Editorial Introduction

There is exciting news! The International Society for Genetic Algorithms (ISGA) and Genetic Programming, Inc. have joined efforts, recombined, and organized a new society: *The International Society for Genetic and Evolutionary Computation (ISGEC)*. This follows the merger of the International Conference on Genetic Algorithms and the Genetic Programming Conference into a single conference: *The Genetic and Evolutionary Computation Conference (GECCO)*.

This is exciting because the new society will have dues that include a subscription to *Evolutionary Computation* as well as the new journal *Genetic Programming and Evolvable Machines*. This will help to link *Evolutionary Computation* more directly to the community that founded it and to associate the journal with a large body of researchers and application developers that would appear to be its natural readership. The journal *Evolutionary Computation* was founded as a grassroots effort and with a spirit of cooperation that embraced all aspects of the field. The new society builds on this theme.

The current board of ISGA was expanded to create the new governing board for the International Society for Genetic and Evolutionary Computation. The new governing board includes the following individuals:

David Andre	Wolfgang Banzhaf	Ken De Jong
Kalyanmoy Deb	Terry Fogarty	David Goldberg
Eric Goodman	John Holland	Hitoshi Iba
John Koza	Una-May O'Reilly	Marc Schoenauer
Hans-Paul Schwefel	Lee Spector	Darrell Whitley

Additional information about ISGEC will be available at `www.isgec.org`.

With the first issue of the year 2000, it is a time to assess where we have come from as a community, where we are, and where we should be going. As recently as 15 years ago, the number of researchers working on evolutionary computation was quite small. Now there are thousands of researchers with an interest in evolutionary computation, as well as some very interesting real-world applications in such diverse fields as Aerospace Design, Drug Design, Scheduling, and Control. As applications have grown, so has the body of results that make up the theoretical foundations of the field. Not all of the theories, hypotheses, and assumptions that were dominant 15 years ago have stood the test of time, but that is a sign of the increasing maturity of the field and the progress that has been made. There are still many gaps in our theoretical understanding of evolutionary algorithms. Empirical work and applications continue to have a strong experimental flavor, often with little connection to existing theory. This is consistent with the fact that many applications of evolutionary algorithms continue to have a strong heuristic influence. Perhaps this isn't surprising; heuristic methods continue to be of critical importance in the fields of Combinatorial Optimization and Operations Research. And as in the field of Artificial Intelligence, "getting the representation right" in evolutionary algorithms is still critical to creating effective applications that work. Designing an effective representation is still at least as much art as science.

On the theoretical side, there is the usual struggle between working on our key theoretical problems that push the frontiers of what we know and doing more incremental theoretical work. Some researchers push the envelope of the current paradigmatic models, while others feel there is a critical need to break out from those paradigms. There have, perhaps, been too many papers that put too much effort into yet another theoretical analysis of ONEMAX, the counting-one's function. We need more results for the kinds of problems we actually encounter in real applications. There is also still a gap between low-level descriptive Markov models that are exact and high level predictive models that tend to make strong assumptions–such as decomposability. Stronger assumptions often allow for strong theoretical results, but do such assumptions really characterize real problems that we actually care about? Addressing this kind of conflict is another way in which good experimentation can be helpful. Experiments can help to determine which methods that have been developed using strong assumptions degrade gracefully when those assumptions are violated. Unfortunately, we do not see many experiments along these lines. Too often, experimentation focuses on whether some operator X is better than some operator Y or whether method A is better than method B. There is a real need for more experimentation aimed at testing hypotheses that can shed light on our theoretical models and the assumptions underlying those models.

Evolutionary Computation will continue to represent the best work being done today in the field of Evolutionary Computation. We also rely on many experts in the field to help select the papers for publication. I would like to take this opportunity to thank those individuals who have contributed their time and expertise to the journal.

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