ANNEX TO REPORT ON MATHEMATICS FOR EUROPE

June 2016

The online consultation on mathematics was carried out from 29 January to 15 May 2016 by the European Commission Directorate General for Communications Networks, Content & Technology (DG CONNECT) in the context of a stakeholder consultation to prepare the Horizon 2020 Work Programme 2018-2020.

This Annex includes the majority of the contributions received. These appear in their original form, as submitted by the participants. All contributions are available.

1. MATHEMATICS FOR THE COMMON GOOD

Mathematics (Luis Vega)

From the point of view of BCAM – Basque Center for Applied Mathematics, the future H2020 work programmes (2018, 2019 and 2020) should address the following two key aspects.

On one hand, BCAM encourages the adoption of mathematical Modelling, Simulation and Optimization (MSO) as a transversal and rich research area. In particular, we would like to support the statements of F. Planchon, Maria J. Esteban and Volker Mehrmann, among others.

On the other hand, we would like to make a special emphasis on the growing demand to develop mathematical tools for the next industrial (Industry 4.0) and social revolution, which requires not only research on Computational Mathematics but also on Data analysis. The growing amount of data in different fields such as Biosciences, Energy or Advanced Manufacturing is leading to a new scenario in which Mathematics will play a key role to face this “Data Revolution”.

Therefore, in line with the European Cloud Initiative, it is crucial to involve disciplines such as High Performance Computing (HPC) and innovative technologies for the management of Big Data. The grand challenge of Algorithmic Mathematics is to design the most appropriate model of computing in order to solve the current computational problems and data infrastructure limitations. A new theory needs to be built for the development and assessment of statistical methods and learning algorithms tailored to Big Data requirements.

The interaction and connection between the mathematical sciences and other fields should be part of the agenda in H2020 for the forthcoming programmes in order to contribute to the societal challenges in the indicated sectors (Advanced Manufacturing, Biosciences, Energy) and the Society in general. In fact, there are large examples of how Applied Mathematics (e.g.: Computational Fluid Dynamics, Image Analysis, Molecular Dynamics, Cloud Computing, Financial Mathematics) can contribute to the better understanding of real world problems. And that is why from BCAM we strongly support the idea that the EU could provide funding to mathematical research and its applications.

Mathematics in Interaction (Fabrice Planchon)

Mathematics have long had natural interactions with many other sciences, as well as industry and social issues. In recent years, important mathematical developments have occurred at the following interfaces and are likely to intensify:

a) Interactions of mathematics with life sciences, biology, health and medicine have progressed and offer a broad potential for innovation. This involves modelling complex biological phenomena, by the use of more-and-more sophisticated techniques (PDEs, stochastics, probability, statistics). This also includes the development of a large range of mathematical techniques dedicated to the study of physiological signals and medical images.

b) Interactions with computer science in general, and HPC and big data in particular, is already well identified and require combined efforts. Statistical learning and deep neural networks have raised important challenges. Important developments in the possibility of dealing with high dimensional problems have emerged, for example by identifying and exploiting sparsity concepts.

c) More traditional interactions with physics at all scales as well as engineering sciences is a fruitful source of potential innovation. We can mention analytical relativity and detection algorithms based on Wilson wavelets as two outstanding examples that contributed to the recent discovery of gravitational waves. In geophysics, the modelling of the “whole-earth system” is a major issue.

d) Interaction with chemistry is still underestimated today. The potential is huge. For example in quantum chemistry and physical chemistry, where powerful numerical approaches are needed to accompany theory and applications.

e) Interaction with social sciences, in the broadest sense, is an important field for investment by mathematics. Either by participating more actively in the development of complex systems, or by seizing questions that have a strong demand for modelling or data analysis.

In most , if not all previous items, one may recognize the trilogy Mathematical Modelling, Simulation, Optimisation that has been highlighted in other posts in this open consultation. More generally, interactions between fundamental mathematics, applied mathematics, modelling and its applications are happening at a fast growing rate and prove to be mutually beneficial. Tackling any of the aforementioned challenges is likely to require combined efforts of mathematicians of several different fields collaborating with scientists from relevant domains.

General and Specific Trends (Fabrice Planchon)

While mathematics is recognized today as essential for addressing major scientific, technological and societal challenges, it is worth recalling that its role in facing such challenges cannot be confined to providing the non-mathematician user with efficient tools, supported by a solid theoretical framework. In fact, new research developments of mathematical nature are explicitly required, at an increasing rate. Such a clear trend may be observed in three general area:

- Interactions of mathematics with other disciplines;

- Numerical simulation and scientific computing;

- Big data modelling and processing.

This justifies specific funding programs that could support mathematical research with potential for addressing the aforementioned challenges, bearing in mind that theoretical research without an immediate applicability is potentially of higher impact as application driven research. Here are examples of more specific mathematical developments that play or might play an increasing role in the above areas:

- The interface between logic, computer science, algebraic topology;

- Statistics, learning, optimization and approximation in high or infinite dimension;

- PDEs and control theory, combined with stochastics, big data, geometry, topology;
- Randomized algorithms in relation with optimization and functional analysis ;
- Numerical analysis and algebra for high performance and parallel computing;

Such a list is by no means exhaustive and could be extended by dozens of other examples. Its main purpose is to illustrate the large range of mathematical topics that may be involved, and to exemplify that, in today's mathematics, drawing a boundary between theory and application would be artificial and counterproductive.

Topics for the future (Volker Bach)

Being the president of the german mathematical society, DMV, I would like to draw the attention to the "Rapport de Prospective" of the french "Comité national de la Recherche scientifique" available under http://www.cnrs.fr/comitenational/doc/conjoncture.htm. This report gives a well-balanced and up-to-date (2014) description of the current research trends in mathematics and their importance for other sciences, technology or society at large, as well as, the impact development outside of mathematics has on modern mathematical research.

2. MATHEMATICS FOR HPC

Novel Mathematical Techniques for Exascale (Peter Dzwig)

Novel Mathematical Techniques for Exascale: In order to be able to address the types of engineering problems - and others - that are envisaged as being addressed by exascale computing we will require algorithms for the solution of very large scale matrix problems (>>10**6) that will scale over many orders of magnitude for use in areas such as FEA of order 10**12 and beyond for problems such as FEA, engineering simulations, general matrix problems. These should be implementable for exascale in standard libraries similar in style to the existing LAPACK/LINPACK.

↳ A small addition to: The Importance of Mathematics for HPC (Jakub Sistek)

I fully agree with this post regarding the collaboration of computer science and mathematics for meaningful HPC, and the importance of theoretical disciplines for progress. I would just like to add another small observation at this point. As the dominant challenge in HPC today, reaching exascale performance is often mentioned. It is certainly the driving force for a lot of exciting research. However, there is a huge, and in my view still growing, gap between the specialized machines and programming tools aiming to reach this goal, and what many simulations can efficiently use. Sometimes it is just about the efforts needed to introduce existing state-of-the-art numerical methods into these simulations, but more often these are limitations to scalability inherent to the numerical methods. Let parallel fast Fourier transform serve as one of the examples here.

In my view, research in numerical methods suitable for HPC, and not only at the largest contemporary computers, still presents a big challenge in its own right. While it may sound rather traditional, it is still a critical part of the chain allowing simulations making profit of HPC. The landscape of parallel computers is dynamic and quickly changing, and mathematics needs to keep up with this. Also, different methods turns out to be optimal for different scales of HPC, and all these deserve our continuous attention, research efforts and support.

Follow-up on "The importance of mathematics for HPC" (Anders Logg)

Olivier Pironneau makes a very good point: theoretical development of new mathematical methods and algorithms are essential to HPC. This is sometimes forgotten. HPC is about performance (and accuracy) in all links of the chain, from mathematics to algorithms to implementation and deployment on HPC platforms. HPC is not about implementation of (simple) off-the-shelf numerical algorithms on big computers. The biggest challenge lies in sophisticated implementation of sophisticated mathematical methods on sophisticated hardware architectures.

Implementations and contemporary architectures (Mirek Tuma)

I would like to emphasize even more the role of implementations for HPC, but not only for them. From the practical point of view, it is not so important what we call a method, what we denote as an algorithm and what we actually consider as an implementation. All of these form an important part of computational mathematics. Since general understanding that the Dennard scaling is over, there is no way to proceed than to get completely new mathematics of implementations for contemporary multicore processors, computers and high-performance systems. Sheer computational power and system environment provided by new machines, not only by HPC, is not enough. Having new architectures without appropriate mathematics is like having a fish without water.

Concerning: The Importance of Mathematics for HPC (Adrian Muntean)

I can only subscribe completely to these statements. A lot has been done, but most difficult issues are still open and it is certainly plenty of place for collaborations between mathematics and computer sciences.

ETP4HPC-Mathematics and algorithms for extreme-scale HPC systems (Marcin Ostasz)

ETP4HPC, the Europan HPC Technology Platform (www.etp4hpc.eu), issues its updates Strategic Research Agenda (SRA) in Oct 2015 and it is available at www.etp4hpc.eu/sra. The objective of the SRA is to outline a roadmap for the achievement of Exa-scale capabilities by the European HPC vendors. It contains a chapter on 'Mathematics and algorithms for extreme scale HPC systems' (5.7 details the objectives of this area 6.7 lists all the milestones that needs to be accomplished). This chapter delineates the position of ETP4HPC in terms of the contents of the HPC technology development related research programmes in Europe - in our view, the milestones listed need to be delivered in order for Europe to be able to produce Exa-scale level supercomputing technology

We have opened a Public Call for Comments on this SRA - available at www.etp4hpc.eu/sra. All stakeholders interested in HPC technology development are requested to use this link to submit comments.

For more information, please contact the Office of ETP4HPC www.etp4hpc.eu. Thank you.

Mathematics for extreme scale problems (Zdenek Strakos)

Many interesting contributions has already been posted. This one resonates well in particular with those on Modeling, Simulation and Optimization (MSO), which emphasize the role of mathematics as a discipline indispensable in development of the key tools for future research, technology as well as for investigation of social phenomena. An efficient use of extreme scale HPC systems certainly requires involvement of mathematics, as thoroughly analyzed ,e.g., in Chapter 5.7 of the Strategic Research Agenda 2015 Update of the European Technology Platform for High-Performance Computing ETP4HPC. Mathematics of MSO goes, however, much beyond that. HPC technology is a tool that is incorporated in MSO in order to serve to mathematics of large scale problems. They can be solved efficiently and to an acceptable accuracy if and only if the whole effort in the problem formulation, its mathematical expression, the consequent discrete representation, solving the discrete system, and the correct interpretation of the computed results, including a trustworthy error evaluation and analysis, are in balance.

The solution pipeline has to be tailored for the given large scale problem (or a set of problems); see, e.g. the Exascale Mathematics Working Group report on Applied Mathematics Research from March 2014 (http://micde.umich.edu/2014/03/14/new-doe-report-applied-mathematics-res...), or the article on the subject by Ulrich Rüde in SIAM News, June 2015.

The main point is that mathematics must not target only the efficient use of the HPC systems; this alone can not be sufficient. It must target primarily the large scale problems themselves. With a large amount of data involved, understanding of the problem is the key concept that must go hand in hand with computing. As elsewhere, the history repeats. Using the words (published in 1961) of Cornelius Lanczos, a prominent mathematician and theoretical physicist and one of the pioneers in using computers for large scale computations (see, e.g. The NIST Special Publication 730 called Mathematicians Learning to Use Computers about INA-UCLA 1947-1954):

"To get an explicit solution of a given boundary value problem is in this age of large electronic computers no longer a basic question. The problem can be coded for the machine and the numerical answer obtained. But of what value is the numerical answer if the scientist does not understand the peculiar analytical properties and idiosyncrasies of the given operator?"

The essay Why Mathematics (from 1996) of the same author convincingly argues how slippery it can be when one makes a distinction between ``old" and ``new" mathematics. Lanczos had certainly a right of saying so - his methods (and codes based on his methods and further developments) are used in many large scale calculations today much more than at his times!

We therefore argue that Mathematics should be supported in Horizon 2020 much beyond the current level and the support cannot be restricted to several newly identified challenges. As an example, mathematical modeling and computation using partial differential or differential algebraic equations may seem to some as an old area. But the associated real world problems are far from being resolved, with the fundamental and very difficult tasks in all parts of the solution process still ahead. The progress in addressing questions like the concept of the solution adequate to the original real world problem, error evaluation (verification and validation), and of stopping the adaptive computation can have much larger overall impact then optimizing the use of the current (or future) HPC systems. In the efficient use of the HPC systems and in the progress of the speed of computations it is mathematics, not the progress in technology, that takes the lead.

Mathematics can be seen as a substance full of inner life like a soil in which other sciences, technology and applications are deeply rooted as magnificent trees or big bushes.

They use Mathematics in building up their bodies. If we care for a good fruit on the trees, we must care also and primarily for the soil. If we stop caring for the soil, then the consequence will not be immediately visible. The fruits of the future will, however, be poor no matter how big effort we will invest into the individual trees (in solving future challenges which we will not be ready for). The support of mathematics can therefore not be restricted only to the topics that may now seem new, with letting alone those that at this moment seem old. Consider, as one of the many examples, problems in computational fluid dynamics. They may seem old to some, still the progress in that area will be without any exaggeration of fundamental importance elsewhere in many decades to come.

I am very thankful for this Consultation. I hope that it can lead to an adequate support of the key areas of mathematical research, like Modeling, Simulation and Optimization that includes also a rigorous mathematics of inexact, in the years to come. As described in the related contributions of this Consultation, there is no other alternative.

↳ Endorsement (Vadim Markel)

I would like to express my support for this post. Indeed, trying to prioritize some sub-areas of mathematics because they are very new or fashionable at the expense of the traditional research disciplines will almost certainly result in a disappointment. The ways in which science will turn out useful in the future are fundamentally unpredictable. I think that the priorities could be established at the level of very specific practical problems in which some evaluation of the outcome is possible. For example, one can say that reducing traffic jams is a priority. How the scientists solve the problem (definitely, there will be some role for mathematics in the solution) is not that important; what is important is to get rid of the traffic jams. But not ALL public funding should go to such prioritized applications.

↳ *Fundamental research and application (Josef Malek)*

I completely share the views on the role of Mathematics stated in this post. I also support the idea that it is necessary to support fundamental research in balance with applications in other sciences and technology. The applications and focus on challenges from sciences and technology can trigger new fundamental research in mathematics. Our experience underlines a permanent need to combine fundamental research with the work on a particular application problem. One must always keep in mind that a path to future fundamental contributions can be unpredictable at its start. Current knowledge without its continuous development is not sufficient for success today, not to mention the future.

↳ *Future is to be thought of carefully (Mirko Rokyta)*

I truly support what has been said in this post. The future is unpredictable from the point of view "which mathematics will be needed in, say, 50 years" and what seems to be "too theoretic" today, may play fundamental role in solving the important challenges of the future. As it was already nicely said by Z. Strakos above, we should take care not only about trees bringing fruits of today, but also about the soil from which the trees grow. Without the soil of today there will be no trees of tomorrow.

Mathematics and Algorithmics for Data Intensive Computing (Jean Roman)

For scientific and engineering computing, exascale is the next step in the long road of exponential performance increase that has continued since more than 50 years. High performance computing advances have been largely dependent on research works on numerical algorithms, software development, architecture design that allowed higher performance for computational models and applications. Today and in the future, advances will strongly depend of new research on data analysis, machine learning tools and on data centric architectures.

Very important research topics (both from applied mathematics and computer science) are identified in order to achieve scientific breakthroughs using exascale systems for challenging scientific and societal problems using extreme amount of data :

- mathematical modelling including multiple criteria optimization, control and uncertainty quantification issues for multi-scale and multi-physics complex problems

- scalable model discretization combining continuous and stochastic approaches leading to new algorithmic approaches including scalable code coupling, parallelization-in-time, model reduction for high-dimensional problems, adaptativity and scalable mesh generation

- highly scalable solvers and numerical libraries

- mathematically well-designed methods and algorithms for data analysis, detection of structures and correlation in big data, statistical approaches

- high performance algorithms for machine learning

- hierarchical algorithms exploiting all the levels of parallelism of the platforms (from very fine to coarse grain of parallelism)

- resilience of algorithms including reproducibility, verification and validation issues

- data intensive computing platforms (big data management coupled with data analysis pipelines both included in the numerical simulation process)

- architecture-aware algorithms and software for exascale computing including programming methodologies, environments and tools, runtime systems allowing the dynamic execution of programs with a very large number of threads (DAG scheduling issue with low communication and synchronization overhead), minimization of data movements for energy consumption issues.

Several of these topics have been identified in the report from the « Workshop for Mathematics and Digital Sciences published in March 2015 (https://ec.europa.eu/digital-single-market/news/workshop-mathematics-and...). This report points out the importance of mathematical support on one hand to HPC development towards exascale, and on the other hand to mathematical solutions for pattern recognition and simulation on big data.

Most of these research topics at studied at Inria which is a public science and technology institution supervised by the French ministries for research and industry with the missions to produce outstanding research in the computing and mathematical fields of digital sciences and to ensure their impacts on the economy and society.

Scalable Mathematics for Data & Compute Intensive Sceince (Vassil Alexandrov)

Novel mathematical methods and scalabale algorithms are key in producing robust applications that can help leverage upcoming exascale computing architectures. The justification, ideas and way forward are well described in the ETP4HPC Strategic Research Agenda -2015 update (http://www.etp4hpc.eu/). On the other hand we observe the Big Data explosion, and key aspects and the importance of mathematics are outlined in Frontiers in Massive Data Analysis, US National Research Council of the National Academies, Report 2013 . What we also observe is the increasing need to tackle and solve both Data Intensive and Compute Intensive problems. What I would like to stress also that recent experiments, for example, on the exascaling workshops in LRZ (Germany) have shown that employing novel mathematics and algorithms can lead to a substantial improvement in the performance of important applications thus clearly supporting the needs for novel mathematics for Exascale, Data Science and Digital Science overall.

Route to exascale: HPC systems continue to scale up in compute node and processor core count. These extreme-scale systems require novel mathematical methods to be developed that lead to scalable scientific algorithms to hide network and memory latency, have very high computation/communication overlap, have minimal communication, have fewer synchronization points. Scalable mathematical methods and corresponding scientific algorithms for multi-petaflop and exa-flop systems also need to be fault tolerant and fault resilient, since the probability of faults increases with scale. Resilience at the system software and at the algorithmic level is needed as a crosscutting and co-design effort. Finally, with the advent of heterogeneous compute nodes that employ standard processors as well as GPGPUs, mathematical methods developed and corresponding scientific algorithms need to match these architectures to extract the most performance. This includes different system-specific levels of parallelism as well as co-scheduling of computation. Key science as well as industrial applications require novel mathematics and mathematical models and system software that address the scalability and resilience challenges of current- and future-generation extreme-scale HPC systems. In more detail important research topics include:

- Develop novel hierarchical multilevel approaches for mathematical multiscale models for modeling variety of physical phenomena that employ that leads to design of hybrid methods enabling calculations with different precision at the different levels of hierarchy and leading to corresponding algorithms with high scalability matching the computer architectures hierarchies.

- hybrid (stochastic/deterministic and deterministic/deterministic) methods for variety of problems including Linear Algebra, Optimization etc. For example, these are Monte Carlo and quasi-Monte Carlo hybrid (stochastic/deterministic) methods as well as deterministic/deterministic ones. These are inherently parallel and are expected to achieve high scalability.

- Developing communication avoiding, falt-tolerant, resilient algorithms needed at exascale , the above mentioned approaches will greatly contribute to this.

Data Science: With the advent of Data Science in the past few years the need of such scalable mathematical methods and algorithms able to handle data and compute intensive applications at scale becomes even more important.

The need is for:

- Scalable methods and algorithms that enable to discover global properties of data (we are talking here about network science methods, multi-objective and multi-constrained optimization, variety of classification methods, machine learning, etc.

Would like to stress the importance of achieving scalability at all level, starting from mathematical models level through algorithms level down to systems level, we call it “vertical integration and validation of mathematical methods and algorithm” in ETP4HPC.

There is certain commonality in the mathematical approaches above both in HPC and Computational Science in the route exascale and in Data Science and in particular in the case of Data and Compute Intensive science and these should be further exploited.

3. QUANTUM

Quantum information vs. communication security (Mika Hirvensalo)

Communication security addresses most European citizens in an invisible way: Commercial banks, credit institutions, and companies such as PayPal provide electronic transactions channels whose communication security should be established in a reliable manner: It is highly undesirable that a third party could interferewith a confidential transaction and potentially obtain information or alter the messages between communicating parties. Until now, classical information and complexity theory has been used to develop protocols providing security against eavesdropping and interference. However, in the beginning of quantum computing era, those solutions may become powerless.

Theoretical investigations on quantum computing have appeared very interesting and promising: The quantum nature of the information sometimes offers computational shortcuts leading to very efficient solutions for tasks which seem very arduous for computers handling classical (non-quantum) information only.

For many purposes, an improvement of the information handling capacity is usually seen as a benefit, but this is not necessarily the case when protecting the information. In fact, improved computation power may be used to break a code used for secure communication. This is actually the case in all currently used asymmetric encryption procedures, which do not require a meeting of communicating parties prior to communication: An owner of a quantum computer could be able use efficient quantum algorithms to decipher the messages in a reasonable time, whereas such a fast deciphering procedure is not known for classical (non-quantum) computers.

However, the technological development towards a quantum computer has been a difficult task, indeed. According to Washington Post January 2014 article, NSA has invested approximately 80 million dollars to develop a quantum computer for breaking codes. Canadian company D-Wave Systems founded in 1999 has developed technical solutions to implement quantum computing, and as of August 2015, offers devices for quantum computing capable of handling more than 1000 quantum bits. On the other hand, no-one, including D-Wave Systems itself, claims that their solution is an all-purpose quantum computer. Rather on contrary, it seems that the technological solutions of D-Wave Systems are too limited to implement quantum algorithms for fast deciphering of the aforesaid communication protocols. However, existing research is not capable to properly estimate the computational power of limited quantum computing architecture used by D-Wave Systems, and hence the potential security threat caused by that type of quantum computers is not known well enough.

Quantum computing with an all-purpose instruction set is currently much more limited. The number of quantum bits technologically available seems not to exceed 15, and this limited amount of information storage is certainly not sufficient to endanger currently used asymmetric security protocols. On the other hand, any two-state quantum physical system can at least in principle serve as an implementation of quantum bit, and therefore the technological development towards a large-scale all-purpose quantum computer remains eminently unpredictable.

Nevertheless, in a due course currently used security protocols may be eventually threatened by quantum computers and need to be replaced with protocols believed to be resistant against a quantum computer attack. Currently however those protocols bear a burden of relatively large encryption key sizes. It may also be emphasized that the quantum-resistance of the proposed protocols is merely an assumption, but so is also the resistance of currently used protocols against classical computing.

Another possibility to improve the communication security is to use quantum information instead of classical already for the communication. Swiss company ID Quantique offers commercial solutions for quantum cryptography, utilizing security provided by the nature of quantum information. However, it is not very straightforward to start using these protocols everywhere, as they technically require a contiguous light fiber between communicating parties to ensure the security. Moreover, the security of the quantum protocol is known for the ideal version, but the practical resolutions are with no exception stressed with imperfections.

To ensure the communication security now and in the future, the computational power of D-Wave type quantum computers should be studied thoroughly, protocols believed to resist quantum attackers should be studied and developed further, and also the imperfect realizations of the protocols utilizing quantum information should be investigated further. The aforesaid type of research typically requires interaction of mathematicians, physicists, and computer scientists.

Quantum Computing Techonologies (Peter Dzwig)

While Quantum Computing represent very worthwhile mathematical areas to be working in and pose a raft of new issues which FET must address, by the same token these are technologies which are neither widely applicable nor are they likely to be within the timescales of H2020. Further there are not quantum computing technologies (as distinct from quantum encryption techniques) that are sufficiently robust that they could be delivered to mainstream, or even research facility, IT departments within those timescales. Therefore, if realisability is a criterion and therefore return on investment for the European tax payer this area cannot be one of the highest priorities.

4. COMPUTATIONAL MATHEMATICS

Uncertainty quantification (Bertrand Looss)

When dealing with complex and CPU-time expensive measurement processes or computer codes, as in all the domains of science, technology and industries, engineers and researchers have to adopt smart strategies in order to improve the robustness and the precision of their study results with a minimal cost (which is often the sample size). Indeed, several uncertainties affect their physical measures or numerical models, the required processing or solving algorithms, and the various model input data and parameters. In this context, it is essential to quantify and to take into account the uncertainties of input parameters and input data of the studied system. This topic is very broad but several key issues can be identified:

1) How to quantify the uncertainty of the system input variables in a coherent mathematical framework (for example via a probabilistic representation), potentially with expert elicitation and calibration techniques?

2) How to propagate these uncertainties through the studied system in order to infer the statistical properties of its output variables? This issue is closely related to the design and analysis of experiments methodology. If the system reliability is of interest, it is also connected to rare event inference and the choice of adequate measures of risk in order to support relevant decisions.

3) How to understand the relationship between the system inputs and outputs, considering all their complexity? This is the sensitivity analysis step, which can be based on various deterministic and stochastic approaches.

4) How to obtain robust solutions when optimizing system performances by using a minimal budget?

Furthermore, an essential challenge has recently appeared for simulated systems (which are more and more used to replace physical experimentation by virtual experimentation): the verification and validation (VV) steps. Verification deals with the correct solving of the model equations by the computer, while validation concerns the comparisons between model predictions and real observations in order to give some credibility to the computer simulations. The overall approach is called VV&UQ.

For all these objectives, methodologies have to be developed and tested on numerical benchmarks and real applications, for example for energetic prospective or defense purpose. From a mathematical point of view, the methodologies can come from various probabilistic concepts and tools, stochastic process manipulations, statistical learning and dimension reduction techniques, operational research methods, etc. while the practical issues are related to optimization, Bayesian inference, numerical analysis, mathematical programming, information visualization, HPC performances, etc.

Mathematical methods in Distributed Computing (Dmitry Feichtner-Kozlov)

Mathematical methods (including topological methods) in Distributed Computing is a new and promising area. Perhaps TDA could be interpreted more broadly to include that area as well.

Boolean functions (Jan-Georg Smaus)

Boolean functions, i.e., functions that take a 0/1-vector to 0 or 1, are the mathematical view of propositional logic and have interested mathematicians, logicians, computer scientists and economists for at least decades. They are at the heart of computer science because at the lowest level, any computation is just a realisation of Boolean functions, and so they have been studied in hardware architecture, more precisely circuit theory. Moreover, propositional logic is extensively used as a modelling language in, among others, artificial intelligence, operations research, and model-checking. Its application is powered by the tremendous success of SAT solving (www.satisfiability.org). Boolean functions are also highly interesting to mathematicians because of their wealth of structural properties. In economics, Boolean functions have been used in game theory.

Both the theoretical properties and the algorithmic problems surrounding Boolean functions give a wealth of problems to study.

Timothy Gowers and Albert Cohen expressed in this Consultation that mathematics research does not always need very large scale funding. In my opinion, this is is true in particular for research on Boolean functions.

5. DATA ANALYSIS

High dimensional data and big data are my preferred fields (Juan A. Cuesta-Albertos)

I thing both subjects are the more relevant challenges that mathematics have today. On the first hand, some useful methods to deal with high dimensional data have been developed, mostly when the data are (continuous) functions, but there are many other problems which have no satisfactory or even approximate solutions.

I have the feeling that the situation in Big Data Analysis is worst. I have the feeling that this field includes a huge quantity of very different problems; most of them being handled today using ideas taken from other fields which may be (or may be not) appropriate here. In this case, I would avoid to include restrictions like "Topological data analysis" or any other because I think that nobody knows for sure where the answer lies.

Approximate statistical inference for intractable models (Umberto Picchini)

The computational statistics revolution of the 1990s provided powerful methodology for carrying out likelihood-based inference, including Markov chain Monte Carlo methods, advances to the EM algorithm, many associated optimisation techniques for likelihoods, and Sequential Monte Carlo methods.

Thanks to these methodological and computational advancements researchers have been able to formulate increasingly complex models; however our ability to statistically analyse massive data sets (and the possibility to store and interrogate large data) has not increased at the same speed as our ability to formulate complex models for data analysis. In other words, we need faster, scalable tools for statistical inference. What is reported below is primarily focussed on methods for estimating model parameters and perform model comparisons (model choice).

Many challenging statistical inference problems of the 21st century cannot be addressed using existing likelihood-based methods. This is why the term "intractable likelihoods" is now used to denote the impossibility (either analytical or computational) for some realistic models to enjoy the treatment from standard statistical inference methods, see http://www.i-like.org.uk/.

Intractability of a model can be due to several, possibly concurrent factors, for example (i) the impossibility to write the likelihood function explicitly due to the presence of unobserved (latent) variables that should be integrated out and/or (ii) large data scenario where even if the expression of the likelihood is available this can't be evaluated within certain time and computational constraints.

A number of approximate inference methods for complex models and/or large data scenarios have been developed in the past 10-15 years, most notably:

- approximate Bayesian computation (ABC)

- Pseudo marginal computations and particle MCMC (for exact inference in a Bayesian framework)

- Composite and pseudo likelihoods

- expectation propagation

- variational Bayesian methods

- synthetic likelihoods

For example genetic studies have benefited enormously from the development of ABC methods since the end of the 90's, though these have spread into mainstream statistics only in the last 10 years or so. ABC methods are now widely applied in systems biology, astrostatistics, ecology etc. Besides being useful for complex (intractable) models, Approximate Bayesian Computation methods and "synthetic likelihoods" are certainly suited to large dataset scenarios, as they are usually implemented when information carried by low-dimensional summaries of data is employed rather than the full dataset, to produce approximate yet often informative inference.

Pseudo marginal methods (notably particle MCMC) are a class of methods particularly suited for inference in dynamical models (e.g. signal tracking, finance, stochastic models for systems biology), however they are computer intensive when applied to large dimensional models, same as ABC methods as both involve many iterations of Monte Carlo based algorithms.

Coupling these methods with data-subsampling strategies (where a certain fraction of the data is considered) has received consistent interest, see for example the review http://arxiv.org/abs/1602.05221

Research to speed up (and of course parallelize) all the considered methods is fundamental. As it is fundamental to achieve a deeper theoretical understanding of the properties of very fast approximate methods such as expectation propagation and variational Bayesian methods (whose theoretical features are still not very well understood). Methodological and theoretical advances should be pursued together with efficient computer implementations.

Monitoring Big Data (Alessandro Di Bucchianico)

Major technological advances in sensors and mobile devices have led to the availability of high volume – high frequency data originating from technical and social networks. These data streams not only contain information about directly measurable quantities but they also contain information about changes in their structure. Timely detection of structural changes of such networks is of paramount importance since our daily life (both social and professional) depends on these networks. Examples include detection of computer network intrusion and terrorist attacks (e.g., it is claimed by the US Army that one could have detected major changes in the Al-Qaeda communication structure a year before 9/11).

In technological systems such as smart grids, telecommunication infrastructure and production plants (in particular in smart industries), sensors offer opportunities to perform real-time monitoring with respect to both physical quantities and structural information. Apart from monitoring the real time performance of systems, these monitoring activities also offer great opportunities to reduce maintenance costs if one could develop data-driven maintenance planning optimization strategies. In the manufacturing industry, some of the actions determined after a warranty data analysis are similar to maintenance actions (in particular to reliability-centered maintenance). For example, early identification of reliability problems will prompt inspections in the production and supply chain; claim prediction in the long term, for the development of long-term warranties, can similarly point to flaws in the production and supply chains that need a corrective action.

Specific challenges in these areas include data reduction to allow real-monitoring of data streams (including efficient algorithms for sparse high-dimensional data), efficient ways to combine data sources (data fusion, inclusion of covariate data) and handling different forms of data like images, and monitoring the monitoring system. These challenges mostly have both a methodological aspect (e.g. how to draw valid conclusions) and an algorithmic aspect (scalability of algorithms for large volumes and/or high frequency data). In maintenance, one key advance would be the ability to streamline well established and effective analytic methods for condition-based maintenance (such as stochastic process models and time series modelling and filtering for failure and deterioration analysis and forecasting) through appropriate data harmonisation and summary.

↳ statistical thinking and methods are key to progress in this area (Ron Kenett)

Monitoring big data involves challenges at the technological, methodological, tactical and strategic level. Statistical thinking can integrate all these considerations and combine competencies from machine learning, information systems, data base design, and monitoring systems management. One proposal for providing such an integrated framework is focused on information quality dimensions. See http://eu.wiley.com/WileyCDA/WileyTitle/productCd-1118874447.html

↳ Vital for European economy (Rainer Goeb)

Big data processing and analyis are critical issues for the evolution of European economy in global competition. It is vital to promote and support research, development, and implementation on European level. Europe risks to stay behind in the field. The current Hannover Fair (25-29 April 2016), one of the worldwide largest industrial fairs, has a strong focus on smart manufacturing ("Industry 4.0"). A poll among companies made on the occasion of the fair shows that 80 % of companies fear that Europe will stay behind in international digital competition due to the dominance of US companies in the sector. The Vicepresident of the VDE (Association for Electrical, Electronic & Information Technologies), Gunter Kegel, urgently required a European agenda for the sector. A particular critical issue for European economy is the large representation of the SME sector: In 2013, the EU28 nonfinancial business sector counted for 21.6 million SMEs with 88.8 million employees and €3666 trillion in value added. SMEs are in danger to stay behind in the digital agenda, particularly in big data analysis. We need a European action to root big data analysis in European SMEs.

Democratic big data analysis (Shirley Coleman)

Waking up to the benefits of analysing their internal company data is a new challenge for small to medium enterprises (SMEs). They can see the enormous potential in terms of offering new products based on visualisation and insight but are not sure how to get started. Levels of confidence in handling data are worryingly low in many SMEs due to diverse factors such as loose responsibility for collecting and checking data, poor numeracy skills, secrecy and privacy fears about giving too much away upsetting customers and losing competitive advantage. SMEs are so important to our EU economy but risk being left out of the great advances in big data analysis realised by large organisations. A thorough review of the needs and barriers experienced by SMEs would be a good start to tackling the knowledge and confidence gap. In addition it is not enough to make help available, it needs to be implemented and this requires strategic intervention which can only happen when promoted and supported by evidence. Therefore further research should be carried out to explore the opportunities, fears and barriers to SMEs using their data resources fully and the interventions that would be effective. Rather than being a social policy or business study, this research needs to be grounded in knowledge of the advances in mathematics which are facilitating new ways of analysing data, for example developments in encrypted data mining.

Challenges of Big Data Analysis in Statistics (Muhammad Amin)

Big Data have new opportunities to modern world and challenges to statisticians. On one hand, Big Data hold great promises for discovering subtle population patterns and heterogeneities that are not possible with small-scale data. On the other hand, the massive sample size and high dimensionality of Big Data introduce unique computational and statistical challenges. These challenges are distinguished and require new computational and statistical paradigm. The penalization techniques to handle ultra high dimensional data are the best way to cope these problems. I just completed my PhD titled "Penalized quantile regression methods for high dimensional data and their applications".

Big Data and CAD/CAE in Engineering (Benjamin Himpel)

WT GmbH Science & Innovation develops and applies new mathematical methods towards problems in IT, Engineering and Consulting. Here are some topics, that we are interested in:

Big Data in Engineering

The automotive industry produces a vast amount of data. Either they do not use it efficiently, or they do not know what to do with it and cannot obtain any information from it. Clearly, these problems will grow and spread, if they are not addressed appropriately. Therefore, they provide the opportunity and necessity for new mathematical methods. We propose to take a close look at a variety of use cases together with research institutes and partners from the automotive, aerospace and health care industry, and develop the necessary mathematics for these cases. There are many different approaches to handle big data, but which methods you choose to pursue and combine, or if you have to develop a new one, depends on the use cases at hand. Therefore, it is necessary to connect experts and stake holders from different subfields. Mathematics is key for creating new methods which are general enough to ensure sustainability.

There are numerous new methods to study big data. Most of these methods are assume a simple data structure. However, most data in engineering is too complex for standard methods. Engineering Know-How is essential to understand different components and develop the best data mining methods. For example, we would like to test and further develop Numerical Tensor Calculation by applying it to test bays in the automotive industry and the simulation of acoustics in the interior of vehicles. Data structures (text, video, audio, etc.) fromdozens of sensors need to be analyzed and used in order to control different parts of the car.

Safety and Security for the internet of things, the connected car and autonomous vehicles: Despite impressive developments in these areas, robust and new mathematics is necessary in order to analyze existing methods, improve them or find alternate ways to ensure safety and security in these new technologies. Big Data certainly plays a big role in this, but also other areas of mathematics (e.g. encryption and data fusion). A collaboration of mathematicians, computer scientists, engineers and industrial partners will be important to make new technology safe and secure.

CAD/CAE in Engineering

CAD/CAE has a long history in design and simulation in the industry. Despite a lot of advances, the basic process has not changed much. In particular, the design and simulation still uses different structures, even though there has been a lot of effort to merge the processes and data structures.

Isogeometric Analysis takes a prominent role in the effort to combine design and simulation: This subject is well-established from a mathematical point of view. It allows for Finite Element Analysis to be integrated in conventional NURBS-based CAD tools, which makes simulations faster and more accurate. We would like to help make the paradigm shift in collaboration with research institutes as well as partners from the automotive, aerospace and health care industry.

Possible additional research areas (Ruth Kaufman)

1) Decision making beyond big data: most research on big data up to now has focussed on classification (recognition, learning, clustering,...) and inference. There is a need to go beyond these efforts and develop efficient algorithms/approaches for decision making and optimisation on big data sets.

This area would fit between or alongside the "Topological data analysis" and HPC.

This is an area where the OR community could make a substantial contribution.

2) There is scope for looking at how decision making under risk and uncertainty, classical components of Operational Research, could be extended to contribute far more than at present to crime prevention, climate change, migration, and defence.

Important topics for H2020 - data (Peter Dzwig)

The list of suggested areas contains some very important topics many of which merit investment through H2020.

However there are some wider points which need to be recognised and taken into account in any proposed programme. Principal among these is the qualitative distinctions to be made between machine-based data analysis and human data analysis. Recent work has demonstrated the substantial difference in analysis outcomes achieved by machines and humans, resulting from greater human flexibility and capability in reduction as well as in data integration. This stands behind the use of citizen scientists as part of the data reduction process in Citizen Science projects.

The human mind can make the kinds of “leaps” that computers, irrespective of level of sophistication, currently cannot. Therefore it would seem appropriate to develop approaches that would address how machines might make similar inferences.

Humans are particularly good at integrating divers sources of data. For example a human can look at stock markets, satellite imaging, climatic/weather data, traffic flow in ports and on sea-lanes as well as other sources to produce predictors of future commodities prices. Such approaches often draw on learned knowledge or use inference processes drawn from similar or related fields.

This might be considered to be addressed by the inclusion of topological data analysis. However this is a field in its infancy. While it may – and this has yet to be adequately demonstrated – work well in certain fields, it is unclear whether or not it will work when integrating diverse datasources, still less in how it might handle the constraints imposed by prior knowledge/expertise in similar but different fields.

Therefore it is appropriate to argue for an extension of the coverage to include the investigation (by topological means or otherwise) of how diverse data can be integrated. Equally important, if not more so, is to understand why some cases are not amenable to the technologies suggested.

To be able to handle large-scale diverse datasets may necessitate the creation of radical new mathematical technologies, but the ability to handle large data in scenarios such as those outlined above would give Europe a world-leading position in data analysis and reductuion.

Broadly I would suggest that the following be added to the list (although there is some overlap in several areas) with the proposed list, in addition to my contributions on Quantum and HPC:

1. Mathematics of complex dynamical networks:

We are starting to build complex dynamically changing structures whether viewed in the form of physical networks or networks of data or dataflows. At both large and small scales these structures will vary in time (for example of dataflows among a large fleet of driverless vehicles moving through an intelligent city, or other ultra-large scale integrated information systems). In such systems even local micro-reconfiguration may have a macro-impact. Within the next decade the number of nodes in the subsystems of such networks may reach substantially in excess of 10**9, yet we lack the mathematical underpinnings to handle such networks. We need exact techniques (probably the most difficult), approximation schemes, heuristics etc with appropriate domains of coverage to enable us to manage such networks. The internet offers examples of some heuristics, but not on the sorts of timescales that these systems could reconfigure.

Networks' based data analysis (Alex Arenas)

Along the lines of topological data analysis, that focus on the application of techniques from topology on data analysis, Europe should move on broadening the range of tools that can help on this. For example, statistical physics techniques and networks' are promising candidates to contribute to the new era of data analysis.

Functional data analysis and object-oriented data analysis (Ricardo Cao)

Statistical analysis of data in infinite-dimensional spaces (like functions) and complex structures (as object-oriented data analysis) is a challenge in this massive data era.

Please have a look at the web:

http://www.psych.mcgill.ca/misc/fda/

and the paper:

http://www.ncbi.nlm.nih.gov/pubmed/24421177

Compositional Data Analysis (Vera Pawlowsky-Glahn)

Many of the previous comments address the need for novel mathematical tools in the Biosciences, in Environmental Studies, in Industrial, Political and Social Studies, in interdisciplinary fields in general. Compositional Data Analysis (CoDA) is such a new tool. Its roots go back to a paper on spurious correlation by K. Pearson, published in 1897, and it has seen an amazing expansion since J. Aitchison introduced in 1982 the logratio approach. Nowadays it builds on the fact that the sample space of compositional data, i.e. of data in proportions, percentages, or, in general, parts of a whole that carry relative information, is a constraint subset of real space that has its own algebraic-geometric structure. More precisely, it has a Euclidean vector space structure which has been extended to a Hilbert space structure for functions with finite or infinite support. Recently, the importance and necessity of these tools in microbiomics, or other "omics", has been recognised, and all the problems related to these fields, like fat data, represent new challenges.

This type of problem is close to the fundamentals of statistics, which is based on the interplay between observations and parameters. Both, observations and parameters, are in measurable spaces, the sample space and the parameter space. Measurability of these spaces is the minimal structure required, but generally richer structures are convenient, if not necessary. For instance, vector space operations have to be defined for attaining central limit results; distances are required for straightforward definitions of mean values and variability (see Fréchet, 1948). Most contributions to statistics are at present assuming that both sample and parameter spaces are subsets of the real space endowed with the sum as group operation and with Euclidean metrics. At most, in functional data analysis or stochastic processes the sample space is a Hilbert L^p space with its metrics. However these structures inherited from real or L^p spaces may fail at modelling the main features of observations and parameters. We can mention some examples: compositional data which sample space can be represented as a simplex with its own Euclidean structure, where the sum is not the group operation; directional data, which sample space is the sphere, which can not be equipped with its own Euclidean structure; random sets, which need special operations and metrics; random positive measures, represented by densities, which can be included in vector spaces (Bayes spaces) where the group operation is Bayes updating.

It would be a great opportunity for European science to have this topic included in the program of Horizon 2020.

Predictive Analytics for Policy Making (Irena Ograjenšek)

Predictive analytics (use of simple and / or complex models based on historical data) has become increasingly important for anticipatory business decision-making in the past few years. Decisions such as whom to target in a marketing campaign, what products to stock, possibility of fraud, or who the "best" customers are for a firm are just a few illustrative examples of predictive analytics applications. Consequently, managers in consumer packaged goods, retailing, banking, gambling, energy and healthcare industries are currently the most active users of predictive analytics.

We believe predictive analytics should also be at the core of policy making. Government agencies should harness the power of (micro) data they routinely accumulate to a much larger extent when designing policy measures to deal with the challenges of population ageing, new migratory patterns, labour market shifts, capital flows, corruption prevention, etc. Predictive analytics should facilitate faster policy making and, when deemed necessary, also faster corrective measures design and implementation - both for the benefit of the general population and its specific vulnerable subgroups.

↳Business analytics work - so why not in politics? (Rainer Goeb)

Business analytics enable success in the private sector. A study from the MIT Center for Digital Business shows that organizations driven most by data-based decision making had 4% higher productivity rates and 6% higher profits. Why should analogous success not be possible in policy making? The problem: even in the private sector, many companies still struggle with adopting data-based business analytics, particularly when it comes to processing and analysing big data. The public sector is much further behind in this respect. Further impeding factors: lack of interest in evidence-based decisions in ideology-driven political parties, reservations and fears in the population against data collection and monitoring, data protection and privacy issues. We need a rational discussion on all these issues in society and politics on European level, and we need sound studies on how predictive analytics can work in policy making.

Mathematics statistics data-science (Han Geurdes)

Mathematics has its large value of e.g. reformulating a problem. However, I am not of the opinion that math is all that one needs to understand e.g. (sensor) data from technical objects such as dikes.

I think it were e.g. Brouwer and Bishop who, in addition to computability, also argued for semantics of mathematical statements. This attitude may help to bridge the gap between the topic under study and the meaning of the data analysis.

Mathematical challenges in Networks (Wil Schilders)

We live in a networked society with structures for transport of people, good, information, energy, and social contacts. Contemporary and emerging networks present unprecedented challenges, both opportunities and threats. Either way, it is of crucial importance for society and mankind that these networks are understood, controlled and optimized. There is a strong need for fundamentally new approaches to control large and complex networks, as networks tends to evolve towards criticality. Think of an epidemic spreading over a population (like the zika virus right now), or a virus invading a computer network.

Networks have also evolved into rich sources of vast amounts of data, and as such of tremendous value for understanding structural properties and optimizing performance characteristics of complex large scale networks, e.g. by extracting connectivity and topology information (see also the topic about Topological Data Analysis), or by exploiting measurements, event records and system traces in order to enable online adaptation, self-organisation and intelligent control.

There are two major grand challenges from the mathematical point of view associated with networks as described in the foregoing:

- Introducing self-organisation into real-life networks: tackling prominent societla problems in energy supply, transportation, communication, security and epidemics by building networks that are self-organized. Needed are stochastic modeling and distributed decision algorithms that rely on limited local information while achieving global near-optimality

- A universal theory of intelligent networks: aim is to formulate abstract theory, using the unifying character of mathematics and computer science in order to achieve robustness in applications

In order to address the above grand challenges, it should be realized that (i) randomness is everywhere (ii) there is a need for very fast algorithms. In terms of methodology, combinatorial optimization and efficient algorithms will provide the tools to deal with the complexity of deterministic networks, probability theory the framework to model and analyze the behavior of random networks, while stochastic operations research is needed to control and optimiza the performance of processes running on such networks, from the design phase to daily operation.

↳New hardware needs new numerical methods (Kees Vuik)

I completely agree: new hardware developments require new numerical methods. Methods that are parallelizalble on fine and coarse grain machines, that are robust and resilient and easy to keep up to date.

Proposed research areas in mathematics (Mason Porter)

"Topological data analysis and other potential mathematical methods for big data analysis" is a good area to promote, and other parts of mathematics that we need to develop further for analysis of Big Data are topics such as networks (e.g. generalizations of usual graphs to things like "multilayer networks" and time-dependent networks).

Applied topology and Topological Data Analysis (Pawel Dlotko)

It is great to see a topological methods emerging as a new, interdisciplinary topic to be promoted in Europe. Despite this discipline is a bit over a decade old, it already have a huge number of applications in data analysis, shape analysis, computational electromagnetism, dynamical systems, material analysis, brain research and many more. Currently there are at least two market applications: one in data analysis (Mapper), and one in computational electromagnetism. I am sure that much more are coming. Especially with this initiative!

It is also worth mentioning that this discipline come with a good, open source, software solutions that are currently intensively developed (like Gudhi, Phat, Persistence Landscape Toolbox, TDA R-package, CAPD and other libraries).

Topological data analysis and other potential mathematical methods (Clemens-August Thole)

Big data analysis of thousands of simulation results has the potential to accelerate the development process for new products and product variants like cars. A huge challenge is the comparisson/analysis of simulation results with different topologies. Mathematical methods have to developed, which rate different designs of parts of the car with respect to their similarity.

Product development like cars makes substantial use of numercial simulation. Many car manufactures use about 50.000 cores to support the engineering development tasks and create several PBytes of data per annum. SIDACT uses mathematics to compress the simulation results and our products are used on a daily basis by more than 20 car manufacturers (www.sidact.com). The SIDACT product DIFFCRASH compares hundreds of simulation results in order to improve the predictability of results.

Applied and Computational Topology (Claudia Landi)

I fully endorse "Applied and Computational Topology" as a key area in mathematics worth special attention in a world where it is so important to manage the data deluge. Indeed, computational topology techniques allow for understanding digital data from a new perspective and a complementary one with respect to other existing approaches for data analysis in terms of the global information they can provide.

Using topology, several areas of application-oriented mathematical research can contribute to this field so to develop techniques to analyze experimental data from a broad range of applications, each with its own specificity. Therefore, in my opinion, a multiplicative factor of success would be to prefer many medium-sized grants to just a few over-sized ones.

As a final remark, I think that the path from applied mathematics to real-world technological exploitation necessarily proceeds quite gradually. Therefore, when investing in a mathematically oriented research area, I would recommend to design a program proceeding toward exploitation little by little.

Topological data analysis and new maths for (Big) Data Analysis (Frederic Chazal)

The recent years have seen all domains of science, economy and even everyday life overwhelmed by massive amounts of data. Extracting and exploiting the best knowledge of this huge variety of data is a fundamental challenge for our society. This challenge raises new important questions and problems showing the need of new methods and approaches, coming from various areas of mathematics, to address them.

For example, (algebraic) topology and geometry, two areas of mathematics that until recently were not considered as playing a central role in data analysis, have shown their fundamental importance during the last decade and led to new successful methods for the analysis of various kind of data. They have given rise to the new field of Topological Data Analysis (

It becomes more and more clear that understanding the global structure, from a topological and geometric point of view, of high dimensional and complex data is of fundamental importance to address modern challenges of big data analysis. It also becomes more and more clear that to be successfully addressed these challenges need a strong combination of various statistical, mathematical and algorithmic approaches and, probably, the emergence of new mathematics areas.

EU reseachers have largely contributed to the emergence and first successes of Topological Data Analysis and other new mathematical methods for data analysis. I'm convinced that EU has the potential to play a leading role in the development of the mathematics of data.

Topological data analysis and other mathematical methods (Elena Celledoni)

I suggest that the topic "Topological data analysis and other potential mathematical methods for big data analysis" could be turn into " Novel mathematical methods for big data analysis including topological data analysis".

Statisticians have been working on various aspects of data analysis and big data for centuries, topological data analysis is an interesting new trend, but topology alone does not have all the answers to the big data challenge. Perhaps one could take into account that several mathematical sub disciplines must contribute to this challenge in order to see major advances in the field happening. Mathematics of information could also be a related and more comprehensive overarching topic.

Proposed research (sub)topic: Inverse Problems (Vadim Markel)

Analysis of large data sets will often involve solving some kind of an inverse problem. While the field of inverse problems is rather old, it is very closely related to and can benefit from the use of large data sets and HPC. Not mentioning inverse problems directly might be a mistake because it can discourage applications from researchers working on more traditional problems, say, in tomography. I propose to have Inverse Problems listed as a separate topic or as a subtopic to large data and/or HPC.

Computer simulations and inverse problems (Edyta Hetmaniok)

Considering the future of mathematics one cannot omit its relationship with the industrial development. Run of many industrial processes can be described with the aid of mathematical equations supported by the properly selected conditions. Appropriate selection of the initial and boundary conditions, as well as the parameters occurring in the equations, determines the desired run of the process and obtainment of the product satisfying the given requirements. Execution of the industrial tests is expensive and time consuming, therefore so important role is played by the simulation studies performed with the aid of such mathematical tools like the inverse problems of mathematical physics and the optimization methods. Development of the computer simulation discipline is strictly connected with the industrial development, thus it is certainly a challenge and an essential object of future research. The proposed research area: computer simulations and inverse problems.

Inverse problems (Olivier Goubet)

Inverse problems have numerous applications for health sciences (EEG, tomography), in mechanics and industry to test the reliability of metal pieces in planes for instance. To attack inverse problems that are difficult problems one need cross approaches using scientific computing, analysis, optimization. Moreover to handle uncertainties tools from stochastic modelling have to be developed.

Mathematics for Image Analysis in Production and Materials Sc. (Ronald Rösch)

"Sparse" data:

Image data sets get larger and larger, a 3D image generated by micro computed tomography can easily be larger than 20GB. Often, the major part of the processing is spend on finding interesting regions - inhomogeneities, defects, particular structural features. Strategies for detecting such regions automatically are needed.

Space-time series:

In-situ experiments produce sequences of spatial data. While imaging and reconstruction methods are improved continuously, quantitative analysis of the resulting data lags behind. Registration methods being able to cope with large distortions/sudden structural changes are needed. Moreover, special tinme-space characteristics and methods for measuring them based on image data have to be developed.

Analysis and modelling of multiscale structures:

Geometric modelling of materials microstrcutures is a prerequisite for structural optimization based on the real materiual. For multiscale structures, strategies for integration of structural information from several scales into one geometric model are missing. Moreover, analysis methods and models for structures (e.g. biological), where the scales are not clearly separated are needed.

Real time algorithms:

Depending on the algorithm, the computing time for series of 2d or 3d images can be enormous. In many applications, however, time is limited: By the cycle time for inline inspection systems in production or by the impatience of the users. A new generation of algorithms adapting automatically to the available hardware (optimization & parallelization & new numerics) is needed. Additionally, methods for estimating the needed computing time in advance have to be developed.

Image analysis with integrated simulation of physical properties:

Numerical simulation of macroscopic physical properties based on image data (e.g. visual, xray, ...) of the microstructure and upscaling has been established during the last years. Applications are mainly in materials science. To make an impact in production, too, this prediction of physical properties should be possible with minimal human interaction, only. The quality (e.g. stiffness, damaging, ...) of each produced single component could be evaluated individually: ok or waste, and/or each individual part could then be applied, where its properties fit best.

6. MODELLING AND SIMULATION METHODOLOGIES

MSO should definitely be in H2020 (Jordi Castro)

Math modelling, math Optimization and Simulation are likely the areas of mathematics that are most used in practical applications of mathematics.

MSO for industrial mathematics (James Gleeson)

Modelling, simulation and optimization (MSO) are crucial for the effective application of mathematics in all domains, but particularly for partnerships with industry. Examples of successful linkages between new mathematics and genuine economic impact are seen in the ECMI consortium (https://ecmiindmath.org/) and Study Groups for Mathematics with Industry (http://www.maths-in-industry.org/). I believe that such linkages should be strongly supported by future H2020 funding calls.

Adaptive modeling, multiscale and tipping points (Wil Schilders)

Further to the discussion of Mathematical Modeling, Simulation Control and Optimization (https://ec.europa.eu/futurium/en/comment/5925#comment-5925): The construction of models and simulation of natural phenomena as well as the improvement of engineering processes is increasingly facilitated by the availability of computing resources and large amounts of data. Not only can models be verified at a greater pace and with higher accuracy than ever before, it also becomes possible to explore many different variations of models. This state of affairs is currently changing our view on the role of a model. Models are generated at a fast pace by an increasing number of human researchers. In some disciplines, models are already being generated, provisionally, by the machine on the basis of large data collections. Models can be wrong and need refinement. In many disciplines, the changes over time make adaptation of models a necessity.

New sensors and infrastructures enabling real-time access to potentially enormous quantities of data allow datadriven model formation and self-adaptation by continuous monitoring of the complex systems under study. Consider, for example climate change. At some point, the observed weather variation turns into an undeniably structural climate shift and a new mathematical model is needed. This makes 'models' an interesting object of research in themselves. Some model changes are simply in the parameters and can be considered as evolutionary adjustments. Other model variations, however, are more fundamental. Reduction of models is another important aspect. Optimisation can be performed mainly with reduced order models, only in the final stages using additional complexity.

In recent years, many new insights have been generated in mathematics, computer science and the computational sciences, giving us the tools to rate and rank models quantitatively and mathematically. In addition, methods for model order reduction have shown enormous development, enabling the construction of models that adequately describe the behaviour but are not unnecessarily large and complex.Together with the availability of massive amounts of data for model validation, we now enter an exciting new stage of scientific computing. The identification of complex systems modelling as a research focus has the potential to increase the effective use of modelling by identifying and sharing insights over a wide range of traditional disciplines. The specialized modelling efforts in each traditional discipline yields fundamental knowledge that can potentially be applied to problems in other, sometimes very remote, disciplines.

In this context, we are faced with the following challenges:

- Tyranny of scales: the ultimate roadblock for many applications; most physical phenomena of interest today operate across large ranges of scale, rendering conventional computational methods simply useless:
    - Spatial scales up to 8 (crack propagation) or 10 (protein folding) orders of magnitude,
    - Temporal scales up to 7 (areothermal heating) or 12 (protein folding) orders of magnitude;
- Complexity of multi-physics, such as multi-phase, multi-discipline, highly nonlinear and evolving domains and interfaces; this calls for a new generation of computational methods;
- Model order reduction will soon become an indispensable tool for computational-based design and optimization, statistical analysis, embedded computing and real-time optimal control;
- Harnessing the power of emerging HPC technologies.

If we concentrate our efforts on the development of models, we can distinguish a number of potential research areas:

1. System reconstruction: For many natural phenomena, models have a mathematical description in terms of coupled differential equations. When the equations are nonlinear, complex behaviour such as 'chaos' may result. Examples are atmospheric convection, power grids and numerical simulations in astrophysics or molecular dynamics.The equations may

also have random terms to take account of noise in the system. Conversely, one may ask the question whether the underlying dynamical model can be (partially) reconstructed from observations of the state of the system. This is called system identification. The complexity increases considerably in high dimensions, as in highly connective networks of systems. Important questions center around extreme events in dynamical systems, such as weather, climate, or ecological systems (tipping points), or rare events such as nucleation events during phase transitions, conformal changes of molecules, and chemical reactions. Observations can also be combined with numerical models in a data assimilation process, as in weather forecasting. Here observations are combined with the numerical results (the forecast) to obtain an estimate of the current state of the system. The model is then advanced in time and its result becomes the forecast in the next cycle. Key questions:

o Can we extend the time series reconstruction method to the big data range?

o How can we obtain data-driven adaptive models for evolving (non-stationary) systems which are continuously monitored?

o Can we model the tails of distributions to distinguish between outliers (extreme events) and noise?

o Can we develop tools using big data sets to understand the dynamics and mechanisms leading to rare but important events in complex dynamical systems?

o Can we develop new algorithms for multiscale modeling that benefit from the parallel architecture of current computers and utilize accelerators?

2. Learning of models: A generic approach to study complex systems is to let the computer learn models by presenting it with data which serve as examples. Models may be considered as a ‘view on the world’, where several competing models are possible, and model selection is required. The process may also occur in an on-line learning fashion, where the system learns in a continuous way as new data come in (‘live’ models). This is especially relevant for studying time-dependent systems, for example evolutionary or adaptive systems. In the past, such systems have been developed by using dynamic neural networks that have been shown to be related to state space methods used for model order reduction. Recently, the MOR community is also targeting data-driven model order reduction (work of Antoulas, Beatty, Gugercin). Big data allows huge amounts of data for learning, leading to increased robustness in the learning process and the possibility of continuous verification of models. This topic clearly links data science with systems complexity. In addition, system identification is important for linking data with system models. Typically, data sets abound where not only the number of observations but also the number of attributes per observation is very large. Although the system may be initially described in a high-dimensional space, a much smaller set of features may suffice to describe the qualitative behaviour of the system, which is the essence of model reduction methods. Key questions:

o How can we bring down the dimensionality of the complex system to a more manageable size by dimension reduction? Model order reduction for analysis and control of complex large scale systems also fits here.

o Using too many variables with too little data may lead to overfitting. How does this change when big data is available for learning?

o Can we effectively combine learning and model-based approaches?

o How do we explore and visualize patterns in high-dimensional big data spaces?

Related to this is the following:

3. Large-scale computing: A characteristic of the big data era is that, although computational power has been increasing exponentially (Moore's law), the same is true for data size, which grows with a much larger rate constant. For example, the Australian/South African Square Kilometre Array (SKA) of radio telescopes project or CERN's Large Hadron Collider are capable of generating several petabytes (PB) of data per day. The calibration of SKA, involving simultaneous modelling of the sky and the ionosphere, might be one of the largest and most complex optimization problems of the coming 10 years. The target computing power of the Human Brain Project is in the exascale regime. In addition, there are clear signs that Moore's law is breaking down (Jagadish et al., Big data and its technical challenges, Commun. ACM 57(7), pp. 86–94, 2014). Techniques such as parallel and high performance computing, GPU computing, multi-core computing, or cloud computing will be of help here. However, for many applications the real bottleneck is not processing time but retrieval time from memory. Parallelization is only effective when the connectivity between parts of the systems is kept low, so that effective data partitioning can be achieved and memory transfers are minimized. Techniques are required which compute 'within the database'. An even more fundamental problem concerns computational complexity, i.e., the fact that some problems scale nonlinearly (sometimes even exponentially) with increasing data size. For example, if an algorithm scales as 2N where N is the number of data objects, then doubling compute power is not of much help. In that case the judicious use of approximations and heuristics or developing entirely new algorithmic approaches is the only way forward. This raises a problem especially for unstructured data which tend to be highly connected. Key questions:

o Can we develop new algorithmic approaches to tackle the increasing mismatch between the rates at which data increase and the increase of computational power?

o How can we design optimal data partitioning for highly connected systems?

o Can we engineer fast and reliable data transfer mechanisms for big data?

o How can we parallelize software in multi-core computing to optimize platform usage in software engineering?

o Can we design real-time techniques for pruning and compressing raw data volumes on the fly?

o How can we reduce the computational complexity of computation with big data with growing size of the number of components?

This aspect is related to 1. and 2., but it requires a more intimate relation with computer scientists.

↯ *Necessity of MSO research: a few examples (Dalibor Lukas)*

I fully subscribe to the previous comments on continuous necessity of mathematical research. I would like to underline that modelling, simulations, and optimization (MSO) is needed and by far not isolated. These disciplines are certainly key pieces in the complete mosaic. MSO follows progress in technology, engineering, physics, HPC, and vice versa. Looking at history, many sound methods for numerical solution to problems relying on partial differential equations such as multigrid or domain decomposition would not succeed without deep understanding of physics, functional analysis, numerical linear algebra, and computer science. Instant increase of computing power is followed by new mathematical concepts as well as by revival of those that might have earlier been rather academical. Nowadays discontinuous Galerkin becomes a method of choice for its robustness while the drawback of doubling the degrees of freedom is no longer an issue. Another example is simultaneous 4d space-time discretization of time-dependent PDEs allowing for massively parallel solution. Finally, high-dimensional problems arising for instance in stochastic modelling, tensor-product methods for computational chemistry, or compressive sensing are some of the mathematical research areas directly addressing current societal needs. The mathematical concepts have to be thoroughly studied with all its interconnections. They need inspirations, but they can not fully develop as side-products, which is unfortunately the current way of funding.

Endorsement: A very interesting topic with many applications (Antonio Gómez-Corral)

Endorsement: MSO: I fully agree! (Wil Schilders)

Mathematics for “Digital (Product) Twins” (Dirk Hartmann)

Mathematics is a key technology for data-based as well as physics-based modeling. Both have a high relevance for industrial applications, as raised by Volker Mehrmann.

However, physics-based and data-based modeling are very disconnected today not only in an industrial context. Physics-based modelling is focusing on early phases in the life cycle of industrial products and systems (design, engineering and commissioning) whereas data-based modeling is focusing on later phase (operation and service). Combining physics and data-based approaches to provide new solutions and services is a key challenge for mathematics.

How can we reuse / adapt engineering models in a (hard) real-time context on standard hardware required for operation? How can we realize hybrid algorithms using the best out of the two worlds? How can we reuse data to I improve engineering models? How can we realize product-integrated simulation algorithms (simulation inside) rather than relying on limited HPC resources? Or using a combination of product-integrated computation and HPC in the cloud.

Providing efficient answers to these questions has the potential to open up a new era of predictive solutions and services, e.g. products and systems designing themselves, autonomous self-optimizing and self-diagnostic machines and systems, or novel assist systems for users (see e.g. http://www.deskeng.com/de/seeing-digital-twin-double)

Mathematical models for solar power towers (Maria Rosaria Lancia)

These systems produce electricity on a large scale. They are unique among solar technologies because they can store energy efficiently and cost effectively. They can operate whenever the customer needs power, even after dark or during cloudy weather. It is of the utmost importance to test new designs for the heat exchanger or, in the case of Solar power towers, for the mirrors , to optimize the production of the electricity.

Mathematical Modelling for Environment, Sustainable Development (Olivier Goubet)

In Ecology and Environment sciences there are a lot of issues that may benefit from a quantitative analysis that requires mathematics. For instance, the mechanism of biological invasions and the study of competitive species.

This require tools from stochastic modelling, scientific computing and other fields of applied mathematics.

Mathematics of Mesoscale Systems (Miguel Bustamante)

Mesoscale systems occur in many areas of science and are characterised by the complex interaction of a large number of degrees of freedom, at length and time scales for which the analysis is neither amenable to a direct discrete approach nor to a size-independent continuum approach. Correspondingly, the modelling of these systems cannot be based solely on analytical results or on purely numerical simulations; rather, it requires for its success the combination of analytical and numerical methods, with a rigorous mathematical basis that is still in development. More specifically, the so-called range of mesoscales of a system is that range of scales at which a direct numerical approach is not feasible due to the lack of either computer memory/speed or robust validation methods, while classical continuum analytical approaches fail too due to the presence of nontrivial dynamics involving intermittency, spatio-temporal chaos, nonlinear resonances or turbulence. Thus, the mathematical modelling of mesoscale systems is one of the greatest challenges of this century, and provides a fertile ground for multi-disciplinary collaborations, spearheaded by mathematical research and going hand-in-hand with high-resolution numerical simulations.

In terms of real-life impact, the understanding of mesoscale systems is, without exaggerating, an urgent matter in Earth sciences, materials sciences, agriculture, food science and the global economy in general. For example:

(1) In the atmosphere, the understanding of mesoscale convective systems and other mesoscale patterns provides continued improvements of numerical weather prediction models, which have strong impact on the world economy and safety. On a more topical note, climate change is the most urgent environmental challenge faced by humankind today. Scientific success on this matter requires a mesoscale approach where the most relevant degrees of freedom can be singled out, in order that accurate predictions are made. Again, an interdisciplinary approach is required involving statistics, mathematical analysis, computer science, physics and mathematical modelling.

(2) In materials science, multi-scale simulations involve the nested coupling of models at several levels of scales: from the smallest quantum mechanics scale to the molecular dynamics to kinetic theory to continuum mechanics.

(3) In fluid mechanics, and more recently in complex fluids involving the presence of active matter (such as microorganisms), the goal is to model mathematically in an adaptive way both the microscopic and the macroscopic dynamics. To validate the models, experiments are performed at all scales using the latest technology, such as high-speed cameras and particle image velocimetry. The same approach is followed in the case of nonlinear optics, where the main goal is to understand how to design optical media so that optical signals are transmitted efficiently and with minimal losses. The greatest challenge in all these areas is the understanding of the inner workings of the nonlinear interactions, which sets the way energy is transferred across scales, and the preferred directions of such transfers.

Beyond urgent issues involving immediate impact, mathematical modelling of mesoscale systems has the potential to contribute to the development of more fundamental areas of research, such as high-energy physics, cosmology and the newborn field of gravitational wave astronomy. In the latter, for example, the LIGO family of gravitational wave detectors might very soon detect events that do not have a theoretical explanation yet, and whose explanation might require the modelling of fast, highly energetic and highly nonlinear processes occurring in massive collisions, involving the interaction of matter, light and gravitation at the mesoscale.

Endorsements (Paolo Orlandi, Michael Wilczek)

6.1. Geometrics based mathematical methods

↳ An important field, connecting several fields (Klas Modin)

Geometric mechanics (including its stochastic formulation) is an important field with connections to both applied and theoretical areas. In particular, I would like to stress the strong link to Geometric Numerical Integration and Shape Analysis, suggested by Elena Celledoni.

In the last two decades, we have seen a clear trend in both analysis and applied mathematics that geometry and geometric techniques are used more and more: Ricci flow, optimal transport, information theory, fluid dynamics, PDE on manifolds, symplectic numerical methods, are just a few examples. There are many others in the pipeline. European universities cannot miss out on these developments.

↳ Endorsement (Klas Modin)

I agree. Information geometry is an emerging field of applied mathematics, with many important applications on the horizon and many open questions rooted in geometry and analysis. Notice also that Information geometry is closely connected to Geometric Numerical Integration and Shape Analysis (as advocated by Elena Celledoni) and to Geometric

Mechanics (as advocated by Darryl Holm); to explore these connections is an active, prosperous line of research.

Geometric numerical integration for technology and innovation (Elena Celledoni)

It is nowadays well understood that numerical methods that mimic underlying geometric structures of dynamical systems have generally superior dynamical behaviour.

A typical example is a Hamiltonian system integrated by a symplectic method: the symplectic numerical solution solves exactly a perturbed Hamiltonian system. This powerful property guarantees good behaviour of the numerical solution for very large intervals of integration and cannot be obtained by simply projecting a non-symplectic numerical solution on the correct energy surface.

Since its independent discovery in the accelerator physics and celestial mechanics communities in the 1980s, the field of geometric numerical integration developed rapidly in the 1990s with many new approaches discovered and, crucially, greatly improved theoretical insight into the behaviour of the methods. International conferences and workshops were held at Berkeley in 1998, Melbourne, Oxford, and Durham in 1999, Beijing and Durham in 2000, Oslo in 2002, Cambridge in 2003, and Oberwolfach and Castellon in 2006, Edinburgh in 2007, Oberwolfach in 2011 and 2016. The top 50 papers in geometric integration have been cited 3400 times; just over half concern applications. A new integrator for the solar system, incorporating all the progress of the 1990s, contributed to the 2004 Geophysical Time Scale and was responsible for re-aligning epochal boundaries by millions of years. Later the same team of researchers led by Laskar and Gastineau produced more and more precise simulations of the solar system, and one of their striking discoveries was to give simulation evidence that however remote a collision Earth-Mercury is possible.

The European mathematical community has gained considerable expertise in the field of geometric integration in the last few decades, contributing, to a greater extent, to the theory development.

The exploitation of this knowledge in a more applied problem setting has great potential for applications in science and technology and for innovation.

Research at the interface of geometric integration and various aspects of fluid dynamics for weather and climate prediction has been pursued in the last decade with success.

One possible example is to apply geometric numerical methods in the study of the dispersion of non-spherical particles in turbulent flows. Suspensions of tiny solid particles in a carrier fluid are most often treated as a mixture of solid spheres and a gas or liquid fluid phase. In practice, however, the particle shape is different from spherical, for instance micro-organisms like phytoplankton in the ocean and volcanic ash particles in the atmosphere.

New fundamental insight into the dynamics of non-spherical particles in turbulent flows could be achieved by simulating the dynamics of the particles by geometrically exact techniques with significant, potential impact on environmental, health and climate research.

Recent studies have shown that incorporating geometric structure in animation of character motion for video games and animation movies, gives improved performance in the manipulation of motion data. A similar line of research has been pursued in the last decades from the research community of shape analysis, and the Riemannian geometry of the infinite dimensional manifolds of curves and surfaces has played a central role. Many problems in object and activity recognition can be formulated in terms of similarities of shapes, successful applications of this research can be found in medical imaging, speech recognition, and applied to studies of bird migration, hurricane tracking and video surveillance.

Future developments of data analysis and data exploitation should combine geometric underlying structure with statistical methods, and use techniques from shape analysis for the numerical solution of complex dynamical systems and for data assimilation. For example, applications to biomechanics, human locomotion and robotics could benefit from the synergetic interplay of these mathematical disciplines in an appropriate applied context. Similarly, geometric insight can bring improved quality of the analysis of spatio-temporal neuro-signal recordings, used to understand animal and human brain activity as for example in studies of perception of space.

MSO (Jean Clairambault)

I fully endorse this proposal for MSO in applied mathematics. I would like to stress that a field in which optimisation should be particularly welcome is medicine. This is seen from the point of view of mathematicians, as physicians seldom even suspect that there can exist a field of mathematics called optimisation and optimal control, applicable to medical treatments. Therapeutic strategies in the clinic usually rely on sheer empiricism, for interactions between therapists and optimisers are scarce. It could be a goal of this MSO initiative to encourage their multiplication on well-defined applications by clearly mentioning therapeutics in its objectives and proposing to actively support it by budgeted lines.

[endorsement not clearly associated to a specific contribution]

6.2. Simulation

Simulation and Virtual Experimentation (Alessandro Di Bucchianico, Grazia Vicario, Ron Kenett)

State-of-the-art

Simulation models of products and of processes are increasingly and intensively used in the last half century as an approach designed to shorten time to market, reduce costs and improve product quality. As an example consider the PENSIM simulation software modeling penicillin production in a fed-batch fermentor. The model includes variables such as pH, temperature, aeration rate, agitation power, and feed flow rate of substrate. The simulation package is used for monitoring and fault diagnosis of a typical penicillin fermentation process. It is also used for both research and educational purposes and is available at http://simulator.iit.edu/web/pensim/index.html. The perspective presented here is to push forward the idea of making simulation a standard design tool for continuous product innovation and for enhanced educational experience. However, to enhance the diffusion of simulation software in engineering design and education requires adequate methodologies for managing the use of simulations and for increasing its effectiveness. In this context, statistical methods play a significance role in both the design and the analysis of computer experiments. Simulating the behavior of a system with a computer is an effective and cost efficient approach to knowledge building. Computer experiments are however different from physical experiments in that the output of a computer run is deterministic; and not affected by measurement error and experimental noise. On the other hand, like with physical experiments, models and knowledge derived from computer experiments need s to be also validated.

Research strategy

The research strategy proposed below builds on four complementary areas focused on the problem of building knowledge via computer experiments. These areas are: i) integration of physical and computer experiments, ii) robust design, iii) prediction models and iv) simulators in training and education. We briefly describe next each of these areas without listing references or specific research clusters.

1. Integration of physical and computer experimentation in engineering design. The objective is to develop methods for alternating computer experiments and physical experiments until a satisfactory design or improvement solution is achieved. In fact, it is possible to design a strategy of knowledge building where physical and numerical experiments are combined in order to formulate new hypotheses. The iteration between physical and computer experiments permits to verify the hypotheses and perform a dynamic validation process of simulation models.

2. Robust design of product/process. The objective of the methodological contribution in this area is the validation of experimental methods integrating parameter and tolerance design using computer experiments. The classical crossed-array and combined array will be generalized with more flexible procedures. Specifically, simulations presenting the transfer of variability in input factors to system responses will be developed and the optimization properties of such models will be evaluated.

3. Accurate predictions with Kriging/Gaussian models and analysis of covariance structure. In principle, the statistical framework of classical Design of Experiments can be applied to simulated experiments. Some precautions are however in order because of the intrinsic difference between physical and numerical experiments. The lack of randomness in the latter render meaningless replications aimed at the evaluation of experimental error. To simulate the behavior of physical systems, a mathematical model is applied to data from computer experiments and practitioners often seek an approximate model (sometimes called emulator or metamodel) appropriate for optimization, fine tuning and control. Since a good design requires a good predictive mechanism, Kriging models are often used because of their ability to provide good predictions. These models require an accurate investigation of the covariance structure and its medialization. It is a complex issue to decide which class of models for the covariance is the best among the available options.

4. Simulators in training and education processes. Simulators enabling the integration of hands-on student engagement in training and educational processes offer new opportunities

for enhance pedagogical effectiveness in training and educational programs. This applies in a wide range of domains starting with basic statistical courses up to complex process control and system design methods. The opportunities are even greater when one consider the growing interest in life long learning and MOOC initiatives with generic and tailored versions serving the needs of specific populations. This research areas will focus on such challenges by combining expertise in education, engineering and statistics.

↳Validation, calibration, integration (Jacqueline Asscher)

This problem is very important in industry. If I have a real system and a simulator, I could want to 1) decide whether or not the simulator is close enough to the real system to be useful to me at all 2) characterize a limited set of conditions for which the simulator is useful 3) calibrate or adjust the simulator (I think this is what the authors mean by "dynamic validation"). I may also have a simulator and two real systems, where one is cheaper to run, such as a pilot version of the process. And I will probably want to use the simulator repeatedly, in which case I'll have data from previous projects.

7. BIOMATHEMATICS

Biomathematics (Clemens-August Thole)

Physics has already been a source of inspiration and insight in mathematics. Nowadays it also biology. The interplay between mathematics and biology at every scale will crucial in the future.

Endorsements (Ivan Area, Delfim F. M. Torres)

↳Mathematical biology including stochasticity (Louise Dyson)

I also fully support this area. In particular, the effects of the inherently stochastic nature of biological systems is an important area for future research, and we need to understand this area and develop tools in it to be able to understand the biological systems and ways in which they may go wrong.

↳ I fully agree (Susanne Ditlevsen)

The importance of biomathematics in itself is increasing, but even so more the understanding of noise effects in biological models, where our understanding is still very limited.

Biomathematics (Tomas Nilson)

In Mathematical Biology many areas of mathematics can contribute and collaborate. One example is Biostatistics that encompasses the design of biological experiments.

Stochastic modelling, pharmacokinetics/pharmacodynamics (Umberto Picchini)

Importance of biomathematics:

See for example how elegant mathematical models such as stochastic differential equations (SDEs) enable descriptions of biological systems and other natural phenomena:

- Stochastic modelling for systems biology, https://goo.gl/yr4bQh
- inference for diffusions with applications in life sciences, https://goo.gl/aJn8BC

Or the modelling of concentrations of medical drugs, useful for comparing the performance of different drugs at a population level (hierarchical, mixed-effects models). This is a classic application in biostatistics but it becomes challenging when SDEs instead of ODEs are used, as we can separate and better identify (ii) within-subject variation; (ii) between subjects variation; (iii) residual variation, well separated from stochastic individual variation. This leads to better inference for the general population, i.e. more accurate results and parameter estimation. See https://goo.gl/8vzoaD

Mathematics for drug discovery (Tomas Vejchodsky)

New drugs capable to cure currently incurable diseases are highly desirable, but the current process of drug discovery is very inefficient. Only one out of 5-10 thousands target molecules passes all stages of tests and becomes a successful drug. All these tests can cost up to a billion EUR and can last even 15 years. If the proposed target fails at some stage of the tests, all the invested money, time, and human capital are wasted.

The main reason for this inefficiency is our lack of knowledge of biochemical processes in living cells. The reason is simply the large scale and great complexity of the involved systems. Yes, in recent years we made an incredible progress in this field. We know the chemical species involved, we have read the DNA, but we still know a little about functioning of these complex biochemical networks. For example, we all take paracetamol to decrease pain and fever, but it is still not known how and why it helps.

We are still at the beginning and it will take decades to considerably improve our understanding of complex biochemical systems. It is a huge and incredibly difficult task with a lot of unknowns. However, one think is clear. To achieve any significant progress we will need new methods, computational power, wide interdisciplinary cooperation, and especially a very deep knowledge of mathematics.

Stochastic modeling in biology (Susanne Ditlevsen)

Mathematical modeling including stochastic effects in biology has huge potentials for improving our understanding of the studied systems. There is a long tradition of studying average behavior of the systems, but it is essential to also include the stochastic behavior, which might have huge impacts on the dynamics, especially in nonlinear systems. Noise is ubiquitous in all living systems, but our understanding is still limited.

7.1. Discussion between Plamen Simeonov and Luis H Kaufmann

Biomathematics as Integral Biomathics (Plamen Simeonov)

I fully endorse this line of research in its embossment as Integral Biomathics, which has been pursued for 5+ years now and includes many fields of basic and applied mathematics and computation: search on Google, arXiv.org and ScienceDirect, as well as:

- 2015 JPBMB Special Issue on Integral Biomathics: Life Sciences, Mathematics and Phenomenological Philosophy (note: free access to all articles until July 19th, 2016)

http://www.sciencedirect.com/science/journal/00796107/119/3

- 2013 JPBMB Special Issue on Integral Biomathics: Can Biology Create a Profoundly New Mathematics and Computation?

http://www.sciencedirect.com/science/journal/00796107/113/1

- 2012 Integral Biomathics: Tracing the Road to Reality

http://www.springer.com/de/book/9783642281105

- 2011 INtegral BIOmathics Support Action (INBIOSA): FP7 Grant number 269961

http://www.inbiosa.eu/

The major problem we face today in life sciences is that many biological phenomena do not have adequate mathematical representations. This is because living systems deploy logic and semiotics beyond our modern conceptions of mathematics and computation. Current approaches to complex problems rely on modelling, but one aspect of the problem is that they rely on a single form of mathematics, switching from it to another to address the next aspect, and so on. All this switching is an indication of how inadequate our mathematical tools are to date (ODE/PDE systems, stochastic models, discrete state-transition systems, topological algebra, etc.). Biological systems function at all these levels simultaneously, so why can't mathematics do the same? It is not biology that is too messy to be modeled; it is our use of current mathematical paradigms that are not able to adequately address these biological problems.

We claim that it is impossible to make significant progress in theoretical biology and medicine without a post-reductionist breakthrough paradigm change towards biology-driven – not "biology-inspired"(!) – mathematics and computation. The goal of Integral Biomathics is to accelerate scientific development in life sciences and personalized medicine through profoundly new theoretical foundations. We pursue a unified view of life that goes over scales, both spacial and temporal.

↳↳This is a good response to my provoking question (Plamen Simeon)

This is a good response to my provoking question, Lou; therefore my vote with 5 stars, since it brings up the question about the true nature of biomath and biocomputing. I am not accusing neither mathematics, nor the mathematicians for not having provided more adequate means to match the living world as we wished. The issue is that even with the well-known DNA or cellular computation we are not computing like cells or molecules as they do in fact. We are enforcing them to perform something mechanistic and not natural. Therefore I think that this model is not a natural one, even if it can be set upon some control. Why do cells divide and differentiate? It certainly not to perform or parallelize some Boolean or other human taught operation. What I hae in mind with such an allegorical critics is the biological analogy of what Richard Feynman said about the necesity of a "quantum automation"to model quanum processes. Hence we need a biological computation (and mathematics behind it), but much more natural, I'd say "naturalistic", than the one we are used to address along the old Church-Turing-vonNeumann type of machinery. Well, we might have some kind of quantum computation at the lower levels, be we will still need a true biocomputing at the cellular, tissue and organ level that match the respective roles of the system components.

↳Integral Biomathics up to Cognition (Andrée Ehresmann)

I endorse Integral Biomathics as an important project, to better understand living systems in their development, up to the emergence of higher cognitive processes. This aim should be attained through the use of different mathematical domains, unified through a 'dynamical' category theory approach (as begun in our MES methodology).

Biomathematics (Plamen Simeonov)

It is natural that some people do not like paradigm changes.

Remark on "one star out of five" voting received for this comment:

Voting with 1 star on this statement only confirms its truth. That such emotion driven “dislikes” without commentary telling “why” will not stop development is also another truth.

Mathematical Biology, Biomathematics and Biophysics (Rui Dilão)

I think that biomath is a growing field of research from two different perspectives:

From the biological point of view it brings a fresh look to biological problems through the introduction of precise and quantifiable models of specific biological phenomena. The biological predictions based on this model are very valuable and contribute to the fast development in biology.

Examples of successful predictive models: Leslie-McKendrick-von Foster demography models; Hodgkin-Huxley action potential model. Bioinformatic and data mining models for DNA analysis.

From the mathematical point of view, biology gives classes of systems that need to be understood from a formal point of view. Biology will have an inspiration role to mathematicians. We compare its role with the role of physics to mathematics. There are mathematical fields that we can no longer say if they belong to physics or to mathematics. Examples of mathematical problems arising from biology are: Morphogenesis and pattern formation, Stefan problems in PDE, Percolation and stochastic models, symmetry problems, genetic algorithms and stochasticity.

BIOPHYSMATH (Jacek Miękisz)

Living cells store, process, and transmit information. They respond to stimuli and make decisions in highly stochastic environments. Such small biological systems should obey (some yet unknown) principles of non-equilibrium physics. So we need collaboration between biologists who know real-life phenomena, physicists to construct models using concepts and techniques of statistical and other branches of physics, and mathematicians to develop new analytical tools to deal with such models and to find relations between abstract structures present in life processes. We need BIOPHYSMATH programmes to understand biological, physical, and mathematical theory and practice of information transmission. This might be useful for example in designing efficient micro-machines in synthetic biology and socio-economic regulations and mechanisms of early warnings of possible crises.

Biomathematics (Susanne Ditlevsen)

On behalf of the board of the European Society of Mathematical and Theoretical Biology, we suggest research themes in Mathematical Biology, where applied mathematicians, statisticians and probabilists can contribute. Mathematics and statistics are playing increasingly important roles in the biological sciences and contribute today at all levels from design of experiments to analysis of data, and exploration of models and their properties. It is becoming clear that most complex systems are highly nonlinear and act in non-intuitive ways, and that the causal mechanisms cannot be understood without mathematical models and their analysis. There are lots of interesting research going on in Europe within Mathematical Biology and it has increased considerably in the last couple of years. Specific calls for projects within this vast and important field are needed.

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8. FINANCE MATHEMATICS

Risk and Performance Scenarios for Investment Products (Ronald Rösch)

With the PRIIPs regulation, risk and performance scenarios for all Packaged and Insurance-based Investment Products need to be calculated. The stochastic models in use are complex, especially when it comes to specific products. We therefore suggest to review the existing approaches for scenario calculation, in particular with a focus on the persisting phase of low interest rates. Moreover, a modelling framework for different product classes should be developed. This ensures the comparability of the results and therefore poses a real benefit for the consumers.

Mathematics for the financial markets (Luis Alvarez Esteban)

The global financial markets have experienced an enormous increase in complexity over the past decades. Both the amount as well as the variety of market participants has more or less exploded and the range of different available financial products has increased significantly. This trend persists regardless of the recent financial crisis and its consequences. In light of this observation, it is clear that there is a need for mathematical approaches that can take into account the multiple sources of risk and uncertainty that this complexity calls for: Namely, credit and liquidity factors (credit valuation adjustment, funding valuation adjustment, counter-party risk, etc.), interest rates and inflation (multiple-curve models), and demographic factors such as longevity risk. As models incorporating these driving components become increasingly complex, their analysis becomes computationally more challenging and call for more efficient numerical techniques based on the recent developments in computational mathematics and mathematical statistics.

9. COMPLEXITY

Complex Adaptive Systems (Rick Quax)

Seemingly very different complex systems may actually display the same kind of behavior. For instance, a phenomenon called ‘criticality’ is a useful type of instability to rapidly respond to external stimuli in neuronal networks in the brain, gene-regulatory networks, bacterial colonies, and the flocking of birds, among others. Other cross-domain phenomena include tipping points, resilience, controllability, and the emergence of scales. From this the picture emerges that the mechanistic details of complex systems may be irrelevant for generating such cross-domain phenomena. However, the young field of complex adaptive systems currently lacks a unified framework to study emergent, cross-domain phenomena in a way that ignores these irrelevant mechanistic details. The current approach is to study such phenomena in domain-specific models, such as a neuronal network, which complicates the matter and limits the rates of scientific progress on questions which span multiple domains.

Given also the fact that H2020 intends to increase its focus on interdisciplinarity, I argue for the topic of 'Complex Adaptive Systems' to be included as a research area in its own right. Foundational research should be invited which specifically crosses domain boundaries and develops theory and mathematical modeling to understand phenomena which are observed in multiple domains.

Complexity and Health (Heleen Wortelboer)

Key words: Complexity, Health, Dynamical system theory, Applied logic, Category theory, Information mining, and computer science

EU wide projects offer a chance to construct multi-country teams of researchers which can study important societal challenges from a European perspective. Providing affordable healthcare for EU citizens is a major challenge, especially due to the continuous rise of chronic complex life style diseases such as obesity, diabetes type 2, cardiovascular problems, and dementia. To solve this problem, new insights and hypotheses are urgently needed where mathematics and complexity science can be of beneficial use.

Personal health

Personal health can be identified as a complex adaptive system, in which multiple health-related nested systems (physiological, mental, societal, spiritual, cultural) are involved. Studying health from a complexity perspective across Europe via EU wide programs offers the opportunity to integrate system levels from small scale to large scale: biology, psychology, behavior, family, work environment, society, country, EU.

We foresee that a trinity of mathematics-logic-computer science is needed. Due to the interdisciplinary nature of this problem lexical ambiguity within the different semantic fields or knowledge domains can arise. Hence what is needed is a perspective on the correspondence of the concepts within the different contexts of the knowledge domains or disciplines. An area suitable to address this segment of the problem is applied logic. Next, to successfully translate the abstract concepts developed from the results of applied logic to more suitable structures computational science is a necessity to deal with processes of processes (models of models) and defining analogies and equivalences in more precise ways, and for this category theory could be a suitable candidate. Beside the more applied computer-science described above, theoretical computer science is probably also needed because of software architecture.

Community empowerment and Citizen science

As an organization of applied research, TNO has the ambition to deliver applications that are of immediate benefit to society. Besides the importance of mathematics on new insights on individual health as described above, health can only improve if people are empowered to become aware of their health, are able to monitor their health, experiment with the effects of life-style changes on health and get feedback on such experiments. To support this, mathematical algorithms and tools should use open standards, should be accessible by Health Data Cooperatives, the EU Open Science Cloud, and should be able to use existing available health data.

Mathematics could also support the development of an EU wide citizen science community. Citizen science projects have shown a huge capacity to involve citizens in research. Citizen science related to health could be a fruitful way to raise health awareness and stimulate healthy behavior changes. Furthermore, a citizen science community can accumulate huge amounts of data and interpret results from algorithms. Additionally, citizens can provide real life data that is actually relevant to the individual. Mathematical algorithms need to be developed that can deal with this real life, often heterogeneous data, captured with multiple devices such as cell phone apps, trackers and in patient records. To improve community empowerment and citizen science we would encourage the requirement of a patient or citizen organization to participate as a recognized partner in H2020 proposals.

The approach indicated could have immediate impact of mathematics and complexity science on the health of EU citizens.

10. VARIOUS MATHEMATICAL TOPICS

Tropical mathematics (Maria Jesus de La Puente)

(https://ec.europa.eu/futurium/en/content/tropical-mathematics)

Tropical algebra and geometry is a new trend in mathematics. It has received a lot of attention since the 1950's by mathematicians all over the world: from Europe, EEUU, Rusia, etc. Tropical algebra is also called max-plus algebra. Several optimization problems arising in job scheduling, location analysis, transportation networks, decision making and discrete event dynamical systems can be formulated and solved in the framework of tropical geometry. Tropical methods have been used by economists Paul_Klemperer and O. Shiozawa. Tropical geometry and algebra are related to convex and discrete geometry (polytopes and polyhedra) and to matroid and oriented matroid theory.

A warning on the adjetive tropical. It might be misleading. Tropical mathematics has no relationship whatsoever with the geographic tropics, nor with tropical countries, clima, diseases, etc.

Statistical problems for Horizon 2020 (Gabrielle Kelly)

Proposed area: Statistical problems (e.g. in geostatistics and environmental science) involving combining several sources of data that are dissimilar in resolution and scales of coverage.

Fractional dynamics (Ivan Area)

By using integrations and differentiation of fractional orders, by methods in the fractional calculus, fractional dynamics could help to investigate the behavior of systems and objects.

Endorsements (Delfim F. M. Torres, Manuel Ortigueira)

Dynamical systems (Olivier Goubet)

The study of dynamical systems partakes of pure mathematics but has numerous applications in real life. To support the fundamental research in dynamical systems (ergodic theory, infinite dimensional dynamical systems,...) will lead to future developments in computer sciences, bio-mathematics, automatic, physics and other fields of science.

Spectrum continuity - a sort of political comment (Massimo Ferri)

In my opinion a central issue is the continuity of the research spectrum. OK with MSO, of course; OK also with “pure” maths, meaning research which apparently does not have any direct applications and which draws its inspirations from within mathematics itself. But it is important to have applications-oriented mathematics, which works at building theoretical frameworks like pure maths, but finding motivation in science and technology.

This builds a connecting link useful for both the applied domain - to which it offers more structered tools -and to the pure fields, to which it gives new impulse and a wider visibility. After all, it is important to make our work understandable by the politician and the tax-payer.

This is the case, e.g., of topological data analysis and topological robotics.

Unfortunately, at least in my country (Italy) this viewpoint meets some resistance in the “pure maths” circle, which praises in the media the great applicability of our discipline but punishes applications-oriented maths in the competitions.

Questions from Matrix Theory (Helena Smigoc)

An abundance of questions from Matrix Theory are motivated by applications. I highlight completion and nearness problem, and matrix factorisation.

Completion problems arise in applications when a partial information on a matrix is known and a completion of this matrix satisfying some conditions is wanted. Similarly, nearness problems are looking for a matrix with some desired properties, but in this case a matrix that is as close as possible (in some sense) to a given matrix is requested. In both problems some structural and spectral constraints are imposed, for example:

Low rank, Positive semidefinitness, Stability, Prescribed inertia, Orthogonality, Zero-nonzero pattern, Sparsity, Positivity of elements.

Matrix factorization is a factorization of a matrix into a product of two (or more) matrices, and it is of interest to find matrix factorizations where the factors have some desired properties, examples of those properties can be taken form the list given above. Matrix factorizations reveal hidden structures of a matrix and are used in machine learning, signal processing, analysis of big data, textural analysis, etc.

While some aspects of the questions that are given above are classical and well researched, there is an increasing demand from applications to impose several conditions at the same time. Moreover, looking at the problems for matrices over finite fields gives another dimension of difficulty. In those cases existing tools do not apply and new ones need to be developed. Problems that demand structural as well as spectral constraints are particularly challenging and they connect several areas of mathematics from linear algebra, to combinatorics, graph theory, and number theory.

The Need for a Systematic Support of Individualistic Decision Making within Complex Societies (Miroslav Karny)

A targeted (purposeful) choice among (sequence of) alternatives made under uncertainty and incomplete knowledge is labelled here more broadly than the common usage as decision making (DM). DM represents the vast majority of human activities underpins functioning of technical tools they use. Contemporary DM theories and their specializations as well as applications have a predominant tendency for centralized formulations, solutions and views. They care about welfare of societies (formed both by humans, technical tools and their mixed groups) and more and more ignore individual multiple objectives and limited abilities (time devoted to individual DM tasks, allocated memory and other deliberation resources, energy, finances, etc.) of DM participants.

It is desirable to seek for a conceptually unified theoretical, algorithmic and technical support of individual DM participants respecting the outlined circumstances. Importantly, the individual participants are living within a society so that they have to be individually equipped with the support, which allows them to seek for an acceptable compromise with neighbors, who are equally individually oriented and who have different objectives and resources but similar DM constraints.

There are strong indicators that the development of such a highly desirable support is possible but it requires a focused and coordinated multidisciplinary effort ranging from behavioral sciences, over mathematics, algorithmic and software art, etc. up to standardization ICT activities (almost any scientific branch may contribute). The foreseen impact on truly democratic society, cyber-physical systems, internet of things, smart cities, factories, homes etc. is immense.

We strongly believe that human brain, successful societies and enterprises do act in this locally harmonized bottom-up way, i.e. they respect individuality of “participants” and support the search for a compromise with neighbors. Thus, a creation of even an incomplete support with the described aim would deepen the understanding of such complex systems and would allow a better prediction of their emergent behaviors.

The above text states the overall problem as simply and generally as we managed. There are preliminary attempts and observations indicating that it is feasible and (pity to say?) strongly connected with the mentioned collection of buzzwords.

Translational research in mathematics (Ruben Sanchez-Garcia)

I would like to stress the need for translational research in mathematics. We have researchers thinking 'inside' and 'outside the box', but we also need mathematicians 'running between the boxes'.

This analogy goes beyond the pure/applied divide, as in fact some pure mathematicians are extremely good at making connections between different fields or applications, and some methods in applied maths can become routine and lack innovation.

I would still say that real-world applications, normally the starting point in applied research, remain a key motivation for ‘running between the boxes’. This can include tackling major challenges (‘hot topics’ if you wish), such as Big Data. I don’t think that motivation from overarching themes is necessarily a bad idea, in particular if it encourages translational research.

In my opinion, to realise translational mathematics we need a combined effort from experts to reach out and communicate effectively beyond their fields (this is a big issue in pure maths), and opportunities for interaction, including with potential end users. This is precisely the right context for large EU grants.

If I could add a final comment about Topological Data Analysis (TDA); I agree with previous comments that topology does not have all the answers, but it is an exciting new avenue and I am hoping it can be successfully integrated with statistical and machine learning techniques, and extended to more geometrical methods.

Mathematical Analysis of Algorithms (Bodo Manthey)

Algorithms are at the heart of many modern systems, ranging from cars to methods for analyzing big data. Algorithms predict our behavior and influence our decisions. However, many of the algorithms used are not well understood. Understanding modern algorithms is pivotal for two reasons: First, it is the only method for humans to stay in command over what happens in a world of algorithms. Second, only a deep and rigorous understanding of the current algorithm provides us with means to design better the algorithms. Because of this, mathematical analysis of algorithms is a key topic with impact far beyond the borders of mathematics and computer science.

Although this sounds rather applied, this is more fundamental research. We still lack the mathematics tools to understand many of the most successful algorithms. There are a few concepts that provide deeper and refined insights than pure (classical) worst-case analysis (such as smoothed analysis, semi-random models, fixed-parameter tractability, or approximation stability), but these are still fields in their infancy that require a lot more research to make them widely applicable.

Following other comments, this area of research does not need a few large grants, but more smaller grants would be preferable.

Roadmap for the Topdrim project in the DYM-CS programme - Dynamics of Multilevel Complex Systems (Jeff Johnson)

The DYM-CS research programme 2012-2016 has made a significant step forward in our understanding of the dynamics of multilevel complex systems, and created a very strong research European community in this vital area.

The focus of the call and the research programme was mathematics, and from the research done we can clearly identify areas of mathematics that should be supported by the European Commission.

The nature of multilevel complex systems means that most projects worked with large heterogeneous data sets and were concerned with the inverse problem of reconstructing the dynamics of multilevel systems from large heterogeneous data sets. A lesson from this is that future mathematical research into multilevel systems should be situated in real-world systems.

A great variety of systems investigated by the nine DYM-CS projects, but the most common areas of application were biology and social systems. In both cases large heterogeneous databases exemplifying Big Data were fundamental to the research. In many cases data sets are proprietary and made available because the owners see benefit in the research. Thus we expect increasingly research into multilevel systems will be conducted in the context of policy in the private and public sectors.

These considerations lead us to point to the following directions for European research into multilevel systems:

[1] Mathematics

The DYM-CS programme has identified the following loosely grouped and intersecting areas of mathematics as requiring further investment to support Europe’s R&D in ICT and other high added-value technologies:

- topology, algebraic topology, computation topology, topology of data, tensor algebra, persistent homology
- graph & network theory, complex networks, network dynamics, multigraphs, multiplex networks, hypernetworks, diffusive processes.

- statistical mechanics, multilevel Hierarchies of subsystems, self-organisation, dynamical organisation & evolution, game theory, evolutionary game theory. Cellular automata.

- dynamical systems and random dynamical system, symbolic and hypersymbolic dynamics

- intermittency and extreme events, cascades of failure. criticality, emergence, phase transitions, tipping points, multilevel dynamics. anticipatory & predictive actions of units,

- statistics, theory of statistical learning, deep learning, information geometry, information-driven emergence, information theory, persistent entropy, state space reduction, multiscale simulation

- category theory, topos theory

Request from the information and communication theory community (Marco Secondini)

The nonlinear Fourier transform can help to develop a new generation of optical fiber systems

Keywords: nonlinear Fourier transform, inverse scattering, optical fiber communications, nonlinear channels, integrable nonlinear equations.

Optical fiber communication systems form the backbone of the world’s communication infrastructure and provide for most of the global data traffic. However, the exponential growth of network traffic is pushing current technology towards its limits. In fact, the maximum rate at which information can be reliably transmitted through the optical fiber is limited by fiber nonlinearity. Apparently, a capacity crunch is looming on the future of optical networks.

Communication engineers are striving to solve this big issue, trying to devise novel transmission techniques that are suitable to achieve reliable communications over highly nonlinear channels. Recently, the nonlinear Fourier transform (NFT), also known as the inverse scattering transform, has been recognized as a fundamental tool to solve this problem. As the (ordinary) Fourier analysis underlies the great development of communication systems occurred during the second half of the twentieth century, the NFT could be the basis to develop a whole new theory of communications over nonlinear channels.

The NFT is expected to eventually replace its linear predecessor, which has emerged from wired and wireless transmission principles and reigned in the communication culture for over a century, in fiber-optic communications. However, though the mathematical theory of the NFT has been well elaborated, the adaptation of nonlinear structural elements in optical transmission systems in place of their linear counterparts and the corresponding “change of paradigm” require serious reconsideration of the communication system's architecture at every scale. For instance, we need fast NFT algorithms for the direct and inverse transforms (something similar to the FFT algorithm), we need to develop modulation and detection techniques that operate in the nonlinear spectral domain, we need to devise optoelectronic devices that can process the nonlinear spectrum, we need to understand how noise affect the nonlinear spectrum, and we need to extend many classical results of information and communication theory (e.g., the noisy channel coding theorem) to this new class of channels.

In order to achieve such an ambitious goal, mathematicians and engineers should work together in an interdisciplinary approach, addressing fundamental theoretical problems, developing new algorithms and techniques, and eventually experimentally demonstrating a new generation of optical fiber communication systems.

Maths for secrecy and privacy (Vincent Rijmen via Greet Bilsen)

Next to information-theoretic security and privacy, there is also a continuing interest in provable security for realistic environments. There is a growing demand for security products with a certified and significant security level. The higher certification levels require mathematical proofs.

In the area of secure implementations, there is a growing recognition for the fact that even schemes that are information-theoretically secure on paper, may be insecure in practice because common computation platforms leak information in the form of radiation, execution time, etc. In order to obtain real-world security, we need to adapt the mathematical security models in order to include implementation aspects.

Furthermore, in the Internet of Things, the secure generation of true randomness turns out the be a problem: cheap small devices operating on harvested power or batteries cannot be relied upon the generate true randomness in a hostile environment. Hence there will continue to be a strong need for cryptography based on methods relying on pseudo-randomness. Fields like number theory, coding theory and other areas of discrete mathematics can be used with succes here.

Infrastructure for Mathematics (Jiří Rákosník)

Many thanks to Anni Hellman for arranging this important discussion, mathematics is lucky to have such enlightened person in the EC structures. I endorse particularly those contributions presenting broader, fundamental views. Even though it might be out of scope of this discussion, I want to remind what has been mentioned already in the previous consultation: the need of a specific infrastructure for mathematics. The term „infrastructure“ appeared four times in the whole discussion, however, in different connections. Mathematics never becomes obsolete, what was true yesterday, remains true today. Mathematical knowledge is a magnificent ever growing edifice in which each building stone has important role. In certain sense, this is kind of big data requiring a specific attitude. We need an infrastructure which will enable not only create new knowledge but also preserve the existing one and provide a sophisticated access to it. This is currently dependant on commercial activities and on rather dispersed public initiatives like the database zbMATH, the EuDML, and number of networks and projects managing the mathematical knowledge. A reasonable support is needed to build upon these efforts and create a stable infrastructure for mathematics which will remain essentially under public control.

11. COMMENTS ON THE CONSULTATION SETTING

Comments on proposed areas (Vlada LIMIC)

Secrecy and privacy does not sound at all like a new area in mathematics. Cryptography and/or information, encoding theories would sound more appropriate.

I do not think that quantum computing is a good bet/priority, in view of current environmental, social and economical issues that Europe faces (these are not only EU but also global problems). Similar comment goes to Exascale and exabyte.

Indeed, I think that in the best interest of Europeans your list should be balanced with some environmentally and socially minded interests. The ongoing climate change preoccupies the whole planet (and Europe as its integral part). Mathematics has been used in studying biological/ecological systems, as well as pollution propagation. It can also be helpful in setting up plans for management of (diminished) resources, and eventually (temporarily or permanently) relocating people, habitats, goods etc...

In addition, extreme environmental and/or economic changes (and other factors), can lead to unpredictable social behaviors. Hopefully highly altruistic individual and community behavior will emerge, but one can never know for sure. Investing intellectual power into security will not help in the long run (in my humble opinion), but investing into mathematics of cooperative behavior might.

On a separate but related thought, the Horizon 2020 (and other of your programs) should consider detrimental changes that occurred in Mathematics since the advent of LaTeX. My personal viewpoint is expressed in the following essay

http://www.math.u-psud.fr/~limic/som/stateofmath.html

There are at least two dangers of EU introducing "hot" areas of mathematics, and funding those only:

(a) the non-serious (practically charlatan) mathematicians who "work" in hot areas [as pointed out in my essay, there is an ever increasing frequency of them, since doctoral theses are more and more frequently written by advisors, postdoctoral work by postdoc advisors, and further publications by collaborators] are valorized higher than extremely clever and serious scientists working in fundamental/traditional areas of mathematics; I would not wish to imply that EU funding would be granted to charlatans, it is just that whole areas of mathematics may get "glorified" (with their many charlatans included and promoted much higher than they would be in a regular area of mathematics),

(b) the core (fundamental) mathematicians throughout Europe could get practically deprived of funds; this has happened gradually in France - the local administration has been inducing more and more drastic measures, and today it does not sponsor even its best people doing fundamental mathematics - it tells them to look for funding on EU level, for example through Horizon 2020; it will take years if not decades to see the true effect of these changes, but personally I am afraid that the imbalance created will hurt (at least) French mathematics (hot areas included) in the long run.

In view of (a)-(b) I think that it would be wise if Horizon 2020

- considers funding programs that invite (and sponsor) highly acknowledged researchers in a fundamental (and non-hot) area of research to join excellent teams of scientists (on Horizon 2020 or similar quality projects) and through that learn new "hot" area of mathematics,

- demands each EU country to give proof of their care for their local mathematics community (sending a delegation on a one or two day visit to a top math department like my own would largely suffice), before releasing any funding to any particular mathematician from that country.

I hope that my somewhat lengthy comments and explanations will be useful to you. Please do not hesitate to contact me in need of further clarifications.

↳transversality (Frédéric Nataf)

Transversality is central to the usefulness of mathematics to an amazingly various number of areas. This good aspect is sometimes a drawback when it comes to apply for support. For instance in my field of expertise on high performance computing (HPC), our demands for access to supercomputers are not standard. We need access to very large machines in order to assess the efficiency of our new algorithms and their implementations. These algorithms are then used in various fields. But we need much less hours than production codes run to get insights into complex physical phenomena. Very often, when we ask for computing hours, these peculiarities harm our demands. Funding MSO activities per se, is thus very important as stressed by M.J. Esteban

↳ MSO, a general methodology for mathematical innovation (Maria Esteban)

To make sure that EU research funding takes into account the usefulness of Mathematics to solve real problems, in my opinion it is necessary to give general answers. I do not expect the EU to issue calls to fund this or that particular mathematical subarea, and in some sense I hope that the calls will be general enough to cover the variety of applications of Mathematics to the solution of a variety of important problems. Mathematics is a very transversal scientific field, which can address questions and needs in many technological areas. Mathematicians develop theories and tools that can be used in very varied situations. This transversality is extremely important, and not always well understood by the funding agencies. Defining calls for specific applications can be interesting, but it cannot be the only way to fund Science or even applied Science. I understand that the EU does not want to fund general scientific areas, because they are too vast and because it is difficult to assert that this is a good way to advance the development of our societies. But still so, it is necessary to understand how scientists, and in particular how mathematicians work. Some of them work for a particular area of application, but this is not the general situation. Most mathematicians develop tools that can be used in many different applications. It is for this reason that the posts that I most like in this consultation are concerned with the general field of mathematical MSO (mathematical Modeling, Simulation and Optimization). These three activities together define at best what a mathematician can do to solve real world and technological problems. This activity can be seen in areas as diverse as Energy, Transportation, Health, Biology, Ecology, Economy, new material science, etc. The same procedure, the same three steps, of course dealing with different situations, different equations and different methods. That is why I think that mathematical MSO could be a field that the EU could fund, that would include many mathematical subareas, since almost all mathematical fields can contribute in MSO activities, and which would help solving problems in societal and technological areas that are of interest to all of us.

Upstream and/or applicable? (Arne Smeets)

"The objective is to be upstream and applicable."

My feeling is that "or" would be a more apt choice of words than "and" in the above sentence. I understand the desire of the EC to see "bang for the buck"; if research is (or becomes?) applicable, great! But true progress in mathematics is often the result of individuals or small groups doing upstream research on whichever topic they feel like thinking about, and upstream research in pure mathematics which is not applicable right now may become the starting point for applicable research in twenty years are more.

So if the EC also wants Europe "to be first in modern science and innovation" in twenty years - and my guess is that this is indeed the EC's ambition - then it is important to realize that one cannot predict which theoretical advances will lead to applications in the future. This means that investing in *both* applicable and pure mathematical research is absolutely crucial; the latter will flourish if funding is distributed in small amounts among many individuals and small groups, who get the freedom to think about the topic of their choice, as other people have already pointed out on this page.

12. COMMENTS ON THE FUNDING MECHANISMS

The size of grants (Timothy Gowers)

I feel that your question has not been framed as well as it might. You restrict yourself to asking for recommendations of areas of mathematics that deserve special attention. But far more important, in my view, is the whole way that funding is allocated. The model is one where a few people get very large grants for projects that involve many people. This is a good model for many sciences, but it does not properly reflect the way that many mathematicians work -- in intense but small-scale collaborations. It would have a hugely beneficial effect if the money available to mathematicians were distributed to many more people in smaller amounts. For example, it would be great if one could apply for support for a single postdoc for three years. More generally, instead of forcing mathematicians to fit their projects artificially into a larger whole, you should trust us to judge for ourselves what we need and support that. (Of course, it goes without saying that proposals would still be peer reviewed and not all would be accepted.)

↳ *Also a problem in theoretical computer science (Pascal Koiran)*

I agree with this comment by Timothy Gowers and another related comment by Albert Cohen on the dangers of oversized grants. They apply very well to my own area of expertise (theoretical computer science).

↳ *Full endorsement (Pascal Weil)*

I completely agree with the comments in this thread, and in Albert Cohen's related thread, on the inadequacy of big funding for mathematics and much (if not all) of computer science. As aptly expressed by Tim Gowers and others, it would be scientifically much more productive to have more, more modest grants.

Let me add that the competition that appears in this consultation, is not a very happy sight. Of course, everybody thinks that their favorite field is to be distinguished, usually for excellent reasons. The fact is that more and more of mathematics finds (sometimes surprising) applications. And that excellence in mathematics in general, at the research level, is a very fruitful breeding ground for people who will end up doing research in other fields as well.

↳ *Full endorsement (Zdenek Dvorak)*

I agree completely. Large grants on a focused topic are a very inefficient way to support mathematics, and in particular run the risk of swamping the potential leading researchers with administrative and organizational tasks. Also, trying to shape the research in mathematics along the lines of "proposed new research areas" (a.k.a. chasing the buzzword of the day) is a terrible idea.

↳ *Full endorsement (Dan Kral)*

I agree fully. Very well pointed out! Instead of having few privileged "new research areas" and then forcing mathematicians to form large international consortia to chase the "buzzword of the day", the high quality research in mathematics would benefit much more from supporting thematically open grant competitions for individual grants rather than large-scale grants with dozens of postdocs and PhD students on few preselected topics. I think it is important that funding bodies are able to listen to the research community what type of grant schemes are viewed by the community to be the most efficient to foster the excellence in particular research areas (e.g., in mathematics, which discuss here) and reflect the community view in their grant schemes.

↳ projects or results? (Raphaele Herbin)

I totally agree with TImothy Gowers' comment. Another point which I would like to make is that grants are far too much focused on projects than on results. Good projects do not always involve good results, and results should be more taken into account in the funding process.

Moreover, writing and refereeing projects has become so time consuming that little time is left for real research, this is illustrated by the fact that ANR recently issued a funding programme just for the single purpose of writing an ERC...

and yet it would be not so difficult to evaluate researchers and teams by what they actually produced than by what they say they will, at least in mathematics.

Mathematics and H2020 (Enrico Scalas)

I am glad to see that several of my concerns on the role of mathematics in H2020 have already been addressed by other mathematicians and I am particularly glad to see that some of them are from the UK.

In particular, I liked the replies by JF Toland, Pavel Drabek and Timothy Gowers.

Within the Maths community, it is commonplace that there is virtually no space for mathematics in H2020, except for a few funding programs such as ERC grants (that give large amounts of money to a very small number of mathematicians), Marie Curie (essentially the only reasonable funding scheme for mathematicians) and, potentially, FET Open (now with success rates well below 3% and close to 1%).

Even if it may sound surprising to applied scientists and politicians, mathematicians have their own research agenda and this is often "orthogonal" to the "top-down" (i.e. scientific research themes decided by politicians) agenda of H2020. The mathematics of today will help inform the applications of tomorrow and the "top-down" approach to scientific research has never been a good idea. Moreover, we do not know what kind of mathematics will be needed in the future and, by funding only a tiny subset of mathematicians, we can eventually kill many promising and potentially useful research programs.

As mentioned by other colleagues, Mathematics will not die if not funded by the EU. However, there is a simple set of actions that could be immediately taken and could become the norm in the next framework program and would help in engaging mathematicians more in European research frameworks. I try to list them below.

1. ERC: Instead of awarding huge grants to a small number of individuals, the ERC could consider awarding individual grants on the order of 100,000 up to a maximum of 500,000 euros for for periods from two to five years. With the same amount of money used now, they could help instead of 1, up to 25 mathematicians. I am also expecting that this simple action would boost the outcome of ERC-funded research in Mathematics.

2. Other H2020 funding programs: When appropriate, the participation of one or two groups in departments of mathematics and/or statistics could become an element of positive

evaluation (similar to the participation of companies). For instance, groups of statisticians are particularly needed in applied proposals where large data sets will be collected (for rigorous design of experiments) and evaluated (for sound data analyses), but this is just an example among many I could give.

a few general remarks (Pavel Exner)

I feel that I should add a few words in my position of the European Mathematical Society President before this consultation closes. To begin with, I want to thank the European Commission, and in the first place to Anni Hellman personally for opening this forum at which ideas and concerns about the European support of mathematics can be voiced.

The discussion brought in many specific topics worth to be pursued, from the expected ones like the big data to some far less obvious. I will not comment on them, however, and I focus on a couple of general issues.

One recurrent topic in the discussion was the size of grants, in particular, in connection with the European Research Council. As a member of the ERC Founding Scientific Council, and the only mathematician there, I always argued there that the half a million lower limit is too big for any mathematician. I was overruled, however, and looking now on what people ask for and get, you find that the average size of a mathematical grant is 0.8 million in the junior categories, and 1.3 in the advanced one, below the ERC average but way above the said lower limit.

If somebody comes here to the conclusion that ERC is not a proper tool to finance mathematics I can only say that such a point of view is fundamentally wrong. One of the sources of the ERC strength comes from the fact that all fields of science, from mathematics to history of arts, speak the same voice and share a common understanding what an excellent research is. The ERC has to be supported and helped to resume the dynamics it had during the Seventh Framework Programme, possibly on a different and more permanent institutional basis which would better shield it from political influences and administrative hurdles. Only is such a way it can become in a long run an equal partner to the institutions such as the NSF in the United States.

What is painfully missing is a “mycelium” from which ERC results can grow, a palette of smaller funding opportunities for mathematicians with the potential to become visible personalities, temporary positions, small- and medium-scale collaborations, research conferences, etc. A part of that was once covered by the now defunct European Science Foundation the demise of which left a palpable hole. It may happen that one day this would be covered by the ERC in a way the NSF does it, but it is a long shot; one should not forget that the latter is almost ten times older than the former.

This concerns, in particular, the Marie Curie Sklodowska Programme which some of the participants of the discussion mentioned. It would certainly fit together with the ERC and other funding schemes to create a unique system covering the whole professional life of a mathematician, from the student age to the retirement, however, one serious obstacle is the fact that they belong to the portfolio of different Brussels “ministries”.

I have lauded Anni Hellman and her colleagues for opening this consultation which I see as a nontrivial act of wisdom. Sad to say, the democratic mechanism are not automatically supportive to long term vision projects; if you need an example, recall the last year attempt to cut the ERC and MSC budgets in the name of boosting the economic growth here and now. And it is our duty to repeat that strong science, and mathematics in particular, is crucial for the future of the European society.

The point is the atmosphere in the society which in a sense could be called schizophrenic.

Many of our fellow citizens enjoy results of scientific research at every angle and at the same time they despise the tools. It would socially destructive to acknowledge publicly that you fail to understand basic rules of grammar, but we see scores of people saying with a pride: “I have never mastered a simple proportion and look how big I became”. Well, you are not surprised when it comes from sports or pop music stars, but sometimes you hear such things from people who feature themselves as political philosophers.

With all that in mind, the European mathematical community is prepared for a permanent dialogue with the European politicians, being convinced that a well devised support of science, and mathematics in particular, is the best way to the prosperous society we all wish at our continent.

↳Grant sizes, diversity in support (Dan Kral)

I fully share the views expressed in the reply of Pavel Exner to this post. A "big grant" may mean something else for each of us. When I joined the debate started by the post of Timothy Gowers, I had in mind grants in the order of many millions EUR for large consortia, which I indeed believe not to be appropriate for mathematics (but they might be appropriate in other areas). However, 100K EUR for a 2-year grant is clearly insufficient if that grant should support even a small group consisting of a single postdoc and a single PhD student. If mathematicians should have time for doing research rather than applying all the time to get funding for a postdoc, a student, etc. separately, the amounts mentioned in Pavel Exner's reply seem in the right range to support the PI together with say 2 postdocs and 2-3 PhD students for 5 years. I believe that the idea is that an ERC grant will be the main source of funding for the PI during the grant period to allow the PI to focus on research (similar for example to the RS URF scheme in the UK), so the PI does not need to apply for any additional funding during the grant period. Marie Curie is a good way of supporting young mathematicians - still, I have seen many strong mathematicians accepting an offer for a postdoc in US/Canada rather than waiting for the results of Marie Curie and risking the offer to be gone. Having postdoc funding on the ERC grants allows European scientists to compete on the global market for top postdocs.

I understand that the budget is and will always be limited. I believe it is better for mathematics (and in fact, any science) in the long term to support top research in a sufficient way (and I mean sufficient, not irrationally large, e.g., multipartner international consortia in mathematics) rather than to support everybody little (in a way not sufficient to do too much). For example, I think that most of UK-based mathematicians would agree that DTCs of roughly half the current size would be much more appropriate for mathematics but DTCs five times smaller might be too small. Similarly, I can imagine ERC grant sizes in mathematics to be 1/2 or 2/3 of the current sizes and still be sufficient for their purpose (but the amounts should be such that the grants can still serve their purpose). Having said all these, there should always be a large amount of diversity in the topics of research supported to keep all areas alive. Because of this, I fear that identifying key research themes is dangerous as the areas that will not be identified might lack funding. So, not surprisingly we can see posts from all different areas of mathematics in this discussion.

Why do we need EU funding programmes? (Jacek Miękisz)

I agree with my predecessors that huge projects with many investigators are not optimal for mathematical research. The best applications arise when we are focused on fundamental research done in small groups. So why do we need EU programmes? I think they are indispensable to foster interdisciplinary research. National science and scientific careers are unfortunately compartmentalized. Moreover, you observe that in some places, branches of science which owe their birth to clever observations of real-life phenomena, like for example dynamical systems, are now investigated in closed self-centered groups. Their members are no longer interested in problems outside mathematics. They created enough interesting inside problems to go on in a conservative way. The other so to speak extreme consists of scientists who use buzz words such as for example chaos theory and complex systems to attract attention and funding. They try to create new paradigms (social networks) and work within them. Unfortunately very often they do not have much contacts with mathematicians and physicists who studied chaos theory and complex systems for decades. One goals of interdisciplinary EU programmes would be to make such conservative and paradigm groups to meet and work on some common tasks.

13. THEORY OF EVOLVING SYSTEMS (TES)

Theory of Evolving Systems (Igor Schagaev)

The big picture

The human world evolves and progresses by applying knowledge derived from observations of and familiarity with repeatable events and phenomena of nature. Our perceptions, understanding and ability to model reality enables us to develop policies, processes and products required - generally speaking our goal is either in order to control (or exploit) the behaviour of natural phenomena, or creating of human-made objects with required properties.

We believe that fundamental distinction and difference between living processes and dead matter is in evolvability and its essential component or ingredient called recoverability.

Thus we simply must redo our systems and change technologies pursuing principles of self-healing, making our systems easier to adjust, implement them with “zero cost maintenance” approach.

While an amoeba has sufficient resources to use and to protect itself from the destructive energy of the environment or an impact, it will recover and continue existence - the amoeba exhibits redundancy and intrinsic ability to apply it in order to survive. Our designs have no such property…

Living objects differ from man-made systems in terms of the time required for recovery and the use of available redundancy.

Turning from natural world back to human made developments it becomes clear that evolving character of our knowledge is the key for our progress in all our research domains, including computer science, mathematics, aeronautics, economics, education.

Surprisingly, there were no any systematic analysis of fundamental features of evolvability of knowledge and in practical domain recoverability and reconfigurability mechanisms of devices, systems and other human race creations.

New math models will be required, knowledge development and delivery require evolving property so far absent; the same is true for computer science, medicine, military systems, economic systems, aerospace.

Thus theory of evolving systems is required.

Some developments created with TES are known:

•Computer systems with the property of evolvability are known (https://www.academia.edu/7685575/Evolvingsystems-WorldComp2014), including implementation aspects (see monographs of Castano V., Schagaev and Schagaev I. Kaegi T.: http://www.it-acs.co.uk)

•Concept of evolving system and underlying theory attracted US teams from NASA and MIT.

•In education only one our paper about a method of evolving curriculum design and delivery

() scored in ACM 6500+ downloads:

•New math models that enable to account evolving property in a flexible way in our developments, but with no support and integration of European expertise available.

•New methods of self-adjusting and evolving system control was patented in UK (http://www.it-acs.co.uk/files/GB2448351B.pdf)

Development of TES (theory of evolving systems) and application in mentioned above domains provide:

-computer systems 10 at power 5 more reliable than now,

-duplicated performance;

-10-fold less power consumption.

In aerospace: performance, reliability and energy efficiency of aircrafts will be increased ten-fold.

In education: Cost of education will be reduced while education quality, availability and accessibility will be increased at order of magnitude.

Economic and prognostic analysis of complex systems will be improved in reliability and speed making possible to predict vulnerabilities and providing solutions in real time of processes.

Finally, a new computing paradigm will be investigated and proposed with full size working prototype making existing hardware technologies applied much more efficient than before.

The work needed:

Cluster of projects in domains mentioned under one logic umbrella of TES:

-Elementary prototypes of hardware main schemes are available; system software for this hardware is required. models of prognostic of complex systems based on new math is required;

-Econometric tool to help project engineering to become a quantitative science will be required as a stand-alone research.

-Educational segment simply must be re-equipped by new concept of evolving knowledge formation, aggregation, delivery and assessment (see EKADA link above).;

-In aerospace a new framework of active system control (as a prototype) must be developed for future aircrafts and delivered for testing.

The opportunity:

Some prototypes of hardware, system, software, econometric models, and educational models are developed. Aggregation, consolidation and distribution of the unified approach and results will return Europe to ICT and knowledge development positions in the world. Expertise of Europe should be exploited with maximum efficient. So far it is not a case.

Few examples such as IoT, AI, or super-brain projects created more questions than solutions.

Role of TES and implementation of key concepts can help EC itself to become manageable and work much more efficient.

Steps already made towards TES (Leonid Perlovsky)

TES opens future in many directions and I. Shagaev described them well. Significant steps toward TES have been already made.

1. Evolution is accumulation of knowledge.

what is knowledge? - it is a collection of representations inside the system about the outside world, and self.

The best evolving system is a human being, more specifically - our brain-mind. And the theory of TES and knowledge accumulation have started with the theory of mind and cognition.

Having adequate representations is required for survival, therefore we are driven toward knowledge accumulation and improvement by the knowledge instinct, KI (or primary drive).

2. Mathematical model of evolution of the mind.

Mathematical models of KI (learning, thinking, mind...) have been attempted since the 1950s. A fundamental difficulty has been computational complexity, CC. It has been related to the Gödel difficulties of logic. This difficulty has been recently overcame by dynamic logic DL.

DL is a process logic. DL processes are "from vague to crisp," from vague representations, plans, etc. to crisp ones. DL explains how classical logic appears in the mind from vague and uncertain brain. DL overcame CC and made possible solving problems that have been unsolvable for decades, including modeling of the mind, and many engineering problems.

3. Modeling of the mind : theory confirmed by experiments

-perception and cognition

-emotions

-hierarchy of perception, cognition, and emotions

-including these mechanisms at the top of the hierarchy: beauty, sublime, meaning

-the beautiful are emotions experienced when a step is made toward the highest meaning (the meaning of life)

-cognitive functions of aesthetic emotions

-music, its cognitive functions, its origin and evolution

-language, its difference from cognition

-language and cognition interaction, their functions in thinking

-the role of conscious and unconscious in thinking

These and other mechanisms of the mind have been mathematically modeled, a number of unexpected predictions have been made. Many of these predictions have been confirmed in experiments.

4. Engineering applications

The theory described above has been applied to many areas of engineering. Dozens of problems unsolvable for decades have been solved.

5. KI and evolution

The idea of KI and its mathematical model, DL, is being extended toward biological evolution from modeling DNA and protein mechanisms to modeling societies and cultures, it would take many pages just to list these new areas of research and engineering being now developed.

6. Publications.

-more than 500 publications

-journal "Physics of Life Reviews" IF = 9.5, Thompson Reuter rating #4 in the world (in biophysics)

-A number of open source publications, search for L. Perlovsky.

Evolving Systems are not about Resilience Only (Boris Gorbis)

My dear friends:I hesitate to add my voice to this august discussion, but I couldn't help it in the 5th grade and I am certainly too old to hold back now. By the way, due to inflation the cost of such interference has risen from the old "putting my 5 cents in" to "a dollar". Here is my dollar's worth of thoughts.

1. An 'evolving system' is first and foremost an 'informational phenomenon' (IP).

2. Aside from its capacity to magnificently acquire, relate, manipulate and produce data, it has a major limitation compared to any human when seen as an IP.

3.A human IP is capable of seamlessly transforming any of the 3 types of Information (at least) into any other type.

4.The three types of Information are: (1) Material (MI) data, input, output, etc.; (2) Physical (PI) causational - leading to any change in status, and (3) Eidetic (EI) cognitive-emotional we are well familiar with.

{N.B. I also postulate existence of a fourth type - Deatic Information - an ideal information, but let's keep this out for now}

5.The fundamental aspect of the three types is the no-hassle morphing of one type into another thus creating a number of outcome trees (scenarios). Call me a bad name and I slap you back and you can take this open-ended transformation sequence of MI to EI to PI to EI anywhere you want.

What is my concern and objection?

The focal point in EC DG festivities is to enable only two morphings, that is MI to PI and PI to MI. I may be unfamiliar with efforts to make the systems "feel". Given my outsider status, I should be forgiven for taking your time but I suspect that these efforts, if any, bypass the key role of Emotional-Cognitive Information within the informational triade.

Let me try a slightly different track.

6.Human behavior, seen as an informational output, is actually a result of ad hock negotiations by and between three decision-making scales - A, B and C.7. An A scale is the old familiar "Right-Wrong" binary choice: 8. A B scale is the one we use but dislike in others:

"Want-Do not Want".9. The C scale is the fuzzy one - "Good - Bad";10. Regardless of individual criteria the outcome of any situation of choice always depends on (at least) two sources of Eidetic information provided by the B and/or C scale.11. In human IF's the end result of 'negotiation' is in fact a product of multiple transformations of EI into PI or MI and back and sideways.

And I am not even adding the "Presence of Others" dimensions to this discussion.

My point? The fundamental anatomy of 'evolving systems' is still a one-scale binary foundation. Without modelling an evolving system capacity to operate with Eidetic information, no system will 'evolve' benignly. No matter how many 'brakes' are provided, it would remain unable to transform data into good or bad.

Presently such an endeavor lacks proper conceptual cellular anatomy (like units of good or units of want) but failure to start with looking for one will inevitably lead to HAL-like situations and faster than we think.

One possible approach might be to design a Universal Eidetic Matrix with scalable modular array linked to universal Relational Spheres, but I am running too fast too far.