The race to remove CO_2 needs more contestants

To the Editor — With the ongoing failure of emissions reduction to adequately address growing atmospheric carbon dioxide (CO_2), increasing attention has focused on the possibility of conducting CO_2 removal (CDR) from the atmosphere ('negative emissions'). Indeed, modelling suggests that hundreds to thousands of gigatonnes (Gt) of CO_2 will need to be removed over this century depending on global mean temperature goals and the success of emissions reduction. Natural CDR processes already remove in excess of 22 Gt y^{-1} of CO_2 from the atmosphere, and thus significantly moderate the climate effects of our current 41 Gt yr^{-1} of emissions[1]. Numerous approaches for increasing this removal by natural or artificial means have been proposed. However, none of these have been demonstrated and proven at the required scales, and their economic, social and environmental disruption are potentially prohibitive. For these reasons, a significant research and development (R&D) effort is urgently needed to fully understand the types, capacities, costs and impacts of such methods so as to best determine which to deploy, and at what scale.

A recent United States National Academy of Science, Engineering and Medicine (NASEM)[2] report addresses the potentials and status of six general methods of CDR, and recommends an R&D investment of tens of billions of US$ over the next 20 years. The report estimates that these six methods comprise a maximum global CDR potential of about 10 Gt yr^{-1} when removal costs are limited to $100 per tonne of CO_2 or less. This level of CDR likely comes with significant land-use tradeoffs — in particular, impacts to food and fibre production, and to environmental services — that could limit CDR to significantly below 10 Gt yr^{-1}. The proposed R&D will better determine this limit, but if it remains at this magnitude it brings into question whether CDR, as currently conceived, will be sufficient to affordably compensate for lack of adequate emissions reductions.

To ensure CDR adequacy and maximize cost-effectiveness, additions to the R&D portfolio need to be considered. For example, in the case of negative-emissions energy, the focus of the NASEM report, and of those preceding it (for example, refs. [3–5]), on biomass energy with carbon-capture and storage ignores the potential for other energy and capture modalities (for example, refs. [6–8]). Mineral carbonation is featured as a potentially high-capacity solution, but is unnecessarily limited by only considering the formation of solid carbonates rather than also including the more carbon-efficient production of bicarbonates[9]. Enhancing the uptake and storage of CO_2 by plants and soils plays a dominant role in NASEM's report and others[2–5], yet only considering land-ecosystems ignores the large potential for marine taxa to contribute[10]. Likewise, the CDR prospects of, potentially, far more efficient approaches using genetic engineering (of microorganisms), synthetic biology, artificial photosynthesis and other avenues (for example, refs. [11–14]) go unmentioned.

While, "The committee recognizes that oceanic options for CO_2 removal and sequestration...could sequester an enormous amount of CO_2 and that the United States needs a research strategy to address them"[2], no marine options are detailed in the report, nor in its recommended R&D budget. This conforms to the report committee's guiding Statement of Task that specifically excluded marine options, with the curious exception of shore-based ecosystems (Blue Carbon), whose CDR potential is shown to be very small[2,10]. Thus, rather than being a CDR agenda that seeks to maximize global opportunities, it is one that excludes 70% of the planet. During the committee's deliberations, written concerns about its narrow focus were submitted to the NASEM by United States Senators Whitehouse and Heitkamp[15], and by myself, but were apparently too late to affect the outcome.

Rather than placing great hope only on the possibility of reducing costs and/or expanding the capacity of currently known and favoured approaches, the emergence of new or hybrid CDR methods also needs to be anticipated. If the evolution of modern technologies is any guide, the trajectory of an immature technology like CDR can change overnight and most certainly over years. An effective CDR R&D strategy must, therefore, have the foresight and nimbleness to support productive technology 'disruption'.

So, in the race to find and develop high-capacity, cost-effective, socially acceptable CDR, the contest not only needs to test and evaluate current frontrunners, but must also encourage additional, worthy contenders. The allocation of R&D resources within and across approaches also needs to be continually evaluated and adjusted based on objective and transparent intercomparisons of R&D results that include economic, social and ethical dimensions. In this way the evolution of CDR can be optimized so as to ensure that it can adequately contribute to solving an urgent global problem. ❐

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