White Paper on "Managing Human-Environment Systems for Sustainability"

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The mathematical and computational sciences must play a leading role in facing the challenges in attaining a position of sustainable global development, "meet[ing] the needs of the present without compromising the ability of future generations to meet their own needs." This is part of a long-standing tradition in some areas of these disciplines, but those areas reflect only a small fraction of this broader intellectual landscape. In this white paper, we will focus on the role that computer science might play in facing these challenges. This presents both tremendous challenges and opportunities, since the scale and complexity of these computational problems are well beyond what can conceivably be attacked today, in spite of the enormous progress driven by the digitization of our internet-connected world.

The history of computer science is one of a gradually changing focus – for many years the sole domain of the discipline was the design of the computing artifact itself, that is, the computer, and the means to make the computer as potent a tool as possible. As a result, a great deal of the first half-century of research in algorithm design was focused on the mechanisms perceived to be inherent in computer design and implementation. During this period, the study of the theory of algorithms flourished, but only in part by the investigation of questions motivated by other disciplines, most notably by questions arising from Operations Research. However, there was not nearly as much connection between external applications and the development of the algorithmic mathematics needed to solve real-world problems, even in those applications for which their discrete optimization nature made them particular germane. However, in the past ten to twenty years, there has been a notable shift in both the focus of algorithm design research, and the extent to which this research has had a significant impact on other scientific disciplines. The most notable example has been for the human-genome project, and in computational genomics more generally, where computational tools and analyses provided the means, for example, to make "shotgun sequencing" a reality.

Throughout the past half century, applied computational work in settings dependent on discrete optimization models and algorithms was a mainstay of progress within the Operations Research, and more specifically, the mathematical programming community. At the core of much of this progress was the dramatic improvements in the speed of solving linear programs (i.e., optimizing a high-dimensional linear function subject to linear inequality constraints) – typical improvements are of a factor of 6 orders of magnitude (3 orders for each of machine speed and algorithmic innovation) so that "a model that might have taken a year to solve 10 years ago can now solve in less than 30 seconds" [1]. One further remarkable aspect of this progress is that the speedup is largely the result of superior engineering of the same basic algorithm (the so-called simplex method). While linear programming is a formidable computational tool with a wide spectrum of applications, its significance pales in comparison to that of integer programming (where one also constrains the variables to take on integer values). Here, progress on linear programming has been leveraged, but further algorithmic innovations have made even more dramatic improvements in solution capability. Taken as a whole, the evolution (or perhaps more accurately revolution) of this area can serve as a poster child of the value of basic research in expanding the applicability of a computational tool to domains previously viewed as insanely unrealistic within a 25-year timespan.

There are a number of distinct areas in which the tools and perspective of computer science should play a leading role in developing the science of sustainability, and these together might be viewed as an emerging area of computer science, called computational sustainability [3]. The first instinct is to focus on the fact that the IT industry (broadly interpreted) should be a good global citizen, and should work to minimize its impact in terms of the resources it consumes. While this is indeed significant, current perception pegs, for example, the total energy consumption of the industry at significantly under 10% of current worldwide demand. So in many respects, the far greater potential of computer science is in its use at limiting and managing the other 90%, as well as a host of other environmental resource management issues. (One caveat – the rapid development of China, India, and perhaps other parts of the former "third world" is likely to increase the percentage of energy consumption for IT.)

However, the greater impact of computer (and information) science is likely to come from domains in which the impact will be felt more broadly, and this role can be divided into roughly three categories: (1) the ability to collect and monitor data about the state of the planet, both for taking, metaphorically, the real-time pulse of the world, but also in order to gain insights into resource usage; (2) the ability to process and interpret this data with the potential of closing the feedback loop so as to provide circuit breakers on runaway usage, and warning signals for newly emerging catastrophes; (3) the ability to model, at both a "macro" and a "micro" scale, and then optimize the management of natural resources. All three areas are important, and rely on differing pieces of the field of computer science: the first, mostly hardware and networking; the second, primarily AI and machine learning; and for the third, a mixture of algorithm design and AI. It is the last of these areas in which my own expertise allows some more detailed discussion, and the rest of this white paper will focus on this aspect.

A common way to structure scientific goals is in terms of grand challenge problems. Once such a problem is articulated (and accepted), research does not of course proceed by immediately attacking the stated problem head-on, but rather seeks to initiate the investigation of (relatively) small pieces that might be closer to the realistic frontier. The role of the visibility of a grand challenge problem is to energize and coordinate several research communities, each of which might have been operating independently, to galvanize intellectual resources to focus on making progress towards this seemingly unachievable summit. The focal point inherent in such a question has the effect of increasing the level of activity in these modest steps in light of the ultimate goal.

One important outcome of our discussions might be the postulation of one such grand challenge problem that could serve to energize a number of interrelated, but currently only loosely coupled research activities. However, in lieu of such an "application domain" grand challenge problem (which also would be useful), an alternative is to propose a differ style of grand challenge problem. Perhaps the time is right to set as a goal to have a general-purpose, non-application-specific stochastic optimization computational tool within the next 25 years. Before this is dismissed as unachievable (which indeed it might be), it is useful to point back to the example of integer programming. If, 15 years ago, one had set a similar agenda for integer programming, one would most likely would have dismissed that as being useless tilting at windmills, whereas today the state of the art is that, for a broad cross-section of applications, there are general-purpose, off-the-shelf tools to solve large-scale problems. It is, perhaps, also instructive to understand the drivers that led to this progress. The most significant driving force was the needs of industry - for example, the oil industry required ever-larger linear programming instances to be solved, and the needs of logistics-intensive companies, coupled with the availability of the required data through the information revolution, drove the expansion of the application of integer programming methods to a scale believed impossible previously. This pushed the development of the tool, step-by-step, to reach the scale that it can handle today.

Why is stochastic optimization an appropriate focal point for progress in the "managing human-environment systems in sustainability"? Of course, if one simply recasts stochastic optimization as "optimization under uncertainty" its importance is readily apparent due the multiple reasons for introducing uncertainty as a means to model, for example, the lack of precision in capturing the current state of the world and the lack of knowledge in predicting the evolution of the world (as a function of the actions taken). One need only look at few prototypical examples of recent work in trying to address questions of resource management with respect to biodiversity, long-term forestry planning, fisheries management, and energy, to gain some appreciation of the value of tools in this domain.

- Sheldon et al. [10], motivated by land-management issues aimed at preserving the red-cockaded woodpecker, considers the integration of a diffusion model that captures the likely migration patterns of this species, within a stochastic optimization model that seeks to maximize the expected number of land parcels where the species is present at the end of the planning horizon subject to budget restrictions. This work makes a number of strong assumptions, including that the planning is not adaptive over time, that the focus is on expected behavior rather more sophisticated measures, and that this is planning focused on the effects based on a single species. All of these limitations are primarily a function of maintaining a tractable optimization model.

- Kim and Powell [5] consider a model for making advance energy commitments for wind farms in the presence of a storage device with conversion losses. This model is based on a number of assumptions about the forecasts for energy generated by wind farms, and more generally is clearly just a starting point for a broader analysis of integrated model that considers the full range of energy sources.

- Ermon, Conrad, Gomes, and Selman [2] consider a Markov Decision process model for policies governing the harvesting of the Northern Pacific Halibut. This considers a relatively stylized model and shows that the optimal policy is what is called from a classical inventory perspective an (S, s) where one consumes the resource until the current level is s, and then restocks it until (without any consumption) until the current level is S; this is a stark contrast from current fisheries management policy which specifies an upper bound of a given constant rate of fishing. Of course, the challenge would be to show that this class of policies is superior, and possibly provably near-optimal, when considering a much sophisticated stochastic dynamical systems model of the environment.

- In a rather different setting, Konoshima et al. [7] present a framework for analyzing efficient spatial allocation of forest management efforts—fuel treatment and harvest—under the risk of fire. This framework is again extremely stylized, and only a much more detailed model could be used a true day-to-day tool, though reaching that point will require substantial improvements in our ability to compute good solutions for the resulting stochastic dynamic optimization model.

- There are many examples of domains of work in which deterministic models have proved challenging enough, and yet would greatly benefit by the introduction of stochastic components in the model. For example, in the area of reserve selection, mathematical programming-based work dates at least to the work of Kirkpatrick [6], and has remained active throughout the intervening years, with examples including work of Williams and ReVelle [11], Hof, Bevers, Joyce, and Kent [4], Pressey, Possingham, and Margules [9], and quite recently, Kremen et al. [8]. The last is a particularly fine example, involving both sophisticated model and the ability to capture a large swath of specifies, and in particular tries to analyze the impacts on biodiversity of global warning. All of these models are at their core, deterministic optimization models, whereas sophisticated stochastic ones, if equally amenable to solution, would be preferable.

These problems, and indeed the very nature of stochastic optimization as a tool for managing human-environment systems for sustainability, have as a commonality a spatio-temporal framework. This is a characteristic that should be used to partition off a sufficiently demanding class of applications, that nonetheless could be targets within the scope of a dramatic sustained effort over next quarter century. Furthermore, the nature of the threat to our planet's survival means that as other research progress proceeds, massive infrastructural changes may be needed in short order. This calls out for actionable management models to assist the planning of these changes. Already the push to integrate biofuels into the mix of our energy sources has shown that the absence of such coordinated planning can have substantial implications. One can further think about issues such as wind-farm location as a function not just of its production, but as a network of supply points in achieving a system balance, and also as potential interference point in the migration patterns of birds. Or if electric-battery-based cars requires a new network of charging stations, again this new infrastructure should be planned in a way to use a few resources as possible while taking into account its own impact.

All of these examples are meant to be exactly that and no more – a few examples offered to prompt discussion on the types of stochastic optimization problems that will be needed to be solved, and will be needed to be solved at a scale of complexity that boggles the mind. But that is exactly the essence of a grand challenge problem.

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