SARTONIANA

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Sarton Chair of History of Science
Ghent University

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Introduction

R.Rubens

In the academic year 2010-11 the 25th Sarton Chair for the history of sciences took place. Sartoniana each year is the printed contribution to this endeavour. At the beginnings the Sarton Committee was led by Michel Thierry, MD, Ph.D. As a professor of Obstetrics he became very early also involved in the history of medicine. It was based upon his energy and leadership as the first Chairman of the Sarton Committee, that the first two decades of the Sarton Chair were possible. The Sarton lectures thus became a part of the University tradition and well known in the field of the history of sciences. Being the respectful heir and now in charge of this tradition it is worthwhile after twenty five years to remember this.

After a quarter of a century it is also important to remember how the Sarton Chair and lectures were started and certainly why Gent University wanted to remember George Sarton.

In 1884 was born in Gent George Alfred Sarton, the son of Alfred Sarton, a railway engineer, and Lèonie Van Halme. Unfortunately for the young George his mother died after one year. The secondary education of the young Sarton was spent at the "Athenée Royal De Gand" (now the Koninklijk Atheneum Gent Voskenslaan) and the "Athenée Royal de Chimay". At that moment in time all education in Belgium was in French. Although Dutch was and is the majority language in Belgium, the official language for administration and education was up to 1930 French. In 1902 he enrolled at the Université de l'Etat de Gand (now Universiteit Gent) to enter the faculty of Arts and Philosophy as preparation to a degree in law. Very soon he became involved with the then starting progressive workers movement in Belgium. Based upon a trip to England where the Fabian

Society very much impressed him he was very active in the beginnings of a socialist student movement. He became also acquainted with Hendrik de Man, who was a very important socialist politician in the interbellum. Based upon societies existing in the Netherlands he founded the society "Reiner Leven". "Reiner Leven" pretended to be a society open for all genders, but with a vegetarian and and temperance ideal. Soon the mostly male society was joined by a group called "De flinken". The ladies society "De flinken" was founded by Celine Dangotte and aimed at a society with equal rights for women, mainly concentrating upon civil rights and education. One of the members of "De Flinken" was Mabel Ewes, who marries Sarton in 1911.After a time spent in the political and academic societies Sarton returned to the university and became dr. scientiae in 1911 with a thesis about the Principia Mathemathica of Newton. Based upon his broad interests he already was interested in the history of science. He therefore founded the journal ISIS, devoted to the history of science. The young couple had a daughter (May Sarton) very soon. Mabel Ewes was the daughter of a Welsh engineer, so being from British origin, the couple had to flee from Belgium in 1914. They left the house in Wondelgem definitively on 12 October 1914. First the family travelled to London but very soon they embarked in 1915 to New York. With the help of Leo Baekeland, a fellow alumnus of Gent University, Sarton was introduced in the American Universities After a short spell at the George Washington University he became lecturer at Harvard University in 1916. From 1918 he also became researcher at the Carnegie Institute in Washington. In 1940 he became full professor. Eleven years later he received the title of professor emeritus. In 1956 he died in Boston aged seventy-two years. He wrote numerous contributions to the history of science during his long academic career but his five volumes Introduction to the History of sciences is still nowadays considered a milestone in the field.

In 1984, the centenary of the birth of George Sarton, Gent University at last remembered the important alumnus who made a brillant scientific career. That year the university had two exhibitions remembering him and in 1985 the Sarton Chair for the history of sciences was founded.

The academic year 2010-11 again saw very important lectures. As a tribute to the quarter of a century Sarton Chair lectures and medals we had two Sarton Chair holders and four medal recipients this academic year.

The first Chair holder coming from the faculty of medicine and health sciences was J. J. Van Rood. He not only writes and communicates about the history of immunology but is an important contributor to the discovery and introduction of Human Leucocyte Antigens (HLA) in the clinic. Numerous transplant patients around the world have a longer life and much less complications based upon his ideas concerning these important biological factors. In a well written review also containing some basic "parochial" information he explains how this important discovery unfolds the last fifty years.

The history of the landscape as a picture and reflection of the interface between human and nature is developed by L. Daels, who received the Sarton medal from the faculty of sciences. Based upon a carefully selected iconography the different phases in the history of the landscape are mentioned.

The paper by Conte, the Sarton medal 2010-11 from the faculty of law, about the history of law mainly concentrating upon the feudal bond gives an insight into the structure and nature of the medieval society. The details and data about transcultural influence in medieval law are very stimulating certainly in a period in which again the society in Europe tries to enhance legal comparison and unification. Out of his paper it appears that based upon contacts and knowledge of legal systems in the different realms of medieval Europe a similarity was approached.

Herman Tyrell, the first recipient fom the faculty of political and social sciences, with a more philosophical paper combines the history and sociology based upon the basic work of George Sarton.

The history of the conquest of the sound barrier makes the content of the paper of Anderson., the medal holder from the faculty of engineering and architecture J.D.Anderson gives a detailed narrative of the painstaking precision needed for the enormous step in aerodynamics.

The second chair holder, James McGuire proposed by the faculty of arts and philosophy gave two very inspiring lectures about Descartes and the origin of empirism. The detailed relationship between Newton and the French rationalist ideas forms the backbone of his research.

Sarton Chair Lectures

Laudatio Jon J. van Rood

Lucien Noens

During the career of a faculty member at the University of Ghent only a few highlights occur. However, today such an occasion takes place, and I am sure that you will, together with me, enjoy it very much.

Indeed, in a few minutes you will hear Prof. Dr. Jon van Rood, world famous pioneer and guide in transfusion and transplant immunology.

Over many years all those directly or indirectly involved in the field of transplant immunology have recognised Jon as the crucial person who together with his co workers laid the foundation as well in basic research as in the clinic for what we now practise daily, and his impulse and that of his team have evolved into critical international organisations, such as Eurotransplant, Europdonor, Bone Marrow Donor Worldwide, The European Federation for Immunogenetics and the World Marrow Donor Association.

All of this is based on his discovery that antileucocyte antibodies are responsible for transfusion reactions. Also during pregnancy antileucocyte antibodies are found, and with these observations the immunogenetics, i.e. typing of leucocyte antigens named human leucocytes antigens or HLA has evolved. These genetic polymorphisms are expressed not only on leucocytes, but on all other body cells, explaining their major importance in cell and tissue transplantation: major histocompatibility antigens. The path that Jon and his group have followed after his initial observations, his pioneering journey will be summarised by himself.

Jon has, in a unique way, combined highly scientific lab research with practical clinical applications, starting in the 50's of last century until actually,

with his ongoing active participation in research, conferences and meetings. He is the real pioneer in the field of human transplantation. The way these HLA antigens interact in immunological reactions, during transfusion and in transplantations, and during pregnancy and what these reactions mean for our patients, remains an extremely capturing field.

During a short period in the second half of the 80's I myself could experience how the team in the blood bank in Leiden under Jon's leadership continuously practised what is called now translational research. This is reflected in our current clinical practice: HLA typing at allele resolution, selecting the right donor organ allocation, localise potential haematopoietic stem cell donors worldwide, but other things that look apparently rather simple such as a scientifically based transfusion practice and gaining insight in the immunobiology of a number of diseases, including the extremely interesting immunobiology of pregnancy i.e. the most important transplantation that exists.

Dear Jon, even if I enjoyed only a short passage in your blood bank in Leiden, I will remain thankful and therefore I am extremely grateful that our alma mater and our faculty immediately agreed to offer you the Sarton Chair and Medal for your remarkable career. In this auditorium, at this university and outside, all my colleagues will support giving you this reward.

It is for me a great honour to invite you as laureate of the Sarton Chair to accept the Sarton Medal issued by our rector and to present us your lecture.

A brief account of a voyage of discovery in transplantation immunology

Jon J. van Rood
Leiden University Medical Center

The discovery of HLA was an adventure in many ways very much like the discovery of the Americas. For this Columbus got the credit, but that is only partially correct. Travellers have been in America before him both from the East and from the West. With HLA it was not really much different. A small group of people got the credit, but far more were involved and contributed.

I have been involved from almost the beginning in the HLA story and have watched – often from nearby – the breakthroughs; insights that opened new aspects of HLA. A discovery is not a gradual process, but one, which goes with jumps and laps.

I will therefore not give a chronological summing up what happened, but limit myself to turning points, the sudden flashes of insight, which made working in HLA so exciting and rewarding.

The discovery of HLA

In the early fifty's of the previous century leukocyte agglutinins were thought to be autoimmune antibodies induced by drugs such as pyramidon. Bernard Amos was the first to recognise that leuco agglutinins recognised the murine MHC, H2, and were thus allo- and not autoantibodies[1]. A year later Dausset noted that blood transfusions were inducing alloimmune leucocyte agglutinins, but did not find such leucoagglutinins in the sera of

pregnant women[2][3]. Miescher was the first to use the term leucocyte group[4].

At that time after finishing my studies, and passing my M.D. examination I started my training as a specialist in internal medicine. In these days the youngest resident was responsible for the blood bank, which was considered to be a minor job. In a sense it was with 4000 blood donations a year, no blood products and a minimal amount of poorly standardised laboratory work.

The Blood bank was located in a single room, where we collected the blood, determined the blood groups and performed the cross matches.

Life was busy. One of our major problems was the frequent occurrence of transfusion reactions. It was for that reason quite important when van Loghem published in 1956 a paper in which he showed that leukocyte antibodies induced by blood transfusions were able to cause non-haemolytic transfusion reactions[5]. Even more important was the finding, that you could prevent the occurrence of these transfusion reactions by removing the white cells from the blood. We started an office, which could be consulted if a patient had suffered from a transfusion reaction. These consultations taught us to differentiate between a haemolytic (haemoglobinuria!), a non-haemolytic (flu-like syndrome!) transfusion reaction and one caused by bacterial infection (red face flush!).

On April 14 in 1958 I was on duty and was called to Mrs H., a mother of four children. Soon after delivering twins she had had a postpartum bleeding which led to pre-shock. She was transfused. The blood pressure rose again but a few hours later she had shaking chills, felt nauseated and collapsed. Thanks to van Loghem's findings, we realised that the patient probably had suffered from a non-haemolytic transfusion reaction caused by leuco-agglutinins. Strong leuco-agglutinins were indeed found, however a new question now arised. The question being where and when the patient had received the blood transfusions. Because of Dausset's publication blood transfusions were up to that moment thought to be the only inducers of leukocyte antibody formation[3]. Leiden is a small town, the patient was born in Leiden and lived there and we could not find in the Blood Bank a record of previous transfusions. I went back to the patient to ask whether, and if so, where she had received these blood transfusions.

She looked at me in horror and said: “This was the first blood transfusion I ever received and as far as I am concerned, it will be the last”.

Only then did we realise that Dausset’s statement[3] might have been wrong and pregnancy per se could indeed induce leukocyte agglutinins.

We went back to the laboratory, and checked whether other women, who like Mrs.H had been pregnant several times, had formed leukocyte antibodies. This turned out to be the case. We published our findings in Nature and were famous overnight[6]. Rose Payne published similar findings shortly thereafter[7].

Next we collected 60 sera from women, who had formed leucocyte antibodies during pregnancy, tested them against a panel of leukocytes obtained from 100 donors (Fig. 1, p. 26) and analysed the results with the help of a computer[8]. Nowadays this is a standard procedure but at that time it was a completely new approach. We found that the sera could be divided into a number of inter correlated clusters. One of them recognised the 4A and 4B antigens, now called HLA-Bw4 and -Bw6. (Fig. 2, p. 27). These data formed the basis of my Ph.D. thesis and with that our position in the field became even stronger[9].

Many people became interested in “leukocyte grouping”. Amos took the initiative to organise the first Histocompatibility Workshop. This was the start of a unique international effort to study the genetics and define the clinical importance of the HLA system. The first workshop was mainly concerned with comparing techniques, during the second one in Leiden we showed our computer trick. Figure 3 (p. 28) shows Terasaki, the inventor of the micro cytotoxicity test with Rose Payne, expressing their interest. Dausset had in that same meeting proposed that these leukocyte antigens belong to one complex system Hu-1[10], as I had also mentioned in my thesis[8]. People were sceptical about the concept, but it turned out to be correct.

At the same meeting Frits Bach and Barbara Bain presented their findings using the Mixed Lymphocyte Culture (MLC) test, opening in that way a complete new approach to study the HLA system[11][12]. We decided to join the fun and study the genetics of the determinants recognised in the MLC test.

We wanted to tackle the problem by using homozygous typing cells (HTC). HTC's are lymphocytes from individuals, which have inherited from both their parents the same genetic information for HLA, the same haplotype. They can be used to type for the genetically determined MLC determinants, because the MLC reaction will be negative if the cells to be typed are not responding to an HTC which carries the same MLC determinants as the responder, i.e. the cells to be typed[13].

The difficulty however is how to identify the individuals, which have inherited the same information for HLA from their parents. The offspring of cousin marriages would be of course ideal, but how do you identify such people? One of my PhD students suggested to contact the Pope, because if you are a Catholic and you want to marry your cousin you need to ask the Pope permission to do so. So we did (Fig. 4, p. 28). We thought it to be highly unlikely, that the Pope would answer us, but He did! Three months later came a letter from the bishop from Haarlem that we had the permission and could we come along to discus the details.

Fortunately Ben Bradley coming from Cambridge, where he had worked with Roy Calne, had joined us. He had already shown in the pig the possibility of obtaining HTC ´s by inbreeding and that such HTC´s could be used for the typing of the MLC determinants[14].

After we had collected a large number of HTC ´s we were able to type – for the first time – a large panel of unrelated individuals for these MLC or HLA-D determinants. Although from the point of genetics this information was quite interesting it was not very useful in the clinic. We needed a serological test to make the typing for the HLA determinants or HLA-D a clinically reality.

It was again serendipity, which helped us out. We were doing at that time experimental skin grafts in man. Mrs Pl. was one of the participants in the study. She received a transplant, which we expected to be rejected between 12 to 14 days after transplantation. But after 24 days it was still in perfect shape!! We knew that she was married and had had three children. I wondered whether she might have formed antibodies and that these antibodies had caused the prolongation of the skin graft survival. We did indeed find antibodies, which were however very difficult to work with. We could show that they were able to inhibit MLC test and could be studied in a modified immunofluorescence test[15]. The test was subsequently

streamlined and that opened up the possibility to type for the HLA-D related antigens, (HLA-DR)[16]. We had in this way developed the techniques to study HLA and had charted most of the loci and their alleles, which were of importance in the clinic.

In an International effort during a number of workshops the HLA community was able to describe the whole polymorphism of the HLA system. It consists of at least 6 different loci on chromosome 6 indicated by the letters A, B and C for those antigens which are expressed on all nucleated cells and DR, DQ and DP for the antigens expressed on cells of the immune system. Each locus has many different antigens or alleles which are numbered. The complexity of the system is nearby unbelievable. The number of total different combinations of the different antigens is over 900 billion. Far more than there are people on earth.

It was quite clear that a system of such complexity must have a biological function. Ruggero Ceppelini summarised – before the opening of the First International Transplant Society Meeting in Paris in 1967 – it as follows: "Nature does not select a genetic system of such complexity only to frustrate transplant surgeons!!".

Some of these HLA combinations or HLA phenotypes are relatively frequent i.e. one in a hundred people will have such a phenotype others are extremely rare e.g. one in a million people carry such a rare phenotype. This complexity begs also a question. The question being: how is it possible that these rare HLA phenotypes do not disappear? The answer to that question is that HLA antigens themselves or nearby loci carry pheromones, which are picked up by female mice and…. human women! Males with pheromones, which are different, which implies that they have also different HLA phenotypes are especially attractive for the female counterpart![17] In that way the polymorphism of the HLA system is secured.

The relevance of HLA in the clinic

The first life saving HLA matching procedure between a patient and her donors occurred in 1964 when Mrs B-L entered the hospital because of a chloramfenicol induced aplastic anaemia, bleeding from all orifices. We were able to treat her successfully with platelet transfusions from unrelated

platelet donors, at that time also a novelty (Fig. 5, p. 29). She had a good platelet recovery for about a month and then formed antibodies against leukocytes (she had been pregnant) and became totally transfusion resistant. Fortunately she had a large family. We were able to find brothers and sisters, which were HLA compatible and could act as platelet donors. In this way she became the first patient whose life was saved by HLA. This was an important case history illustrating the importance of donor selection and cross matching[18].

Of course we and others had speculated whether HLA was, like H-2 in the mouse, the major histocompatibility system in man. To prove or disprove that, following the work in the mouse, we had started doing skin grafts in humans. I gave of course the good example, and received in two sessions three grafts, but our colleagues and even the technicians collaborated as well. The request came for some of them as a shock, but after an extensive explanation most of them complied. Others did similar experiments. One of them, my friend Ruggero Ceppellini in Torino, came with the request for volunteers for these skin transplants at the end of his last lecture to students shortly before their examination on the immuno-genetics of HLA. This was all long before medical ethic committees were started! We used the split skin technique as developed by Felix Rappaport. We could indeed show that the HLA antigens were transplantation antigens[19].

After a meeting in Lugano on the way back from Zurich to London I discussed with Michael Woodruff, the transplant surgeon, our findings. He became quite exited and asked me to come to Edinburgh to type his patients who had received a kidney transplant from a family member. And so we did. After Edinburgh we typed the patients in six other transplant centres in Europe and the USA including Tom Starzl´s patients in Denver. He was at that time quite enthusiastic about HLA matching. We could indeed show that the survival of a kidney donated by an HLA identical sibling was excellent, while an incompatible graft did significantly less well[20]. On the basis of these findings we proposed to start the first international organ sharing operation – Eurotransplant[21]. Eurotransplant is now functioning near 45 years and is still going strong. We have shown – from the beginning – that HLA matching between donor and recipient does not only improve graft, but also patient survival. In this way Eurotransplant has saved thousands of patient's years[22].

The start of Eurotransplant marked a new phase in the history of Medicine as well. Up to the 19th century Medicine had been very much a one man affair. That changed when surgery became of age, which requested that a team of MD's worked together to help and treat one patient. Those working in and with Eurotransplant take this collaborative effort one step further. Two teams of surgeons, anaesthetists, internists and other staff work together with the Eurotransplant staff to help one patient! The logistics require a foolproof protocol, communication on an international level and an unselfish approach.

Shortly after the start of Eurotransplant a four-month-old baby, Johan M., was admitted to the paediatric department suffering from a severe combined immune deficiency with a life threatening candida infection. We had been involved in setting up a special unit to treat radiation accident patients and were able to save Johan´s life by a successful stem cell transplant from his HLA identical sister[23]. He was the first long-term survivor after receiving a stem cell transplant in Europe. More or less at the same time that Bob Good and co-workers transplanted successfully two immune deficiency patients in the USA[24]. Like us, Good used the protocol which van Bekkum and his group had developed in Holland. Johan is alive and well almost 40 years later and lives with his family near Leiden. These three successful transplants was the beginning of stem cell transplant activity in Europe and elsewhere.

We soon realised that only a third of the patients had a suitable HLA identical family donor. This led to the start of the first international registry for stem cell donors, Europdonor[25]. Here the start was much slower than that of Eurotransplant. The concept of a registry of unrelated stem cell donors became only a reality when Shirley Nolan started what would become the Antony Nolan foundation.

It took another ten years before others followed the example of Europdonor and the Nolan. At the end of the eighties there were 8 registries; 7 in Europe and the NMDP in the USA.

This proliferation created a problem. If you needed a donor you had to contact all 8 registries.

Obviously the solution is to have a central listing of all HLA phenotypes of all 8 registries and that is what Bone Marrow Donors World-wide

(BMDW) is doing. After consulting BMDW only the registries, which might have a suitable donor are contacted, in this way saving time.

The logistics are simple: The encrypted file with all the available donors of all registries is send every month to Leiden, the data are checked and then the file can be consulted with a password on Internet by all collaborating registries and accredited transplant centers.

We formulated our proposal and had the first meeting with the 8 registries in Leiden in February '89, and BMDW became a reality[26]. Since then others followed suit and in 2003 over 70 different registries and cord blood banks have joined Bone Marrow Donors World-wide, which can be during the years BMDW consulted on Internet. The increase of donors since 1988 is really impressive; from of a few hundred thousand donors in 1989 of which only 6% was A-, B- DR and DR typed, we have at the end of 2010 at near 15 million donors, of which 53% are not only typed for HLA-A and -B but also for -DR. A gain a true global effort. About half of the donors are located in Europe and Asia-Oceania, the other half in the USA.

HLA and Tolerance

I would like to end my review with discussing the possibility that HLA could play a role in tolerance induction. This line of thought started with some preliminary observations by Peter Morris and significant findings by Opelz and Terasaki[27]. They demonstrated that although pre-transplant blood transfusions (PTB) can immunize, overall, patients who have received a PTB fare better than those who had not. In a follow up study Lagaay observed that if the PTB donor and recipient shared an HLA-DR antigen, graft survival was significant improved, but if they were HLA-DR mismatched, graft survival was similar to that of non-transfused patients. We call such PTBs HLA-DR or haplotype-shared transfusions (HST)[28].

These findings have been confirmed and extended in heart transplant patients miniature pigs, monkeys, and mice. Importantly such Haplotype shared transfusions hardly induce antibody formation.

Fortunately only few people need a pretransplant blood transfusion, but the immune system of all of us has been confronted during foetal life and

shortly thereafter with the alloantigens on the cells of our mothers. This encounter can have a life long impact on the immune response.

To facilitate the discussion of this phenomenon we use a new nomenclature, summarised in Fig. 6 (p. 29). In a family we can recognize four haplotypes; two from the mother and two from the father. The patient in need of a transplant has inherited one from the mother and one from the father, the inherited maternal and paternal antigens (IMA and IPA); the other two are not inherited: NIMA and NIPA. We were alerted to the possible importance of exposure to the NIMA's during pregnancy by the analysis of the sera of hyper immunised patients on the waiting list of Eurotransplant with a panel reactivity of 85% or more. These sera were studied to identify so-called "permissible mismatches". Permissible antigens are HLA antigens to which these hyper immunized patients had not formed antibodies. We found that the crossmatch of the serum of these patients with cells, which were only mismatched for the patients NIMA's was negative in about half of the cases, whereas it was almost always positive with cells mismatched for their NIPAs[29].

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The NIMA concept was originally well received; but when it became clear that maternal grafts had a similar survival as those donated by the father, interest waned[30].

The concept that exposure to the NIMAs might have a lifelong influence on the immune repertoire gained new credibility thanks to a publication bt Burlingham et al. Not only the parents but also haploidentical siblings can be mismatched with the recipient for either the NIMA or the NIPA (Fig. 6). This makes it possible to study the impact of the NIMA and NIPA mismatches on graft survival in these haploidentical siblings. In this multicenter study by Burlingham et al. a striking improvement was found with renal grafts from haploidentical siblings mismatched for the NIMA haplotype but sharing the inherited paternal antigens IPA with the recipient[32].The graft with the NIPA mismatch do as poorly as those from the parents. The graft from NIMA-mismatched haploidentical siblings does as well as those from HLA-identical siblings. These findings are interesting but puzzling. It is as yet unclear why a graft from the mother fares less well

than that of a haploidentical sibling, although they both are mismatched for the NIMA antigens. Because they differ for the haplotype they share with the recipient (IMA versus IPA), it is possible that this might cause the difference in outcome. That Burligham´s findings are due to chance has become unlikely, because a similar phenomenon has been observed in a study on the impact of the NIMA effect on the occurrence of Graft versus Host disease (GvHD) after haplo-identical stem cell transplantation; Haploidentical transplants donated by siblings showed less GvHD than those donated by the mother[33].

Furthermore in two recent publications evidence was presented that by taking the Cord Blood NIMA into account a mismatched Cord Blood unit might not only have an improved survival and possibly a reduced relapse, but might be able to provide an optimal survival to many more patients with different HLA phenotypes as well[34].

In brief 1121 patients with leukemia received a zero (6/6), one (5/6) or two (4/6) antigen mismatched CB transplant. Those grafts in which one of the mismatched antigen(s) in the recipient was identical to the CB's NIMA was analyzed separately and identified as "1 (N=25) or 2 (N=54) HLA mismatch – 1 NIMA match". No 2 HLA mismatched grafts with 2 NIMA matches were available for analysis.

Transplant related mortality (TRM) was reduced (p= 0.034, for patients >10 years p= 0.012), as was neutrophil take (p= 0.043, for CB units with < 2.5 × 10\7/KG p= 0.031), while one antigen mismatched but NIMA matched CB grafts showed a trend to reduced relapse (p= 0.07). The latter finding is in accord with unpublished observations of a previous IBMTR study (p= 0.10)[33] and was recently confirmed[34].

Preliminary evidence was given that by replacing CB antigens by NIMA antigens and in this way creating new or "virtual phenotypes" the number of these would increase 18 fold if only one or two mismatches were thus analyzed. Obviously some of these phenotypes will be duplicates of existing phenotypes.[33][34]

The NIMA effect provides us with a completely new tool to study and to modulate the allograft reaction with blood transfusions, peptides, or cytokines. It might provide us with an answer why some mismatched grafts survive so well.

The excellent outcome of NIMA mismatched haploidentical sibling transplants clearly indicate that immune modulation if done correctly can permit ignoring HLA mismatches – a full haplotype – and can facilitate graft survival that is near equal to that of HLA-identical grafts. The study of Burlingham et al. and on the effect of NIMA on Cord Blood transplant outcome give thus new credibility to the importance of the NIMA effect.

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Many questions remain unanswered, as long as the mechanism by which the NIMA effect can manifest itself is unknown. Furthermore, much more information should be collected on the role of the NIMA antigens in unrelated organ transplantation and to what extent haplotype shared transfusion's or (better) HLA peptides could boost this effect. The challenge remains to find out how important it is or can be made in the clinical situation.

Concluding Remarks

And this is my story about the discovery of HLA and our quest to understand how the information it provides can be best used in the clinic. It has been a fascinating experience. For me the main lesson has been how great the impact is of the encounter of our immune system with allogeneic cells during pregnancy and breast feeding on the further education of our immune repertoire. It is therefore in my eyes surprising that this has received so little attention in the past. Pregnancy has a life long effect, not only on the mother, but also on the child!

In the discovery of HLA women have played a central role, not only because they provided us with the reagents to unravel the genetics of HLA or because they gave us an insight how nature handles tolerance, but also because their enormous contribution scientifically. I mentioned already Rose Payne, but Aad van Leeuwen, Els Goulmy and Julia Bodmer should here be recognised as well, together with many others. Finally serendipity plays always an important role in the making of a discovery, but in developing the tools to study the genetics of HLA its impact was really enormous.

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Figures

Fig. 1: Agglutination pattern of 34 sera with leukocyte antibodies tested against 100 leukocyte samples. Each vertical column of squares and hyphens shows the results obtained with one serum against the panel; each horizontal row shows the results of the 34 sera with one leukocyte sample.
■: Agglutination positive; -: agglutination negative. Doubtful results were recorded as agglutination negative.

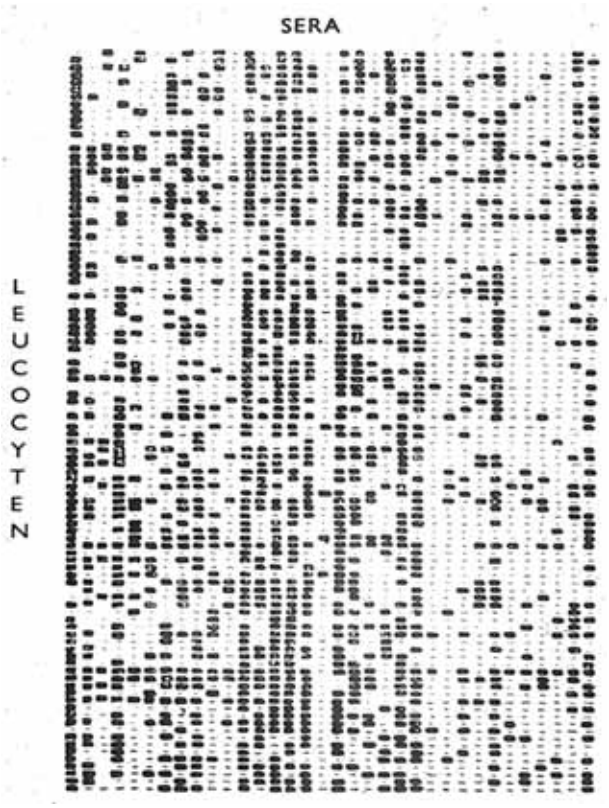


Fig. 2: Agglutination pattern of eight sere with the leukocytes of the panel. Each vertical column of squares and hyphens shows the results obtained with one serum against the panel; each horizontal row shows the results of the eight sera with one leukocyte sample.
■: Agglutination positive; -: agglutination negative. Doubtful results were recorded as agglutination negative.

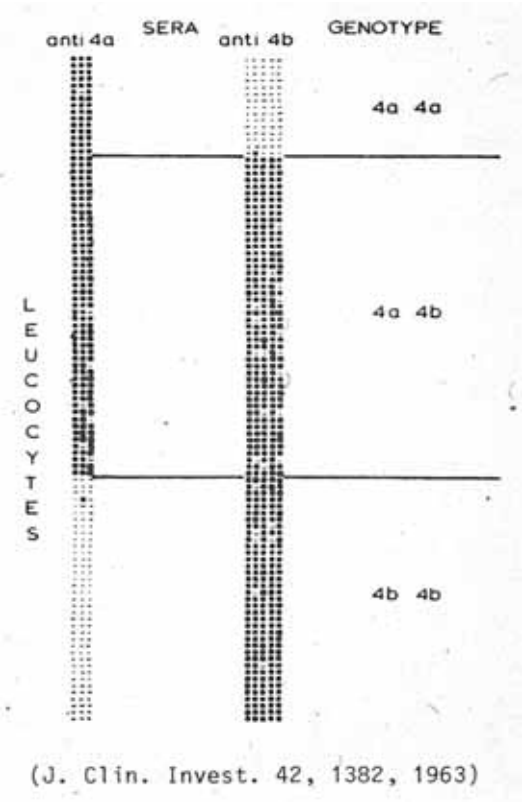


Fig. 3: I show Rose Payne and Paul Terasaki the first workshop computer printout ever.

Fig: 4. Part of the letter send to the Pope in 1970.

Pope Paul VI
Vatican City

AZL

23 March 1970

Your Holiness,

we are at the moment involved in the study of children suffering from a congenital dysfunction of their immune system. To cure these children we need the collaboration of individuals with a rare combination of tissue groups.This rare combination occurs in children born out of a cousin marriage. For that reason we ask You respectfully to allow us to consult the files of cousin marriages……..

Fig. 5: Successful treatment of a thrombocytopenic patient with repeated platelet transfusions, first from random and later from HLA compatible sibling donors.

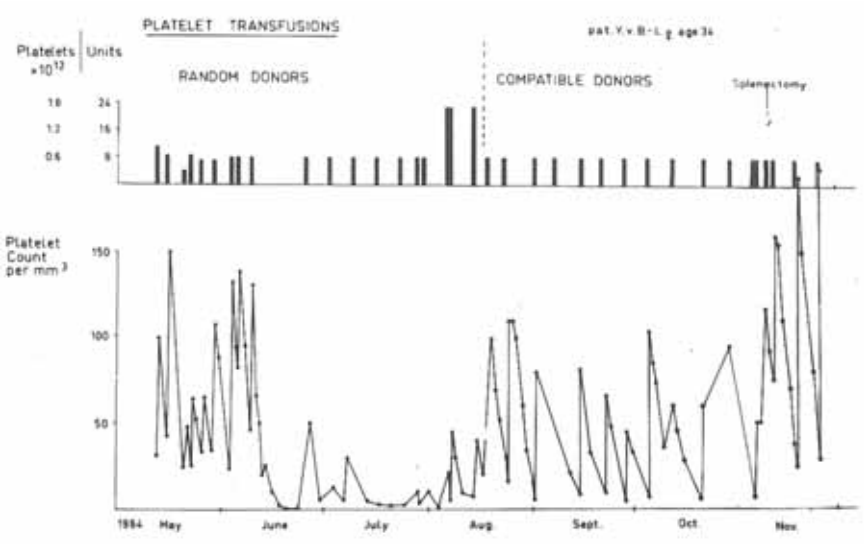
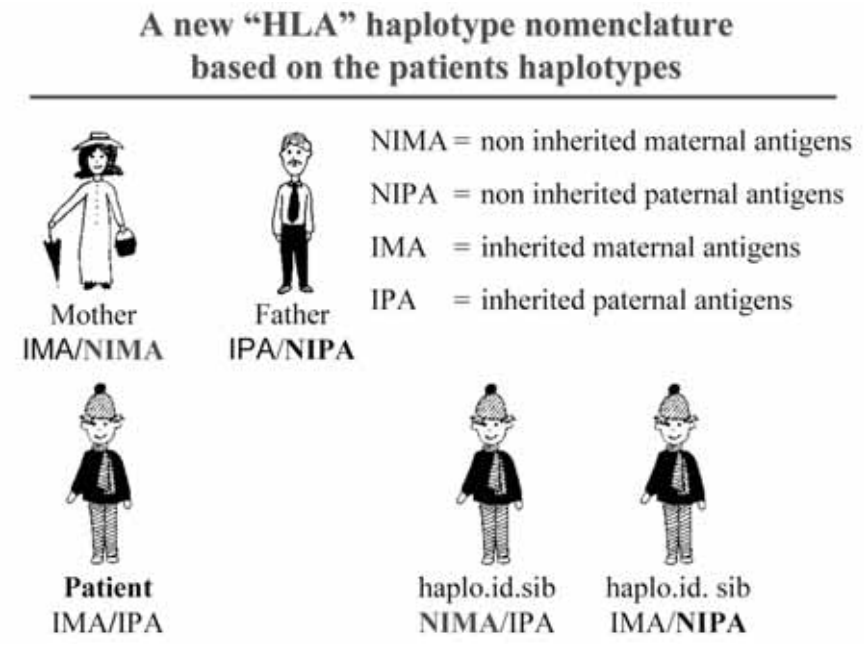


Fig. 6: A new nomenclature to describe the genetic relationship between mother and Child.



Laudatio James E. McGuire

(lecture on Thursday April 28, 2011)

Maarten Van Dyck

Professor McGuire's first major paper, which he had co-written with his then colleague at Leeds University Piyo Rattansi, came out in 1966 and carried the somewhat enigmatic title "Newton and the 'Pipes of Pan'". At this point he had been lecturer in history and philosophy of science at Leeds University for some years, after having studied at Oxford University, King's College in London, and the University of Western Ontario. He would stay in Leeds until 1971, when he joined the department of history and philosophy of science at Pittsburgh, where he is still very active as professor emeritus today. Both in Leeds and in Pittsburgh he was part of remarkably strong groups of historians of science. In this respect, his distinguished career testifies at least to the partial fulfilment of George Sarton's lifelong struggle to get history of science recognized as a fully autonomous discipline, with strong institutional backing. And it needs no mention, our being gathered here today to honour this distinguished career is also a sign of recognition of the discipline itself. That we gather in this magnificent old auditorium moreover shows that such recognition often goes hand in hand with a number of *traditional* measures, such as offering a symbolic "chair" and a medal, or wearing these very otherworldly academic gowns. Now these are of course merely symbolic gestures, and as such could be considered contingent and even arbitrary, but they arise from a more essential impulse: they testify to the fact that a scientific discipline is partly constituted by the relation it takes up to its own history – by the specific ways in which it itself becomes a *tradition*. In saying this, I am actually merely

paraphrasing George Sarton himself, who opened his text entitled "Science and Tradition" as follows: "The title of this group of lectures … is paradoxical. It would seem natural to twist it a little and instead of saying Science and Tradition, to say Science versus Tradition. Indeed, the two terms are to some extent antithetical. The word tradition suggests preservation and continuity; on the other hand, science is the most revolutionary force in the world." (Sarton 1952, p.3) But he goes on to argue: "Far from there being any conflict between science and tradition, one might claim that tradition is the very life of science." (*ibid.*, p. 11) This, I believe, was the true core of Sarton's vision of the necessity of the discipline of the history of science. It is only through history of science that science can reconnect with its own essence and *telos*.

A very similar vision seems always to have animated professor McGuire's work in the history of science. It is no accident that the collection of his major papers on Isaac Newton was called *Tradition and Innovation*. And in *Science Unfettered,* a book that he published in 2000 together with Barbara Tuchanska, the constant dialectic between tradition and innovation is analyzed in detail using the philosophical framework of a hermeneutic ontology. Not only has his own historical and philosophical work been constantly guided by this perspective, it was also a topic that greatly occupied the mind of his prime object of study: Isaac Newton. No doubt, almost everybody present here is familiar with the famous saying by Newton that he had been able to see a little bit further than people before him, but only because he was standing on the shoulders of giants. Now, George Sarton, who had written his PhD-dissertation here in Ghent on Newton's mechanics, took a particular interest in this saying, and tried to trace its earlier history, a work that was further elaborated on by his student, the great sociologist and recipient of the very first Sarton Chair, Robert K. Merton. But it is professor McGuire who more than anybody else has shown how fundamental was Newton's commitment to the tradition of which he considered himself to be merely the last in line. In the 1960s McGuire discovered among the Newton manuscripts a series of documents that proved that when working on a second edition of his landmark *Principia,* Newton delved deeply in ancient sources, looking for evidence that rather than charting absolutely new territories with his theory of universal gravitation, he was actually rediscovering ancient wisdom – and he found

such evidence too, as he pointed out that when the Ancients talked about, and now I quote Newton, "the God Pan's playing upon a Pipe and attributing musick to the spheres" (McGuire and Rattansi 1966, p. 118), they were obliquely referring to the mathematical law of gravitation that was instituted by God in creating the world. Not very convincing evidence, we would be tempted to think, and it is clear that it depends on an already firmly held belief in the existence of a golden age in the past of human culture, with the knowledge of our modern age by definition being nothing but an attempt at recovering what had been lost since. As McGuire and his co-author pointed out in great detail in their paper on "Newton and 'the pipes of Pan'", Newton was not an exception in his time in holding this idea. One example, not mentioned by them, was the Flemish mathematician and engineer, Simon Stevin, about whom George Sarton wrote a long article in 1934, in which he commented as follows on Stevin's ideas on the so-called *Wysentyt* (the "Age of the Sages"): "The idea of a primordial golden age is one of the oldest conceits of the human mind. … The fact that such strange ideas may be found alongside others of the purest scientific kind is but another illustration of the infinite complexities of the human mind. No man is always consistent, certainly no man of genius." (Sarton 1934, p. 260) It is at this critical juncture that professor McGuire, from his very first publication onwards, has consistently chosen another perspective than did Sarton. Rather than distinguishing between purely scientific ideas and strangely inconsistent, mythical addenda to these, he has always attempted to excavate the underlying nexus in which these apparently very different ideas were intrinsically tied together for Newton. After his piece on the "pipes of Pan", McGuire engaged in a sustained enquiry in the fine-structure of the central concepts of what he has called Newton's 'metaphysics of nature'. In a series of groundbreaking papers he showed how Newton's understanding of the basic concepts of his natural philosophy, such as space, time and force, can only be fully illuminated by taking into account his theological preoccupations. Newton's view of God as an omnipotent creator, first and foremost characterized by his absolutely free will, was essential in sustaining his natural philosophy, including his theory of universal gravitation. Now, it is one thing to state this as a general claim, it is quite another thing to convincingly fill it out in every detail, using new manuscript evidence along the way. It is obviously the latter that Professor McGuire has done. Each of the papers collected in his *Tradition and Inno-*

vation is a small masterpiece of conceptual analysis, laying out the intricate relations between crucial Newtonian concepts. Taken together they are widely recognized to be unsurpassed until this day. In their combination of archival research, textual interpretation, contextual embedding, *and* a full understanding of the mathematical and empirical content, they show the kind of exciting results that a fully established discipline of history of science can give rise to. So while in a number of respects the resulting picture differs crucially from Sarton's views on how the history of science should be written, it can only do this because it exemplifies the notion that was dearest to his heart: that of progress. And here more specifically, progress in the history of science itself; progress that is only possible because it has become a scientific discipline in its own right.

Full justice is not of this world; hence I will not expand on the rest of professor McGuire's extensive work, such as his recent book on René Descartes, published together with his colleague Peter Machamer, his work on rhetoric and science, or his recent and as yet unpublished studies on ancient philosophy. I will also not list his students who made their own name not only in the field of history and philosophy of science, but in other fields as well. I will not do all this, but I do want to close with a few remarks that start from the wide range of interests characterizing professor McGuire's work and personality. I have stressed the importance of the coming of age of history of science as an autonomous discipline, but for Sarton that was always a means, not an end. The end was what he called a "new Humanism", and this humanism was antithetical to excessive specialization, the danger always lurking behind the necessary professionalization of research. History of science would be exactly a privileged place for integration and unification of insights from different fields; a place where our culture could come closest to a satisfactory interpretation of itself. Our humanism cannot and need not be that of Sarton, who wrote for his times, not for ours. But we can still uphold the ideal that history of science could play a crucial part in constituting who we are and want to be at the beginning of the twenty-first century. And it is here that we can find professor McGuire's work on the metaphysical and theological infrastructure of Isaac Newton's new science of the utmost importance and relevance. Not because it would allow us to directly answer any of the pressing questions concerning the possible place of religion in our contemporary worldview,

which to a large extent is shaped by scientific theories that are often inspired by Newton's; but maybe because it could help us to formulate better questions. In teaching us to understand Newton's thought in a historically and contextually sensitive way, professor McGuire's work also forces us to grapple with the historicity of our own thought. In the activity of interpreting past science, we ourselves are always essentially involved. Or to put it for the last time in Sarton's words: "If the past were not part of your present, if it were not a living past, it would be better for you to leave it alone." (Sarton 1920, p. 6) We should all be grateful to professor McGuire for having shown how to make so much of seventeenth and eighteenth century science into a living past, one that from now on we can leave alone only at our own peril.

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Ideas and Texts: Newton and the Intellectual History of Science

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Today I reflect on my career in the History of Science. My chief concern is texts – printed or in manuscript – which record the beliefs of by-gone days. The approach I advocate orients itself towards ideas in textual settings and their effect on cultures and societies. I am, in other words, an intellectual historian. Let me contrast the way I do intellectual history with the approach I reject. It reifies ideas and transforms them into Platonic realities. As a result, the goal of intellectual history becomes an exercise in showing how particular ideas are historically embodied in time and place. And only ideas, Platonically reified, are recognized as the proper unit of historical analysis.

According to the approach I endorse, an historian may concentrate on ideas, or on some cognate notion, but this need not reify ideas or make them the sole unit of analysis. To conceive history as an interplay of ideas marching dialectically towards a final end is one thing; it is quite another to treat ideas as acts of conceiving and understanding in which humans indulge. In this way we can study ideas without invoking an indefensible notion of historical understanding. If we abandon ideas conceived Platonically and consider human reasoning in *textual* settings, our relation to historical texts will be dialogic, it will be an interplay of questioning and answering. We may ask what motivates a text; ask why an agent reasons thus; ask towards whom the reasoning is directed; and ask whether it's effectual in achieving its goals.

To approach ideas dialogically is to view them as containing the residue of human thinking once present dynamically in the give and take of communication. Viewed in this light, the historian's task is to reveal the aims and goals of the cognitive processes now dormant in the texts before him. But to engage in this task, the historian cannot rely on what's inscribed in the texts alone. These artifacts must be placed in an appropriate setting; their source ascertained; their linguistic features considered and compared with related texts; their place in the corpus of a thinker or group of thinkers established: and finally, how well they present their case evaluated. In short, the intellectual historian must generate a dialogic relationship with texts at his disposal. In fact, he needs to cultivate what I shall call textual understanding.

Clearly, the sort of intellectual history I endorse demands specific skills and abilities. Skills for analyzing and criticizing ideas; skills for investigating what a text is telling us; the ability to relate one's learning to textual issues at hand; a reconstructive imagination akin to a writer or creative artist; and, lastly, the ability to engage critically belief worlds entirely different from our own.

Here, then, is the agenda of my talk. It falls into three parts. First, I consider textual understanding and its role in intellectual history. Next, I illustrate textual understanding by analyzing some Newtonian texts concerning God's relation to space. Lastly, I briefly consider some wisdom I have learned as a practicing historian of ideas.

First, what is textual understanding and what's its role in intellectual history? I begin with a meta-level reflection on what it is to understand. Hans-Georg Gadamer reminds us of a parallel between the understanding that emerges from the give and take of conversation and that which comes about when an interpreter interacts with a text.[1] Just as each interlocutor in a conversation attempts to reach an agreed understanding of a *subject matter,* so the interpreter of a text tries to understand its truth claims. Hence, to understand a text is to understand the question to which it is the answer: that is, we must discover the question the text answers, if we are to understand the text *as* an answer.[2] The text, so conceived, far from being a

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1 Hans-Georg Gadamer, *Truth and Method* 2nd edition, (The Continuum Publishing Company, New York, 1994), Part II, 2, pp. 362-369

2 Gadamer, *op.cit.* Part two, section 3, 369-379.

repository of fossilized statements, is seen as a dynamic reality posing questions. Questions posed open up and constrain the possibilities for understanding the textual answers to which they have directed us. Furthermore, and most importantly, we must accord normative authority to these answers and remain open to the possibility that the text is telling us something coherent and true. But to understand them fully, these answers must arise from questions that we have put to the text from the perspective of our time and place.

In other words, textual *understanding* is always application. It is always grounded in, and constituted by, the concrete, temporal-historical situation in which we, the interpreters, find ourselves. But textual understanding also situates itself within an interpretative tradition which we have actively appropriated. This in turn sets the normative framework that directs our questioning of a text. Here again the conditions of conversation nicely clarify textual understanding. In a genuine conversation each partner is concerned equally with the subject matter, desiring to reach a mutual understanding of it.[3] But the understanding achieved, while mutual, is not the original position of either interlocutor; on the contrary, it is an emergent understanding which transforms their initial positions. Textual understanding has a similar structure. Here too the 'truth' of the subject matter is at issue. By means of a questioning process the claims of the text are taken seriously, and, by testing our understandings of the past against these claims we reach a new understanding of text's subject-matter. Thus, textual understanding involves acts of appropriation which have been integrated into our understanding of the subject matter. Appropriations constitute that mediation between past and present and the alien and familiar that is part of any sincere attempt to understand. The horizon of the text is therefore 'fused,' and brought into productive alliance with the interpretative horizon we bring to the event of understanding. In this sense, textual understanding is always an open-ended and transformative process: texts have an inexhaustible capacity to reveal new depths and meanings, seldom disclosing all they are on a first reading. This way of doing intellectual history has informed my approach to the understanding of texts throughout my career.

I have said that intellectual history is an exercise in understanding texts dialogically. To make this concrete, I turn to the second part of my talk –

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3 Ibid. Part Three, section 1, 383-389

Newton's views on the relation of God and space. This will provide content for my contention that the practice of intellectual history is a dynamic interplay of ideas and texts conducted by a process of question and answer.

In the General Scholium or note to second edition of the *Mathematical Principles of Natural Philosophy* (1713) Newton says of God (1) "He is not eternity and infinity, but eternal and infinite; he is not duration and space, but he endures and is present. He endures always and is everywhere present, and, by existing always and everywhere, he constitutes duration and space."[4] Notice that Newton's understanding of the *origin* of space and time is in terms of a spatio-temporal theology of Divine existence. There is no time at which God can fail to exist and no space with respect to which God can fail to be present: God is always and everywhere. Thereby, God is co-eternally present with time and space. The claim that God is not identical with space and time is clear enough. But how do we understand the claim that space and time are 'constituted' by God? To provide an answer, I shall consider what Newton says about God's relation to space in two important texts.

The first is *De gravitatione et aequipondio fluidorum*. It's dating is controversial. The part I want to consider probably dates from the early 1680s. After criticizing Descartes's views on space and motion, Newton presents his own conception of space.

(2) Space is an *affection* (affection) of being just as being (*entis quantenus ens*). No being exists or can exist which is not related to space in some way. God is everywhere, created minds are somewhere, and body is in the space it fills; and what is neither everywhere nor anywhere does not exist. And hence it follows that space is an emanative effect (*emanativus effectus*) of the first existing being (*entis primario existentis*): for whatever being is posited, space is posited.

Much here needs clarification. But the basic claim is clear enough: things exist in so far as they bear a specific relation to space. We are also told that (3) "space extends uniformly to infinity in all directions…" This is what we should expect: if God is everywhere the space to which God relates must itself be infinite. In another text Newton makes this relation explicit:

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4 Isaac Newton, *The Mathematical Principles of Natural philosophy* [1687] Translated and edited by I. Bernard Cohen and Anne Whitman assisted by Julia Budenz (University of California Press, Berkeley and Los Angeles, 1999) p. 941

(4) "space is eternal in duration and immutable in nature… because it is the emanative effect of an eternal and immutable being"[5] Notice first that the phrase "being just as being" in text (2) doesn't name some mysterious entity called 'being.' It refers to what exists just in so far as it exists: or, put otherwise, it pertains to anything *in virtue of the fact that it exists*. For Newton a feature of each and every being is its relation to space. So gathering texts (2), (3) and (4) together the interpretation I defend is this. The space to which beings pertain in virtue of their existence is infinite. Since this is so space cannot be an affection of just any being. It must be an affection of an infinite being. This being can only be God, the eternal being on whom all beings depend for their existence.

Text (2) has been otherwise interpreted. The second clause of the last sentence has been interpreted as telling us what the sentence itself means. The sentence is then read to say that "space is an emanative effect of the first existing being; [for if I] posit any being whatsoever [I] posit space."[6] On this reading, space is simply an 'emanative effect' of just *any* "first existing" being: so no matter what being is posited, space is posited. However, this does not do justice to the text as a whole. It does not address its central concern, namely, the infinitude of space and its relation to God.

To see the problem with this reading, consider two semantic issues. First, how is the phrase "emanative effect" to be understood? Second, what does Newton mean by saying "space is an *affection* of being just as being."? Henry More, Newton's contemporary, defines an "emanative effect" as what is co-present with its cause. It is "such a cause as merely by Being, no other activity or causality interposed, produces an Effect."[7] This is precisely how Newton understands the notion. For him, too, an "emanative effect" is an effect co-present with its cause. So the claim in (2) that space is "an emanative effect of the first existing being," and in (4) that it is "the emanative effect of an eternal and immutable being," mean, when taken together, that God and space are co-eternally present. Thus, the phrase "the

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5 Isaac Newton, *De gravitatione et aequipondio fluidorum* in *Unpublished Scientific Papers of Isaac Newton*, translated and edited by A.R. Hall and M. B.Hall (Cambridge: Cambridge University Press, 1962) p. 103

6 Howard Stein, "Newton's Metaphysics" in *The Cambridge Companion to Newton* ed., by I.B. Cohen and G.E. Smith (Cambridge: Cambridge University press, 2002) 256-307 See pp 268-9. Andrew Janiak, *Newton as Philosopher* (Cambridge; Cambridge University Press, 2008)

7 Henry More, *The Immortality of the Soul* [1659] (Bristol: Thommes Press,1997) Bk 1, Cap.6, 32-33

first existing being," cannot refer to just any first being. It must refer to the 'primary' being, God, the being on whom all other beings necessarily depend. Hence only God can be the source from which infinite space 'emanates,' the being whose eternal power is necessary for sustaining co-eternal space. Those who place the interpretive weight on the closing sentence of (2) violate the dictum that a sound interpretation should reciprocate between part and whole and whole and part. By privileging the closing sentence they make sense neither of the text itself nor of how it coheres with texts (3) and (4).

But the phrase "whatever being is posited space is posited" still remains. How does it fit my interpretation? After all, it generates the interpretation I reject. It comes to this. Since space is infinite, uncreated and eternal, it exists independently of created things. Accordingly, infinite space cannot be accounted for if, as the interpretation I reject maintains, we take Newton to mean that positing just any being posits space. This is borne out if we consider Newton's atomistic commitments. For atomism, space is an infinite and uncreated void, the 'receptacle' that houses created beings. To say that space and beings are posited together is to say that the latter exist in relation to an uncreated 'receptacle' space. In short, to say any being posits space simply means that created beings cannot exist without being related to an *independently* existing space. Thus, "God is everywhere, minds are somewhere, and body is in the space it fills." The text does not mean that any being whatsoever, by dint of its sheer presence, posits the space of its own existence. But this, I think, follows from the interpretation I reject. The integrity of the texts is preserved only if the positing relationship between space and beings is interpreted as that which obtains between an infinite space and an infinite being.

I turn to the second semantic issue. How do we understand Newton's claim that space is an *affection* of being, especially of God's being? In 17th century usage the term 'affection' denotes an attribute or property of something, or its state or condition. If Newton is taken to mean by affection that space is a *property* of "being just as being," this would make God liable to divisibility, in contradiction of the traditional conception of Divine immutability. Furthermore, to claim that space is an 'emanative effect' of God, relates space to Divine omnipresence as an effect relates to its cause. But an effect is distinct from its cause. How, then, can space be an affection of

God's being if it is distinct from that being? In short, how does Newton square his causal discourse with his affection discourse? Here Newton deploys another distinction: he distinguishes extrinsic from intrinsic affections.[8] Being in space and time are extrinsic affections or states of things that pertain to their existence. In contrast, properties are intrinsic affections that inhere in things. Thus, space and time are not in things like shapes or sizes, nor do they contribute to the sorts or kinds by which things are defined. They pertain to things only in so far as they exist actually, where to exist actually is to relate to space and to endure through time. Newton states this succinctly in an unpublished text dating around 1692-93. We are told that space and time are (5) "common affections of all things without which nothing can exist…what is never and nowhere is not in *rerum natura*."[9] The claim is clear. Things presuppose space and time in order to exist as an actual instance of a sort or kind. Otherwise they are "never and nowhere." God too exists spatio-temporally; but does not exist as an instance of a sort or kind. On the contrary: God is everywhere and always and space and time are consequences of God's omnipresence.

But viewing space and time as common affections of things does not end the story. Let us consider space. In *De gravitatione* Newton tells us unhelpfully that space "is more thing-like than an accident, and more closely approaches the nature of substance."[10] Later in 1719, he gives a more nuanced answer. This appears in an *Avertissement au Lecteur*, sent to Des Maiseaux in 1719 for inclusion in the latter's 1720 edition of the *Clarke-Leibniz Correspondence*.[11] In reading Samuel Clarke's correspondence with Leibniz, Newton became dissatisfied with Clarke's sloppy use of the terms "quality" and 'property" when characterizing how God relates to space and time. There are five drafts of the *Advertissement* in Newton's hand. I quote from draft B:

(6) The reader is desired to observe, that whenever in the following papers through unavoidable narrowness of language, infinite space or immensity & endless duration or eternity, are spoken of as Qualities or Properties of

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8 Johann Magirus, *Physiologia peripateticae contractio* (Cambridge, 1642) Lib. 1, Cap. 4, 57 and Cap. 8, 92

9 J.E. McGuire, "Newton on Place, Time, and God: An Unpublished Source" *British Journal for the History of Science*, 11, pp. 117

10 Ibid., Note 5, 99

11 A. Koyre and I.B. Cohen, "Newton and the Leibniz-Clarke Correspondence" *Archives internationals d'histoire des sciences* 15 1962

the substance wch is Immense or eternal, the terms Quality & Property are not to be taken in that sense wherein they are vulgarly, by the writers of Logick & Metaphysiks applied to matter: but in such a sense as only implies them to be modes of existence in all beings, & unbounded modes & consequences of the existence of a substance which is really necessarily & substantially Omnipresent and eternal: Which existence is neither a substance nor a quality, but the existence of a substance with all its attributes, properties & qualities, & yet is so modified by place & duration that those modes cannot be rejected without rejecting the existence.[12]

This closely reasoned text offers an interpretive challenge. I can best illustrate this by contrasting how I understood the text in 1978 with how I understand it now. This will give further substance to my remarks on dialogic understanding and the fact that interpretation is a never ending process.

I begin with a précis of my 1978 interpretation.[13] I noted then that the text warns us against taking the terms 'quality' and 'property,' in reference to God's relation to space and time, in the manner in which they are predicated of matter. Indeed, Newton tells us that space and time are *modes* of the *existence* of all things, and in reference to God that they are "unbounded modes & consequences" of Divine existence. Next I noted that Newton distinguishes 'modes' from 'qualities' and 'properties.' The term 'mode' refers to the manner of something's existence; whereas properties say something about what it is. Prompted by these understandings, I construed the text to say that space and time make reference to something's *existence*, God's included, but not to any of the intrinsic characterizations which makes something what it is.

Well and good. But then I went astray. I distinguished two sorts of predication. One type, a first-level predication, characterizes properties that belong to something as the sort or kind that it is. The other type, a second-level predication, does not contribute to the sort or kind that things are: it denotes only their existence. I then advanced the following interpretation. On the one hand, I took the text to deny that existence is a first-level pred-

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12 Ibid., 96-97

13 J.E. McGuire, " Existence, Actuality and Necessity: Newton on Space and Time." In *Tradition and Innovation: Newton's Metaphysics of Nature.* (Kluwer Academic Publishers, Dortrecht, 1995) 1-51

icate – one that attributes a real attribute, property, or quality to something – and, on the other hand, I took it to affirm that existence is a second-level predicate, one which refers only to the sheer actuality of things. Distinguishing predicates in this way fits with Newton's conception of finite things whose existence is not a necessary part of what they are. However, in the theological tradition Newton inherits, to exist is a perfection and thus belongs to Divine being necessarily. Interpreting the text as saying that existence is not a first-level predicate, as I did in 1978, violates the unity of God's being, and threatens to saddle Newton with the view that existence does not belong to God's necessarily! With this result, I broke two cardinal rules of interpretation. First, I failed to address the text as putting forth a coherent truth. Quite obviously my answer to the textual questions I posed threatens that coherence. Second, I failed to interpret the text as a whole, testing parts against whole and whole against parts. I took the sentence in which Newton speaks of space and time "… as only implies them to be modes of the existence in all beings" as the key that unravels the entire text. This, in turn, led me to conclude that space and time bear reference only to the *existence* of things. Clearly, in making this move I went badly astray.

Recently, I amended my interpretation.[14] I put my present self into dialogue with my former self and put a new question to the text. Although Newton does not use the term *propria*, I asked whether he was thinking of space and time as *propria* of God's substantial existence. *Propria* function as predicated properties but with an important difference. A *proprium* is not part of something's essence or defining nature, but it is compatible with the thing according to the way it is. Consider the sentence "man is able to laugh". Being able to laugh is not part of the definition of 'man'; nevertheless it is predicated *per se* of man as a *proprium*, because it signifies something standing for 'man,' and does so by invoking the "ability to laugh" which, though not essential to human nature, is nevertheless uniquely appropriate to being human. Newton seems to think of space and time in this way when he claims that God's "existence is neither a substance nor a quality, but the existence of a substance with all its attributes properties & qualities, & yet so modified by place & duration that those modes cannot be rejected without rejecting the existence". Understood as *propria*, space and time are

[14] J.E. McGuire and E. Slowik,"Newton's Ontology of Omnipresence and infinite Space." Forthcoming in *Oxford Studies in Early Modern Philosophy*

not predicated as intrinsic affections of God's substantial nature; on the contrary, they function as predications appropriate to God's way of existing as an eternal and omnipresent being. In other words, my new approach to the text avoids the issue that plagued me earlier, namely, whether existence is a first or second level predicate. This is borne out in Draft D, another version of Newton's *Advertissement*. There Newton says that to characterize God's existence by the terms infinite space and endless duration is not to ascribe a 'property' or a 'quality' to God as standardly understood. It answers the questions: where is God located (*ubi*), namely, 'everywhere', and when does God exist (*quando*), namely, 'always.' In contrast to this, finite things are located 'here' or 'there' and they endure 'today' or throughout 'last year.'[15] Thus, space and time are modes of things because they specify the where and when of their location. They function, then, as *external propria*, which refer to the sheer actuality of things. In this role, they pick out the conditions of spatio-temporal location which anything must satisfy, God included, if it is to be denoted an actually existing thing.

But what about the other key sentence of the text? Does my interpretation square with it? Newton cautions us that space and time are the "… unbounded modes & consequences of the existence of a substance which is really necessarily & substantially Omnipresent and eternal." Space and time are "unbounded modes" because they are infinite in nature. They are 'modes' of God's omnipresence because they depend, as external *propria*, on God's necessary being. Stated otherwise, they are the co-eternal effects of God's substantial omnipresence, itself the necessary foundation for their existence. By speaking of space and time as 'modes' of Divine existence, Newton avoids the Spinozistic connotations that lurked in his earlier use of the phrase "emanative effect."

My textual enquiries raise big historical issues. Many 17th century thinkers, apart from Newton and More, associate space and time with Divine existence. Why is this conception so prominent in the 17th century? To stress the spatio-temporality of created things is one thing; it is quite another to apply this notion to Divine omnipresence. Certainly, Medieval thinkers postulated infinite imaginary space by which to conceive the omnipresent immensity of God. But none ascribed dimensionality to imaginary space on

[15] Ibid., Note 5, 99

the pain of spatializing Divine nature. Francesco Patrizi in 1588 was the first to make Divine space dimensional possibly because he had appropriated certain elements of Stoic thought. But why Patrizi's way of thinking took root in the 17th century has yet to be answered.

You will be pleased to learn that I shall not tackle these big issues today. It would require me to expand my historical stage, and to ask questions that go beyond, but certainly include, textual exegesis. Appealing to my interpretive practices to illustrate textual understanding brings me, in the last part of my talk, to a topic at the core of textual understanding – the relation of innovation to tradition. As we have seen, Newton appropriated a theological tradition that conceives space and time to be related to Divine presence. His treatment of this conception of space and time is innovative and differs markedly from his contemporaries. For example, Henry More holds that God and spirits are incorporeally extended, and he identifies God and infinite space by making space one of God's Divine attributes.[16] This is precisely what Newton rejects. Interestingly, although More and Newton appropriate the same tradition their texts reveal how differently they respond. Earlier I said that textual interpretation is an act situated in a time and place and informed by tradition. This alerts us to the fact that historical texts are an amalgam of tradition and innovation. All thinkers begin within a tradition that informs their outlook, however much they may appear to reject it. At bottom the capacity to understand is tradition-impregnated. Tradition is the root from which understanding springs and provides even the radical innovator with an initial orientation towards the world. This means that innovation is never *creatio ex nihilo*, but always a transformation of ideas, always an act that transcends and yet continues what it finds. It means also that the criteria by which innovation is judged are themselves historical. Always/already we are in an historical horizon, in a situation in which historical conditioning is already in play.

But putting questions to historical texts yields understandings that go beyond the texts themselves. Historical studies based on textual exegesis help to generate compelling narratives of how we have become what we are. Such narratives provide perspectives we might not otherwise possess and illuminate dead ends down which we have gone. In closing, here are

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[16] Henry More, *Henry More's Manual of Metaphysics: A Translation of the Enchiridium Metaphysicum* (*1679*), Parts I and II, translated by A. Jacob, (Hildesheim: Olms, 1995)

some historical perspectives worth considering. It is futile to think that the contingencies of time and place are overcome by positing metaphysical entities supposedly immune to sweep of history. Human cognition is locked forever in time-bound worlds that structure our views of the past, the present and what is yet to come. The contingencies of time and place have consequences for our views of truth, of human knowing, and of the objectivity of what we know. If our norms and standards (both cognitive and social) are embedded in historical situations, what counts as truth, knowledge, understanding, and objectivity is largely constituted by possibilities inherent in those situations. Every cultural reality, science included, carries its own logic, its own values and norms, its own ways of carving up experience, its own dynamics of change, and must be judged accordingly. From an historical perspective there are no timeless truths, identical in all spheres of human activity – moral and political, social and economic, scientific and artistic. Hence, there is no ultimate method, based on reasoning alone, that will solve all basic problems and deliver one true theory. It is pointless, then, to seek for situation-independent truths to which a cognitive self, disengaged from tradition, is directed and to which cognition relates universally: there is no such self and there is no such truth. Both are illusions of metaphysical objectification and of the human need for transcendence. These perspectives do not, however, council despair. After all, we have one another and the power of human agency and the power of human imagination. If history is witness to what we have attained, it is also the harbinger of what we may yet achieve.

Well, you can choose. You can stand with Henry Ford who said famously: “History is bunk.” Or you can agree with the ancient saying: “The one who knows no history is doomed to live as the child.” I hope I’ve done something towards convincing you that the second choice is worthy of consideration.

Laudatio James E. McGuire

(lecture on Friday April 29, 2011)

Steffen Ducheyne

It gives me much pleasure to introduce to you, as one of the involved *collegae proximi*, our Faculty's 2010-2011 Sarton Chair Holder, Professor J. E. McGuire, who is professor emeritus at the History and Philosophy of Science Department at the University of Pittsburgh. As a follow-up to the official *laudatio* of yesterday, in my introduction to this afternoon's lecture I shall dedicate some words to Professor McGuire's contributions to Newton scholarship. There will be some slight overlap with the *laudatio* of yesterday – for which I apologize, but nevertheless, myself being a Newton scholar, I hope to unearth some additional perspectives on Professor McGuire's intellectual trajectory.

At the start of Professor McGuire's career, Newton Scholarship underwent some dramatic changes. Several scholars and institutions decided that the time had come to begin editing Newton's unpublished work. In 1959 the first volume of Newton's correspondence appeared and its seventh and final volume fell from the press in 1977.[1] In 1962 the Halls published a selection of Newton's scientific manuscripts.[2] Meanwhile D.T. Whiteside began editing Newton's mathematical papers: the first volume appeared in 1967 and its eighth and final volume saw the light in 1981.[3] While Profes-

1 Isaac Newton, *The Correspondence of Isaac Newton* (7 vol.), edited by H. W. Turnbull, J. F. Scott, A. Rupert Hall, and Laura Tilling (Cambridge, Cambridge University Press: 1959-1977).

2 A. Rupert Hall and Marie B. Hall (eds.), *Unpublished Scientific Papers of Isaac Newton* (Cambridge, Cambridge University Press: 1962).

3 Isaac Newton, *The Mathematical Papers of Isaac Newton* (8 vol.), edited by D. T. Whiteside with the assistance in publication of M. A. Hoskin (Cambridge, Cambridge University Press: 1967-1981).

sors McGuire and Whiteside disagreed on how Newton should be interpreted, Whiteside taught McGuire that one simply goes nowhere without mastering Newton's manuscripts. Soon thereafter Professor McGuire came to know other Newton scholars such as I. Bernard Cohen, Rupert A. Hall, Henry Guerlac, Richard S. Westfall, John Herival, and B.J.T. Dobbs, with whom he passionately shared his academic interest in *Newtoniana*. From early on, Professor McGuire came to realize that restricting one's self to the scientific and mathematical Newton would result in an anachronistic view of the Lucasian Professor of Mathematics at the University of Cambridge. Accordingly, in his scholarly work he has devoted considerable attention to the philosophical, metaphysical and theological dimensions of Newton's natural philosophy. Not only does Professor McGuire's work bear testimony of the professionalization of the History of Science, as was pointed out in the *laudatio* of yesterday, it also bears testimony of that of Newton Scholarship.

In many of his papers, which were published in leading international journals such as *Annals of Science*, *Studies in History and Philosophy of Science*, *The British Journal for the History of Science*, *Notes and Records of the Royal Society of London*, *Historical Studies in the Physical Sciences*, *Centaurus*, *Ambix*, *Journal of the History of Philosophy*, and many others, Professor McGuire has contributed – and continues to do so – to our understanding of the theological underpinnings of Newton's doctrine of space and time,[4] which Professor McGuire addressed in his Sarton lecture of yesterday, Newton's views on the dominion of God *in rerum natura* in terms of *potentia ordinata* and *potentia absoluta*, the theological significance of Newton's active principles, Newton's concept of (actual) infinity and matter, Newton's reaction to Descartes' (natural) philosophy, and the origins of Newton's third *regula philosophandi*. It is clear from these research papers that Professor McGuire is highly concerned with understanding and explicating the intellectual context in which Newton was working and thinking. As Professor McGuire himself has made clear in the

[4] McGuire was the first to transcribe and translate a number of important folios which contained Newton's views on space and time as he developed them in the early 1690s. These folios have since then to be come known as *Tempus et locus*. See J. E. McGuire, 'Newton on Place, Time and God: An Unpublished Source', *The British Journal for the History of Science*, 11 (1978), pp. 114-129.

introduction to *Tradition and Innovation* (1995),[5] in which several of his key-publications were collected:

> [M]y purpose is to establish the motives and intentions that inform his [Newton's] reasoning: to unearth the unstated assumptions and presuppositions that drive his arguments; and to assess the implications of the positions he develops. Consequently, the unit of analysis is not the disembodied "idea" or "proposition" detached from embodiment in Newton's mental activity. *Rather, I focus on how Newton organizes his experiences in light of the cultural resources available to him, and how he appropriates texts expressing certain beliefs and representing certain traditions*.[6]

For obvious reasons of time constraint I cannot now and here penetrate into the depth and richness of all of Professor McGuire's work on Newton. However, I would like to bring at least some of his ground-breaking studies to your attention. Although my choice is somewhat determined by own interests, the studies I shall mention serve as representative samples of his work. Both in his "The Origin of Newton's Doctrine of Essential Qualities" (1968) and in his "Atoms and the 'Analogy of Nature': Newton's Third Rule of Philosophizing" (1970), Professor McGuire has shown that in conceiving his third *regula philosophandi*, which basically sets out to clarify the conditions under which certain qualities of bodies can be gener-

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5 In this tome the following papers were collected: J. E. McGuire, 'Existence, Actuality and Necessity: Newton on Space and Time', *Annals of Science*, 35 (1978), pp. 463-508; *id*., 'Atoms and the 'Analogy of Nature': Newton's Third Rule of Philosophizing', *Studies in History and Philosophy of Science*, 1 (1970), pp. 3-58; *id*., 'Body and Void and Newton's *De Mundi Systemate*: Some New Sources', *Archive for the History of Exact Science*, 3 (1966), pp. 206-248; *id*., 'Space, Geometrical Objects and Infinity: Newton and Descartes on Extension', in: William R. Shea (ed.), *Nature Mathematized* (Dordrecht, Reidel: 1983), pp. 69-112; *id.* 'Force, Active Principles, and Newton's Invisible Realm', *Ambix*, 15 (1968), pp. 154-208; *id*., 'The Origin of Newton's Doctrine of Essential Qualities', *Centaurus*, 12 (1968), pp. 233-260; and, *id*., 'Transmutation and Immutability: Newton's Doctrine of Physical Qualities', *Ambix*, 14 (1967), pp. 69-95. See additionally: *id*., 'Neoplatonism and Active Principles: Newton and the Corpus Hermeticum', in: Robert S. Westman and James E. McGuire, *Hermeticism and the Scientific Revolution, Papers read at a Clark Seminar March 9, 1974* (Los Angeles, William Andrew Clark Memorial Library: 1977), pp. 93-142; *id*., 'Space, Infinity and Indivisibility: Newton on the Creation of Matter', in: Zev Bechler (ed.), *Contemporary Newtonian Research* (Dordrecht, D. Reidel Publishing Company: 1982) pp. 145-190; *id*., 'Predicates of Pure Existence: Newton on God's Space and Time', in: Philip Bricker and R.I.G. Hughes (eds.), *Philosophial Perspectives on Newtonian Science* (Cambridge (Mass.), MIT Press: 1990), pp. 91-108; and, *id*., 'The Fate of the Date: The Theology of Newton's *Principia* Revisited', in: Margaret J. Osler (ed.), *Rethinking the Scientific Revolution* (Cambridge: Cambridge University Press: 2000), pp. 271-295.

6 McGuire, *Tradition and Innovation: Newton's Metaphysics of Nature* (Dordrecht, Kluwer: 1995), xvi [italics added].

alized, Newton was highly concerned with methodizing transductive inferences, i.e. inferences by which conclusions are drawn from the nature and properties of macroscopic bodies to the nature and properties of the microparticles that constitute these macroscopic bodies. These accomplishments have provided crucial insight in the specifics of Newton's natural-philosophical methodology and it taught me important lessons which I took to heart in my own research.

Professor McGuire has furthermore shown that, although Isaac Newton, perhaps the greatest of all scientific innovators, clearly believed in an infinitely extended void, he did not at all work in an intellectual void. When Newton was making sense of the results harvested by his physico-mathematical researches, he oftentimes drew on past and contemporary metaphysical and theological traditions. McGuire's studies have shown that Newton closely aligned himself to such traditions and he has spelled out many of the subtle details concerning such alignments. Looking back on his own studies on the occasion of the publication of his book *Tradition and Innovation*, Professor McGuire wrote:

> For Newton, the incorporation of ancient wisdom into his vision of nature is more than a ritualistic deference to tradition: it constitutes an active appropriation of tradition into the structure of his understanding of nature. As Newton construes it, understanding comprises more than sets of propositions linked together into chains of arguments, or the active comprehension of the content either of propositions or occurrent mental states. *For him it is an event dynamically poised at the cognitive interface between historical patterns constitutive of particular cultural embodiments and individual perspectives emerging from those embodiments. Indeed, for Newton, tradition is a cultural appropriation that both enables and limits innovative thought*.[7]

It is not at all a coincidence, it might be noted, that in their joint monograph *Science Unfettered*, which develops a socio-historical ontology of science, Professors J. E. McGuire and Barbara Tuchańska, who is also present with us today, have come to stress the importance of interpretation, tradition and the interpretation of tradition – but I digress.[8] In an extensive research paper co-authored with Piyo M. Rattansi and published in 1966, Professor

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7 *Ibid*., xi [italics added].

McGuire has brought to the fore Newton's so-called 'Classical Scholia,' which Newton composed a couple of years after the first edition of the *Principia* in the early 1690s.[9] These manuscripts are – and I can testify of this on the basis of my personal experience – notoriously difficult to decipher. As both authors have shown and documented, Newton composed a specific genre of scholia which he seriously contemplated to be included in a new edition of the *Principia* as a means to supplement the natural-philosophical demonstrations of Propositions IV-IX of Book III, i.e. the propositions in which the theory of universal gravitation is derived. In these scholia Newton intended to show that aspects of his theory of universal gravitation had been anticipated by natural philosophers of Graeco-Roman antiquity. By referring to these sources of *prisca sapientia*, Newton sought to justify components of his theory of matter, space and gravitation.[10] Newton, for instance, argued that Pythagoras discovered the inverse-square proportion in the vibrations of strings and that he extended this proportion to the weights and distances of planets from the sun, that Lucretius had conceived of gravity as a real physical force acting in a non-resisting void – a train of reasoning he later reiterated in *Query 28* of *The Opticks*, in which he wrote as follows: "And for rejecting such a Medium, we have the Authority of those oldest and most celebrated Philosophers of *Greece* and *Phœnicia*, who made a *Vacuum*, and Atoms, and the Gravity of Atoms, the first Principles of their Philosophy; tacitly attributing Gravity to some other Cause than dense Matter."[11] – and that ancients had revealed God's dominion over the material world "by calling God Harmony representing him & matter by the God Pan and his Pipe, or by calling the Sun the prison of Jupiter because he keeps the Planets in their orbits."[12] "For him [i.e. Newton]," McGuire and Rattansi concluded, "they [i.e. the Classical Scholia] represented a deeper penetration into the *prisca sapientia*, possible only when the preliminary work had been accomplished through experience."[13] It was in his years at Leeds University (1962-1971) that

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8 J. E. McGuire and Barbara Tuchańska, *Science Unfettered: A Philosophical Study in Sociohistorical Ontology* (Athens (Ohio), Ohio University Press: 2000).

9 J. E. McGuire and P.M. Rattansi, 'Newton and the 'Pipes of Pan'', *Notes and Records of the Royal Society of London*, 12 (1966), pp. 108-143.

10 *Ibid*., p. 112.

11 Isaac Newton, *The Opticks, or A Treatise of Reflections, Refractions, Inflections and Colours of Light* (New York, New York, Dover: 1979 [1952] [based on the 1730 edition]), p. 369.

12 Quoted from McGuire and Rattansi, 'Newton and the 'Pipes of Pan'', p. 118.

13 *Ibid*., p. 137.

McGuire put together his early work on Newton. According to his own testimony, during that period he "wrote passionately as if an unseen force was directing him."[14]

So far I have familiarized you with at least some of the content of McGuire's Newton studies, but now I would like to bring a particular feature of his studies to your attention. McGuire's studies are carefully based on Newton's manuscripts and many of them contain transcriptions and translations of important pieces which he alone has brought to our attention. Because of the meticulous nature of his manuscript studies, they remain as valuable today as they were at the time when they were first published. It is this sense 'timelessness' – insofar as this is humanly attainable – that I want to bring to your attention: Professor McGuire has produced a series of studies which still remain useful today. In this respect, he is undoubtedly an inspiring example for all researchers present here today.

Given Professor McGuire's great concern for primary texts it was only natural that he came to edit Newton's Trinity Notebook together with Martin Tamny. The new science came to Newton in the form of books. During his graduate years Newton devoured the works of Descartes, Hobbes, Hooke, Boyle, Charleton, Magirus, Stahl, Vossius, Gassendi, Wallis, etc. – works which fell outside the official Cambridge curriculum at the time. In their edition of Newton's Trinity Notebook, which was published in 1983 with Cambridge University Press, Professors McGuire and Martin Tamny have studied in detail how these works came to shape the early Newton's thought.[15] Their transcriptions of Newton's Trinity Notebook is accompanied by a detailed commentary counting more than 300 pages.

Moving on then to the lecture of this afternoon. Newton construed his natural philosophy, epistemology, mathematics and methodology in close opposition to Descartes.[16] In his latest monograph, *Descartes's Changing Mind* (2009), which is co-authored with Peter Machamer and published by Princeton University Press, Professor McGuire provides a thought-

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14 Private e-mail correspondence on 12 February 2011.

15 J. E. McGuire and Martin Tamny, *Certain Philosophical Questions: Newton's Trinity Notebook* (Cambridge, Cambridge University Press: 1983).

16 Cf. J. E. McGuire, 'A Dialogue with Descartes: Newton's Ontology of True and Immutable Natures', *Journal of the History of Philosophy*, 45 (2007), pp. 174-200.

provoking reassessment of Descartes' intellectual trajectory.[17] The main thesis of *Descartes's Changing Mind* is that Descartes' philosophical programme was subject to several – occasionally gradual and occasionally more dramatic – epistemological and metaphysical shifts. At some point along his intellectual journey, Descartes was pressed to explicate what sort of notion of efficient causation was required to account for God's self-causation.[18] In his talk of this evening, entitled '*'In God Power and Essence are not distinguished:'* Descartes on *Causa Sui*,' Professor McGuire will go into the details of Descartes' solution to this pressing theological problem. Before the floor is officially yours, Professor McGuire, I would to thank you for your lasting contributions to the field and, once more, I would like to applaud you on your being our Faculty's 2010-2011 Sarton Chair Holder. Professor McGuire, the honour being entirely ours, the floor is now yours.

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17 Peter Machamer and J. E. McGuire, *Descartes's Changing Mind* (Princeton, Princeton University Press: 2009).

18 *Ibid*., Chapter 3.

"In God Power and Essence Are Not Distinguished": Descartes and Spinoza on *Causa Sui*[1]

J.E. McGuire
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In the initial *Objections* to the *Meditations* Caterus asks Descartes to clarify his use of the phrase "from itself." Caterus notes that *ens a se* – being from itself – admits of two senses: one positive, meaning "from itself as from a cause;" the other negative, meaning "not from another," i.e., "without a cause" (*First* Objection, 1641.AT 7: 95: CSM 2: 68). What prompts Caterus's query is Descartes's asking in the *Third Meditation* whether his existence is derived from himself or from another (Ibid., 48: 33). This is the extent of the query. But Descartes, quite unbidden, brings God's existence into the picture, saying that it is not inappropriate to call God "the cause of himself" (Ibid., 95: 68). He then rejects Caterus's claim that the proof of God's existence in the *Third Meditation* is similar to the causal regress arguments used by Aristotle and Aquinas to establish first causes. These proofs seek first causes by invoking causal chains that stretch back in time. But Descartes points out that his proof is different. In it Descartes seeks the cause that is now conserving him at this very moment, and proceeds directly from an inner awareness of himself as an existing, thinking being to ask for the present cause of his existence. Straightaway, Descartes emphasizes that his concern is with the co-presence of cause and effect.

[1] I thank Jim Bogen, Peter Distelzweig, Geoffrey Gorham, Peter Machamer and Barbara Tuchanska for comments on earlier drafts.

These clarifications in place, Descartes continues: "…the light of nature does establish that if anything exists we may always ask why it exists: that is, we may inquire into its efficient cause, or, if it does not have one, we may demand why it does not need one. Hence if I thought that nothing could possibly have the same relation to itself as an efficient cause has to its effect, I should certainly not conclude that there was a first cause. On the contrary, I should go on to ask for the cause of the so-called "first' cause, and thus I would never reach anything which was the first cause of everything else. However, I do readily admit that there can exist something which possesses such a great and inexhaustible power (*tanta & tam inexhausta potentia*) that it never required the assistance of anything else in order to exist in the first place, and does not now require any assistance for its conservation (*conservetur*), so that it is, in a sense, its own cause (*sui causa*): and I understand God to be such a being" (Ibid., 109: 78). Notice that the phrase 'its own cause' refers to God's "inexhaustible" power from which God's existence derives "as from a cause"(Ibid., 112; 80). Descartes adds an important qualification. He tells Caterus that if "…we attend to the immense and incomprehensible power that is contained within the idea of God, then we will have recognized that this power is so exceedingly great that it is plainly the cause of his continuing existence, and nothing but this can be the cause. And if we say as a result that God derives his existence from himself, we will not be using the phrase in a negative sense but in an absolutely *positive* sense" (Ibid., 111: 80). Thus, what derives its existence from itself in this positive sense is a true first cause since it depends on nothing apart from itself for its existence. But"…a cause which possesses such a great power that it can conserve something outside itself must, *a fortiori*, conserve itself by its own power, and hence derive its existence from itself (*atque adeo a se esse*)" (Ibid., 111: 80). The scene is now set for one of the longest exchanges in the *Objections and Replies*, an exchange which goes well beyond anything found in the *Meditations*.

Clearly for Descartes everything which exists derives its existence either from a cause or from itself as from a cause. Accordingly, in reference to God, "from itself" can be interpreted causally "because of the superabundance of power involved – a superabundance which, as is very easily demonstrated, can exist only in God alone" (Ibid.,112: 80). In the *Second Reply* (1641) Descartes elevates the principle that everything has a cause,

God included, to the status of an 'axiom or common notion.' (AT 8: 165: CSM 2:116) His claim, then, to Caterus, and later to Arnauld, is that the conception of God as 'self-caused' throws important light on the arguments for God's existence in *Third Meditation*. Accordingly, if, as Descartes states, finite existents continue existing only if they are conserved by an external cause, and if we can always ask why anything exists what, then, is the cause of God's continued existence?

Descartes's arguments are developed most fully in his *Fourth Reply* to Arnauld (1641). In effect, they constitute a reinterpretation of the philosophical conception of God. Denying that God conserves himself "by some positive force," Descartes states that " …since that inexhaustible power, or immensity of the Divine essence (*inexhausta potentiae, sive essentiae immensitas)*is as positive as can be, I said that the reason or cause why God needs no cause is a *positive* reason or cause." A few sentences later he states that "…the immensity of his power, or essence (*immensitatem potentiae, sive essentiae*) in virtue of which he does not need to be conserved, is a positive thing" (Ibid., 236: 165). In the context of Descartes's argument these statements have to be read as saying that God does not need any cause *apart* from himself not that he is without a cause. Notice that *sive* – the inclusive 'or' which indicates equal alternatives – connects the expressions "inexhaustible power" and "immense essence." Unlike the variant *aut*, which distinguishes expressions, and *vel*, which coordinates them, *sive* denotes equivalent expressions which refer interchangeably to one and the same subject, in this case to God. The significance of this must not be overlooked. Descartes's conception of *causa sui* embraces an important claim – i.e., that in God essence and power are identical. This claim should be keep separate from the nature of the Divine attributes, such as immutability and omniscience. Attributes variously characterize what constitutes Divine nature, none of which exhausts that nature. But in these passages Descartes is saying something more substantial: he is saying that in God power and essence are one and the same. He makes this explicit in a letter to Henry More of April 15, 1649. There he denies More's claim that God exists 'everywhere' with respect to spatial extension. Certainly, God is everywhere with respect to power; but this is just to say, Descartes avers, that "…in God power and essence are not distinguished (*in Deo potentia & essentia non distinguantur*)"(AT 5; 343: CSMK:373). Descartes's identifi-

cation of Divine essence with Divine power is clearly not an inadvertence. Indeed, in the *Fourth Reply* he attempts to develop an ontology which links the identity of God's essence and power with the necessity of his self-caused existence.

Descartes's identification of Divine power and Divine essence chimes with his commitment to voluntarism. This doctrine claims that God, consistent with his nature and will, could have done otherwise that he did. In 1630 Descartes tells Mersenne that "In God, it is one and the same thing to will, to understand, and to create, without one being prior to the other, not even in reason (*ne quidem ratione*) (27th of May, 1630. AT 5: 343: CSMK: 25-6). Earlier, on May 6,th 1630, again to Mersenne, he gives a similar formulation of the claim (Ibid., 24; 149), which he repeats four times in later published and unpublished writings (CSM 1: 201; AT 8A: 14; AT 7: 135; CSM 2: 97; AT 4: 113; CSMK 119; AT 5: 166). Descartes's voluntarism remains unchanged throughout his life and guides his conception of God and creation. This is evident in his *Replies* to Caterus and Arnauld in which we witness a growing awareness of what he calls God's immense and "superabundance of power (*exuperantium potestatis*)" (AT 7: 112; CSM 2:80). Descartes's claim that in God power, essence, understanding, willing, and creating are indistinguishable clearly expresses the awe in which he holds God's immensity. His *Replies* reaffirm this sensibility by articulating how Divine power or essence is the cause of God's existence. There is no doubt that for Descartes God's existence is 'from himself' in a positive and causal sense.

In Descartes's view, this conception of God's existence, if properly understood, illuminates the traditional attribute of divine omnipotence. To speak of God as "self-caused," or "self-constituted," has a long history and it does not originate with Descartes. It can be traced back to John Scotus Eriugena, and is found in Jacob Boehme.[2] An influential source of the idea, circu-

[2] In this tradition God is conceived as an entity that becomes rather than as pure being. In the major Western tradition, however, God is conceived as pure being and Descartes must be counted in its number. Those who conceive God as pure being hold that *operatio sequitur esse*; whereas, the minor tradition holds that *esse sequitur operatio*. This means that God in creating himself *ex nihilo* proceeds *ex nihilo in aliquid.* Thus God is conceived as undergoing an absolute process of self-creation which passes from possibility to actuality. Descartes is clearly far removed from this way of thinking. For an insightful discussion of Eriugena, Boehme, Schelling and this tradition see Emil L. Fackenheim, *Metaphysics and Historicity* (Marquette University Press, Milwaukee, 1961) pp. 29-33.

lating in Latin translation from the 13th century onwards, is the neo-Platonically inspired *Liber de causis*. Proposition 25 claims that a self-subsistent substance cannot be subjected to corruption. Every self-subsistent substance is one, simple, and without composition; it is all at once "its own cause and effect." Therefore, a self-subsistent thing would be corrupted if it were separated from its cause. This is impossible, since "a self-subsistent substance never separates from its cause, because it is inseparable from its essence, since its cause is itself in its self-formation." Thus, a self-subsistent substance is "perpetually related to its own cause," and being its own cause is identical with its essence. Such a being exists just so long as its cause for existing is inseparable from its essence. Given this criterion, it is not subject to corruption, if it remains dependent on the cause that conserves it.[3]

There is no evidence that Descartes knew the *Liber*, but it was widely known in the 17th century, and its arguments find an echo in Descartes's replies. The *Liber* was known to Aquinas, who summarized its contents including those which treat of incorruptible substances.[4] Aquinas rejects the identification of self-subsistence with being self-caused. In his "Second Way" he protests that it is impossible "that something be the efficient cause of itself (*causa efficiens sui ipsius*), because it would be prior to itself, which is impossible." (*Summa Theologiae* 1a, q.2, art.3) Aquinas's opinion states what was to become the received view of the concept of self-cause. In his *Fourth Reply*, Arnauld objects to Descartes's position in a manner similar to Aquinas, arguing that to suppose a necessary being is the cause its own existence is absurd and unintelligible.

Here is the order of business. First, I present Descartes's arguments for conceiving God as self-caused based on the *Objections* from, and the *Replies* to, Caterus and Arnauld. Second, I consider whether Descartes notion that God is the cause of himself is at odds with the received conception of Divine essence and its attributes. Third, I assess the arguments of Descartes and Arnauld and ask whether Descartes's conception of God's continued existence, threatens to temporalize the nature of Divine being. And lastly, I assess critically Descartes's conception of God as self-cause,

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3 Dennis J. Brand trans., *The Book of Causes* (Marquette University Press, Milwaukee, 1981) p. 40.
4 Ibid., pp.8-10.

and consider his position in comparison with Spinoza, whom he influenced, concerning God's essence as *causa sui*.

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Why does Descartes affirm that God's existence is from himself as from a cause? There are two reasons. First, he embraces the principle that everything must have a cause. He asks therefore whether God's existence derives from a cause and what sort of cause it is. What motivates Descartes is the belief that we can best proof God's existence from his effects in creation. His fullest statement of this view is in his *Fourth Reply* He says to Arnauld: "…I think it is clear to everyone that a consideration of efficient causes is the primary and principal way, if not the only way, that we have of proving the existence of God. We cannot develop this proof with precision unless we grant our minds the freedom to inquire into the efficient causes of all things, even of God himself. For what right do we have to make God an exception, if we have not proved that he exists? In every case, then, we must ask whether a thing derives its existence *from itself* or *from something else*; and by this means the existence of God can be inferred, even though we have not given an explicit account of what it means to say that something derives its existence 'from itself'" (AT 7: 238: CSM 2: 166). Second, he believes in the need to speak of God as the cause of himself because of the limitations of the human intellect. By speaking of God as self-caused we can represent God's incomparable power in a way that is consistent with our cognitive resources.

In Descartes's exchange with Caterus and Arnauld there are *two* main senses of efficient causation in play. One sense is mobilized by Descartes to articulate what he means by calling God the cause of his existence. The other sense, which Descartes recognizes but rejects as inappropriate to his argument, Arnauld invokes to launch his critique of Descartes's claim that God is the cause of himself. Matters are complicated further by the fact that Descartes also introduces an epistemic sense of efficient cause that he claims can be extended analogically to Divine nature. To set the scene, consider Descartes's initial reply to Caterus's query. He says: "There is no need to say that God is the efficient cause of himself, for this might give rise to a verbal dispute. But the fact that God derives his existence from

himself, or has no cause apart from himself, depends not on nothing but on the real immensity of his power; hence when we perceive this, we are quite entitled to think that in a sense he stands in the same relation to himself as an efficient cause does to its effect, and hence that he derives his existence from himself in a positive sense" (AT 7: 111: CSM 2: 80). Here Descartes neither denies nor affirms outright that God is the efficient cause of himself. Rather, his concern is to point out that the way God's existence stands to the immensity of his power can be understood on analogy to the relation of an efficient cause to its effect. Accordingly, it is not efficient cause as such to which Descartes wishes to draw attention, but rather it is to the *relation* of the efficient cause to its effect. But notice Arnauld's reaction to this passage. He says: "Ms Descartes maintains that 'deriving ones existence from oneself' should not be taken negatively but positively, even when it refers to God, so that God 'in a sense stands in the same relation to himself as an efficient cause does to its effect.' This seems to me to be a hard saying and indeed to be false" (Ibid., 208: 146). Arnauld goes on to state categorically that "it is a manifest contradiction that anything should derive its existence positively and as it were causally from itself" (Ibid.) It's clear that Arnauld understands Descartes to mean that God is the efficient cause of himself and takes this to be unintelligible.

In response, Descartes claims that he has been misunderstood. He says that the phrase " 'in a sense' stands in the same relation to himself as an efficient cause does to its effect" does not mean that God is the efficient cause of himself. On the contrary: "… in saying that God 'in a sense' stands in the same relation as an efficient cause, I made it clear that I did not suppose he was the same as an efficient cause; and in using the phrase 'we are quite entitled to think' I meant that I was explaining the matter of these terms merely on account of the imperfection of the human intellect" (AT 8: 235: CSM 2: 164-5). Descartes makes two points. First, he denies that he is claiming that God is the efficient cause of himself. Second, he claims that Arnauld has missed the force of the phrase 'in a sense.' It is meant to indicate that in the face of God's inexhaustible power we are entitled to forge concepts that adequately represent that power according to our understanding. For Descartes *causa sui* is such a concept.

Given the apparent level of misunderstanding between Arnauld and Descartes I shall proceed by laying out the two main senses of efficient

causation at play in their exchange. We can begin with a passage in which Descartes sums up the extent to which he agrees with Arnauld's position. Descartes says that Arnauld "…explains at length that God is not the efficient cause of himself, since the notion of efficient cause requires that it be distinct from its effect. Next he shows that God does not derive his existence from himself in the 'positive' sense where 'positive' is taken to imply the positive power of a cause. And then he shows that God does not really conserve himself, if conservation is taken to mean the continuous creation of a thing. All this I gladly admit" (AT 7: 243: CSM 2: 169). This is an apt summary. As indicated by the first point of agreement, Descartes recognizes efficient causation in the sense in which the cause is understood to be distinct, and separate, from its effect. Indeed, this is not surprising. He had already made it clear to Caterus in the *First Reply* that efficient cause in this sense is inapplicable to Divine nature, since "everyone knows that something cannot be prior to, or distinct from, itself" (AT 8: 108: CSM 2:78). Descartes dubs this conception the *strict* sense of efficient causation (AT 8:108: CSM 2:78: AT 8: 239; CSM 2: 166). He expresses the same opinion to Arnauld (AT 8: 240: CSM:167). So long, then, as efficient causes are understood in this strict sense Descartes agrees that nothing can be the efficient cause of itself. As to Descartes's second and third points, once again he had already told Caterus that when he speaks of God conserving himself this "is not to be understood as that which comes about by the positive influence (*influxum*) of an efficient cause (ibid., 109: 79). This he repeats in his *Reply* to Arnauld. There he says that he can agree with "…everything my critic puts forward to prove that God is not the efficient cause of himself and that he does not conserve himself (*se conservare*) by any positive power or by continuously recreating himself" (AT 8: 237: CSM 2: 165.) Once again Descartes's reference is to efficient cause understood in the strict sense. It's not surprising, then, that Descartes denies "…God conserves himself (*se conservare*) by the some positive force (*positivum aliquem influxum*), in the way that created things are conserved by him. I simply said that the immensity of his power or essence (*potentiae sive essentiae*), in virtue of which he does not need a conserver (*conservatore*), is a positive thing" (ibid., 236: 165). Accordingly, the notion of agency whereby, in virtue of its power, an agent acts on a patient is applicable to God. Divine power or essence is transcendent and cannot be understood in terms of notions of power appropriate to the action of finite agency. For

Descartes, as noted, willing, understanding, and causing are one and the same in Divine nature and none is prior to the other. If, then, efficient causation is understood in the strict sense in which an agent acts on a patient, Descartes denies that it is possible for God to be the cause of himself.

This is the first sense of efficient cause on which both Descartes and Arnauld agree. Surprisingly, despite Descartes's numerous disclaimers, Arnauld consistently interprets him as invoking the strict sense of efficient cause when he claims that God is the cause of himself. Indeed, each of Arnauld's criticisms turns on the assumption that Descartes conceives God to be the efficient cause of himself in the strict sense of the term. I shall need to ask why this is so; but first let's consider Arnauld's arguments. He takes his bearings from Descartes's discussion of the continued existence of things in the *Third Meditation* (AT 8: 49: CSM 2: 33). In proving God's existence Descartes notes that *at* the moment he performs the *cogito* he is assured that he exists; but he is also aware that he lacks the power of continuing to exist into the next separate and non-overlapping moment of time. Descartes concludes that there is a cause other than himself from which his existence is derived and conserved. Commenting on this, Arnauld observes that a causal sense of continuing to exist is inappropriate to a "supremely perfect or infinite being." It is absurd to ask why an infinite being continues to exist because this implies a 'before' and 'after,' and a 'past' and 'future,' temporal notions inapplicable to a being who exists all at once in the *nunc stans*, the timeless present. Therefore, the concepts of original 'self-creation' and 'self-conservation' do not apply to God, since they imply a beginning and a continuation in time (ibid., 211:148). Moreover, to think that God stands to himself in the same relation as an efficient cause does to its effect, makes God both different from himself and dependent on himself. This threatens Divine unity, leading to the absurdity that God would have to exist prior to bestowing existence on himself, i.e., God would have to exist before he existed (AT 8: 219: CSM 2: 147).

As noted, in his *Reply* to Caterus, Descartes had already indicated that this absurdity would ensue were the strict sense of efficient cause applied to Divine nature. Why does Arnauld appear not to notice this disclaimer? In part, the answer is that he considers causal reasoning irrelevant to an understanding of the nature of the Divine being. For Arnauld, it is simply

misguided to seek a causal answer to why God exists, or continues to exist, whether it's labeled an efficient cause, or "…an quasi-efficient cause (I'm arguing about the reality, not the name); instead, we should confine our answer to saying that the reason lies in the nature of a supremely perfect being" (ibid., 213: 149). Arnauld's use of the term "quasi-efficient cause" is a reference to Descartes's statement to Caterus that those who "…attend to the literal and strict meaning of the phrase 'efficient causation' and thus think it impossible for anything to be the cause of itself…do not see that there is a place for another kind of cause analogous to an efficient cause." (ibid., 109: 79). In Arnauld's view, this "other kind of cause" is merely a verbal distinction which miss-represents the nature of efficient causation, and attempts to disguise the fact that efficient causation applies only to things whose existence can be distinguished in reality from their essence. But in Divine being, as Arnauld vividly points out, essence and existence are indistinguishable (AT 8: 213: CSM 2:150).

On the face of it, Descartes's position seems to comport badly with the received theological view of God's existence. In this regard, Arnauld goes on to make another salient point. He reminds Descartes that we look for something's cause "only in respect of its existence, not in respect of its essence." It is absurd, therefore, to look for the cause of why "it belongs to the essence of an infinite being that it exists," since its existence lies in its very nature. In other words, it is absurd to think that God's infinite essence, to which existence belongs necessarily, can be understood causally. Indeed, "…it belongs to the essence of an infinite being that it exists…no less than it belongs to the essence of a triangle to have its three angles equal to two right angles" (AT 8: 212: CSM 2: 149). In Arnauld's view, God is an infinite being, whose existence is without beginning and end. Therefore, it's evident that God is an indivisible, self-subsistent being whose existence is all at once and non-successive. In God, there is no past or future, no before or after, only eternally present existence (AT 8: 211: CSM 2: 148). Accordingly, it is unintelligible to speak of God as the cause of himself. It suffices to understand that God is an infinite being whose existence belongs to his essence (ibid., 213: 150).

Clearly, for Arnauld, there is no positive sense in which God's existence can be conceived as derived causally from himself; there is only the negative sense in which divine existence cannot be derived from anything else

(ibid., 210: 148). But what conception of causation informs Descartes's claim that God can be thought of as the cause of himself? Descartes agrees with Arnauld, as noted, that the term 'efficient cause' covers causes that act prior to, and are distinct from, their effects. In Descartes's view, however, this does not exhaust the category of efficient causation. He tells Caterus that "…the natural light does not establish that the concept of an efficient cause requires that it be prior in time to its effect. On the contrary, the concept of a cause is, strictly speaking, applicable only for so long as the cause is producing its effect, and so it is not prior to it" (AT 8: 108: CSM 2: 78). He makes the same point to Arnauld: "The fact that a cause need not be prior in time is clear from the fact that the notion of cause is applicable only during the time when it is producing its effect…" (ibid., 240: 239). Again, he tells Caterus "that the concept of efficient cause does not require that it be prior in time to its effect" (ibid., 108: 78); and to Arnauld he says that"…the restriction 'prior in time' can be deleted from the concept while leaving the notion of efficient cause intact" (ibid., 240: 239). This is the second sense of efficient cause and for Descartes it is the more fundamental sense. It is this sense of efficient cause that informs Descartes's conception of God as the cause of himself, not the strict sense which, Descartes agrees, leads to absurdities. For Descartes this second sense of the concept is based on the following commitments. (1) Efficient causes need not be prior in time to their effects; (2) there are *per se* efficient causes – causes whose effects derive entirely from their natures – that act co-presently with those effects (ibid.,108: 78). Lastly, (3) the *per se* efficient causes that act co-presently with their effects produce those effects *only at the very moment they are causing* (AT 8: 240: CSM 2:147). Thus, for Descartes, the sort of *per se* efficient cause that is applicable to Divine existence, must in causing be conceived of as acting co-presently with its effect, such that there is no temporal gap between the cause and the effect. These commitments are not clearly evident in the *Meditations*. And it's only in the *Replies* that Descartes cash out their import with respect to the question of the cause Divine existence.

I can now construct Descartes's reasoning. To bring God's existence under the rubric of causation, we must consider whether God's existence is derived from himself or from something else. To Arnauld Descartes states that we can "…form a concept of cause that is both an efficient cause and

a formal cause: that is to say, what derives its existence 'from another' will be taken to derive its existence from that thing as an efficient cause, while what derives its existence 'from itself' will be taken to derive its existence from itself as a formal cause – that is, because it has the kind of essence which entails that it does not require an efficient cause." Accordingly, apart from the efficient cause, in the strict sense, and 'no cause at all' there is, Descartes claims, "a third possibility, namely, 'the positive essence of a thing' to which the concept of an efficient cause can be extended" (AT 7: 238: CSM 2:166). For Descartes, as noted, it's intuitively self-evident that a *per se* cause can be said to cause just in case it's producing its effect. In other words, in Descartes's view, something can no more act *when* it is not than it can act *where* it is not. Given this commitment, Descartes claims that the 'cause' of Divine existence is co-present with, and inseparable from, Divine existence itself (AT 7: 108: CSM 2: 78). So given that there are *per se* causes co-present with their effects, that their effects take place at the very moment they are causing, and that there is no temporal gap between cause and effect, God's essence *can be conceived* as if it were a cause which *in rebus* is indistinguishable from its effect. What controls Descartes's thinking is clearly the formal cause understood as expressing the essence of God (ibid., 241-242: 168-169). It is within this framework that he thinks an extended concept of the efficient cause will be useful in providing a basis for proofing God's existence within the causal order.

For Descartes, then, there is a sense in which the efficient cause can be extended to something's essence, God's included. This is why Arnauld is prompted to remind him that we ask for an efficient cause only in regard to the cause of something's existence. As noted, Descartes agrees that this is true. But immediately he points out that "in the case of God, existence is not distinguished from essence (*non distinguitur essentia ab existentia*); hence we can ask for the efficient cause in the case of God." This is the basis of Descartes's position. He accepts the received view that the unity of Divine being is essentially non-compositional. In virtue of this conception Descartes can say that "…the formal cause will be strongly analogous to an efficient cause, and hence can be called a quasi-efficient cause (*quasi causa efficiens*)" (AT 7: 243: CSM 2:169). Moreover, this also explains why Descartes attempts to combine, in one causal concept, features of the efficient and the formal causes. Clearly, it is this conception to which

Descartes gestures when he says to Arnauld that God's "own essence is the eminent source itself which bestows on him whatever we can think of as being capable of being bestowed on anything by an efficient cause" (Ibid., 241: 168)

Further light is thrown on Descartes's 'quasi-efficient cause' in his *Reply* to Gassendi's *Fifth Objections*. Gassendi argues that the characteristics parents bestow on their offspring derive "not from the efficient but from the material principle" (ibid., 289: 201). Descartes replies bluntly that "it is unintelligible that perfection of form should ever preexist in a material cause; it can only do so in an efficient cause" (ibid., 366: 252). What does this claim come to when applied to Divine nature? Recall that Descartes's identification of the immensity of God's power with the immensity of his essence. Also bear in mind his claim that in God willing, understanding and creating are one and the same. Now in the light of Descartes's view that understanding and willing are indistinguishably the same in God, the efficient cause (identified with willing) and the formal cause (identified with understanding) are also indistinguishably present in Divine nature. Just as there is in God no distinction between willing and understanding, so likewise there is none between the efficient and formal cause. Accordingly, the claim that the form preexists in the efficient cause means for Descartes, from the side of the causes, that in Divine nature there is no distinction or priority between the reason and the action by which God unconditionally brings something into existence. When applied to the claim that God is the cause of himself it means, given that existence belongs necessarily to Divine essence, that God's essence is itself the cause of God's existence. Accordingly, Descartes's reasoning can be reconstructed as follows. If everything's existence has a cause, and if the cause of God's existence is contained in Divine essence, and if each of God's attributes are positively established from his essential nature alone, these attributes are necessary in themselves and not dependent contingently on anything apart from themselves. A being such as this can be described as 'cause of itself' – one whose essence involves existence – since it possesses in itself the sufficient explanation of its own existence. Only God's substantial nature can be so described.

This seems plausible enough: such a being can be so described. But Descartes has achieved no more than this. He has simply re-described the

view, accepted by Arnauld, that the ground of God's existence lies necessarily in Divine essence. Clearly, Descartes's re-description of God's essence as the cause of his existence is superfluous. In other words, to re-describe God as self-caused cannot, of itself, provide an *independent* explanation of the existence of Divine being. Moreover, it is circular. The necessity of God's existence is already built into the conception of the infinite perfection of Divine nature. In other words, such a re-description already presupposes the received view that God is a necessary being whose existence belongs to his essence. Furthermore, to conceive cause and effect as existing co-presently in Divine being threatens to construe God's existence as temporal in character, since co-presence makes no sense, as Arnauld points out, apart from implicit reference to past existence and continuing existence in the future (AT 7: 211: CSM2: 148-9). Therefore, Descartes's appeal to the co-presence of cause and effect in Divine nature fails to capture the received theological claim that God's existence lies in the timeless nature of a supremely perfect being. Accordingly, it is the standard ontological conception that existence belongs to God necessarily that does the explanatory work, and to speak of Divine existence as self-caused is circular and vacuous. Moreover, if Descartes's claim that God can be conceived ontologically as a self-caused being seems to have initial plausibility, it is well to note that the same reasoning also supports the view that God's existence is without a cause. But in this case the claim rests on an established ontology, the ontology of a being to whom existence belongs necessarily, the ontology of an uncaused being.

A question arises immediately. In the face of these consequences (which Arnauld intents his criticisms to reveal) why does Descartes persistently speak of God as the cause of himself? My conjecture is this. In the *Fourth Reply* Descartes begins to emphasis the epistemic usefulness of extending the conception of efficient cause the nature of God. This probably indicates that Arnauld's criticisms made him more aware that his view of God as the cause of himself can only result in an epistemic conception whose value is heuristic. As noted, for Descartes everything has a cause in an unqualified sense. This leads him to ask if God has a cause. He is also struck increasingly by the infirmity of our cognitive powers, especially in the face of the infinitude of Divine power. But given his unwavering belief that God's essence is power, Descartes wants a way of thinking of God's immeasur-

able power that is adequate to our understanding. For Descartes, this means that God must be conceived under the principle of causation as the cause of himself. It is this line of reasoning that prompts him to put forward an extended conception of efficient cause which, as we'll see shortly, he analogizes to Archimedian proof procedures. Certainly God's inner being cannot be understood: it is ineffable and God's immense power or essence is beyond our comprehension. We know only God's attributes, such as immutability and omniscience, which for us represent the perfections inherent in an infinite and necessary being. Nevertheless, we have an idea "… of the uncreated and independent thinking substance, that is of God. Here we must simply avoid supposing that the idea adequately represents everything which is to be found in God" (*Principles* Part 1, 54: AT 8A 26: CSM 1: 211). The question becomes: in the face of our limited cognitive perspective, how can we form an adequate conception of God's existence, a conception that does justice to God's superabundant power? Our concepts, if they are consistent and non-contradictory, can provide adequate but never complete understanding of our experience.

Descartes's 'Archimedian' conception of efficient cause is such a concept and he first introduces it in reply to Arnauld. Descartes explains it in the following way. Just as Archimedes conceives the sphere in terms of the polyhedron, so Descartes advances the efficient cause, understood as a cause co-present with its effect, to explain features which "in fact belong to the formal cause, that is, the very essence of God" (Ibid., AT 7: 241: CSM2: 168). From this perspective he extends the efficient cause to speak of the essence and power of God (24) "just as the arc of an indefinitely large circle is customarily extended to the concept of a straight line; or the concept of a rectilinear polygon with an indefinite number of sides is extended to that of the circle." Moreover, just as an rectilinear figure doesn't apply to the sphere, as such, but only to its many-sided substitute, likewise, an efficient cause doesn't literally apply to God but allows us to think of God's existence as derived causally and positively from himself, i.e.," from the real immensity of his power" (ibid., 111:80). Accordingly, Descartes's strategy is this. Although efficient causation does not apply literally to Divine nature, the concept can be *used* in this extended sense to aid us in understanding positively the immensity of God's power. We are therefore able to think of God's incomparable power by means of a concept

which is analogous to an efficient cause and which is also available to us epistemically. At the end of the day, Descartes's extended concept of efficient cause is a heuristic strategy for understanding the immensity of God's power. Nevertheless, this epistemic move chimes with his avowed belief that our understanding of Divine nature is restricted by the infirmities of human cognition.

Behind Descartes's reasoning is another basic commitment. He holds that causality and existence are coextensive notions, each tied inextricably to the other. Thus, the brute fact of something's existence is interchangeably connected with the co-present action of a cause. Caterus's query concerning the phrase "from itself" awakens in Descartes an awareness of the logic inherent in his position. If God's act of efficient causation always implies existence, we must ask whether and in what sense God's existence is itself self-caused. Pondering this, Descartes is lead to a novel understanding of the doctrine that God exists necessarily in virtue of his infinite power. For Arnauld, God is necessarily infinite and what is infinite doesn't need a cause. Descartes turns this on its head. For him, it's precisely because God *is* infinite that he can be conceived of, in the appropriate sense, as the cause of himself. Put more precisely: it's in virtue of the infinite immensity of God's power or essence that he exists. This appears to put Descartes at odds with the belief that God's being is anchored in an essence that is wholly present in a *nunc stans*, in a timeless now. And it leads Arnauld to imply that he's advancing a 'dynamic' picture of divine existence. In Arnauld's view, this is tantamount to 'temporalizing' the immutability and unity of Divine being (AT 7: 211: CSM 2:148-9).

Is Descartes guilty of this charge? I think not. In the first place, he puts forth a conception of God's being that accords well with the philosophical conception of God. Throughout his exchange with Caterus and Arnauld Descartes is at pains to stress that God, in virtue of his nature, necessarily exists. This commitment is signaled in his *Replies* when he says that in God power and essence are identical so that "the formal cause will be strongly analogous to an efficient cause, and hence may be called something close to an efficient cause." (AT 7: 243: CSM 2: 169-70). Accordingly, Descartes understands the cause of God's existence in terms of God's essence or formal cause (ibid., 243: 170). Furthermore, as noted, he emphasizes the absurdity of claiming that God acts on himself in the manner of

an efficient cause understood in the strict sense (AT 7: 236: CSM 2: 165). So not surprisingly, as the ontological argument of *Fifth Meditation* attests, Descartes's core conception of God is that existence is a perfection and belongs to Divine essence necessarily (AT 7:66-70: CSM 2: 245-48). As a result of Arnauld's criticisms, I think Descartes came to see more clearly that efficient cause, understood as what is intrinsically co-present with its effect, is superfluous as a means of grasping the immensity of Divine power. We have already grasped that power when we grasp the infinitude of Divine being. It suffices that we can appeal to a concept, analogous to an efficient cause, which functions heuristically and is available to us epistemically.

This brings me again to why Arnauld repeatedly ignores Descartes's denial that he is invoking efficient causation in the strict sense when speaking of the cause of God's existence. I said above that Arnauld does so because he believes that causal language is irrelevant to the question: why does God exist? But there is another reason explicit in Arnauld's conception of cause. Arnauld tells Descartes that "...it is absurd to conceive of a thing's receiving existence yet at the same time possessing that existence prior to the time when we conceive that it received it….For what is the notion of cause? The bestowing of existence. And what is the notion of effect? The receiving of existence. The notion of cause is thus by nature prior to the notion of an effect (*Prior est autem natura causae notion, quam notion effectus)*" (AT 7: 210: CSM 2: 147). Arnauld clearly holds that cause is prior to effect in the order of being. But since efficient causation is at issue for Arnauld the cause must also be prior in time. This is the direct opposite of Descartes's understanding for whom temporal priority of cause to effect is not essential. Consequently, Descartes's claim that there are efficient causes that act co-presently with their effect has no sense in Arnauld's scheme. This leads him to relegate Descartes's concept of efficient cause to a place in his own causal scheme and to construe Descartes's arguments accordingly. Put otherwise, Arnauld conflates Descartes's view that God is the cause of himself with the strict concept of efficient cause that Arnauld already accepts. Thus, given his causal perspective it's not surprising that Arnauld perceives untoward consequences in Descartes's arguments. For his part Descartes ceases to press his ontological claim that there is 'a sense' in which God is the co-present cause of himself. So he allows his

efficient-causal view of God as source of his own existence to recede to the background and begins to stress what I have called the 'Archimedian' conception of sufficient cause as a useful epistemic and heuristic tool which, when extended analogically to God's nature, helps us to conceive that God, in virtue of his immense essence or power, is the cause of his own existence.

Descartes's claim that God can be thought of as the cause of himself, in the 'Archimedian' sense, is an instance of what Peter Machamer and I have called Descartes's epistemic stance.[5] This understanding of the scope and limits of human knowing begins to emerge in the *Sixth Meditation*, is developed in the *First* and *Fourth Replies* and then further in Descartes's later thought. In a late expression of this view Descartes states that "…our mind is not the measure of truth and reality; but certainly it should be the measure of what we can affirm or deny." (To Henry More, February 5th, 1649. AT V 274: CSMK: 364) The claim is that we can have epistemic warrant for thinking that what we know of the world is based on how the world is; never at any time, however, do we have complete knowledge of all that lies in reality. Earlier, Descartes made the same claim to Gibieuf, saying that "…there are many properties of which I have no idea; I only deny that there are any which are inconsistent with the ideas I do have…" (19th January, 1642: AT V111 478: CSMK 202-3) Notice the criterion of what can be intelligibly affirmed or denied is that our ideas be consistent or non-contradictory. Around 1644, however, Descartes rethinks the implication of his non-contradiction criterion and begins to consider the nature of possibility in cases where we attribute actions to God and make conclusions about reality. He notes that in recognizing God's power to be unlimited, we easily realize that "our minds are so created [as] to be able to conceive as possible the things that God wished to be possible, but not to conceive as possible the things which God could have made possible, but which he has nevertheless wished to make impossible…" (To Mesland, May 2, 1644. AT IV 118-19: CSMK 235) Accordingly, we may conceive as possible what God wishes to be possible. However, since our thought cannot encompass what God can do, God could have done things that would seem impossible to us. These cases would show up as contradictions

5 Peter Machamer & J.E. McGuire, *Descartes's Changing Mind* (Princeton University Press, Princeton, 2009).

in our thought. Nevertheless, for Descartes, what is contradictory for us is no longer the only test of judgments about how God created the world, or how the world really is. Contradictions indicate limitations in our cognitive powers, not limitations in the power of God or in the nature of reality. Complete knowledge is open to God alone; but the knowledge we attain is adequate enough for us to establish a science of nature and to know ourselves. However, although our ideas cannot embrace the whole of reality, Descartes believes that they are world-involving to the extent that they are clear, distinct and non-contradictory.

2

Descartes's conception of God as the cause of himself and his identification of Divine power and essence did not fall on barren ground. Spinoza systematically employs these ideas in his *Ethics*.[6] Certainly Spinoza knew Descartes's writings, and the *Cogitata Metaphysica* shows his familiarity with Descartes's conception of God. Spinoza's God, unlike Descartes's, is non-transcendent. For Spinoza, there is only one God or substance. Whatever exists, is in God, and all things follow necessarily from God in infinite ways. (*Ethics* Part 1, Props. XV and XV1). Moreover, Spinoza's God is not mysteriously remote from human understanding. For Spinoza, to know anything, God included, is to know its cause. To know God, therefore, is to know God's cause, i.e., to know that God is a self-caused being (Part 1, Axiom 1V, and Prop. XXX1V). Spinoza's bold 'rationalist' stance regarding the notion of 'self-cause' is evident from the very first Definition of the *Ethics*, Part 1, where we are told that what is the "cause of itself" is "that whose essence involves existence, or that whose nature cannot be conceived unless existing." This is clarified further in Proposition V11, Part 1, in which substance or God is said to be "the cause of itself, that is to say (Def.1), its essence necessarily involves existence, or, in other words, it pertains to its nature to exist."

Spinoza's intrepid cognitive sensibility contrasts sharply with Descartes's cautious epistemic stance blatantly evident when Descartes is confronted by the inscrutability of the transcendent Christian God. For Spinoza, God

6 Benedict de Spinoza, *Ethics* ed. James Gutman, (Nafner Publishing Company. New York, 1949).

or substance can be known by human cognition. Indeed, he states that "The power of God is His essence itself." Therefore, "From the necessity alone of the essence of God it follows that God is the cause of himself, and of all things. Therefore the power of God by which He Himself and all things are and act is His essence itself." (Part 1, Proposition XXXV1) In these passages Spinoza makes three identifications. He identifies God's power and essence: God's essence and existence: and God's essence is identified as the cause of himself. For Spinoza, then, power, essence, existence, and self-cause are indistinguishably present in God's substantial nature. In Proposition 11, Part 2, of the *Ethics* he cautions against comparing Divine power with the power of kings: "…the power of God is nothing but the active essence of God and therefore it is as impossible for us to conceive that God does not act as that He does not exist." Unlike Descartes, Spinoza does not refrain from identifying God's power with an *active* essence from which all things follow necessarily. In this respect Spinoza's "active essence" echoes the Platonic dictum that being is dynamic power. (*Sophist* 247-c, 248-249) Nor does he shirk from the implications of the identification of power, essence and cause in God or substance. For Spinoza, the nature of substance, and what substance does, are expressed through its infinite attributes. Thus, God's essence or power is inherently active and identical with what God does, so that the 'gap' between essence and causal activity, between what God is and what God does, is eliminated. One and the same internal power or essence is the cause both of God's existence and God's activity. For Spinoza, then, all effects are literally contained in God or substance as cause. This move is not open to Descartes. He must preserve the distinction between God as the transcendent cause of creation and creation itself as a separate effect. So unlike Spinoza, Descartes cannot maintain that all happenings and comings-to-be are effects contained causally in God as the supreme self-cause. Thus, he is unable, in Spinozistic fashion, to identify God as the active cause of himself with God as the productive cause of everything else. In the end, he falls back on the 'Archimedian' or epistemic conception of God as self-cause.

By comparing Descartes's and Spinoza's view of substance the differences between them becomes clearer. Spinoza defines substance as that "which is in itself and is conceived through itself; in other words, that the conception of which does not need the conception of another thing from which it

must be formed." (*Ethics*, Part 1, Definition 111). In Proposition V11 he states that 'It pertains to the nature of substance to exist." Notice that substance not only exists in itself but is understood through itself. This clearly states that substance is independent of all external causes, and therefore that it is the cause of its own existence. In this positive sense it is a self-independent and self-sufficient being. Spinoza accepts the principle of sufficient reason in a strong form. If everything has a reason or cause for existing, and if that reason or cause is not external to the thing, it must lie necessarily in the thing's nature alone. (*Ethics* Part 1, Prop. X1)

Descartes gives different definitions of substance in different places. His general conception is that substance needs only itself to exist, a condition that properly speaking God alone satisfies. (AT V111A 24: CSM 1: 210). In other places, he articulates a substratum account. In these contexts, he claims that we lack immediate knowledge of substance, a view he emphasizes to Hobbes, Arnauld and Burman (AT V11 176, 222: CSM 11: 124, 156) In the *Third Reply* Hobbes is told that "…we do not come to know a substance immediately, through being aware of the substance itself; we come to know it only through its being the subject of certain acts." (AT V11 176: CSM 11: 222) Descartes makes the same point to Arnauld. "We do not have immediate knowledge of Substances…we know them only by perceiving certain forms or attributes which must inhere in something if they are to exist;…" (AT V11 222: CSM 11: 156) To Burman in 1648 he says that "Beyond the attribute which specifies substance, there must further be conceived substance itself which lies under that attribute, as, since mind is a thinking thing, there is beyond thought the substance which thinks, and so forth" (AT V 156: CSM 11: 124). Substance is not, then, simply its principal attribute substantialized. For Descartes there are two conceptions of substance: (1) it is that the existence of which is self-independent from all else: (2) it is that which bears properties from which the existence of an underlying substratum can be inferred.

For Spinoza substance is also that which is self-independent. However, he rejects the inherence or substratum conception: if a thing's existence is inferred from that of another, such as an attribute, it is not a substance. (*Ethics* Part 1 Definition 111) This is precisely the view Descartes espouses: the view that while a substance needs only itself to exist, nevertheless its existence must be inferred from its attribute and therefore it

cannot be known through itself. Such a conception is an anathema to Spinoza. It posits something which is unknowable and of whose nature we have no immediate or positive idea. By contrast, for Spinoza, anything which *really* exists in itself must be conceived through itself. Accordingly, with respect to substance 'existing in itself' and 'conceived through itself' are co-referential notions.

Despite these differences, Descartes and Spinoza agree that substance is self-independent (exists in itself) and that this entails that God or substance is the source of its own existence. For both, it's important to invoke the notion of independent existence: but also it is important to affirm a positive notion of an existence that is necessary and consequently self-explanatory. Accordingly, God's nature is self-independent and contains within itself the cause of its being.

But here similarities end. If for Descartes there is inferential but never immediate knowledge of finite substances, the more so is God's innermost nature unknowable and Divine power beyond our comprehension. Just as finite substances are known only through their attributes, similarly God is known solely through those attributes manifest to us. This includes, for Descartes, the notion that God is the cause of himself, a notion consistent with our finite grasp of God's immeasurable power. Descartes's commitment to the substratum view of substance chimes with his adherence to the doctrine of God's ineffable transcendence. So, while we apprehend *that* God is omnipotent, our understanding cannot *comprehend* the content of that power. God's inner essence is beyond the categories of finite cognition and too remote to be known. To grasp it we are forced to think with limited concepts such as self-cause.

Spinoza's cognitive sensibilities are quite otherwise. For him, it is precisely substance or God that exists in itself and therefore must be conceived through itself. Thus, the being whose essence is the cause of its own existence can be known directly by the human intellect. There is no need for an epistemic stance, or for the creation of an extended concept of efficient causation as Descartes proposes, in order to think the cause of God's existence. The career of *causa sui* in the thought of Descartes and Spinoza is remarkable indeed. The more so as it begins with Caterus's simply query concerning three little words: *ens a se*.

Sarton Medal Lectures

Laudatio Emanuele Conte

Georges Martyn

I thank the dean for introducing me and for sketching, in a nutshell, the role of George Sarton for the history of sciences, and the objectives of the Ghent University Sarton Committee. For our Legal History Institute, each occasion to present a candidate for the Sarton medal is a delightful way to honour an esteemed colleague in the international research field of legal history, and especially the history of jurisprudence. As every jurist who has ever attended any course of legal history commonly knows, the development of both Roman and canon law has been of paramount importance for the legal science. In continental Europe, one could not imagine legal science without the learned foundations, laid by the professors of the medieval *ius commune*. Starting in Northern Italy in the late 11th century, these scholars studied, annotated and commented on the texts of the old Roman Empire, as well as those of the Catholic church, which also has Rome as its centre. This in itself could already be reason enough to single out an excellent scholar of this *ius commune* in Italy, and in its capital in particular. However, there are many more reasons why my colleagues Dirk Heirbaut, Rik Opsommer, and myself, did not hesitate to present Emanuele Conte as a candidate for the Sarton medal.

Professor Conte is an internationally respected specialist of medieval law and medieval doctrine in particular. He is professor of legal history at the *Facoltà di Giurisprudenza* of Roma Tre University, where, as Director, he is the head of the Department of legal history and legal theory. The members of the Ghent Legal History Institute are happy to welcome professor Conte, as well as three of his most dynamic and charming

collaborators. I thank Stefania Gialdroni, Sara Menzinger and Silvia Di Paolo, as well as Antonia Fiori from *La Sapienza*, for having presented their recent research this afternoon. I hope that the connections that have been formed today between the young legal historians of both our Roman and Ghent research groups, may be the fruitful base for future scientific collaboration. I wish all of them a splendid career, to the example of their director, professor Conte, with whom they have the privilege of working.

Emanuele Conte, born in Rome in the late summer of 1959, obtained his law degree *cum laude* at the famous *La Sapienza di Roma* in 1983. His thesis, written under the direction of the well known professor Cortese, was rewarded a special university prize. It was certainly a unique honour for him as a student to be able to follow in the footsteps of a great scholar as Ennio Cortese, one of the biggest names in the Italian tradition of medieval legal history.

From 1986 to 1988, Emanuele Conte followed a doctoral school programme in the research field of Italian Legal History at the *Università Statale di Milano*, and he defended his thesis in 1988. Meanwhile, he had enjoyed several stays in the beating heart of research into European legal history, the *Max Planck Institut für Europäische Rechtsgeschichte* in Frankfurt am Main, where he especially appreciated working with professor Gero Dolezalek, a retired professor of the universities of Aberdeen and Leipzig, and an internationally renowned *ius commune* specialist. Also in the following years, the Frankfurt Max Planck Institute would remain a frequently visited study centre. But Emanuele Conte also had many other inspiring contacts, of which the French scholar Yan Thomas deserves special mention. Another legal history laboratory repeatedly visited by Emanuele Conte is the famous Robbins Collection at the University of California in Berkeley. As *professore* Conte entrusted me a while ago, his collaboration with the famous Stephan Kuttner in Berkeley was decisive for his own research plans.

In more than fifty scientific publications, Emanuele Conte has developed insights and ideas on medieval and early modern law, as well as the recent developments of legal history itself. His profile as a writer is a perfect fit for the Sarton Medal. Many of his publications deal with the history of the science of law, for instance with the study of Roman law at the late medieval universities (and here I would particularly like to refer to his *Accademie*

studentesche a Roma… De modis docendi et discendi in iure: On the modes of learning and teaching law). In several contributions, he especially focuses on the way learned law and everyday practice interact, for example in his '*Diritto comune. Storia e storiografia di un sistema dinamico*', '*Servi medievali*' or his recent 'Roman law vs. Customs in a changing society', describing Italian society in the 12th and 13th centuries. The way learned lawyers, trained and teaching at university, evaluated the customary rules of feudal society will also be the subject of his Sarton lecture in a few minutes.

Emanuele Conte has a thorough knowledge of the works of the earliest professors of law in Bologna, their scholastic methods and their writings. He studied these works, both at the material level, interpreting their contents, and at a more formal level, as far as their bibliographical form is concerned.

Professor Conte paid special attention to the formation and the roles of jurists throughout the centuries, not only as academics, but also as servants of the political system and as defenders of private interests. He is acquainted with the classical theories of property, possession and prescription, but also studied more specific statuses in depth, such as the exact status of the *coloni*. In a 1997 article on this subject, he concludes: *Observée sur la longue période, la législation justinienne sur le colonat révèle donc une curieuse histoire: plus ou moins oubliée pendant cinq siècles, elle a subi une interprétation énergique de la part des romanistes médiévaux, influencés par des soucis plus pratiques qu'on ne pourrait l'imaginer d'emblée*. It is these kinds of 'curious histories', in-depth investigations into generations of jurisprudence revealing renaissances and re-interpretations of old rules, that make up the research field in which Emanuele Conte is a fully skilled artisan.

And he is not only an expert at the texts of the Justinian corpus, but he also published on themes in canon law, like ecclesiastical property. For instance, I enjoyed reading his contribution to an *Ecole française de Rome* volume on the papal bull *Unam Sanctam*. In this text, he argues that pope Innocent III's 1209 compilation of canonical texts was the first real authentic code of the Middle Ages, and that popes like Gregory IX and Boniface VIII were the first real sovereign lawgivers, at a much earlier time than all temporal rulers. The church copied the Justinian example long

before kings did so. And the popes built their power on both legal and theological grounds.

In his writings, professor Conte does not only analyse the learned texts in detail, but he also has a full grasp of the social and political context, which he describes in detail. Medieval law texts are not just skins of dead animals (parchment), but colourful pictures of a living culture. Canon law and roman law are not separated fields of study, just like learned law and customary law are not unconnected bodies either. It is for this shaded and colourful, panoramic view on the living law, that we want to honour Emanuele Conte by offering him the Sarton Medal. Although most of his works were initially written in Italian, more and more articles have been published in English, French, Spanish and German in the last decade. The solidity of this research made him a welcome teacher and researcher all over Europe. He was frequently guest at Peterhouse College in Cambridge, director of the *Weeks of doctoral studies in the frame of the European Doctorate on History, Theory, Sociology and Anthropology of European Legal Cultures*, director of the *International School of Ius Commune del Centro di Cultura Scientifica di Erice*, director of a research unit *Manoscritti giuridici medievali*, and guest lecturer at the *Ecole des Hautes Etudes en Sciences Sociales* in Paris, as well as at the universities of Barcelona, Lyon, Nanterre, Paris II Panthéon-Sorbonne and Toulouse.

After his doctorate and some study periods abroad, Emanuele Conte started his academic career as researcher at Rome's *La Sapienza*, more particularly in the school for archivists and librarians. In the 1990's, he was associate professor of legal history at the universities of Roma Tre and Catania, Sicily. Meanwhile, he became substitute professor in Cagliari, Sardinia, and since 2000, he has been working as *professore ordinario di Storia del Diritto Medievale e Moderno* at the Law Faculty of Roma Tre.

Living, studying and teaching in *la città eterna*, an inexhaustible source of the Western legal tradition, must surely be a privilege, especially for a charismatic and enthusiastic teacher, who also boasts a charming wife and a loving daughter and son. I welcome madam Conte and thank her for honouring us with her presence, and I join her in my sincere congratulations to her husband.

Dear professor Conte, as an expression of our appreciation of your scientific work, we are glad to present you the Sarton Medal. To use a medieval

feudal term, my colleagues and I are honoured to seize this opportunity to render *hommagium* to professor Conte as an exceptional scholar. Thank you for being with us today. May I now invite you to deliver your Sarton Lecture.

Framing the Feudal Bond

A Chapter in the History of the Ius Commune in Medieval Europe

Emanuele Conte
Università degli Studi Roma Tre

I wish to thank my colleagues Georges Martyn, Dirk Heirbaut and Rik Opsommer, the Law Faculty of the University of Gent and the Sarton committee for this honour, which touches me deeply and has also come as a real surprise to me. Or rather, there were three surprises for me: first when Georges Martyn and Dirk Heirbaut originally suggested proposing my name to the Faculty, then when the Faculty accepted my nomination, and finally when the Sarton committee not only decided to award me this honour, but also to consider the law a science, and legal history as a kind of history of science. I note that some of my eminent colleagues in legal history, who have preceded me in the honour of speaking to this assembly, have drawn attention to the striking fact that a great historian of natural sciences and medicine is also commemorated by inviting a legal historian such as myself (*licet infimus*) to speak.

Is law a science?

In fact, the scientific status of jurisprudence was a matter of debate among the German lawyers of the second half of the 19th century, as the firm belief of the historical school that law was a "*Wissenschaft*" began to waver. But the topic is much older! A newly-rediscovered source from the classic age

of the *ius commune*, the thirteenth century, seems to show that doubts about the scientific character of legal study are long-standing and depend on the simple fact that laws and statutes change. This inevitably condemns the complicated constructions of lawyers to evanescence and insures that one day they will all fall. To illustrate this, a medieval theologian, Gentile da Cingoli, tells the story of the two greatest professors of the university of Bologna, Accursius and Odofredus, who went to see the Emperor Frederick the Second, probably in 1239.[1] The powerful imperial chancellor, the famous Pier della Vigna, introduced them by describing them as the greatest practitioners of their science in the world: "*Et Petrus de Vinea dixit Imperatori ut faceret eis honorem, eo quod essent maiores homines de mundo in scientiam*". But Frederick refused to spend much time with them, saying that he had the power to destroy all their knowledge just by changing the laws upon which it was founded.

The same criticism was levelled at middle-19th century German jurisprudence, when Julius von Kirchmann held his famous speech in Berlin on "*die Wertlosigkeit der Jurisprudenz als Wissenschaft*"[2]. In this speech, he attacked the successful school of scientific jurisprudence by emphasizing the peculiarly provisional nature of any attempt to provide a theoretical framework for positive law. "Three rectifying words of the legislator – said Kirchmann – and whole libraries are turned in waste paper".

This same problem of the scientific nature of jurisprudence was raised twenty years later, by the great Rudolf von Jhering, who devoted to the theme his first lecture given at the University of Vienna in 1868.[3] The same Jhering, a few years later, returned to this topic in his famous satirical work "Scherz und Ernst in der Jurisprudenz",[4] in which he imagined himself in

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1 Edited by G. Fioravanti, 'Sermones in lode della filosofia e della logica a Bologna nella prima metà del XIV secolo', in:D. Buzetti, M. Ferriani & A. Tabarroni (ed.), *L'insegnamento della logica a Bologna nel XIV secolo*. Studi e memorie per la storia dell'Università di Bologna, n.s. VIII, Bologna, 1992. The text of Gentile remained unattended by legal historians until the article of Andrea Padovani, '"Tenebo hunc ordinem". Metodo e struttura della lezione nei giuristi medievali (secoli XII-XIV)', in print, which I have read thanks to the kindness of the author. I wish to thank him warmly for that.

2 Edited in Berlin, 1848. There are many reprints: recently in Dornbirn (Austria) in 1999 and 2003 and in Heidelberg in 2000. On Kirchmann, E. Landsberg, *Geschichte der Deutschen Rechtswissenschaft*, III, München, 1910, 737-740.

3 R. Jhering, *Ist die Jurisprudenz eine Wissenschaft? Jherings Wiener Antrittsvorlesung vom 16. Oktober 1868*, (O. Behrends ed.), Göttingen, 1998.

4 The first edition of the book is now online on the site of the Max Planck-Institut für europäische Rechtsgeschichte: http://dlib-pr.mpier.mpg.de/. Many successive editions have been printed in Germany and the work has been translated into different languages.

the heaven of legal concepts, where lawyers who consider themselves scientists construct a network of pure intellectual creations that are unconnected to reality. Jhering's heaven is also home to an interpretation machine, invented by theologians who use it in marvellous ways, and adopted by lawyers to inflate concepts into the words of the laws, and to purge from these concepts the ones that do not fit with their own theories.

Both Jhering and Kirchmann belonged to the most theoretical legal tradition ever, yet manifestly felt uncomfortable with the idea that jurisprudence could be considered a science. They were aware of the singular peculiarity which legal reasoning shared with theology: the creation of concepts which are entirely abstract and non-existent for the human sensibility. What applies to the legal institutions of obligation, property or responsibility also goes for the Trinity, sin, God himself. They are concepts that can be apprehended only as supernatural quantities, non-existent in nature. This is why we cannot claim that jurisprudence is a science similar to natural science. It is exactly the opposite! Instead of explaining what exists in nature by observing it, jurisprudence forces the happenings of real life into abstract concepts that are by definition unnatural. This creates a gulf between legal and empirical natural science – the science based on the experimental method. Mathematical abstractions, which can of course be very subtle, are nevertheless intended to demonstrate a natural reality, and have to be effective in explaining natural rules. We could summarize this major difference by saying that mathematical abstractions are aimed to serve the comprehension of reality, while legal abstractions force natural reality to conform to abstract categories.

However, even if this substantial difference in epistemology called into question the very status of jurisprudence as a science in the late 19th century, it was certainly accepted with equanimity in the age of scholasticism. Despite what Frederick II said, indeed, the medieval idea of science was largely created by the school of Bologna, where lawyers started to read and teach the ancient books of Roman Law, extracting from them legal concepts, as Jhering imagined the lawyers and the theologians doing with their marvellous interpretation machine. In some ways, this tendency of law and theology to re-shape reality into abstract categories is the principle reason for the extraordinary success of medieval scholasticism, as well as for the parallel birth of the universities and their spread all over Europe.

In this sense, even if law and theology do presuppose an epistemology completely different from that which underpins the natural sciences, they have nevertheless played a major role in establishing a 'scientific' approach to knowledge. The outcome is a science very different from natural science, which was harshly criticized by the founders of modern science, but which nevertheless formed the basis of high culture in Europe during the Middle Ages.

Law as a medieval science

In theology, as in law, the medieval idea of science was eminently dialectic, founded on the systematic practice of contradiction and doubt. Yet, law had a peculiarity, one that I think was an important ground for the success of expert 'learned' law in Europe. The intellectual process itself of re-describing in technical vocabulary the very different experience of real life was the essential pre-condition for the application of technical legal procedure, for holding a public trial and ultimately for demanding justice in the most richest region of Europe.

Let me explain this statement. Starting in Italy in the first half of the 12th century, many important courts of Europe began to run according to the Roman procedure, which consisted of the basic rules given by Justinian, principally in his Institutes.[5] There, in book four, the Emperor recalls the classical system of the actions, based on a complex catalogue of ritual formulae, which intended to give a particular legal shape to every complaint made by a plaintiff. In the age of Justinian, the procedure in the courts had evolved, and the ancient formulae for actions and exceptions were not in practical use anymore. Therefore, the very presence of the title *de actionibus* in the Institutions has been considered as a sign of Justinian's 'classicism', showing that he was actually a Byzantine Emperor who dreamed of restoring the glory of classical Rome.[6]

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5 Already Julius von Ficker, *Forschungen zur Reichs- und Rechtsgeschichte Italiens*, III, Innsbruck, 1872, 288-305, noted the penetration of legal culture in the judicial practice of Italy. See also the classic studies of K.W. Noerr, *Zur Stellung des Richters im gelehrten Prozeß der Frühzeit: Iudex secundum allegata non secundum conscientiam iudicat*, Münchener Universitätsschriften, Reihe der Juristischen Fakultät, vol. 2, München, 1967, especially 7-16, and the articles collected in Id., *Iudicium est actus trium personarum. Beiträge zur Geschichte des Zivilprozeßrecht in Europa*, Goldbach, 1993, 87.

6 K.H. Schindler, *Iustinians Haltung zur Klassik. Versuch einer Darstellung an Hand seiner Kontroversen entscheidenden Konstitutionen*, Köln, 1966.

But there was a subtler reason for Justinian to include in the Institutes this long and complex chapter on an obsolete procedure. In fact, the forms of actions foreseen in classical procedure offered the most effective way to interpret the reality of social and economic relationships through the logic of law. That is why the early glossators were particularly drawn to the study of the chapter in the Institutes devoted to the forms of actions, and that is why a very rich literature on actions was quickly produced by the 'learned' lawyers.

Legal science and practice in the age of scholasticism

In the eyes of F.C. von Savigny, the rich literature devoted to the technicalities of the trial which flourished quickly and widely between the 12th and the 13th centuries was a by-product of real legal science, the *Rechtswissenschaft* that he considered reborn only with the advent of commentaries on the Corpus Iuris of Justinian in the forms of glosses and *lecturae*. Despite their very broad geographical diffusion and the numerous manuscripts and later printed editions in which they survive, Savigny did not pay much attention to the genre of *ordines iudiciorum*, nor to collections of *quaestiones*, which he considered as trivial 'practical' literature.

Now, Savigny disregarded the fact that from the 12th century onwards, the authors of the collections of *quaestiones* and of the small and practical treatises on actions were the most prominent 'scientific' and abstract lawyers teaching in Bologna and anywhere else. Already Bulgarus wrote a small work on actions and started to collect the *quaestiones* he discussed in school. His pupil Ioannes Bassianus did the same, in turn his pupil too, the great Azo, one of the most subtle and coherent jurists ever, published a collection of *quaestiones* clearly related to practical concerns. And so on... Savigny was wrong to distinguish so rigidly between theory and practice, wrong in according the character of 'science' only to those writings of which the purpose was to explain the texts of the law.

On the contrary, practice was extremely important in shaping theory. This was probably already clear to Justinian, who inserted the long chapter on actions in the Institutions, the textbook for first-year law students. When does a lawyer actually 'read' reality through the categories of law? He does so when he has to select the right formula with which to ask the judge

for the protection or restoration of the rights of his client. All the theoretical work he has done on the thousands of texts of Justinian is intended to reach this precise goal: to be able to act in court in the most effective way, or in other words, to frame reality in the abstract framework of a legal system.

A shared idiom for Europe

The significance of the title of my lecture today should now be a little more clear. I want to show you a path trodden by some important jurists of the 13th century as they sought to give an abstract framework to the concrete relationship in which feudal society placed lords and vassals at many different levels. Before I do that, I should briefly explain why I think this story is an important chapter in the adventures of *ius commune* in Europe. I am aware of the many critiques by prominent scholars of the idea of a late medieval Europe living in harmony under the same legal system. This picture, proposed by Francesco Calasso in mid-collapse of the Europe of Nations[7] and relaunched by Helmut Coing with the foundation of his Max-Planck Institute devoted to 'European Legal History',[8] is certainly too idealistic.

But neither Calasso nor Coing based their interpretations on nothing at all. The medieval sources give some impressive testimonies to the large and rapid diffusion of a new way of thinking about law. Even if it did not create a general system for ruling Europe, the culture and education of lawyers was, for some centuries, remarkably uniform. Not only was the language of teaching, writing, reading and professional action (Latin) common to the whole continent, but the foundation of texts to which every reference was made was also the same. That is why we see the same texts leaping over boundaries to be read and cited in Italy, Spain, France, Burgundy and other nations. We see French lawyers arguing in a shared idiom against Italian colleagues and we discover that a Catalan book presenting itself as a treaty

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7 The famous books of Francesco Calasso, *Introduzione al diritto comune*, Milan, 1951, and *Medioevo del diritto*, Milan, 1954, had been prepared with many articles written in the 1930's, starting from *Il concetto di diritto comune*, published in *Archivio giuridico*, 1934, 111.

8 Among other works by Helmut Coing, his article *Die europäische Privatrechtsgeschichte der neuren Zeit als einheitliches Forschungsgebiet* is noteworthy in this respect. It was printed in the first issue of the review of the Max-Planck-Institut für europäische Rechtsgeschichte, *Ius Commune*, 1967, 1-33.

on local customs is only an adaptation to local reality of the work of a Burgundian lawyer, written in Italy under the influence of the Bolognese school.

This is probably not the perfectly working 'system', described by Calasso more than sixty years ago, but it is incontestable proof of a shared cultural patrimony, which is peculiarly fitting to recall here, only a few miles away from the heart of the institutional centre of the European Union.

Jean de Blanot and his treaty on actions

Let us now come to the core of our subject, a story taking place in a short period of twenty years. We can start with the work of a Burgundian jurist, Jean de Blanot, written in Bologna in 1256. It is an interesting point of departure, because Jean de Blanot wrote what has been called a '*tractatus de feodis*', a short collection of *quaestiones* that enjoyed wide circulation in Europe.[9] The story is well known among specialists: Blanot inserted the *quaestiones* in his major work, a commentary on the title *de actionibus* of the Institutes of Justinian. The text was then transcribed separately from the rest of the commentary by an unknown Italian writer, who put together an entire manuscript containing legal texts preserved today in Parma. This writer adapted the texts he collected to his practical needs, for example he was interested in Italian fiefs, but did not scruple about attributing to the French professor Jacques de Revigny an Italian treatise on fiefs. Likewise, his version of the *quaestiones* of Blanot is also freely adapted: quotations of the *Libri feudorum* that are absent from the original are introduced in the text, and it is not mentioned that the piece was originally part of a broader work on the actions in general.

In drawing attention to the history of this text, I am not indulging in erudition for erudition's sake. The misleading manuscript of Parma was edited by Jean Acher in 1906[10] and this edition has been used pretty widely by legal historians, simply because it is much easier to use than the manu-

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9 Cfr. R. Feenstra, *Jean de Blanot et la formule Rex Franciae in regno suo princeps est*, in: Id., *Fata iuris romani*, Leiden, 1974, 139-149, and E. Caillemer, *Jean de Blanot*, in: *Mélanges Ch. Appleton*. Annales de l'Université de Lyon, nouvelle série, II, fasc. 13, Lyon, 1903, 53-110.

10 In J. Acher, *Notes sur le droit savant au moyen age*, *Revue Historique de Droit Français et Etranger*, 1906, 138-178.

scripts or the old printed editions. But if we only read the Blanot of the abridged version of the Parma manuscript, we lose exactly what we are looking for: the effort of this lawyer to cast the social and customary reality of his age in the abstract categories offered by Roman law. We can only appreciate this effort by considering the whole structure of the *Tractatus de actionibus* of Jean de Blanot, in which this Burgundian jurist, who had been educated in law at Bologna, tried to teach practitioners in his homeland how to fit the real relationships prevailing in that region of Europe to the procedural patterns described by Roman institutions. By choosing the proper action for lords seeking to assert their justified feudal rights over their recalcitrant vassals, Jean de Blanot had to reflect on the legal nature of this relationship in light of the categories of Roman Law.

This connection between action and substantive right is clear from the first line of his treatise on actions (but not if we only read the abridged version edited by Acher as a treatise on homage). There Jean deals with a classical problem, upon which Placentinus and Ioannes Bassianus had already disagreed during the second half of the 12th century. The dispute arose from a passage of the Institutes where Justinian says that an action is nothing else than the right to pursue in court that to which we are entitled. For Placentinus, this meant that an action is the same as its *causa*, our subjective right which we have by virtue of a real property right or thanks to contractual obligation. His contemporary Ioannes Bassianus challenged this, because an action is altogether different from a subjective right.[11]

Jean de Blanot deals with this, by his time long-standing, controversy by recalling an old metaphor: the action is the daughter of the obligation. As long as the daughter is still inside the belly of the pregnant mother, they are in fact still the same, but as soon as the *actio* is born, it becomes something separate and different from the obligation, that is, from its *causa*. That is why the Institutes of Justinian describe a large number of different actions, each attached to a different kind of individual right. This is a scientific discursive method, entirely in tune with the medieval idea of science. Yet, it is also a method of analysis intimately concerned with practice and

[11] H. Kaufmann, "Causa debendi' und 'causa petendi' bei Glanvill sowie im römischen und kanonischen Recht seiner Zeit', *Traditio*, 1961, 107-162; L. Fowler-Magerl, *Ordines iudiciarii and Libelli de ordine iudiciorum*. Typologie des sources du Moyen Age Occidental, fasc. 63-A-III.1, Turnhout, 1994, 38.

oriented towards practice, directed as Jean de Blanot's text undoubtedly is, to the legal practice of his own country.

By now, it should be clear why it is so important to know precisely what action Blanot thinks is an appropriate means of getting a court to acknowledge the existence of a feudal bond. His choice falls on a particular Roman action, the *actio praeiudicialis in rem*.[12] In proposing this action, the plaintiff affirms that someone is a *libertus*, a freed slave who is forced to perform some services for his former owner. This action is termed *praeiudicialis* because it is a necessary preliminary to and can therefore 'prejudice' every other action. It is an *actio in rem* because it does not arise from contractual obligation. It aims at the recognition of a personal *status*, from which particular duties arise. This independence from a personal obligation is enough to qualify this procedure as an *actio in rem*, because the same book of the Institutes says that '*actione in rem agimus cum eo qui nullo iure nobis est obligatus*' (Inst. 4.6.1: 'we proceed by a real action against someone who is not under any kind of obligation to us').

This is an important point in my argument. Our Burgundian lawyer Jean is saying he thinks that the feudal bond is something else than a contractual obligation. It approximates more closely to the personal status, created by a particular legal act: the solemn freeing or manumission of a slave, at which point a new legal subject comes into being (the *libertus*), whose *status* is burdened by certain specific duties in respect of his former master, duties which are intimately connected with the very person of the *libertus*.

Jean de Blanot was seriously thinking of all this as he wrote his chapter on prejudicial actions, where he decided to insert his famous questions on the fief. As a good graduate of Bologna, he quoted the *Summae* of Azo on the *liberti*, as well as a short treatise on procedure by Pillius, rewritten by Bagarotus, a Bolognese professor whose procedural works were apparently based on other people's writings.[13] Actually, concerning the nature of the duties of the *libertus*, this work of Pillius and Bagarotus set out a rather different opinion on the matter at hand, the precise nature of the obligations

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[12] Described by Inst. 4.6.13: *Praeiudiciales actiones in rem esse videntur, quales sunt, per quas quaeritur, an aliquis liber vel an libertus sit, vel de partu agnoscendo. Ex quibus fere una illa legitimam causam habet, per quam quaeritur, an aliquis liber sit: ceterae ex ipsius praetoris iurisdictione substantiam capiunt*.

[13] On Bagarotus, see H. Lange, *Römisches Recht im Mittelalter*, I, *Die Glossatoren*, München, 1997, 297-299. On the work of Pillius, rewritten by Bagarotus: Fowler-Magerl, *Ordines iudiciarii*, 68.

incumbent on the *libertus*[14], but I do not wish to dwell on that here. I mention it merely as an illustration of the breadth of this jurisprudential undertaking. The starting point in the attempt to abstractly define the concrete reality of the fief was taken from a much older Italian work by Pillius, which had been modernized by Bagarotus. A foreign lawyer then used this work to build a procedural framework for disputes on fiefs in Burgundy, in which he considered it of principal importance to give a definition of the feudal bond in terms of real or personal right.

Action and subjective right: Jacques de Revigny

Why is all this so important? And how is this related to jurisprudence as a science? To try to answer these questions, we can turn to the work of a French lawyer who is of some significance for Flanders: Jacques de Revigny. As the most important master of the school of Orléans, Revigny has been a favourite subject of research for Dutch and Flemish legal historians, because many Dutch and Flemish medieval lawyers were educated in Orléans. Hence, we have only come a few years forward in time from 1256, to around 1270, and to a new university not far from Paris, where some important innovations were taking place in the method of learning and teaching law.

Jacques de Revigny also wrote a lecture on the Roman actions[15] and was familiar with the treatise of Jean de Blanot on the actions, of which he probably did not have a very high opinion. But very distinctly as a jurist, he gives us an admirably clear idea of the importance of the work on actions. In an introduction to his lecture, he explains the distinction between personal and real actions, and clearly shows how a certain kind of action is

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[14] Bagarotus, *Summa Haec arbor duos*, (ed. Palmieri as a work of Pillius de Medicina, *Libellus de preparatoriis litium et earum preambulis "Hec arbor duos"*), Bologna, 1901, 15-68. On the *praeiudiciales,* Bagarotus says that one can ask the *libertus* to perform his duties without expressing the *causa* (i.e. the obligation) because these kinds of duties arises from his *status*: … *Item cur in operis obsequialibus causam impositionis non denotasti, sicut in aliis fabrilibus sive artificialibus? ... respondeo: quia circa petitionem operarum obsequialium non est necessaria causa, nisi quia libertus est; nam ipsa natura, sine ulla impositione debentur a liberto*..

[15] Critical edition by van Soest-Zuurdeeg, *La Lectura sur le titre de actionibus (Inst. 4,6) de Jacques de Révigny*, Leiden, 1989. Discussions on authorship are resumed by K. Bezemer, *What Jacques saw. Thirteenth century France through the eyes of Jacques de Revigny, professor of law at Orleans*, Ius Commune Sonderhefte, 99, Frankfurt am Main, 1997, 140-141, who concludes that Revigny can be considered to be the author, at least of the part dealing with actions.

tied to a particular subjective right. Both personal and real rights are based on a triad of terms. As far as personal rights are concerned, we need to distinguish between a contract, an obligation and an action: a contract creates an obligation and from the obligation arises an action. In the same way, a real action (*rei vindicatio*) must arise from ownership and ownership from a lawful ground (*titulus* and *traditio*).[16] This is why Revigny's distinction between two kinds of actions (and rights) is so important. As you act in court, you must recall the immediate grounds of your action, i.e. the obligation for personal rights and the ownership for real ones. And this difference radically sets real actions apart from personal ones.

That is why it is important to decide whether the feudal bond embodies a personal right or a real right, because that definition determines the role which this bond is to play in the theatre of the law, where men and women are *personae* and their relationships are *obligationes*, where their dealings with one another are *contractus*, and where a man can demand a performance from another because he has a property right in something or – in the case of feudal bond – because he has a property right in certain services which the other must perform because of his *status*. For Jean de Blanot, then, the feudal bond creates the permanent status of vassalage and a concomitant individual right for the lord. This right is a kind of real property in certain performances of the vassal. Therefore, the lord can act against his vassal by means of a real action.

Scattering a legal framework throughout Europe

Does all this seem too subtle, too far removed from real medieval life and important only for a very small group of learned lawyers? Many historians of medieval law think so. For such people, the so-called 'learned law' had a very limited influence on the realities of legal intercourse in medieval communities. This attitude partly echoes the old reactions against *Begriffsjurisprudenz* and nineteenth-century 'Pandectism': reactions of lawyers

[16] *In personalibus est ista tria reperire per ordinem: contractum, obligationem et actionem; contractus est causa remota, obligatio causa proxima. In rei uendicatione similiter tria reperiuntur: titulus, traditio et dominium; titulus et traditio sunt causa remota, dominium est causa proxima et immediata rei uendicationis. Cum ergo in reali debeat allegari in libello causa proxima et inmediata, scilicet dominium, ergo et in personali oportet allegari causam proximam, scilicet obligationem.*

writing around the middle of the-century, such as Julius von Kirchmann, with whose objections to the excessive abstractness of legal doctrine we began, or historians like Georg Beseler, who published his essay on the conflict between the law of the people and the law of the lawyers – *Volksrecht und Juristenrecht* – in 1843.[17] As we have seen, Kirchmann's critique was centred on the claim that positive laws change so quickly as to condemn legal theory to irrelevance. Beseler, as an historian, collected evidence for the resistance of local institutions to penetration by the learned law. He offered a felicitous image of a *Volksrecht* intimately tied to the spirit of a nation and rudely repressed by the power of the lawyers, learned in Roman and canon law. Among historians, this romantic image was, and still is even now, very broadly accepted. Even in Italy, a hundred years ago, legal historians cast the relationship as the struggle of the Italian people against a curiously 'alien' Roman law. Customs and local statutes are still today largely considered as the true expression of popular legal creativity.

Yet, things are more complicated than that. We can no longer accept this old interpretative schema of a conflict between learned and customary law.[18] This is particularly evident in our case, because the very same text could be presented as a treatise of learned law and as a collection of customary law. Let me spend a few minutes on this strange coincidence in the later fortunes of Jean de Blanot's chapter on the feudal relationship, originally written in Bologna for his fellow-Burgundians.

Blanot is perfectly aware of regional variation, of different customs regulating in different ways the creation, extinction and protection of feudal bonds. Describing the ceremony which creates the relationship, *commendatio*, he repeats that in many places (*pluribus partibus*) it contains different symbols: here a kiss is exchanged, there the vassal puts his hands between the hands of the lord. As a customary institution, this commendation or homage can differ from place to place. However, says Jean, although it was not introduced by law, it can be 'helped' by law, that is by

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17 G. Beseler, *Volksrecht und Juristenrecht*, Leipzig, 1843. On Beseler, cfr. B.R. Kern, *Georg Beseler: Leben und Werk*, Berlin, 1982, and F. Wieacker, *Privatrechtsgeschichte der Neuzeit. Unter besonderer Berücksichtigung der deutschen Entwicklung*, Göttingen, 1967, 408-410.

18 I have discussed this in my article *Roman Law vs Custom in a Changing Society: Italy in the Twelfth and Thirteenth Centuries*, in: P. Andersen & M. Münster Swendsen (ed.), *Custom. The Development and Use of a Legal Concept in the Middle* Ages, Copenhagen, 2009, 33-49.

Roman law. Despite local peculiarities, legal science can give a common framework to the feudal bond: it is a particular status, which gives to the lord a kind of real right over his vassals. Jean says very clearly that the feudal homage was not introduced by Roman law, being a customary institution. But it can very well be interpreted (and legally framed) by means of Roman law.

This is not merely a vague statement. The interpretation of the feudal bond in the context of Roman law, as proposed by Jean de Blanot, enjoyed a very considerable success across Europe. In one especially significant case, it was even presented as the custom itself, as we shall see, as we will now turn to events in Catalonia.

Pere Albert and the customs of Catalunia: disguising learned law

It is an amazing story. A Catalan lawyer, Pere Albert, has traditionally been considered by legal historians as a collector of the old good customs of his land.[19] He wrote a book called '*Commemoracions*', in which he claimed to describe Catalan customs. Historians of Catalan society have used it accordingly, as a source describing local law. In fact, when dealing with the feudal relationship, Pere Albert just copies more than twenty chapters of the treatise on the *actiones praeiudiciales* written by Jean de Blanot.[20] Only, he adapts them to Catalan regional practice. First of all, he eliminates hundreds of quotations of Roman law, because a statute passed by the Cortes of Barcelona in 1251 had prohibited all such quotations of Roman law in court. This was not difficult. Pere Albert did not change the argu-

[19] On Pere Albert, see D.J. Kagay, *Pere Albert, Barcelona Canon, Royal Advocate, Feudal Theorist*, *Anuario de Estudios Medievales*, 2001, 39-74; E. Ferran Planas, *El jurista Pere Albert i les Commemoracions*, Barcelona, 2006.

[20] The *Commemoracions* of Pere Albert have been printed in the 16th century together with a commentary of that age: Ioannis de Socarratis iurisconsulti cathalani *in tractatum Petri Alberti de consuetudinibus Cathaloniae inter dominos et vasallos... commentaria*, Lugduni, apud Antonium Vincentium, 1551. I've read it in this edition, confronting the text with a couple of manuscripts: E. Conte, *Servi medievali. Dinamiche del diritto comune*, Roma, 1996, 230-234, where I mentioned the identity of the Catalan treaty with the *quaestiones* of Jean de Blanot. An English translation is now available: *The Customs of Catalonia between Lords and Vassals by the Barcelona Canon Pere Albert: a Practical Guide to Castle Feudalism in Medieval Spain*, translation and commentary by D.J. Kagay, Medieval and Renaissance Texts and Studies, 243, Temple (Arizona), 2002. As far as I can see, Kagay doesn't notice the fact that the text of Pere Albert is actually an adaptation of the work of Jean de Blanot.

ments themselves, based on learned law, he just left out the quotations of passages of the *Corpus Iuris Civilis*, saying that some interpretations are based on common sense and rationality. He also changed the names of persons and regions mentioned by Jean de Blanot: the king of France became the king of Aragon, Germany became Narbonne, Burgundy was Barcelona and Lotharingia Ampurias... In a very smooth way, he made sure that a legal work originally written in Bologna for the demands of legal practice in Burgundy could be adapted for the use of Catalan courts. What is even more interesting, he managed to present a product of learned law, replete with Roman quotations, as a compilation of local customs, completely independent of Roman law.

Still, the fortune of the text of Blanot went further. Towards the end of the 13th century, it was included in the most important and successful handbook of legal procedure of the entire middle ages: the *Speculum Iudiciale* by Guilllaume Duranti.[21] Used everywhere in Europe in manuscript and later as an early printed book (16 incunable editions between 1473 and 1501), the big book written during the 1270s by this professional in the ecclesiastical courts, who later became a high officer of the church and finally bishop, was composed as a real patchwork of existing works. It is probably the best example of how old and new texts, written by civilians and canonists, professors and practitioners, by Italians, Frenchmen, and Englishmen, could interact with one another and form the theoretical basis of legal practice. A great canonist of the 14th century, Johannes Andreae, seems to have understood the particular manner in which the *Speculum* had been constructed, as he wrote an apparatus of glosses on it in which he was able to identify the original authors of the different components gathered together by Duranti.

The particular part of the patchwork where Duranti deals with feudal relationships is an amalgamation of an Italian *tractatus* by Martinus de Fano[22], our *quaestiones* by Jean de Blanot and some passages from a work by Roffredus Beneventanus. It really gives an impression of the fortuitous product of a settled culture, in which abstraction is not pure speculation, but

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21 On the life and works of Guillaume Durand, see the entry *Durand (Durant, Duranti) Guillaume l'ancien* written by F. Roumy for the *Dictionnaire historique des juristes français* (P. Arabeyre, J.-L. Halpérin & J. Krynen ed.), Paris, 2007.

22 C.E. Tavilla, *Homo alterius: i rapporti di dipendenza personale nella dottrina del Duecento. Il trattato de hominiciis di Martino da Fano*, Napoli, 1993.

the only effective way of deploying the same conceptual procedures in the very dissimilar local contexts created by different European customary laws.

Jacques de Revigny & Raoul d'Harcourt: a dialectical science

While in Catalonia, Pere Albert was using Jean de Blanot's *quaestiones* in his presentation of Catalan local customs, a few hundred miles to the east, Guillaume Duranti was fusing that same text with other Italian materials to create the chapter on fiefs for his *Speculum*. Meanwhile, in Orléans, Jacques de Revigny was working on the same topics and reading the same texts. Like Jean de Banot, Jacques was engaged on a treaty on actions, and he therefore faced the same problem of fitting the successful and pervasive medieval institution of homage into the grid imposed by the Roman forms of action, where no action existed that was specially devoted to such relationships.

On a purely formal level, regarding the order of treatment in his discussion, Jacques decided to keep to the path opened up by his Burgundian predecessor, whose work he must have had on his desk[23] as he composed his own treatise.[24] He thus addressed fiefs when dealing with *actiones praeiudiciales*. Yet, when it came to the actual grounds of action, he disagreed with Jean de Blanot on the legal nature of the feudal bond. Even if he treated the matter in his chapter about a particular kind of real action or *actio in rem*, which, as we have seen, the *praeiudicialis* is, for Jacques de Revigny the rights of the lord over his vassal had nothing to do with property rights. Rather, they were personal rights, arising from an obligation!

Revigny was a very good lawyer: he argued clearly and methodically, proceeding step by step in his demonstration. On the *praeiudiciales*, he dealt first with the proper action, the *praeiudicialis directa*, concerning the very *status* of a person: a serf, a *libertus*, a son. He then passed to the *actiones praeiudiciales utiles*, actions lawyers can create by way of inter-

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23 Both Blanot and Revigny quote the same passage of the cited work of (Pillius) Bagarotus in the same position. That cannot be a coincidence.

24 On the relationship between Jacques de Revigny and Jean de Blanot, see R. Feenstra, 'Quaestiones de materia feudorum de Jacques de Revigny', *Studi Senesi*, 1972, 379-401, now in Id., *Fata iuris romani*, Leiden, 1974, 298-320.

pretation, *ex mente legis*, i.e. by extending the very meaning of the law to similar cases. Here is where we encounter Blanot's discourse on feudal homage, and this is where Revigny forthrightly denies that one can use such an action *in rem* in court against a vassal who does not behave as he should. As we have seen, for Jean de Blanot the use of an action is justified on the basis of the right of the plaintiff – in this case, a real right. For Revigny, a lord has no such real right over his vassal, because the nature of the feudal bond is purely personal.[25] Let us dwell a little on this important statement. It is not only a matter of theory: if there is no real right in the relationship between lord and vassal, there is no possession, because only things can be possessed. But if there is no possession, it is also impossible to acquire the right by prescription. That means the plaintiff cannot prove his right by appealing to the passage of time, because time does not create any personal obligations, only real rights. Therefore, Revigny's location of the feudal bond in the framework of personal rights had profound implications in practice, because the only way to prove the existence of such a relationship was to show the court the record of a contract.

This idea of Revigny is confirmed in the work of his pupil, Raoul d'Harcourt, who also wrote a commentary on the Roman actions, with a similar emphasis on the *praeiudiciales*. Raoul explains very clearly that homage, the feudal bond that binds a lord and his vassal, is not properly a personal *status*.[26] He tells us that Revigny made a clear distinction between vassalage and the condition of peasants, tied to the land of their lord during the middle ages as they were already in late antiquity. For them Justinian

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[25] Van Soest-Zuurdeeg (ed.), *La Lectura*, 328: *Dictum est de preiudicialibus directis. Quedam, dicunt ipsi, sunt utiles ex mente legis, ut in quasi servis et in quibuscumque hominibus libere condicionis; ad hoc est C. in quibus ca. bo. tra. do. accu. pos. l. i. (C. 11.50.1). Istam extensionem bene approbo. […] Est alia utilis. Tu es vasallus meus si agnoscis homagium meum. Cum sis homo meus, licet sis liber, habebit locum utilis, ut dicunt. Dico quod istam extensionem non approbo, quia in persona vasalli non pono aliquid iuris realis nec actionem in rem directam uel utilem, sed personalem obligationem recipiendo feudum ad diversa genera officiorum secundum consuetudines terrarum. […] Vnde breviter dico quod in homagio non attendo ius reale, sed tantum personalem obligationem, et tempus non est modus tollende vel inducende obligationem, nisi sit tantum a quo non extat memoria.*

[26] Cfr. the *Repetitiones* of Raoul printed under the name of Iacopo d'Arena (*I. de A. Parmensis viri clarissimi, iuris utriusque professoris Commentarii in universum ius civile*..., Lugduni, 1541, f° 294), bibliography in Conte, *Servi medievali*, 200-201): *Item dicit Io. de Ble. in homine: "Dico contra te quod tu es homo meus ligius, unde cum tu neges hominem meum <esse>, peto te pronunciari meum" et sicut dixi in ascriptitio. Dominus negat ius reale in homine, unde format libellum in personali: "Dico quod promisisti fidelitatem et servitium talem per talem stipulationem" si sit stipulatus, vel "per iuramentum: unde peto te condemnari ad servitium prestandum".*

had foreseen a special personal condition, the colonate, which is something between the *status* of a freeman and a slave. The *actio praeiudicialis in rem* could well be used in court for the acknowledgment of this condition. On the other hand, while a lord can ask a judge to confirm in court that a peasant is his *colonus*, and as such is obliged to work the land where he is forced to live, he cannot do the same with a vassal, because the act of homage is a contract, formally created by *stipulatio* or by an oath.

Whereas the legal construction given to the feudal bond by Jean de Blanot was spreading all over Europe, largely thanks to the successful work of Guillaume Duranti, it was also subjected to criticism by the most important masters of the school of Orléans, at that time the most progressive and innovative centre for the study of Roman law.

The practice of the courts, in France as elsewhere, was therefore confronted with an ambiguity, because the same social reality was described in different ways by acknowledged scientific legal texts. For some, homage created a sort of real right of the lord over the body of his vassal; for others, the same act produced nothing but a personal obligation. From this basic difference, there descended a whole cascade of further distinctions: as would-be lord you had to plead by means of a different action, offer a different mode of proof, which may or may not include the appeal to the passage of time.

Now, we can consider this ambiguity as a failure on the part of this medieval system of 'scientific' jurisprudence, which we might justifiably expect to use the same conceptual tools for the analysis of varying local institutions. But this would be wrong, because the very structure of the medieval scholastic idea of a 'science' is completely different from the modern one. The opposition between conflicting opinions was absolutely fundamental to the dialectic character of knowledge typical of that time. Disputes held in school as *quaestiones*, or in court as trials, were necessary means of confronting different theoretical views of the same practical problem, and this daily familiarity with doubt was actually at the very core of a 'scientific' attitude towards reality.

In our case, the two different ways of embedding the feudal relationship into the framework of learned law, whose origins we can trace back to the mutual opposition of two French schools, are actually the root of a long-lasting divergence between Roman and canon lawyers. Jean de Blanot was

perhaps less subtle than Jacques de Revigny, but he certainly identified and shared the spirit of his age. Probably, the ecclesiastical rulers, the popes, had this same intuition as they tried to regulate the countless hierarchical relationships which bound the churches and other sacred institutions of the whole continent into an immense network. Since the times of Innocent III, well before Jean de Blanot, these relationships had been regulated in the papal decretals as subjective rights, a kind of peculiar property, not depending on any contractual obligation.[27] That is probably why a canonist and a practical lawyer as Guillaume Duranti adopted Blanot's position in his *Speculum*.

From status to contract (and back)

By way of simplification we might say that during the 13th century, some very influential jurists, such as Jacques de Revigny and some Italians before him, proposed a shift from status to contract, to recall the famous formula of Henry Sumner Maine.[28] However, in the same decades, ecclesiastical legislation and the interpretations of some lawyers, who were more attentive to the demands of practice, tended to hold true to tradition, by considering *status* as the main source of legal obligations and social relationships.

How did the story end? Let us look briefly at the state of the discussion a century later, at the end of the 14th century. In the commentary of the canonist Antonius de Butrio, we encounter the same old parallel drawn by Jean de Blanot between the personal conditions such as those of the *libertus* or *colonus* – or citizen, or son – and that of the medieval vassal. Antonius adds the monastic and priestly conditions to the list of personal *status* that can be treated as generating a kind of real right. Antonius is very explicit: as an obligation to do or to give something depends on a state of subjection

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27 See, among others X. 2.13.17 (ownership of a *ius parochiale*), X. 2.30.4 (*possessio subiectionis*), X. 2.27.21 (*quasi possessio obedientiae*), X. 3.36.7 (where a bishop claims the ownership of his rights over a church), X 5.33.14 (ownership of privileges and immunities); X. 2.20.30 (*possessio archidiaconatus*), X. 2.28.46 (*possessio prepositurae*), X. 1.3.22 (*possessio corporalis abbatiae*), X. 2.30.6 (*possessio prioratus*), X. 1.10.6 (*possessio cantoriae*). The rights to receive grants are considered as a real right in many constitutions of the title X. 3.8 *de concessione praebendae.*

28 H.S. Maine, *Ancient Law. Its Connection with the Early History of Society and its Relations to Modern Ideas* (1861), Boston, 1963, 163-165. For a commentary from a legal-historical point of view: P. Stein, *Legal Evolution. The Story of an Idea*, Cambridge, 1980, 85.

or bondage, it assumes the form of a real right. As he puts it, it *smells* like a real right.[29]

In the exact same period, the great Baldus de Ubaldis, who was both a Roman and a canon lawyer, uses the very same wording to align himself on the side of the canonists. There are obligations that depend on a personal subjection; they are not material things, yet they taste like real rights.[30] Baldus repeats the same statement in his additions to the *Speculum* of Guillaume Duranti, where his well-known inclination to philosophical interpretation of law meets a text openly dedicated to practice.[31] The circle is then closed: the criticism raised by the French lawyers against Jean de Blanot is now overcome by the later Italian followers of the same method invented by the school of Orléans and Jacques de Revigny. There are obligations, says Baldus, that are not simply based on contracts. He calls them *obligationes relativae*, because they express a duty that arises from a personal subjection, as is the case with a vassal or a peasant. These obligations are real rights. As such, one can own them, protect them with possessory proceedings or acquire them by the passage of time. This is exactly what

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29 A. de Butrio, *Supra prima secundi Decretalium commentaria*, Venetiis, 1578, *in decr. In causa* (X. 2.12.8), num. 14, 112va: *Secundo dicebam quod erant quaedam iura quae dabantur in personam velut rem: et si ista habent in se dubium, vel eius partem, dabitur vera possessio, ut in iure servitutis: […]: Ius libertinitatis, ius ascripticiatus, ius monachatus, ius clericatus, ius civilitatis, ius patriae potestatis et multa iura quae in homine<m> haberi possunt [...] Et tunc omnium illorum iurium est dare quasi possessionem, quia et illorum lata proprietas... Dico quod quaedam sunt praestationes personales quae alteri iuri annectuntur, et huius debiti est dare quasi possessionem ut est praestatio census ratione subiectionis, ut hic et d. c. Querelam (X. 1.6.24). Nam annectitur iuri reali subiectionis: illius ergo sapit naturam [...], ut est praestatio fidelitatis respectu vassallitici iuris, quod est reale, ut in li. feu. c. i. et Qua olim. feu. po. ali. c. i. (LL.FF. 1.1.1 et 2.9)*.

30 Baldus de Ubaldis, *Lectura decretalium*, in X. 2.26 rub. § 4 (ed. Lugduni, 1551, f° 324): *Vel dic quod ibi est tale ius personale quod vindicari potest, quia sapit realitatem subiectionis, arg. ff. ad muni. l. De iure (D. 50.1.37).* – Id., in c. *Querelam* (X. 1.6.24): *"Et scias quod omne quod potest praescribi possidetur vel quali, alias praescriptio non posset procedere. Sileant ergo Iaco. de Ra. et sui sequaces, qui contra hanc decretalem somniarunt, ut ipsi referunt C. de pactis l. Si certis annis (C. 2.3.28). Est praeterea quaedam possessio praestationis relativa, sicut possessio feudi soldatae. Nam si dominus possideat vasallum, idest qualitatem eius, reciproce et vasallus feudum, argument. insitutio. de actionibus § Praeiudiciales (Inst. 4.6.13)*.

31 The additions of Baldus are printed in the many 15th and 16th century editions of the *Speculum*. In the *additio* to the title *de restitutione spoliatorum* Baldo writes: *Reditus et servitia inter immobilia computantur, et ideo quasi possidentur, et habent locum quasi interdicta, versi. Consilium ergo G. Hic nota quod ista iura non habent proprie naturam servitutum, quae in patiendo, non in dando consistunt, ff. de servi. l. Quot. (D. 8.1.15.1). Nam omnis servitus aut debetur a re rei, aut a re personae, ut ususfructus, aut a persona non omnino libera, sed quasi subiecta alterius personae, ut vasallus domino, vel a colono glebae annexo […] Quomodo ergo ius reddituum quasi possidetur? Resp.: quaedam sunt obligationes simplices absolute, quaedam respective seu connotative: in simplici obligatione non cadit quasi possessio, sed in obligatione relativa ad quasi servitutem vel subiectionem vel dominium sic*.

Pillius and Bagarotus said two centuries earlier, when considering the *operae libertorum* as a kind of property of the former owner in his former slave.

To sum up, if we try to penetrate the jungle of scholastic argumentations to read them in the context of the real life of the courts, we can see how closely the medieval science of law was connected with the life of European societies. Thanks to the continent-wide circulation of doctrines, the so-called 'learned law' of the last four centuries of the Middle Ages was able to reach a common conceptual framework for legal practice across the continent. This framework was not unambiguous nor univocal, being the creation of a scholastic science whose main feature was dialectic. But it provided a common language, a common grammar for the thousands of professional lawyers engaged in the courts and in the government across the whole continent. For such a growing economy and such an evolving society as that of the 13th century, the feudal bond could be seen as a merely personal contract. For the seigniorial society of the late 14th, it was more a form of property right, marking the powers of noble families and the onerous duties of the so-called third estate. All the debates surrounding this topic were only possible thanks to the abstract categories invented by legal science.

Conclusions

We should beware of excessive enthusiasm in eulogizing the 'common legal past of Europe' or some irenic 'medieval order'. Medieval Europe was a mess, probably even worse than today's Europe.

But the resilience of a general culture based on a conceptual system did make things very different from the way they are today. Legal rules given by customs and by an increasing number of legislators were interpreted in light of this broad culture, which easily metabolized the new legislation and the old customs, arranging them into the conceptual framework created by doctrine.

In fact, it was the great age of the triumph of science over legislation, rightly evoked by the most important supporter of the idea that law is a science, a *Wissenschaft*: F.C. von Savigny. He very clearly acknowledged

how important the twelfth-century foundations were of the idea that the endless variability of human relationships could be framed in a grid of a few basic legal institutions and analyzed in the abstract. That idea, famously, would have a long future. In the 19th century, the age of the ruling power of the State, Savigny was able to impose this medieval idea on continental Europe – for which his early critics reproached him. Despite that attempt by Savigny, and despite all the intervening successes of Pandectism in the law faculties of Europe, things are now completely different. Legal statutes and norms at every level apply today to the very reality of daily events, so that mediation by means of abstract concepts tends towards the useless. The age of legal frameworks is perhaps forever behind us.

Laudatio Luc Daels

Philippe De Maeyer

As a close colleague, on behalf of the Faculty of Sciences of Ghent University I have the honour of introducing colleague Luc Daels.

In its session of 27 January 2010 the Faculty of Sciences proposed awarding the Sarton Medal in the 2010-2011 academic year to colleague Luc Daels, an honorary professor at the Faculty. The Sarton committee approved this proposal.

Luc Daels was born on 22 May 1929 in Gent. In 1953 he was awarded the diploma of Licentiate in the Sciences in the Geography Department at Ghent University. In the following years he worked as a geography teacher at the state secondary schools in Berchem and Brussels. Six years later – in 1959 – he became an assistant to Prof. Frans Snacken at the Seminar for Regional Geography at Ghent University. In September 1962 Luc Daels received the doctoral degree in Sciences for geography.

A year later he was appointed and later confirmed as assistant professor. In 1969-1970 he took the opportunity of a research residency and lectureship at the *University of Pennsylvania*. In the following years he appeared a number of times as a visiting lecturer in countries including the US and Greece. In 1973 Luc Daels became the associated lecturer to the seminar for regional geography, in 1979 associated professor, and in 1987 full professor. On 1 October 1994 he became an honorary professor. Between 1988 and 1994 Luc Daels was also director/department head of the Laboratory for Regional Geography and Landscape Science

In his career at our university, right from the start Luc Daels conducted research on the historical development of the landscape in Flanders. He

was one of the pioneers in critically using historical maps in the study of the genesis of the landscape. The historical approach – also applied by Dr Verhoeve and the late Prof. Schmook – could then not yet base itself on the many technical resources that are now available.

But the geographer does not only look at the past. The present and even future town and country planning form part of the research field of the geographer. Luc Daels hence introduced the new technologies of aerial photography interpretation at the Ghent University. The identification of landscape relics not only appears possible on aerial photographs; as soon as satellite images are obtained with a useful resolution these are also used for historical landscape research. The application fields of satellite images are not, however, limited to historical landscape science. For many years Luc Daels also applied remote sensing in soil science research, and became a PhD supervisor in soil science. Luc Daels being at the cradle of remote sensing research is demonstrated by his involvement as a co-founder of EARSEL, the coordinated European body of remote sensing laboratories.

Since the sixties Luc Daels also widely broadened his research field, with Greece and the Cyclades being new regional landscapes for study in particular. Not only was his research into the landscape genesis to become dominated by the azure Mediterranean Sea colours, his artistic activities were also strongly influenced by the intense Mediterranean landscape impressions.

After all, behind each researcher also lurks a person with reason and emotions. Luc Daels wanted to be able to share his perception of the landscape.

For years he was the driving force of *Het Andere Landschap* (The Other Landscape). Het Andere Landschap was born on 20 March 1989. In the auditorium and the peristyle an exhibition was then inaugurated with the focus on the landscape: the landscape as a phenomenon, as a field of research, as an experience.

A series of lectures followed this first initiative in the autumn of 1989. It is no coincidence that the first lecture was devoted to Greece. Each year between 1989 and 2005 autumn and winter lectures were presented in a crammed auditorium. So, for us to honour him here is a deliberate choice.

In the spring, excursions in Flanders, Hainault and French Flanders completed these explorations of the 'other landscape'. Together with his colleague Marc Antrop, Luc Daels organised more than 60 readings and 70 excursions within the context of 'Het Andere Landschap'. These activities were only made possible by the spontaneous cooperation of the members of the university community, but also from further afield. This initiative made a perfect link between research and society.

What is now included in job descriptions under the denominator "external social services" has been supplemented by Luc Daels with every enthusiasm. Luc Daels also worked as a versatile artist, and he has made a major contribution in Ghent to the flourishing of the cultural experience. He has contributed to the resurrection of the Ghent Festivities, and he is still active today as the vice-chairman of the Board of "Het Huis van Alijn".

That a vision of the origination of the landscape can also *be 'otherwise'* will certainly demonstrate your reasoning encompassed by **'The landscape: the memory of humanity'**.

The landscape: the history of mankind

Luc Daels

The landscape is a natural picture of the history of mankind.

The main idea is well expressed by a citation of the most interesting book[1] of the Dutch geographer-philosopher, Ton Lemaire: "The philosophy and the vista, each encountering with the horizon of our existence, are practices of the aspiration of the general, of the all embracing and the universal. They invite us to pass the limited circles of our daily occupations. He who stays prisoner, within the limits of his work, does not have an open-mind for the landscape and the philosophy."

The meaning of this paper is indeed a search for feeling and thinking, in other terms of time and space. Therefore, we will explore different landscapes. For a correct understanding, there is a need to formulate the definition of landscape and landscape-memory.

The *landscape* is considered as the amalgamation of different physical factors and their reciprocal influence. Much later, the human factors will act and react upon the physical landscapes and create the cultural landscape.

It is amazing to consider the different ideas about the age of the Earth. Radiometric dating of the oldest minerals of the Earth's crust yields an age of 4,570 million years. The Greek and the Germanic mythologies attribute only some thousands of years to the age of the Earth. The creationists ascertain the Earth to be between 6,000 and 12,000 years old. Most surprising is

[1] Filosofie van het landschap, Ton Lemaire, Ambo Baarn, p. 11 (1970)

the fact that the old Hindu mythology proposes 4,320 million years (this is the duration of one day of the life of Brahma!).

The *memory* is defined as the ability of humans and animals to retain information. This memory is stored in their brains. The Earth memory is localized in and on the Earth's crust, comprising three aspects: the storage, the conservation and finally the understanding of it. The information can be considered from a purely chemical point of view – the chemical elements, or from the aspect of the minerals as the combination of the chemical elements, or from the point of view of the rock material that is the composition of different minerals. The human activities occurring much later, are also inscribed in or at the surface of the Earth.

The second aspect of the memory is the capability to keep or to preserve the information. The rock material, of various origins, is deposited in successive layers, called a geological profile. It is obvious that erosion, accumulation and a great number of physical and human factors have altered the initial succession of these layers.

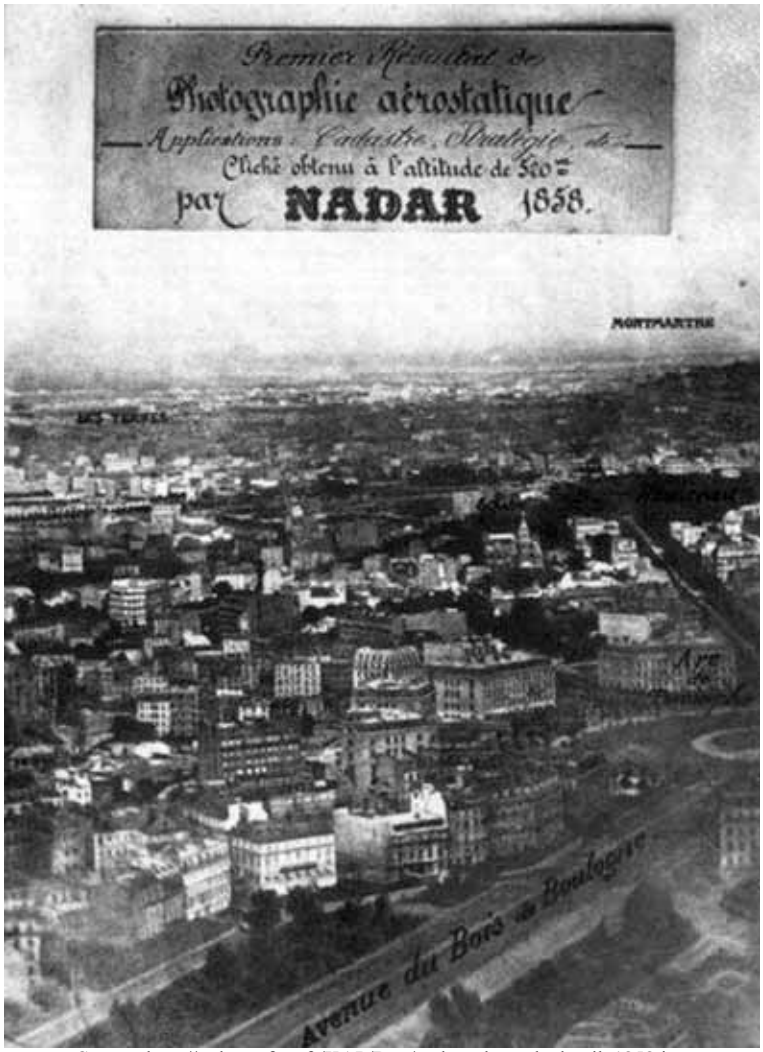
Considering all different factors, it becomes evident that it is impossible to define geography in terms of research subject; indeed, everything dispersed on the Earth's surface belongs to a geographical examination. Immanuel Kant (1724-1804), the German philosopher, defined clearly the place of geography amongst the other sciences: "… Geography is a point of view, a system of processes and mutual influences, it is a synthetic science …".

The reading or understanding of the enormous amount of information forms the third part of the Earth-memory and requires a multidisciplinary approach.

The aerial photographs and their interpretation are a great help in seeing the surface patterns and the understanding of their meaning. It first began in 1850 when Mr. Felix Tournachon, also known as Nadar, took the first aerial pictures of Paris.

It is in about the last 50 years that scientists have begun using the images taken by the astronauts. These space images are beautiful and interesting, but were taken occasionally. Systematically recorded satellite images are available since 1973. They are realized at different altitudes (between 600 km and 36,000 km) and with varying sensing methods. These images offer

Figure 1. Nadar: first aerial photograph of Paris



Source: http://volmag.free.fr/KAP/Doc/nadar_place_de_letoile1858.jpg

abundant information and became indispensable for geographers, geologists, soil scientists, archaeologists, botanists, and so on.

In the following some different landscapes will be discussed to illustrate the information of the Earth's logbook.

Figure 2. The Grand Canyon (Arizona, USA)

The Grand Canyon (Arizona, USA) gives most probably the best reading of the long history of the Earth, when examining the 1600 meter high canyon walls. The Grand Canyon is the result of an uplifting (mountain building in the north-western part of the USA) and the subsequent cutting by the Colorado River. The result is that at the bottom very old (4000 million years) rocks are visible, followed by a succession of gradual younger materials up to the surface, partly covered by volcanic layers of about 1,000 years old. Not only can all the geomorphologic processes be seen clearly, but also the evolution of the different life-environments (plants and animals) and on the top some evidence of human activities are present.

The White Desert, in Egypt (Farafra, Sahara el Beida) is another remarkable example of the long history of Earth's history. One of the most important floodings (transgressions) on Earth happened during the Cretaceous period (Mesozoïcum, 250-65 million years ago). This world sea (the Thetys Sea) covered the north-western part of Egypt, reaching Luxor. At the bottom of this rather shallow and warm sea, thick layers of a white sediment rich in lime and chalk were deposited. Due to the differences in hardness, together with an intricate system of joints, a most bizarre scenery was

Figure 3. The White Desert (Farafra, Egypt)

created; A part of the long terrestrial development can be read from the successive blinding white layers.

Approximately 2.5 million years ago, a glacial period started again. The evidences of these very cold periods (glacial) alternated by less colder intervals (interglacial) are numerous in our landscapes. These cryoturbations, for instance the pingos or frosthills, and the frost-cracks occur in all areas where a periglacial climate prevailed, characterized by a permafrost.

It is also in this epoch that man entered upon the Earth's scenery. He marked his presence by making tools out of flint (the Stone Age). Men liked to confirm their presence by making inscriptions on rock walls or in caverns.

These petroglyphs are widely spread all over the Earth. Sometimes these marks took surprising forms and dimensions. These drawings on macro scale, are in most cases visible only from above. The Nasca petroglyphs (Andes, Peru) are amongst the most exceptional. They consist of enormous mystical biomorphic figures or of straight lines, 14 km in length.

Our ancestors liked to express their cultural belief in different shapes and dimensions; the enormous sculptures of the Eastern islands or the giant stone-circles of Stonehenge. Excavations evidenced a very old age, approximately 8,000 BC, but the present Stonehenge, more or less, is dated at about 2,300 BC.

Figure 4. Frost-cracks (cryoturbations) in Aalter, East Flanders, Belgium

Source: Department of Archaeology, Ghent University

Figure 5. Petroglyphs (Wadi Rum, Jordan)

Figure 6. Biomorphic figure (Nasca, Peru)

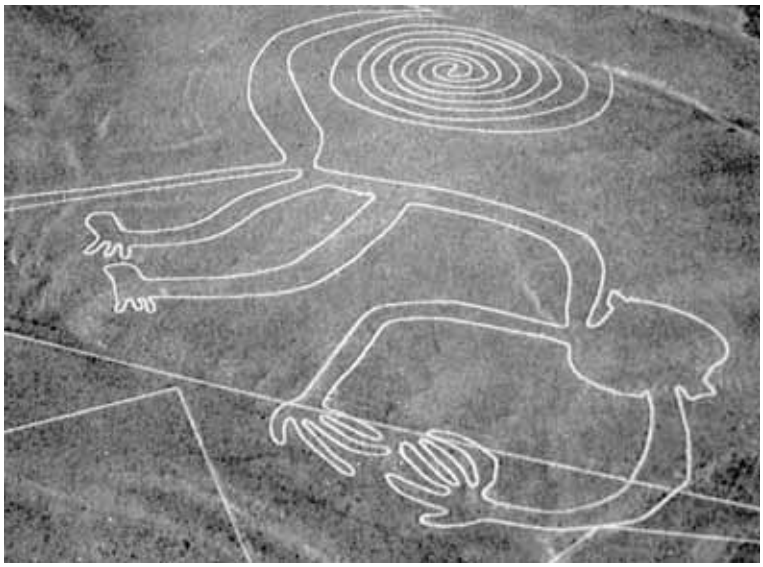


Figure 7. Stonehenge (UK)

In areas where the hard rock material is scarce as for instance in Flanders, people constructed similar circular structures, but they used wood instead of stones. Is this the explanation for the remarkable circles, as can be seen near Bruges, or elsewhere in Flanders?

Figure 8. Vegetation marks near Bruges, Belgium

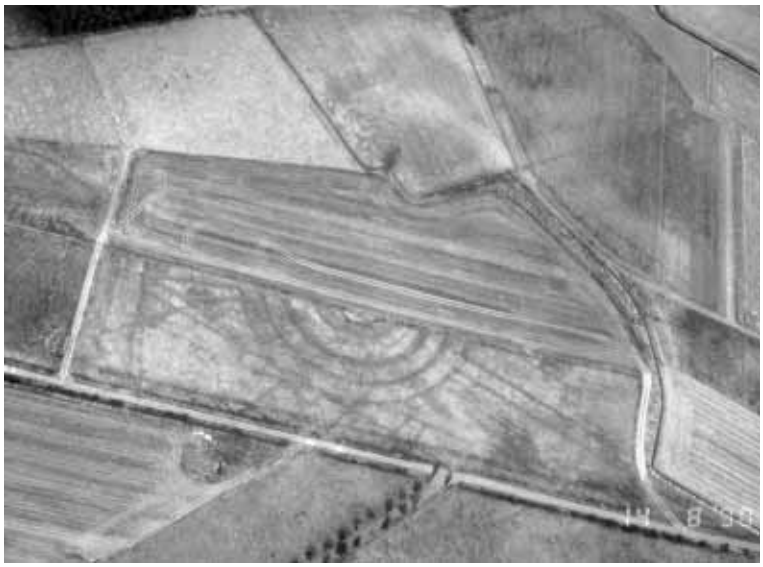
Source: Department of Archaeology, Ghent University

It is very tempting to discuss widely these megalithic monuments more widely in Europe. There are more than 50,000 such monuments in Europe, from Denmark to Portugal, varying in size and meaning.

The aim of this paper, however, is to look at the evidence of human activities which are hidden in the landscape. Respect for the deceased can be seen in the landscape in all time periods, in all parts of the world and in varying shapes, for instance in the pyramids of Egypt, or the churchyards in Europe.

A special mark of honour and respect is evident in the hill tombs of the Bronze Age (1,600 BC). In Flanders, they are mostly flattened due to agricultural activities. A great number of the hill tombs, however, could be localized thanks to aerial photographs.

Figure 9. Vegetation marks in Flanders, Belgium



Source: Department of Archaeology, Ghent University

Figure 10. Hill tomb of the Bronze Age in Flanders, Belgium

Source: Department of Archaeology, Ghent University

Political situations also created specific landforms. An example of this is the castral moat, a witness of the turbulent feudal period (11-13th century in Western Europe). These strongholds were numerous in Flanders. They comprised two parts: the smaller and high elevated escape moat; and the larger and low lying area, the bailey. The latter forms an enclosed courtyard where the master and his family lived, together with the soldiers, the servants and the cattle. The total stronghold, smaller or bigger, formed the shape of the figure "8". In the present day landscape they are easily recognizable.

Figure 11. Castral moat: the "Hoge Wal" in Ertvelde, Belgium

Source: Department of Archaeology, Ghent University

Agricultural techniques obviously influenced the landscape, namely the cultural landscape. It seems clear that in North-western Europe the first farmers, the people bound to a specific piece of land, became active in approximately 3,000 BC. Evidences of the early agricultural landscape are rather rare. The "Celtic Fields" with their irregular chessboard pattern represent the oldest evidences. They appeared upon the higher situated sandy loam soils, between 700 BC and 200 AD. They represented the agricultural fields of the Iron Age; A careful observation of the shape and

Figure 12. Castral moat in Werken, Belgium

Source: Department of Archaeology, Ghent University

dimension s of the fields and parcels taught a great deal about our ancestors' way of life.

The open field landscapes are the areas in which no enclosures are present between the field plots. In the framework of these open field areas, the old land organization of the "kouters" (cultura) arose. This agricultural organization is characterized by a severe three-fold rotation system (Gallo-Roman to early Middle Ages). This system was an expression of a joint agricultural organization. These open areas are surrounded by closed agricultural areas (called in Dutch "Bulken"), which showed an example of the private land management.

Another landscape building element is formed by the village greens (Dutch: "dries"), an open space where the village herd was confined during the night for protection.

The landscape of the "Meetjesland", an area situated in the northwest of the province of East Flanders, illustrates a prime example of the influence of combined actions, human (political) and physical factors, upon the landscape genesis. The countess of Flanders (Johanna of Constantinople, 1194-1244) took the decision to reclaim the Meetjesland due to the need for

Figure 13. Open field landscape near Huise, Belgium

Source: Department of Archaeology, Ghent University

Figure 14. Bulken landscape in sandy Flanders, Belgium

Source: Department of Archaeology, Ghent University

higher food production, given the fast growing population of the 11th-13th century. This resulted in a systematic land reclamation in square, regular field blocs, which were subsequently divided in elongated narrow parcels.

Figure 15. Land division in the Meetjesland, East-Flanders, Belgium

Source: Department of Archaeology, Ghent University

At the end of the 14th century, the area was strongly affected by floodings; the northern part of the Meetjesland was covered by a sheet of clay. Upon this "new" surface, a new division of land was created. Where the clay sheet is shallow, the former parcelling is translucent through the clay layer.

This clearly illustrates the idea formulated by Immanuel Kant that "time and space are one point of the reality". This paper must be read as an incentive to ponder in terms of space and time, attempting to read the landscape as an open book.

Based upon the comparisons coming out of the aforementioned examples it is obvious that the landscape has its own character and is a proof of the important relation between nature and mankind.

Figure 16. Old parcels appearing through the clay layer in Assenede, Belgium

Source: Department of Archaeology, Ghent University

Laudatio Hartmann Tyrell

Raf Vanderstraeten

It is a great honor and a great pleasure for me to welcome Prof. Hartmann Tyrell here as the recipient of the George-Sarton-Medal of Ghent University. This award was instituted exactly 25 years ago, at the centenary of Sarton's birthday. The first award of the Sarton committee was accepted by a former student of Sarton, named Robert King Merton. Merton wrote his Ph.D. under the supervision of Sarton, this work "Science, Technology and Society in Seventeenth Century England" was published as a monograph in Osiris, a periodical also run by Sarton. In the academic year 1986/87, Merton delivered his inaugural lecture here on the Matthew effect in science. Hartmann Tyrell only is the second sociologist to receive this award of the Sarton committee. And he is a well-deserved successor to Merton.

Hartmann Tyrell spent most of his academic career at the University of Bielefeld (Germany). This university was established in the second half of the 1960s; it was explicitly conceived of as a research university. From the onset, it also put a premium on history and sociology. Both history and sociology were and are not simple university departments, they constitute large and independent faculties. Both faculties were also able to recruit the best staff and attract the best students. It is no coincidence that the first sociologist to receive a honorary doctoral degree from Ghent University was a Bielefeld professor, namely Niklas Luhmann. Hartmann Tyrell lectured at the University of Bielefeld for nearly four decades. He has introduced generation after generation into the history and theory of sociology. He must have given countless lectures on Emile Durkheim, Max Weber, Georg Simmel, Talcott Parsons, Robert Merton and other classical sociol-

ogists. Even now, in the first years after his retirement, he continues to lecture in this domain. As few others, and I have often seen it myself (although I have never been a student of him), Hartmann Tyrell is able to stimulate interest in the history of the discipline and to motivate people to devote their efforts to work within this domain. He can be an unrelenting and sharp critic, also of the work of young researchers, but for them his remarks often constitute a difference which makes a difference.

In the past 40 or so years, Hartmann Tyrell has published influential work – perhaps especially on processes of social differentiation and historical change, on the one hand, and on sociology around 1900, on the other. His most important papers were recently reprinted in the volume *Soziale und Gesellschaftliche Differenzierung*.[1] But his contributions to science work in a direct as well as in an indirect way. For almost a decade, until his retirement, he was also the editor-in-chief of the leading German journal in the field, the *Zeitschrift für Soziologie*. In this regard, his profile is highly similar to that of George Sarton, who was also the editor of main scholarly outlets, namely the journal *Isis* and the companion yearbook *Osiris*. These journals facilitate the communication of research findings. They bring together a research community, they stimulate interchanges, they set standards, they define themes. This editorial work is often less visible – but when it is well-done, its scientific relevance cannot be underestimated. In this regard, too, Hartmann Tyrell deserves much praise. There are not many colleagues, who primarily define their own role in terms of facilitating the scholarship of others.

Let me finally also say something about the Department of Sociology here in Ghent. The Faculty of Political and Social Sciences, of which it is part, is about to celebrate its 20th anniversary. The social sciences existed in Ghent before the 1990s, but they were part of the Faculty of Law. The 'emancipation' from the Faculty of Law in the early 1990s facilitated the growth and expansion of Sociology and other departments. We need to thank Herman Brutsaert, Hilary Page and others, because they made this consolidation possible. We also need to thank John Vincke, who was head of the Department of Sociology and dean of the Faculty of Political and Social Sciences until 2009, when he died much too early. This room is

[1] H. Tyrell, *Soziale und gesellschaftliche Differenzierung: Aufsätze zur soziologischen Theorie*. Wiesbaden 2008.

dedicated to his memory. John Vincke saw the expansion of the Department as an opportunity to create a more balanced composition of its research and teaching staff. In recent years, the Department of Sociology has been able to establish a strong interest in social theory, in sociology of science, in the history of sociology. In the near future, we hope to be able to continue working in these domains. We also sincerely hope that we will remain able to rely on Hartmann Tyrell for critical advice.

History and Sociology – Some Encounters

Hartmann Tyrell

Please allow me to start with some words about myself and the places in which I grew up. I was born in Dresden, in Saxonia; I am however a *Westphalian*. That can already be seen when you look at the three cities, with which my life was most strongly connected. Dortmund, where I grew up, and whose football team still counts me among its fans (at present particularly!), Muenster, the city, in which I studied sociology, history and history of art, and Bielefeld, where I worked in the Faculty of Sociology for far more than thirty years. In the nineties this University also attracted and pulled Raf Vanderstraeten, now my Ghentian collegue. I must add, what concerns Bielefeld: The Bielefeld University is a comparatively young one, it was founded in the 1960s. The first professor, who was appointed at this university, was a sociologist, namely *Niklas Luhmann*. I can say: already in Muenster I attended his lectures and was his student. In Bielefeld, too, I remained in good contact with him. We were always part of the same research group. But I was never a pure system theoretician, although I owe the major topic of my research work – *social differentiation* – to Niklas Luhmann. That your University awarded him a Honorary Doctoral Degree in the year 1984 was proudly registered in Bielefeld.

Westphalia certainly is not one of the most important German regions. But it was at the center of European politics in 1648, when the Westphalian Peace was negotiated. Meanwhile, we live as is well-known in a 'post-westphalian era'. Compared to the Low Countries, particularly their southern part, Westphalia has always been situated in the periphery – especially in cultural regard. My brother and I visited this culturally central

region (north of the Alps) in 1964. We undertook from Dortmund a cycling tour to and through Belgium. For us Belgium was at that time the cycling nation par excellence, not only because of the Dortmund six days. We passionately collected autographs! It was still the time before Eddy Merckx, it was the time of Rik van Steenberghen!

As students of history, we both wanted to visit *Flanders*, particularly the Flemish cities which were metropolitan cities in the period of "the waning of the Middle Ages" and in the early-modern era: Ghent and Bruges (as well as Ypres, which was terribly destroyed in the First World War) already in medieval times, later Antwerp. The important historians of the late Middle Ages we were familiar with, were *Johan Huizinga* and *Henri Pirenne* – the latter particularly as the analyst of class conflicts and social revolts in the proto-industrial context of these cities.[1] I personally was also very fascinated by "the early netherlandish painting " – in the works of Jan van Eyck, of Rogier who called himself "de la Pasture" in Tournai and "van der Weyden" in Brussels, for Hugo van der Goes and the others. In Germany, Hans Belting speaks today in view of this generation of artists of "the invention of painting in the Netherlands".[2] It remains astonishing how this beautiful art could be produced in an era which was so full of conflict and upheaval.

I will not dwell on such forms of astonishment. But let me briefly report about three *surprises*, which happened to me in the course of the last year. I thus take an egocentrical point of departure. Instead of surprise one can also speak of ‚transformation', of the wondrous transformation from preceding ignorance to knowledge. Surprises are only possible when one does not know or expect something. But one only learns about this ignorance – in the case of the true surprise – *in retrospect*. In terms of Immanuel Kant, this might be a self-indebted ignorance, but it might also be a legitimate unawareness, which cannot be blamed on the person who is unaware of, or ignorant about, something.

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1 See H. Pirenne, *Sozial- und Wirtschaftsgeschichte Europas im Mittelalter*. Tübingen / Basel 1946.

2 H. Belting, *Spiegel der Welt. Die Erfindung des Gemäldes in den Niederlanden*. München 2010.

Now to my three ignorances. The *first* in my view is a legitimate one. How could I expect that someone in the beautiful center of Ghent would come upon the idea to confer to me, who comes from the westphalian periphery, the honor which is conferred upon me today? Of course, I knew Raf Vanderstraeten, the very estimated colleague and friend from Bielefeld and other times, who was appointed here not so long ago. Somehow he could have something to do with what happens today. You see once more: the human *alter ego*, even if you feel yourself familiar and befriend with him, he remains always *strange*, somewhat transcendental, to whose original intentions and thoughts you never have immediate access. Social Phenomenology speaks here of the problem of inter-subjectivity; Georg Simmel speaks of *secrecy*, because the *alter ego* can remain a silent one. And you can always be surprised by him. If this is already the case in the relation with the *familiar* Other, how much more reasons to be surprised in relation to the many unknown members of this Faculty. So I think I am completely innocent regarding today's surprise. The more so, why I can discover only little on my side, what lets me be worthy of the honor you bestow on me. All the more cause to thank most sincerely for this honor.

This brings me to my *second* surprise. It has to do with my ignorance, regarding the two historians already mentioned, namely Pirenne and Huizinga. With both, I stayed in touch during my training as sociologist: with *Pirenne* for example in the context of the sociology of organizations, especially in relation to his discussions of medieval corporations and early forms of modern organizations.[3] And with *Huizinga* in the context of the sociology of conflict; in regard to the nearly incomprehensible density of conflicts during the Middle Ages – not at least the "partijstrijd" – he explicitly asks for a sociological explanation.[4] Maybe he alluded to Georg Simmel's famous essay on "Der Streit" (engl. "the conflict"). And it was *Norbert Elias*, who in the 1930th – in the context of his civilization theory

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3 See R. Mayntz, *Soziologie der Organisation*. Reinbek bei Hamburg 1963, p. 10.

4 J. Huizinga, *Herbst des Mittelalters: Studien über Lebens- und Geistesformen des 14. und 15. Jahrhunderts in Frankreich und den Niederlanden.* Stuttgart 1961, p. 21. In Huizinga's Dutch text, one reads: "Niemand zal de aanwezigheid van economische oorzaken voor die partijgroeperingen willen loochenen, doch onbevredigd door het succes, waarmee zij tot nu toe zijn aangewezen, is men geneigd te vragen, of ter verklaring van de laat-middeleeuwse partijstrijd een sociologisch gezichtspunt voorlopig niet meer profijt oplevert dan een politisch-economisch". J. Huizinga, *Herfsttij der middeleeuwen*. Amsterdam 1997, p. 26.

– took up Huizinga's challenge and provided a sociological answer to Huizinga's question.[5]

And now the surprise begins: Pirenne and Huizinga have themselves become a case for historians. In recent years, there has appeared a lot of historical work on both authors, and on the relationship between both authors.[6] I will now – in adoration for the *genius loci* – only briefly speak about *Henri Pirenne*, who wrote the history of his own nation. He saw its cultural identity above all "in that Mixture of Romanism and Germanism" and in the mutual relations between both.

Thus to my ignorance: the tragic of Pirennes relationship to Germany remained unknown for me until last year, and I owe it to you that it is now remedied. Nothing did I know about the early, close and friendly connection to *Karl Lamprecht* and Leipzig[7], nothing about the fact that the first volume of his History of Belgium appeared in 1899 first of all in German[8], nothing about the 'diplomatic' role of Pirenne in-between German and French historians before 1914, nothing about his resistance against the German occupation of Belgium from 1914 onwards, nothing about his open resistance against the German language politics (*Flamenpolitik*), especially about your university, nothing about Pirennes deportation and his internment in Germany until the end of the First World War, nothing finally about his complete break with Germany in 1918. Pirennes break with Germany was reflected, as is often said, in his view upon Europe's history, especially concerning Belgium; as Tollebeeks says: "Belgium no longer lay between Romanic France and Germanic Germany, but belonged

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5 N. Elias, *Über den Prozeß der Zivilisation: Soziogenetische und psychogenetische Untersuchungen*. Band I: *Wandlungen des Verhaltens in den Oberschichten des Abendlandes*. 2. ed., Bern / München 1969, pp. 270; or see the Dutch translation: N. Elias, Het civilisatieproces: Sociogenetische en psychogenetische onderzoekingen. Utrecht 1990, p. 268. See also J. Goudsblom, *Zum Hintergrund der Zivilisationstheorie von Norbert Elias: Das Verhältnis zu Huizinga, Weber und Freud*, in: P. Gleichmann / J. Goudsblom / H. Korte (ed.), Macht und Zivilisation. Materialien zu Norbert Elias' Zivilisationstheorie. Frankfurt/M. 1984, 129-147, p. 130; G. Schwerhoff, *Zivilisationsprozeß und Geschichtswissenschaft: Norbert Elias' Forschungsparadigma in historischer Sicht*, in: Historische Zeitschrift 226, 1998, 561-605, p. 576.

6 See M. Boone, *L'Automne du Moyen Âge. Johan Huizinga et Henri Pirenne ou "plusieurs vérités pour la même chose"*, in: P. Moreno / G. Palumbo (ed.), Autour du XV^e^ siècle: Journées d'étude en l'honneur d'Alberto Vàrvaro. Genève 2008, 27-51; J. Tollebeek, *At the Crossroads of Nationalism: Huizinga, Pirenne and the Low Countries in Europe*, in: European Review of History 17, 2010, 187-215.

7 See for the ending of that friendship in 1915 G. Roth, *Politische Herrschaft und persönliche Freiheit. Heidelberger Max Weber-Vorlesungen 1983*.Frankfurt/M. 1987, p. 192.

8 H. Pirenne, *Geschichte Belgiens.* Band I: *Bis zum Anfang des 14. Jahrhunderts* (Deutsche Übersetzung von F. Arnheim). Gotha 1899.

completely to the Western Europe of France and Great Britain".[9] The intellectual costs of this interruption of contact with Germany are both high and unfortunate. They concern the relationship of history and sociology, too. What hurts me as a Weberian, is that the Pirenne of the after war period, as far as I can see, couldn't make himself familiar with the work of Max Weber. Weber's work became available in the form of books only in the 1920s. How much, however, we would not have liked to read a judgment of Pirenne, who was the author of *Medieval Cities* (1925), about Max Weber's reflections on "the sociology of the city"! In Weber's comparative and historical sociology, as you may know, the type ‚of the medieval city', north and south of the alps, plays a crucial role.[10]

Due to the German invasion and occupation of Belgium, the biography of Pirenne drifted away from the intra-European equilibrium, by which it was so strongly determined until 1914. The German mistreatment of his person and of his country turned the 'natural' mediator and European scholar of Belgian nationality into a West-European citizen, who turned his back towards Germany and its once so admired universities. My unawareness of all this was self-indebted, if I may say so. For the historians in Germany, Pirenne is still read today. His *Mahomet et Charlemagne* (1936) – a book, which re-raised the question of the periodization between Antiquity and the Middle Ages – was reprinted in Germany in 1985. This reprint contains a long and instructive epilog by Dan Diner, and therein Pirenne's German misfortune is explicitly dealt with.[11] Well-known in Germany, too, is Pirenne's important role in the 1920s in the *Annales*-project, as the interlocutor of *Lucien Febvre* and *Marc Bloch*, who, for their part, were connected with the sociology of the Durkheim school.[12] Above all, the book of *Cinzio Violante*, which discusses "the break in the academic world of Europe" during the First World War so impressively, and which uses the

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9 See Tollebeek, *op. cit.*, p. 195.

10 Compare H. Pirenne, *Medieval Cities: Their Origins and the Revival of Trade.* Princeton 1925; M. Weber, *Wirtschaft und Gesellschaft. Die Wirtschaft und die gesellschaftlichen Ordnungen und Mächte*. Nachlaß. Teilband 5: *Die Stadt*, ed. W. Nippel. Max Weber Gesamtausgabe I/22-5. Tübingen 1999.

11 D. Diner, *Ideologie, Historiographie und Gesellschaft: Zur Diskussion der Pirenne-Thesen in der Geschichtswissenschaft. Ein Nachtrag*, in: H. Pirenne, Mohammed und Karl der Große. Frankfurt/M. 1985, 207-282, pp. 214. After the war Pirenne's book was available in the *Fischer Bücherei* since 1963.

12 See P. Schöttler, *Henri Pirennes Kritik an der deutschen Geschichtswissenschaft und seine Neubegründung des Komparatismus im Ersten Weltkrieg*, in: Sozial.Geschichte 19, 2004, 53-81, p. 55.

personal and intellectual fate of Henri Pirenne to illustrate this break, is since 2004 also available in a German translation – from Italian.[13] It affects me, as a historically-interested human being, deeply that the university of Pirenne honors someone like me, who comes from the other side of the Rhine, with the Sarton medal. If one thinks about what happened one hundred years ago, there is really good reason for being astonished. It is part of the ironies of history that today's academic ceremony takes place in a *united* Europe, but also in Belgium, in a country whosenational identity seems presently to be in dissolution. And further: that this happens at the University of Ghent, where one is entitled to speak and teach in Flemish, and where English is used during transnational ceremonies.

Let me now come to my *third* surprise, which came over me last year. I must add immediately: I actually do not speak about *one*, but about a whole series of surprises and novelties for me. *First* of all: I knew ISIS, the famous journal of history of science, but of *George Sarton*, its founder and editor for many years, I knew at best his name. What was completely unknown to me was that the journal was founded in 1913. The second volume was published in June (!) 1914, but it does not yet anticipate anything about the outbreak of the war, which was then so near. The first volume bears the sub-title: "Revue consacrée à l'histoire de la science, publiée par George Sarton". The place of publication is Wondelgem-lez-Gand. Unknown to me was also Sarton's strong (patriotic) attachment to *Adolphe Quetelet* (who held a doctorate from Ghent University) as "*the* founder of sociology". He questioned that Auguste Comte could claim that role. *Furthermore*: OSIRIS, too, the book series in history of science, was not unknown to me. And, of course, I knew of the Ph.D. thesis of *the* twentieth century sociologist *Robert King Merton*, entitled *Science, Technology and Society in Seventeenth Century England* that was published in 1938. But I did not know that he wrote it in Harvard under the supervision of Sarton, and that it was published in volume IV of OSIRIS. *Finally*: which sociologist does not know Merton's essay *The Matthew Effect in Science* from 1968? Also *The Matthew Effect in Science, II*, published in 1988, which contains important reflections on the problem of "intellectual property", was not unknown onto me. But I was unaware of the occasion in

13 C. Violante, *Das Ende der 'großen Illusion': Ein europäischer Historiker im Spannungsfeld von Krieg und Nachkriegszeit, Henri Pirenne (1914-1923) – Zu einer Neulesung der "Geschichte Europas"*. Berlin 2004.

November 1984, which inspired this text. This occasion was the celebration of the "birth of George Sarton a hundred years ago" here in Ghent. *The Matthew effect in science, II* was Merton's inaugural lecture, in November 1986, as the first George Sarton Chair of your University.[14]

Let me now finally say some words about the three data, which I mentioned: 1913/1914, 1938, 1984/86. These considerations again have in view the encounters of history and sociology.

To the first date: 1913/14. It is from the point of view of the history of science probably a singular case that the emergence of *one* discipline is, I exaggerate only a bit, the project and the work of *one* man and *his* journal. For science history, this may be claimed and one may add: ISIS was until the 1930s the private property of George Sarton, its founder and editor.[15] If one looks back on the year of its foundation – thus on the first two volumes of ISIS (1913, 1914) –, then two things need to be underlined. That is, on the one side, the explicit and Europe-wide claim to *internationality* of this journal. The styling of the journal's first issues is a French one and Sarton writes in French, of course, but ISIS invites authors from everywhere in Europe and offers them the possibility to write in their own language. Consequently one finds in the first two volumes contributions in French, English, Italian and German language (although nothing in Dutch, if I have not overseen something!). Here Belgium's mediating role within Europe's academic world – until 1914! – shows up again and impressively. What is remarkable, on the other side, is the broad disciplinary range addressed by ISIS: historians where of course included, but also sociologists. Among the members of the "Comité de Patronage" you find for example *Karl Lamprecht*, but also *Émile Durkheim*. And Sarton's introductory essay *Histoire de la science* not only invites contributions from historians and philosophers, but explicitly also from sociologists.

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[14] See R.K. Merton, *George Sarton: Episodic Recollections by an Unruly Apprentice*, in: ISIS 76, 1985, 470-486 ("originally presented to the Sarton Centennial meeting, 15 November 1984, University of Ghent"); R.K. Merton, *The Matthew Effect in Science. Cumulative Advantage and the Symbolism of Intellectual Property*, in: Sartoniana 1, 1988, 23-51; R.K. Merton, *The Matthew Effect in Science, II: Cumulative Advantage and the Symbolism of Intellectual Property*, in: ISIS 79, 1988, 606-623 (containing "the main part of the inaugural lecture of the George Sarton Leerstoel, 28. November 1986, University of Ghent"); see also ibid., p. 669.

[15] Compare I.B. Cohen, *The* ISIS *Crises and the Coming of Age of the History of Science Society. With Notes on the Early Days of the Harvard Program in History of Science,* in: ISIS 90, 1999, S28-S42.

Sarton also was a person, who could not live with the German occupation of Belgium during the First World War. He fled the country; his way led him – via Great Britain, where his wife came from – into the USA. But he did not give up ISIS; the journal started to appear again in 1920, and again it was produced on Belgian soil, in Brussels. Volume III is still largely a French one, but from volume IV onwards, ISIS is presented as "*International* Review devoted to the History of Science and *Civilization* ". And both elements are remarkable: the stress on internationality (after the world war and against it) as well as the addition of Civilization and thus the expansion of the journal in the direction of cultural history, giving more room to sociology. And Sarton becomes now, in his own journal, an English writing author.

To the second date: 1938. Merton has described in much detail how he, as a student of sociology, got into contact with George Sarton. He has described his own Harvard history so entertainingly – and he has told it here in Ghent – that any re-telling of his story is forbidden. Important here is only: the "young sociologist-in-the-making" – with his research plans in the history and sociology of science – felt encouraged and supported by Sarton. You know to what it lead to: regarding the genesis of modern Capitalism we have the *Weber thesis*, regarding the beginnings of modern Science we have the *Merton Thesis*. Both Weber and Merton direct their attention to the Puritans in England in the 17th century. And Merton therein felt encouraged by Max Weber; he built upon Weber's *Protestant Ethic* and quoted not from Parsons' English translation, but from the German edition of 1920.

At this place, I am able to offer you a small novelty – probably the only one, which this lecture has to communicate. [I leave aside the fact that Merton put much emphasis on the notion of 'secularization' (which is only hardly used in Weber's writings)]. On one place, Merton is disappointed in Max Weber, namely concerning the central relationship between Puritanism and "science and technology". He had expected more explicit support from Weber. Weber, in his view, merely spoke of the "possibility of such a connection". This disappoints Merton.[16] But Weber is far more on his side than he thinks. In a footnote, Merton quotes from one of Weber's many

[16] R.K. Merton, *Science, Technology and Society in Seventeenth Century England*. New York 1970, p. 59.

footnotes. The crucial Weber sentence, as Merton quotes it, goes like this: "Auf die Bedeutung [des Puritanismus] für die Entwicklung der Technik und der empirischen Wissenschaft kommen wir nicht zu sprechen."[17] Merton, however, overlooked one word. Weber wrote: "kommen wir *hier* nicht zu sprechen." (komen wij *hier* niet te spreken!)[18] From what follows in the footnote, it is evident that the connection was clear and unambiguous according to Weber. But more still! You must know: the *Protestant Ethic* is a fragment, it is an unfinished work. Originally much more was planned – also with regard to science and technology. This can be seen, when the *first* version of the *Protestant Ethic*, which dates from 1904/5, is taken into account. Here one reads at the relevant page: "Auf die Bedeutung für die Entwicklung der Technik und der empirischen Wissenschaften kommen wir *später* [later] zu sprechen."[19] Weber had thus in mind what Merton later accomplished – surely in a more elaborate way. The concluding remarks of the *Protestant Ethic* also indicate Weber's project.[20] But as so many of Weber's projects, this one too was not realized. In one word: Weber provides much more support for the Merton thesis, than Merton himself has noticed!

To the third date: 1984/86. Everything, what I want to say about this date, took place in Ghent. What Merton spoke about, can be found in ISIS (1985, 1988), but, as mentioned before, it also appeared in the first volume of the Sartoniana. The foreigner, who I am, cannot comment on that! One thing can be added, however: with Robert King Merton, you honoured at that time a Giant in the field of sociology – and not only in sociology. His relevance for the sociology of science, especially the historical sociology of science, is from my point of view as large as the relevance of Sarton for the history of science – and it is probably of more lasting relevance. We may add: the centennial of Merton's birthday was celebrated at several places last year. In Germany, the *Berliner Journal für Soziologie* used this anniversary to dedicate a special issue to Merton. This issue opens with the German translation of *The Matthew effect in science, II*, taken over from

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17 Merton, *op. cit.*, p. 59, n. 9.

18 Max Weber, *Gesammelte Aufsätze zur Religionssoziologie I*. Tübingen 1920, p. 188, n. 2.

19 Max Weber, *Die protestantische Ethik und der "Geist" des Kapitalismus. II. Die Berufsidee des asketischen Protestantismus*, in: Archiv für Sozialwissenschaft und Sozialpolitik 3, 1905, 1-110, p. 97, n. 59.

20 *ibid.*, 109; compare also F.H. Tenbruck, *Max Weber and the Sociology of Science: A Case Reopened*, in: Zeitschrift für Soziologie 3, 1974, 312-320.

ISIS 79, 1988. The award of the George Sarton Chair in Ghent is explicitly mentioned as inspiration for this text.[21]

What remains to be said? I think of two things! *First*, one other small surprise for me: Merton's most beautiful book – the fans call it OTSOG ("On the Shoulders of Giants") – was stimulated by Sarton. The first publication *Standing on the Shoulders of Giants* – and used by Merton – dates from 1935; its author is *George Sarton*, who starts *his* historical search, as Merton did later, from *Newton's* use of the aphorism. Sarton understood it as a metaphor for scientific progress; as all of you know: a dwarf on the giants shoulder may look farther than the giant.[22]

Secondly and finally: with Robert King Merton, you have honored one of the giants of science; with me, you honor one of its smaller dwarfs, who does not think that he looks very much farther than Merton did. But it fulfills me with extraordinary pride, even with euphoria, to be found worthy of succeeding him as the next sociologist to receive the Sarton-Medal.

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21 R.K. Merton, *Der Matthäus-Effekt in der Wissenschaft, II. Kumulativer Vorteil und der Symbolismus des intellektuellen Eigentums*, in: Berliner Journal für Soziologie 20, 2010, 285-308.

22 G. Sarton, *"Standing on the shoulders of giants"*, in ISIS 19, 1935, 107-109; R.K. Merton, *On the Shoulders of Giants: A Shandean Postscript*. New York 1965.

Laudatio John D. Anderson, Jr.

Jan Vierendeels

Dr. John Anderson, Jr. was born in Lancaster, Pennsylvania on October 1, 1937. He grew up in Gainesville, Florida, and attended the University of Florida, graduating in 1959 with High Honours and a Bachelor of Aeronautical Engineering Degree.

From 1959 to 1962, he was a lieutenant and task scientist at the Aerospace Research Laboratory at Wright-Patterson Air Force Base. The newly established laboratory was at that time the premier basic research laboratory for the U. S. Air Force. Here, he carried out basic research on hypersonic aerodynamics, a program driven by intense interest in hypersonic flight associated with the beginnings of the manned space flight program. For three years Dr. Anderson conducted experiments at Mach 11 in the Hypersonic Wind Tunnel of the Aerospace Research Laboratory, studying the viscous interaction effect between the growth of the thick hypersonic boundary layer on the surface of a sharp right-circular cone and the outer hypersonic inviscid flow at the edge of the boundary layer. He also carried out theoretical calculations of the hypersonic viscous interaction phenomena for comparisons with the experimental data.

From 1962 to 1966, he attended the Ohio State University under the National Science Foundation and NASA Fellowships, graduating with a Ph.D in Aeronautical and Astronautical Engineering. For his Ph.D. research, he studied the high temperature radiating air behind a strong reflected shock wave at the end-wall of a shock tube. He treated cases where the air temperature behind the reflected shock was as high as 11,000K, more than twice the surface temperature of the sun. This is the

temperature in the shock layer over the nose of the Apollo return vehicle entering the earth's atmosphere after a mission to the Moon. For this case, the combined convective and radiative heat transfer from the gas behind the reflected shock to the shock tube end-wall simulated the aerodynamic heating to the nose of the Apollo vehicle. More than 30 percent of the total aerodynamic heating to the end-wall was due to radiation from the high temperature air in the shock layer. Dr. Anderson developed an early computational fluid dynamic program to calculate this high temperature radiating flow taking into account the non-adiabatic flow in the shock layer due to radiative energy loss from the flow field. His work was published in the *Physics of Fluids*.

In 1966, he joined the U. S. Naval Ordnance Laboratory at White Oak, Maryland as Chief of the Hypersonic Group. Here, he developed a time-marching computational fluid dynamic analysis for the calculation of the flow field over a hypersonic blunt body, and expanded on his work dealing with high temperature radiating flows. Later, he became involved in a large program in the U.S. to develop high energy gasdynamic lasers. At this laboratory, he developed one of the first computer programs for calculating gasdynamic laser performance carrying out a time marching finite difference solution of the vibrational nonequilibrium high-temperature flow through the nozzle of a gasdynamic laser.

In 1973, he became Professor and Chairman of the Department of Aerospace Engineering at the University of Maryland, serving as Chairman until 1980. In 1982, he was designated a Distinguished Scholar/Teacher by the University. His research at the University included finite-difference calculations of the interaction of laser radiation impinging on an absorbing hypersonic boundary layer on an aerodynamic surface, effects of uncertainties in chemical reaction rates on nonequilibrium chemically reacting flow fields, finite difference solutions of the flowfields inside internal combustion reciprocating engines, Navier-Stokes solutions of the low Reynolds number flow over airfoils, and the low speed flow over wings of general aviation airplanes with leading edge wing extensions to prevent stall-spins.

Beginning in the early 1980's, he pioneered a hypersonic aerodynamics research and teaching program at the University of Maryland, involving over 40 graduate students over the years. With considerable funding from the Hypersonic Propulsion Branch at the NASA Langley Research Center,

he had a number of students working on Navier-Stokes solutions of the flow through the combustor region of supersonic combustion ramjet engines (ScramJets). He was blessed with a number of excellent graduate students. (His first Ph.D. graduate from the University of Maryland was Dr. Michael Griffin, who went on to become the director of the National Aeronautics and Space Administration (NASA) for four years, and this year will become the President of the American Institute of Aeronautics and Astronautics.)

During 1986-87, while on sabbatical from the university, Dr. Anderson occupied the Charles Lindbergh chair at the National Air and Space Museum of the Smithsonian Institution. This began his serious work on the history of technology with emphasis on the history of aeronautical engineering and aerodynamics. He continued with the Air and Space Museum one day each week as their Special Assistant for Aerodynamics, doing research and writing a book on the history of aerodynamics.

In addition to his position as professor of aerospace engineering, in 1993 he was made a full faculty member of the Committee for the History and Philosophy of Science and in 1996 an affiliate member of the History Department at the University of Maryland. In 1996 he became the Glenn L. Martin Distinguished Professor for Education in Aerospace Engineering. In 1999 he retired from the University of Maryland and was appointed Professor Emeritus. He is currently the Curator for Aerodynamics at the National Air and Space Museum, Smithsonian Institution. At the Smithsonian, he continues to conduct research on the history of aeronautical engineering. His current research is focused on the evolution of the intellectual methodology of conceptual airplane design in the twentieth century, working towards a book entitled *The Grand Designers* for Cambridge University Press.

Dr. Anderson has published ten books: *Gasdynamic Lasers: An Introduction*, Academic Press (1976); and under McGraw-Hill, *Introduction to Flight*, 1st Edition (1978), 2nd Edition, (1985), 3rd Edition (1989), 4th Edition (2000), 5th Edition (2005), 6th Edition (2008), *Modern Compressible Flow with Historical Perspective*, 1st Edition (1982), 2nd Edition (1990), 3rd Edition (2002), *Fundamentals of Aerodynamics*, 1st Edition (1984), 2nd Edition (1991), 3rd Edition (2001), 4th Edition (2007), 5th Edition (2011), *Hypersonic and High Temperature Gas Dynamics,* 1st

Edition (1989), 2nd Edition (2006, AIAA), *Computational Fluid Dynamics: The Basics with Applications* (1995), *A History of Aerodynamics and Its Impact on Flying Machines*, Cambridge University Press, (1997), *Aircraft Performance and Design*, McGraw-Hill, (1999), *The Airplane: A History of Its Technology*, American Institute of Aeronautics and Astronautics, (2002), and *Inventing Flight: The Wright Brothers and Their Predecessors*, Johns Hopkins University Press, (2004).

He is the author of over 120 papers in radiative gasdynamics, re-entry aerothermodynamics, gasdynamic and chemical lasers, computational fluid dynamics, applied aerodynamics, hypersonic flow, and the history of aeronautics.

Dr. Anderson is a member of the National Academy of Engineering, the American Society for Engineering Education, the History of Science Society, and the Society for the History of Technology.

He is a Fellow of the Royal Aeronautical Society, London. He is also a Fellow of the Washington Academy of Sciences

In 1988, he was elected as Vice President of the AIAA for Education. The AIAA is the American Institute of Aeronautics and Astronautics.

In 1989, he was awarded the John Leland Atwood Award jointly by the American Society for Engineering Education and the American Institute of Aeronautics and Astronautics "for the lasting influence of his recent contributions to aerospace engineering education."

In 1995, he was awarded the AIAA Pendray Aerospace Literature Award "for writing undergraduate and graduate textbooks in aerospace engineering which have received worldwide acclaim for their readability and clarity of presentation, including historical content."

In 1996, he was elected Vice President of the AIAA for Publications. He was honored by the AIAA with its 2000 von Karman Lectureship in Astronautics, and with its History Book Award for 2002 for *a History of Aerodynamics*.

In 2002, he was awarded the position of Honorary Fellow of the AIAA, the Institute's highest award.

Dr. Anderson is known for his professional and educational activities both nationally and internationally. He has given over 40 short courses to the

major aerospace companies, the Air Force Academy, the government, and in Europe at Rolls-Royce in England, and the von Karman Institute in Belgium. In terms of the publishing world, in 1987 McGraw-Hill chose Dr. Anderson to be the senior consulting editor on the McGraw-Hill Series in Aeronautical and Astronautical Engineering. His books published by McGraw-Hill have recently been honored with a special label and symbol called “The Anderson Series, celebrating the impact thiscollection has had on the discipline and on students past and present.”

Breaking the Sound Barrier: the Aerodynamic Breakthroughs that made it possible

John D. Anderson, Jr.

Curator for Aerodynamics, National Air and Space Museum, Smithsonian Institution, Washington, DC

Professor Emeritus, Aerospace Engineering, University of Maryland, College Park, Maryland

> " We call the speed range just below and just above the sonic speed – Mach number nearly equal to 1 – the transonic range. Dryden (Hugh Dryden, well-known fluid dynamicist and past administrator of the National Advisory Committee for Aeronautics) and I invented the word 'transonic'. We had found that a word was needed to denote the critical speed range of which we were talking. We could not agree whether it should be written with one s or two. Dryden was logical and wanted two s's. I thought it wasn't necessary always to be logical in aeronautics, so I wrote it with one s. I introduced the term in this form in a report to the Air Force. I am not sure whether the general who read it knew what it meant, but his answer contained the word, so it seemed to be officially accepted…. I will remember this period (about 1941) when designers were rather frantic because of the unexpected difficulties of transonic flight. They thought the troubles indicated a failure in aerodynamic theory."
> Therodore von Kármán, Aerodynamics, Cornell University Press, 1954, p.116.

The morning of Tuesday, October 14, 1947, dawned bright and beautiful over the Muroc Dry Lake, a large expanse of flat, hard lake bed in the Mojave Desert in California. Beginning at 9:00 a.m., teams of engineers and technicians at the Muroc Army Air Field readied a small rocket-powered airplane for flight. Painted orange, and resembling a 50-caliber machine gun bullet mated to a pair of straight, stubby wings, they carefully installed the Bell X-1 in the bomb bay of a four-engine B-29 bomber of

World War II vintage. At 10:00 a.m., the B-29 with its soon-to-be historic cargo took off and climbed to an altitude of 20,000 feet. As it passed through 5,000 feet, Captain Charles E. (Chuck) Yeager, a veteran P-51 pilot from the European theater during World War II, struggled into the cockpit of the X-1. This morning Yeager was in pain from two broken ribs incurred during a horseback riding accident the previous weekend. However, not wishing to disrupt the events of the day, Yeager informed no one at Muroc about his condition, except his close friend Captain Jack Ridley, who helped him to squeeze into the X-1 cockpit. At 10:26 a.m., at a speed of 250 miles per hour, the brightly painted X-1 dropped free from the bomb bay of the B-29. Yeager fired his Reaction Motors XLR-11 rocket engine and, powered by 6000 pounds of thrust, the sleek airplane accelerated and climbed rapidly. Trailing an exhaust jet of shock diamonds from the four rocket nozzles of the engine, the X-1 soon approached Mach 0.85, the speed beyond which there existed no reliable wind tunnel data on the problems of transonic flight in 1947. Entering this unknown regime, Yeager momentarily shut down two of the four rocket chambers, and carefully tested the controls of the X-1 as the Mach meter in the cockpit registered 0.95 and increasing. Small invisible shock waves danced back and forth over the top surface of the wings. At an altitude of 40,000 feet, the X-1 finally started to level off, and Yeager fired one of the two shutdown

rocket chambers. The Mach meter moved smoothly through 0.98, 0.99, and to 1.02. Here, the meter hesitated then jumped to 1.06. A stronger bow shock wave now formed in the air ahead of the needlelike nose of the X-1 as Yeager reached a velocity of 700 miles per hour, Mach 1.06, at 43,000 feet. The flight was smooth, there was no violent buffeting of the airplane and no loss of control as feared by some engineers. At this moment, Chuck Yeager became the first pilot to fly faster than the speed of sound, and the small, but beautiful Bell X-1 became the first successful supersonic airplane in the history of flight.[1]

As the sonic boom from the X-1 propagated across the California desert, this flight became the most significant milestone in aviation since the Wright brothers' epochal first flight at Kill Devil Hills forty years earlier. But in the history of human intellectual accomplishment, this flight was even more significant; it represented the culmination of 260 years of research into the mysteries of high-speed gas dynamics and aerodynamics. In particular, it represented the fruition of twenty-three years of insightful research in high speed aerodynamics carried out by the National Advisory Committee for Aerodynamics (NACA) – research that represents one of the most important stories in the history of aerodynamics and aeronautical engineering. The purpose of this paper is to tell this story.

Prehistory: Speed of Sound and Shock Waves

Most golfers know the following rule of thumb: When you see a flash of lightning in the distance, start counting at a normal rate – one, two, three, four, five…. For every count of five before you hear the thunder, the lightning bolt struck a mile away. Clearly, sound travels through air at a definite speed, much slower than the speed of light. The standard sea level speed of sound is 1,117 feet per second (340 m/sec) – in five seconds a sound wave will travel 5,585 feet, slightly more than a mile. This is the basis for the golfer's "count of five" rule of thumb.

The speed of sound is one of the most important quantities in aerodynamics; it is the dividing line between subsonic flight (speeds less than that

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[1] This description of the first supersonic flight is excerpted from John D. Anderson, Jr., *Modern Compressible Flow: With Historical Perspective*, 3rd edition, McGraw-Hill Book Co., 2003, p. 9-10.

of sound) and supersonic flight (speeds greater than that of sound). The Mach number is the ratio of the speed of a gas to the speed of sound in that gas. If the Mach number is 0.5, the gas flow velocity is one-half the speed of sound; a Mach number of 2.0 means that the flow velocity is twice that of sound. The physics of a subsonic flow is totally different from that of a supersonic flow – a contrast as striking as that between night and day. This is why the first supersonic flight of the X-1 was so dramatic, and why the precise value of the speed of sound is so important in aerodynamics.

Knowledge of the speed of sound is not a product of twentieth century science. Precisely 260 years before the first supersonic flight of the X-1, Isaac Newton published the first calculation of the speed of sound in air. At that time it was clearly appreciated that sound propagated through air at some finite velocity. Newton knew that artillery tests had already indicated that the speed of sound was approximately 1,140 feet per second. The seventeenth century artillery men were preceding the modern golfer's experience; the tests were performed by standing a known large distance away from a cannon, and noting the time delay between the light flash from the muzzle and the sound of the discharge. In proposition 50, Book II of his *Principia* (1687), Newton calculated a value of 979 feet per second for the speed of sound in air – fifteen percent lower than the existing artillery data. Undaunted, Newton followed a now familiar ploy of theoreticians; he proceeded to explain away the difference by the existence of solid dust particles and water vapor in the atmosphere. However, in reality Newton had made the incorrect assumption in his analysis that the air temperature inside a sound wave was constant (an isothermal process), which caused him to underpredict the speed of sound. This misconception was corrected more than a century later by the famous French mathematician, Pierre Simon Marquis de Laplace, who properly assumed that a sound wave is adiabatic (no heat loss), not isothermal.[2] Therefore, by the time of the demise of Napoleon, the process and equation for the speed of sound in a gas was fully understood.

On October 14, 1947, as the Bell X-1 nudged closer to Mach one, a region of the aerodynamic flow over the wing became locally supersonic. This is because the airflow increases its velocity while moving over the top of the

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[2] Pierre Simon Marquis de Laplace, "Sur la vitesse du son dans l'aire et dan l'eau," Annales de Chimie et de Physique, 1816.

wing, and hence there is always a region of the flow over the wing where the local velocity is larger than the velocity of the airplane itself. As the X-1 accelerated through Mach 0.87, a pocket of locally supersonic flow formed over the top of the wing. This supersonic pocket was terminated on the downstream end by a shock wave oriented almost perpendicular to the local flow direction. This shock was the culprit which made flight through Mach one such a harrowing concern at that time. Finally, when the X-1 accelerated through Mach one to supersonic speeds, another shock wave formed a short distance in front of the nose; this shock, called the bow shock, was curved and more oblique to the flow. Shock waves are extremely thin regions – much thinner than the thickness of this page – across which dramatic and almost discontinuous increases in pressure and temperature occur. Shock waves are a fact of life in the aerodynamic flow over transonic and supersonic airplanes.

Knowledge of shock waves is not unique to the twentieth century; their existence was recognized in the early nineteenth century. The German mathematician G. F. Bernhard Riemann first attempted to calculate shock properties in 1858, but he neglected an essential physical feature and hence obtained incorrect results.[3] Twelve years later, William John Rankine, a noted engineering professor at the University of Glasgow, correctly derived the proper equations for the flow across a normal shock wave. Not cognizant of Rankine's work, the French ballistician Pierre Hugoniot rediscovered the normal shock wave equations in 1887. To the present day, the governing equations for flow across a shock wave are called the *Rankine-Hugoniot equations*, in honor of these two men. This work was expanded to include oblique shock waves by the famous German aerodynamicist, Ludwig Prandtl and his student Theodor Meyer at Göttingen University in1908. Hence, only five years after the first flight by the Wright brothers, the necessary theory for the calculation of shock wave properties in a supersonic flow was in hand, albeit considered a purely academic subject at that time.

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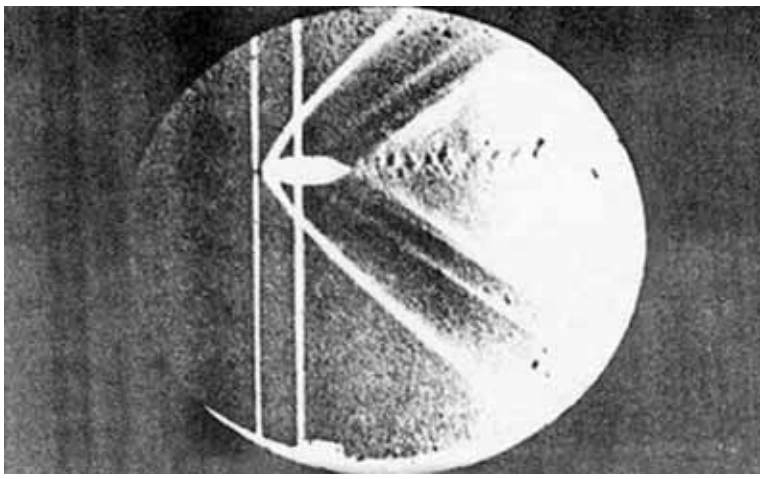
[3] A shock wave is, in thermodynamic language, an irreversible process, caused by viscosity and thermal conduction effects inside the shock wave. A measure of the amount of irreversibility is a thermodynamic variable called entropy, which from the Second Law of Thermodynamics always increases in any process involving such irreversibilities. The entropy of a gas always increases as it passes through a shock wave. Unfortunately, Riemann made the incorrect assumption that the entropy remained constant across a shock.

The nineteenth century was also a time of experimental work on supersonic flow. Perhaps the most important event was the proof that shock waves were not just a figment of the imagination – they really existed in nature. This proof was given by the physicist-physician-philosopher Ernst Mach in 1887. Mach, while a professor of physics at the University of Prague, took the first photographs of shock waves on a body moving at supersonic speeds. Shock waves are normally invisible to the naked eye. But Mach devised a special optical arrangement (called a shadowgraph) by which he could see and photograph shock waves. In 1887, he presented a paper to the Academy of Sciences in Vienna where he showed a photograph of a bullet moving at supersonic speed. Using his shadowgraph system, the bow shock and trailing edge shock were made visible. This historic photograph allowed scientists, for the first time in history, to actually see a shock wave. The experimental study of shock waves was off and running.

Photograph of a bullet in supersonic flight, published by Ernst Mach in 1887.



Comment: Science, Engineering, and Engineering Science

In his seminal book *What Engineers Know and How They Know It*,[4] Walter Vincenti clearly makes the following distinction between science and engineering: science is the quest for new knowledge for the sake of enhancing

[4] Johns Hopkins University Press, 1990, Baltimore, Maryland

understanding, and engineering is a self-standing body of knowledge (separate from science) for the sake of designing artifacts. Vincenti, a Professor Emeritus from Stanford University and a member of the National Academy of Engineering, is a distinguished engineer and researcher in high-speed aerodynamics who later became a full-time scholar in the history of technology. He holds the da Vinci Medal from the Society for the History of Technology (SHOT), so he speaks with authority from both disciplines. Vincenti emphasizes that engineering in *not* applied science, as has been the view held by many scholars. Rather, engineering stands alone with its own accumulated body of knowledge based on experience. For example, engineers will sometimes apply a theory that is known to violate physics in some way, but because it gives reasonable engineering results it is useful. For example, accurate pressure distributions around the nose of a hypersonic flight vehicle can be reasonably predicted by simple Newtonian theory (based in Newton's famous sine-squared law that dates back to the *Principia*), but which is known (even by Newton at the time) to be physically unrealistic in nature.

After World War II, the term *engineering science* surfaced, predominately in academic circles, to indicate some type of closer relationship between science and engineering. Its meaning, however, is amorphous and frequently depends on the mind of the beholder. For the purpose of the present paper, I suggest the following definition of engineering science: *Engineering science is the search for new scientific knowledge for the explicit purpose of (1) Providing a qualitative understanding which allows the more efficient design of an engineering artifact, and/or (2) Providing a quantitative (predictive) technique, based on science, for the more efficient design of an engineering artifact.*

The knowledge gained about the speed of sound and shock waves discussed in the previous section is clearly an example of *science*. The work was carried out mainly by researchers who were interested in the subject on an academic basis only. The researchers involved in this work were after scientific knowledge, and just that. There was no force behind these researchers driving them to design any related engineering artifacts at the time. The true practical value of this work did not come to fruition until the advent of supersonic flight in the 1940s. However, this is an example of the value of basic research on problems that appear only purely

academic at the time. In the 1940s, when basic supersonic flow theory and fundamental understanding of shock waves was suddenly needed due to the advent of high-speed airplanes and rockets, it was there – quietly residing and sleeping in a few dusty books and archive journal articles in the library.

The research described in the remainder of this paper is, however, an example of *engineering science*, research that was clearly motivated by the engineering need to design high-speed flight vehicles.

Compressibility Problems: The First Inklings (1918-1923)

Airplane aerodynamics, from the time of the Wright Flyer to the beginning of the World War II, assumed that changes in air density from one point to another were negligible as the air flowed over the airplane. This assumption, called *incompressible flow*, was reasonable for the 350 mile per hour or slower flight speeds of airplanes during that era. Theoretically, it was a tremendous advantage to assume constant density, and physically the low-speed aerodynamic flows usually exhibited smooth variation with no sudden changes or surprises. All this changed when flight speeds began to sneak up close to the speed of sound. Aerodynamic theory had to account for changes in the air density in the flow field around the airplane, and, physically, the flow field sometimes acted erratically, and frequently surprised and greatly challenged aerodynamicists. In the 1930s these phenomena were thrown into one pot and called, generically "compressibility problems."

Ironically, the first inklings of compressibility problems occurred during the age of the strut-and-wire biplanes, with flight velocities about as far away from the speed of sound as you can get. It had to do with an airplane part, namely the propeller. Although typical flight speeds of World War I airplanes were less than 125 miles per hour, the tip speeds of propellers, because of their combined rotational and translational motion through the air, were quite large, sometimes exceeding the speed of sound. This fact was appreciated by aeronautical engineers at the time. This drove the British Advisory Committee for Aeronautics to show some interest in compressible flow theory. In 1918 and 1919, G.H. Bryan, working for the Committee at the Royal Aeronautical Establishment, carried out a theoretical analysis of subsonic and supersonic flows over a circular cylinder (a

simple geometric shape chosen for convenience). He was able to show that in a subsonic flow the effect of compressibility was to displace adjacent streamlines farther apart. His analysis was cumbersome and complex – a harbinger of things to come – and provided little data of value. But it was evidence of the concern felt by the British over the effects of compressibility on propeller performance.[5]

At the same time, Frank Caldwell and Elisha Fales of the propeller branch of the Army Air Service Engineering Division at McCook Field in Dayton, Ohio, took a purely experimental approach to the problem. (This was the beginning of a blurred dichotomy between British and American research on compressibility effects. Over the next two decades, the major experimental contributions to understanding compressibility effects were to be made in the United States, principally by the National Advisory Committee for Aeronautics (NACA), and the major theoretical contributions were to be made in England.) In 1918, Caldwell and Fales designed and built the first high-speed wind tunnel in the United States – purely to investigate the problems associated with propellers. The tunnel range was from 25 to a stunning 465 miles per hour. It had a length of almost nineteen feet, and the test section was fourteen inches in diameter. This was a big and powerful machine for its day. Six different airfoils, with thickness ratios (ratio of maximum thickness to the chord length) from 0.08 to 0.2 were tested. At the higher speeds, the results showed "a decreased lift coefficient and an increased drag coefficient, so that the lift-drag ratio is enormously decreased." Moreover, the airspeed at which these dramatic departures took place was noted as the "critical speed."[6] Although this research was performed at an Army Laboratory, Caldwell and Fales were under contract from the NACA, and their work was published in NACA TR 83.[7] This was

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5 G.H. Bryan, "The Effect of compressibility on streamline Motions," R & M no. 555, Technical Report of the Advisory committee for Aeronautics, vol. 1, Dec. 1918.

6 The Critical Mach number is precisely defined as that freestream Mach number at which sonic flow is first encountered on the surface of a body. The large drag rise due to compressibility effects normally occurs as a freestream Mach number slightly above the critical Mach number; this is called the drag-divergence Mach number. In reality, Caldwell and Fales had reached and exceeded the drag-divergence Mach number in their experiments. But their introduction of the word "critical" in conjunction with this speed was eventually the inspiration for its use in later coining the term "critical Mach number."

7 F.W. Caldwell and E. Fales, "Wind Tunnel Studies in Aerodynamic Phenomena at High Speed," NACA TR 83, 1920.

the first published data in the history of aerodynamics to show the adverse effects of compressibility on an airfoil shape.

The Compressibility Burble – NACA's Seminal Research, 1924-1929.

During the 1920s, the NACA continued their work on compressibility effects by sponsoring a series of fundamental experiments in high-speed aerodynamics at the Bureau of Standards with Lyman J. Briggs and Dr. Hugh L. Dryden. Hugh Dryden was a fresh, young Ph.D. graduate from Johns Hopkins University in physics; he had received his Ph. D in 1919 at the age of twenty, the youngest Ph.D. graduate in the history of the institution. (Dryden much later was to become Director of Research for the NACA from 1947 to 1958.) This work progressed in three stages. The first involved measurements carried out in a jury-rigged high speed tunnel at the Lynn works of the General Electric Company. A vertical standpipe thirteen inches in diameter and thirty feet long was connected to a large centrifugal compressor. At the other end of the pipe was a cylindrical orifice that served as a nozzle 12.24 inches in diameter. With this device "air speeds approaching the speed of sound were obtained."[8] Lift, drag, and center-of-pressure measurements were made on wing models of rectangular planform with a span of 17.2 inches and a chord of three inches. The results supported the earlier trends observed by Caldwell and Fales. In particular, Briggs et al found

1. Lift coefficient for a fixed angle of attack decreased very rapidly as the speed increases.
2. The drag coefficient increases rapidly.
3. The center-of-pressure moves back towards the trailing edge.
4. The "critical speed" at which these occur decreases as the angle of attack is increased and the airfoil thickness is increased.

In 1924, the culmination of this work, as well as that of Caldwell and Fales, was the waving of a red flag – compressibility effects were nasty, and they markedly degraded airfoil performance. But nobody had any fundamental

[8] L.J. Briggs, G.F. Hull, and Hugh L. Dryden, "Aerodynamic Characteristics of Airfoils at High Speeds," NACA TR 207, 1924.

understanding of the physical features of the flowfield which were causing these adverse effects. This was not to come for another decade.

Briggs and Dryden made an important step towards this fundamental understanding in the second stage of their work.[9] Because the Lynn Works compressor was no longer available to them, Briggs and Dryden moved their experimental activity to the Army's Edgewood Arsenal in Maryland, where they constructed another high-speed wind tunnel, this one much smaller, with an air stream only two inches in diameter. However, by careful design of the small airfoil models, two pressure taps could be placed in each model. Seven identical models were used, each one with different locations of the pressure taps. With this technique, they measured the pressure distributions over the airfoil at Mach numbers from 0.5 to 1.08. The results were dramatic! Beyond the "critical speed," the pressure distributions over the top of the airfoil exhibited a sudden pressure jump at about one-third to one-half the distance from the leading edge, followed by a rather long plateau towards the trailing edge. Such a pressure plateau was familiar – it was very similar to that which exists over the surface of an airfoil in low-speed flow when the airfoil stalls at high angle of attack. And it was well known that airfoil stall was caused by flow separation over the top surface of the airfoil. Briggs and Dryden put two-and-two together, and concluded that the adverse effects of compressibility were caused by flow separation over the top surface, even though the airfoil was at low (even zero) angle of attack. To substantiate this, they constructed oil flow tests, wherein a visible, pigmented oil was painted on the model surface, and the model was placed in the high-speed air stream. During the tests, the tell-tale flow separation line formed on the oil pattern. Clearly, beyond the "critical speed," flow separation was occurring on the top surface of the airfoil. The next question was: Why? What was causing the flow to separate? The answer to this question was still eight years in the future.

In the aeronautical literature, the separated flow region over the airfoil that causes airfoil stall had been labeled the "stall burble." By analogy, the separated flow encountered beyond the critical speed became known as the "compressibility burble."

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[9] L.J. Briggs and Hugh L. Dryden, "Pressure Distribution Over Airfoils at High Speeds," NACA TR 255, 1926.

Was this work of Briggs and Dryden engineering science? Emphatically yes! Their experiments were designed to obtain basic scientific information about the physics of the high-speed flow over an airfoil, but always for the purpose of learning how to design better airfoil shapes for high-speed flight.

The third stage of the work by Briggs and Dryden was utilitarian, and was in keeping with the stated duty of the NACA to work on the problems of flight "with a views to their practical solution." Towards the end of the 1920s, they carried out a large number of detailed measurements of the aerodynamic properties for 24 different airfoils at Mach numbers from 0.5 to 1.08. The airfoils chosen were types conventionally used by the Army and the Navy for propellers, consisting of the standard family of British-designed RAF airfoils, and the American-designed Clark Y family. These data provided the first measurements on *standard* series of airfoils showing compressibility effects.[10] However, near Mach one the data suffered from aerodynamic interference problems inherent in all transonic wind tunnel tests until the breakthrough development of the slotted-throat transonic tunnel in the late 1940s.

It should be noted that theoretical solutions of high-speed compressibility effects in a subsonic flow were virtually non-existent during the 1920s. The only major contribution was that by the famous British aerodynamicist Herman Glauert, who rigorously derived a correction to be applied to the low-speed, incompressible lift coefficient in order to correct it for compressibility effects.[11] This was the first of a series of theoretical rules labeled "compressibility corrections." Because it was known that Ludwig Prandtl in Germany had also derived the same rule a few years earlier, but had not published it, Glauert's result has come down through the decades as the *Prandtl-Glauert Rule.* However, such compressibility corrections are applicable to the variation of lift coefficient at speeds below the "critical speed," and hence have no way of predicting the lift coefficient in the "compressibility burble." Moreover, no theoretical result, correction or otherwise, was available for the drag coefficient in this speed range.

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[10] L.J. Briggs and Hugh L. Dryden, "Aerodynamic Characteristics of Twenty-Four Airfoils at High Speeds, NACA TR 319, 1929.

[11] H. Glauert, "The Effect of Compressibility on the Lift of an Airfoil*," Journal of the Royal Society* 118 (19270;113

Throughout this, the primary motivation for all the above work on compressibility effects was for application to airplane propellers. But the focus was about to change, and change dramatically.

John Stack and the NACA Compressible Flow Research – A Breakthrough

In July 1928, a young New Englander, born and raised in Lowell, Massachusetts, began his career with the NACA Langley Memorial Laboratory in Hampton, Virginia. Having just graduated from the Massachusetts Institute of Technology with a B.S. degree in aeronautical engineering, John Stack was assigned to the Variable Density Tunnel, the premier wind tunnel in the world at that time. Stack was absolutely dedicated to aeronautical engineering. While in high school, he earned money so that he could take a few hours of flight instruction in a Canuck biplane. He helped out with the maintenance of a Boeing biplane owned by one of his part-time employers. Before he went to college he had made up his mind to be an aeronautical engineer. However, his father, a carpenter who was also very successful in real estate, wanted his son to study architecture at MIT. Instead, when Stack entered MIT, he enrolled in aeronautical engineering, keeping it a secret from his father for the first year, but with the understanding approval of his mother. Much later, Stack commented: "Then when Dad heard about it, it was too late to protest."

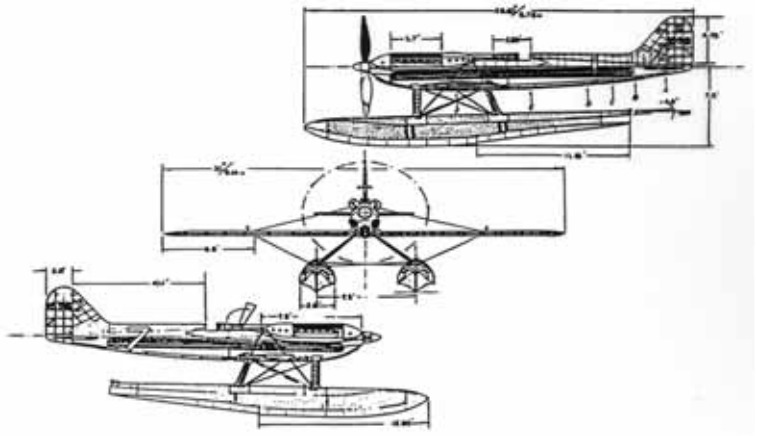
When Stack first walked into the Langley Laboratory that July of 1928, a year's worth of design work had already been done on Langley's first high-speed tunnel, and the facility was already operational with an open throat test section. Success had been achieved by the work of Briggs and Dryden, and the growing importance of high-speed research was perceived by some visionaries. Because of this perception, Joseph S. Ames, President of Johns Hopkins University and the new Chairman of the NACA, in 1927 gave priority to high-speed wind tunnels and research. Eastman Jacobs, who had joined the NACA in 1925 after receiving his B.S. degree in mechanical engineering from the University of California, Berkeley, was the chief designer of the open-throat eleven-inch High Speed Tunnel. (Jacobs would later earn an international reputation for his work on the famous NACA airfoil sections in the 1930s, and for his conception of, and pioneering

research on, the NACA laminar flow airfoils just before the beginning of World War II.) An innovative aspect of the eleven-inch High Speed Tunnel was that it was driven from the twenty atmosphere pressure tank of the Langley Variable Density Tunnel. When a change of models was made in the Variable Density Tunnel, the twenty atmosphere tank which encased the entire tunnel had to be blown down to one atmosphere; this represented a wasted energy source which the Langley engineers ingeniously realized could be tapped for the eleven-inch High-Speed Tunnel. The 5,200 cubic foot capacity of the high pressure tank allowed about one minute of operation for the tunnel. John Stack was given the responsibility for improving the High-Speed Tunnel by designing a closed throat. This improved facility was operational by 1932. It was his participation in the design and development of the eleven-inch High-Speed Tunnel that launched John Stack on his life-long career in high-speed aerodynamics.

While Stack was working on the High-Speed Tunnel, an event occurred in England which made a great impression on him, and which would rapidly refocus the NACA high-speed research program. On Sunday, September 13, 1931, a beautiful, highly streamlined Supermarine S.6B flashed through the clear early afternoon sky at Calshot, near Portsmouth along the southern English coast. Flown by Flt. Lt. John N. Boothman, this exquisite racing airplane averaged a speed of 340.1 miles per hour around a long, seven-lap course, winning the coveted Schneider Trophy permanently for Britain. Later that month, on September 29, Flt.Lt. Geroge Stainforth set the world's speed record of 401.5 miles per hour in the same S.6B. Looking at this airplane, even the non-aerodynamic observers appreciated that the concept of streamlining to reduce drag had taken root. The Supermarine S.6B simply looked like it could fly at 400 miles per hour – at Mach 0.53, over half the speed of sound. Suddenly, the aeronautical engineer's concern over compressibility effects on propeller tips, an important but tolerable situation, became an absolutely major concern over compressibility effects on the airplane itself, a problem of showstopper proportions.

Such concern was beginning to dawn on the aircraft industry itself. In 1936, Lockheed Aircraft began early design studies for the P-38, which was the first airplane to encounter major, and sometimes fatal, compressibility effects. By the mid-1930s, the aircraft industry was wading into uncharted

The Supermarine S.6-B



water, and the NACA's high-speed research program became absolutely vital to the future progress of high-speed airplane design.

Stack became acutely aware of this new compressibility challenge. In 1933, he published in NACA TR 463 the first data to come from the newly modified, closed-throat High-Speed Tunnel. Although the airfoils were propeller sections, Stack wrote in the introduction, obviously referring to the Schneider Trophy racer:

> *A knowledge of the compressibility phenomenon is essential, however, because the tip speeds of propellers now in use are commonly in the neighborhood of the velocity of sound. Further, the speeds that have been attained by racing airplanes are as high as half the velocity of sound. Even at ordinary airplane speeds the effects of compressibility should not be disregarded if accurate measurements are desired.*[12]

For the most part, Stack's data in 1933 served to confirm the trends observed earlier. The large drag rise at high speeds was clearly evident. He also confirmed that the onset of the adverse compressibility effects occurs at lower Mach numbers as either or both the airfoil thickness and angle of attack increase. One of his conclusions reflected on the theoretical Prandtl-Glauert compressibility correction mentioned earlier. From his measure-

[12] John Stack, "The N.A.C.A. High-Speed Wind Tunnel and Tests of Six Propeller Sections," NACA TR 463, 1933.

ments, Stack concluded: "these results indicated that the limited theory available may be applied with sufficient accuracy for most practical purposes only for speeds below the compressibility burble." This conclusion presaged almost forty years of a theoretical void. The aerodynamic equations applicable to the transonic flight regime, Mach numbers between about 0.8 and 1.2, are non-linear partial differential equations that defied solution until the 1970s. And even then the solution was by brute force – numerical solutions using the power of the newly-developed discipline of computational fluid dynamics carried out on high-speed digital supercomputers.

Driven by the conviction and foresight of John Stack, the NACA now waved the red flag of compressibility problems to the whole world of aeronautical engineering. In January 1934, the first significant professional aeronautical society in the United States, The Institute of Aeronautical Sciences, published the first issue of its archive journal, the *Journal of the Aeronautical Sciences*. It contained an article by Stack entitled "Effects of Compressibility on High Speed Flight." In the first paragraph, Stack makes clear the theme that would be played out by the NACA for the next several decades:

> *The effects of compressibility have commonly been neglected because until the relatively recent development of the last Schneider trophy aircraft the speeds have been low as compared with the velocity of sound, and the consequent local pressures over the surface of high-speed airplanes have differed but slightly from atmospheric pressure. At the present time, however, the speeds associated with the fastest airplanes approach 60 percent of the velocity of sound, and the induced velocities over their exposed surfaces lead to local pressures that differ appreciably from the pressure of the atmosphere. When this condition exists, air can no longer be regarded as an incompressible medium. The effects of compressibility on the aerodynamic characteristics of airfoils have been under investigation by the N.A.C.A. in the high-speed wind tunnel, and it is the purpose of this paper to examine the possibility of further increases in speeds in the light of this relatively recent research.*[13]

[13] John Stack, "Effects of Compressibility on High Speed Flight," Journal of the Aeronautical Sciences 1 (January 1934):40-43.

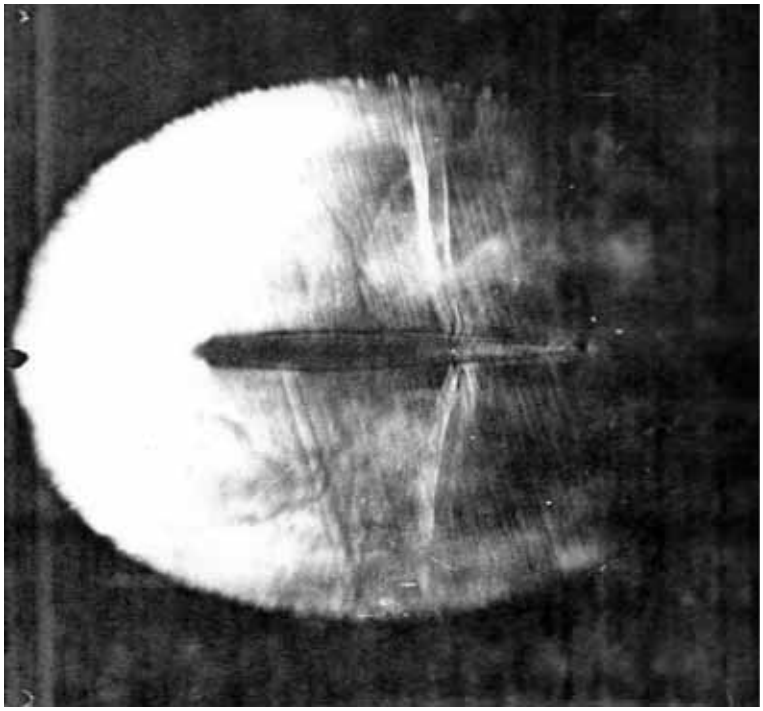
By this time, it was clear that the NACA was the leading research institution in the world in the area of compressibility effects. Through its influence and sponsorship of the fledgling experiments in the 1920s by Caldwell and Fales at McCook field, and by Briggs and Dryden at the Bureau of Standards, and now by its own carefully conducted experiments at Langley, the NACA had been able to identify the first two aspects of the basic nature of compressibility effects, namely that (1) above a certain "critical speed," the lift decreased dramatically and the drag skyrocketed almost beyond comprehension, and (2) this behavior was caused by sudden and precipitous flow separation over the top surface of the wing or airfoil. There remained one question, the most important of all – Why?

John Stack and the NACA were responsible for the answer to this question – a breakthrough that occurred in 1934. By this time, Stack had a new instrument with which to work – a schlieren photographic system, an optical arrangement that made density gradients in the flow visible. One of Nature's mechanisms for producing very strong density gradients in a gas is a shock wave; hence a shock wave ought to be visible in a schlieren photograph. Stack's boss, Eastman Jacobs, was familiar with such optical systems through his hobby of astronomy; it was in keeping with Jacobs' innovative mind to suggest to Stack that the use of schlieren system might make visible some of the unknown features of the compressible flow field over an airfoil, and might shed some light on the nature of the compressibility burble. It did just that, and more!

With the 11-inch tunnel running above the "critical speed" for an NACA 0012 symmetric airfoil mounted in the test section, and with the aid of the schlieren system, Stack and Jacobs observed for the first time in the history of aerodynamics a shock wave in the flow over the surface of the airfoil. This shock wave interacts with the thin, friction-dominated boundary layer adjacent to the surface of the airfoil, causing the boundary layer to separate from the surface. A massive region of separated flow trails downstream, greatly increasing the drag and decreasing the lift. *This was a breakthrough of enormous intellectual and practical importance.* It led to the final understanding of the physical nature of the compressibility burble, and it was totally due to the work of two innovative and highly intelligent aerodynamicists at the NACA Langley Laboratory, John Stack and Eastman Jacobs.

Was this work engineering science? Absolutely yes! It provided the fundamental physical understanding of the root source of compressibility problems. This understanding was mainly qualitative at the time, but it allowed designers of high-speed airfoils to make more intelligent decision about proper airfoil shapes – it helped to make the uncharted waters more navigable.

An early schlieren photograph of the shock pattern on an NACA 0012 airfoil in a freestream above the "critical speed."Flow is from left to right. From the first group of schlieren photographs of the compressibility burble taken by John Stack, 1934. In this photograph the nature of the flow pattern causing the compressibility burble was seen for the first time. From the John Stack papers in the NASA Langley Archives. Courtesy of Richard Layman, Archivist.



As with many new discoveries in science and technology, there are always those skeptical at first. One of those was Theodore Theodorsen, the best theoretical aerodynamicist in the NACA at the time, with a worldwide reputation for his pioneering papers on airfoil theory. John Becker, who

joined the NACA in 1936 and who went on to become one of the most respected high-speed aerodynamicists at Langley, tells the following anecdote about Theodorsen's reaction to the schlieren photographs taken by Stack and Jacobs. It is repeated here because it reflects just how much a radical departure from the expected norm the results were:

> *The first tests were made on a circular cylinder about ½ inch in diameter, and the results were spectacular in spite of the poor quality of the optics. Shockwaves and attendant flow separations were seen for the first time starting at subsonic stream speeds of about 0.6 times the speed of sound. Visitors from all over the Laboratory, from Engineer-in-Charge H.J.E. Reid on down, came to view the phenomena. Langley's ranking theorist, Theodore Theodorsen, viewed the results skeptically, proclaiming that since the stream flow was subsonic, what appeared to be shockwaves was an 'optical illusion,' and error in judgment which he was never allowed to forget.*[14]

An interesting confluence of events occurred in 1935 that allowed the NACA in a timely fashion to inform the international research community of this intellectual breakthrough in understanding compressibility effects and the compressibility burble. One was the existence of the data itself – fresh, exciting, and revolutionary. The other was the scheduling of the fifth Volta conference in Italy. This conference was organized by General Arturo Crocco, an aeronautical engineer who had become interested in ramjet engines in 1931, and therefore was well aware of the potential impact of compressible flow theory and experiments on future aviation. The topic of the conference was "High Velocities in Aviation." Participation was by invitation only, and the select list included all the leading aerodynamicists at that time. Because of his reputation in the design and testing of the famous NACA four-digit airfoil series, and the fact that he was the Section Head of the NACA Variable Density Tunnel which had put the NACA on the international aerodynamic map in the 1920s, Eastman Jacobs received an invitation. He took the opportunity to present a paper on the new NACA compressibility research. Hence, during the period between September 30 and October 6, 1935, the major figures in the development

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[14] John V. Becker, *The High-Speed Frontier: Case Histories of Four NACA Programs, 1920-1950* (Washington, DC: NASA SP-445, 1980),p. 16.

of high-speed aerodynamics of the 1930s (with the exception of John Stack) gathered inside an impressive Renaissance building in Rome that served as the city hall during the Holy Roman Empire, and discussed flight at high subsonic, supersonic, and even hypersonic speeds. The fifth Volta Conference was to become the springboard for new thought on the development of high-speed flight. (It was at this conference that Adolf Busemann introduced for the first time the concept of the swept wing for high-speed flight.)

In the midst of all this discussion was Eastman Jacobs representing the NACA. Jacob's paper, entitled "Methods Employed in America for the Experimental Investigation of Aerodynamic Phenomena at High Speeds," was both tutorial and informative. He described the NACA High-Speed tunnel, the schlieren system, and the airfoil experiments carried out in the tunnel. Then came the blockbuster. He showed, for the first time in a technical meeting, some of the schlieren pictures taken at Langley. Conscious of the NACA's penchant for perfection, especially in its publications, Jacobs apologized for the poor quality of the photographs, a very modest gesture considering their technical and historical importance. "Unfortunately the photographs were injured by the presence of bent celluloid windows forming the tunnel walls through which the light passed. The pictures nevertheless give fundamental information in regard to the nature of the flow associated with the compressibility burble."[15] With this, the NACA high-speed research program was not only on the map, it was leading the pack.

Jacobs' paper at the fifth Volta conference represented in some sense a celebration of the second phase of the NACA research on high-speed flight. The first phase was the embryonic wind tunnel compressibility work of the 1920s, clearly oriented towards applications to propellers. The second phase was the refocusing of this high-speed wind tunnel research on the airplane itself, complemented by a new initiative – the design and development of an actual research airplane.

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[15] Eastman Jacobs. "Methods Employed in America for the Experimental Investigation of Aerodynamic Phenomena at High Speeds," NACA Misc. Paper 42, March 1936, a copy of the Volta paper.

The "Sound Barrier"

At this time there was a common public belief in a "sound barrier." This myth originated in 1935 when the British aerodynamicist W.F. Hilton described to a journalist some of the high-speed experiments he was conducting at the National Physical Laboratory. Pointing to a plot of airfoil drag, Hilton described "how the resistance of a wing shoots up like a barrier against higher speed as we approach the speed of sound."[16] The next morning, the leading British newspapers were misrepresenting Hilton's comment by referring to the "sound barrier". The idea of a physical barrier to flight – that airplanes could never fly faster than the speed of sound – became widespread among the public. Furthermore, even though most engineers and scientists knew that was not the case, they still had no idea how much the drag would increase in the transonic regime, and given the low levels of thrust produced by airplane powerplants at that time, dealing with the speed of sound certainly loomed as a tremendous challenge. The only way to find out, ultimately, was to build an airplane and try.

The High Speed Research Airplane

The idea of a research airplane – an airplane designed and built strictly for the purposes of probing unknown flight regimes – can be traced to the thinking of John Stack in 1933. On his own initiative, Stack went through a very preliminary design analysis of a hypothetical airplane for flying well into the compressible regime for the single purpose of obtaining aerodynamic data in this regime. This concept and a design sketch was presented in his paper published in the first issue of the *Journal of the Aeronautical Sciences*, referenced earlier in this paper. Ultimately the NACA did not act on helping Stack find a developer for the airplane, but this preliminary proposal started the thinking process that finally led to the Bell X-1.

We began this paper by transporting ourselves back to October 14, 1947, and riding with Chuck Yeager as he flew the Bell X-1 through the sound barrier, becoming the first human to fly faster than sound. The detailed events concerning the design, construction, and early flight testing of the

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[16] Hilton, W.F. "British Aeronautical Research Facilities," Journal of the Royal Aeronautical Society,1966, 70:103-4

X-1 are nicely related by the historian Richard P. Hallion[17], and more details concerning the evolution of the technology surrounding the X-1 can be found in Anderson.[18] Such matters are beyond the scope of the present paper. Rather, we have emphasized that the first supersonic flight of the Bell X-1 represented the culmination of 260 years of research into the mysteries of high-speed aerodynamics. It was especially the fruition of 23 years of insightful research in high-speed aerodynamics by the NACA-research that represents one of the most important stories in the history of aeronautical engineering.

Postscript

On December 17, 1948, President Harry S. Truman presented the thirty-seven-year-old Collier Trophy jointly to three men for "the greatest aeronautical achievement since the original flight of the Wright brothers' airplane."[19] The Trophy, officially the Collier trophy for the year 1947, was the highest possible official recognition for the accomplishment embodied in the X-1. The announcement page from the December 25, 1948 issue of *Collier's* magazine shows Lawrence D. Bell, labeled the manufacturer, Captain Charles E. Yeager, labeled the pilot, and John Stack, labeled the scientist. The citation to Stack read: "for pioneering research to determine the physical laws affecting supersonic flight and for his conception of transonic research airplanes." *A major purpose of the present paper is to bring meaning to this citation* – so much is hidden in these few words.

The 1947 Collier Trophy was also a recognition of the role of engineering science in the ultimate success of the Bell X-1. Note that in the award John Stack is explicitly recognized as a scientist (not an engineer). This is somewhat of a misnomer – Stack was performing as an engineering scientist in this activity, neither a pure scientist nor a pure engineer.

At the time of this award, John Stack was assistant Chief of Research at NACA Langley. In 1952, he was made assistant Director of Langley. By that time he had been awarded his second Collier Trophy, the 1951 Trophy,

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17 Hallion, Richard P., *Supersonic Flight*, Macmillan, New York,1972.

18 Anderson, John D., Jr., *A History of Aerodynamics*, Cambridge University Press, New York, 1997.

19 Collier's, December 25, 1948

for the development of the slotted-throat Transonic Wind Tunnel. In 1961, three years after the NACA was absorbed into the National Aeronautics and Space Administration, Stack became Director of Aeronautical Research at NASA Headquarters in Washington. Despairing the de-emphasis of aeronautics in NASA, after thirty-four years of government service with the NACA and NASA, Stack retired in 1962 and became vice president for engineering for Republic Aircraft Corporation in Long Island. When Republic was absorbed by Fairchild Hiller in 1965, Stack was appointed a vice president of that company, retiring in 1971. On June 18, 1972, Stack fell from a horse on his farm in Yorktown, Virginia, and was injured fatally. He is buried in the churchyard cemetery of Grace Episcopal Church in Yorktown, only a few miles away from NASA's Langley Research Center. Today, F-22s from the nearby Langley Air Force Base fly over the churchyard – airplanes that can routinely fly at twice the speed of sound, thanks to the legacy of John Stack and the NACA high-speed research program.