Complex systems science: expert consultation report

Other

Complex Systems Science

Expert Consultation Report

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Executive Summary

A new programme of research in Complex Systems Science must be initiated by FET

The science of complex systems (CS) is essential to establish rigorous scientific principles on which to develop the future ICT systems that are critical to the well-being, safety and prosperity of Europe and its citizens. As the "ICT incubator and pathfinder for new ideas and themes for long-term research in the area of information and communication technologies" FET must initiate a significant new programme of research in complex systems science to underpin research and development in ICT. Complex Systems Science is a "blue sky" research laboratory for R&D in ICT and their applications. In July 2009, ASSYST was given a set of probing questions concerning FET funding for ICT-related complex systems research. This document is based on the CS community's response.

Complex systems research has made considerable progress and is delivering new science

Since FET began supporting CS research, considerable progress has been made. Building on previous understanding of concepts such as emergence from interactions, far-from-equilibrium systems, border of chaos and self-organised criticality, recent CS research is now delivering rigorous theory through methods of statistical physics, network theory, and computer simulation. CS research increasingly demands high-throughput data streams and new ICT-based methods of observing and reconstructing, *i.e. modelling*, the dynamics from those data in areas as diverse as embryogenesis, neuroscience, transport, epidemics, linguistics, meteorology, and robotics. CS research is also beginning to address the problem of *engineering* robust systems of systems of systems that can adapt to changing environments, including the perplexing problem that ICT systems are too often fragile and non-adaptive.

Recommendation: A Programme of Research in Complex Systems Science to Support ICT

Fundamental theory in Complex Systems Science is needed, but this can only be achieved through real-world applications involving large, heterogeneous, and messy data sets, including people and organisations. A long-term vision is needed. Realistic targets can be set. Fundamental research can be ensured by requiring that teams include mathematicians, computer scientists, physicists and computational social scientists.

One research priority is to develop a formalism for multilevel systems of systems of systems, applicable to all areas including biology, economics, security, transportation, robotics, health, agriculture, ecology, and climate change. Another related research priority is a scientific perspective on the integration of the new science with policy and its implementation, including ethical problems related to privacy and equality.

A further priority is the need for education in complex systems science. Conventional education continues to be domain-dominated, producing scientists who are for the most part still lacking fundamental knowledge in core areas of mathematics, computation, statistical physics, and social systems. Therefore:

1. We recommend that FET fund a new programme of work in complex systems science as essential research for progress in the development of new kinds of ICT systems.
2. We have identified the dynamics of multilevel systems as *the* area in complex systems science requiring a major paradigm shift, beyond which significant scientific progress cannot be made.

3. We propose a call requiring: fundamental research in complex systems science; new mathematical and computational formalisms to be developed; involving a large ‘guinea pig’ organisation; research into policy and its meta-level information dynamics; and that all research staff have interdisciplinary knowledge through an education programme.

Tangible outcomes, potential users of the new science, its impact and measures of success

Users include (i) the private and public sectors using ICT to manage complex systems and (ii) researchers in ICT, CSS, and all complex domains. The *tangible output* of a call will be new knowledge on the nature of complex systems in general, new knowledge of the particular complex system(s) studied, and new knowledge of the fundamental role played by ICT in the research and implementation to create real systems addressing real-world problems. The *impact* of the call will be seen through new high added-value opportunities in the public and private sectors, new high added-value ICT technologies, and new high added-value science to support innovation in ICT research and development. The measure of success will be through the delivery of these high added-value outcomes, and new science to better understand failures.

1. Introduction

This document makes the case for FET to make a major new investment in the science of complex systems which, it will be argued, is essential to establish rigorous scientific principles on which to develop the future ICT systems that are critical to the well-being, safety and prosperity of Europe and its citizens.

In July 2009 the ASSYST project was provided with a set of probing questions to solicit the views of the complex systems community concerning FET funding for ICT-related complex systems research. Many scientists were sent the questions (Appendix A).

The most important view to come from the questionnaire is that Complex Systems Science and ICT are tightly conjoined. *ICT is fundamental to CSS, and CSS is fundamental to ICT*. The CS community believes (perhaps not surprisingly) that Complex Systems Science is essential to ICT. Conversely, the example of Pervasive Adaptation Research (PERADA) shows that CSS concepts are already used in ICT.

Assuming that FET will support CSS research as part of its mission to support "blue sky" ICT research and development, how best can this be done?

The questionnaire shows that most people in the CS community believe that there is a need for new theory, but that this theory will only emerge from applications. In this respect it must be noted that ICT *involve people*. They are complex socio-technical systems, and ICT-oriented research must include the human dimension. This includes robotics that involves humans interacting with robots, or robots being used to investigate emergent interaction dynamics within communities of autonomous intelligent embodied agents as simplified models of human beings.

For many respondents 'theory' involves mathematics and computation in one way or another. There are mathematical models that use theory such as the properties of Lyapunov exponents in dynamical systems, and other models that are more descriptive or computational, *e.g.* networks displayed as pictures. Either way, mathematics and computers share the same foundations and, arguably, every data structure is an object capable of being analysed by mathematical and computational methods.

Real-world applications will generally involve large heterogeneous data sets that are 'messy' by being partly incomplete, partly incorrect, and partly incompatible. Often these data sets will pre-exist in ICT systems (*e.g.* census data, one-off survey data, digital maps, customer data, other legacy data). Finding them and transforming them into useful information is a major challenge for both CSS and ICT.

2. CSS is essential to ICT research & development.

This proposition is at the heart of our proposal that FET establish a long-term programme of research in CS in order to support its ICT mission. We make the argument as follows.

ICT systems are complex, at every scale.

At the microlevel of a program running on a single processor enormous complexity can emerge from interactions between the data and the dynamics of the computation. Even at this level there are major ICT problems of robustness, adaptation, self-configuration and self-repair.

At higher levels of aggregation wired and wireless networks create systems of the immense complexity exemplified by the Internet.

ICT systems are *socio-technical systems*, with the behaviour of the whole emerging from and coevolving with the behaviour of individuals, groups, and the mass of people worldwide.

As an example, the Internet based global economy consists of an exceedingly large and ever increasing number of agents (suppliers, customers, investors, lenders, borrowers and middlemen) engaged in rich interaction at great speed and has all characteristics of a complex system:

1. There is no centralized control - its global behaviour emerges from the interaction of local behaviours of agents and is deterministically unpredictable, *e.g.* the system exhibits cyclic increases and decreases of economic output but we can't predict the timing or severity of the next slowdown
2. Contrary to conventional economic theory, the system is frequently disturbed and is permanently "far from equilibrium" - it has no time to return to equilibrium
3. The system is nonlinear - small disturbances may be amplified and cause very large disruptions
4. The system is capable of self-organising and changing its dynamics.
5. In common with all social systems, its constituent agents have declared and undeclared objectives and the propensity to pursue these objectives in competition or co-operation with other agents

Current ICT cannot adequately support businesses, administrations and social institutions that operate in such a complex environment because it is based on conventional software, which is a rigid structure of algorithms calling algorithms.

There is an urgent need to develop fundamentals of a new ICT that is *adaptable* and is capable of providing *Autonomous Real-Time Management of Business, Administrative and Social Processes.*

This is possible only by designing complexity into ICT systems.

Future ICT will be capable of effectively supporting human activities in complex economic, political and social environments only if it is based on key concepts of Complexity Science, such as:

1. emergence
2. self-organization
3. adaptation to changes in environment
4. self-repair
5. behaviour "far from equilibrium"
6. sensitivity to initial conditions
7. path dependent dynamics
8. co-evolution
9. multilevel dynamics

For example, Future ICT must be based on software that is capable of

1 Creating "emergent intelligence", *i.e.* intelligence that is not present in any of the constituent components and yet it emerges from the interaction of these components
2 Autonomously self-organizing in response to an unpredictable disturbance
3 Operating as required under conditions of frequent unpredictable disturbances, which prevent it from returning to an equilibrium
4 Irreversibly co-evolving with its business environment, as business strategy, policy, processes and structures change with time

Many of the 'bottleneck problems' in ICT come from their complex systems properties. For example:

1. **ICT systems fail when new parts are added**: they are non-linear and non-incremental, and the addition of new hardware and software can lead to unpredicted and undesirable behaviours.
2. **ICT system are not adaptive to changing environments:** ICT systems do not adapt to changing communities of users with changing requirement. No matter how good the original specification, in time ICT systems become less well adapted to users and user needs. Upgrading ICT systems triggers Type-1 problems.
3. **ICT systems do not self-repair.** Generally ICT systems have little or no ability to self repair.
4. **Technical ICT systems are syntactic with limited success in semantics (*e.g.* semantic web).** Search remains highly limited and non-humanlike. Human cognitive processes for recognising patterns and generalising them into models (right-left brain activity) are not well understood and not implemented in computers. We remain a long way from *The Ultimate Google* identified by the FET *Evergrow* project.
5. **Automated pattern recognition, *e.g.* machine vision, remains very limited.** Despite being the key to many very high-added value ICT application, automated pattern recognition remains primitive. For example, current machine vision systems are unable to evolve from one application (*e.g.* recognising car number plates) to other applications (*e.g.* recognising rocks on Mars), using mostly logical and arithmetic operations with the programmers' semantic model of the system implicitly 'hard wired' into its hardware and software.
6. **ICT systems generally have poor Human-Computer Interfaces:** ICT systems are socio-technical systems, with the human and technical aspects massively intertwined. Because of Type-4 problems, the massive amounts of data available cannot be synthesised and presented to users in useful ways. 'Visual analytics', the use of computer graphics to present information in ways that human can see complex patterns remains a pragmatic second best.

These and other bottleneck problems show that research in CSS is essential for Blue Sky R&D in ICT. At the same time complex systems science is ICT-enabled, and ICT is essential for CSS.

3. Possible FET calls for CSS and IT

3.1. Towards a Paradigm Shift in Complex Systems Science for ICT

Through FET support Europe has become a world leader in complex systems research, with a well networked community of about two thousand researchers that continues to grow each year. This community has its own international conference, the European Conference on Complex Systems that showcases cutting edge research: ECCS'04 (Torino), ECCS'05 (Paris), ECCS'06 (Oxford), ECCS'07 (Dresden), ECCS'08 (Jerusalem), ECCS'09 (Warwick), ECCS'10 (Lisbon) and ECCS'11 (Vienna).

This research spanned many areas of applications. Furthermore there have been national programmes of complex systems research, especially by EPSRC in the UK and CNRS in France. There are many masters and PhD programmes in complex systems science springing up across Europe.

All this is evidence of the science maturing, and indeed some areas of complex systems research can be seen as 'normal science', in the sense of Thomas Kuhn. For applications of the new science this is a very good thing. For example, Agent-Based Modelling (ABM) has developed to the extent that it the basis of large high added-value commercial applications:

> During the ten-year period, 1999–2009, Magenta Corporation and Knowledge Genesis have developed, based on principles and methods discovered and articulated by Professor Rzevski, a very large number of complexity management systems based on Complexity Science concepts and principles, which are in commercial use. All these systems have one feature in common – they have succeeded in solving problems, which were considered too complex for generally available conventional methods and tools.
>
> Examples of selected successfully developed and implemented complexity management systems, include:
>
> 1. Managing in real time a fleet of 2,000 taxis, for a transportation company in London
> 2. Managing in real time a large fleet of car rentals, for one of the largest car rental operators in Europe
> 3. Managing in real time 10% of the world capacity of crude oil sea-going tankers, for a tanker management company in London
> 4. Resolving clashes in aircraft wing design for the largest commercial airliner in Europe
> 5. Real-time scheduling of a large fleet of trucks transporting parcels across the UK
> 6. Agent-based simulator for modelling the airport and in-flight, RFID-based, catering supply chain, luggage handling processes, and passenger processing, for a research consortium in Germany
> 7. Selecting relevant abstracts for a research team using agent-based semantic search, for a genome mapping laboratory in the USA
> 8. Discovering rules and patterns in data using agent-based dynamic data mining technology, for a logistics company in the UK
> 9. Managing social benefits for citizens with electronic id cards for a large region in Russia
>
> http://www.assystcomplexity.eu/success.jsp

This example illustrates the close relationship between complex systems science and ICT. Inspired by complex systems science, the technology of ABM is ICT-enabled and the systems delivering this technology involve extensive communications and software engineering.

Other aspects of complex systems science are becoming common. These include the use of networks to investigate large scale interactions, and their statistical analysis. It is now well known that many networks have small-world properties supporting rapid transmission of change, and that some networks may evolve through mechanisms such as preferential attachment.

Recent work by Szella and Thurner illustrates progress in the application of these methods, and that the science is progressing towards the discovery of empirical laws:

> Quantification of human group-behavior has so far defied an empirical, falsifiable approach. This is due to tremendous difficulties in data acquisition of social systems. Massive multiplayer online games (MMOG) provide a fascinating new way of observing hundreds of thousands of simultaneously socially interacting individuals engaged in virtual economic activities. We have compiled a data set consisting of practically all actions of all players over a period of three years from a MMOG played by 300,000 people. This large-scale data set of a socio-economic unit contains all social and economic data from a single and coherent source. Players have to generate a virtual income through economic activities to `survive' and are typically engaged in a multitude of social activities offered within the game. Our analysis of high-frequency log files focuses on three types of social networks, and tests a series of social-dynamics hypotheses. In particular we study the structure and dynamics of friend-, enemy- and communication networks. We find striking differences in topological structure between positive (friend) and negative (enemy) tie networks. All networks confirm the recently observed phenomenon of network densification. We propose two approximate social laws in communication networks, the first expressing betweenness centrality as the inverse square of the overlap, the second relating communication strength to the cube of the overlap. These empirical laws provide strong quantitative evidence for the Weak ties hypothesis of Granovetter. Further, the analysis of triad significance profiles validates well-established assertions from social balance theory. We find overrepresentation (underrepresentation) of complete (incomplete) triads in networks of positive ties, and vice versa for networks of negative ties. Empirical transition probabilities between triad classes provide evidence for triadic closure with extraordinarily high precision. For the first time we provide empirical results for large-scale networks of negative social ties. Whenever possible we compare our findings with data from non-virtual human groups and provide further evidence that online game communities serve as a valid model for a wide class of human societies. With this setup we demonstrate the feasibility for establishing a `socio-economic laboratory' which allows to operate at levels of precision approaching those of the natural sciences.
>
> Abstract from ‘Measuring social dynamics in a massive multiplayer online game’ Michael Szella, Stefan Thurner (arXiv:0911.1084v1 [physics.soc-ph]).

Such laws, based on rigorous scientific principles, mark a major step forward for the science of social systems and again illustrate the combined power of the methods of complex systems science and ICT, and their inevitable co-development. The discovery of general laws of social networks will inform the conception and design of new ICT systems, while ICT is essential to gather and process the data required to establish such laws.

The paradigm of investigating large social networks is now well established, especially where those networks involve digital traffic that can be captured automatically using the ICT infrastructure of the internet and wireless communications systems. The newly launched COSI-ICT programme is likely to give Europe cutting edge expertise in this area for applications in industry and the private sectors.

Given these successes, what then are the challenges for complex systems science and ICT? Where are the frontiers and what are the challenges that require major scientific paradigm shifts?

Previously, we identified systems that have not yet yielded to the new science of complex systems or intense research in computer science:

1. ICT systems fail when new parts are added.
2. ICT system are not adaptive to changing environments.
3. ICT systems do not self-repair.
4. Technical ICT systems are syntactic with limited success in semantics.
5. Automated pattern recognition, *e.g.* machine vision, remains very limited.
6. ICT systems generally have poor Human-Computer Interfaces.

This is because today's ICT systems are qualitatively fundamentally different from natural complex systems. Engineered ICT products are generally made of a number of unique, heterogeneous components assembled in a precise and complicated way, and are usually intended to work deterministically following the specifications given by the designers. By contrast, self-organisation in natural complex systems (physical, biological, ecological, social) often sprouts from the repetition of identical or heterogeneous agents and stochastic dynamics. As a result, the latter create spontaneous patterns (spots, stripes, waves, trails, clusters, hubs, etc.) that are mostly random or shaped by external constraints, but never truly exhibit an *intrinsic architecture* like the former do.

There are, however, two major exceptions that blur this dichotomy and show a possible path toward tomorrow's (a) CSS-inspired ICT systems and (b) ICT-guided CSS:

1. On the one hand biological development, a major category of natural complex systems, strikingly demonstrates the possibility of *combining pure self-organisation and elaborate architectures*. Multicellular organisms are composed of segments and parts arranged in specific ways, yet they entirely self-assemble in a decentralised fashion, under the guidance of genetic and epigenetic information spontaneously evolved over millions of years and stored in every cell. In other words, they are unique examples of *programmable self-organisation*—a concept not sufficiently explored so far, neither in CSS (for the "programmable" part), nor in traditional ICT engineering (for the "self-organisation" part).
2. Conversely, *large-scale ICT systems already exhibit complex systems effects* (albeit mostly unwanted and uncontrolled). Segmentation and distribution of large computing systems over a myriad of smaller and relatively simpler components has become both a growing need and an inevitable reality in many domains of computer science & engineering, AI, and robotics. Faced with an explosion in size and complexity of computing systems at all scales, whether hardware (integrated parts), software (program modules), or networks (applications and users), engineers are gradually led, more or less willingly, to give up rigidly designing systems in every detail and only "meta-design" them, *i.e.* focus on generic conditions allowing their endogenous growth, function, and evolution.

Thus, CSS already includes natural systems that seemingly exhibit all the attributes of ICT systems, while ICT systems are already becoming natural objects of study for CSS. Both of these cross-boundary examples point to a new field of research that would explore the design and implementation of autonomous systems capable of developing complex functional architectures with little or no central planning.

In summary, a solution to any of the above bottleneck problems (1. to 6.) in ICT would have enormous commercial and social value. All of them have been subject to intense research over many decades. Clearly, to solve them requires new thinking. From the perspective of complex systems science it is striking that these problems all have the same characteristic:

> ***they are systems of systems of systems, and we have no scientific formalism for representing the bottom-up and top-down dynamics of multilevel systems from micro-levels to macro-levels through meso-levels.***

This scientific deficit manifests itself across the sciences. In biology there is no formalism able to integrate the dynamics of cells with the dynamics of organs or the dynamics of the whole body. Instead we have many partial models that fit together, at best, descriptively. In geography and environmental planning we have no formalism that can integrate the choices and behaviour of individuals at the microlevel with the emergence of cities across the globe. In social and political science we have no formalism that can explain why the values and beliefs of individuals aggregate into mutually destructive policies at national level, as illustrated by the recent collective inability of nations to agree a strategy on climate change. And in ICT we have no formalism that can integrate individual computers and their users at the microlevel into the macrolevel phenomenon of the internet.

For all these systems, complex systems science gives reasons why their behaviours are hard to predict. Conventional science assumes that subsystems can be isolated, but complex systems science shows that they may coupled by *weak links*. This makes subsystems with *ill-defined boundaries* that are hard to identify and model. These subsystems *evolve* and *coevolve* in ways that can only be predicted by modelling their *interactions*. These interactions do not just occur at particular levels of representation, *bottom-up* dynamics can cause macroscopic changes, and *top-down* macroscopic dynamics can cause microscopic changes. It is well known that discrete microlevel interactions aggregate into continuous (or not) mappings at high levels, but there is no formalism for representing this in a way that coherently integrates the micro and macro through the meso.

> ***Creating a formalism for multilevel systems of systems of systems and demonstrating its applicability is on the critical path for science. It is necessary if not sufficient to make progress in many domains. It requires an essential paradigm shift for complex systems science and ICT.***

3.2. Characterising a new FET call in CSS for ICT

The respondents to the ASSYST-FET questionnaire unanimously believe that any call should require projects to combine both theoretical and applied research, the general feeling being that the former must emerge from the latter, while the latter requires theoretical rigour to achieve useful scientific outcomes. Some respondents warned against focussing on developing tools.

The CS community believes that a call can be framed to ensure an appropriate balance between theory and application. Generally theory is expected to be developed as formal models, often expressed in mathematical terms that interface well to ICT implementations. Applications are generally expected to involve very large data sets in complex public and private sector systems. The contribution to ICT is in finding new approaches to make it effective in the context of real world complex systems, rather than it looking inward towards its own inherent complexity using its own traditions.

In directing the initiative, priority application areas could include any complex system that presents clearly defined challenges to ICT. The basis for selecting ICT relevance is where there are clearly problems in applying ICT for reasons that can be articulated using the concepts and methods of CSS, and where innovations in CSS can be expected to produce radical new kinds of ICT.

The follow <u>non-exhaustive</u> list illustrates areas that have been suggested:

cloud computing
real-time supply chains
pattern discovery
semantic search
distributed self-organising systems, *e.g.* peer-to-peer and social computing platforms
self-assembling communications systems
adaptive machine vision
using geographic data
linking expert – non-expert knowledge
design and management of complex systems
meta-design of ICT systems
self organising knowledge systems
ICT in large private and public organisations
ICT in climate change

It is to be expected that consortia researching such areas would have demonstrable expertise in CSS, ICT, mathematics, statistical physics and social science, and possibly some other specialisms.

Any FET programme should have a coordination action responsible for making the programme work as a whole, and making contacts with other communities.

It is generally felt that the users of this research will be:

people working with complex systems in the public sector
people working with complex systems in the private sector
ICT companies – IBM, Google, Siemens, BT, Nokia, etc
complex systems scientists and practitioners
ICT scientists and practitioners
scientists working in particular domains.

The complex systems community does not believe that a call should be made on the basis of theory/fundamentals alone. It believes that the research should combine theory with applications.

The tangible output and impact of a call combing theory and fundamentals with applications will be high added value applications in the private and public sectors, solving real problems and establishing new CSS methods and new ICT to solve other problems of this type.

Other tangible outcomes will include an increase in scientific and technical knowledge in CSS and ICT.

The measure of success of the research in CSS is

1. the delivery of high-added value applications of ICT as judged by the users in the public or private sector, and
2. new science and understanding as to why these benefits were or were not delivered as judged by the members of user community and the scientific community.

Designing rigorous ways of establishing the second of these should be a required part of any proposal.

Education is an essential part of this research programme. Given the interdisciplinary nature of complex systems science and the fact that almost all doctoral students and post-doctoral research assistants are deficient in some core domains (*e.g.* mathematics, computation, physics, social systems) it is essential that the programme includes education to establish a common base of knowledge across the programme. It is essential for members of individual IPs to understand what the others are doing, and for the IPs to work together as scientific teams in which each individual shares a common understanding and an overview of the science being created. It is not possible for this research to be divided into disconnected work packages that are integrated late into the project.

3.3. An Outline Proposed Call for Fundamental Research in CSS and ICT

The call should require the creation of new mathematical and computational formalisms for representing multilevel systems of systems.

The call should require an application in at least one complex multilevel system, identifying a microlevel, a macrolevel and possible meso-levels. Although the number of mesolevels should not be specified in the call, it is noted that, for example, city zones often exist at seven or more levels, and that many complex systems have identifiable dynamics at many levels.

The call should require the availability of data at all appropriate levels, with the expectation that it will be heterogeneous and voluminous, and that it is likely to be incomplete and inconsistent.

The call should require formulation and demonstration of the inter-level dynamics, where this likely to be a combination of mathematical and computational models.

The call should require addressing the interaction and combination of the seemingly antagonistic concepts of ‘programmability’ and ‘self-organisation’, *i.e.* how spontaneous systems need not always be random and engineered systems need not always be directly designed.

The call should require engagement with policy in the design and management of the system(s) investigated. In particular this should include a formalism for policy statements and how these interface with the multilevel representation, possibly involving meta-propositions taking substructures at different levels as values. The formalism should also include ways of calculating the ‘truth values’ of these propositions in (possibly new) logics appropriate to the decision making meta system.

The call should suggest that project have an institutional partner offering their organisation, its activities, and its environment as an example of a multilevel system of systems of systems.

The call should require that all the IPs publish all their data, or make it available at no cost to members of the complex systems community with the same constraints as the IPs themselves. This is to avoid the common problem that projects may collect data using European research funds and then ‘hoard’ it as their own intellectual property. It is essential that experiments are replicable using the same data and published algorithms as the IPs.

The call should be made for IPs to enable them collect and process very large data sets.

The call should include a scientific coordination action and require that all the funded IPs work together as a programme, with frequent scientific exchanges open to the whole complex systems community. The coordination action should include funding to allow scientists who are not part of the IPs to attend workshops and meetings, and be responsible for ensuring that a wide spectrum of the complex systems community.

4. Recommendations

1. We recommend that FET fund a new programme of work in complex systems science as essential research for progress in the development of new kinds of ICT systems, and major improvements in current ICT systems.
2. We have identified the dynamics of multilevel systems as the area in complex systems science where there is need for a major paradigm shift, beyond which significant scientific progress cannot be made.
3. We have proposed a call with the theme Systems of Systems of Systems.
   The call will
   - require fundamental research in complex systems science
   - require new mathematical and computational formalisms be developed
   - require addressing the combination of 'programmability' and 'self-organisation'
   - require the involvement of a large 'guinea pig' organisation
   - require research into policy and its meta-level information dynamics
   - require the IPs work together assisted by a coordination action
   - require that all data collected be published in an open archive by the CA
   - require that all software developed be published in an open archive by the CA
   - require all staff have interdisciplinary knowledge through an education programme

Appendix I: The FET-ASSYST Questionnaire

Q1: **Relation and contribution of a potential future call of CS to ICT?**

Q2: **What makes CS research suitable for FET ICT?**

Q3: **Can universal concepts in systems be expected that can be applied to/influence future ICT?**

- are there steps forward in this direction worth chasing
- what has been the progress in the last 10 years towards this unification?
- Are there any observables that may be general?
- What (if any) are the reasons to study dynamics of CS?
- Can there be “Thermodynamics” of CS?
- what are the arguments for studying “out of equilibrium” dynamics and what is its
- relevance for ICT

Q4: **What could be an appropriate balance between theory and application?**

Q5: **How do we make sure if we have a mixed call that theory (or fundamentals) are worked on?**

Q6: **What should be the priority application areas? On what basis are we selecting ICT relevance?**

Q7: **Is there value in bringing together CS with other communities, *e.g.* PERADA?**

Q8: **Who are the users and how are they involved (for the research results)?**

Q9: **What is going to be the tangible output/impact of a call in the theory/fundamentals?**

Q10: **What is going to be the measure of success of the research in CS?**