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*Original Paper*

# Personal Experience on the Problem of Ethical Behavior in Scientific Publications

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***Abstract***

*The American Scientist published in 2011 an article by Anderson Melissa S. et al. dedicated to the problem of ethical conduct in the publication of technical and scientific papers. The author of this article was motivated to reconsider events which occurred to him in the years 1990-1996 at the Argonne National Laboratory (ANL) U.S., under the viewpoint of the analysis made in the mentioned article. While at ANL the author invited Prof. Katsuhiro Sakai, of the Osaka University, Japan, to cooperate with him, during a one-year stay, on numerical problems related to thermal and fluid-dynamics computer simulations. At the end of the stay Prof. Sakai wrote a report documenting his work made with the author. The section manager, who had not cooperated to the technical work, pretended to be considered as co-author of the report, excluding the author of this article. This blatant violation of professional ethics was reported to higher management levels which, however, engaged in a cover-up policy, thus violating the Code of Ethics established by the Laboratory. Those events are summarised in this article and reviewed from the viewpoint of the considerations illustrated in the article by Anderson Melissa S. et al.*

***Keywords***

*unethical behavior, code of ethics, plagiarism, anomie, ANL (Argonne National Laboratory)*

## 1. Introduction

I worked for fifteen years, from 1974 to 1990, at the Institute for Reactor Development (IRE, Institut für Reaktor Entwicklung) at the German Research Centre of Karlsruhe, KfK (Kernforschungszentrum Karlsruhe), in the development and application of computer programs for safety analysis of fast breeder reactors under hypothetical accident conditions. Most applications of the computer codes were dedicated to the safety analysis of the German fast reactor prototype SNR-300, built, but not taken into operation, at the site of Kalkar, in north-west Germany.

Because of the termination in Germany, for political, not technical, reasons, of the SNR-300 project, I decided to leave KfK at least temporarily, looking for a chance to continue, in another Country, to dedicate myself to fast breeder reactor technology and safety analysis. At that time the U.S. were still supporting the Clinch River Breeder Reactor (CRBR) fast reactor project and therefore a possibility was represented by an appointment in the U.S., for instance at ANL (Argonne National Laboratory), where I had already been a few years before and where I had received consideration from Dr. Sha, the leader of the Analytical Thermal Hydraulic (ATH) section of the CT (Components Technology) Division.

Following my resignation at KfK, I joined Dr. Sha's section at ANL, on August 1, 1990, at first as visiting scientist, with a one-year term. At that time the Analytical Thermal Hydraulic staff was working on the optimisation of a new advanced single-phase code version COMMIX-1C. The acronym stands for COMponent MIXing, and refers to a series of codes dedicated to the numerical simulation of transient, three-dimensional fluid flows.

The ATH research program also aimed at deriving from the COMMIX-1C single-phase code version a multi-component, multi-phase code version which was called COMMIX-M. Having studied the documentation of the COMMIX-1C program and analysing preliminary results I realized that the problem of numerical diffusion in COMMIX-1C had not been solved satisfactorily. With the term "numerical diffusion" are called numerical errors arising from the discretization of differential equations. While at KfK I had been cooperating with a friend of mine, Prof. Katsuhiro Sakai of the Osaka University in Japan, on the task of minimizing the numerical diffusion in our computer programs. Looking back to the successful co-operation with Prof. Sakai at KfK, I suggested Dr. Sha to invite Prof. Sakai to ANL for one year to help us by introducing into the COMMIX-1C code the QUICK (Quadratic Upstream Interpolation for Convective Kinematics) method, one of the most popular algorithms for minimization of the numerical diffusion.

Dr. Sha accepted the suggestion and, following a formal invitation from the management of the Laboratory, Prof. Sakai came in August 1991, as visiting scientist, to join the Analytical Thermal Hydraulic research program at ANL, with a contract for one year. Soon thereafter Prof. Sakai and I started to introduce in the COMMIX-1C code the QUICK methodology which had already been successfully developed and tested at KfK and in Japan. After about half a year from the beginning of Prof. Sakai's stay and in view of the continuation of the work after his departure from ANL, the young colleague Dr. J. Sun, who had just graduated form the Illinois University at Champagne-Urbana, and, although brilliant as a student, had not experience in numerical methods, started co-operating with Prof. Sakai and contributed to the development of the code. At that time, to help Dr. Sun to get acquainted as fast as possible with the QUICK method, I gave him a Japanese report, written in English, and all my personal notes with very detailed documentations of the analytical treatments involved. The notes were about 40 pages in total.

Meanwhile, on September 16, 1991, after termination of my first term as visiting scientist, I had become staff member of ANL in the CT Division. Dr. Sha had opened a position in his section and formally had declared me as "*the ideal candidate*" to fill that vacancy.

Beside the cooperation with Prof. Sakai, my main tasks to be tackled as staff member involved development and verification of the above mentioned computer code COMMIX-M. This code had to be applied to the NRC supported project on Pressurized Water Reactors (PWR) safety analysis, entitled "COMMIX PWR Applications", for which Dr. Sha and I were both PI (Principal Investigators). I was also involved with modeling of turbulence in single-phase flows with thermal stratifications and, later on, in particulate two-phase flows.

**2. Prof. Sakai's Report**

Before his departure from ANL in summer 1992, Prof. Sakai wrote a report of about 130 pages documenting his work done at ANL in co-operation with Dr. Sun and myself. I amended his English, improved the text and returned him the draft before the end of 1992 for further revision and possible additions. A second version of the draft report was sent from Prof. Sakai to Dr. Sha at the end of 1993. Dr. Sha prepared a front page in which he inserted as co-authors the names of Sakai, Sun and Sha, but ignoring my name and therefore my contribution to the full work. It became clear to me that Dr. Sha wanted to share the credit of the work with Prof. Sakai and Dr. Sun, but was trying to exclude me from co-authorship. This attitude was retaliation for open criticism I had expressed about Dr. Sha's unethical way of conducting the section. Details about the reasons of disagreement between me and Dr. Sha are given in (Bottoni, 2013). Dr. Sun, after a preliminary temporary term, had just been promoted to staff member. As a counterpart for his promotion he was requested to accept Dr. Sha's imposition to join him in the attempt to sideline me. Thus far Dr. Sun and I had been on good friendly terms, used to spend free time together in the countryside of Illinois. In a vis-à-vis talk I asked Dr. Sun whether he really wanted to continue in his hostile attitude towards me and spoil our friendship. His answer was: "*I have no choice*". In fact he was aware that in case of refusal to act as requested by Dr. Sha his life, already difficult, would have turned to hell. Dr. Sha's attitude of imposing to a young scientist an unethical attitude is a blatant violation of what Zuckerman says is <*perhaps the first commandment of science ... that "thou shalt not mislead thy colleagues"*> ((Zuckerman, 1977, pp. 87-138), quoted by E. Garfield in (Garfield, 1987, 88-92)).

In January 1994 I requested Dr. Sha and Dr. Sun to give me a copy of the second version of Prof. Sakai's report, because I wanted to continue to revise it. As the best connoisseur of Prof. Sakai's work I was best entitled to continue that task. Dr. Sha and Dr. Sun refused to give me a copy of the report. I then asked Prof. Sakai i) to send me a copy of the second version (draft of December 1993) of the report and ii) to state clearly in written form who had contributed to his work and co-operated with him during his work at ANL. Prof. Sakai sent me immediately a copy of the report. Furthermore, with a letter dated December 28, 1993, Prof. Sakai made clear statements about the co-operation at work.

Excerpts from his letter are as follows [The English sentences written by Prof. Sakai are quoted with minor improvements of the style but without change of the meaning]:

*<Regarding the co-authors, frankly speaking, my understanding as to individual contributions to my work is as follows:*

*The contributions of Dr. W. T. Sha to the above-mentioned work*

*1) Dr. W. T. Sha arranged the budget matters for this work, by which I could carry out this work at ANL.*

*2) The original program which I implemented is COMMIX-1C, which was improved by Dr. Bottoni based on the COMMIX series originated by Dr. W. T. Sha.*

*3) Dr. W. T. Sha did not make contributions to my work at ANL from the rigorous technical point of view.*

*The contributions of Dr. Bottoni to the above-mentioned work*

*1) Dr. M. Bottoni made a lot of technical contributions to my work at ANL from the rigorous technical point of view.*

*2) At the beginning of my work at ANL, Dr. Bottoni kindly gave me his private notes regarding the QUICK scheme implementation, which were very helpful to me during my work in ANL.*

*3) When I implemented the higher order schemes, Dr. Bottoni gave me the quite valuable idea to move the terms, other than those regarding the nearest neighbor cells, to the source terms in solving the algebraic equations. According to his idea, I could get the essential structures of the solving algorithm employed in the original COMMIX-1C, which … gave impact to my work.*

*4) At the beginning of my work at ANL, Dr. Bottoni kindly introduced and directed me about implementation of COMMIX-1C program. He gave me the original source of COMMIX-1C and input data for some test calculations.*

*5) During my work, I often visited Dr. M. Bottoni's office to discuss with him. He kindly took time for discussions with me.*

*6) From the view points of mathematics and physics in connection with my implementation of higher order schemes in the COMMIX-1C, Dr. M. Bottoni surely made a lot of technical contributions to my work.*

*The contributions of Dr. J. G. Sun to the above-mentioned work*

*1) Dr. J. G. Sun found out one serious programming error of using seven characters in subroutine XMOMI, by which one variable was undefined and then the higher-order effects could not be simulated sufficiently in momentum equations resulting in no Karman vortex shedding. With a better compiler the computer would have detected such programming error, as Osaka University computer can do. Anyway I understand that this serious error caused him a lot of extra time.*

*2) Dr. J. G. Sun found out some other programming error.*

*3) Dr. J. G. Sun made manuscripts for the chapter of the FRAM damping technique and some sample calculations. He took care of my report after I left USA.*

*4) Dr. I. G. Sun made a plotter program for streak lines in co-operation with Mrs. Leshan Wang. I used it.*

*Consequently, frankly speaking, I prefer co-author with Dr. M. Bottoni as well as Dr. J. G. Sun.*

*Moreover I wish that Dr. Bottoni continues to take care of my report, since he kindly made preliminary corrections to my report and understands well it theoretically.>*

For the reader unaware of fluid-dynamic problems, I recall that the so-called "von Karman vortices", mentioned in Prof. Sakai's letter, are eddies which form beyond an obstacle invested by a fluid. For a computer program simulating numerically fluid-dynamic problems the capability to reproduce formation, shedding and dissipation of the vortices in the wake of the fluid beyond the obstacle is a condition "sine qua non" for the correctness of the simulation. The problem of von Karman vortices had therefore been chosen by Prof. Sakai to test numerically the impact of the introduction of the QUICK algorithm upon the accuracy of the calculations. Because numerical solutions obtained using the QUICK algorithm may be affected by unphysical oscillations, a technique called FRAM (Filtering Remedy and Methodology), also mentioned in Prof. Sakai's letter, can be used to smooth out unphysical oscillations.

**3. Memorandum to Director-ET**

In the following, to make anonymous the management cadres of the Argonne National Laboratory, we use the following conventions: The Director of the Energy Technology Division is referred to as **Director-ET**; the General Manager of the Energy and Industrial Technologies is referred to as **Manager-EIT**; the Associate Laboratory Director is referred to as **Director-ALD**; the Laboratory Director is referred to as **Director-ANL**; the Director of the Human Resources (a woman) is referred to as **Director-HR**.

On June 1, 1992, the Director-ANL, signed and sent to all Argonne employees a document called "*Employee Problem Resolution Policy and Procedures*". In a cover letter to that document Director-ANL explained that policy and procedures for resolving employee problems had been revised to incorporate new laws and regulations and to provide a formal process to address problems involving technical and scientific issues. The main objective of the document remained however essentially the same, namely <*to provide employees a means to have problems addressed fairly and promptly*>.

Because Dr. Sha continued to claim the right to exclude me from co-authorship of Prof. Sakai's report, I decided to address myself to Director-ET, with a letter dated October 17, 1994, centred on the subject: "*Violation of the Code of Ethics, by Dr. W. T. Sha and Dr. J. Sun*". From this letter, in which first of all I summarized the situation explained previously, I give following excerpts:

<*The attempt by Dr. Sha and Dr. Sun to exclude from the co-authorship the colleague who has suggested, initiated, and contributed to the work with Prof. Sakai is a clear violation of the Code of Ethics. I think you should be made aware of it.*

*Dr. Sha's attitude is not new. In 1985 he excluded from co-authorship of the documentation of the COMMIX-2 code Dr. Lyczkowski, who, together with Dr. Domanus and me, was one of the principal contributors to the code.*

*From June 1991 through May 1992, I have been principal investigator, together with Dr. Sha, of the project entitled "COMMIX PWR Applications". My contribution to this project has been making the COMMIX-M code instrumental for the calculations needed for numerical simulations and in developing a "film tracking model" to describe the cooling of a containment with a liquid film flowing down its inner and outer surfaces. In May 1992 Dr. Sha ignored my contribution to the work and dropped my name from a technical publication presented at the Co-operative Severe Accident Research Program in Bethesda, Maryland.*

*Dr. Sun has been under constant pressure from Dr. Sha to act as he did. ...*

*This situation is inadmissible in a Laboratory where the calmness of the working environment should be granted for the sake of the quality of scientific investigation and of technical achievement.*

*Regardless of the technical significance of the report prepared by Prof. Sakai, I consider it a matter of principle that all contributors be acknowledged appropriately. I would therefore like to request you to take the necessary steps to correct an abnormal situation. I will greatly appreciate your consideration of this matter.*

*Because the issue concerns professional ethics, I am also bringing this to the attention of the Head of Human Resources>*.

A copy of the above letter (October 17, 1994) addressed to Director-ET was also sent to Director-HR.

Meanwhile, in a memorandum from Director-ET to the members of the ATH section, dated December 15, 1992, and called "*Change of Responsibilities*", the Division Director informed that Dr. Sha had been "*relieved of his administrative responsibilities*" as section manager and that the Associate Director-ET would be "*Acting Section Manager of the Analytical Thermal-Hydraulics Section in addition to his other responsibilities as Associate Division Director*".

I received on November 23rd, 1994 a reply to my memorandum to Director-ET, in which he stated [excerpt]: "*... the authors* [of the Sakai's report] *have agreed to credit your contribution in the acknowledgment. At this time, I just do not have enough evidence to overturn* [the decision of the Associate Director-ET] *not to require Sun, Sha and Sakai to include you as an author of their manuscript*".

The decision of the Associate Director-ET had apparently been given informally. I have no written record of it. It became evident to me that the management was ignoring Prof. Sakai's statement about the contributions to his work. It became clear to me that Director-ET was afraid of taking a decision against Dr. Sha's will. The reason for this attitude was in everyone's mouth: Dr. Sha was friend of Director-ANL, for whom—gossips went on—he had arranged invitations to visit China, through the mediation of his wife Joan's uncle, who was minister in China. If a decision would have been taken against Dr. Sha's will, he would have marched to Director-ANL's office to complain, and trouble for

the decision-makers had to be expected.

**4. Petitions to Higher Decision Levels**

**i) Petition to Manager-EIT**

Director-ET had not been willing to make justice. I was well aware that he had not taken any decision without consulting the Manager-EIT. Thus recourse to Manager-EIT would have been useless. I had however to follow the Laboratory's procedure which did foresee an application to the next management level. Manager-EIT had been promoted only months earlier to that new position. To comply with the Laboratory's rules I addressed myself to Manager-EIT with a memorandum dated December 20, 1994. After summarizing the situation, as sketched in the previous part of this account, and after recalling the statements written by Prof. Sakai, concerning the contributions given to his work at ANL, I concluded my petition asking that all contributors to Prof. Sakai's work should be considered as co-authors of his report.

Manager-EIT's response came on January 23, 1995. As expected he did not want to make justice. He concluded his answer by stating:

<*My review has not determined any reason to reverse the decision already reached. Therefore, I encourage you to accept acknowledgement of your efforts in the report, and to put the matter behind you*>.

**ii) Petition to Director-ALD**

I addressed myself to next higher management level, namely to Director-ALD. In a memorandum dated February 2, 1995, I explained him the situation and I asked for corrective actions to be taken. Director-ALD's answer, dated February 7, 1995, was very quick, following my petition by only five days. He wrote [excerpt]:

<*I ... see no further reasons for changing the position on authorship taken* by [Director_ET and Manager-EIT]. *Issues on authorship can be difficult, and there is no perfect process for resolving conflict. I trust that you will recognize this and, as* [Manager-EIT] *said, "put the matter behind you"*>.

I appealed Director-ALD's decision and I was invited to have a talk with him, in his office, on February 20, 1995. In that occasion Director-ALD confessed to me that he had not examined the many memoranda and annexes he had received with my first petition to him, but had just tried with a quick answer to get rid of the problem. As the problem persisted, following my appeal to him, he was willing to look for a fair solution. Because of lack of time, however, he said he could not get into the details of all documents exchanged, but was making the proposal to delegate the decision to a person of his choice outside of the Laboratory, provided Dr. Sha would also agree on the proposal. I agreed with the proposal at the only condition, accepted by Director-ALD, that all documentation, which I had given him, would be forwarded to the person appointed as decision-maker. Before leaving his office, Director-ALD assured me he would talk with Dr. Sha and let me know about his willingness to accept the proposal.

Dr. Sha did not accept the proposal. Obviously, he was sure that an impartial person, outside of the Laboratory and not exposed to anyone's retaliation, would recognize the absurdity of his claims.

Months passed by and I did not receive any updating of the situation. I therefore decided to appeal to Director-HR.

**iii) Petition to Director-HR**

In a memorandum dated September 12, 1995, addressed to Director-HR, after introducing the general situation, I denounced the violation of the Code of Ethics in the issue of the cooperation with Prof. Sakai and I asked for corrective actions to be taken.

According to the rules of the "*Employee Problem Resolution Policy and Procedures*", signed by the Director-ANL, on June 1st, 1992, the normal response time for providing a written answer was set in 10 to 15 days. I would therefore have expected an answer from Director-HR, within a comparable period of time, but she never answered my complaint. She seemed to have forgotten that the goal of the above mentioned document was, in Director-ANL's words, *<to provide employees a means to have problems addressed fairly and promptly>*. In thus doing, Director-HR ignored and herself violated the Code of Ethics, obliterating the rules that had been established.

**5. Letter to Director-ANL**

From the previous account it becomes evident that the ANL management preferred to wash hands, rather than taking seriously into consideration the application of the "Code of Ethics". In 1996 it was known that Director-ANL was going to retire in a short time and in fact a nationwide search for a new Director to fill the vacancy was going on. The Laboratory Director was the grantor of the correct application of the Laboratory rules and procedures, especially regarding the ethical conduct of the staff members. ANL rules and procedures did not foresee an appeal to the Laboratory Director but I decided anyway to address myself to him. I had a faint confidence that he would consider the issue of the violation of the "Code of Ethics" and possibly take actions for its enforcement. For these reasons, on June 20, 1996, I addressed to Director-ANL the following letter:

*<To: Director-ANL*

*From: M. Bottoni, ET-308*

*Subject: On the question whether the ANL "Code of Ethics" is a document of only platonic value.*

*On October 6, 1995, I sent you a copy of a memorandum, dated September 12, 1995, directed to Director-HR, in which I denounced a blatant violation of the Code of Ethics (signed by you on June 3rd, 1993), being perpetrated at the Argonne National Laboratory.*

*In the past two years I painstakingly followed the "Employee Problem Resolution Policy and Procedures" (signed by you on June 1st, 1992) in the attempt to enforce an equitable solution of the problem outlined in the above mentioned memorandum. Unfortunately, the ANL management, at all levels, from the Division Director to the Associate Laboratory Director, was not willing to enforce the Code of Ethics. The Director of Human Resources did not even consider it appropriate to give a formal*

*answer to the complaint addressed to her consideration.*

*The "Employee Problem Resolution Policy and Procedures" does not foresee an appeal to the Laboratory Director, and therefore I am not writing to you in observance of these procedures.*

*Because, however, you are preparing your farewell from the Laboratory, I am addressing myself to you to raise the question whether you would like to see your signature put upon the "Code of Ethics" duly honored, or you accept that, in spite of your signature, the "Code of Ethics" continues to be a document of only platonic value.*

*Enforcing the observance of the "Code of Ethics" would be a very honorable conclusion of your leadership at the Argonne National Laboratory.*

*I thank you in advance for your consideration of the matter, and I wish you the best for the years to come>*.

A copy of this letter was sent to Director-HR.

Director-ANL did not reply to my letter. This attitude contradicted the good intentions formulated in the last two paragraphs of the Intra-Laboratory Memorandum on the "*Code of Ethics*", signed by him on June 3, 1993:

*<On matters concerning the Code of Ethics on which you feel you cannot or have not received satisfactory attention or response, you always have access to senior management officials at Argonne, including your respective Division Director or Associate Laboratory Director, the Director of Human Resources, our Chief Legal Counsel, and me.*

*The confidence that others have in the Laboratory to carry out its mission with integrity depends largely upon the quality of decisions we make in our daily work, and strict adherence to the concepts and values outlined in the Laboratory's Code of Ethics is one assurance that our excellent reputation for integrity will continue>*.

Contrary to Director-ANL's nice statements, one should draw the conclusion that *<the confidence that others have in the Laboratory>* is actually infirmed by the lack of integrity at management level and by the disregard of the *<adherence to the concepts and values outlined in the Laboratory's Code of Ethics>*.

Apparently Director-ANL was not interested in seeing honored his signature put upon the "*Code of Ethics*", which remained once more defrauded of his value.

**6. Eugene Garfield on the Ethics of Scientific Publication**

Every scholar in the United States of America, and also most of the scholars abroad, know Eugene Garfield, of the Institute for Scientific Information (ISI) of Philadelphia, PA, the information scientist, who, after a brilliant career stretching over more than 60 years, passed away in 2017. Among the several essays written by Eugene Garfield, particular interest raised in me an article published in 1978 concerning "*The Ethics of Scientific Publication*" (Garfield, 1978, pp. 644-651). Other significant articles on the topic of fraud and other forms of intellectual dishonesty in science were issued in 1987

and are recalled as (Garfield, 1987, pp. 88-92) and (Garfield, 1987, pp. 93-100). I quote now these articles, and especially the first one of 1978, because they enlighten the kind of procedures which should be definitely considered as fraudulent. They also allow us to consider the events occurred at the Argonne National Laboratory under the light of the opinions of recognized scholars dealing with several aspects of fraudulent actions.

The correctness of the practice used in the United States for attributing authorship to scientific papers is strongly questioned in a survey performed in 1970 among psychologists and since known as the "*Spiegel Survey*", from the names of its originators Spiegel and Keith-Spiegel. As reported by E. Garfield, the authors of the survey state that

<*It is unethical to give co-authorship to someone of higher status in one's organization unless he makes a substantial contribution to the project… The results* [of the survey] *suggest that neither power nor status should be determinant of credit assignment*>.

E. Garfield also reports that in a letter dated 1977 addressed to the Editor of "*Drug Intelligence and Clinical Pharmacy*" three researchers, who preferred to remain anonymous, strongly attack the practice of superiors receiving authorship status for work performed completely by their subordinates. The letter states:

<*This is not only an act of egomania, but a means by which non-achievers fabricate achievement. A non-achiever could actually obtain a job over the achiever, simply because he has more publications listed on his curriculum vitae, thereby appearing to be more accomplished. … If the person(s) did not intend to use the false-authorship for further self-benefit, then why it is so important that they demand their name(s) be included? The reasons are obvious*>.

The problem of co-authorship, I continue paraphrasing E. Garfield in (Garfield, 1978, pp. 644-651), is actually twofold: From one side there is the problem of the non-contributor who should not be entitled to co-authorship status, but eventually gets it in virtue of his powerful status in the establishment. The second problem concerns the real contributor who does not receive recognition of his co-authorship when in fact it is warranted. In some cases research organizations or other professional societies have created an internal code of ethics and formed ethics committees which do usually acknowledge the existence of ethical problems but also remark that seldom complaints are addresses to them. In the vast majority of cases people do accept injustice rather than exposing themselves to retaliation from the powerful superior.

<*If strict guidelines were adopted*, —writes Dr. Garfield—*they might deter the powerful from using their influence to get their names on papers to which they did not contribute…*

*However, Spiegel and Keith-Spiegel conclude that guidelines, however fair and comprehensive they may be, will still no guarantee an equitable distribution of credit if the decision is left to a manipulative, egoistical, or unethical individual: "Unfortunately, it is the person with the most status and power who usually makes the ultimate credit determination. The lower status individual, if dependent on the higher status individual for his job, may be reluctant to even bring up the issue of credit"*.

<*Only the most outrageous and persistent egomaniac* - concludes Dr. Garfield - *could put his name on papers that were not his*>.

Having been illuminated by Dr. Garfield's insights into the problem of ethics in scientific publishing I want now to make a few comments on the deeds of people at the Argonne National Laboratory. Let us consider a few excerpts:

Excerpt 1: <*It is unethical to give co-authorship to someone of higher status in one's organization unless he makes a substantial contribution to the project…*> Putting the emphasis upon the verb "*to give*" we would deduce the following. Not only the claim for co-authorship status, made by Dr. Sha in case of the report documenting Prof. Sakai's work at ANL, is unethical, but also is unethical the attitude of the ANL representatives at higher management levels who did concede it, though being well aware of the false-authorship status of Dr. Sha. The accusation of unethical behavior is therefore addressed also to the Associate Director-ET, to Director-ET, to Manager-EIT, to Director-ALD and to Director-HR.

Excerpt 2: <*...guidelines, however fair and comprehensive they may be, will still no guarantee an equitable distribution of credit if the decision is left to a manipulative, egoistical, or unethical individual ...*> These epithets apply perfectly to Dr. Sha, as if a psychologist had written them, knowing the person.

Excerpt 3: <*The lower status individual, if dependent on the higher status individual for his job, may be reluctant to even bring up the issue of credit*>. This was unfortunately true in case of several subordinates in Dr. Sha's section, who did not dare to complain about Dr. Sha's unethical practices.

As remarked elsewhere in Dr. Garfield's essays, fraud and intellectual dishonesty in scientific endeavors are, after all, rare events, occurring with a rate much lower than in other human endeavors, where fraud, embezzlement and other kinds of crimes are much more frequent. Nevertheless, what happened at the Argonne National Laboratory in the section headed by Dr. Sha, over a couple of decades, was characterized by an exceptionally high rate of fraudulent occurrences, which were not undetected but simply tolerated by the higher cadres of the management.

## 7. Interpretation of the Events from the Viewpoint of "Authorship Diplomacy"

The quoted article by Anderson et al. (Anderson, Kot, Shaw, Lepkowski, C., & De Vries, 2011, pp. 204-207) starts with the following truly appropriate sentences on the value of authorship:

<*Among scientists, authorship is a very big deal-and for good reasons. It not only establishes the record of scientific progress but also stakes a scientist's claim to originality and priority. As sociologist Robert Merton noted decades ago, recognition for original work is the coin of the realm in science. Authorship is the basis for promotion, tenure, salary, honors and invitations to participate in prestigious initiatives*>.

I comment on a few words of the above quotation: "*Authorship is the basis for promotion*, *tenure, salary, honors*...". The question might arise about the motivation for a person like Dr. Sha, in the position of Section Manager in a great National Laboratory, which could be the dream of many researchers, to act in the way he did in the case of the Sakai's report, claiming credit for work not done, and trying to exclude Prof. Sakai's main co-operator. Without being a psychologist, I am convinced that the key point for a rational explanation of this irrational behavior lies in the recognition that, for Dr. Sha, to become division director was dream, struggle and nightmare. Many similar psychopathic behaviors, originating in minds that can be considered deranged, are explained on the basis of the anomie theory. The etymology of the word (<a>, arising from the privative alpha in Greek, followed by <nomos> or <law>) implies "lawlessness" in current language. The definition given in the Merriam-Webster dictionary is "*social instability resulting from a breakdown of standards and values*". As reported in (Garfield, 1987, pp. 93-100) by information scientist Eugene Garfield, the theory of anomie was developed in its more advanced forms by Merton in a series of pivotal articles, from 1938 through the 1970's. In the above quoted article Dr. Garfield writes:

<*In 1957 Robert K. Merton, Department of Sociology, Columbia University, New York, gave an interpretation of deviant behavior in science in terms of the race for priority. He observed that competition in the realm of science, intensified by the great emphasis on original and significant discoveries, may occasionally generate incentives for eclipsing rivals by illicit or dubious means*>.

According to Merton, aberrant behavior in scientific conduct, resulting in frauds and unethical practices in publishing, is often originated by the perception of some individuals of the own ineptitude to reach goals, and therefore social recognition, they had long time aimed at. This would explain why some scientist, with a mind degenerated by self-imposed pressure, would resort to cheating to keep the pace of more able colleagues and struggle to replicate their achievements.

The concept of "anomie" has gone over the past century through a series of shades in its meaning. Recent research on the subject has been often devoted to analyzing Morton's work and interpretation on the light of new sociological events. It is now recognized (Featherstone & Mathieu, 2003, pp. 471-489) that

<*Robert Morton presented two, not always clearly differentiated theories, in his seminal explorations on the social-structure-and-anomie paradigm: a strain theory and an anomie theory*>.

Differentiation and partial overlapping of these two theories continues to be discussed in current literature on sociology, but we do not pursue further this topic because it does not shed more light on the series of events we are discussing. The reader interested in a broad discussion on anomie theories and their relevance in the frame of sociology may consult the well known book by Gerber and Macionis (Gerber & Macionis, 2010).

With reference to another unethical practice, referred to as "*Errors of Omissions*", the Anderson et al. article states that <*One of the most obvious problems in collaborative authorship is omitting authors from a paper. The classic form of omission occurs when two collaborators are in conflict (professional*

*or personal) and one leaves the other's name off a paper out of spite>.*

This was indeed the situation which occurred about the co-authorship of the report I wrote on the documentation of the COMMIX-2 computational program. Disregarding the fact that Dr. Lyczkowski, with his theoretical contributions to the analytical description of two-phase flows, was one of the key persons for the final success of the code development, Dr. Sha pretended to exclude, and was successful in excluding, his name from the list of authors. The unspoken reason for this attitude was Dr. Sha's animosity towards Dr. Lyczkowski, due to the simple fact that he had decided to change Department within ANL. In that case the management of the Argonne National Laboratory tolerated, if not endorsed, this violation of the Code of Ethics perpetrated by Dr. Sha.

I quote again from Anderson et al., article:

*<... sometimes authors agree to be left off a publication in exchange for some other form of compensation, usually financial. This arrangement, known as "ghost authorship" is a problem in U.S. as well as in other parts of the world>.*

Ghost authorship was also practiced very intensively by Dr. Sha. As I was guest at ANL in 1984, Dr. Sha, who had been working at the Laboratory for about twenty years, gave me a list of his publications with about 200 entries. This would have implied that he wrote about ten publications per year. I think that even the most prolific scientists would admit that it is simply impossible to have something new to publish with the frequency of almost once a month. This frequency can be reached either with a "salami technique", in which publications are repeated in different journals or presented at different conferences, adding just a comma to the previous one, or can be explained by use of the "ghost authorship". Both techniques were amply used by Dr. Sha.

Scrutinizing Dr. Sha's list of publications I perceived at once that not only the "salami technique" was used, but, much worse than that, he was also cheating. As already explained earlier, during my stay of 1983-84 at ANL I was developing a new version of the two-phase flow code COMMIX-2, together with Dr. Lyczkowski and Dr. Domanus, as principal co-operators. Before this code development every attempt to make two-phase flow calculations in nuclear reactor fuel bundles under hypothetical accident conditions, which was the main goal, had failed.

After completion of the development of the COMMIX-2 code Dr. Lyczkowski and I were able to simulate completely a two-phase flow experiment during typically a few seconds. This was the first time such calculations were made at ANL and maybe also in the United States. Apart from the COMMIX-2 report, documenting the details of the code, our work was made known to the scientific community, with one publication (Bottoni, Lyczkowski, Chi, Chien, & Domanus, 1985) presented at the 23rd National Heat Transfer Conference, Denver, Colorado, August 4-7, 1985. In this work a simulation of 4.5 seconds of two-phase flow regime is reported.

Before this publication presented by Dr. Lyczkowski at the Denver conference, Dr. Sha had made several publications in which it was reported about "*successful*" simulations of two-phase flow. In reality only a few tenths of a second could be simulated in the two-phase flow regime, because the

preliminary version of the code did fail, becoming unstable, just after boiling initiation. In making several publications presenting work not successfully completed, not only the "salami slicing" technique was applied, but it was cheated, declaring operational a code which was not properly working. The question which I could never answer was how peer-reviewers could have accepted those articles for publication. Facts like this cast suspicion about the competence or the rectitude of some peer-reviewers.

Dr. Sha was also a master in the application of "ghost authorship" to inflate his publication list. He had some Chinese friends, professors at the University of Champagne-Urbana in Illinois, who were regularly paid to write reports which were published under Dr. Sha's name, without any technical contribution from him. The quality of the reports was generally very good, at a level that Dr. Sha would not have been able to reach on his own. The professors were paid with money which should have been destined to make research within ANL, fostering young talents. I concede that under particular circumstances the support provided by consultants is necessary and gives new impulses to the work of a section. However, to rely almost exclusively upon consultant work and to reduce young co-workers to the rank of mere programmers of theoretical formulations prepared outside of the section was a disregard of the professional capabilities of the young scientists. It was frustrating to see how fine academic minds were degraded, year after year, in Dr. Sha's section, to perform tasks which should have been carried out by programmers.

## 8. Conclusion

My activity at the Argonne National Laboratory during the years 1990-1996 was extremely important for my professional formation, mainly due to fruitful cooperation with two exceptional scientists who were for me examples of professional capabilities and integrity: The first was Dr. Henry M. Domanus, the very genius in Dr. Sha's group, who unfortunately passed away prematurely; the second was Dr. Robert Lyczkowski whose broad and multifaceted experience taught me a lot about modelling of two-phase flow. The working environment was however unpleasant because of the tense atmosphere created by the presence and attitudes of Dr. Sha, even after his demotion from the position of section manager. All events mentioned shortly in this article have been explained, with abundance of details, in my referenced book. Writing, editing, publishing the book have been demanding tasks, but I do not regret to have spent a considerable part of my time in accomplishing that task. The reason for this can be easily understood by reading the very last beautiful sentences of the article by Anderson et al., which I quote:

*<Maintaining the integrity of authorship is complicated in the global contest, but the stakes can be high for all concerned. It is worth the time and effort required to get it right>.*

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