

**“Another science is possible!”
A plea for slow science**

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I first wish to thank deMens.nu and the Vrije Universiteit Brussel for appointing me for this year Calewaert Leerstoel. My special thanks to Professor Jan Danckaert for organizing this lecture with kindness and efficacy. I thank them all for their trust, and all the more so as, when they appointed me, they probably knew that I may be something like a trouble maker. Indeed it might be said that I take a bit too seriously the commitment to free thinking as well as the motto of your university and mine, “scientia vincere tenebras”, a motto which for me is a very, very demanding one, since it entails that we never stop questioning both what we call science and the darkness it may defeat. And that we question also the way we are used to identify and denounce the darkness: does it not consolidate a static and too reassuring opposition between light and darkness? The taking for granted of such an opposition might well be part of the problem.

In other circumstances I could have chosen this theme, of light and darkness and of the possibility of “dreaming the dark”, in order to develop what may be a possibility of peace when war, as represented by the angel’s spear, seems the logical outcome. My lecture would then have explored how we, university researchers, can avoid the proud ritual gesture of defiance, when science challenges what it defines as darkness, for instance, the questioning of Darwinian evolution.

However, when Professor Jan Danckaert asked me about the theme I would address, an event had just happened that made any hesitation impossible.

I have, since many years now, been concerned by what is called knowledge economy and by the new politics of public research. Even Belgian French speaking universities have now begun to submit to the general imperative of benchmark evaluation in the competitive academic market. We came to that rather late, but it was not a matter of deliberate, daring resistance. It was rather inertia. Our authorities are now implementing it with the zeal of new converted.

However to complain and describe the sad probabilities that university research will be redefined in terms of the criteria used to evaluate it would not add to the situation anything relevant. These probabilities are what many among us already know and denounce. I consider that my job, as a philosopher, is to activate the possible, and not to describe the probable, that is, to think situations with and through their unknowns when I can feel them.

And it is the feeling of such an unknown which was activated by the repercussions among researchers of the event which happened in Leuven on June the third of this year, when Barbara Van Dyck was sacked by her university for having publicly explained and endorsed the action against a genetically modified potatoes field in Wetteren. A very tenuous unknown, certainly, but what I call unknown is always about possibility, not probability, about what calls for imaginative engagement instead of submission to the given definition of a state of affairs, even if it is in order to denounce it.

As some of you might know, one of the spin offs of Barbara Van Dyck's dismissal is an initiative by researchers who intend defending and promoting "slow science", that is, resisting the fast, competitive, benchmarked research, which is, seemingly unavoidably, becoming the norm. Needless to say, "slow" does not mean idle. The choice of the expression "slow science" makes this initiative part of the "slow motions", the best known of which is "slow food", resisting fast, bad quality and 'ready to eat' food and the system that produces it. Slow science is about the quality of research, that is also, its relevance for today's issues.

Thus I believe that I am true to our universities motto, *scientia vincere tenebras*, science will defeat the darkness, in using the opportunity of the Willy Calewaert Leerstoel to publically address the question of the darkness, which this new slow science initiative asks us to confront.

Let me first briefly recall why the Barbara Van Dyck's firing has such repercussions in the research community, which is, a such, a remarkable event since researchers are educated as strong individualists, distrusting any mixture of science and politics, rather affirming that their first responsibilities are the advancement of their science and the securing of funds for the young generation of researchers. They are educated to trust that if they are good enough, partnering with industries will turn at the advantage of their field. Does not industry need reliable science, that is, good research? For some of them, however, the sanction against Van Dyck has resounded as an alarm signal.

It may well be that the Leuven university authorities felt the need to send a strong signal to potential troublemakers and to turn Barbara Van Dyck in the living proof of the existence of a new boundary which until now it was the business of the law, and only the law, to discuss and determine. They chose not to wait and decided that supporting the Wetteren action against genetically modified potatoes, which clearly belongs to the *political* practice of civil disobedience, was equivalent to a breach of trust, justifying the resignation of Van Dyck's work contract. Such strong signals, however, always carry the risk of being heard in a different ways, which is what is now indeed happening.

Some of you may have been impressed by the Leuven authorities' argument that the ethics of free opinion, speech and debates implies the exclusion of violence, which is deemed to destroy the very possibilities of communication. I would recall that the very possibility of an effective debate about genetically modified crops has been obtained through actions of civil disobedience. More precisely those actions have imposed a modification of the terms of the debate. In Europe, the GMOs are no longer what must satisfy administratively defined criteria of safety. The question has been enlarged upstream and downstream, upstream when the history of the negotiation of the very lenient nature of this administrative definition of safety was made public, downstream when the many consequences of the GMOs crops were unfolded, including the very question of the kind of agriculture we wish in the future.

It may thus be affirmed that the civil disobedience actions about GMOs have opened, and maintained open, a space of collective learning. Knowledge, or *scientia*, has been produced, to which I am myself indebted. If *scientia* operates against darkness, in this case at least, it is civil disobedience, not academic science, which has activated resistance against the “darkness” that invades our democracies – a darkness to which Pascal Lamy, then European commissioner for Trade (he is now Director-General of the World Trade Organization), gave its most striking expression: “You do not stop the clocks”. In other words, you may democratically babble and express yourselves, but the ticking of the clocks commands our common future.

The story of the resistance against GMOs is about a right – not the right to stop the clocks, there are no clocks – but a primordial democratic right, that of thinking the future. Thanks to civil disobedience, the ultimate violence against democracy, that of the ticking clocks argument, has been thwarted. But : even if it has been thwarted, it was not defeated. The sacking of Barbara Van Dyck is a strong signal indeed.

When firing Van Dyck without waiting for any penal decision, because of a position she took as a citizen and not as a researcher, the Leuven authorities have clearly identified an act of civil disobedience opposing the ascent of a new GMO to the economic market with an act of violence against scientific research itself. This is indeed the reason given to justify her immediate and “urgent” dismissal: the action which Van Dyck publicly defended was directed against an experimentation planned by colleagues scientists! This, let me emphasize, has nothing to do with the already questionable prohibition of whistle-blowing, of making public hidden information one has access to because of one’s functions. It is not because she was a colleague that Van Dyck knew about the Wetteren potatoes. She had only the kind of information about them that any citizen can obtain. Her dismissal rather means that the Leuven university feels entitled to control its workers’ whole life and not only what they do at work, that is, in this case, as researchers. Such control goes against the very gist of the labour contract, and brings us back to medieval corporations, when the member of a corporation was indeed not a citizen but a member of a body, with no independent life. As a medieval corporation the university claims the right to its own court of justice, quick and summary justice indeed, and with no regulation authorizing it. But we know that such regulations are in preparation, and that, even when they act as citizens, researchers will now have to respect the interest of their corporation. The exception the university will claim, regarding usual work contracts, will most probably rest on the consensual idea that the production of scientific knowledge serves the progress of humanity and is entitled to be protected as such. And the regulations will entail that whatever the members of the corporation are doing will be considered as contributing to this production. As a result, the sort of immunity that scientists justifiably deserve when they produce robust and reliable knowledge will unjustifiably extend or overflow to something very different, that is, to industrial and economic undertakings.

However, I will not use the argument that the Wetteren potatoes were serving applied research not a pure scientific one, that they were not contributing in a disinterested way to the growth of human knowledge, and thus to the general progress of humanity. I am not, as I will explain later, a defender of what is called the “linear model”, sharply dividing between neutral, disinterested production of knowledge on one side, and development and industrial innovation on the other. More generally, I claim that we should not consider the question of the knowledge economy in the terms of disinterested science versus a production of knowledge aiming at interesting applications. As I will argue later, the knowledge economy rather

questions the very production of knowledge, the very conditions of reliability of scientific knowledge. And this is precisely why we must pay attention to the justification given by the Rectorate of the Leuven University, that the potatoes experimentation was planned by colleague scientists. This justification is clearly meant to bring consensus, because it seems to mean that it was an experimentation planned in order to produce reliable knowledge, a knowledge which, even if it is produced in the perspective of an industrial development, would have a value of its own. Whatever our position regarding some industrial developments, should we not all wish that, at least, such developments would incorporate and take into account reliable knowledge?

However, as all scientists know, if scientific knowledge has a reliability of its own, it is due to the collective dynamics that organizes publication, from the first critical verification by referees to the objections by competent colleagues for whom the reliability of the published claim is of vital importance for their own work and projects. Thus what about the publications following GMO field tests in general? And also, more precisely, what was the aim of the so-called experimentation in Wetteren? What kind of research has justified the new kind of loyalty a university feels entitled to demand from its members? What had to be so strongly defended and protected by the veil of scientific research?

It is clear that the Wetteren potatoes were not enrolled in a disinterested quest for knowledge. But they were not even enrolled in the production of any kind of knowledge concerning them. They were mainly meant to test, or more precisely, to promote, the acceptability of genetically modified potatoes the BASF consortium has created, in the very country where potatoes are sacred. It has something of a very daring marketing campaign to promote “The potato of the future” in the country of the fried potatoes!

Propaganda aiming at the acceptability of a new product is usually the business of enterprises and one could say that the disruptive action of the 29th of May, was part of the risk such campaign has to accept. Propaganda does not mean knowledge, but action. It is nevertheless this propaganda action that the Leuven authorities endorsed in the name of science when they sacked Barbara Van Dyck. This is a strong signal indeed, signalling a new aspect of the knowledge economy, of the required partnership between academic research and the industry. We could say that again civil disobedience has produced a “scientia” that helps us to think about our world and its threatening darkness. It has made us realize that not only researchers, equipments and public money are of interest for the industry, but also the very stamp of scientific legitimacy. This, to me, is the gist of the Van Dyck affair. It is about the new academic service available for the industry: the offer of safe grounds, grounds which will be defended in the name of science.

I have proposed as a title for my lecture a “plea” for slow science. This situates me as a philosopher. A plea is addressed to those who are empowered to respond by a disempowered party. It is as such that, as a philosopher, I address the scientists who listen to me today. Indeed they have a choice which we, philosophers, do not have. Fast, competitive science exists, that is, it did not wait for the knowledge economy. As I will briefly show later, it may even be said that the discovery of the possibility of fast science was one of the marking nineteenth century events. Since that time it has shaped the whole of scientific research, even becoming something like an existential imperative. For many scientific researchers, to slow down and lose ones’ time with questions that do not directly contribute to the immediate and

evaluatable progress of their field is even something akin to a sin – to a temptation a true scientist knows he has to resist. However, there is no fast philosophy. When objective, benchmarked evaluation will get the effective power to determine what it takes for somebody to work as a philosopher, philosophy will have been killed, at the university at least.

More precisely philosophy will have become something completely different, about which I can only say that if this is philosophy, I would never have become a philosopher. I became a philosopher to take the time needed to formulate questions which, I knew, had no place in the fast science for which I had been trained, that is, chemistry. It is as if this function of philosophy, welcoming people who need time to create their own questions, questions they will be responsible of, is over. We will have, as in the US, philosophers fast publishing in specialized journals about professionally recognized issues that interest nobody but other academic and fast publishing philosophers. I am thus in the situation of a supplicant addressing a plea to those who can make a difference. Only scientists who have the choice, who feel they can survive doing fast science but choose to resist may make this difference. I, as a supplicant, depend on them.

But my plea is also formulated with others, still more voiceless than I myself, in mind. Gilles Deleuze wrote that “the feeling of shame is one of philosophy’s most powerful motifs” (108) and indeed I feel ashamed before those young people entering university with the hope of getting a better understanding of the world we live in. We know that those who enter university today belong to the generation that will have to face a future the challenges of which we just cannot imagine... Can we claim that what we are proposing them meets, or even vaguely meets, this situation? Can we also claim that we deserve the trust people still have in the role we, who are working at the university, and those we train, can play in this future? It is heard that our ways of life will have to change, and this certainly entails a change in the way we relate to our environment, social and ecological. Can we claim that such change does not also entail a change in the ways our academic knowledge relates to its environment?

Furthermore, do we really think that the trend our universities obediently follow, with the injunction to participate in the general competition on the market of education and technoscientific innovation, helps us facing this urgent need for collective change? To borrow Al Gore’s title, the inhabitants of this Earth have to face some very inconvenient truths, and if we, who work at the university, who have been selected, and are paid, for our capacity to think, are not able to face such truths, how can we hope anybody else will be able to do it?

Let us be clear. For many of those who try to actually enact practical consequences of these inconvenient truths today, and, for instance, for many of those who insist on the radical unsustainability of our agriculture, the case is already closed. Universities as such, and the so-called experts they produce, are rather part of the problem than of the solution. Activists may come from universities but, as Barbara Van Dyck, they act as citizens. The acceptance by our universities of the knowledge economy may well mean that those who think that they have nothing to hope coming from the university as such, are probably right. The slow science theme, however, opens a tenuous possibility, or a tenuous unknown, against this probability. And this exactly what commits me to propose to you the definition which the mathematician and then philosopher, Alfred North Whitehead, gave, in 1935, of the task of the university.

“The task of a university is the creation of the future, so far as rational thought, and civilized modes of appreciation, can affect the issue. The future is big with every possibility of achievement and of tragedy.”

What I find interesting in Whitehead's apparently innocuous proposition is that the future is associated neither with the growth of knowledge nor with progress, but rather, with radical uncertainty. We do not know what our future will be. And we do not know either if, and to what extent, rational thought and civilized modes of appreciation can affect the issue. This is why this proposition is relevant today, more than ever.

But what did Whitehead mean by rational thought and civilized modes of appreciation? In fact he meant something very demanding, to which I will come back later, associating it with "slow science". But I will first emphasize that Whitehead's proposition was, already in 1935, something like a plea. Indeed what made him a philosopher cannot be disentangled from his deep feeling of anxiety concerning the effects of what he characterized as an important discovery marking the nineteenth century: the "discovery of the method of training professionals, who specialize in particular regions of thought and thereby progressively add to the sum of knowledge within their respective limitation of subject".

Let me make it clear, right from the beginning, that the point is not to criticize specialization, or abstraction. Whitehead was a mathematician, and for him, you just [I quote] "cannot think without abstractions". Whitehead would never have criticized the way sciences abstract what matters for each of them from an always entangled world. However for him rationality was not the capacity for abstraction, it was rather the ability to be vigilant about one's abstractions, to not be blindly led by them. As we all know, a good craftswoman does not know only how to use her tools. She is the one who will not envisage a situation in terms of the demands of the particular tool she is used to but rather envisage the fitness of this tool for this situation. For Whitehead, it is the same for the exercise of thought – you need to be vigilant about your modes of abstraction.

This vigilance is precisely what is lacking with those who Whitehead characterizes as professionals. They are what Whitehead characterizes as "minds in a groove. Each profession makes progress, but it is progress in its own groove. (...) The groove prevents straying across country, and the abstraction abstracts from something to which no further attention is given. (...) Of course, no one is merely a mathematician, or merely a lawyer. People have lives outside their professions or their business. But the point is the restraint of serious thought within a groove. The remainder of life is treated superficially, with the imperfect categories of thought derived from one profession." (197)

As such, professionals, fixed persons with fixed duties, are not new to the world. However, Whitehead remarks, "in the past, professionals have formed unprogressive castes. The point is that professionalism has now been mated with progress. The world is now faced with a self-evolving system, which cannot stop." (205) One cannot stop the clocks, Pascal Lamy claimed.

It is important to emphasize again that what characterizes professionals is not specialization. Whitehead rather characterizes them as "lacking balance". Their training, he writes, as it neglects "to strengthen habits of concrete appreciation of the individual facts in their full interplay of emergent values", leaves them prey to the power of a particular set of abstractions, promoting a particular value. I rather like the "lacking balance" formulation, because of its affinity with images of scientific creativity that, as we will now see, announce the invention of the method of training scientists as professionals during the nineteenth century, that is also, the invention of what I would call "fast science". The plea of Whitehead

regarding the task of universities thus also aimed at a “slowing down” of science, which is the necessary condition for thinking with abstractions and not obeying to abstractions.

I turn now to the invention of this type of training, which has become the general model in our universities. It is strikingly illustrated by the radical redefinition of what it is to be a chemist by Justus von Liebig.

In the “chemistry” entry of the Diderot and d’Alembert Encyclopedia, the chemist Gabriel François Venel had characterized chemistry as a “madman” passion. It took a lifetime, he wrote, to acquire the practical knowledge and ability to master the wide variety of subtle, complex and often dangerous chemical operations pertaining to the many arts or crafts of chemistry, from that of the perfumers, to that of the metallurgists or the pharmacists. In contrast, in Liebig’s laboratory a student would in contrast obtain his doctoral degree after four years of intensive training. He would learn nothing, however, of the many traditional crafts and recipes. He would use only purified well identified reactants and standardized protocols, and learn only up to date methods and instrumental techniques. Liebig was named the “chemist breeder” because of the hundreds of students who were trained in his laboratory at Giessen, between 1824 and 1851, many of whom created similar university laboratory, while others played a crucial role in the creation of the new chemical industry.

The invention by Liebig of what we may call “fast chemistry” entails a cut, which does not divide pure and applied chemistry, but rather the whole continent of chemical crafts on the one side, and, on the other, both academic research and the new network of industrial chemistry, the two of them entertaining a new symbiotic relation as each needs the other, feeds the other and is fed by the other.

Symbiosis however is a balance that must be maintained. It is striking that Liebig, who played a very important role in the development of industrial chemistry, also became, as soon as 1863, a passionate promoter of the need for pure, autonomous academic research. He is the father of what we call now the “linear model”, together with the famous “goose with golden eggs” argument: it is for its own best interest that industry should keep its distances from academic research, leave the scientific community free to determine its research questions, because only scientists can tell, at each step, which questions are fruitful ones, which will lead to fast cumulative development and which others will only result in some empirical gathering of facts leading nowhere. For industry, to dictate its own questions would be like killing the goose and losing the eggs.

We have heard multiple variants of the same argument, as the motto of the arrangement which many scientists associate with the Golden Age, when science was recognized as the free source of novelties, which would lead to industrial innovation, ultimately benefitting the whole of humanity. However some aspects of the argument are seldom developed. The first one is the division, a true class division, between scientists who work on protected academic grounds, and those who sell their labour force to the industry and are usually denied autonomy and the freedom to contribute to public knowledge. The second is that the goose and her golden eggs metaphor is hiding an important aspect of the role of the scientist now trained as a professional of fast science.

The official story is that the goose lays her eggs and is happy to learn that some of them have turned golden in terms of industrial development. She hopes that it will ultimately result in benefits for humanity but she cannot be considered as responsible for misuse. This

corresponds to the “ivory tower” image of academic science, and it is reinforced by another current image of scientific creativity, that of the sleepwalker, always pictured as walking on a narrow ridge without fright or feeling vertigo because he is blind to the danger. As a result, the fact that the remainder of the scientist’s life is, as Whitehead wrote, “treated superficially, with the imperfect categories of thought derived from one profession” becomes a condition for creativity. Asking a creative scientist to be effectively concerned by the consequences of his work would be the equivalent of waking the sleepwalker, making him aware that the world is far from obeying his categories. He would doubt and fall, that is, be lost for science.

This image of scientific creativity, as intrinsically lacking balance, in Whitehead terms, is deeply ingrained in fast science education. One way or another, explicitly or not, scientists learn that questions which concern the wider world, the world where the golden eggs will make a difference should be globally defined as “non scientific”, even if such questions are the object of a lot of scientific work in other departments, dealing with cultural, social or economical problems. They learn that for a “fast science” researcher to lose his time with these questions is a very bad sign, is even something like a sin, at least a weakness manifesting that he is not completely committed to the advancement of true science. Interest in the world we live in becomes like a temptation which should be resisted if the sleepwalker is not to lose his balance and fall down from the ridge into the morass of turbid opinions.

From that point of view, Marc Von Montagu was quite justified when he claimed that GMOs were the rational solution for feeding the hungry, quietly ignoring the social and economical mechanisms creating hunger in our world. He did just show himself to be a truly creative scientist, a sleepwalker blind to what would endanger the trust he needs in order to lay his eggs. But this last example is also sufficient to show what the official story dissimulated. There never was an ivory tower. The valorisation of their work, the connection with those who can turn their eggs into gold, has always been part of the activity of academic scientists even if, like Pasteur or Marie Curie, their name is associated with disinterested research. What characterizes fast science is not isolation, but rather working in a very rarefied environment.

Already when he cut chemistry in the making from chemical arts and crafts, Liebig also cut it from the social and practical concerns those arts and crafts were imbedded in and responded to. The only true interlocutors of the new academic chemists, the only ones who understood their language, were now those who inhabited the industrial world, also in the making. And this still corresponds to the intellectual equipment fast sciences formation provides to scientists. They will easily dismember a situation into what would be its objective or rational dimensions, and what would be only a matter of contingent, arbitrary complications. And the objective, rational dimensions which correspond to fast science’s categories are rather naturally the very ones which are relevant for industrial development since both agree about ignoring the same type of complications. No direct mobilization is necessary here, only the symbiotic relation between two modes of abstraction.

As we know, this is no longer sufficient. The knowledge economy is now destroying the protected home where the goose was laying her eggs. The relative autonomy of scientific research, which was obtained by Liebig and his colleagues, is part of the past. Some may be tempted to claim that it never existed anyway, because of the intimate connection between academic fast science and the industry. I disagree and claim instead that what is in the process of being destroyed is the very “social fabric” of scientific reliability. In the future we may well have scientists at work everywhere, producing facts with the speed that new

sophisticated instruments make possible, but the way those facts will be interpreted will mostly confirm the landscape of settled interests.

As all working scientists know, if a scientific claim can be trusted as reliable, it is not because scientists are objective, but because this claim has been exposed to the demanding objections of competent colleagues concerned by its reliability. And it is this shared concern which may well be destroyed if these colleagues are mostly bound to industrial interests, that is also, bound by the need to confirm the promises that attract their industrial partners. What may well prevail then is the general wisdom that you do not saw off the branch on which you are sitting together with everybody else. Nobody will object too much if objections against the weakness of a particular claim may lead to a general weakening of the promises of a field. Dissenting voices will then be disqualified as minority views that need not to be taken into account, as they spell unnecessary trouble. What will then happen has already got a name, “promise economy”, when what holds protagonists together are no longer reliable scientific eggs that may turn golden for industry, but glimmering possibilities nobody is interested to assess any longer. Knowledge economy means that speculative economy, bubble and crash economy, succeeded to annex the production of scientific knowledge.

It should be useless to emphasize that something entirely different is demanded by the dangerous future we are confronted to, with the accumulation of very, very inconvenient truths about the climate, the pollution and poisoning of our environment, the depletion of crucial resources. What this future may well urgently demand instead is what my plea is about: slow science.

From what I told you, you will have understood that to me slow science is not the return to the Golden Age, when the autonomy of scientific research was respected. It is not “back to the sixties”. The autonomy of fast science may well have protected the reliability of scientific claims, but never ensured the reliability of a mode of development which we are now shamefully forced to recognize as having been, and still being, radically unsustainable. This is by no means an accident. The reliability of fast science’s results is relative to experimentally purified, well-controlled laboratory experiments. And *competent objections are competent only with regards to such controlled environments*. Which means that scientific reliability is situated, bound to the constraints of its production. Which also means that when the eggs turn golden, that is, when they have left their native environment, they have left behind this specific reliability and robustness. What reliability they will have now is no longer an issue of scientific judgment only, but rather a social and political issue.

For instance, airplanes are safe enough because of the existence of a consensus about the need to avoid at all cost airplanes’ crashes. In contrast the concern for the sustainability of our mode of development, which is not a new one, has, till recently, been all but consensual. People who objected on this ground were not even listened at, rather derided and attacked as those who would send us back to the cave! Certainly, it was recognized by the tips of the lips, many innovations have unwanted consequences, but scientific-technical progress was bound to become able to cure the damage. To doubt it, was to doubt progress! (and that is, as we know, blasphemous).

Here we can recognize an echo of Whitehead’s point about the professionals’ restraint of serious thought within a groove, while the remainder of life is treated superficially. And the answer of many scientists is just as superficial when they claim that it is not their fault if sustainability was not a public concern and that they cannot be considered as accountable for

the way “society” decides to use what they produce. This is the typical golden eggs goose answer: as usual it ignores that their claimed irresponsibility for what regards the use of what they produce never prevented academic scientists to associate scientific progress with social progress, to join in the “back to the cave” argument, to present their science as bringing at last rational solutions to problems of general concern, to frame objections in terms of a simple opposition between science and values – as if those aspects of a concrete situation which they are not equipped to take into account could be reduced to a question of value! To tell it politely, we have no memory of a scandalized scientists’ collective outcry, publically denouncing a colleague who made such pretences.

But slow science is not, emphatically not, about the goose becoming an omniscient intelligence, able to envisage the consequences of the innovation her science makes possible. It rather coincides with the seemingly modest definition by Whitehead of what universities should foster: rational thought and civilized modes of appreciation. Rational thought would mean active lucidity about what is actually known, avoiding any confusion between the questions that are actually answered and the questions that will arise in the wider and inevitably messy environment. A civilized mode of appreciation would imply to never identify what is well-controlled and clean with some truth transcending what is messy. What is messy from the point of view of fast science is nothing else than the irreducible and always embedded interplay of processes, practices, experiences, ways of knowledge and values that make up our common world.

This may be the challenge slow science should answer: to enable scientists to accept what is messy not as a defect but as what we have to learn to live and think in and with. The symbiosis of fast science and industry has been privileging disembedded and disembedding knowledge and strategies, abstracted from the messy complications of this world. But messiness is returning with a vengeance. Ignoring it, dreaming of its eradication, we discover that we have messed up our world. I would then characterize slow science as the demanding operation which would reclaim the art of dealing with, and learning from, what scientists too often consider messy, that is, what escapes general, so-called objective, categories.

Reclaiming is a word crafted by US activists. It refers inseparably to operations of reappropriation of what we have been separated from, and of healing, that is, of recovering or reinventing what this reappropriation demands. Reclaiming always begins by accepting that we are sick rather than guilty, that is: by understanding how our environment makes us sick. From this perspective we may consider the manner in which our universities, once so proud of their autonomy, have accepted, in the name of the market, the imperative of competition and benchmarking evaluation, and also the way researchers have accepted without too much resistance the redefinition of research by the knowledge economy. Whatever the explanations we can offer, they all testify of the deep vulnerability of what we were so proud of. The arrangement which promoted fast, disembedding and disembedded science as a model for scientific research made us too sick to defend it. The Van Dyck affair has been the occasion to measure what this leads us to.

Reclaiming operations are never easy ones. If reclaiming scientific research means a reembedding of science in a messy world, it is not only a question of accepting this world as such, but of positively appreciating it as such, of learning how to foster and strengthen, in Alfred North Whitehead’s words, “the habits of concrete appreciation of the individual facts in their full interplay of emergent values.” This, as I have already emphasized, does not mean avoiding specialization and abstraction, which have an obvious value of their own. But

concrete appreciation does not just mean abstaining to consider as a simple remainder what our abstraction are abstracted from, abstaining to judge it superficially away. We also need to learn how to situate our abstractions in what Whitehead called the interplay of emerging values.

For instance, I have insisted upon the fact that the reliability of fast science results depends upon competent colleagues, objecting, putting to the test, verifying, and I have also emphasized that this reliability is not a stable attribute. Reliably getting out of the research environment should need a radical redistribution of the expertise, the exploration of the often messy web of other and new questions that matter in the new environment. In other words, “out there” reliability would depend on something which may be very different from the objections by competent colleagues, who all share the same values, and work in similar environments. It may also be significantly different from the nice objections coming from colleagues in interdisciplinary meetings, where politeness, mutual respect and often lazy hypocrisy are too often the rule. Even in the best of all cases, when a minimum of trust prevails among protagonists, when it is a question of the emergence of intercorrelated values, including those of protagonists who are not academically formatted but are empowered to object, the process will be, and must be, slow, difficult, rich in friction, pulling and tugging between diverging priorities. Any nostalgia for the clean, competent environment of dear colleagues will result in the conclusion that people are unable to participate, that they need that people who know better impose a rational solution.

In other words, goodwill is not sufficient. Reclaiming is never a matter of goodwill only, of the kiss of peace turning the disappointing frog into a nice, polite and constructive partner. Learning is needed to get interested in the frog itself, that is, in the mess in which all, scientists included, are participants.

And here is also where we may touch the radically asymmetrical knowledge developed under the model of fast sciences. We know a lot about developing material, or so-called immaterial, technologies, but when it comes to much older techniques, needed when people gather around an issue which divides them, and need to learn from each other about this issue through their very disagreements, we are not good at all, and we have apparently learned nothing yet, and even lost what we knew and what other peoples would call civilization. Just think to the technology of PowerPoint which is becoming an imperative of communication – to make one’s point in a striking, authoritative and schematized manner. In “bullets” (just listen to this word ...)

Just think also to the boredom we are so used to, silently and patiently half-listening to a dear colleague speaking for an hour, just as I am doing now. We have departments of psychology, psycho-sociology, pedagogy and all that, but we have not learned even a small fraction of what US reclaiming activists had to learn because they wanted to become able to work together without commanding authority. They have learned indeed to consider each meeting as what I would call with Whitehead an “individual fact”, depending on the interplay of emerging values, of values that can emerge only because those who meet have learned how to give to the issue around which they meet the power to effectively matter, to effectively connect them.

It may well be that producing knowledge about such individual facts demands an approach that does not conform to the model of fast science that rules our universities, with its imperative of general, disembedded knowledge. Please think to those moments of emergent

values, for instance the moment when somebody feels transformed by the understanding of the perspective of someone else, the gathering which gets the transformative power to have its participants thinking together, or the experience that something which appeared insignificant may indeed matter. Such moments cannot be disembedded, submitted to general categories. This is why they have been superficially treated, with the imperfect categories derived from the imperative of reproducibility. They have been judged unfit for knowledge, or worse, relegated to the irrational, to what we should not pay attention to. But it may well be that the knowledge they need is just a bit different, that what we can learn about them is not how to define them, rather how to foster them : what supports and sustains them, and what thwarts or poisons them – something more similar to the slow knowledge of a gardener than to the fast one of the so-called rational industrial agriculture. In this case, the knowledge produced in our university is indeed radically lacking balance and we are all paying the price for it.

Again, reclaiming first means recognizing that we are sick, and in need for recovery. Slow science is not a ready made answer, it is not a pill. It is the name for a movement that may gather many paths of recovery. What of slow meetings, that is meetings that are organized in such a way that participation is not formal only? What of slow talks, not only inviting people one really wishes to listen but reading and discussing beforehand so that the meeting is not reduced to the ritual of attending a prepared talk ended with some questions? What of taking the habit of demanding that when colleagues speak about issues that are beyond their field of expertise, they present the information, learning and collaborations that allow them to do so? What of paying attention, when expertise is needed about an issue of common concern, that co-experts are present and able to effectively represent the many dimensions relevant to the issue? From the point of view of fast science, all such proposals have a common defect. They all mean a loss of time, or worse, the duty to cultivate an active lucidity about the partial character of one's own questions – the awakening of the famous sleepwalker.

We have to learn how to cultivate slow science, and this is why I have spent much more time telling you about fast science than about what slow science would be. Accompanying the movement of those who claim today that “another science is possible”, my job, as a philosopher, was to try and activate imagination, which entailed going beyond the question of the present direct mobilization of research called the knowledge economy and confronting the consequences of the older mobilization of research and their powerful hold on our imaginative resources.

It is certain that in the present situation the cry “another science is possible” may sound like utopia, but then we should accept that the very idea that our future may escape the worse is also utopia. My wish is that research institutions and researchers learn to be affected, actively affected, by the fact that for many people the task of universities, our task, and thus our responsibility, are engaged in the creation of this future, a future which would be worth living. Many still trust us, even if their trust is no longer blind and has become, happily so, a more discriminating and demanding one, refusing any exaggeration of the power of scientific knowledge and the correlative superficial judgements professional, mobilized researchers offer about the remainder of life.

I fear the future wherein general mistrust will replace this more demanding trust, but I am also aware that no university today is free to escape the rules that make fast, competitive, science a matter of life and death. That is why I wish, ending this talk, to emphasize the difference between, on the one hand, adhering to a rule and, on the other, recognizing its power while looking for the opportunities to experiment outside its bounds, creating interstices where

another science could discover its own demands. To take just an example, a university may be free to define such reclaiming explorations of slowing down practices in terms of “services to the community », which they are indeed, and recognize them as such. This would be a strong signal, indeed, that we are not a corporation, proudly obeying the ticking clocks of an unsustainable progress, but a community trying to give some meaning to the now consensual but often empty claim: the claim that we have to change our ways of behaving in this world.