

In this research statement, I describe my existing research, perceived contributions, and future goals. I organize the presentation around the three objectives listed in my cover letter, and close by describing how the various threads connect and where I envision focusing my work in the coming years.

1. *Measuring and characterizing the private and social benefits of environmental goods and services*

**A major portion of my work focuses on careful measurement of the economic value of environmental goods and services.** The benefits generated by the environment are neither static across time and space, nor well reflected in readily observable prices for environmental goods, due to a general inexistence of competitive markets for them, and to their typically large unpriced external effects. I thus rely heavily on survey data – mostly primary data that I collect in my own research projects – that is context-appropriate, and analyze it using microeconomic methods. This research interest stems from a very basic, critical insight that came to me during my service as a water and sanitation Peace Corps Volunteer in West Africa. It was a former engineer's realization that rational, self-interested behavior matters a great deal, even in the context of serious external harms (such as environmental pollution), and that a key aspect of understanding behavior is to understand and measure individuals' preferences and incentives. This part of my research agenda is partly methodological, but is also highly applied. I aim to identify solutions to environmental management problems facing human societies (e.g., mitigating complex and dynamic environmental health risks, or improving access to environmental services).

In the jargon of environmental economics, this element of my research portfolio falls squarely in the domain of nonmarket valuation. The methodological toolkit that I apply consists of both revealed preference methods (when market behaviors or consumption of complementary goods can reasonably serve as proxies for the nonmarket goods of interest) and stated preference techniques (when they cannot). I choose from these tools on the basis of both theoretical criteria (e.g., completeness in the sense of total economic value) as well practical usefulness (e.g., paying attention to the tradeoffs that are most influential in affecting behaviors). Below I discuss the nature of my contributions to the measurement of demand for three specific categories of environmental improvements, and finally highlight several methodological contributions of my work.

Demand for cholera vaccination. The first major nonmarket valuation application in my research was for characterizing the benefits of reduced cholera risks. A cholera vaccine is a product with well-defined characteristics, but it is not generally available in the marketplace in most endemic settings due to government policies on immunization. In order to understand demand for protection against cholera, **I therefore used surveys and nonmarket valuation methods – specifically the contingent valuation and the travel cost methods (CVM and TCM) – to estimate the willingness to pay (WTP) for these vaccines** in Beira, Mozambique. My paper in the *Journal of Development Economics* is one of very few applications of the travel cost method to valuation in the domain of health, and, along with a companion CVM study, supports the idea that stated and revealed preference techniques can yield theoretically and empirically consistent estimates of demand. My research on the benefits of cholera vaccines also leverages data on the private benefits of vaccines to consider the value of social spillover benefits from herd protection.

Demand for safe and/or reliable water supply and sanitation. True to my bent for pragmatic and informative research, and building on my Peace Corps experience, I have long been interested in the problems associated with poor water and sanitation. These problems are concentrated in low-income contexts, and remain one of

the leading environmental risk factors in the global burden of disease,<sup>1</sup> despite recent progress in extending access to improved services (see for example item 76 in the publications list on my CV). **My work on the demand for water, sanitation and hygiene (WASH) services considers the puzzle of low demand in the face of high risks.** Many of the usual influences on demand that economists typically discuss – for example, higher income, education, household composition (e.g., more young children), and risk aversion – apply for WASH. I have also investigated other possibilities, however. One simple yet underexplored hypothesis is that **households are sensitive to taste or aesthetic disamenities** (e.g., distaste from chlorine) of WASH and other environmental health “improvements.”

Alternatively, one might posit that **households may not fully appreciate the benefits of water supply improvements, or conversely, the risks posed by their existing water sources.** To be sure, environmental health risks, exposures, and consequences are part of a complex chain linking cause and effect, in which behavior plays a key mediating role. Describing risks is thus important, and some of my joint work with environmental scientists focuses on risk characterization and its consequences in particular settings. Scientific understanding of risks, however, will generally be insufficient to solve environmental problems if potential beneficiaries do not also appreciate them. My work suggests that perceptions of water safety are strongly related to demand for improved water quality in Cambodia, and reliability in Jordan. Of course, the causal effect of perceptions on demand is difficult to isolate, since the two are related, dynamic, and linked to other unobserved household factors. I therefore implemented a field experiment to investigate the connection between the two, a study that revealed that households informed and most unaware of health risks did purchase and use chlorine tablets at higher rates than those who were uninformed or those informed yet more aware (This paper is forthcoming in the *Journal of Environmental Economics & Management*).

Preferences and demand for improved household cooking technologies. The themes discussed above also appear in my recent work on the demand for efficient household cooking technologies. Improved cookstoves (ICS) have been touted for their potential to deliver triple benefits to household health and productivity, reduced deforestation, and for global climate change mitigation. Like many other health-improving technologies, however, the scale-up of ICS has run into major implementation challenges, due to the interaction of low demand and inadequate supply chain and private sector development, as well as policy that creates incentives that may conflict with long-term impact. **Some of my recent work therefore focuses on the demand for these cleaner cooking technologies.** This work especially highlights the considerable **heterogeneity in preferences**, which is typically ignored in the design of environmental health interventions.

Methodological aspects. While most of my research in the domain of valuation is highly applied, a part of it is also methodological. In particular, I develop more reliable methods for asking and analyzing stated preference questions, about which many economists have misgivings for a variety of reasons discussed in the literature. For example, I have tested whether allowing respondents’ time to reflect on a valuation scenario affects estimates of demand; this work published in *Environmental and Resource Economics* shows that “time to think” decreases both the magnitude and uncertainty of stated WTP. Several of my CVM surveys have tested for order effects or sensitivity to the scope of the improvement being valued. Some of my work further contributes to the literature on revealed versus stated preference comparisons, or comparisons of different stated preference

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<sup>1</sup> Pruss-Ustun et al. 2016. “Preventing disease through healthy environments: A global assessment of the burden of disease from environmental risks.” WHO: Geneva.

methods.<sup>2</sup> Much of it is informed by the need to demonstrate consistency with theoretical predictions, by analyzing the determinants (or correlates) of demand, or by investigating whether complementarities exist across health-improving goods.

2. *Developing and evaluating interventions to improve environmental quality and resource management*

The measurement of demand described above is a critical cog in my larger research agenda that centers on understanding the extent to which: a) people actually understand (or perceive the importance of) the benefits of environmental improvements; b) private provision of these goods and services leads to adoption and use; and c) public sector or outside intervention is needed and/or justified on the grounds of external effects or other market failures that impede private provision. To address such issues, I rely heavily on theory-based evaluation (the focus here) and rigorous benefit-cost analysis (Section 3).

Testing interventions to stimulate adoption of environmental health improvements. **Having studied the demand for technologies and services related to environmental health, my research has gone on to develop and test interventions to enhance their adoption and use.** My research on the demand side has focused primarily on monetary, knowledge, and preference-related constraints. This demand focus is also completed by consideration of supply-side and contextual features that include the micro-institutional organization of service delivery, and the role of local socio-environmental environment.

Economic theory suggests that an obvious way to raise investment in preventive behavior is to lower its cost to individuals. **Several of my studies examine the effects of subsidies or of financing on behaviors.** In a rural setting in India, I showed in a quasi-experimental analysis that capital subsidies to communities for construction of advanced community water systems led to a 10 percentage point increase in purchases of highly treated drinking water from a baseline of nearly zero. In India and Cambodia, subsidies similarly have large effects on purchases and use of improved cookstoves. These findings are consistent with the idea that demand for environmental improvements is highly price elastic.

A second, common way of stimulating demand is to provide potential beneficiaries with information about environmental risks. The logic behind information interventions is that these risks are uncertain and difficult to observe, especially in low-income settings where multiple risks are present. Given the link between perceptions and demand, it would appear that targeted information about household-specific risks could make a difference. **In several field experiments, I find that households respond to information about water contamination, by seeking out safer sources, or by purchasing and using chemical treatment to disinfect water.** The latter study in particular develops a unique experimental method for disentangling heterogeneity in the effects across optimistic and pessimistic household types.

Perhaps because of methodological challenges, there is much less published research in the literature that considers supply-side interventions. **Given the potential importance of supply issues where there are thin markets for preventive goods, I apply creative methods in this work to clarify the nature of this problem.** I worked with a multi-investigator team, for example, to assess the effectiveness of different forms of post-

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<sup>2</sup> See discussion in Adamowicz et al. 1998. "Stated Preference Approaches for Measuring Passive Use Values: Choice Experiments and Contingent Valuation." *American Journal of Agricultural Economics* 80(1): 64-75, or Carson et al. 1996. "Contingent valuation and revealed preference methodologies: comparing the estimates for quasi-public goods." *Land Economics* 72(1): 82-99.

construction technical assistance (e.g., provision of spare parts, training of mechanics, regular visits to monitor infrastructure function) for rural water supply interventions using a matching approach. Using a similar approach, I have shown that a history of interaction with a promoting micro-institution, and the greater trust this instills in potential beneficiaries, makes a difference in adoption rates and final outcomes in a new type of intervention. In general, my work highlighting the need to use mixed methods and modeling to carefully incorporate features of the supply and demand context suggests that research to identify the most critical limiting factors to scale-up needs to go beyond those that are easiest to test in simple experiments.

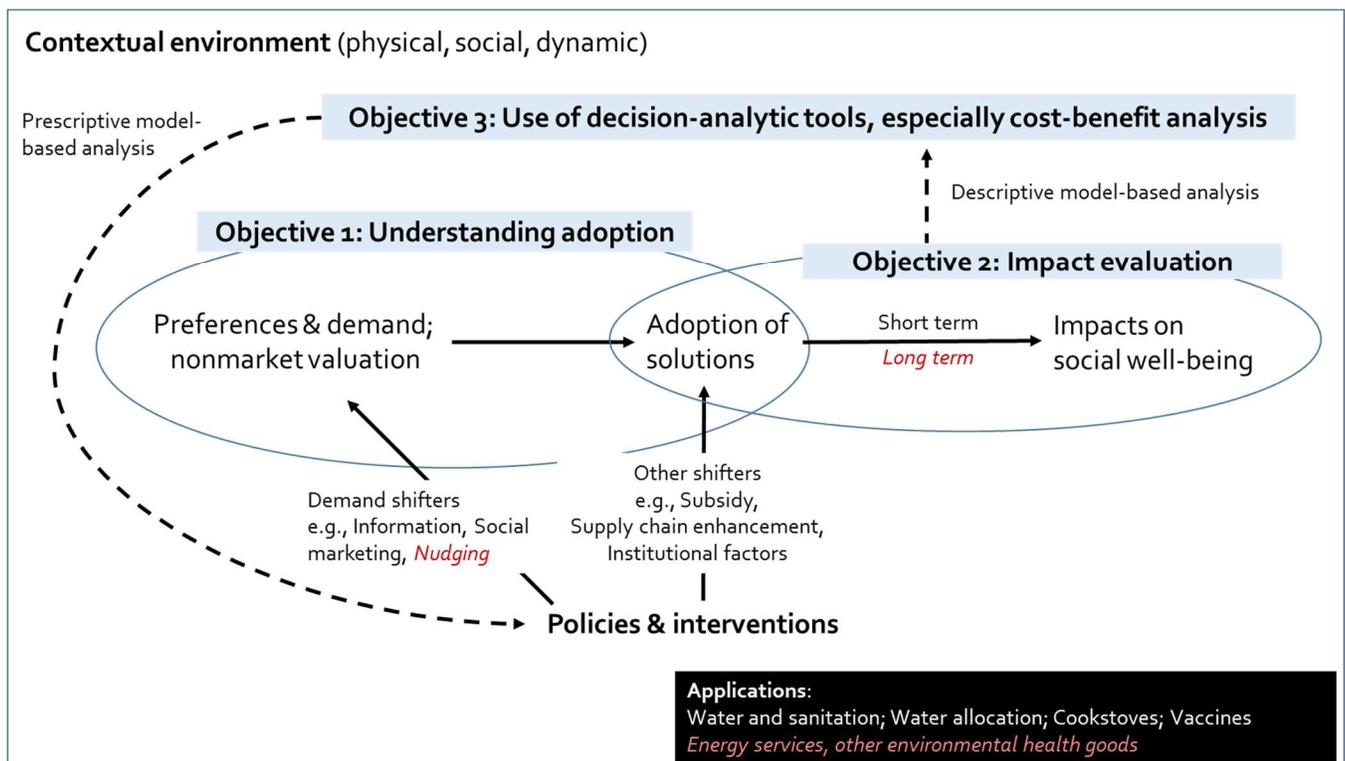
Health and other impacts of environmental health interventions. In addition to testing the effectiveness of interventions to stimulate household demand and use of environmental health improvements, **my work has also considered the impacts of such technologies on a range of outcomes.** I use **experimental, quasi-experimental, and econometric methods to get at such impacts,** which include **subjective perceptions of benefits, health endpoints** like diarrheal and respiratory illness, and **time and fuelwood savings.** In general, this body of work suggests that such interventions can be effective, but that behavioral adjustments sometimes reduce or negate impacts via predictable mechanisms. The dynamics of these adjustments – which include insufficient use to achieve health improvements, substitution away from other modes of self-protection, and so-called rebound effects – add to the complexity in the causal chain that links environmental health interventions to impacts.

### 3. *Applying economic concepts and cost-benefit analysis to obtain behavioral and normative insights relevant to environmental policy making*

The third broad domain of my research aims to capitalize on the findings from my research on demand and impacts of interventions, to **conduct economic analyses that are useful for informing policy.** Importantly, this objective also serves to close the loop in my research portfolio, by helping me to identify key gaps in understanding that inhibit sound policy-making (Figure 1). For example, uncertainty over the value of a specific parameter may contribute disproportionately to uncertainty about an intervention's costs or benefits. Some of these analyses tend towards the descriptive and are conducted with the primary purpose of elucidating how key complexities affect behaviors within socio-environmental systems. In particular, in this work I have often been interested in developing and applying methods to characterize the asymmetry and heterogeneity of private benefits arising from environmental interventions, and their implications. Other model-based analyses have a more normative bent that focus on the divergence between the private and social benefits of interventions, and on the social efficiency or equity implications of policies that aim to produce greater social benefits. Below I describe the main categories of such analyses in my work.

Analyses of the economics of transboundary water resource management. I have worked extensively on **modeling the economics of infrastructure investment and changes in water management in transboundary water systems, using a combination of simulation and optimization (hydro-economic) models.** These operate at a hydrological river basin scale, and help analysts and policy makers to understand changes – due to infrastructure, institutional re-organization, or external drivers such as climate change – in physical flows of water in surface water systems, as well as their economic consequences for riparians. My most significant contributions in this domain (starting from my PhD) have been twofold. First, **I have worked to develop new simulation methods that extend existing hydrological frameworks by more carefully considering the economics of water resources development.** Much of this work is motivated by the tremendous uncertainty in

future climate and development trajectories. Though existing frameworks do consider various hydrological uncertainties, they are not readily applicable to the assessment of adaptation or planning options, due to incomplete consideration of economic principles and the complexity induced by social to natural system feedbacks. The frameworks I developed allow a more general treatment of uncertainty as demonstrated in two publications in the leading water journal *Water Resources Research*. These papers highlight the fact that the economic risk of infrastructure investments from reduced water availability can in some cases be offset by economic adjustments in the shadow value of the water-related benefits those infrastructures generate, which increase with water scarcity. This work illustrates my ability to meaningfully combine methods and insights from economics, hydrology and water resources engineering, and has led to my involvement in important policy discussions.<sup>3</sup>



**Figure 1.** Schematic representation of my research interests and scholarly activities

**Notes:** Solid arrows denote the relationships that my work aims to elucidate; dotted arrows denote model-based analyses I conduct; and areas of future emphasis are denoted in red italics.

Second, working with diverse teams of economists and water resources engineers, **my research has generated new insights that I believe are important for influencing negotiations over, and relations related to, shared water resources.** Without careful consideration of the linkages and thresholds in the relationships between people, communities, and countries and the hydrological cycle, and without considering the political economy of riparian interactions, there is tremendous possibility for misunderstanding opportunities – and therefore

<sup>3</sup> For example, I was involved in the production of an expert report on the need for cooperative agreement concerning the Grand Ethiopian Renaissance Dam. The report is available here: [https://jwafs.mit.edu/research/publications/2014/ethiopian\\_dam\\_report](https://jwafs.mit.edu/research/publications/2014/ethiopian_dam_report).

ineffectively allocating resources – for enhanced water resource management. My work in the Nile Basin for example shows that politically strategic actions on large infrastructure, for example building a single large infrastructure rather than multiple smaller dams, or delaying investment, can have opportunity costs that reach the billions of US\$, which is generally on par with the investment costs of such projects. Each hydrological system is clearly unique: in the Nile upstream development can have large effects on downstream riparians and can thus raise the political stakes of decisions, whereas the interdependency among riparians in other systems that I have studied is more limited. Without use of a systems modeling framework that accommodates the dynamics of these socio-hydrological systems and the strategic interactions of riparians, it is easy to miss similarly crucial points.

Finally, while much of the aforementioned work on transboundary water management is concerned with the potential for improving the economic efficiency of water allocation across multiple uses and stakeholders, I have also worked on region-specific issues. Most of this work is in water-scarce regions, for example on the economics of allocative and technological options for water saving in Central Asia, on the potential for wastewater reuse in the Middle East and North Africa, or on the potential for water rights trading in the Diamond Valley of Nevada (work in progress).

Cost-benefit analysis of environmental health interventions. The final area to which I have devoted substantive effort **is in conducting cost-benefit analysis of environmental health interventions.** This body of work connects very clearly to the first and second major themes in my research. In particular, I have developed and used simulation methods to demonstrate that *ex ante* benefit-cost predictions about such interventions are highly variable across target (low-income) locations. As shown in several papers (published in *PLoS One* and *World Development*), much of the variability in the distributions of costs and benefits arises from real-world spatial and dynamic heterogeneity in benefits as well as costs. Perhaps most importantly, these findings suggest the need for caution in using simple deterministic metrics to rank or prioritize interventions across sectors. In addition, selection of effective intervention strategies typically require sensitivity to local context, and should allow for feedback that incorporates local knowledge that technocrats and decision-makers will usually lack.

#### 4. *Synthesis and future directions*

As described above, my contributions to thinking about problems of environmental health and water resource management in low income settings are generally consistent with the perspective of environmental economics, which has always been concerned with interaction of human and natural systems. **My research collaborations nonetheless remain heavily influenced by other perspectives, which I believe provide essential inputs to understanding.** My masters level training in environmental engineering and systems analysis has provided me with tools and frameworks (hydrological routing, operations research models) to think more carefully about the economics of water, while my environmental health collaborations rely on a rich network of interactions with medical and public health experts, natural scientists, other social scientists, as well as policy makers and implementing organizations. I have consistently found that these interdisciplinary collaborations – while requiring substantial investments of time and presenting unique challenges – enrich my work and allow me to produce better, more nuanced and informative policy analyses.

**In the future, I expect to continue to develop my research agenda within the aforementioned themes, applying the conceptual models, tools and methodologies discussed above to a wider array of empirical**

**environment, health, and development problems.** I will continue to study preferences and demand, and the implications these have for adoption of environmental improvements. I also envision a move towards combining survey data, econometric analyses, and systems models, to study **the dynamics of resource transitions – in the water domain this could be termed socio-hydrology – in terms of behaviors and outcomes** at the household and population level. Furthermore, I would like to work to provide valuable inputs of evidence into what is often a confused policy environment. These goals fit well with my work to co-convene (with Subhrendu Pattanayak at Duke) a global group of social science scholars comprising the Sustainable Energy Transitions Initiative (SETI).

Finally, I hope to make progress in helping local implementers to apply tools that help them embed their keen awareness of context within the dynamic complexity of the systems whose behavior they aim to influence. One of the main mechanisms through which decentralization is supposed to improve decision-making is via its ability to integrate local preferences, constraints and opportunities into solutions, given its proximity to local knowledge and the potential for increasing accountability. Yet dynamic complexity clearly challenges this narrative, if the connections between local knowledge and outcomes are too difficult to understand.