and succeed or fail[10]. In this respect, Michael Lipsky's seminal study is worth mentioning, as he shows how street level bureaucrats reinterpret policy guidelines to deliver actions that fit their beliefs and sense of justice[11].

Recent research on adaptation starts to offer possible alternative routes to policy analysis that explore deeper causal processes at work. For example, Dowd *et al.*[12] used social network theory and showed that earlier and more transitional adapters were less likely to have close ties with family and community, and more likely to have external network ties, than their counterparts. Similarly, Cashore and Wejs[13], adopting a legitimacy perspective on policy-making, explored the regulative, normative and cultural institutional dimensions of constructing legitimacy through the climate secretariat in Aarhus, Denmark, and the effect that different forms of legitimacy had on resulting adaptation planning. Their analysis provides detailed insights that allow for concrete interventions in practice, for example, when regulatory elements are needed to build legitimacy. These studies are informed by current work in the social sciences and are conceptually nuanced and empirically grounded.

Our Correspondence is not merely an expression of academic or methodological concern: A mismatch between academic models and the practical realities in which practitioners operate translates into poorly informed future policy prescriptions. Almost ten years of barrier thinking and analysis have yielded very limited advice about how to intervene in practice to secure better outcomes[14,15]. The examples mentioned above provide detailed explanations of the decision dynamics and causal processes that go into climate change policy-making and practice, and therefore are far more useful to practitioners and academics than functionalist approaches to adaptation. By opening up the black box of decision-making a whole range of more tailored interventions become available to address the challenges of adaptation in practice. Hence we argue that the biggest 'barrier' to adaptation might very well be the concept of barriers itself and how it is currently being used in studying adaptation decision-making. ❐

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Reply to 'Opening up the black box of adaptation decision-making'

Eisenack *et al.* reply — We are encouraged by the fact that our recent Perspective on the new frontiers of adaptation barriers research[1] is generating academic debate. We hope that others will engage and thus help to advance a scientifically rigorous and practically relevant research agenda. Here we would like to respond to the Correspondence from Biesbroek and colleagues[2]. We see as their main points that research on adaptation barriers unavoidably implies a 'functionalist', simplistic view of adaptation processes, and that 'barrier thinking' (and presumably all research on barriers) should be discarded altogether.

Although we join Biesbroek *et al.* in criticizing previous research on adaptation for often being naive about individual and collective decision-making, we see scope and reason for research on barriers that goes beyond what they call a functionalist framing[2]. First, we would argue that it is crucial in a world of climate change to analyse whether the pace of changing institutions and practices in place to fulfil particular societal purposes is commensurate with that of external change, and if not, to explain that disconnect. Such a line of inquiry is not just interesting scientifically, but also important for practical decision-making. It is inherent in actors' justifications for pursuing adaptation to climate change. Second, we would argue that researching barriers to adaptation is not necessarily tied to a naive conceptualization of decision-making. Although we appreciate that Biesbroek *et al.* propose implementation research as a further approach to investigate how adaptation occurs, we do not perceive this approach as an exclusive alternative. Researching the implementation of adaptation is merely the flip side, and in many ways the logical twin, of researching barriers (which aims to explain 'implementation deficits', as coined by Hupe[3]).

The research focuses outlined in our Perspective are concrete proposals for 'opening up the black box of adaptation decision-making', so as to identify and explain the reasons why adaptation is delayed, less effective, or does not take place. Asking for greater attention to "power struggles, misfortune, organized irresponsibility and social learning — as well as policy innovation and diffusion" repeats this very request for more explanatory and actor-centred adaptation research. Such research is one possible way to analyse the many facets and dynamics of individual and collective adaptation decision-making. We explicitly emphasize that explanatory adaptation research needs to consider the dynamics of barriers, to avoid an inappropriate static picture

of adaptation. It is further essential to understand the interdependence of barriers within and across scales.

Importantly, and this may be where we truly differ in our thinking from Biesbroek and colleagues, the barriers concept does not imply a top-down or simplistic linear framing of adaptation decision-making. Some case studies, for example, show how grandfathered water-use rights can impede autonomous adaptation by local and private actors[4–6]. Such water rights might stem from governmental policies or might be old traditions of self-organized user communities. Adjustments of such social norms or institutions are messy, non-linear and complex. We think that comparative research would be worthwhile to explain under which actor- and context-specific conditions grandfathering rights systematically support or impede adaptations. Numerous further studies now analyse barriers with approaches that acknowledge complexity, unforeseen contingencies and dynamic processes[7–11].

Biesbroek *et al.* further suggest that focusing research on barriers implicitly entails the normative assumption that decision-making should result in adaptation. Quite to the contrary, we overtly decouple the definition of barriers from the discussion of adaptation success: "a 'barrier to adaptation' is (1) an impediment (2) to specified adaptations (3) for specified actors in their given context that (4) arise from a condition or set of conditions. A barrier can be (5) valued differently by different actors, and (6) can, in principle, be reduced or overcome"[1]. We thus explicitly state that barriers are in the eye of the beholder, and that some actors may well welcome perceived barriers. There is no claim that valuations are shared and conflict-free between actors. Thus, barrier research can deal with the issues raised by our colleagues in an analytically rigorous and practically relevant way without being tied to one particular normative view.

We appreciate the Correspondence from Biesbroek *et al.* for emphasizing three aspects for furthering the research agenda on barriers to adaptation. First, we wholeheartedly agree that a better understanding of real-world adaptation policy and decision-making processes is absolutely essential if science is to explain barriers adequately and — maybe more importantly — usefully inform these societal processes. Second, research on identifying, explaining, and thus helping to deal with barriers, is not the same as adopting a functionalistic black-box approach. The barriers concept is compatible with nuanced frameworks and theories of decision-making from different disciplinary perspectives, as many examples of published research have shown. Better use should be made of existing frameworks and theories in future adaptation research, for example from political, decision and cognitive sciences. Third, in our view, discarding the concept of barriers to climate change altogether would risk losing an important device for fruitful interaction: barriers serve as a 'boundary object', intuitively and widely understood by both practitioners and scholars from different disciplines. This fosters a key priority for the future: collaborative and comparative research that enhances trans-disciplinary learning across cases, about empirically proven ways in which particular actors can deal with particular barriers to adaptation. This promises to be real-world research of potentially high academic and societal value. ❐

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CORRESPONDENCE:

Emissions accounting for biomass energy with CCS

To the Editors — Sanchez *et al.*[1] provide a viable technological roadmap for using biomass energy with carbon capture and storage (BECCS) in the western United States[1]. However, they oversimplify emissions accounting by assuming a zero or negative carbon emissions factor. Accounting for total lifecycle emissions is perhaps the greatest challenge in deploying biomass (in solid, gaseous, or liquid form) to reduce carbon emissions[2,3].

When utilized to generate electricity, emissions sinks and sources for biomass occur in two different sectors. As plants grow, they take up CO_2 and store it. When combusted, the stored CO_2 is released and contributes to emissions. Accordingly, counting the emissions factor for biomass electricity generation as zero, or negative in the case of BECCS, causes double-counting of emissions on a cross-sectoral basis[4]. These accounting challenges persist when developing national or international carbon control