

U N I V E R S I T Y *of* H O U S T O N

Department of Physics

Paul Ching-Wu Chu
Executive Director T_cSUH

262 Houston Science Center
Houston, TX 77204-5002

713-743-8222
Fax: 713-743-8201

May 2, 2007

Richard B. Bannerot, Ph.D.
Professor, Department of Mechanical Engineering
University of Houston
N231D Engineering Building
Houston, TX 77204-4006

Re: Grievance Filed by Pei Heng Hor, Ph.D.

Dear Dr. Bannerot:

This letter responds to your letter of April 3, 2007, as Chair of the Ad Hoc Subcommittee of the University Grievance Committee, which included the grievance and exhibits filed against me by Dr. Pei Heng Hor.

I first became aware of this issue in January 2006 when Dr. Hor and Ru-Ling Meng first complained to Dona G. Hamilton that they were the true inventors of the superconductivity-related patent applications filed January-March 1987. Over 2006, Hor and Meng engaged several attorneys, and Hor's attorney threatened to file a lawsuit on several occasions.

The law firm of Akin Gump Strauss Hauer & Feld, who has long represented the University regarding the patent applications in which inventorship is disputed, was asked by the University to investigate these charges on behalf of the University and me. Akin Gump will also provide legal counsel to me in this grievance proceeding.

These challenges by my colleagues Pei Hor and Ru-Ling Meng have been very disappointing to me personally. Over the last fifteen months, my role as inventor has been subjected to false claims made by Affidavit and by attorneys representing Hor and Meng.

Almost twenty years passed without any suggestion that I was not the proper inventor. Now, at least one potential witness is deceased, and others who participated in these events either cannot remember or were never personally involved in these issues. I am pleased, however, with one helpful event which occurred within the last week—my wife located my calendar from this period—which contains my personal entries that expose the falsity of these charges. However, you are entitled to a detailed rendition of the events which led to these important discoveries, and I shall attempt to provide you and the entire Grievance Committee with truthful information that will hopefully put this dispute to rest.

I. DEVELOPMENT OF THE CLAIMED INVENTIONS

The patent family tree of Ex. 1 shows a number of patent applications filed between January 12, 1987 and March 26, 1987, as well as additional but related applications filed even later. However, only the patent application filed March 26, 1987 has issued. U.S. Patent No. 7,056,866 ('866 Patent) issued on June 6, 2006 (Ex. 2). The only pending U.S. application has Serial No. 07/300,063 (Ex. 3), which can be referred to as the "Mixed Phase Patent Application." While Dr. Hor has challenged the inventorship of the '866 Patent, if the Grievance Committee is to resolve this dispute, inventorship of the Mixed Phase Patent Application should also be considered in view of Dr. Hor's claims.

A. The '866 Patent Claims in Issue

As I understand the law relating to inventorship, which is described in Attachment 1 to this letter, the question of inventorship is to be directed to the claims of the patent or patent application, rather than to the technical description that precedes the claims, known as the specification. The following are representative claims of the '866 Patent:

Representative Claims of U.S. Patent No. 7,056,866 ("'866 Patent") (Ex. 2)

Composition

Claim 1. A composition which is superconductive at a temperature of 70°K and higher, comprising: a metal oxide of the formula $[L_{1-x}M_x]_aA_bO_y$ wherein; "L" is yttrium (Y), lanthanum (La), neodymium (Nd), samarium (Sm), europium (Eu), gadolinium (Gd), dysprosium (Dy), holmium (Ho), erbium (Er), thulium (Tm), ytterbium (Yb), lutetium (Lu), or mixtures thereof; "M" is barium (Ba), strontium (Sr), or mixtures thereof; "A" is copper, "x" is from about 0.65 to 0.80; "a" is 1; "b" is 1; and "y" is a value from about 2 to about 4 that provides the metal oxide with zero electrical resistance at a temperature of 70°K or above.

NOTE: This would include, for example, $La_{0.33}Ba_{0.66}Cu_{1.0}O_{2.4}$, which is "1-2-3." A nominal "2-1-4" is not included.

Claim 8. A material containing a sufficient quantity of a superconductive crystalline phase to cause the material to exhibit substantially zero electrical resistance at a temperature of 77°K or above; said crystalline phase composition having the formula $LM_2Cu_3O_{6+\delta}$, wherein "L" is Y, La, Nd, Sm, Eu, Gd, Dy, Ho, Er, Tm, Yb, Lu, or mixtures thereof; "M" is Ba, Sr or mixtures thereof; and δ is a value from about 0.1 to about 1.0 that provides the composition with zero electrical resistance at a temperature of 77°K or above.

NOTE: This would include $Y_1 Ba_2 Cu_3 O_{6+2}$, which is "1-2-3." A nominal "2-1-4" is not included.

Process (This is the only process claim in dispute).

11. A method for making a superconducting metal oxide, comprising the steps of:

(a) mixing solid compounds containing L, M, A and O in amounts appropriate to yield the formula $[L_{1-x}M_x]_aA_bO_y$ wherein "L" is yttrium, lanthanum, neodymium, samarium, europium, gadolinium, dysprosium, holmium, erbium, thulium, ytterbium, lutetium, or a combination thereof; "M" is barium, strontium, or a combination thereof; "A" is copper, "a" is 1 to 2; "b" is 1; "x" is about 0.01 to about 1.0; and "y" is a value from about 2 to about 4 that provides the metal oxide with zero electrical resistance at a temperature of 40°K or above;

(b) compacting the mixture into a solid mass by application of pressure from about 100 to about 30,000 psi;

(c) heating the solid mass in air to a temperature of from about 800 to about 1000°C for a time sufficient to react the compacted mixture in the solid state; and

(d) quenching the solid mass to ambient temperature in air.

NOTE: This includes "1-2-3." A nominal "2-1-4" is also within the scope of this claim.

Representative Claim of U.S. Patent Application Serial No. 07/300,063 (Ex. 4)

Composition

22. A superconducting composition exhibiting zero electrical resistance at a temperature of 40°K or above consisting essentially of yttrium, barium, copper and oxygen.

In summary, any determination of inventorship should consider whether or not Dr. Hor was an inventor or co-inventor of these patent claims, as well as the other claims of the '866 Patent and the Mixed Phase Patent Application in their current form. (Exs. 2 & 4).

II. DEVELOPMENT OF THE CLAIMED INVENTIONS OF THE '866 PATENT

In 1995, I was asked to write a chapter entitled "High Temperature Superconductivity" for a new book to be entitled "History of Original Ideas and Basic Discoveries in Particle Physics," which was published in 1996. Chapter 42 is included as Ex. 5. This chapter describes

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much of my recollection of the events that occurred over the general time period November 1986-March 1987.

I began my work at the University in 1979 (in 1977-1979 on leave from the Cleveland State University) in the Department of Physics. I brought together a group of physics professors, technicians, doctoral, masters and undergraduate students to formulate potential superconductive compounds, synthesize the compounds into samples (most often in pellet form) and test the pellets for superconductive properties. My group continued this work throughout the early 1980s, and we produced many papers published in the scientific community.

In 1986, in order to help maintain the funding for research programs in superconductivity, it was necessary for me to take a temporary position as Program Director at the National Science Foundation. This required me to work in Washington, D.C., but I returned to Houston regularly over many extended weekends to continue directing my group in experimentation. From my point of view, I personally led all developments in the area of superconductivity in this period of time, and supervised directly or indirectly all individuals working in the superconductivity lab, including Dr. Hor (then a Research Graduate Assistant) and Ru-Ling Meng (then a visiting scholar). Going to Washington, D.C. four-five days a week to work for the NSF was a tremendous personal sacrifice, but it was necessary for us to keep the future funding going. And, in order to keep the lab progressing with technical developments, I worked with my group every weekend that I returned to Houston to ensure that we stayed on track with our projects and that my ideas to move our research in certain directions were carried out. I was in phone contact almost daily when I was not in Houston. Over the pertinent months of November 1986-March 1987, I returned to the University 11-20 days per month with the knowledge and support of the NSF.

The Bednorz & Müller article (Ex. 6) opened the door to great possibilities in the area of superconductivity. Upon reading the article in early November 1986, I immediately met with Ru-Ling Meng and decided that we would confirm the work of Bednorz & Müller, but using the dry reaction method that we had been using for some time in the preparation of our own samples. In the following weeks in November, we had great success in formulating and testing products within the Bednorz & Müller nominal formulae for superconductivity. On December 3, 1986, I sent Drs. Bednorz and Müller a note informing them that my group at UH had reproduced their results and noting my search for the important and yet unknown phase. (Ex. 7).

My next step was to apply physical pressure to the synthesized pellets during the testing for superconductivity. The application of pressure to enhance or suppress superconductivity was a known scientific practice, but the Bednorz & Müller La-Ba-Cu-O systems were new, and presented a new opportunity to determine if these compounds could have a superconductive

temperature (T_c) enhancement by the application of pressure. As set forth in my declaration of Ex. D in Dr. Hor's grievance letter, the application of pressure to compositions of the La-Ba-Cu-O system was found to enhance T_c . (¶ 3).

I then began to consider utilizing chemical substitutions of the elements in the basic Bednorz & Müller La-Ba-Cu-O system to simulate the effect of these pressure techniques. In searching for substitutions, I looked to the periodic table and considered the interatomic distances of potential substitute elements. As described in ¶ 4 of Ex. D, I envisioned the enhancement T_c by reproducing the chemical effects of pressure by substituting for either the Ba atom or the La atom with alkaline earth metal atoms and rare earth metal atoms, respectively, having smaller atomic radii.

Long before the meeting in late December 1987 or early January 1987 relied upon by Dr. Hor as his "original" conception of the use of yttrium (Y), I had already determined that a program of chemical substitution was the next step in the development process and implemented the first phase of that process with instructions given to Dr. Wu to substitute strontium (Sr) for barium (Ba). I had recalled in Ex. D, ¶ 4 to the Hor grievance that on Monday, December 8, I called Dr. Wu of the University of the Alabama to give him his first assignment, which was to substitute Sr for Ba in the La-Ba-Cu-O compositions. (However, the call may have been on December 6 as noted on my calendar entries quoted below.) I assigned to Ru-Ling Meng the substitution of calcium (Ca) for Ba. Meng's work began in early December and continued into January. On December 14, 1986, I sent a Christmas card to Dr. Wu in which I said that I was "full of confidence of 77K." (Ex. 8).

Just a few days ago, my wife discovered my calendar for 1986-87, which I thought was discarded long ago. Ex. 9 includes the calendar pages for December 1986 – March 1987. All entries are written by me during that time period, typically on a daily basis just as shown. The following entries confirm my prior statements that I solely conceived of the substitution of Sr and/or Ca for Ba and of the substitution of yttrium (Y), lutetium (Lu) and/or ytterbium (Yb) for La:

December 4, 1986:

"Boston MRS

Pre-view LaBCO w/Kitazawa

Asked Wu to join the project. Lunch together."

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December 6:

“called CY

MK Wu

for Michel & Raveau paper.”

December 18:

“complete replacement of La by smaller Y, Yb, Lu - - - -”

December 19:

“No Y...”

December 26:

“...Y, Lu have to work ____”

January 2, 1987:

“think about Patent – Cu-Nb, Zr, V, Ta, W, N?

La-Sc, Y, Yb, Lu

Ba-Sr, Ca”

I had stated in my Declaration of C.W. Chu dated December 1, 1990, that I had disclosed to Meng the concept of substituting Y and/or Lu for La in a telephone call which occurred about mid-December 1986. (See Ex. D, ¶ 4, to Hor grievance letter). Additionally, Meng had filed at least one declaration and provided deposition testimony agreeing with my account. Then, in early 2006, Meng stated that she had lied previously, and that no such conversation ever occurred. Now, my calendar entries confirm the truth, and it should be clear that Meng’s retractions in 2006 were not truthful.

While it is possible that Dr. Hor independently also thought of Y for La at the meeting in late December 1986 or early January 1987, his idea was a duplication of my own and therefore, was not original. Further evidence of the concrete nature of the ideas that I had in late November and December 1986 are found in my patent disclosure, which I personally completed on January 9, 1987. (Ex. 10). This patent disclosure was formally written up as my first patent application, which was filed January 12, 1987. (Ex. 11). That first patent application served as the basic

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patent document for a series of patent applications which were instituted after that, ending with, for purposes of this dispute, my patent application filed March 26, 1987. (Ex. 12).

In order to be certain that my ideas were fully described, the University retained a different patent firm and my application of January 12 was re-written and re-filed on January 27 (Ex. 13). That patent application clearly shows my appreciation for Y and other interatomic substitutions to make new superconductive compounds.

The sample of Y-Ba-Cu-O that Dr. Wu brought to Houston on January 29, 1987, was entirely consistent with my patent applications filed January 12 and 27, and thus I had not only conceived but reduced to writing in two U.S. patent applications a range of formulations that included the very compound that Dr. Wu brought to UH. (Exs. 11 and 13). And, I showed my application to Dr. Wu, and I believe he agreed. I also told both Hor and Meng about this patent application, though they have apparently suffered a lapse of memory.

Once we studied Dr. Wu's sample and made our own more stable samples of Y-Ba-Cu-O in late January-early February 1987, we ascertained the existence of two predominant phases, a black phase and a green phase. My next step was to institute a series of reformulations of Y-Ba-Cu-O in order to determine which of the two phases was superconductive, and to obtain the specific formula and the crystallographic structure for that superconducting phase. Over the early weeks in February, I had the lab working hard to resolve the question of which of these phases was the superconductive phase by comparing the superconductive volumes of samples with different compositions. As confirmed by my calendar of Ex. 9, on February 13, 1987, I enlisted the help of Drs. David Mao and Robert Hazen of the Carnegie Institute of the National Geophysical Laboratory in order to use the laboratory's sophisticated equipment to help determine the exact atomic formulation and structure of the superconducting phase, a phase I had been searching for since mid-December 1986, as stated in my article of Ex. 14 at 407.

Throughout this period of work with Dr. Hazen, no one from my group dealt with Dr. Hazen except me. Finally, the exact formulation known as "1-2-3", $Y_1Ba_2Cu_3O_{6.5}$, for the black phase was determined and the crystallographic structure was also finalized so that by early March, we knew much more about the Y-Ba-Cu-O system and how those compositions accomplished superconductivity at high temperatures.

During February-early March, we wanted to be certain that we understood the scope of discoveries to date, so we began a process of "doping" these compounds of Y-Ba-Cu-O with small amounts of magnetic rare earth elements from the periodic table to determine if Y in Y-Ba-Cu-O is crucial to the high temperature superconductivity observed by its partial replacement with the magnetic rare earth element Gd. (Ex. 15). To our delight, we determined that the

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superconductivity of Y-Ba-Cu-O remained the same, so that after the 1-2-3 formulation became known, the next step was to formulate an entire series of rare earth metal substitutions for Y in the basic Y-Ba-Cu-O formula to determine the full scope of the invention. Once this work was completed, I filed the March 26, 1987, patent application, which ultimately issued as '866 patent of Ex. 2.

In 1988, the University received a payment from DuPont under a licensing arrangement relating to these patent applications. I was to receive \$684,779.70. (Ex. 16). However, I wanted to share that reward with the staff that worked so long and hard throughout this incredibly busy but wonderful period of discovery. To that end, I gave contributions to a group of individuals which included Ru-Ling Meng, Pei Hor and Dr. Wu of the University of Alabama in amounts \$137,000 each. Much smaller amounts were given to various students. I elected to receive \$239,529.70. It is interesting, yet saddening, that my payments are now being used against me as supposed evidence of contributions to the level of inventorship by Dr. Hor. (*Id.*)

III. RESPONSE TO FACTS ASSERTED BY DR. HOR IN F.1-11

Response to F.1:

Throughout the period in dispute, November 1986-March 1987, even though I was at the NSF on a full-time basis acting as Program Director, I spent 11-20 days per month in my lab at UH directing the advances made from Bednorz & Müller's La-Ba-Cu-O compounds to the new family of 1-2-3 superconducting compounds including various rare earth elements such as Y.

When I accepted my position at NSF in about September 1986, it was necessary for me to name an alternate principal investigator (PI) on NSF projects to avoid any apparent conflict of interest. We were currently working under NSF Award No. DMR-86-126539 (Ex. 17) which was awarded based on my proposal of July 1, 1986, in which I was identified as the sole PI. (Ex. 18). I took this opportunity to have doctoral student and then Research Assistant Hor assume that position, but only as an alternate. As set forth in the October 1, 1986, letter of Julie Norris, Hor was appointed as "Acting Principle Investigator" during my absence. (Hor Grievance, Ex. B, Att. A.1-1). In Hor's letter of December 8, 2006, to the NSF, he agreed "to serve as an alternative PI for Dr. Paul Chu's NFS projects . . . during his one-year assignment at NSF." (Hor Grievance, Ex. B, Att. A.2). At no time did Dr. Hor take my place, except in name only, as the PI for the superconductivity lab at UH.

During this period of time, because of my intense interest and direction of this ongoing and exciting project to develop high temperature superconductivity, I never relinquished my role to anyone. Surely Dr. Hor cannot honestly suggest that he acted as my superior in any

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development effort relating to superconductivity. I was in charge, and Hor was an alternate for purposes of the NSF only.

Dr. Hor did not become a Ph.D. until 1990. We appointed him a Visiting Assistant Professor effective September 1, 1987, in order to satisfy the NSF requirement to be an alternate PI. But, of course, a Visiting Assistant Professor has no supervising authority at all. From the standpoint of UH, during this period November 1986 through March 1987, Hor was a Research Assistant until December 15, 1986, when I recommended him to become a Research Associate, and nothing more.

Response to F.2:

It is my personal belief that Dr. Hor believes his suggestion of Y for La in a meeting with Dr. Wu and Ru-Ling Meng was "original" because of his lack of consistent participation in the development process which was ongoing at the time. Almost a month prior to the meeting that Dr. Hor relies on, I had already made the decision and given directions to substitute alkaline earth metal atoms Sr and/or Ca for Ba, and in mid-December, I instructed Meng to make substitution of Y and/or Lu for the rare earth element La. My conception of Y as a substitute for La is clearly set out in my handwriting on my December 1986 calendar on December 18, 19 and 26, 1986. (Ex. 9).

Response to F.3:

Dr. Hor was the PI in name only because of my employment as Program Director for NSF. I began this project with directions to Ru-Ling Meng to begin synthesizing superconductive compounds according to the nominal formulas in the Bednorz & Müller paper and carried the work through the discovery of 1-2-3 formulations of Y-Ba-Ca-O as set out in the '866 patent, based upon the patent application filed March 26, 1987. (Ex. 12). During this period of development, Hor as a Research Assistant, and as an alternative PI to me, never acted as a true PI. Hor wrote none of the articles. Hor made no invention disclosures and, in fact, no claim to inventorship.

As early as May 29, 1987, Hor signed a Patent Assignment Agreement at the University of Houston stating that he would "notify the University . . . of any invention which is conceived during the period of my University employment." (Ex. 19). Again on December 5 and 12, 1988, Hor signed similar statements. (Ex. 20). Insofar as I know, Hor never notified the University of any invention at any time. In fact, Hor is not listed as an inventor on any U.S. patent application filed during the period in dispute or any time up to the present. It was my responsibility and further, because of my direction in the development of these high temperature superconductors,

my knowledge that drove the development of the claimed inventions of the '866 Patent. At most, Hor helped supervised the lab along with Ru-Ling Meng and helped me and Ru-Ling in thinking through all of the issues relating to these developments. While Hor deserves credit, and I have given him credit at every opportunity, from the standpoint of determining patent inventorship, Hor acted as another "pair of hands" at best. (See The Law of Inventorship of Attachment 1).

While Hor states that I filed patent applications without his knowledge, that is not true. I showed Dr. Wu a copy of my patent application filed January 27, 1987, to inform him that I had already filed on the concept of using Y-Ba-Ca-O. (Ex. 13). I believe that the fact of filing that patent application was most likely known to Hor because he likely knew that I had shown my application to Dr. Wu. Additionally, Hor had to know of the secrecy imposed upon our group not only because of the intense competition that was ongoing but also because of the advice of University patent attorneys that no information about our work was to be released in order to protect ongoing patent filings. (Ex. 5 at 812).

Response to F.4:

For the first time in any of his contentions made over the last year, Hor now suggests he first wrote down a formula that included scandium (Sc) during Wu's trip to Houston. Referring to my January 2, 1987, calendar entry, Sc is conceived as a substitution for La weeks before Dr. Wu's trip on January 29, 1987. (Ex. 9) Further, I understand Hor's claim is not pertinent to the issue of inventorship because Sc is not recited in any of the claims of the '866 patent or in any of the claims of the Mixed Phase Application. Regarding Hor's implication that he was actually the first person to disclose to me a formula that included Y when he supposedly provided Wu's formula on January 29, 1987, he is obviously wrong in view of my calendar (Ex. 9), patent disclosure of January 9, 1987 (Ex. 10); and patent applications of January 12, 1987 (Ex. 11) and January 27, 1987 (Ex. 13).

Response to F. 6:

This statement relates to Hor's alleged original discovery of gadolinium (Gd) as a substitution for Y. In this paragraph, Hor suggests that he asked Meng to replace Y completely with Gd after March 12, 1987, when the formula for 1-2-3 became known. However, this is not Hor's first statement regarding Gd. In his statement entitled "High Temperature Superconductivity Research at U of H During 1986 - 1987 by P.H. Hor" 3/7/06 (Ex. 21), Hor states on the third page that

“[i]n mid-February, I was thinking about the mechanism of high temperature superconductivity. In order to study the T_c

suppression effect due to magnetic ions, I told Meng that I had a strange idea and asked her to replace Y by Gd. To my surprise T_c did not degrade. I realized that all of the rare earth substituted samples could be high temperature superconductors. Meng quickly synthesized a whole series of new high temperature conductors by the mid-March.”

In Hor’s affidavit of March 14, 2006, Exhibit B to his Grievance, Hor describes his discovery of Gd as follows:

“In mid-February . . . I told Meng that I had a wild idea and asked her to replace Y by Gd.” (Hor Grievance, Ex. B at 4).

But in F.6, Hor describes his instructions to Ru-Ling Meng as taking place after March 12, 1987, once the 1-2-3 phase was known. This statement is probably more accurate, but Hor fails to understand the reason for the accuracy.

The decision to substitute Gd for Y after the discovery of the formulation 1-2-3 had little to do with Hor. It was my decision to substitute a group of rare earth elements which were magnetic, including Gd, for Y using the new 1-2-3 formula, and Meng did so. The earlier work done in February was not the result of some “wild” or “strange” idea by Hor, but was rather applying a known principle of testing of superconductive compounds using the replacement of small portions of magnetic elements such as Gd for Y to determine the Y effect on the compound. (Ex. 15). If the addition of the magnetic rare earth element diminished the superconductivity, this meant that Y was actually influencing the superconductivity. That work was being done in February at my instruction. But after the specific 1-2-3 formula was determined, and already knowing that the magnetic elements did not affect Y, it was natural to run the entire spectrum of magnetic elements as complete substitutions for Y. I made this point in my paper of Ex. 5 at 814.

Response to F.7:

Unfortunately for Hor, his statement that no conclusion can be drawn from any type of partial substitution is contrary to science. The partial substitution which I supervised in February was consistent with prior scientific known work described in Ex. 15. This work was always contemplated by me. This type of substitution of magnetic elements to probe the role of the rare earth element was anticipated by me and my lab in January, as supported by the request of January 12, 1986, for cerium (Ce) and Gd in addition to Lu and Y, long prior to Hor’s “wild” or “strange” idea. (Ex. 22).

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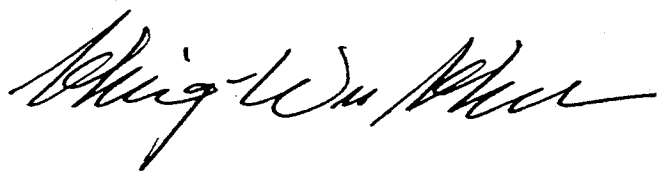
Response to F.11:

I have always done my best to encourage my students, undergraduate and graduate, to achieve professionally as high and far as possible. Part of my philosophy is to include as many of those students as possible on my papers. During the period from November 1986 through March 1987, I personally wrote every paper on this topic. These papers are Exs. 14, 23-25, and 29-31 in the notebooks. It was my practice to always put my name first when I entered into a new development. For this reason, my name was listed first on the article submitted December 15, 1986 (Ex. 14) as well as the article submitted December 30, 1986 (Ex. 23). For the articles that followed in February-April, I put my students and other collaborators first because I believed in sharing the professional recognition after I had established my initial role as the technical leader in a new field. Dr. Hor was never listed in any particular location as an author because of any contribution that he made, and his suggestion that I included him as first because he came up with the idea of Y as a substitution for La is simply false.

In Hor's Grievance Letter, Ex. B is his Affidavit of March 14, 2006, which includes at A5-1 my memorandum of December 15, 1986, recommending Hor as a Research Assistant as some evidence of Hor's contributions. Of course, Hor did work with the entire group in this exciting period, but my glowing comments are typical of my style in recommending students for promotion or new positions, and not evidence that he is an inventor. (See letters of Ex. 32).

In closing, I hope I have provided the Committee with credible evidence of my leadership and inventorship during this historic period of research. While I have tried to be generous in praising and rewarding my dedicated staff throughout my career, I believe that I have been properly identified as the sole inventor of these important discoveries and that these belated and untruthful charges should be dismissed without recognition or compensation to Dr. Hor. I look forward to discussing these issues with the Committee. Thank you.

Very truly yours,



Paul C. W. Chu, Ph.D.

Attaches: Attach. 1 – THE LAW OF INVENTORSHIP
Attach. 2 – LAW OF LACHES AND ESTOPPEL
IN VIEW OF CLAIM OF INVENTORSHIP
NOTEBOOK OF EXHIBITS

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The Breakthrough: The Race for the Superconductor,
Robert M. Hazen, 1988.