

AFFIDAVIT OF P.H. HOR, Ph.D.

THE STATE OF TEXAS)

COUNTY OF HARRIS)

On this the 14th day of March, 2006, before me, a Notary Public in and for the State of Texas, personally appeared P. H. Hor, Ph.D., who, being duly sworn, upon oath deposes and says:

That I am of full legal age, have never been convicted of a felony or a crime involving moral turpitude and am in all respects competent to make this affidavit; that I reside in Houston, Harris County, Texas and that my business address: Superconductivity Center, Houston Science Center, Houston, Texas 77204.

High Temperature Superconductivity Research at UH during 1986-1987

by P.H. Hor 3/14/06

Spring - Summer, 1986:

I was a graduate student and a Research Assistant in Dr. C. W. Chu's group. I was finishing up my experiments and getting ready to write my dissertation to graduate in Fall so I could start working as a Post Doctoral fellow for Dr. Frank DiSalvo at Bell Laboratory and Dr. Jean-Marie Tarascon at Bellcore in Spring 1987.

In the mean time, as the most senior student in our laboratory after M. K. Wu graduated and left for a faculty position at U. of Alabama in 1985, I also helped, functioning as a postdoc, in supervising lab activities and training junior graduate students which was a tradition in our laboratory. I learned how to perform high pressure experiments from Wu.

In 1986 superconductivity research funding was drying up and our group was facing a funding crisis. Dr. Chu arranged and agreed, contingent upon an automatic renewal of our National Science Foundation (NSF) grant, to serve as Program Director at NSF for one year.

Dr. John Vasilliou graduated from the U. of Chicago was hired as a Visiting Assistant Professor to serve as the Principal Investigator (PI) of our group during Dr. Chu's one year assignment.

It was either close to the end of the Spring semester or early Summer, 1986, when Dr. Chu told me that John Vassiliou was not capable enough, and that he did not feel comfortable letting Vassiliou leading our group. He asked me to stay and serve as the PI of our group [A. 1].

Sept. - Dec., 1986:

Dr. Chu left for Washington D.C. and assumed his full time position at NSF. I assumed the duties of the PI [A.2] and supervised the laboratory. Dr. Chu returned to Houston regularly over weekends during which time we would discuss about our research work.

In mid-November, Ruling Meng got a copy of the paper that reported a possible high superconducting transition temperature (T_c) compound based on Ba-La-Cu-O system and discussed it with Dr. Chu. I arrived at laboratory later that day and learned about this system. Meng gave me a copy of the paper and our group started to explore this material system.

As PI of the group I reviewed all the data to pin down the material synthesis conditions to produce optimal T_c . Meng was the only person with a background in material science, and was therefore responsible in synthesizing all the samples. Continuous discussion with Meng was necessary while reviewing the sample preparation conditions. High T_c was quickly reproduced in Ba-La-Cu-O system initially, but was unstable and poorly reproducible. Better control of T_c was achieved by refining the synthesis parameters such as sintering temperature and processing time through continuous feed-back of T_c measurements.

While routine measurements were performed by junior graduate students, I would either set up or perform more elaborated experiments such as AC susceptibility, I-V curves and upper critical fields etc., whenever needed. When possible, I also tried unconventional approaches to sample preparation. For instance, on Nov. 15, 1986 I sintered a Ba-La-Cu-O sample on a copper substrate and observed the indication of $T_c \sim 70K$, the very first high T_c produced and observed in our laboratory. I measured the sample under magnetic field and followed the transition 4 times. It was a genuine

superconducting transition but T_c was unstable and degraded at the 4th measurement [A.3].

During the second week of December, a high pressure measurement was performed on the best possible sample available at that time and a phenomenal pressure effect and world-record $T_c > 40K$ was observed. A Physical Review Letters paper was published based on this work [A.4], and I was recommended to be appointed as a Research Associate by Dr. Chu [A.5].

During the last week of December, we started to mimic the pressure effect using iso-valence substitution of Ba by chemically equivalent elements with smaller ionic radius such as Sr and Ca, a well-known and standard practice in the field. Dr. Chu met Wu at the Material Research Society (MRS) Fall meeting in Boston and informed him about our success in observing high T_c in the Ba-La-Cu-O system. He suggested to Wu that he should work on the Sr-substitution. I recall that when Ruling came to me and indicated that she wanted to continue working on the Sr-substitution, I had stopped her because there was no sense in duplicating the effort.

Wu came to Houston to measure his Sr-substituted sample near the end of 1986. At that time we had already known that while Sr-substitution increased T_c , the Ca-substitution could not further increase T_c . During a group discussion in my office either at the end of 1986 or the beginning of 1987 (with Wu, Meng, and Gao present), I pointed out this negative result to Wu. Since there were unstable $T_c \sim 70K$ and the highest stable T_c observed at that time was $\sim 52 K$ under pressure, it indicated that further increase of T_c by chemical means should still be possible, but could not, however, be achieved by simply replacing Sr by Ca. I placed a periodic table in front of everyone and tried to find an alternative way to further enhance T_c . By considering the ionic radius mismatch and magnetic properties, I came up with the idea of replacing La by Y. Immediately after our discussion, Meng placed the order for Y_2O_3 since UH Chemistry stock room did not carry this material.

January-March, 1987

I remember that I had mentioned to Chu when he returned to Houston one weekend that I was going to work on the Y-substitution. He did not have any response.

We received Y_2O_3 later and started the synthesis of the Y-substituted samples. Before we could expand the composition range and fully explore the Y-substitution system, we received reports from our collaborators that our samples of Ba-La-Cu-O system were not superconducting. We dropped the Y-substitution system and turned around to check the production of the Ba-La-Cu-O system. Before we could figure out what was wrong in making the Ba-La-Cu-O samples, on Jan. 29th, 1987, Chu came into my office and said that Wu had just called and indicated that he had observed $T_c > 77K$. Chu said he asked Wu to come to Houston to further check the sample but Wu asked to talk to me. I went and picked up the phone to talk to Wu. He indicated that he observed a resistive transition above liquid Nitrogen temperature. I asked, "Is it stable?", and he replied, "Yes." I then said, "You should come to Houston to measure the magnetic property of the sample", a measurement necessary to verify if it was a genuine superconductor. He hesitated, so I told him, "Don't worry, just come." He agreed to come. Before we hung up, I asked, "What is it?" and he replied, "It is what we had discussed in Houston." After I hung up, Chu asked me, "Is he coming?" and I replied, "Yes." He asked me again, "Did you ask him what it is?" to which I responded, "Yes, he says it is what we had discussed in Houston." Chu asked me to write down the formula. I did. [A.6]. Ruling Meng and Y. Q. Wang had worked overnight to calculate, weigh, and synthesize the compounds.

Wu arrived on Jan. 30th and Wu's first sample that we measured showed $T_c > 90K$. The subsequent AC magnetic susceptibility measurement indicated that it was a genuine superconductor.

In mid-February, I was thinking about the mechanisms of high temperature superconductivity. In order to study the T_c suppression effect due to magnetic ions, I told Meng that I had a wild idea and asked her to replace Y by Gd. To my surprise, T_c did not degrade. I realized that all the rare earth substituted samples could be high temperature superconductors. Meng quickly synthesized a whole series of new high temperature superconductors by mid-March. A Physical Review Letters paper was published based on this work [A.7].

In early March, through extensive testing of various samples with systematic composition variation, the identification of the YBCO phase was achieved in

collaboration with Bob Hazen at the National Geophysical Laboratory in Washington D.C.

Over the entire period from 1986 to 1991, worked as a PI, I was in charge of all research related activities of our laboratory: discussing and communicating with collaborators and visitors, writing papers and proposals, serving as referee to review scientific papers and proposals, and serving as consultant to some outside companies. Starting from the Fall semester of 1987 I also worked as a member of the physics faculty, doing classroom teaching and supervising graduate students and postdoctoral fellows in our laboratory.

In July 1987, I was offered a Visiting Assistant Professor position by UH with the understanding that this position would be transformed into a tenure track position after I received my Ph.D.; the time served as a Visiting Assistant Professor would be credited towards my tenure. I graduated and got my Ph.D. in 1990, and I was appointed as Assistant Professor of Physics in the Fall semester, 1991. I was then promoted to Associated Professor with tenure in the Fall semester of 1992. From 1988-1990, I was also the Associate Director for Science of The Texas Center for Superconductivity. From 1990-1992, I was the Associate Director for Material Research of The Texas Center for Superconductivity.

Chu had once nominated me for an award, the Overseas Chinese Physics Association Award, in 1992. I have located one of the recommendation letters [A.8] written by Dr. Louis Testardi who held the world-record of T_c before 1986.

I was the PI of our group until spring of 1992, when our group split into two. Chu and I became the PIs of our own groups.

The research on high temperature superconductivity during the period of 1986 – 1987 at UH was a group effort that involved undivided dedication, open-hearted collaboration, tremendous mutual trust and sacrifices from all of the group members. C. W. Chu has contributed a lot in presenting our results to the outside, drafting all the papers, establishing contacts with outside collaborators and participating in almost all phases of research activities in our group. Ruling Meng has contributed a lot in monitoring the production and synthesizing all the samples in our group. One Research Scholar, YaQi Wang, and graduate students Li Gao, Z. H. Peter Huang

(deceased), and Jeffrey Bechtold have contributed tremendously to our success in our high temperature superconductivity research by continuously performing various measurements and screening and testing samples through countless sleepless nights. Without all these measurements, nothing could be achieved.

Over the past 19 years, as far as I can recall, there were only two occasions when I have met with a patent lawyer concerning the YBCO discovery.

1. Sometime between 1987 or early 1988, not long after the Texas Center for Superconductivity at UH (TcSUH) was established. (Chu, Cox, Meng, Hor)

In the meeting, Charles Cox asked, "Who was the first person to propose the Y-substitution?" Chu immediately turned around and pointed to Meng and said, "Ruling, do you remember that I called you and told you to do the Y-substitution?" Meng said, "I don't remember." Cox then asked, "Then do either one of you (referring to Meng and I) remember which one of you came up with Y-substitution?" Although I was shocked, I did not want to embarrass Chu, so I said, "I do not remember either." Cox then said, "It seems that from the discussions amongst you that no one seems to remember who the first to come up with this idea was." At this moment, Chu mentioned that Meng and I should be included as inventors. Cox then said, "No, no, no! Not everyone can be an inventor. A pair of hands cannot be considered an inventor." Disturbed by this comment, I stood up and walked out of the room. Chu followed me outside, apologized to me, and said, "I am sorry. This lawyer does not know anything about our group. I will go back to tell him and straighten things out." I walked away.

2. Several years ago, the U. of Alabama came back to challenge UH again. Under the request of either the patent lawyer or UH IP officer, I met with Charles Cox and John Warren (I do not remember if anyone else was present) in the conference room in the E. Cullen building.

In this particular meeting, I had described in detail what had happened in the meeting with Wu when he had visited Houston during the end

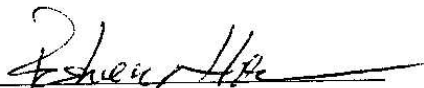
of 1986 and the beginning of 1987. What I had reported then is identical to what I write now.

In the past three years, during several occasions and under extremely stressful situations, I have revealed the truth concerning the discovery of YBCO to Prof. C.S. Ting and Prof. S. H. Pan at UH; Prof. Z. X. Zhao at Chinese Academy of Sciences and Prof. Z.L. Chen at UT San Antonio.

On January 19th, 2006, Ruling Meng came into my office to apologize to me for not speaking the truth about the discovery of superconductivity above 77K. She revealed to me that her conscience has been bothering her for all these years and she wanted to speak out.

On January 27th, 2006, Ruling Meng and I went to John Warren's office and reported the entire incident.

FURTHER, AFFIANT SAYETH NAUGHT.


P. H. Hor, Ph.D.

SUBSCRIBED AND SWORN TO BEFORE ME this 14 day of March, 2006.

Notary Public

