Adjusted R-squared	0.456					
Liklihood	71.04	75.24	.	79.86	.	71.04
Fixed Effects		X		X		
Random Effects			X		X	
Hierachaical						X
Cluster at primary author		X	X			X
Cluster at primary model				X	X	X
Cluster at approach level						
Number of clusters		6	6	6	6	8
Cameron & Trivedi's decomposition of IM-test: p-values						
Heteroskedasticity	0.3766	0.4928		0.3971		
Skewness	0.5179	0.1954		0.4663		
Kurtosis	0.1975	0.7478		0.2461		
Total	0.4015	0.3569		0.4145		
Test of overidentifying restrictions: fixed vs random effects						
Sargan-Hansen statistic		RE collapses to pooled regression		RE collapses to pooled regression		
P-value						

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6b. Complex meta-analysis - Simplified panel regression techniques using data corresponding to low temperature increases

Model	(1)	(2)		(3)		(4)
VARIABLES	damage	damage	damage	damage	damage	damage
T2_new	0.00375**	0.00203	0.00339*	0.00250*	0.00375**	0.00375**
	(0.00136)	(0.00172)	(0.00186)	(0.00108)	(0.00165)	(0.00166)
current	0.00545	0.00794**	0.00603**	0.00510	0.00545*	0.00545**
	(0.00419)	(0.00275)	(0.00276)	(0.00327)	(0.00289)	(0.00268)
Time	-0.00301**	-0.00368**	-0.00310***	-0.00388**	-0.00301**	-0.00301***
	(0.00131)	(0.00104)	(0.000905)	(0.00114)	(0.00128)	(0.000842)
Time2	0.000170**	0.000191**	0.000171***	0.000195**	0.000170***	0.000170***
	(6.85e-05)	(4.94e-05)	(4.70e-05)	(5.92e-05)	(6.38e-05)	(4.37e-05)
Constant	-0.00685	0.00535	-0.00477	0.00492	-0.00685	-0.00685
	(0.0110)	(0.0118)	(0.0147)	(0.00726)	(0.0141)	(0.0130)
Observations	21	21	21	21	21	21
R-squared	0.615	0.579		0.655		
Adjusted R-squared	0.519					
Liklihood	70.94	75.14	.	76.51	.	70.94
Fixed Effects		X		X		
Random Effects			X		X	
Hierachaical						X
Cluster at primary author		X	X			X
Cluster at primary model				X	X	X
Cluster at approach level						
Number of clusters		6	6	6	6	8
Cameron & Trivedi's decomposition of IM-test: p-values						
Heteroskedasticity	0.2251	0.4189		0.3368		
Skewness	0.4662	0.1047		0.3034		
Kurtosis	0.2361	0.8915		0.3571		
Total	0.2598	0.2315		0.3001		
Test of overidentifying restrictions: fixed vs random effects						
Sargan-Hansen statistic		38.63		RE collapses to pooled regression		
P-value		0				

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 7a. Complex meta-analysis - Panel regression techniques using all data

Model	(1)	(2)		(3)		(4)
VARIABLES	damage	damage	damage	damage	damage	damage
T2_new	0.00658***	0.00523***	0.00658***	0.00519***	0.00658***	0.00658***
	(0.000308)	(0.000303)	(0.000314)	(0.000459)	(0.000360)	(0.000272)
current	0.0386	0.157	0.0386	0.172	0.0386	0.0386
	(0.0342)	(0.0983)	(0.0367)	(0.112)	(0.0377)	(0.0318)
omit	-0.0498	-0.0483	-0.0498	-0.166	-0.0498	-0.0498
	(0.0412)	(0.0600)	(0.0444)	(0.122)	(0.0444)	(0.0385)
cross	0.0203	0.00100***	0.0203	-0.0200	0.0203	0.0203
	(0.0352)	(0)	(0.0332)	(0.0215)	(0.0318)	(0.0288)
Time	0.00432	0.000900	0.00432	-0.00767	0.00432	0.00432
	(0.00754)	(0.00817)	(0.00634)	(0.00927)	(0.00717)	(0.00549)
Time2	-1.19e-05	0.000319	-1.19e-05	0.000898	-1.19e-05	-1.19e-05
	(0.000420)	(0.000617)	(0.000374)	(0.000720)	(0.000396)	(0.000324)
Constant	-0.0612**	-0.0962*	-0.0612***	-0.0547	-0.0612***	-0.0612***
	(0.0267)	(0.0478)	(0.0119)	(0.0335)	(0.0194)	(0.0103)
Observations	25	25	25	25	25	25
R-squared	0.922	0.925		0.934		
Adjusted R-squared	0.896					
Liklihood	35.35	46.75	.	48.34	.	35.35
Fixed Effects		X		X		
Random Effects			X		X	
Hierachaical						X
Cluster at primary author		X	X			X
Cluster at primary model				X	X	X
Cluster at approach level						
Number of clusters		8	8	9	9	10
Cameron & Trivedi's decomposition of IM-test: p-values						
Heteroskedasticity	0.2027	0.3631		0.4058		
Skewness	0.2313	0.1194		0.533		
Kurtosis	0.2003	0.5377		0.6616		
Total	0.1466	0.1921		0.5103		
Test of overidentifying restrictions: fixed vs random effects						
Sargan-Hansen statistic		RE collapses to pooled regression		RE collapses to pooled regression		
P-value						

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 7b. Complex meta-analysis – Simplified panel regression techniques using all data

Model	(1)	(2)		(3)		(4)
VARIABLES	damage	damage	damage	damage	damage	damage
T2_new	0.00715***	0.00592***	0.00715***	0.00605***	0.00704***	0.00698***
	(0.000376)	(0.000493)	(0.000323)	(0.000328)	(0.000326)	(0.000336)
Constant	-0.0303***	-0.0127	-0.0303***	-0.0145**	-0.0292***	-0.0260**
	(0.00906)	(0.00703)	(0.0107)	(0.00467)	(0.0110)	(0.0115)
Observations	25	25	25	25	25	25
R-squared	0.899	0.870		0.880		
Adjusted R-squared	0.895					
Liklihood	32.07	39.84	.	40.90	.	32.10
Fixed Effects		X		X		
Random Effects			X		X	
Hierachaical						X
Cluster at primary author		X	X			X
Cluster at primary model				X	X	X
Cluster at approach level						
Number of clusters		8	8	9	9	10
Cameron & Trivedi's decomposition of IM-test: p-values						
Heteroskedasticity	0.0003	0.0518		0.0699		
Skewness	0.2414	0.9967		0.999		
Kurtosis	0.1861	0.0127		0.0393		
Total	0.0007	0.1205		0.251		
Test of overidentifying restrictions: fixed vs random effects						
Sargan-Hansen statistic		RE collapses to pooled		89.385		
P-value		regression		0		

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 8a. Damage function estimation - Panel regression techniques using data corresponding to low temperature increases

Model	(1)	(2)		(3)		(4)
VARIABLES	d_25_75	d_25_75	d_25_75	d_25_75	d_25_75	d_25_75
T2_new	0.00581***	0.00316	0.00581***	0.00457*	0.00581***	0.00581***
	(0.00161)	(0.00301)	(0.00130)	(0.00206)	(0.00140)	(0.000206)
T2_current	0.00205*	0.00380*	0.00205*	0.00440**	0.00205*	0.00205**
	(0.00116)	(0.00203)	(0.00107)	(0.00147)	(0.00122)	(0.000862)
T2_omit	-0.00140	-0.00352	-0.00140	-0.00760**	-0.00140	-0.00140
	(0.00120)	(0.00343)	(0.000950)	(0.00257)	(0.00112)	(0.00128)
T2_cross	-0.00103	0.000310	-0.00103	-0.000585	-0.00103	-0.00103
	(0.00159)	(0.00100)	(0.000989)	(0.00120)	(0.00157)	(0.00163)
T2_Time	-0.00103**	-0.00115	-0.00103***	-0.000802	-0.00103***	-0.00103***
	(0.000440)	(0.000718)	(0.000150)	(0.000453)	(0.000357)	(0.000109)
T2_Time2	6.00e-05**	6.75e-05	6.00e-05***	4.91e-05*	6.00e-05***	6.00e-05***
	(2.41e-05)	(3.85e-05)	(5.47e-06)	(2.48e-05)	(2.06e-05)	(9.59e-06)
Observations	21	21	21	21	21	21
R-squared	0.862	0.885		0.929		
Adjusted R-squared	0.807					
Liklihood	64.74	66.63	64.74	71.63	64.74	64.74
Fixed Effects		X		X		
Random Effects			X		X	
Hierachaical						X
Cluster at primary author		X	X			X
Cluster at primary model				X	X	X
Cluster at approach level						
Number of clusters		6	6	6	6	8
Cameron & Trivedi's decomposition of IM-test: p-values						
heteroskedasticity	0.3614	0.357		0.3971		
Skewness	0.2717	0.9136		0.9986		
Kurtosis	0.7797	0.2482		0.3715		
Total	0.3485	0.6563		0.8527		

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 8b. Damage function estimation – Simplified panel regression techniques using data corresponding to low temperature increases

Model	(1)	(2)		(3)		(4)
VARIABLES	d_25_75	d_25_75	d_25_75	d_25_75	d_25_75	d_25_75
T2_new	0.00600***	0.00464	0.00600***	0.00603***	0.00600***	0.00600***
	(0.00150)	(0.00260)	(0.000316)	(0.00183)	(0.00141)	(0.000316)
T2_current	0.00148	0.00198	0.00148	0.000936	0.00148	0.00148
	(0.00120)	(0.00134)	(0.00101)	(0.00128)	(0.00145)	(0.00101)
T2_Time	-0.00127***	-0.00128*	-0.00127***	-0.00124*	-0.00127***	-0.00127***
	(0.000372)	(0.000600)	(0.000200)	(0.000569)	(0.000392)	(0.000200)
T2_Time2	7.29e-05***	7.13e-05*	7.29e-05***	6.74e-05*	7.29e-05***	7.29e-05***
	(2.05e-05)	(3.43e-05)	(1.21e-05)	(3.23e-05)	(2.08e-05)	(1.21e-05)
Prediction						
T2	0.0068	0.0047	0.0068	0.0055	0.0068	0.0068
Observations	21	21	21	21	21	21
R-squared	0.843	0.865		0.869		
Adjusted R-squared	0.806					
Liklihood	63.38	64.98	63.38	65.25	63.38	63.38
Fixed Effects		X		X		
Random Effects			X		X	
Hierachaical						X
Cluster at primary author		X	X			X
Cluster at primary model				X	X	X
Cluster at approach level						
Number of clusters		6	6	6	6	8
Cameron & Trivedi's decomposition of IM-test: p-values						
Heteroskedasticity	0.188	0.3797		0.3368		
Skewness	0.0719	0.1794		0.066		
Kurtosis	0.8242	0.0906		0.9731		
Total	0.1011	0.1786		0.1451		

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 9a. Damage function estimation - Panel regression techniques using all data

Model	(1)	(2)		(3)		(4)
VARIABLES	d_25_75	d_25_75	d_25_75	d_25_75	d_25_75	d_25_75
T2_new	0.00404***	0.00257**	0.00404***	0.00163	0.00404***	0.00404***
	(0.000405)	(0.000932)	(0.000119)	(0.00104)	(9.10e-05)	(0.000119)
T2_current	0.00189	0.00844	0.00189	0.00845	0.00189	0.00189
	(0.00141)	(0.00541)	(0.00144)	(0.00519)	(0.00144)	(0.00144)
T2_omit	-0.00311***	-0.00262**	-0.00311***	-0.00305**	-0.00311***	-0.00311***
	(0.000681)	(0.000937)	(0.000255)	(0.001000)	(0.000236)	(0.000255)
T2_cross	-0.000364	-0.00182	-0.000364	-0.00434	-0.000364	-0.000364
	(0.00145)	(0.00286)	(0.00101)	(0.00469)	(0.00110)	(0.00101)
T2_Time	-0.000361	-0.000769	-0.000361***	-0.000398	-0.000361**	-0.000361***
	(0.000243)	(0.000913)	(0.000106)	(0.000785)	(0.000153)	(0.000106)
T2_Time2	2.42e-05	2.69e-05	2.42e-05***	9.16e-06	2.42e-05**	2.42e-05***
	(1.55e-05)	(5.39e-05)	(9.28e-06)	(4.70e-05)	(1.08e-05)	(9.28e-06)
Observations	25	25	25	25	25	25
R-squared	0.950	0.979		0.980		
Adjusted R-squared	0.934					
Liklihood	39.10	49.88	39.10	50.41	39.10	39.10
Fixed Effects		X		X		
Random Effects			X		X	
Hierachaical						X
Cluster at primary author		X	X			X
Cluster at primary model				X	X	X
Cluster at approach level						
Number of clusters		8	8	9	9	10
Cameron & Trivedi's decomposition of IM-test: p-values						
Heteroskedasticity	0.2018	0.4628		0.519		
Skewness	0.7819	0.8875		0.9293		
Kurtosis	0.247	0.1032		0.1055		
Total	0.3351	0.6564		0.7432		

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 9b. Damage function estimation – Simplified panel regression techniques using all data

Model	(1)	(2)		(3)		(4)
VARIABLES	d_25_75	d_25_75	d_25_75	d_25_75	d_25_75	d_25_75
T2_new	0.00413***	0.00357***	0.00413***	0.00305***	0.00413***	0.00413***
	(0.000452)	(0.000499)	(0.000222)	(0.000731)	(0.000196)	(0.000222)
T2_omit	-0.00310***	-0.00278**	-0.00310***	-0.00309***	-0.00310***	-0.00310***
	(0.000630)	(0.000998)	(0.000249)	(0.00101)	(0.000228)	(0.000249)
T2_Time	-0.000466**	-0.00104	-0.000466***	-0.000658	-0.000466***	-0.000466***
	(0.000220)	(0.000649)	(0.000119)	(0.000479)	(0.000158)	(0.000119)
T2_Time2	3.56e-05***	6.59e-05*	3.56e-05***	4.60e-05	3.56e-05***	3.56e-05***
	(1.18e-05)	(3.70e-05)	(6.25e-06)	(2.65e-05)	(8.46e-06)	(6.25e-06)
Prediction						
T2	0.0073	0.0062	0.0073	0.0061	0.0073	0.0073
Observations	25	25	25	25	25	25
R-squared	0.949	0.971		0.972		
Adjusted R-squared	0.939					
Liklihood	38.96	46.03	38.96	46.22	38.96	38.96
Fixed Effects		X		X		
Random Effects			X		X	
Hierachaical						X
Cluster at primary author		X	X			X
Cluster at primary model				X	X	X
Cluster at approach level						
Number of clusters		8	8	9	9	10
Cameron & Trivedi's decomposition of IM-test: p-values						
Heteroskedasticity	0.0187	0.3504		0.4058		
Skewness	0.5355	0.9939		0.8992		
Kurtosis	0.2471	0.0921		0.0889		
Total	0.0369	0.6826		0.6005		

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 10a. Complex meta-analysis – Simplified panel regression techniques using all data and including catastrophic indicator variable

Model	(1)	(2)		(3)		(4)
VARIABLES	D_new	D_new	D_new	D_new	D_new	D_new
T2_new	0.00654***	0.00588***	0.00654***	0.00600***	0.00654***	0.00654***
	(0.000390)	(0.000489)	(0.000139)	(0.000332)	(0.000461)	(0.000133)
cat	0.0915	0.0226***	0.0915*	0.00960***	0.0915	0.0915**
	(0.0606)	(0.00255)	(0.0481)	(0.000456)	(0.0594)	(0.0460)
Constant	-0.0386***	-0.0154*	-0.0386***	-0.0144**	-0.0386***	-0.0386***
	(0.00737)	(0.00748)	(0.00655)	(0.00482)	(0.00741)	(0.00627)
Observations	25	25	25	25	25	25
R-squared	0.922	0.874		0.883		
Adjusted R-squared	0.915					
Liklihood	35.39	40.50	.	41.54	.	35.39
Fixed Effects		X		X		
Random Effects			X		X	
Hierachaical						X
Cluster at primary author		X	X			X
Cluster at primary model				X	X	X
Cluster at approach level						
Number of clusters		8	8	9	9	10
Cameron & Trivedi's decomposition of IM-test: p-values						
Heteroskedasticity	0.0001	0.0724		0.0948		
Skewness	0.0346	0.9981		0.9997		
Kurtosis	0.1809	0.0132		0.038		
Total	0	0.1814		0.3419		
Test of overidentifying restrictions: fixed vs random effects						
Sargan-Hansen statistic		RE collapses to pooled regression		RE collapses to pooled regression		
P-value						

Robust standard errors in pare
*** p<0.01, ** p<0.05, * p<0.1

Table 10b. Damage function estimation – Simplified panel regression techniques using all data and including catastrophic indicator variable

Model	(1)	(2)		(3)		(4)
VARIABLES	D_new	D_new	D_new	D_new	D_new	D_new
T2_new	0.00207***	0.00189***	0.00207***	0.00170**	0.00207***	0.00207***
	(0.000239)	(0.000370)	(0.000101)	(0.000584)	(9.79e-05)	(0.000101)
T2_cat	0.00412***	0.00454**	0.00412***	0.00484**	0.00412***	0.00412***
	(0.000800)	(0.00150)	(0.000606)	(0.00175)	(0.000663)	(0.000606)
T2_omit	-0.00100	0.000115	-0.00100**	0.000113	-0.00100*	-0.00100**
	(0.000587)	(0.00136)	(0.000436)	(0.00153)	(0.000518)	(0.000436)
T2_Time	-0.000417**	-0.000834*	-0.000417***	-0.000683**	-0.000417***	-0.000417***
	(0.000147)	(0.000404)	(6.54e-05)	(0.000295)	(0.000113)	(6.54e-05)
T2_Time2	2.65e-05***	4.57e-05*	2.65e-05***	3.70e-05**	2.65e-05***	2.65e-05***
	(8.38e-06)	(2.30e-05)	(5.07e-06)	(1.55e-05)	(7.86e-06)	(5.07e-06)
Prediction						
T2 non-catastrophic	0.0032	0.0017	0.0032	0.0014	0.0032	0.0032
T2 non-catastrophic plus 25%	0.0039375	0.002106	0.0039375	0.0017425	0.0039375	0.0039375
T2 total plus 25%	0.0080575	0.006646	0.0080575	0.0065825	0.0080575	0.0080575
Observations	25	25	25	25	25	25
R-squared	0.950	0.968		0.968		
Adjusted R-squared	0.937					
Liklihood	39.43	44.97	39.43	45.04	39.43	39.43
Fixed Effects		X		X		
Random Effects			X		X	
Hierachaical						X
Cluster at primary author		X	X			X
Cluster at primary model				X	X	X
Cluster at approach level						
Number of clusters		8	8	9	9	10
Cameron & Trivedi's decomposition of IM-test: p-values						
Heteroskedasticity	0.3503	0.3503		0.4058		
Skewness	0.8393	0.9968		0.9985		
Kurtosis	0.2473	0.0955		0.0962		
Total	0.5277	0.7242		0.7981		

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Figure 1a. Tol (2009) data points – original and corrected

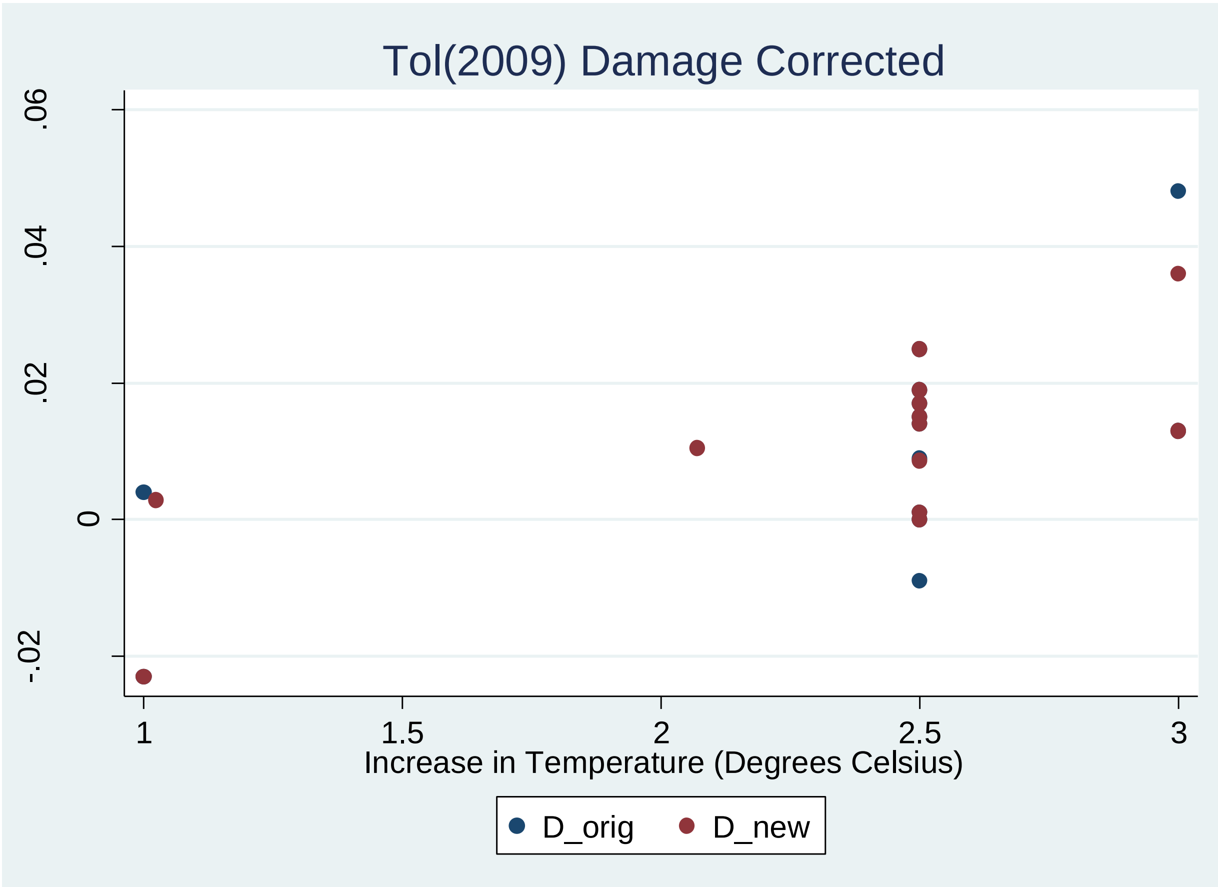


Figure 1b. Data points corresponding to low temperature increases

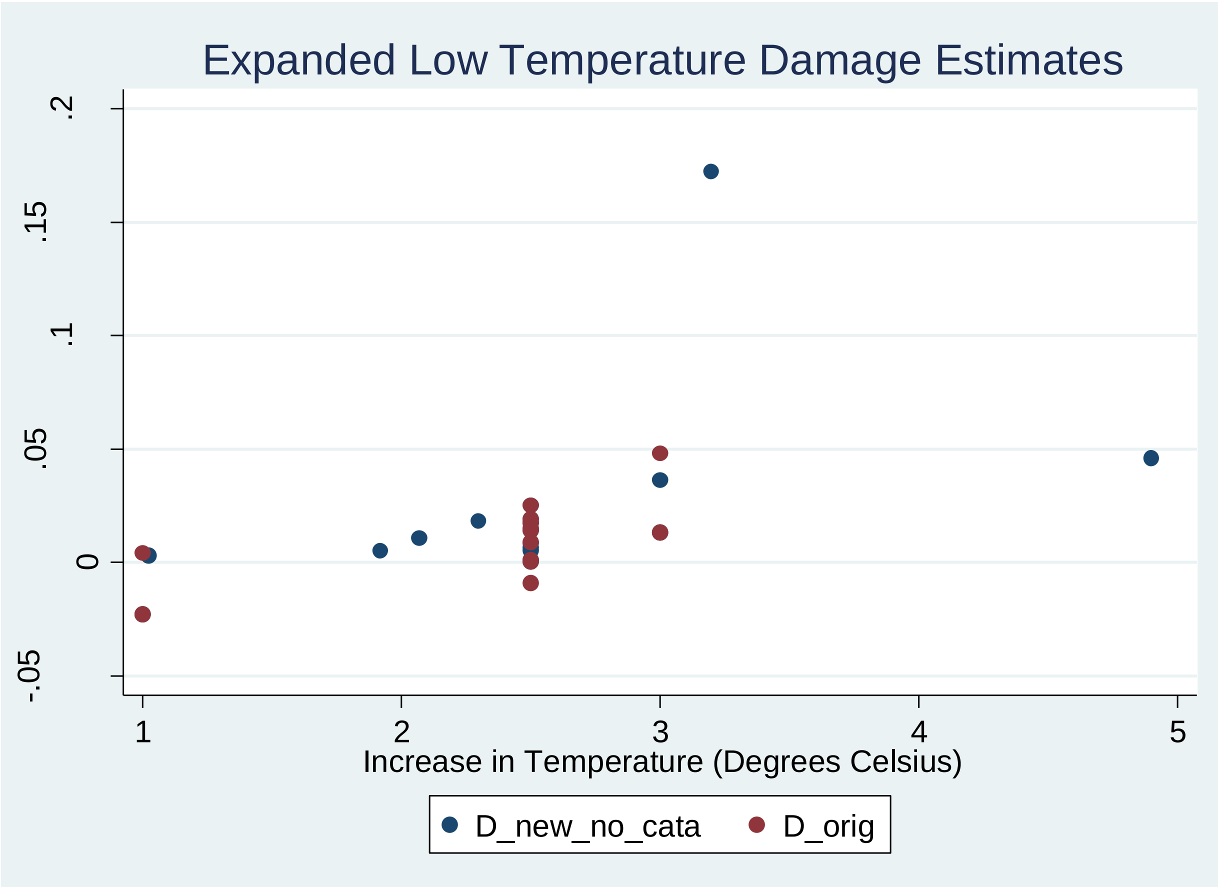


Figure 2a. The 3 Major Updates of DICE from 0 °C to 12 °C

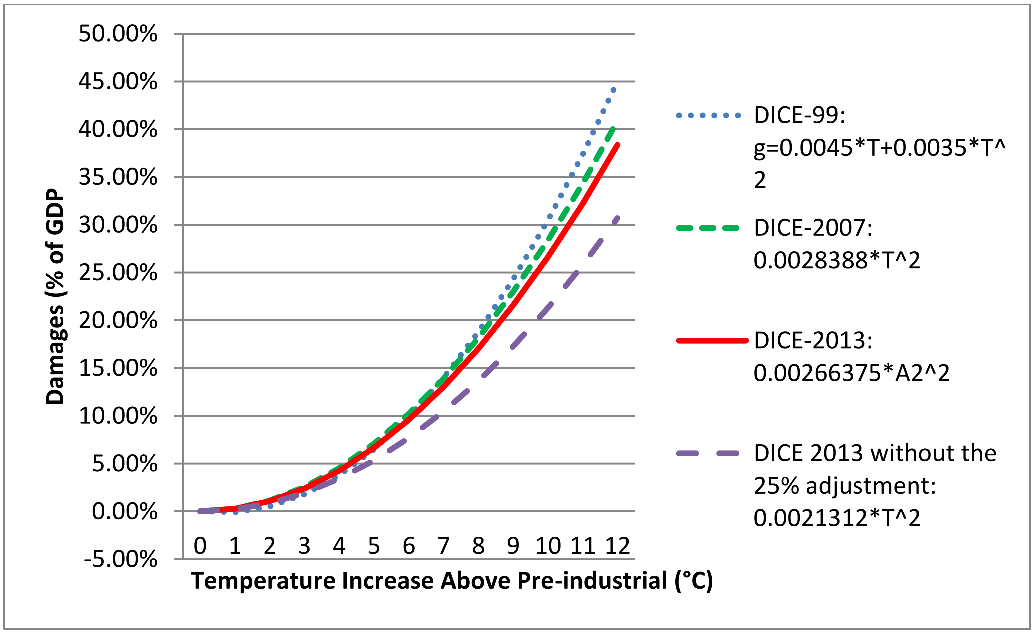


Figure 2b. The 3 Major Updates of DICE from 0 °C to 6 °C

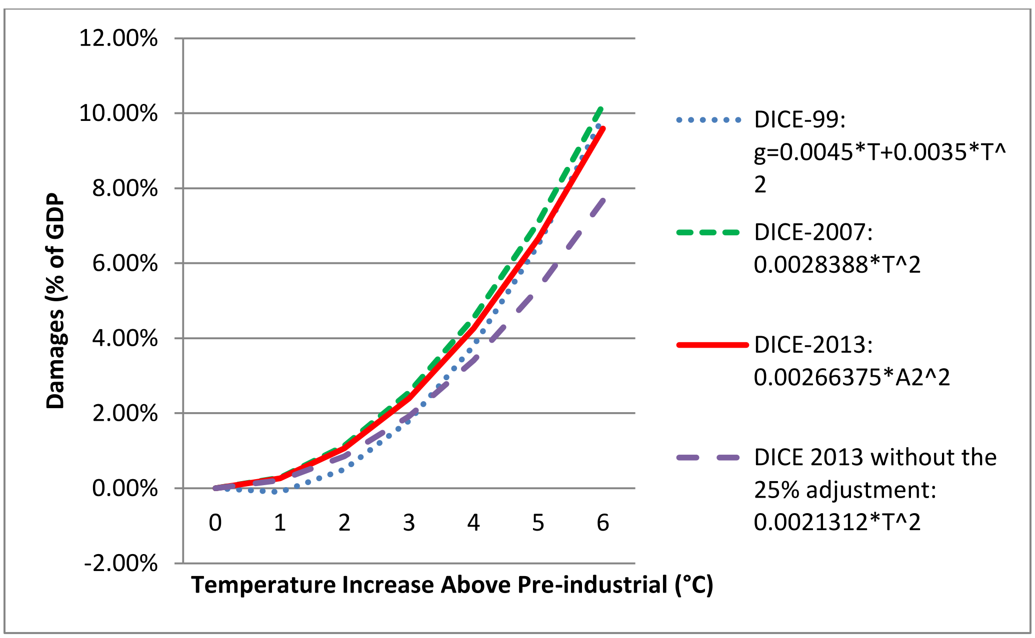
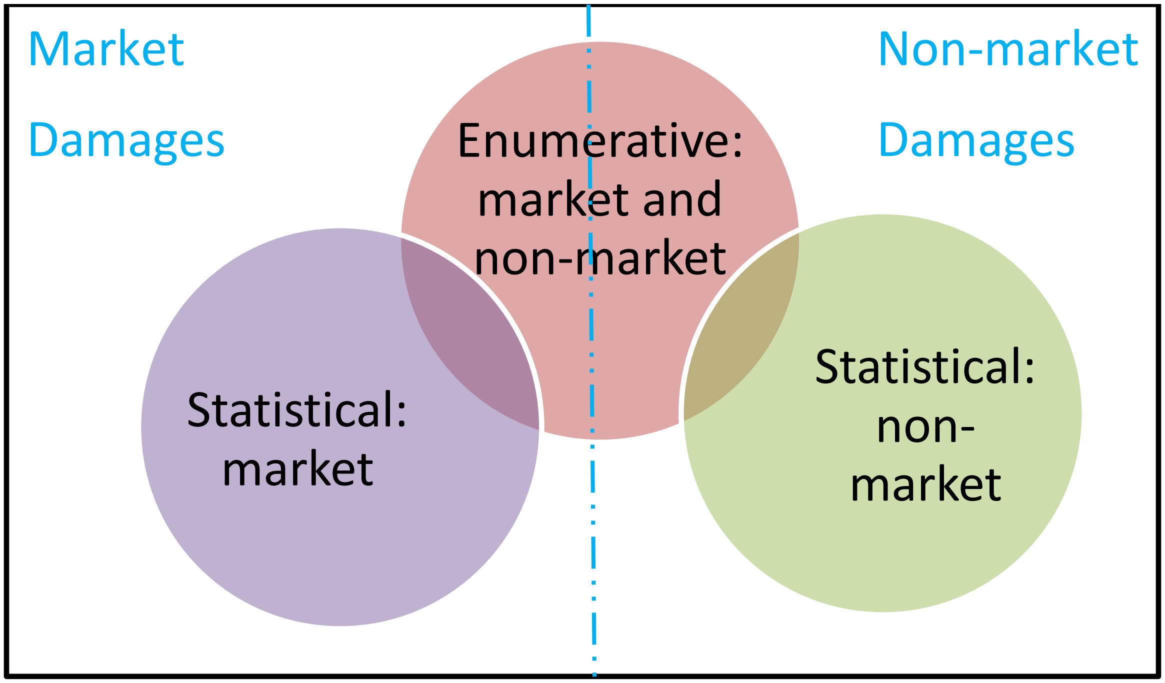


Figure 3. Venn Diagrams of enumerative and statistical impact studies



Appendix – Methodologies for Estimating Global Climate Damages

There are five general ways to estimate climate damages. Each of these methods relies on significant arbitrary judgments by the analysts.

Statistical approach. The first approach, the statistical approach, utilizes econometric methods to estimate climate damages using current observations of the climate. In this approach, authors rely on current spatial variation of climate and economic activity to identify the effect of climate on economic activity. Of the studies cited in Tol (2009), four estimates are based on the statistical approach; this number is expanded to six in the expanded dataset.[44]

While the statistical approach is advantageous in that it directly captures current levels of adaptation and does not require benefit transfer, it has several shortcomings due to their cross-sectional nature. First, these studies often rely on cross-sectional analysis, and thus suffer from omitted variable bias. In particular, cross-sectional analyses can be problematic when they are at the national or regional (above-national) scales, because it can be difficult, if not impossible, to separate non-climatic factors at the national and regional scales from climatic factors. Therefore, climate damage estimates may, and likely do, suffer from omitted variable bias. Of the four statistical studies cited in Tol (2009), only Nordhaus (2006) conducts analysis at a sub-regional scale to avoid this complication. Second, cross-sectional analyses often omit various climate impacts. Because these studies rely on current spatial variation, these studies omit effects that do not vary spatially (e.g., the direct effects of sea level rise which occurs globally), have not yet occurred (e.g. catastrophic impacts, CO2 fertilization, and ocean acidification), or that do not capitalize into the studies market. For example, Redhanz and Maddison (2005) utilizes a hedonic approach that fails to capture market damages and non-market damages that do not capitalize into local land markets; this includes ecosystem services, recreational values of non-residents, many ecosystem services, and non-use and existence values. Finally, many of these studies only aim to capture the effect of climate change on market or non-market benefits. In Tol (2009) and the expanded dataset used in this paper, all of the statistical estimates of climate damages represent incomplete climate damage estimates. In particular, in the expanded dataset, three studies capture only market impacts and the remaining three capture only non-market impacts. This represents a serious complication for meta-analysis studies.

[44] Mendelsohn, Morrison, Schlesinger, and Williams (2000), Nordhaus (2006), Maddison (2003), and Redhanz and Maddison (2005) rely on the statistical approach. Mendelsohn et al (2000) is a hybrid approach in that the authors combine experimental evidence, which corresponds to the second approach discussed, and cross-sectional evidence in a computer general equilibrium model. Additional statistical estimates in my expanded dataset are Nordhaus (2008b) and Rehdanz and Maddison (2011), which are updates of Nordhaus (2006) and Redhanz and Maddison (2005), respectively.

Enumerative approach. The second approach, the enumerative approach, utilizes physical impact studies from the sciences (climate models, impact models, and/or laboratory experiments), and then assigns these impacts a price using market prices or economic models. In the case of non-market services (health, biodiversity, etc.), benefit transfer is often necessary to apply values derived in non-climate studies, such as the value of a human life, to the climate literature. Many of the scientific and/or valuation studies in developed nations or regions, often the United States or Europe, so benefit transfer is also employed to elicit the value globally. Many of the enumerative valuation studies rely heavily on author discretion. Eight estimates in Tol (2009) are based on the statistical approach, and this number is expanded to twelve in the expanded dataset; several of these estimates are explicitly based on or updates of other cited estimates.[45]

While the enumerative approach is advantageous in that it is based on scientifically estimated effects, the enumerative approach has several disadvantages. First, as discussed in the previous paragraph, these studies rely on the benefit transfer approach to transfer benefits over space and time. In addition to the possibility of substantial estimation error from the use of benefit transfer methods (Brouwer and Spaninks 1999), enumerative studies often transfer damage estimates to significantly different regions against the consensus in the benefit transfer literature (Johnston and Rosenberger, 2010). Second, these models often unrealistically model adaptation. These estimates underestimate or overestimate adaptation, often assuming that there is no or perfect adaptation (Tol, 2009).[46] Third, enumerative studies often rely on sector by sector analysis whereby the damages are pulled from disparate studies. According to Tol (2009), this may result in overlap of sector damages, such as in agriculture and water resources. However, many other economists argue that inter-sector damages are missing, such as the effect of water resources on agriculture (Howard, 2014). Fourth, many types of climate impacts and market and non-market sectors are omitted from the damage estimates, and uncertainties are large (Howard, 2014). Fifth, the enumerative approach often relies on economists drawing on physical impact studies without working directly with scientists; this piecemeal approach, which ignores the advantage of scientists and economists working together, and can result in the misuse of scientific data. In particular, results from one to two case studies are often extrapolated deep into the future with temperature and income levels that far exceed what the original case studies intended (Howard, 2014). Last, many of the non-market values are derived

[45] In Tol (2009), Fankhauser (1995), Nordhaus (1994a), Tol (1995), Nordhaus and Boyer (2000), and Tol (2002a; 2002b) use the enumerative approach. In addition, several of the studies cited by Tol (2009), indirectly utilize the enumerative approach by calibrating their damage function using a previous enumerative study's damage estimates: Nordhaus and Yang (1996), Plambeck and Hope (1996), and Hope (2006). The extended dataset in this paper includes four additional enumerative studies - Hope (2009), Nordhaus (2008a), Tol (2013), and Ackerman and Stanton (2012) – of which the first three are updates to previously included enumerative studies.

[46] According to many economists, enumerative studies error on the side of too much adaptation.

using willingness to pay measurements, rather than willingness to accept. Given that the former is often lower than the latter, current measurements may be underestimating damages.

Interviews. Damage estimates can also be derived by interviewing experts; Nordhaus (1994b) is the only study in Tol (2009) that employs this strategy. While this approach may be advantageous in that it can capture difficult to estimate (catastrophic and non-market impacts), it suffers from several disadvantages. First, like stated preference studies, the resulting estimates are based on opinions, rather than market observations. Second, in a related problem, individuals have difficulty considering low probability, high damage events, e.g. black swan events; as a consequence, interviewees may be prone to underestimate climate damages. Third, interviewees may be biased if they know the interviewers opinion on the topic, as Nordhaus mentions that they do (Nordhaus, 1994b). Last, in a related problem, the pool of interview subjects maybe non-random, and may overly represent the bias of the interviewer/author.

Computer general equilibrium. Though none of the estimates discussed in Tol (2009) utilized a computer general equilibrium model (CGE), which is a model of the world economy calibrated with data and specifying how various regional production and consumption sectors in the economy interact such that prices are endogenously determined, several estimates included in the extended dataset, i.e. Bosello et al. (2012) and Roson and van der Mensbrugghe (2012), utilize them.[47] Both studies employ the enumerative strategy of plugging in region-sector damages, but unlike the enumerative strategy, assume that damages effect economic growth rather than consumption. Introducing these climate impacts into the CGE model framework allows analysts to observe how various climate impacts interact to produce a comprehensive estimate of climate damages (Bosello et al., 2012; Roson and van der Mensbrugghe, 2012).

Science. The scientific approach is to estimate the temperature at which the planet becomes uninhabitable to humans; this produces a damage estimate for high temperature increases. While none of the papers discussed in Tol (2009) rely on scientific approach, the extended dataset used in this paper includes one such study, Ackerman and Stanton (2012). In this study, the authors utilize damage estimates drawn from Weitzman (2010) who develops damage estimates based on the physiological limits of human adaptation to rising temperatures estimated in Sherwood and Huber (2010).[48]

[47] Following Tol (2013), two estimates are drawn from Roson and van der Mensbrugghe (2012): one for a 2.3 °C increase and another for a 4.9 °C increase.
[48] Two estimates are drawn from Weitzman (2010): one for a 6 °C increase and another for a 12 °C increase.