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SOME RESEARCH CHALLENGES IN THE ECONOMICS OF CLIMATE CHANGE

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The paper reviews progress in understanding the economics of climate change with an emphasis on identifying promising advances that are both significant and nonmarginal, as well as areas in which there are key gaps in our knowledge. We highlight several important areas in which important policy-relevant research questions remain: improving estimates of climate damage used for determining the social cost of carbon, refining integrated assessment models, and designing better climate policies. As the world moves to understand and implement country emissions reduction pledges in the context of the UNFCCC, understanding the economics of the problem will become even more important. The hope is that the paper is not only informative for existing economists already working in climate change economics, but that it also serves as an inspiration for economists in other areas, or even those in other disciplines, on ways in which to contribute to solving the problem at hand.

Keywords: Climate economics; social cost of carbon; integrated assessment models; policy analysis; research frontiers.

1. Introduction

A number of recent publications have highlighted the importance of economics to formulating climate policy while at the same time underscoring the inadequacy of current knowledge on the economics of climate change. For instance, the September 2013 forum in the *Journal of Economic Literature* as well as various chapters in the IPCC's recent Fifth Assessment Report highlighted both the substantial recent progress as well as the large remaining knowledge gaps in understanding the economics of climate change.[1] The focus of this paper is in delineating research opportunities and frontiers in the economics of climate change — frontiers that are important to bridge if the world is to make significant progress in climate policy. By "economics of climate change" we mean the economic study of actions that may be taken to manage changes

[1]The *JEL* forum contained three papers: Stern (2013), Pindyck (2013), and Weitzman (2013). The IPCC content is found particularly in Chapter 3 of the third working group (Kolstad *et al.*, 2014), as well as Chapter 17 of the second working group Chambwera *et al.* (2014).

in the climate (such as mitigation, adaptation, and geoengineering), as well as the significance for human wellbeing of any climate change that does occur.

For nearly half a century, a small but growing group of economists has been striving to understand the economics of climate change.[2] Remarkable advances have been made, primarily through the use of empirical methods and the development of numerical and theoretical models of the climate–economy system (e.g., Nordhaus 1977, 1992). But climate change is a complex and challenging problem and there is still much that we do not yet understand. As the world moves to implement country pledges in the context of the UNFCCC, understanding the economics of the problem will become even more important. It is therefore imperative to take stock of the current state of knowledge as well as understand which areas are the most fertile for future work.

In this paper, we highlight key gaps in our knowledge of the economics of climate change and suggest promising directions for new research, focusing in particular on three topics that we believe to be of distinct importance. First, we discuss the social cost of carbon (SSC) and how it could be improved, including the consideration of catastrophes, nonmarket damages, impacts in developing countries, growth versus level effects, adaptation, and the use of discount rates. We then turn our attention to the integrated assessment models (IAMs) used in the computation of the SCC, arguing that, in addition to the need for incorporating the latest scientific understanding, we need to examine leading models' consideration of uncertainty, the aggregation of heterogeneous agents, and technology options. Finally, we look at ways to improve climate policy design, in particular through the use of *ex post* analyses, insights from behavioral economics, the consideration of technology policy, and considerations specific to the developing world. With significant time and resources, we believe that progress can be made and many of these gaps filled.

2. Improving Understanding of the SCC

The SCC is a central concept in climate change economics, but there remains much controversy over how it should be computed and subsequently utilized. The SCC is the expected value of a monetized estimate of the change in social welfare over all future time from one more ton of carbon emitted today, conditional on a specific trajectory of future global emissions and economic and demographic growth. Such a calculation is by no means trivial and is sensitive to a number of assumptions. It is usually estimated using an integrated assessment model (IAM — see next section), though widely used values for the SCC are often criticized for the underlying empirical basis used by these IAMs to project climate damages. In calculating the SCC, IAMs rely on a number of assumptions such as those about future population levels, growth of productivity and

[2] Nordhaus (1975, 1977) is the first "modern" analysis, to our knowledge, of the economics of climate change.

innovation, the climate sensitivity parameter (i.e. how global temperature would react to doubling in atmospheric CO_2 concentration), and the nature of the damage function (i.e. how the model translates temperature increases into reductions in GDP). As currently defined, the SCC assumes a trajectory of emissions which may be inconsistent with using the SCC to reduce emissions. The importance of these assumptions is reflected in how sensitive the SCC is to their assumed values and functional forms. However, given our current state of knowledge, many of the assumptions used in today's IAMs can only ever be educated guesses.

The significant uncertainty underlying the SCC has led some to claim that IAMs can tell us "very little" about climate change policy and that their inadequate treatment of the possibility of catastrophes severely limits their usefulness in determining an appropriate value for the SCC (Pindyck, 2013). Such criticism is often used as motivation for exploring other methods of SCC calculation. One such alternative that has been suggested is expert elicitation, a method that would be not be as affected by the complex interacting uncertainties associated with modeling climate–economy interactions. Nevertheless, an expert elicitation approach attracts its fair share of criticism as well, particularly given the fact that IAMs are frequently parameterized using expert elicitation and therefore their uncertainty about things like climate sensitivity are already reflections of the uncertainty of experts in that field.

However, despite these questions and criticisms pertaining to the calculation of the SCC by IAMs, IAM-estimated SCC values are actively being used in a host of regulatory analyses of policies directly and indirectly related to climate change both in the US and in other countries. In the US, the SCC has been estimated by the Interagency Working Group (IAWG) (Greenstone *et al.*, 2011). The effort utilizes three IAMs to calculate an "average" SCC estimate: Dynamic Integrated Climate and Economy (DICE), Policy Analysis of the Greenhouse Effect (PAGE), and Climate Framework for Uncertainty, Distribution and Negotiation (FUND). The latest IAWG central estimate for the SCC is $36 (IAWG, 2015). Since the initial estimation of the SCC by the IAWG in 2010, the US Environmental Protection Agency (EPA) has utilized the figure to establish standards such as the latest Corporate Average Fuel Economy (CAFE) standards, New Source Performance Standards (NSPS), standards for new stationary sources, and proposed carbon pollution standards for future power plants. It is clear that policymakers will continue to need and use these estimates in policy analysis. Thus, there is significant value in ensuring that the SCC estimate utilized by policymakers is refined, well-supported, and up-to-date (Pizer *et al.*, 2014). To this end, we highlight six promising research directions related to the SCC.

2.1. ***Catastrophes***

The first needed improvement is to better understand the likelihood and impact of extreme climatic events. In an uncertain world, catastrophes (low-probability, high-damage states) may be far more important, in terms of the *expected value* of climate damages, than more likely events with moderate damages. The SCC, by definition, represents the cost to society of additional CO_2 *on the margin*. Therefore, an event that tipped the climate into a high-damage state, even if it is a low-probability event, would significantly affect how we viewed the social cost of the ton of CO_2 that caused the tip. Without an understanding of such high-damage states, such as the melting of the Greenland ice sheet, we may greatly underestimate the marginal SCC. It has been shown that low-probability, high-damage events will significantly alter the SCC estimates and therefore understanding these events is important, particularly if probability distributions exhibit "fat tails" (Weitzman, 2011).

Other recent work in this area has focused on modeling endogenous tipping points in the climate, with damages different in the pre- and post-tipping world and a policymaker's actions affecting the point at which the climate tips, if at all (Lemoine and Traeger, 2014). There has also been recent research arguing that the interdependence of catastrophic events has been largely ignored and can be extremely important in determining which actions to take (Martin and Pindyck, 2015). Finally, in addition to a better understanding and modeling of catastrophes, improving our understanding of the consequences of large, possibly noncatastrophic, changes in the climate (e.g., more than a few degrees average temperature change, or an increase in particularly destructive hurricanes) could significantly improve the robustness of impact estimates. It is therefore imperative that estimates of damages include analyses of both catastrophes and very large non-catastrophic outcomes. Though natural science understanding of climate extremes, including likelihoods, is also a work in progress, better representation in damage estimates of advances in the natural science continues to be an extremely valuable and productive area for research. Conversely, economists can also be useful in identifying high payoff areas for natural science research related to these nonmarginal events.

2.2. *Nonmarket damages*

Second, research is needed on how best to represent potential damages that are poorly captured in typical economic output measures. These nonmarket damages range from standard effects which economists have been studying for decades, such as ecosystem services or health effects, to less well-known areas, such as the potential costs of increased civil conflict or impacts on subjective well-being. These damages could be quite large (and may be positive or negative), but are either omitted from current damage estimates or are represented in an *ad hoc* way. For instance, recent studies have presented evidence linking climatic events to human conflict (Hsiang *et al.*, 2013). It is likely that research on nonmarket damages will benefit from close collaboration with other disciplines such as political science, public health, and the natural sciences, among others. While estimates of nonmarket damages are already incorporated into some SCC calculations, these estimates are coarse and are often implemented as proportionality factors relative to monetized damages (as in DICE). As research on nonmarket damages grows, it is critical that these improved estimates be incorporated into the SCC.

2.3. *Developing countries*

Third, we need a better understanding of whether impacts in developing countries are substantially different than those in developed countries. Impacts estimated in a developed world context are often extrapolated to developing countries, usually for pragmatic reasons of data availability, but there is surprisingly little quantitative understanding of how vulnerability to climate change shifts as countries develop. In fact, recent evidence suggests little difference between rich and poor countries in either the historical sensitivity of agricultural output or economy-wide output to changes in temperature (Burke *et al.*, 2015; Schlenker and Lobell, 2010), but it is unknown whether these findings extrapolate to other sectors or hold for the large expected temperature increases in coming decades. Many estimates of damages assume that vulnerability does not change over time, while others — such as those based on the FUND model — assume larger proportional damages at lower income levels. We need a much better understanding of impacts over a broad range of countries and how these impacts might change with the process of development, specifically whether changes in vulnerability are simply due to changing sectoral compositions of economies (i.e., lower-income countries tend to have more agriculture) or due to more complex factors (e.g., institutional willingness and capacity to invest in adaptation). Determining the burden on low-income households is particularly important in the developing world. While some data are available (e.g., expenditure surveys and national accounts), further methodological work is necessary in order to extend such work to countries with low data availability.

2.4. *Growth versus level effects*

Fourth, much more work is needed on how aggregate economic output is affected by changes in the climate. One central area of debate is whether or not changes in temperature and climate affect the contemporaneous level of output or are more persistent. Some studies have found that higher temperatures can substantially reduce economic growth in poor countries by affecting growth *rates* rather than just the *level* of output (Dell *et al.*, 2012; Burke and Emerick, 2015; Moore and Diaz, 2015). Other studies have suggested the opposite (Heal and Park, 2016). Additionally, there is research that shows overall economic productivity is nonlinear in temperature in both developed and developing regions (e.g., Burke *et al.*, 2015) and that increased temperatures could affect capital accumulation, therefore creating dynamics that would not be captured by the standard focus on "level effects" (Fankhauser and Tol, 2005). Further work focusing on structural modeling of the mechanism through which this growth effect takes place, as well as empirical work devoted to discerning these pathways, are necessary in order to more confidently be able to project damages out of sample.

2.5. *Adaptation*

The fifth major area in which there is much scope, and necessity, for future research pertains to adaptation. Adaptation has the potential to drastically influence the realized damages from climate change, but is one of the least explored areas of climate economics and there exists very little empirical guidance on how much the potential for future adaptation should be built into damage functions (Chambwera *et al.*, 2014). A great deal of progress has been made in estimating the effect of long-run differences in climate on economic activity (e.g., Mendelsohn *et al.*, 1994; Kelly *et al.*, 2005). An even larger literature exists on the effect of short-run fluctuations in temperature and precipitation ("weather") on economic activity. However, there is typically little guidance as to what these responses to weather imply about responses to longer-run changes in temperature or precipitation ("climate"). Cross-sectional estimates provide some information on these longer-run responses, but it is difficult to determine whether cross-sectional variations across geography can be projected into changes over time at a single geographic point. Long differences approaches offer a way to identify the impacts of longer-run changes in climate over time (Burke and Emerick, 2016), but this approach is not always applicable to relatively data-poor regions. As suggested by Dell *et al.* (2014), a next frontier for research is in creatively and credibly connecting estimates of the effect of weather to the effects of changes in the climate to understand both the nature of economic impacts and the scope for adaptation.

Another area in which understanding adaptation would add value would be in considerations of sea-level rise. Whether or not communities choose to adapt or retreat will be an important question with large consequences for the cost of sea-level-rise associated with increasing global temperatures and therefore plays an important role in determining the SCC. Additionally, society's ability to adapt is sure to change over time, particularly with technological progress.

2.6. *Discount rates*

Finally, the choice of discount rate has always been contentious, given both the sensitivity of the SCC to the parameter and the longstanding controversial nature of discounting more generally. Analyses using lower discount rates have suggested more stringent optimal policies. A particularly controversial example of this is the *Stern Review on the Economics of Climate Change* (Stern, 2006). In it, the use of an extremely low discount rate leads to a conclusion that without action, climate change will result in a loss of at least 5% of global GDP each year "now and forever" and that the estimates of this damage could rise to 20% or more if we take wider impacts into account. In order to avoid such great losses, significant action is called for. This result has yielded much criticism for being inconsistent with other intertemporal societal choices (Nordhaus, 2007; Weitzman, 2007). This debate has become very well known and there has been effort in recent years to think more carefully about the "right" discount rate for climate change economic analyses.

Recent work has highlighted the fact that the above debate between high and low discount rates could be partly resolved by considering the fact that there could be multiple discount rates, depending on the nature of the decision-maker: one tied to the market rate of interest and one derived from a social welfare function (Goulder and Williams, 2012). Current IAMs seem to conflate the two, and this conflation has contributed to debates over whether discount rates are "too high" or "too low" — a debate often grounded in each side focusing on a different aspect of what is being discounted. Economists should be aware of these implications when choosing between discount rates.

Other recent work on discounting has highlighted two additional concerns. First, even within the less normative market discount rate, there are open questions as to how the parameter should incorporate, or reflect, uncertainty. Second, there is important disagreement about whether to use a constant exponential discount rate or a Declining Discount Rate (DDR) schedule. The US government currently uses a constant rate, but there is much to suggest that a DDR is a compelling alternative (Arrow *et al.*, 2014).

Overall, the discount rate is extremely controversial, in part due to the subjective nature of some of its aspects (see Kolstad *et al.*, 2014 for an extended discussion of discounting in climate change analyses). It is clear that considerations of the SCC will continue to be very closely tied to considerations about discounting and therefore further clarity in this area is an important area for future research.

3. Improving Integrated Assessment Models

A second main area for research in the economics of climate change is in improving one of the field's main tools for understanding climate–economy linkages: the IAM. IAMs continue to play a central role in the economics of climate change, but as mentioned above they are controversial and not embraced by all economists or policy-makers. Some view IAMs as having little direct policy relevance, only providing intuition on how the assorted components of the climate–economy system are connected. Others view IAMs as useful for policy but plagued by weak empirical foundations. Some in the field are skeptical of IAMs, but feel that they provide the only structured and theoretically consistent approach available for thinking about the SCC. Absent this approach they argue that we are left only with *ad hoc* estimates. Needless to say, this remains a controversial area within the economics of climate change. Leaving aside the ultimate usefulness of IAMs, we highlight four research areas (beyond the better representation of climate damages discussed above) that could improve IAMs and shed additional light on their utility, particularly with respect to their use as the main method by which to calculate the SCC. Generally, our discussion here applies to those IAMs that are "complete" in the sense of including both mitigation costs and the damage from changes in climate, and our comments pertain mainly to those "complete" IAMs used to estimate the SCC. Models which only deal with mitigation and not damages can be useful, but not for estimating the SCC.

3.1. *Updating*

First, IAMs should have a more structured way to incorporate and update new information about key model parameters, such as information about the damage function. For instance, despite the new empirical evidence generated in the last decade on how various sectors of the economy respond to changing temperatures, damage functions in IAMs often rely on studies done 10–25 years ago. Additionally, the three models utilized by the IAWG to calculate the SCC — DICE, FUND, and PAGE — not only draw to some extent on dated literature, but they are also calibrated to one another therefore creating interdependency between the models (Rose *et al.*, 2014). In many cases, whether or not a model's parameterization and functional forms are up-to-date depends very much on the modeling team. The creation of a more structured way to incorporate new information into IAMs would be of great value in keeping the models current, particularly those whose outputs are actively being used in policymaking.

3.2. *Uncertainty*

Second, there are a number of areas for structural improvements in IAMs. One such area is the treatment of uncertainty. We often view uncertainty in IAMs simplistically in terms of parametric uncertainty on the part of the modeler while assuming deterministic decision-making within the model, converting uncertainty in key parameters to probability distributions on outcomes. However, such uncertainty is external to the model, in contrast to models where agents within the model make decisions under uncertainty. Such models can be computationally complex (e.g., Kelly and Kolstad, 1999) and the gains from adding this complexity to IAMs, including improving computational methods, is currently an area of active research.

Given an environment with many stochasticities and types of uncertainty, including but not limited to climate sensitivity, damages, and tipping points, the nature of learning is also important in determining the optimal policy. In this area, further research into modeling structural uncertainties, uncertain economic parameters (e.g., cost of new technology), and imperfectly unobserved tipping points is warranted.

3.3. *Aggregation and equity*

Third, key existing IAMs examine the wellbeing (utility) of a representative agent, either globally or by region. However, this may be inappropriate if impacts differ greatly across a region or by type of agent. For instance, many IAMs aggregate rich and poor economies, which could lead to under-representation of impacts on the more vulnerable developing world as well as overlooking the role that trade may play in redistributing local climate impacts. In this manner, changes to wellbeing in the first world may inappropriately swamp welfare changes in the third world (e.g., Anthoff and Tol, 2010). As climate change is treated as a global problem, but with more regionalized decision makers, understanding the nuances of how equity weighting affects, or should be utilized in, welfare analyses is another research area of interest and importance, particularly to related fields such as political economy.

3.4. *Technology options*

Finally, how IAMs consider technology options, both present and future, is an area that has historically been one of great research interest, but is also one where there is much to still be done, particularly in terms of endogenizing technological change. IAMs are not all created equal in terms of their treatment of uncertainty, how aggregate their representation of the economy is, how detailed their representation of the climate system is, the nature of their damage function, and how technological progress is represented (if they model specific options at all). Some IAMs — DICE, for example — do not have any technological detail and represent technological progress as an autonomous efficiency parameter that increases the efficiency of the economy by a given amount each year. Other IAMs have detailed representations of various energy technologies, such as coal, gas, nuclear, various renewables, biofuel, and carbon capture and storage (CCS), though this detail may produce nothing more than the appearance of precision. In determining what the optimal carbon tax should be IAMs can be very sensitive to which technologies are available to them and at what cost. While we have a good idea of today's technologies, it is very uncertain what future technologies will look like, and it is also uncertain how current and future technologies will diffuse. Areas such as technological diffusion, the possibility of geoengineering, the impacts of research and development and learning-by-doing are areas of critical research for the IAM community.

4. Improving Climate Policy Design

A final critical area for research is in improving our understanding of climate policy design, both in terms of how to best achieve certain policy objectives given political realities, as well as how to best anticipate and understand the implications of particular policy choices. For instance, substantial political resistance to carbon pricing coupled with the emergence of a piecemeal approach to both domestic and international climate policy-making means that it is no longer sufficient to simply study how to price the climate externality in a "first-best" world with no other economic distortions. While existing research emphasizes that many existing "second-best" policies such as efficiency standards and support for renewables are cost-ineffective relative to a carbon pricing, such second-best policies continue to be implemented for political reasons. Simply "getting the price right" is therefore no longer enough, and research moving forward must consider the practical dimensions of how to best design and implement such policies in order to meet the goals that lie ahead (e.g., Borenstein, 2012).

4.1. *Policy evaluation*

More *ex post* policy evaluations of the effectiveness of various climate-relevant regulations can be highly valuable in this regard (Greenstone, 2009), especially as countries begin to actualize their pledges in the context of the UNFCCC process following the Paris meeting in 2015. A variety of policies, such as carbon pricing schemes, tradable obligations, fuel taxes, renewable portfolio, and energy efficiency standards, are already in use in different parts of the world. Rigorous empirical analysis of the effectiveness of these existing policies could be of significant help to countries trying to cost-effectively meet a given emissions target. For instance, recent work has developed clever approaches to credibly forecasting what the marginal abatement cost (MAC) of a yet-to-be-implemented policy could be (Meng, 2013). Overall, whether it be *ex ante* or *ex post*, refining our methods of policy analysis is a valuable task.

4.2. *Behavioral economics*

Using the lens of behavioral economics to understand how individuals' climate- and energy-related behavior is shaped by incentives is also an active area of research, and an important agenda going forward. This research includes new analyses of how individuals respond to social norms and information provision when making energy consumption decisions. By neglecting these responses and only considering the explicit costs of a technology, many engineering estimates widely used in policy analysis could provide a biased estimate of mitigation and adaptation costs. The "energy efficiency gap" is one area which illustrates the significance of failing to take behavioral responses into account (though there are also plenty of neoclassical nonbehavioral reasons for the "gap"). Recent work has found that engineering studies may overestimate the size of this gap and thus overestimate the impact of potential policies (Allcott and Greenstone, 2012; Gillingham and Palmer, 2014). When the effectiveness of a given policy is closely tied to how individuals interact with it, it is important to understand these dynamics more clearly. A "behavioral" approach to energy and climate economics will be an important component of the improved *ex post* and *ex ante* analyses referenced above.

4.3. *Technology policy*

While better estimates of the likely costs of existing policies is critical, the costs of addressing climate change in the long run given current technologies could be very large relative to global output, making technological progress critical to limiting climate change. This issue has been highlighted in the context of IAMs. A large body of work on the pattern of innovation exists, but there is less empirical evidence on how combinations of R&D and climate policies work in driving innovation (Popp *et al.*, 2010). How innovation is influenced by the global nature of both technology flows and climate policy is also poorly understood. Technology diffusion within and across countries is likely to play a key role in meeting future climate policy targets. Additionally, the setting of these targets tends to involve predictions of what future technologies will look like and the costs of achieving policy goals also depends heavily on available technology. Further research to better understand how low-carbon innovation comes about and diffuses within and across countries will be critical to understanding the broader climate policy landscape.

4.4. *Policy in the developing world*

Finally, it is important to understand that policy considerations will vary across countries and particularly between the developed and the developing world. A policy that works well in one place may not be appropriate for another. For instance, while some developing countries such as Mexico and China are experimenting with carbon pricing, such pricing has faced opposition elsewhere in the developing world, mainly out of concern that it could hamper economic growth by both constraining emissions and increasing their cost. However, while carbon pricing will inevitably raise costs for certain sectors, tax evasion rates in developing-country settings are often lower for energy taxes as compared to income taxes, meaning that implementation of a carbon tax may allow for a developing-country government to raise revenue while simultaneously achieving climate policy goals. A significant area of research will thus be to better understand the feasibility and impact of different policy tools in different political and institutional contexts. These differing contexts include countries with heavily subsidized fossil fuels, large informal sectors, and/or large state-owned sectors. Therefore, research to understand the policy options to make low-carbon technologies more easily adoptable in the developing world will also be essential.

5. Moving Forward

In this paper, we have highlighted some of what we view as the most important and fertile areas for future research in climate economics. The SCC has received prominence given its visibility and widespread use in the economics of climate change, but climate economics is not just about the SCC. IAMs are tools that not only help us think about what an appropriate value for the SCC might be under various conditions, but they are also useful for thinking about a variety of questions in the energy-economic-environmental nexus. Policy analysis at both the micro- and macro- levels is also an area of climate economics that can be related to the SCC, but is much broader in scope. In order to tackle the deep and complex environmental issue that is climate change, we need expertise across all of these areas of climate economics. Substantial progress has been made across the board since the late 20th century when climate economics began to differentiate itself as a field in itself within the broader umbrella of environmental economics. However, many unanswered questions remain in many key areas. The way forward includes meticulous data-intensive empirical work to strengthen the foundation upon which policy-relevant "end products" are based (such as the SCC and IAMs), along with research aimed at carefully defining and reframing the questions that should be asked.

Our suggested research agenda is not comprehensive, but we believe it goes a long way in attempting to understand and articulate what the frontier of knowledge in the field is and identifying areas of promising research that can be both significant and nonmarginal. With significant time and resources we believe that progress can continue to be made and many important knowledge gaps filled. As the global community moves to tackle the climate change problem, there is much scope for economists to positively contribute to the effort. It is our hope that this paper is not only informative for those already working in the area, but also serves as inspiration to other economists, and those in other disciplines, for ways in which to contribute to understanding what is one of the deepest and most complex environmental — and economic — issues of our day.

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