DA Vision 2015: From Here to Eternity (invited paper)

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Abstract—**Design automation (DA) is at a historical moment where it has a chance -- after mathematics, statistics, and computer science -- to establish itself as the fourth universal approach with widespread applications in a variety of scientific, engineering, and economic domains. We start by outlining some of the most important research contributions and industrial applications of the design automation; we identify key conceptual DA techniques and describe how they interact to form synthesis and analysis flows. Next, we discuss the most attractive emerging and pending DA domains by analyzing several recent technologies, applications, and conceptual trends. Our emphasis is not just on the most challenging research or the most lucrative application areas but also on the technological trends relevant to DA. Furthermore, we elaborate on the types of new DA techniques and tools that are required for further fundamental progress and improved practical relevance. In order to provide a balanced picture of DA, we also identify the most pronounced dangers in false starts and false research in DA. Finally, we briefly discuss the need for a DA educational reform and community social reorganization that are beneficial for rapid and impactful research and development contributions.**

Keywords—design automation; machine learning; statistics; science; engineering

I. Introduction: Motivation and Objectives

We start this article by evaluating the status of DA from several points of view. We then outline our strategic objectives as well as the technical and logistic DA challenges and opportunities. Like any other vision study of a broad field, our estimates are at a high risk of being overly optimistic or too pessimistic. Hence, future may invalidate some or even all of our specific estimates or suggestions. Nevertheless, we think and hope that our approach of using the historical and emerging trends for guiding the future efforts has some intrinsic value and that (at least) a few of the proposed techniques or topics will aid in revolutionizing DA and elevating its impact.

A. *Design Automation: Research and development field Assessment - Major DA successes*

The field of design automation has well over 50 years of remarkable and high impact accomplishments. It created a significant industrial basis that has been acting as an essential enabler of the modern semiconductor industry. The semiconductor industrial segment is the heart of an extremely broad, profitable, and pervasive digital economy. Given the key role of DA in development of contemporary computing and communication technologies, it is not surprising that all top universities with engineering and computer science emphases have rigorous DA research programs with multiple visible faculty and researchers. A number of large and highly profitable companies such as Samsung and Intel also have active DA research and development groups. The graduating students with the DA research portfolio are being actively recruited by the most visible and most profitable companies in other fast growing and profitable industry segments, for e.g., Google, Facebook, and Apple.

The field of DA has an impressive record of high impact contributions. The most significant has been continuous enablement and support of CMOS scaling over the past few decades. Other noteworthy contributions of DA include introduction of the first application-specific techniques for power and energy optimizations, formation of models and efforts to address the impact of process variation and thermal management of integrated circuits, as well as a number of significant advances in the algorithmic dimension. These latter advancements have found widespread applications in several other fields. For instance, iterative improvement problem solving paradigm that is applicable to essentially any combinatorial optimization was introduced first in the DA domain [1]. Also simulated annealing, one of the first generic automation problems related to design of IBM computers [2], was suggested by the DA researchers. One of the most effective and widely used SAT solvers, Chaff, was presented at the Design Automation Conference [3]. Efficient manipulation of Boolean functions enabled by SAT solvers is an essential task not just in many synthesis and analysis steps, but also beyond the boundaries of design automation [4].

Recently, application protection and system security have emerged among the premier implementation and operational metrics. One of the most effective ways to ensure a secure system is using hardware-trust anchors; DA techniques and tools become handy in automating the flow and integration of such hardware-based trust anchors into secure systems and applications [20]. Another important impact area of DA is in intellectual property protection (IPP). Significant contributions in protection of functional artifacts by fingerprinting or watermarking were first demonstrated by DA researchers, e.g., for design specification, microprogram or programs [5].

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Consequently, the generic paradigm for IPP using constraint manipulation created by DA researchers was also demonstrated in other communities such as compilers. De facto manifestation and a major impetus for introducing security metrics into the design objectives and constraints was a DAC paper [6]. Presently, the most popular hardware security primitive is physical unclonable function (PUF)[7]. Although the first PUF papers where not published in DA venues, the authors are DA researchers. Numerous PUF improvements/new ideas have been introduced or shown by DA researchers. Thanks to these advancements, PUF is now an ultra-fast and ultra-low power full-fledged security and cryptography primitive that can be used to realize a wide spectrum of security protocols and designs.

This is only a very short list of major DA accomplishments. Still, the potential of DA is far from being well explored and realized. We believe that DA is in its singularity point where it has a chance to redefine its scope, greatly enrich its algorithmic and modeling arsenal, while enabling much more efficient and relevant development of tools required for synthesis and analysis of electronic devices and systems. It can also address many strategically and economically important problems well beyond its current scope. At the same time, it is important to stress that there are not only many opportunities but also numerous challenges. These challenges are not just scientific, engineering, or educational, but also related to DA community organization.

The technical opportunities and challenges can be traced along three directions. The first is considerations and retargeting to new technological advances. The second is related to emerging applications. Finally, DA shall enrich itself by including machine learning/statistical techniques and optimization algorithms and by exploring the best ways to organize DA flows for a specific application and technology.

B. DA definition

From its early beginning with programs for analysis of currents and voltages in very small circuits DA experienced a rapid growth. The developments were both quantitative and qualitative along the lines of specification and optimization on ever higher levels of abstraction. Initially, the field was split into two subareas: technology CAD and synthesis CAD that was consequently renamed to EDA to emphasize the importance of interaction between the designers and tools.

Synthesis DA branch comprises of system synthesis, behavioral synthesis, register transfer synthesis, logic synthesis, and physical design. Each of these DA areas has several subareas and a large number of steps. For instance, major steps in behavioral synthesis include partitioning, transformations, resource allocation, scheduling, assignment, clock cycle definition, and template matching. These steps are organized in various behavioral synthesis systems in different flows that include feedback to the designer. Similarly, major physical design steps include partitioning, placement, global routing, and detailed routing. While there is a combinatorially large number of ways to define each DA step and to order them, still there is a significant unified commonality of essentially all DA synthesis systems. The starting point is a compact initial specification that is consequently refined and mapped to lower levels of abstraction with increasing levels of detailed realization. After each step, the design is checked for functional and implementation correctness. The quality of DA synthesis system is quantitatively evaluated using benchmarks - sets of representative designs.

Although the objectives, constraints, specification formats, cardinality of specifications, complexity and accuracy of models greatly differ depending on the specific task, targeted application domain, and implementation technology, it is possible to extract common essential properties. The preliminary steps of DA include selection of an appropriate computation model, library of synthesis components, and models for calculating design metrics. The heart of the DA synthesis is a multi-step procedure. Each step is treated as an optimization problem. The specification is partitioned for large designs and each partition is individually solved.

C. Current DA drawbacks and limitations

A major constraint of DA research is that DA industry is relatively small. The problem is not just that DA has a slow growing industrial base with limited profitability but that even the VLSI IC industry is rather mature and in a slow growth mode. Some of the most advanced VLSI companies have been closing their DA operations. While several VLSI companies still have very respectable revenues, their valuations are significantly lower compared with other high-tech sectors such as Internet, wireless providers, and social computing. The major exception is Apple that was remarkably able to redefine itself as an embedded system company, mainly for wireless devices. Without a strong, profitable, and growing industry and their customer base it is impossible to have a sustainable and dynamic DA research.

Perhaps the biggest restricting factor for DA research is constrained funding which forces a shift of focus from longer-term visionary topics to short-term production of tools that can be directly transferred into industrial products. There are several notable exceptions that resulted in rapid growth of some specialized DA research. Probably the most visible in this category is the availability of research funds for security and trust of integrated circuits. Overall, the DA funding in US has significantly decreased from its historical levels while the number of academic researchers is still growing. Also, it is apparent that the DA funding situation is significantly better in other parts of the world, in particular Europe and Asia.

It is well known that DA researchers in general have relatively low citation records in comparison with researchers in other computer science and electrical engineering fields. With the exceptions of authors who also write papers in other research fields, or the ones who focus on books, survey papers or benchmarks, recent research studies show that while DA researchers have longer publication records than authors in other computer science and electrical engineering fields, the citations rates are among the most modest in comparison [8]. There are easy ways to correct the situations such as increasing the page limits at DA conferences and journals, but such immediate fixes would not address the essence of the problem. Informal analysis indicates that while 3 decades ago many researchers from other fields, such as computer science theory, artificial intelligence, and databases, where publishing in DA venues, more recently some of best DA researchers are pursuing research in other domains. Surprisingly, DA researchers not only pursue the most popular or emerging fields, but also well-established and mature research fields such as VLSI circuits, computer architecture, and real-time systems.

The last and perhaps most damaging reason for reduction in interest in DA is the lack of excitement in DA community across industry and academia. Several DA researchers and development engineers do not believe that they are working on exciting or ground-breaking problems; they either stay unhappy at their current positions, or left to other, more appealing fields such as search or social networking. The conference attendances also reflect this lack of excitement. With the exception of DAC which attracts attendance by diversifying its program to include the trendier subjects and exhibitions, the attendance at ICCAD has a negative slope. Some related conferences with focus on emerging topics, for example, on hardware/system security or Internet of Things, are popular. Overall, the small number of start-up, retention problems, and the decreasing number of students interested in DA are indicators of a mature research field in need for disruptive new ideas.

D. *Our Objectives*

Our strategic goal is to propose system of changes that aim not just to revive DA research but also to profoundly change its focus, scope, and internal organization. We understand that this is a highly ambitious objective, and there is a risk that at least some of our specific proposals may be counterproductive. Hence, we also try to identify major technical and logistical obstacles related to our suggestions. Finally, we emphasize that our goal is not to present ultimate solutions, but to start a constructive discussion.

II. Opportunities and Challenges driven by New Implementation Technologies

Synthesis and analysis of integrated circuits and systems requires modeling and optimization. In order to build effective optimization techniques, models characterizing the properties of the used components as well as means for interfacing those components are needed. At a very high level of abstraction, components and systems can be classified as analog and digital. Some systems may be mixed of digital and analog, a.k.a., mixed-signals. Depending on the purpose, components may belong to several types such as computation, storage, communication, sensing, actuators, and power distribution network. History indicates that some of greatest revolutions are at least partly consequence of appearance of new technologies such as semiconductor transistor, static and dynamic RAM cells, and CMOS transistor. It is well known that new materials or technologies have been essential for a significant portion of the overall progress and have greatly dictated DA developments. For example, historically Intel has been alternatively applying for each new generation of processors only architectural and design innovations or only technological innovation that consisted of ever smaller feature sizes and new materials.

A number of highly influential papers at DA best conferences and journals are dedicated to synthesis techniques that target new (emerging or pending) technologies. There is a close relationship between new technologies and new architectures and in particular microarchitectures. For example, emergence of FPGA, appearance of several types of nonvolatile storage elements such as flash memory, and rise of process variation or device aging each attracted a great deal of attention in the past and sparked new (micro-)architecture research and development. The current new technology frontier includes finFET, a great variety of nano technologies, optical nanophotonics, topological insulators [9], superconductors, and several families of smart materials for synthesis of next generations of integrated circuits and systems. Other popular topics at higher levels of abstractions or system levels are electrical vehicles, 3D printing, biological and neurological systems. Several research groups have envisioned and made steps toward design of intelligent mechanical and electrical systems. A special present focus area is on materials that manifest quantum mechanical properties. These materials introduce new long term scientific and engineering challenges while they have a fascinating potential to resolve many long-standing problems.

Furthermore, it is important to note that in certain cases new technologies by themselves create not just new design metrics, but also new application domains. For example, 3D printing has the potential to redefine classical industrial process of silicon printing and integration. In some situations properties of new technologies directly create new applications and new quantitative measures. For instance, process variation and device aging created several new security primitives, including novel and stable types of PUF.

Obviously, it is important and sometimes unavoidable that DA introduces and addresses new technologies. At the same time, it is crucial to realize that there is a relatively low chance for any new technology to find direct industrial applications. In fact, false starts are historically much more common that actual new economical technologies. Even more important is that proposals and papers acceptance shall be based on the opinions of actual technology experts. In many situations it is and will be difficult to find DA committee members and associate editors that are simultaneously experts in a particular new technology and are quality DA researchers.

III. New Application Domains

It is time for design automation to move on from the confines of traditional electronic systems design. The important problems that can benefit from EDA are no longer found on the surface of a silicon chip, they come from the fundamental long term problems for society as a whole.

Many of these problems can be addressed, at least in part, by engineering solutions. And, since engineering is centered on the design process these solutions can be enhanced by the use of the tools and techniques of design automation. Therefore, moving beyond the limiting constraints of chip design we can and should consider how to apply our methodology to engineer solutions to the pressing problems facing the globe. As examples, the 2014 Longitude Prize [10], the NSF/NAE top 14 grand challenges [11], and the X Prize 2011 [12] have each developed lists for the most pressing problems the world faces that could be addressed by engineering solutions. Not surprisingly, there is significant overlap in the topics for these challenges. We as EDA professionals should be working on these problems as the next applications for EDA technologies.

DA success historically has come from the confluence of two strengths: a deep understanding of the problem domain (electronics) and a mastering of computational techniques that can be applied to abstractions of the problems [13]. Our early successes in EDA came from the fact that we were the same people "using computers to build computers." We were researchers who intimately understood the domain of the problem (semiconductor materials, devices, circuits, optics, etc.) and could apply state of the art algorithms for modeling, simulation, and optimization tools to address the needs of electronic systems design. Since those early days (the 1960s and 1970s) the engineers using the tools and those creating the tools have diverged; but there is still tremendous overlap in "computer engineering." Most of our students know how to design an adder circuit from NAND and NOR gates as well as how to code a sorting routine in C.

This is no longer the case. And, in order to expand our horizons we need to be willing to learn new skills. While the computational techniques can be ported to other domains, the need for deep understanding of the problem domains takes time and dedication. Quick forays into "the next big thing" will not have lasting impact or success in solving the difficult problems facing the world. Similarly, trying to show the abilities EDA with "one-of" special issues in journals, special sessions at conferences, or topical workshops with no long-term commitment by the community both discourages EDA researchers from branching out into new fields and is a barrier to bringing domain experts into our community. We have seen that even after inviting domain experts to give keynotes, tutorials, and special sessions at our conferences, they do not submit papers in following years. Or worse, we reject their submissions as not being relevant to core EDA research or the following year's hot topics.

Rather, we need to cultivate our relationships to outside communities and develop deep long term relationships to understand their problems. We need to encourage and reward those EDA researchers who are willing to make those commitments and take that time to learn new domains and make real contributions. This is possible. In many fields the cost of design via prototyping and Edisonian techniques is prohibitive, and people have turned to what is called "model based design." Of course this is another name for CAD that is performing the steps of modeling, simulation, design exploration, synthesis, optimization, testing and debugging. Significant progress has been made in the fields of DA for bio, DA for drugs, DA for medicine.

However, this is just a start. A summary of some of the important global problems identified by the groups listed above which could be directly impacted by contributions from DA researchers includes:

- Ensure everyone can have access to safe and clean water
- Ensure everyone has nutritious sustainable food
- Prevent the rise of resistance to antibiotics
- Advance health informatics and engineer better medicines
- Restore movement to people with paralysis
- Provide energy from fusion and make solar energy affordable
- Develop wireless transmission of electricity
- Restore and improve urban infrastructure
- Fly without damaging the environment
- Develop carbon sequestration methods and manage the nitrogen cycle
- Reverse-engineer the brain and provide brain-computer interfaces
- Enhance virtual reality and personalized learning
- Engineer the tools for scientific discovery

The EDA community needs to make a serious, long-term, financial and intellectual commitment to solving one, some, or all of these problems. This involves multi-year funding by federal and industry sources, permanent placeholders in journals and conferences for research into these areas, and expanding the breadth and depth of expertise in our peer review processes. Enabling the design of better computing and communications systems has been the mainstay of our work; but we can, and should do more. This means broadening our perspective and being willing to make the commitment to follow through on these grand challenges.

IV. New Metrics

Closely related to new implementation technologies are new design metrics. Quantitative and even more qualitative changes in both technologies and applications have expressed ramification on what are most constraining or the most important objectives. For example, in early phases while integration was very costly minimization of the number of transistors (look-up tables, register transfer elements) or area usually were considered most important metrics. As the complexity of applications and the level of integration were increasing delay in one of its many definitions such as throughput, latency, and the worst or average case execution time gained importance. More recently several metrics related to power such as energy consumption and hot spots replaced both area and delay as the premier metrics. Currently a great variety of security metrics at different levels of abstraction that aim different security aspects have been pursued. While some of them are inspired by generalized notions of observability and controllability other have their starting points in statistic and information theory. Furthermore, a number of reliability metrics also have been proposed.

We expect that new generation of synthesis and analysis metrics will be developed. Some of them will target new design dimensions and some will focus on early or detailed characterization of existing dimensions. More radical future design metrics include intelligence per watt or per delay unit, adaptivity and flexibility are also pending.

V. Conceptual DA Advances

DA has its roots in tools that were supporting designers of integrated circuits by allowing them to focus on creative and conceptual problems and by automating computationally complex and tedious tasks. The first DA tools were mainly developed by designers themselves. This strategy of directly supporting practicing designers was very effective and resulted in DA tools that were able to support exponentially growing rate of VLSI integration dictated by the Moore's law. Interestingly, even after several decades of DA development some of the best DA research groups were the ones that were in close collaboration with design groups (UC Berkeley, IMEC, and Stanford). However, the strategy had also some potentially negative ramifications. The most important among them is that DA did not develop sufficiently as a scientific and engineering field on its own. Thus, there is a huge potential for many constructive action items along this direction. We now briefly discuss a small subset of options.

Uncertainty is intrinsic in both modern DA applications and implementations. Therefore, there is a strong and natural need for integration of statistics and machine learning into DA flows. This need is even higher for some of the new potential DA domains such as electrical vehicles and social interactions. Such a stochastic methodology would enable several new optimizations including statistical algorithm tuning and statistical validation. A drawback of the current algorithms is that often more tuning parameters are used than the number of designs in the considered benchmark. Since the benchmark is static and one of the major acceptance criteria is the improvement over the best previously published results, they often risk over tuning to the considered benchmark.

Several popular emerging applications have a human-in-the-loop architecture. For these applications, DA and other computer engineering fields require accurate behavioral modeling based on actual traces. Several wireless and Internet providers collect these data on a regular basis but the data is not openly available. A similar situation is with manufacturing data for ICs and systems. The data is considered business secret and there is a need to develop techniques that preserve the privacy of users and companies while enabling realistic and frequently updated behavioral and manufacturing profiles.

We also require systems that allow rapid customization. A new generation of iterative improvement optimization algorithms is essentially mandatory to address ever larger designs. Many of today's circuits have billions of components and only very low complexity heuristics are used for their optimization. Incremental synthesis used to be a popular DA research area but it requires new and fresh ideas that can be utilized on the industrial strength designs. Finally, we argue that for generalization of existing synthesis and optimization techniques, a statistical understanding of the types of objective function, constraints, instances, and algorithms is required.

VI. Educational Reforms

Attracting young talent, as well as their effective education and training, is essential for sustained development and growth of any scientific and engineering field. Until now, DA has fared quite well in attracting and nurturing talent. Most top universities have well established DA research programs that also translate to broad and foundational educational programs. These programs have so far attracted very good quality students. Moreover, conferences such DAC and ICCAD have nurtured our students through various scholarship and summer school programs, in addition to providing outstanding venues for presentation of their research contributions. Together, the ecosystem of academia, industry, research and development has contributed to the success of DA. However, a worrisome recent trend points to a waning of interest in core DA, particularly among the *top* electrical and computer engineering and computer science students. DA education needs to reform so as to continue to attract top talent, as well as to provide the broader foundation necessary to deliver upon the challenges offered by new technologies and application domains discussed in the previous sections.

DA is one of those domains that are both analytic and synthetic – i.e. it is one that "strives to understand" the nature of the problem to solve it better/more optimally, as well as one that "strives to create" what can be [14]. It epitomizes *design under constraints* such as cost, power, manufacturability, verifiability, etc. DA pedagogy needs to leverage this unique character, where modern mathematical and algorithmic fundamentals are synergized with hands-on programming, system realization and operational experience. Various DA communities (synthesis, physical design, test and verification) archive benchmark designs, optimization and verification instances; many of these are actually (modified versions of) real designs provided by industry. Many computing architectures are also open-source. This availability of real-world data should be used creatively to further attract top talent to DA classes, preferably in the undergraduate programs to excite them about DA early in their education.

Contemporary undergraduate computer engineering curriculum definitely teaches the use of CAD tools to do design, but does not cover fundamental DA concepts in any depth. Lack of dedicated undergraduate DA textbooks is a further hindrance. Almost all areas of electrical and computer engineering have dedicated introductory textbooks, but one along the lines of "*Fundamentals of Design Automation*" is sorely missing. The ones available, e.g., [15][16], are more appropriate as an inter-disciplinary nature of DA. There is a need to emphasize the *common underlying theme* among various DA techniques and applications – namely, functional abstraction, model reduction, optimization and decision procedures, and domain-specific algorithmic implementation. A first course in DA needs to educate the students about "*how to think design automation*."

At the senior and graduate level, it is important to teach and build upon the core skill-set of (continuous and discrete) mathematics and fundamental algorithmic techniques that form the various disciplines in DA. Many of the EDA-disciplines are still fundamental/ foundational, and will definitely be required to address DA in several emerging/post-CMOS technologies and applications beyond electronics (see Fig. 1, pp. 19, in [17]). For example, concepts such as simulated annealing [2], Binary Decision Diagrams (BDDs) [4], Satisfiability search and its efficient implementation [3], etc., were all motivated by applications in electronic design automation; however these are now employed in many non-EDA fields as well.

In addition to the already-established core-DA techniques, there is a need to introduce *more intensive use of statistics, machine learning, functional approximations and abstractions* in the educational curriculum. These are needed to address the forthcoming DA challenges. For example, several aspects of synthesis and analysis naturally require statistical reasoning: process variations, aging, metrics for reliability, yield, estimation, etc. Similarly, analysis and verification of cyber-physical systems requires expertise in functional approximation models. Likewise, education in various higher level models/abstractions and associated decision and optimization procedures is essential for addressing synthesis and verification at the system-level and also for hybrid systems. Training of graduate students in CAD for mixed-signal design – an area that already accounts for a large percentage of ICs produced today – needs to be more widespread. These somewhat weak links in contemporary DA-educational curriculum have to be strengthened, and the curriculum shall be diversified, without which one cannot expect to establish the universality of DA.

VII. Community Organization Changes

Historically social organization of any research community has been having profound and tremendous effect on the effectiveness and the long term research impact. While today there is no much danger of burning researchers that propose radically new ideas that contradict the established research dogmas, it is obvious that funding allocation, process criteria for papers acceptance, and procedures for selecting program committees and awards are among major enablers or disablers of research and development. These issues are notoriously rarely discussed in open forums. While creating new organizational mechanisms are far beyond the reach and ability of the authors, we believe that there are several simple localized policies that may help not just fairness, but also the overall progress of DA community. In this section, we briefly review the current DA structure, point out some mistakes, and propose a new and hopefully better DA social organization. Due to the lack of our industrial and economic expertize we do not discuss DA economic chains and their organization.

A. Status

There are two DA premier synthesis conferences (DAC and ICCAD), several other prestigious conferences (e.g. DATE, ASP-DAC, and ICCD). Similarly, there are two premier test conferences (ITC and VTS) and several more specialized testing conferences. There are also first class verification conferences (e.g. CAV), and a number of conferences on the boundary between design and design automation (e.g. CICC). Some of the DA subfields have also highly visible conferences. For example, the three quality conferences in the FPGA subfield (FPGA, FPL, and FCCM) are selective and well attended. Many other subfields such as physical design, logic synthesis, and hardware security have visible and well attended conferences and have significant impact factors. For example, ISLPED is the most reputed conference dedicated to both low power design and synthesis topics. Also note the conferences that form Embedded System Week that is a premier ACM SIGBED conference.

There are also conferences in closely related fields that have a high number of DA papers such as SIP, ICASSP and ISCAS. Our list is by no means complete. Therefore, one can easily come to the conclusion that there is certainly no shortage of DA publication venues. We believe that a great majority of papers at these conferences are overly focused. This approach is most likely motivated by noble factors such as transfer of academic research and clear indication that superior experimental results to previously reported have to be achieved. However, it also has a direct consequence that the research focus has been placed on incremental algorithmic and software development goals. We believe that in order to redefine DA and transform it from being a support and perspiration field into a creative and inspirational research area, in addition to focused projects there is a need for more generic and more global research projects. One example of such an effort is retiming [19]. It has been consequently combined with other techniques and applied at several levels of abstraction for optimization of a variety of design metrics such as throughput, latency, area, power, and testability.

The initial DA interest was mainly in the US and a few European and Japanese institutions such as Katholieke Universiteit Leuven, IMEC, Meio, and Osaka University. Today, almost all top European universities have excellent and high impact design automation including EPFL, ETH, Karlsruhe Institute of Technology, TUM, TWHT, and Braunschweig. Some other countries, such as Sweden, Netherlands, and Italy have several DA centers of excellence.

Much more spectacular growth has occurred in other parts of the world. While initially a majority of Asia-South Pacific DA contributions were coming from Japan and their US-based research labs, more recently other DA centers of excellence have emerged in particular in Taiwan, Hong-Kong, Korea, and Australia. The Chinese universities have also recently been demonstrating an exponential growth not just in terms of quantitative presence but also in terms of the impact that to some extent can be quantified using citation indexes.

Probably the most important enabler of any research is funding. A couple of decades ago DA has several solid sources of funding including DARPA, NSF, SRC, and MICRO program in California that was a great facilitator of amplified industry-based sources. Today, the situation is drastically different. The funding from DARPA and other DoD agencies is significantly smaller, mainly reduced to focused large projects in a few areas. Interestingly, NSF also follows this trend. Programs such as MICRO have been either eliminated or reduced. Industrial funding is still available but favors focused, short-term efforts. The most disturbing observation is that while there is a low correlation between funding and publishing/citation records, there are indicators of high correlation on several non-scientific parameters.

In summary, there are already too many DA conferences and journals. Some of them are highly selective and prestigious. For example, DA authors and conferences are often identified as the most significant -- significance is calculated under the reasonably mild and intuitive assumption that the best authors mainly publish at the best conferences and that the best conferences are the ones where the best authors publish [19]. That paper identified top DA authors as the most significant in a broadly defined hardware and architecture field and DAC and ICCAD as the best conferences in the same domain.

Analysis of program committees and editorial boards indicates a surprisingly high impact of potential conflict of interest. Several DA conferences and journals have authors with a rather low tangible record of accomplishments that are often authors with very large number of papers. Currently there is no mechanism that enforces complete removal of potential conflict of interests. The situation is in particular unpleasant and counterproductive with the rapidly growing number of new journals and conferences that are sometimes dominated by a very small number of authors, typically the organizers. We need comprehensive statistical and causality analysis as well as comparison with other scientific/ engineering fields in terms of their organizations and policies.

B. *Internal Organization: DA agenda*

DA must pursue new topics. More attention shall be paid to new conceptual breakthroughs that advance the state-of-the art of DA. At the same time, it is crucial that new technologies and new applications are not treated in naive, superficial, or overly simplified ways that fit DA research templates. For any topic that requires an operational technology, application knowledge and full familiarity with a domain, it is essential that program committees of top DA conferences have established experts that actively publish in top venues of the pertinent technology and/or application domain. Another even better option is that DA papers are published in respected conferences and journals of the targeted application or technology. Otherwise, top DA conferences and journals will be considered by researchers in these fields as second rate or worse which will have detrimental effects on DA. The only exception to this policy shall be presentation of tutorial papers that aim to introduce the new DA topics where at least a part of authors are established researchers in the native fields.

VIII. CONCLUSIONS

Design automation is a generic, powerful and practical approach for addressing a great variety of strategically and economically important problems. It has demonstrated a great success as the enabling technology for VLSI ICs and systems. Decades of fascinating DA developments through the increase of the levels of abstraction have been targeting ever newer technological generations with qualitatively more complex timing, power, and yield models. There is a great potential for exciting research directions driven by the new technologies and applications. There is also a strong need to systematically include machine learning and statistical techniques in tools to enable better synthesis and analysis approaches when systems are subject to uncertainty due to process variation, device aging, operating conditions (e.g. temperature) and workload. Furthermore, we expect that several new optimization techniques such as convex programming and stochastic linear programming will find applications in design automation.

At the conceptual level, we need new emphasis not just on addressing important specific problems but also on generic principles and techniques that indicate the most effective/accurate choice of optimization, models, synthesis, and analysis flows. Finally, it is crucially important to realize not just the potential but also the limitations of design automation including the bounded knowledge of DA researchers and developers. Inclusion of a deep understanding of the new technologies along with proper abstractions could avoid the situations where utterly complex problems are addressed in naive and oversimplified formulations.

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