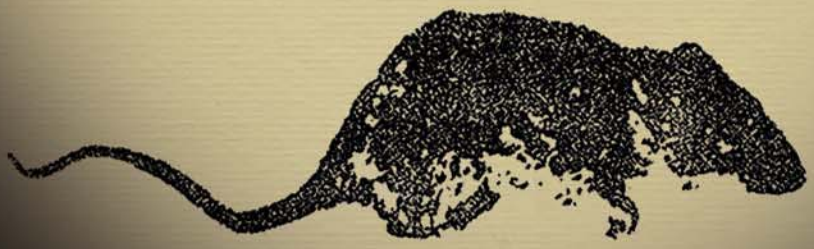
"Lying to the American people
wasn't part of my job description."

Ralph W. Moss, PhD

SECOND OPINION

LAETRILE AT SLOAN-KETTERING

IN THEATERS - FALL 2014



This 48-page document:

Second Opinion Special Report: Laetrile At Sloan-Kettering

was written in the summer of 1977 by Ralph W. Moss, PhD and other anonymous rank-and-file employees of Sloan-Kettering

Second Opinion: Laetrile At Sloan-Kettering is available on Blu-ray, DVD, and Video On Demand.
Also available is ***Doctored Results***, a new book & companion guide to this documentary by Ralph W. Moss, PhD

www.secondopinionfilm.com • Facebook/SecondOpinionMovie • #2ndOpinionMovie

SECOND OPINION SPECIAL REPORT:

LAETRILE AT SLOAN—KETTERING

TABLE OF CONTENTS

On June 15, 1977 Memorial Sloan-Kettering Cancer Center released pre-publication copies of its long-awaited papers on amygdalin, or Laetrile as it is popularly known. At a morning press conference, the leaders of the nation's largest private cancer center claimed that their extensive tests in animal tumor systems proved that amygdalin is useless in the treatment, prevention or cure of cancer. Many newspapers quickly dubbed this the "definitive test" of Laetrile.

"We have no evidence that Laetrile possess any biological activity with respect to cancer, one way or the other," said Dr. Lewis Thomas, president of MSKCC.

"We have found no reproducible evidence that amygdalin, or Laetrile, is active," said Dr. Robert A. Good, president and director of Sloan-Kettering Institute, the research division of the Center.

"Laetrile has been found absolutely devoid of activity, period. It's just that simple," said Dr. Daniel S. Martin, a surgeon at the Catholic Medical Center who collaborated in a number of the experiments.

However, this opinion was not unanimous. When Dr. Kanematsu Sugiura, the 85-year-old Sloan-Kettering researcher who claims positive results with amygdalin, was asked if he stuck by his earlier results, he said, emphatically, "I stick!" Second Opinion also doubted the validity of the official statements, and distributed a "Special Bulletin" outside the conference, detailing some of our doubts.

When Dr. Good was asked by reporters to answer these questions he responded, "All of those questions that are raised on that sheet are clearly answered and can be answered right within the scientific paper."

Dr Martin agreed: "It's all there in black and white, if you take the trouble to read the paper."

In our "Special Bulletin" we also strongly urged the reporters and staff members to read the paper: "Second Opinion hopes that everyone will obtain a copy of this report and study it carefully. SKI is betting on the fact that few science reporters or scientists will actually probe deeply into the report...."

Both sides seem to agree that the truth will emerge from a careful study of the report, which is due to be published in the Journal of Surgical Oncology at the beginning of 1978. We again strongly urge scientists, science writers and interested parties to obtain pre-publication copies of the Sloan-Kettering report or to study it when it appears. We have tried to write this critique bearing in mind the reader who may not have a copy of the official report in front of him.

By studying this monograph, as well as the Sloan-Kettering report, the reader will be in a position to decide whether we or they are correct in the assessment of this important series of experiments.

ANALYSIS OF THE FIRST S.K.I. PAPER

The Sloan-Kettering report on amygdalin is divided into two papers. The first, "Antitumor Tests of Amygdalin in Transplantable Animal Tumor Systems," reports on experiments performed in the second half of 1972

under the direction of C. Chester Stock, Ph.D., vice president of Sloan-Kettering Institute , 1275 York Avenue, New York, N.Y. 10021.

Transplantable tumors have been widely used in the study of anti-cancer agents. Yet they have been repeatedly criticized for their questionable relevance to human cancer, which is spontaneous in nature (1). One must therefore take all negative results in transplantable experiments with a grain of salt.

The SKI paper claims to be a complete record of amygdalin experiments in such transplantable tumor systems. As we shall show, at least one experiment in such a system has been omitted from the report.

There are a number of curious things about these experiments.

First, the dose in most of these experiments is quite low, 250-1000 mg/kg/day. This is higher, on a pound-for-pound basis, than the dose usually given to humans, however it is considerably lower than the 8,000 mg/kg/day found to be safe in SKI's tests, or the 2000 mg/kg/day which Sugiura later found to be the optimal dosage.

(2) The "median survival time" is only given for one experiment. It is therefore impossible to tell, in the vast majority of cases, whether amygdalin had any beneficial effects in prolonging the life-span of the animals.

(3) In the experiments showing the effects of a combination of amygdalin and other known anti-cancer agents, there are some very unusual figures.

For example, the median survival time was exactly the same for mice receiving 1000 mg/kg/day of amygdalin as for those receiving 500 mg/kg/day. It was exactly the same for mice receiving just 5-FU or 5-FU plus amygdalin. It was exactly the same for mice receiving Cytoxan or Cytoxan plus amygdalin.

Despite this remarkable congruity, the two sets of controls differed considerably-- in the first experiment the controls lived an average of 17 days, in the second only 11 days.

The same is true in the experiment on L1210 mouse leukemia. The mice which received Cytoxan lived exactly the same number of days as those which received Cytoxan plus amygdalin; those which received 5-FU lived exactly the same as those which received 5-FU plus amygdalin, and the same for the experiment with Ara-C.

Even if these figures have been rounded off to the half-day, this is a remarkable result in a system whose controls show such a great degree of variability. The odds against such uniform results are astronomical and it is unfortunate that more data is not provided to see how they were achieved.

(4) In one experiment, one of the rats treated with amygdalin intraperitoneally (ip) had a complete remission of all tumors some time between the fourth and the eighth week. Instead of letting this rat live on, it was sacrificed after four weeks of remission. No explanation is given for this seemingly premature sacrifice.

In the animals treated subcutaneously (sc) with amygdalin, one had a complete remission by the fourth week. By the eighth week no animals are shown in remission, yet we are not told what happened to the rat that had been in remission.

(5) The report states that "amygdalin did not noticeably influence the toxicity or impair the efficacy of" a standard anti-cancer agent known as "M" (2a-methyldihydrotestosterone propionate). Yet there appears possibly to be some interference by the amygdalin in the treatment of animals with this hormonal substance.

Rats that received "M" alone had complete remissions in 45 percent of the cases at week three, and 70 percent at week eight. But rats which also

received amygdalin (ip) had only 27 and 46 percent remissions, respectively. Is this significant? It certainly would seem to warrant further study. Neither side in the Laetrile controversy, we might add, would be likely to welcome this finding, for it might show that amygdalin was neither perfectly compatible with other forms of therapy (Krebs) nor completely inert (Lewis Thomas).

In general, we found flaws in the experiments with transplantable tumors, but the results seemed consistent with the findings of other researchers that in general amygdalin is not an effective therapeutic agent in these systems.

ANALYSIS OF THE SECOND S.K.I. PAPER

The second paper, "Antitumor Tests of Amygdalin in Spontaneous Animal Tumor Systems" is longer, more controversial and more questionable in many of its conclusions.

The major part of SKI's amygdalin experiments were carried out between 1972 and 1976 in mice which spontaneously develop cancer. These are used because of the growing belief that spontaneous tumors are more analogous to human cancers than the transplantable types. Spontaneous tumors in animals are also much more difficult to obtain for scientific experiments. Three such animal systems were used in these experiments:

(1) Swiss Albino mouse: This animal, obtained as an old, retired breeder from animal supply houses, develops spontaneous mammary tumors in about 60-70 percent of the cases. These mice have in the past been fairly easy to buy, but never in large numbers. One problem with this animal as a model is that it sometimes has spontaneous remissions (or "cures") of its cancer. It is otherwise very difficult to obtain cures in this system.

(2) AKR leukemia: The presence of leukemia is seen in a large percentage of cases. It is generally detected by palpating the spleen and other internal organs for signs of swelling, as well as by microscopic examination of the blood. Leukemia in these animals is difficult, in fact almost impossible, to cure (Kassel, 1977).

(3) CD8F1 mice: These mice are crosses of BALB/cfC3H and DBA/8 mice. Both these strains are infected with cancer-producing murine mammary tumor viruses (MMTV). Since 1958, at the suggestion of Bittner, these mice have been bred and maintained in the laboratory of Daniel S. Martin, M.D. of the Catholic Medical Center, Queens, New York. Dr. Martin and his colleagues (Fugman, Stolfi, and Anderson) have published several articles on the characteristics of this mouse.

The most critical question in studying these systems is how one decides if there is in fact an anti-cancer effect. In all three models we feel that Sloan-Kettering has seriously misrepresented the facts.

In AKR leukemia, a recent publication by Robert Kassel (in a book edited by Robert A. Good) makes clear that while prolongation of life is the most certain sign of anti-cancer effects, it is very rare. Scientists therefore take a shrinkage of internal organs greater than twenty percent to be a sign of anti-cancer activity. (Kassel, 1977) While Sugiura saw such effects, and commented on them in memos, this is never mentioned in the text of the report.

A more serious distortion occurs in the discussion of CD8F1. The report claims that spontaneous tumors in this type of mouse can regularly be cured

through the use of cytotoxic chemicals employed in the treatment of human breast cancer. Amygdalin is then compared, unfavorably, to standard chemotherapy. Much was made of this comparison at the Memorial Sloan-Kettering press conference. The SKI paper states explicitly:

> "Of those 8 agents declared clinically active against human breast cancer by the National Cancer Institute, all 8 agents also are active against this murine breast cancer....Thus, the negative laetrile findings in this animal tumor model appear particularly significant."

The implication here, as at the press conference, is that amygdalin has failed where other drugs have succeeded. Yet reference to the Catholic Medical Center papers on CD8F1 mice reveals exactly the opposite: when used against a primary, mammary cancer in CD8F1 mice, in the way that amygdalin was tested, no drug is effective. It is only when small tumors are transplanted into male CD8F1 mice, or surgically enucleated, that chemotherapy is seen to have any appreciable effect:

> "Evaluation against spontaneous mammary tumors in their autochthonous hosts is the most rigorous test system....Cure has thus far been impossible to achieve by chemotherapy alone on large primary tumors. Hence, this most difficult methodology has been largely shelved in favor of evaluation by combined modality therapy, i.e. in conjunction with the surgical reduction of primary tumors." (Martin, 1975)

Not only has complete cure never been observed when standard chemotherapy was tried against CD8F1 tumors "in their autochthonous hosts" but only one drug, Cytoxan (cyclophosphamide) has even yielded temporary or partial remissions in this system:

> "Nineteen chemotherapeutic compounds and two non-specific immunity-stimulating agents have been studied at length as to their effectiveness in the treatment of this tumor.
>
> "Spontaneous mammary cancers in mice have proved to be quite resistant to influence by chemotherapy alone. Such anti-cancer agents as uracil mustard, Endoxan, 6-mercaptopurine and thioguanine, as well as many others which have been shown to influence the growth of transplanted tumors, have been ineffective in this spontaneous tumor system." (Martin, 1970)

This discrepancy between the words of the report and the facts about CD8F1 will come as a surprise to all those who have heard Dr. Martin and others insist that amygdalin's failure to cure CD8F1 tumors was "particularly relevant" and damning.

Equally disturbing is the fact that SKI researchers undoubtedly knew that no drug alone could cure primary tumors in CD8F1 mice yet proceeded to test amygdalin in this "largely shelved" system. It is almost as if they wanted it to fail.

SKI's knowledge of this problem is revealed in an unsigned memo of June, 1973 (The memo was among the documents "leaked" from S.K.I. in 1975 and acknowledged to be authentic at that time). It states that

"Dr. Martin...has been employing this strain in examining the efficacy of various chemotherapeutic and immunotherapeutic protocols upon the post-surgical recurrence of malignancy....As a possible extension of this sort of work, amygdalin might be used in this way to determine its effect upon recurrent disease." ("Anatomy...", 1975)

This was never done, to our knowledge. Until it is, one cannot make a fair comparison between amygdalin and other, known anti-cancer agents.

Detecting Metastases

The effect of amygdalin on metastases is one of the crucial questions explored in this research. Therefore, how to find and determine the extent of metastases in Swiss Albino and especially CD8F1 mice is one of the most important technical problems that must be considered.

There are basically three separate methods used at various times and in various combinations in this paper. (The lack of uniformity in the experimental techniques is a major, complicating factor throughout the document.)

(1) Macro-visual method, or gross observation of the lungs. According to the report, "these are subjective determinations and may vary with the observer."

It seems true that there is a possibility of subjective errors in this procedure, e.g. "false positives," in which the investigator sees a white spot on the lung which he believes to be a metastasis, but which turns out to be a non-malignancy; or "false negatives," in which a metastatic growth is actually present but is not seen by the observer.

An experimental technique was invested in the 1960s by Wexler of the National Cancer Institute which aids in the visual discovery of metastases. It consists of injecting an India Ink solution into the lungs of the mouse before the metastases are to be counted. The metastases then stand out as little white dots on a black background.

In 1973, when Sloan-Kettering collaborated with Dr. Martin's group on the first collaborative experiment the chosen method for determining metastases was this Wexler technique, which is essentially a macro-visual method. Earlier, in 1971 Dr. Martin's group apparently paid little attention to metastases and when they did, used gross observations plus occasional histology:

> "Because of the low incidence of metastatic spread [in CD8F1] histologic examination is generally omitted unless indicated by gross examination of the organs at death." (Stolfi, 1971)

CD8F1, in the autochthonous host, at least, has a very high incidence of metastases, up to 100 percent in Sugiura's and Martin's later experiments. This was apparently not known by Dr. Martin's group one year before the start of the amygdalin experiments. In 1974-75, after Sugiura's claims of positive effects of metastases were "leaked" from SKI, Martin became an advocate of a different method of finding metastases, the "bioassay." The present report is very pro-bioassay and anti-gross observation.

The reader is told that there are commonly large "subjective differences" in how different observers perceive the incidence of metastases when they use macro-visual observation and that these can create an "all-important difference." In fact, however, there are no experiments in this report in which different

macro-visual observations made the difference between evaluating an experiment as positive or negative.

The paper points to one case to support this argument, a case in which Dr. Schmid of SKI found 80 percent metastases in the controls and 44 percent in the treated, while Sugiura found 100 percent metastases in the controls and 38 percent in the treated.

The report brings out only one aspect of this, namely that there was a difference between the two observers. But it ignores the main aspect: that by the rules of statistics both observers saw essentially the same thing, viz.,that amygdalin significantly inhibits metastases. The report also ignores the fact that the histological (microscopic) examination supported both Schmid and Sugiura's observations. The microscopist in this case saw 80 vs. 31 percent, a figure roughly midway between the two macro-visual observations.

The report also passes over in silence the complete agreeement between the visual observations of two observers noted elsewhere in the paper. Schmid reports that "the investigator and an assistant agreed in their independent evaluations of the lungs grossly for metastases."

In the next to the last experiment, the collaborative "blind" experiment, the paper also claims that there was very good agreement between the two groups of observers in detecting metastases by the macro-visual method.

Judging from this paper, then, we must conclude that visual observation is not the totally subjective procedure the main authors of the report contend it is. This is not to say, however, that macro-visual examination alone is a thoroughly convincing and objective method. For this reason, Sugiura always used some other method as well, as did Martin. But in most of their experiments, Schmid and Stockert used only macro-visual observations. Any argument which SKI musters against this method should properly apply not to Sugiura (against whom they seem intended) but rather against Schmid and Stockert, who claim negative results with amygdalin.

The Microscopic Method

The paper raises some major criticism of the microscopic technique of finding metastases, as well. It claims that since "only portions of the lungs are observed microscopically...the choice of those portions sent for microscopic assay is, like the macro-visual method, a subjective decision."

Our understanding of the microscopic procedure is as follows: the scientist removes the lungs and has one lung preserved in a paraffin block. This lung is then sent to a pathologist, who preparing a slide on the microtome, stains and examines it. A determination is then made as to whether or not malignancy is present.

A potential source of error in this procedure is the possibility that the pathologist will miss a small, single metastases, "for only one slide is examined from the paraffin block of one lobe of the lungs. Thus an area of metastasis may be missed." (The report does not mention the possibility of doing "serial sections" of the lungs which are more accurate but also more expensive.)

The report claims that a "large subjective element is present" in the microscopic technique as well as the macro-visual method.(emphasis added) There is undoubtedly a subjective element here; but how large is it?

First, we must point out that a combination of macro-visual and microscopic determinations are the only kinds of assays used at Memorial Hospital

in the diagnosis of surgical patients. The implication of the SKI report would seem to be that many cancer patients have been tragically misdiagnosed, if these methods are in fact so unreliable. The possibility exists for doing "bioassays" instead of such "subjective" method, e.g. in the hamster's cheek pouch or the nude mouse, in the diagnosis of human patients. But few surgeons or pathologists accept the need for this, or the idea that their tried and true methods are entirely subjective.

In the same month as the press conference, in fact, four Memorial Hospital doctors published a paper on metastases in human breast cancer which relied entirely on "subjective" histological examinations (Attiyeh, 1977). We wonder what techniques Dr. Martin relies on in his surgical practice.

In the more relevant field of animal research the combination of macro-visual and microscopic examinations are still the standard methods of detecting metastases in rodents, and will probably remain so for some time to come. A review of standard cancer research journals over the last few years reveals that most scientists are still using the methods polemicized against in the SKI report, although a few alternate methods are being explored.

The "bioassay" method championed by Dr. Martin and his group has not yet been adopted by many researchers. In fact, according to references in Citation Index (a standard bibliographic tool) no group other than his own, as of this writing, has published papers on experiments employing his "bioassay" method.

Between January, 1976 and February, 1977, however, there were eight articles dealing with the question of metastases in rodents in the journal Cancer Research. The one paper by Dr. Martin's group does use the bioassay technique. All seven others use the basic macro-visual and/or microscopic examination similar to that employed by Sugiura.

Thus, Harada of Shiongi, Japan uses "light microscope"; Yuhas and Ullrich of Oak Ridge use "gross and microscopic examination"; Liotta of Cleveland uses a macroscopic technique called "transillumination"; Franks of the Imperial Cancer Research Fund, England, employs "light microscopy... routine histological sections"; Dubois and Serrou of France use a "binocular magnifying glass (x80); Carmel and Brown of Stanford University use the macrovisual "counting of all surface lung colonies"; and Poggi of Italy uses macrovisual technique and weighing of lungs to determine presence of metastases-- only tumor cells in the bloodstream are determined through a bioassay technique.

It seems significant that even after the completion of the SKI amygdalin experiments, four well-known researchers at Memorial Sloan-Kettering itself, including leading neurologists and pathologists, published a paper on "Metastatic Tumor of the Brain: Development of an Experimental Model" which uses techniques virtually identical to those used by Sugiura (Annals of Neurology, July, 1977).

After gross, macrovisual observation of the animal,

> "Histological sections were prepared from each block of tissue and stained with hematoxylin and eosin. In several animals, histological sections were prepared from the vertebral column, spinal cord, lung and liver."

The microscope was considered the ultimate arbiter of whether or not a

tumor was actually present:

> "None of these rats had macroscopically evident tumor in the brain parenchyma. However, on microscopical examination 4 brains revealed multifocal tumor deposits in small clusters of loosely organized cells."

Not only was the bioassay method not used, but there is no mention of it in the paper on brain metastases, nor any suggestion that the microscopic method is unsatisfactory or even controversial.

Sugiura's Methods Misrepresented

The SKI report gives the impression that Sugiura used mainly the macrovisual method of looking for metastases, and did not generally confirm his visual impressions with more objective means.

The first sentence of the report states, "Sugiura noted by macrovisual observation with some histology..." (emphasis added).

At the Laetrile press conference, Dr. Martin told reporters:

"Dr. Sugiura's results were based on visual observations as to whether or not there were metastases in the lung. This is proven, scientifically, to be subject to innocent error. You cannot look at this with the same objectivity as the microscope. And in the findings that were done where the visual observations were compared with the microscope, there was a discrepancy."

And so, we must ask, did Sugiura check his gross findings with the microscope? The answer is that he did. Sugiura's data, in the report itself, always shows in the right hand margin both the gross examination and the microscopic examination for each and every CD8F1 mouse-- almost 150-- which he tested over a three year period.

The only exceptions are mice that died prematurely and were not included in the final tally, or a number of cases in which Sugiura was asked to try the bioassay technique. The same is true of Sugiura's "Prophylaxis Experiment" and his "Swiss Albino" experiment. In fact, after sixty years at Memorial Sloan-Kettering, having histological examinations performed for him at Memorial's Pathology Department must be second nature.

Sugiura's dignified response to Martin's unfounded assertion at the press conference was, "My gross observation of lung metastases was checked by microscopic examination. That's all." Then he added, "And they checked each other very nicely."

In general that is true, although we did note some discrepancies in the Swiss Albino experiment (to be discussed below).

The Bioassay

The SKI report suggests that bioassay is widely acknowledged to be the only reliable method for finding metastases. At the present time, in most systems, it is in fact used as a supplement to macrovisual and histological techniques all over the world-- if it is used at all.

In the bioassay technique, as described by Martin's group, "all of the lungs of each animal are shredded (by scissors) and injected subcutaneously into two male CD8F1 mice....If a tumor subsequently arises at an injection site, it indicates that cancer cells (at least 10^5 cells) were present in the lungs."

At the press conference, Martin stated, "Most importantly, the most important test is by bioassay....That's where you take the lungs and you put them in another animal and the animal grows a tumor if there are cancer cells in the lung. There, consistently, laetrile always was negative. And that's as objective as you possibly can be."

We believe there are a number of serious difficulties with this statement.

First of all, Martin fails to distinguish between metastases and "micrometastases," very small or microscopic colonies of cancer cells. (Stock, with lawyer-like precision, did make such a distinction at the conference). It seems reasonable to accept Martin's assertion that a subsequent tumor in the bioassay indicates that at least 10^5 cancer cells (100,000) were present in the original mouse's lung. (Others who have worked with different bioassay systems, such as DeWys, found that as few as ten cancer cells could sometimes initiate a tumor.)

But a visual metastasis is made up of many millions, even billions, of cancer cells. Thus, even if a tumor does eventually grow in the bioassay, does that indicate that a chemotherapeutic agent had no effect in the original mouse? Not necessarily.

There is a difference, after all, between a single grasshopper and a plague of locusts, and there is also a difference between a nest of ten or even 100,000 cancer cells and a visible tumor made up of many millions of cells. What if a chemical controlled the growth of tumors, kept the metastases small and microscopic? Would that kind of anti-cancer activity show up in the bioassay system? It would seem unlikely.

The most serious objection to the bioassay as practiced by Martin's group was raised by Sugiura himself at the press conference. Unfortunately, few heard him and fewer still understood him.

"Gross examination of metastases," he said, "is very difficult. Therefore everybody should make microscopic examination of the lungs...I did [bioassay]. Bioassay by myself agreed very nicely with my gross observation. But remember, bioassay- you need good experience. Because sometimes tumor keep on growing for two, three or [even] eight weeks. That's adenocarcinoma [the kind of breast cancer found in CD8F1 mice, ed.]

"But when tumor sometimes grow for a couple of weeks and then start to regress, that's not adenocarcinoma. Of course, then you need examination by microscope. If you look under the microscope, that's not adenocarcinoma."

At this point, Sugiura was interrupted by Martin who said, "If you'll read the paper...I think you'll be better able to understand what I don't think was well expressed by Dr. Sugiura here in-- ehr-- English.

"The sentences there will explain some of the things you just think you heard, but I don't think you heard it right. It's all there in black and white...if you take the trouble to read the paper. An unequivocal, biological report to you that Laetrile is without biological activity."

Sugiura's point was an important one, crucial, we feel, to this entire question. If a mouse is injected subcutaneously with a mass of lung cells he says there will frequently be an inflammatory reaction. A lump will form and an inexperienced person will "read" this reaction as a tumor and record a positive score on the bioassay. The mouse will then be sacrificed, even after

only two or three weeks.

When an experienced scientist like Sugiura performs the bioassay he allows the mouse to live for at least eight or nine weeks. When he does so, many of the seemingly "positive" tumors, he says, regress and disappear-- something which adenocarcinoma normally does not do.

Sugiura said at the press conference that when he examined some of these "regressing" tumors by microscopic examination he found that they were in fact nothing but inflammatory reactions! This histological examination Martin does not do; yet Martin always claims a very high rate of "positives" in his bioassay. Unless the bioassay is accompanied by a microscopic examination it would not seem to be the "totally objective" method the SKI report claims. Also, one must have great patience to watch the bioassayed mice for two months after the nominal end of the experiment.

If Martin has, in fact, been observing inflammatory reactions and calling them "metastases" this calls into doubt all of the experiments since 1973 which the Catholic Medical Center group performed.

It is also simply not true that Sugiura did not do bioassays, or that all bioassay tests proved Laetrile negative, as Martin claimed at the press conference.

Sugiura performed bioassays in experiments #4 through #6 of the CD8F1 treatment series.

In experiment #4, eleven out of twelve bioassays confirmed Sugiura's macrovisual observations.

In experiment #5, ten out of twelve confirmed his visual observations.

In experiment #6, five out of eight confirmed his visual observations.

Overall, 26 out of 30 bioassays (87%) confirmed Sugiura's visual observations-- they agreed "very nicely" as he said.

On the whole, then, we must conclude that Sugiura's methods of detecting metastases are acceptable and routine methods; that when he used the bioassay method himself it confirmed his visual and microscopic findings; and that Sugiura has raised important questions about the Catholic Medical Center's method of performing the bioassay procedure-- questions which the SKI report fails to answer or even to raise.

Sugiura's CD8F1 Treatment Experiments

Almost all of Sugiura's experiments follow the same pattern. Each mouse in each experiment is listed, and the initial and final size of all its tumors are given in centimeters. The averages of these are then given. The duration of the experiment (in days) is given for each mouse and the average given. There is an observation on tumor growth. This includes a notation on whether all, or some, of the mouse's tumors continued to grow, or whether they stopped, and if so, for how many days. Finally, there is a notation on the number of lung metastases, both by gross examination and by microscopic examination. In experiments #4-6 some of the microscopic determinations are replaced by bioassays.

In Sugiura's six treatment experiments with CD8F1 mice, he noted "an overall average of 21 percent of mice with lung metastases when treated with 1000-2000 mg/k/day of amygdalin compared with 90 percent of the control mice.

"Temporary inhibition of tumor growth...was observed in a number of instances....All mice with large tumors appeared in better health in the treated group as compared with similar controls."

Although he considers this inhibition of metastases to be an important finding, with possible implications for therapy in humans, he points out that, "In none of the six experiments was there a significant difference in the duration of life of the treatment mice with respect to their controls."

As he stated on another occasion, "Laetrile is not a cure for cancer but a good palliative drug."

The overall impression left by Sugiura's CD8F1 treatment studies is this: they appear to be honest and competent reports, based on very copious and detailed notes over a three year period. Correspondence between macrovisual, microscopic and bioassay methods are generally very good.

The studies would have undoubtedly been strengthened had there been more mice in the experiments, as well as more bioassays. But this was limited by factors beyond Sugiura's control: a lack of CD8F1 mice, which are supplied by Dr. Martin under a National Cancer Institute contract.

Sugiura began his experiments with CD8F1 mice in 1972. By early 1975 he could have published his results. By this time, however, news of his positive results had already "leaked" from SKI and the administration was adverse to letting him publish these results without first getting "independent confirmation" of them from other laboratories.

This led to the amygdalin experiments by Dr. Elizabeth Stockert and Dr. Franz Schmid of Sloan-Kettering. As we show below, Dr. Stockert and her associates performed a number of positive experiments <u>not</u> reported in the SKI official report. Two "negative" experiments are included in the report, however.

First Stockert Experiment

Stockert's "Trial of Amygdalin against CD8F1 Spontaneous Mammary Tumors" was begun in January, 1975. It included 13 control animals, given saline injections, and ten amygdalin-treated animals.

Because of the paucity of the data for this and the following experiment-- together they occupy less than two pages of a 90 page report-- it is difficult to critically evaluate them.

For instance, there is no way of telling which mice were sacrificed and which lived out their term. Since the time at which an animal is sacrificed is often governed by subjective factors on the part of the researcher, figures on "survival days after 1st injection" have little objective meaning.

Stockert gives "initial host weight in grams" but does not give host (i.e. mouse) weight at death. It is therefore impossible to see what effect, if any, the treatment had on the animals' weight.

She gives tumor diameter (in centimeters) at death, but does not give tumor diameter at the start of the experiment. The text says only that "mice with tumors approximately 1 cm average diameter were used." This column is therefore also difficult to evaluate.

Stockert gives the number and proportion of mice with lung metastases at the end of the experiment. But these figures were arrived at solely by the macrovisual method-- that same "purely subjective" method that is criticized in the introduction to the report itself. This is curious, since like Sugiura, Stockert had the opportunity of utilizing Memorial Hospital's

Pathology Department but, according to Medical World News, she and an unnamed colleague "decided against a histologic examination because visible results were so clear-cut."

Since the saline controls have 22 percent more metastases than the treated animals, we wonder exactly what was so clear-cut.

In the first experiment, five of the control mice developed second or third primary tumors, while only two of the treated did. This is not commented on.

The report states that "mice dying from amygdalin injections were excluded" but there is no mention of how many mice died from such injections nor what condition (number of metastases, etc.) these mice were in at death.* A large number of "injection deaths" could have altered the significance of these experiments.

The first experiment was ruled invalid at the time it was performed because Stockert failed to make the experimental protocols conform to those of Sugiura's CD8F1 treatment experiments. According to the current SKI report, "Experimental conditions were the same as in Dr. Sugiura's experiments except in the first of the two experiments the mice received a different diet."

This is not what was said two years earlier. According to Medical World News, whose reporter interviewed Dr. Stockert in 1975, in the second experiment "she even altered the daily light cycle in the animal room to conform with [Sugiura's] original conditions and furnished the cages with the same litter shavings."

The implication is clearly that in the first experiment the shavings, food and light cycle were different than Sugiura's. The last point is especially important since light controls the "circadian cycles" which can sometimes have a dramatic effect on the response to chemotherapy in experimental animals.

On the whole, in the first experiment Stockert violated one of the first rules of a good scientific experiment attempting duplication of another researcher's work: keeping all parameters identical, including seemingly trivial factors which may influence the outcome of the test.

Stockert's Second Experiment

Stockert's second experiment is almost as difficult to evaluate as the first.

In this experiment, begun in April, 1975, 15 controls and 17 amygdalin-treated animals were used. The same questions must be raised about the lack of meaningful data. To find metastases in this experiment, Stockert again used only the macrovisual technique. She saw 67 percent metastases in the controls and 65 percent in the treated.

This time she saw new tumors in seven of the treated animals, as opposed to three in the controls. Once again, this is not commented upon.

In general, Stockert's experiments seem marked by insufficiency of data and a vagueness of experimental design. She did not succeed in accurately or faithfully reproducing Sugiura's experiments, or in reporting results in a way that would make them easily comparable to Sugiura's.

---

* Amygdalin accidentally injected into the intestines of an animal can cause death due to the breakdown of cyanide-bearing molecules by bacteria.

First Schmid Experiment

On April 22, 1975, Dr. Franz Schmid, a research veterinarian working under Dr. Stock, began a series of three experiments with amygdalin in CD8F1 mice.

In the first experiment's summary of data, Schmid gives the "mouse weight" (presumably at the start of the experiment) but does not give the mouse weight at the end of the experiment. Obviously, this tells nothing about the effect of amygdalin on the weight of the animals.

The second column gives "survival days." The average survival time is higher in the amygdalin-treated animals than in the controls, 53 days vs. 40 days. This difference is not mentioned in the report, nor are we told if this is statistically significant.

The third column gives "tumor diameter at death" (averages) but not at the start of the experiment. It is therefore also impossible to evaluate the effect of amygdalin on tumor diameter in this experiment.

The number of mice with metastases is given as 58 percent in the controls and 70 percent in the experimental category. We are not told if this is significant. Here, as in Stockert's experiments, only the macrovisual technique is used to determine metastases-- no histology or bioassay.

Second Schmid Experiment

The second Schmid experiment cannot be considered a serious attempt to duplicate Sugiura's results, although it may have some relevance to the overall question of Laetrile's efficacy.

As we have seen, Sugiura typically injected 2000 mg/kg/day of amygdalin into each mouse. Schmid, however, injected only 40 mg/kg/day, or one-fiftieth Sugiura's normal dosage. The reason for this is that 40 mg/kg/day corresponds to the dosage typically being given to human cancer patients at "Laetrile clinics" in Mexico and Germany.

There is certainly no objection to performing such an experiment. There seems to be an attempt at obfuscation, however, when this is included as one of the "negative" experiments refuting Sugiura's work.

Two provocative facts emerged from this experiment.

The amygdalin-treated animals lived about 50 percent longer (41 days for the controls vs. 63 days for the treated). And the treated animals had more metastases (88 percent for the treated vs. 44 percent for the controls). There has been some speculation that these two findings are related-- that the treated animals had more metastases because they lived longer. The report, however, does not comment on these possibly important findings.

As in the previous experiment, only the "subjective" macrovisual technique is used to find metastases.

Third Schmid Experiment

This was actually a collaborative experiment between Schmid and Sugiura.

It was designed as a "tie-breaker" experiment, after Schmid's previous two experiments had failed to confirm Sugiura's, and was performed in the fall of 1975 at the Walker Laboratory of SKI, Rye, New York.

Each of the scientists made his own independent evaluation of the lungs of the animals for metastases by the macrovisual method, which was then followed up by a microscopic report from the Pathology Department, Memorial Hospital.

The results unequivocally confirmed Sugiura's contention that amygdalin inhibits the spread of metastases in CD8F1 mice.

Schmid found by macrovisual observation 80 percent metastases in the controls and 44 percent in the treated. Sugiura found 100 percent metastases in the controls and 38 percent in the treated. The Pathology Department found 80 percent metastases in the controls and 31 percent in the treated. N.B., this was the first experiment by Schmid which utilized microscopic observation, in addition to macrovisual.

All three sets of figures are statistically significant. This is never clearly stated in the report. Instead, the following statement is made:

> "There is concordance in the results of the first two experiments with respect to % of mice with lung metastases even though the dosages given were quite different. For the same criteria the first and third experiments at the same dosage present opposing results; however, in the third experiment there is some discrepancy between individuals (F.S. and K.S.) in evaluation of the number of mice with lung metastases."

Anyone who can figure out from this explanation that Schmid had in fact confirmed Sugiura's findings is remarkably perceptive!

The "General Discussion" of the SKI Report also takes up the question of Schmid's third experiment: "One of the three experiments by Schmid did give results in the same direction as Sugiura's early observations but was near the borderline of acceptable statistical significance, while evaluation by Sugiura was more highly significant."

Simply "in the same direction"? Certainly, the authors know that the reason the concept of "statistical significance" was developed was to rule out fortuitously positive results, such as can occur when the difference between two samples is so small that a "positive" finding might be the result of chance.

When statisticians say "statistically significant" they <u>mean</u> "statistically significant." To use a phrase like "near the borderline of acceptable statistical significance" is to undermine one of the basic ground rules of science. The authors are playing a dangerous game here.

Can we now go back and question all the results <u>they</u> reported in the past which were only "near the borderline" of statistical significance? For example, in 1971 Martin and his colleague Stolfi reported that tumor incidence in CD8F1 mice was "found to be statistically higher ($P = 0.05$) in the CD8F1 hybrids (70%) as compared with that in its maternal BALB/cfC3H strain (57%)."

The reader can see at a glance that these results are <u>less</u> significant than either Schmid or Sugiura's. Yet the entire value of the CD8F1 model is based on this "borderline" statistic!

Not only does the report fail to state clearly that Schmid confirmed Sugiura's findings but, on the contrary, after hedging somewhat it actually states in the concluding section that "All experiments of 3 independent observers [Stockert, Schmid and Martin, <u>ed.</u>]...have failed to confirm Sugiura's initial results."

When a reporter at the press conference asked Dr. Schmid about this discrepancy he became nonplussed and eventually handed the microphone to Robert A. Good, president and director of Sloan-Kettering:

Reporter: In fact, in your third test, did you in effect confirm Dr. Sugiura's findings?
Schmid: Yeah, in the third test, yes. But that is two to one.
Reporter: And in the second test, you used a very small dosage of amygdalin.
Schmid: Yeah, that's true.
Reporter: Okay, then why don't you state in the findings that one of your independent investigators confirmed Dr. Sugiura?
Good: (taking microphone from Schmid) Of course, we do say that in the findings. If you read the paper, we do bring forth every bit of evidence and we discuss it."

Prophylaxis Experiment of Sugiura

Sugiura also performed a prevention, or prophylaxis, experiment on CD8F1 mice over a period of two and a half years, injecting each mouse six days a week. "Daily examinations of the mice were made to detect the appearance of tumors and to determine the condition of each mouse." Only five animals died accidentally by injection of amygdalin into the intestines. As Dr. Stock himself generously admitted at the press conference, this experiment stands as a tribute to Sugiura's patience, skill and ingenuity.

Eighty-one percent of the control animals developed lung metastases and eighty-two percent tumors. "Of the treated group 17 percent developed lung metastases and 72 percent tumors."

Thus, while amygdalin did not have a significant effect on the percentage of mice developing primary tumors, it again appeared to dramatically inhibit the formation of metastases. This effect is quite similar to the effects seen in the six treatment experiments in CD8F1 mice.

Sugiura also found that there was a delay of over one month in the appearance of tumors in the treated group (411 days in the control and 449 days in the treated). SKI statistician Isabel Mountain did not consider this a statistically significant difference.

Once again, there is confusion over what method Sugiura used to detect metastases.

A glance at the data reveals that both macrovisual and microscopic examinations were performed in every case. According to the text "histological examination of lungs of control animals and amygdalin-treated animals for lung metastases revealed good agreement with that of the gross findings."

Yet the "General Discussion" of the paper has this to say about the "prophylaxis" experiment: "The difference in lung metastases was evaluated only by macrovisual observation."

This, of course, is completely untrue as can be seen by reference to the experimental data itself.

Effects Upon Cells

Upon microscopic examination of the animals' tumors, Sugiura noted:

"There were many mitotic figures among the control tumor cells while tumor cells of amygdalin-treated animals appeared more hemorrhagic, degenerated, and contained fewer mitotic figures."

The appearance of "mitotic figures" is generally a sign of active malignancy, while the presence of "hemorrhagic" (bleeding) and degenerated cells is a sign that something is killing those cells. This important observation simply falls by the wayside and is never mentioned again. Documents we have obtained and are now publishing (below) show that Sugiura made similar observations in the case of the lymph nodes of AKR leukemic mice, but these observations were omitted from the report.

Sugiura's Swiss Albino Experiment

In this experiment, Sugiura tested the effect of amygdalin on Swiss Albino mice, obtained as retired breeders from Taconic Farms, New York. His conclusion:

"There was no destruction of the tumors by amygdalin. Smaller tumors stopped growing temporarily in 24 percent of the controls and 52 percent of the AM [amygdalin, ed.] treated. In the control group 91 percent showed lung metastases by macrovisual observation while there were 22 percent in the AM treated mice. The general health and appearance of the AM treated mice with large tumors seemed to be better than corresponding controls."

The experiment shows that amygdalin inhibited the formation of metastases in Swiss Albino mice. There is a relatively large number of discrepancies, however, between Sugiura's macrovisual observations and the microscopic observations, in this experiment. By microscope, there are only 53 percent metastases in the controls vs. 27 percent in the amygdalin treated mice. This is probably still signficant, but not as impressive as the gross observations. We feel that Sugiura should have mentioned and accounted for this discrepancy in his discussion.

One possible explanation for this lack of concordance is that these false positives occur when less than five metastatic nodules are present in the lung. When only a few nodules are present it is quite possible for the technician preparing the slide to miss the "suspicious" area. Lungs with more metastases show a good concordance between macrovisual and microscopic observations (except one case, mouse #30).

The "General Discussion" of the report makes several misleading comments about this experiment, however. It claims that "the relative numbers of Swiss albino mice showing lung metastases in the treated and control groups has [sic] not been subjected to the challenge of independent confirmation."

As we show below, Sugiura's positive results with Swiss Albino mice were independently confirmed in Stockert's laboratory in late 1973, but these results were not included in the official SKI report.

Second, the report states that "the results must be looked at questionably, however, in the light of the paucity of information on metastases in Swiss mice, the lack of bioassays and in view of the lack of confirmation of Sugiura's metastasis studies in CD8F1 mice."

Swiss Albino is one of the best studied spontaneous tumor systems. Sugiura himself performed studies using this system as early as 1943, at the request of C.P. Rhoads, former director of Sloan-Kettering [Sugiura, 1947].

Since there was an internal control in the Swiss Albino amygdalin experiment it is hard to see what information on metastases is lacking.

The "lack of confirmation" in CD8F1 mice (ignoring for a moment Schmid's positive findings) is equally irrelevant. Is it not possible, after all, that amygdalin might work in one experimental system and not in another? Of course it is. This is the principal reason that putative anti-cancer agents are today tested against a "spectrum" of animal test models, work which Dr. Stock and Dr. Sugiura are justly famous for.

Nowhere does the report state in clear, unequivocal terms that Sugiura's Swiss Albino studies confirmed his findings originally made in CD8F1 mice that amygdalin inhibits metastases. But that is what happened.

Sugiura's Tests against Spontaneous Leukemia

Sugiura carried out a large number of experiments using mice with spontaneously developing leukemia, the well-known AKR strain. In the report, this extensive work is condensed into three brief pages which omit the most important and provocative findings which were made. We have obtained Sugiura's laboratory notes and memoranda on these experiments, some of which we reproduce below.

In general, we find that the anti-leukemic features which Sugiura noted in his memoranda have been eliminated from the discussion in the SKI report. Only negative aspects are subjected to comment.

Concerning the prophylaxis (prevention) experiments with AKR mice, we are simply told by the report, "In none was there prevention of the development of leukemia or significant increase in survival of the mice...." But, as we have already pointed out, the prevention of leukemia or significant increase in survival time in this particular mouse model is extremely rare--virtually unreported. For that reason, SKI scientists who work with this model, such as Dr. Robert Kassel or Dr. George Tarnowski of the Walker Laboratory regularly look at the reduction of size of the internal organs (e.g. thymuses, spleens or inguinal lymph nodes) as an index of anti-leukemic effect. A reduction of twenty percent or more is considered significant (Kassel, 1977).

Bearing this in mind, a glance at the first Sugiura AKR prevention experiment reveals that the average weight of the thymuses in the controls was 392 mg. while in the treated animals it was 92 mg., a better than 75 percent reduction.

The lymph nodes were less than half the weight in the treated animals; the spleens were also smaller, but not significantly so. Similar positive results were seen in the other prevention experiments, but not commented on in the Report. Here is how Sugiura summarized these important findings in his unpublished memo of March 25, 1975:

> "Results show that repeated intraperitoneal injections of 2000 mg/kg/day of amygdalin had a definite inhibitory effect on the development of leukemic thymuses...and leukemic lymph nodes, but no effect on treated spleens."

Other promising anti-leukemic effects which Sugiura noted in this memo include:

-- A delay in the appearance of leukemia in these mice. On 1/16/75 he noted that 60 percent of the controls had developed leukemia, while only 20 percent of the amygdalin-treated animals had.

-- Lymph nodes of the treated animals dying of leukemia showed "extensive necrosis" while those of the controls did not. (It is worth noting in passing that development of a "tumor necrosis factor" is one of the principal goals of several SKI laboratories.)

-- "Four mice in the amygdalin-treated group lived more than 100 days while only one mouse in the control group lived more than 100 days."

None of this appears in the SKI report. The report states only that "experiments on prevention and treatment of mouse leukemia were negative as far as benefits from amygdalin were concerned."

In the second AKR prevention experiment all of the mice, controls and treated, died sooner than in the first experiment. This difference appeared to alter the drug's action on lymph nodes, spleens and thymuses, Sugiura wrote. Nevertheless, he concluded that "amygdalin had a certain inhibitory action on the development of leukemia in mice." He reached the same conclusion in his third AKR prevention experiment (memo of July 25, 1975):

> "In the case of the amygdalin-treated animals the size of the reticuloendothelial organs were definitely smaller than those of controls- inguinal lymph nodes- moderate inhibition (55% inhibition), spleens- moderate inhibition (42% inhibition), and thymuses- slight inhibition (18% inhibition)."

He noted that "3 of the 10 amygdalin-treated animals or 30 percent lived much longer- twice the average survival time of the control animals. In the control group all of the 10 animals were dead at the end of 87 days."

None of this found its way into the final report.

AKR Treatment Experiments

Sugiura also performed five treatment experiments with AKR mice. The SKI report states "...there clearly was no benefit in the leukemia as seen in Table XIV."

This is <u>not</u> what Sugiura originally reported, nor is it what is actually shown by Table XIV.

In his unpublished memo on the first treatment experiment Sugiura states

> "Post mortem examination of the control animals showed enlargement of lymph nodes, spleens and thymuses in all cases. There were tumors of the mesentery in 3 animals. In the case of amygdalin-treated animals the size of spleens and thymuses of 3 animals were small. The size of inguinal lymph nodes in 3 animals which received from 14 to 25 injections of amygdalin was definitely smaller than that

of the control animals. This indicates that amygdalin had a certain inhibitory action on the development of leukemia in mice."

Sugiura reported similar findings in his other four treatment experiments. The amygdalin definitely appeared to be having a beneficial effect on these mice, although not a curative one. Sugiura concluded "amygdalin is not a cancer cure but a good palliative drug." (unpublished memo of July 25, 1975)

Mitomycin C Experiments

It is interesting to note in passing that Sugiura carried out experiments preliminary to testing amygdalin in conjunction with another agent, mitomycin C.

Amygdalin had shown least effect on the size of spleens in AKR mice. Sugiura had shown in 1951 that Mitomycin C, a now recognized anti-cancer agent, "had a complete inhibitory effect on Friend virus leukemia in Swiss albino mice-- no alteration of spleen from normal, non-leukemic animals." (unpublished memo of May 17, 1975) He therefore decided to "test a combination of amygdalin and mitomycin C on the spontaneous leukemia in AKR mice" in the first part of 1975.

First, however, he conducted preliminary studies to find out if mitomycin C would "inhibit the development of [enlarged] spleen in AKR mice." Mitomycin C turned out to reduce the size of the spleens and other organs somewhat, but to be prohibitively toxic. Sugiura's conclusion was that this recognized anti-cancer agent "has a slight but definite inhibitory effect on the growth of leukemia in AKR mice." He made the interesting observation that "the anti-leukemic action of mitomycin C is essentially the same as that of amygdalin," although amygdalin was non-toxic in this system (memo of July 18, 1975).

The combined amygdalin-Mitomycin C experiment was apparently never carried out because of mitomycin's toxicity.

Catholic Medical Center experiments

In the summer of 1973, Dr. Daniel S. Martin, a surgeon at the Catholic Medical Center, Queens and Brooklyn, New York, and his colleagues became active participants in the SKI amygdalin experiments.

Since that time, Dr. Martin has become one of the most outspoken opponents of Laetrile within the medical profession. He is a frequent debater on "talk shows", lectures on the dangers of "quackery" at scientific conferences and is the author of an anti-Laetrile pamphlet, "A Review of Amygdalin-Laetrile" (Martin, 1976).

Dr. Martin developed and breeds a colony of CD8F1 mice in Woodside, Queens. Although at the beginning of his involvement with the SKI experiment he was having difficulty funding his colony, since 1975 he has received a contract for one million dollars from the National Cancer Institute and "his" mouse, CD8F1, has been chosen by N.C.I. as one of four major animal tumor models in the national screening program for anti-cancer agents.

Martin's group's first involvement with amygdalin was a collaboration with Sugiura and George Tarnowski, also of SKI's Walker Laboratory, in a joint experiment.

The SKI report (of which Martin is second author) refers to Martin's experience with CD8F1 mice as "extensive and unique". Yet, oddly, in 1971 they published a paper in which they stated that there is a "low incidence of metastatic spread" in this animal (Stolfi, 1971). It wasn't until 1974, after Sugiura had performed his treatment experiments with CD8F1, that Martin recognized that there was in fact a high incidence of metastases in this animal, ranging up to 100 percent (Anderson, 1974)

Although there appear to be several distinct advantages to this hardy animal strain, it is noteworthy that few scientific groups around the country have adopted it for their own published studies.

First Collaborative Experiment

The "first collaborative experiment" failed to draw any valid conclusions about amygdalin and CD8F1 mice. In fact, like the first Stockert experiment, it was declared flawed and invalid at the time of its performance, but has been strangely "resurrected" for inclusion in the report.

The "first collaborative experiment" nevertheless was a blow to the credibility of Sugiura's more positive findings. It was apparently on the basis of this experiment that Dr. Good told science writer Barbara Culliton "We now have evidence on both sides of the fence" on Laetrile (Science, 1973).

The problems with the SKI-Catholic collaboration began even before the experiment itself. According to a letter in our possession, Dr. Stock originally requested 220 mice for this experiment, with ten more to follow for enzyme studies.

After six weeks, half of each group (treated and control) would be sacrificed and the other half would be allowed to live until they reached 90 days. Dr. Morton Schwartz, a MSKCC biochemist, would conduct enzyme studies of the mice at six weeks (letter of July 13, 1973, Chester Stock to Ruth Fugmann, Ph.D.).

This is not what happened. On July 24, 1973, Ruth Fugmann, Martin's colleague, responded to Stock that she had initiated the amygdalin experiment that morning and informed him that only 93 animals would be made available.

"For the present, we will have to limit the experimentation with amygdalin to this number, in that tumor-bearing animals must be used to satisfy other immediate obligations, and our available personnel is limited in this vacation season."

Instead of sacrificing the animals at six weeks and 90 days, they were sacrificed in two groups at three and a half weeks and five and a half weeks. Since most of the animals only had small tumors and few metastases, this was too early for any significant differences to appear between treated and untreated mice.

Why the animals were sacrificed prematurely we do not know.

The report states that "to use the carbon black technique on all mice, the experiment had been terminated too early for more metastases to appear." But there is nothing in Wexler's paper on the carbon black technique which statesmice have to be terminated very early to use it (Wexler, 1966).

According to the figures given in the report, there were 21 percent metastases in the controls after three and a half weeks and 42 percent in the treated. It states that there were 36 percent in both treated and control

after five and a half weeks.

The report simply states that "lung metastases [were] evaluated by the SKI group" but does not give the results of the microscopic examinations performed at MSKCC. This may be because there were even fewer metastases by microscope than by the macrovisual "Wexler" technique, according to documents in our possession.

To be specific, mice #68 and #71 in the treated group, sacrificed at three and a half weeks, were actually "false positives" by microscope. Mice #78 and #99 in the controls were also "false positives." Thus, the actual (histological) figures for the three and a half week experiment were:

2/19 metastases in controls, or 11 percent
6/19 metastases in treated, or 32 percent.

In the five and a half week segment of the experiment the actual (histological) figures were:

3/14 metastases in the controls, or 21 percent
2/11 metastases in the treated, or 18 percent.

The slide of mouse #13 was inexplicably lost in transit from the Catholic Medical Center to MSKCC, a fact which is not mentioned in the report. These histologically-determined figures, omitted from the report, make the "first collaborative" experiment look even less relevant than it does in the official version. The animals were all sacrificed too early for the report to be meaningful. Yet the authors of the report state "no significant difference was observed between the control and treated group with respect to % with lung metastases."

To admit that the experiment was bungled and then to go on to draw conclusions from it is simply an invalid way of arguing and is in basic violation of the code of the scientist.

Catholic Medical Center Independent Experiment

The Catholic Medical Center's independent experiment with amygdalin also appears flawed, especially if its purpose was to replicate Sugiura's methods and test his claims.

Unlike Sugiura, who gave amygdalin injections six days a week until the death of the animals, the CMC (Catholic Medical Center) group gave injections only until the 46th day of the experiment (40 successive injections, six times a week). Mice were then apparently left untreated, and sacrificed when tumors reached a size calculated to be four grams (this actually varied from 2.4 to 8.3 grams).

Not only is this in itself a significant departure from Sugiura's protocols, but this method could actually have resulted in a transient anti-cancer effect unrecorded in the data.

The method of counting metastases is also very unusual.

There are 24 mice in the control group. Only four of these showed metastases by macrovisual observation. Eleven mice showed metastases by bioassay. And so it is concluded that 15 out of 21 mice had metastases,

or 71 percent.

This is what is called the "combined bioassay and macro-visual observations" This is difficult to interpret. It is possible, for instance that, contrary to its own polemics, the CMC group has accepted macrovisual observations as fool-proof in four cases, and then only subjected the remainder to bioassay. In that case, there is a serious lack of internal consistence in the entire report on the value of macro-visual observations.

Another possibility is that the CMC group subjected all mice to macrovisual observation and then subjected all to bioassay. They then simply added the positives from macrovisual to the positives from bioassay and came up with a grand total. The problem with this method would be that some mice (as many as four) could be counted twice in computing the final figure.

It is hard to believe that the CMC group would make such an elementary error in statistics; we must therefore conclude that they simply failed to subject a number of mice to bioassay,for whatever reason. This ignores the possibility of "false positives," i.e. mice which appear under macro-visual observation to have metastases but turn out in the bioassay or microscopic tests to be cancer-free. This happened in both "blind" experiments.

There is therefore a real possibility that these figures on metastases are inflated. If so, it would appear that this experiment had been terminated too early, like the "first collaborative" experiment had been, before significant differences between the two groups had time to appear.

Second Cooperative Experiment

According to the report, both the CMC and Sugiura kept records on weights and diameters of tumors, and both made macrovisual observations of the lung metastases in this experiment.

Yet the only data given in the report is the "CMC summary of results." None of Sugiura's results are given. The data as given is thus incomplete.

"Average Tumor Size at Initiation of the Experiment" is given, but not at the end of the experiment. As in the Stockert experiment, one end point gives the reader little to go on.

Martin himself is an exponent of the theory that chemotherapy works best when the tumor is small, and that in general the larger the primary tumor, the more metastases.(Anderson,1974) Yet the initial size of the tumor in this experiment ranges up to 650 mgs., a fairly large tumor. Because the data on all the mice is averaged (and not given per mouse, as in Sugiura's studies) it is impossible to tell if amygdalin had a greater effect on the smaller tumors than on the larger ones (as in Sugiura's CD8F1 experiments).

Here again we find the peculiar CMC method counting metastases. Interestingly, by macrovisual observation the control group had twice as many metastases as the treated group-- which may be statistically significant. (We aren't told). When the CMC group adds in the bioassay numbers, as well as two amygdalin-treated mice "which died without macrovisual evidence of metastases" but showed such under the microscope, the final figures are 82 percent metastases in the controls and 69 percent in the treated. The sudden appearance of a microscope in the CMC experiment-- otherwise scorned-- prevents these figures from appearing pro-amygdalin. The report is able to conclude that "the difference in % of metastases is not statistically significant."

(One would certainly like to see Sugiura's figures for the gross observations. We are also told, in a footnote, that "all animals that died without macro-visual evidence of metastases, as well as all animals with such metastases, were examined microscopically." Where is the histology report on these animals?)

Both histology and bioassay?

The report states that both "histological sections" and bioassays were prepared on the same animals in this experiment. This is puzzling.

The histological examination requires that a section or preferably one entire lung be prepared in paraffin, stained and then microtomed for slide preparation. The bioassay, on the other hand, to be complete requires that both lungs be shredded and injected subcutaneously into two male CD8F1 mice (this is spelled out in the "Materials and Methods" section of the report).

It is logically impossible to perform both types of tests on the same mouse, as the tests are described in the report. In Sugiura's experiments, for example, whenever he performs a bioassay he necessarily omits the histological exam.

It appears therefore that the experimenters "cut corners" here: they removed a portion of the lungs for histological examination: by doing so they may have removed the malignant portion from the bioassay, thus opening up the possibility of some "false negatives" in the bioassay.

This, in itself, is probably no big matter and may have been required by the exigencies of the experiment. What disturbs us is the way in which they apparently try to "slip this past" the reader, failing to call attention to a contradiction in their procedures.

First "blind" experiment

The controversy engendered by the growing use of Laetrile, and Sugiura's apparently positive tests, as well as Schmid's apparent confirmation of those results in 1975, created the conditions for a "blind" experiment.

The first "blind" test was therefore performed at the Catholic Medical Center in the summer of 1976. Sugiura travelled to Queens, N.Y. and weighed the mice, measured their tumors and observed their lungs, when sacrificed, for metastases. Sugiura was not to know which mice received the amygdalin and which the saline control.

Bioassays were performed on all sacrificed mice, but neither Sugiura nor anyone else at SKI took part in this aspect of the experiment. They therefore were performed according to the method of Martin, in which (according to Sugiura) the mice are sacrificed immediately after exhibiting any "growth" at the point of injection, including possibility inflammatory, non-malignant "growths."

This experiment ended in controversy, with SKI declaring the "blindness" lost. As the report itself states, the experiment "suffered a loss of assurance of blindness because of some early deaths in the AM treated group as a result of some of the injections..."

This is confusing for it is impossible to see on the basis of accidental deaths how Sugiura could have even hoped to have guessed which mice were treated and which were controls (such deaths could occur from a number of causes, includi the accidental injection of amygdalin into the intestines).

In fact, Sugiura believed he knew which mice were treated and which were controls on the basis of objective, anti-cancer effects. He said so at the time in a number of memos, which he gave to Drs. Stock and Old, the first after only four weeks of the experiment.

The 70 mice in this experiment were divided into 14 groups, five mice to a cage. Seven of these groups (cages) received amygdalin and seven received saline solution.

Sugiura's surmise was, quite simply, that the first seven cages (35 mice) were the control animals and the second seven cages (35 mice) were the treated animals. He noted the following effects in the two groups (as reported in his eight week report, a copy of which we have obtained):

	"controls" (1-35)	"treated" (36-72)
Number of animals living at end of eight weeks	1/35	4/34
Number of tumors stopped growing	8/35 or 23%	21/34 or 62%
Number of new tumors developed	8/35 or 23%	2/34 or 6%
Lung metastases (macro-visual)	21/35 or 62%	13/34 or 43%

It was shortly after receiving this memo from Sugiura that Stock declared the blindness of the experiment compromised. It is hard to see why.

The report declares that "early deaths in the AM treated group" caused a "loss of assurance of blindness". But, according to Sugiura's figures, more mice were dying in the control than the treated categories, and he attached no importance to accidental deaths in his memos.

The really confusing thing is that the experiment, as presented in the SKI report, bears little resemblance to Sugiura's memo. In the official report, the mice are not in two discreet groups of 35 each, but the cages are scrambled.

If that is the way the mice were arranged, then Sugiura obviously was completely in the wrong in his assessment. One would think that SKI would have, at that point, announced to the world that Sugiura had "flunked" this crucial test, and informed him of the same.

Instead, Dr. Stock abruptly cancelled the experiment (as a "blind" test at least) and scheduled another "blind" test to take place at SKI itself. He never apparently responded to Sugiura's memos claiming "victory". He left Sugiura thinking he had in fact guessed correctly.

Blind Experiment at SKI

The final experiment with amygdalin in this report took place at SKI's Walker Laboratory in the fall of 1976. The purpose of this experiment was to redo the previous experiment at the Catholic Medical Center, in which the blindness was supposedly compromised.

"It was decided to conduct a further blind test in which there would be added safeguards against a compromise of blindness in the conduct of the experiment," in the words of the report.

SKI's basic plan was to so randomize the mice in the various cages that Sugiura could not possibly guess which half was experimental and which half control. Treated mice and control mice would be caged together, distinguished only by earmarks, punched according to the international numbering code. Only Stock and a technician in Tarnowski's laboratory who was to do the actual injecting would have access to the code.

In principle, this is a fair and rigorous way of conducting a blind experiment, seemingly foolproof. Nevertheless, Sugiura objected to this. As he has said many times, he felt that there was too great a danger of the treated animals accidentally being given inert saline solution and of the controls being injected with amygdalin. This error could seriously compromise the results.

Error could also occur through the accidental tearing of the punched earmarks, through biting, or through the ingestion of amygdalin-laden feces by control mice (mice are coprophagic). We know of no studies showing whether or not amygdalin survives digestion in the mouse, however.)

Sugiura asked his colleagues to house the amygdalin-treated and the control mice in separate cages and then to randomize the cages. He felt that this would provide adequate "blindness" without the drawbacks of randomization within cages.

Sugiura was overruled and the experiment proceeded as planned. The results, as recorded by Sugiura and other observers, showed no significant difference in metastases between the amygdalin-treated and the control animals.

In studying the data, Sugiura noticed that there was initial tumor stoppage of tumor growth in 40 percent of the control animals, and in 27 percent of the treated animals. This seemed like an unusually high percentage of spontaneous stoppages in the first weeks.

"We people in chemotherapy," Sugiura told the press conference, "use saline solution because it does not stop tumor growth. Now this happens." Sugiura believes that his fears had come true and that the control animals were inadvertently injected with amygdalin, and vice versa. Although the SKI blind experiment as a whole appears well-designed and amply recorded this must be considered seriously.

The report, taking a peculiar adversary relationship towards one of its own authors, claims that Sugiura "in at least one of his experiments observed nearly as high a percent of growth stoppage in controls."

Like so many of the arguments in the paper, this one is tendentious and misleading. Sugiura performed six treatment experiments with CD8F1. The number of spontaneous stoppages in controls was five out of sixty or only 8.3 percent (for one week in every case). In only one experiment, number six, did he see three out of ten initial tumor stoppages.

To claim on the basis of this that Sugiura saw anything close to 40 percent for up to five weeks is demonstrably false.

The SKI blind experiment is certainly the strongest piece of evidence

that amygdalin does not work. We cannot accept it as an error-free study, however, because of serious questions about the unexplained tumor stoppages.

Nor is it certain, even if this test were valid, that a failure in one experiment rules out the validity of other tests. Variables in the material used, method of its preparation, litter of mice, site of injection and a number of other factors could lead to disparate results.

The extreme carefulness of Sugiura's experiments, contrasted with the general sloppiness of most of the other tests, is a noteworthy feature of this report. The full documentation of his study contrasts favorably with the paucity of his detractors. The often repeated effort to conceal problems and contradictions does not speak well for the anti-Laetrile portion of the experiment.

We therefore remain unconvinced that Sugiura was mistaken in his general conclusions, or that amygdalin has been proven worthless. Perhaps the greatest tribute paid to his skill is related in the foreward to the "Memorial Edition" of The Publications of Dr. Kanematsu Sugiura (1965). The words may some day seem prophetic:

> "Few, if any, names in cancer research are as widely known as Kanematsu Sugiura's...Possibly the high regard in which his work is held is best characterized by a comment made to me by a visiting investigator in cancer research from Russia. He said, 'When Dr. Sugiura publishes, we know we don't have to repeat the study for we would obtain the same results he has reported.'"

The author of this tribute was Chester Stock.

Missing Experiments

According to the SKI report, "this report presents data from all anti-tumor experiments with amygdalin in these spontaneous tumor systems."

This idea, that the present study is complete and thorough, was amplified and reiterated at the press conference by MSKCC leaders:

"I think that the institution [MSKCC] is providing you with just about every shred of information that we have, and as much candor as we can collectively summon," said Dr. Lewis Thomas, president of MSKCC.

"If you read the paper, we bring forth every bit of evidence and we discuss it," Dr. Good added.

This is simply not true. A number of experiments with amygdalin (and related compounds) were performed at Sloan-Kettering between 1972 and 1976 which were not included in this official SKI Report. We have obtained solid information on a number of these, including in some cases laboratory notes, which we publish below.

Swiss Albino Experiment

Between December, 1973 and January, 1974 workers in the laboratory of Dr. Elizabeth Stockert carried out an experiment with amygdalin in Swiss Albino mice.

The results were positive. According to a handwritten memo from a technician in that laboratory, Ms. Shelly Jacob, to Dr. Lloyd Old, vice president of SKI:

> "Those mice in the group receiving the highest dose of amygdalin (100 mg/ml) not only live longer but remain healthier and more active than do mice receiving lower doses of the drug, no treatment or plain saline injections....Tumor growth in terms of days is retarded... a higher incidence of lung metastases in the control mice, the 50 mg and 10 mg when compared with the 100 mg treated group." (memo of December 20, 1973).

This memo is supported by four pages of laboratory notes, which show effects remarkably similar to those noted by Sugiura. (N.B. the dosage given is actually more than 50 percent higher than Sugiura's, since 100 mg/ml equals approximately 3333/mg/kg/day.)

Efforts will no doubt be made to nit-pick at the details of the experiment. The fact remains that it took place, it duplicated the essence of Sugiura's conclusions, and it was not reported. No amount of sophistry will be able to obscure that fact.

Combination amygdalin-enzyme experiments

Experiments utilizing a combination of amygdalin and the enzyme Bromelain were carried out in the laboratory of Dr. Elizabeth Stockert by Dr. Stockert, Ms. Jacobs (presently a technician at the Stanford University Medical Center, Department of Medicine) and Dr. Lloyd Schloen. Dr. Schloen is currently employed in the Office of Grants and Contracts, MSKCC.

In the first experiment, four animals with Meth A tumors were give 10 mg/0.5 ml of amygdalin intraperitoneally five days a week. In addition, they were given Bromelain (Food Grade- Rorer) 0.2 mg/0.2 ml intratumorally

at the same time. The results on these transplanted tumors was quite impressive: all tumors completely regressed, leaving only a dry, flat scab.

Date	9/29	10/1	10/8	10/15	10/22	10/29
Average Tumor Diameter (mm)	9.1	5.8	1.5	0	0	0

Dr. Stockert attempted to duplicate this experiment of Schloen and Jacobs in November-December, 1973. The dosage was the same as in the previous experiment and eight animals were tested:

Date	11/12	11/15	11/19	11/23	11/26	11/30
Average Tumor Diameter (mm)	8.4	8.4	5.6	6.0	5.5	8.3

12/3	12/10	12/17
9.1	9.1	-

In this experiment, although most of the tumors grew, there were two complete regressions of the type observed in the Schloen-Jacobs experiment above.

Controls: In this, and the previous experiment, controls with Meth A tumors having an average diameter above 6.0 mm never spontaneously regressed or ceased growth.

These experiments are noteworthy for the light they shed on amygdalin, as well as the combined use of amygdalin and the enzyme Bromelain. While Stockert's experiment only partially confirmed that of Schloen and Jacobs, it is unfortunate that the entire line of research was suddenly dropped at the beginning of 1974.

Unreported Walker rat experiment

Dr. Stockert also carried out an experiment in Walker 30B12 transplantable tumors in rats in December, 1973. No positive results were achieved in this system. In fact, according to laboratory notes on the experiment, the rats receiving 500 mg/kg/day amygdalin treatments appeared to die sooner than the controls (16.2 day survival vs. 22.14 day survival).

Experiments in cats and dogs

From 1972 on, Dr. William Hardy carried out sporadic experiments with amygdalin in domestic cats and dogs. According to an SKI memo a "Meeting on Laetrile" took place in the Therapy Field on December 1, 1972, with Drs. Stock, Old etc. in attendance.

The memo reads, "Dr. Hardy will continue oral therapy in 'out-patient'

dogs with cancer but will switch to intravenous administration if permission can be obtained from the owners for hospitalization....If study goes forward 40 to 60 dogs with cancer will eventually be treated for a period of 30 days." (emphasis added)

No mention of dog or cat experiments is made in the report, although in many ways domestic pets make ideal experimental animals, and amygdalin experiments in these animals could be most revealing.

We have heard of a number of cats and dogs with breast cancer and renal carcinoma treated with 100 mg/kg/day of amygdalin intravenously, apparently without any beneficial effects. We wonder what became of the more ambitious plans outlined in the December 1 memo?

Other AKR Leukemia Studies

Further studies with AKR leukemic mice were carried out in the laboratory of Dr. Robert A. Kassel of SKI. At the request of Dr. Old, we have been told, Dr. Kassel's laboratory set out to duplicate Dr. Sugiura's results, which were then being interpreted in a more positive fashion than in the present report. (Dr. Kassel is a recognized authority on this system).

The experiment was plagued by sickness in the animal rooms at the Walker Laboratory and was terminated before completion. But the amygdalin-treated animals appeared to be livelier and to have a better appetite than the controls.

Planned Studies

The December 1, 1972 memo also contains references to experiments with amygdalin which were either not performed or not included in the final report. Mention is made of a "combination experiment" in which amygdalin and the enzyme beta-glucosidase were given in tandem. Dr. Morris Teller was to try amygdalin in his Huggin's rat system. Dr. Tarnowski was to test at least one spectrum tumor with the drug given orally. Dr. Schwartz would make beta-glucosidase determinations of tumor tissue (see Nisselbaum below) and examine their urine for products of enzymatic action.

Tissue Culture Studies

Dr. Jerome Nisselbaum, in conjunction with Drs. Old and Schwartz, carried out very extensive studies on the biochemistry of amygdalin and prunasin (a related compound) in tissue culture between 1973 and 1975. Technically, Sloan-Kettering was not bound to include these studies in its report, since the report is only concerned with animal studies. We are including it here because it is relevant to the overall discussion and because SKI has not indicated its intention to publish this data.

Official mention of Nisselbaum's work with amygdalin can be found in the SKI Annual Reports of 1973 and 1974. We feel it lends some credibility to the theoretical basis of cancer therapy with cyanogenic compounds, although not necessarily in the form postulated by Krebs.

Nisselbaum, we have learned, has also found that certain cancer tissues have elevated levels of beta-glucosidase, allegedly a cyanide

"liberating enzyme," while having variable levels of rhodanese, a supposedly "protective enzyme." The only normal tissues in which beta-glucosidase activity was comparable to that of cancer cells were liver and kidney. One rabbit liver, mouse fetuses and digestive tissues were found to be capable of breaking down amygdalin in Nisselbaum's test tube. This would seem to rule out Krebs's proposed mechanism for amygdalin's action. Yet, interestingly, four mouse tumors and normal liver could all break down prunasin, a compound closely related in structure to amygdalin. They apparently did so by elevated levels of beta-glucosidase. This would seem to indicate that prunasin is a cyanogenic compound which should be more closely studied and which may be expected to be more active than amygdalin, if this class of compounds turns out to have anti-cancer properties.

In vivo prunasin experiment

In the light of the above, it is interesting to note that Sugiura has already performed a number of experiments using prunasin in CD8F1 mice. Results have appeared to be fairly positive, with smaller doses needed than in the amygdalin experiments. The number of mice in these experiments has been too small to draw any valid conclusions, however.

The following are excerpts of unpublished results from amygdalin testing. They were either entirely covered-up or their significance distorted in the report

1. Sugiura's memo of July 25, 1975. His comments reveal objective anti-leukemic effects of amygdalin in AKR prevention experiment not mentioned in report

Table 3 shows summary results of 3 preventive experiments performed on November 7, 1974, January 11, 1975, and March 1, 1975. At the time of the experiments these mice were approximately 6 months old and had not developed leukemia. There was neither enlargement of inguinal lymph nodes nor enlargement of spleens (by palpation).

Table 3 shows that repeated injections of 2000 mg/kg/day of amygdalin had no destructive effect on the growth of leukemia in AKR mice. The average survival time of amygdalin-treated animals was essentially the same as that of control animals - 70 days against 62 days. However, size of reticulo-endothelial organs of amygdalin-treated animals was definitely reduced - 50% in the case of thymuses, 45% in the case of inguinal lymph nodes, and 25% in the case of spleens. This indicates that amygdalin is not a cancer cure but a good palliative drug.

Kanematsu Sugiura

Kanematsu Sugiura
July 25, 1975

2. Sugiura's memo of May 17, 1975 on treatment experiment. His comments reveal objective effects of amygdalin even though animals were very sick. (Memo abridged. All handwritten corrections are in original.)

SLOAN-KETTERING INSTITUTE *for* CANCER RESEARCH

DONALD S. WALKER LABORATORY, 145 BOSTON POST RD., RYE, N.Y. 10580 OWENS 8-1100

Leukemia treatment experiment of November 14, 1974, was repeated on March 13, 1975.

All female AKR mice at start of the experiment had advanced leukemia - large inguinal lymph nodes and large spleens (by palpation). Most of the animals were in poor health and emaciated....

The results show that repeated intraperitoneal injections of 2000 mg/kg/day of amygdalin had no destructive effect on leukemia. At the end of ~~13~~ 32 days all of the 9/10 amygdalin-treated animals were dead. ~~However, one animal was alive at the end of 25 days.~~ In the control group all of the 10 animals were dead at the end of 22 days. The average survival time in the control group was 12 days and for amygdalin-treated group was ~~11~~ 13 days.

Post mortem examination of the control animals showed great enlargement of thymuses, spleens and inguinal lymph nodes in ~~all~~ majority of cases. The average weight of these tissues were thymus 346 mg., spleen 351 mg. and inguinal lymph nodes 49 mg. In contrast, 6 out of 9/10 thymuses of amygdalin-treated animals were small - normal size or less (av. 75 mg.), while 3 thymuses were greatly enlarged (av. 398 mg.)....

Spleens of leukemic animals did not respond to amygdalin treatment - all spleens were greatly enlarged (av. ~~406~~ 375 mg.). On the other hand, repeated intraperitoneal injections of amygdalin prevented the enlargement of lymph nodes in AKR mice - approximately 50 per cent....

Kanematsu Sugiura

Kanematsu Sugiura

~~April 8, 1975~~

May 17, 1975

(3. Positive Swiss Albino test revealed in letter from Dr. Stockert's technician to Dr. Old.)

Dec 20, 1973

Dear Dr. Old,

Here are some brief notes and general observations on the Swiss mice presently being treated with Amygdalin.

① Three groups of Swiss mice are now being injected 4-5x per week with 3 different doses of Amygdalin. (100 mg, 50 mg and 10 mg).

② In contrast to the 18 or 20 mice used at the beginning of this experiment, the data I give you now are on mice previously selected for initial tumor size. (No tumor larger than approximately 16/16 mm is used either for treatment or control.) These selected tumors are then observed for at least 1 week to be sure all are growing.

* ③ Although the numbers are relatively small, and all tumors eventually do grow, I think we can say that those mice in the group receiving the highest dose of Amygdalin (100 mg/ml) not only live longer but remain healthier and more active than do mice receiving lower doses of the drug, no treatment or plain saline injections.

* ④ I don't think however, that the actual tumor line is affected by Amygdalin but only that tumor growth for periods of days is retarded.

* ⑤ I also see a higher incidence of lung metastases in the control mice, the 50 mg & 10 mg when compared with the 100 mg treated group

⑥ The control mice (untreated) seem to remain healthy with steady tumor growth and live an average of 42 days as compared with 26.5 day = 50 MG MKG and 39 days = 10 MG A

Shelley

3. Positive experiment in Swiss Albino: data from S. Jacobs to Lloyd Old.

SWISS SPONTANEOUS MAMMARY TUMOR TREATED WITH AMYGDALIN (1/4/74) 47

TREATMENT	MOUSE #	NUMBER OF INJECTIONS	TUMOR GROWTH FINAL SIZE	TUMOR GROWTH +/- mm*	TUMOR GROWTH NO. DAYS OBSERVED	VISIBLE LUNG METAS			Average Surv. time / Average Tumor growth
	52	104	31.5	+15	142	O	HEALTHY - SAC 1/3/74		
	113	44	31	+22	57	X	HEALTHY - DIED	ALL HEALTHY	
	111	53	15	+5	81	O	HEALTHY - SAC 1/3/74		71.9 days
	95	60	17	+7	81	O	HEALTHY - SAC 1/3/74	1/6 DEAD	+ 5.5mm
100MG/ML	112	55	31.5	+14	81	10-15	HEALTHY - SAC 1/3/74	2/7 METAS	
in 1.0ML	150	33	9.5	+0.5	52	O	HEALTHY - SAC 1/3/74		
(5x/wk IP)	155	29	15.5	+8	52	2	HEALTHY - SAC 1/3/74		
(≈ 3333 mg/kg)	176	19	10	-0.5	29	O	HEALTHY - SAC		
	90	43	31.5	+20.5	63	X	VERY SICK - DIED 12/3		
	92	36	6	-4	50	YES	VERY SICK - SAC 1/20	5/9 DEAD	
	119	19	14.5	+4	23	YES	VERY SICK - DIED	7/7 METAS	44.5days
	123	50	28.5	+22	73	>50	SAC 1/4/73		+11.5mm
50 MG/ML	126	48	38.5	+32	70	X	SICK - DIED	ALL SICK	
in 2.5mL	158	25	29.5	+19.5	38	750	SICK - DIED	11/28/73	
(5x/wk IP)	160	29	6	-2	41	1	SAC 1/3/74		
(≈ 2666 mg/kg)	168	7	13	+3.5	9	YES	SICK - DIED		
	174	19	18.5	+6.5	29	4	SAC 1/3/74		
	84	44	17.5	+10.5	63	YES	SAC - SICK 11/19/73		
	87	49	16.5	+9.5	69	YES	SICK - DIED 11/24	ALL SICK	56.2days
10MG/ML	94	41	26.5	+18.5	55	YES	SICK - DIED 11/24	4/5 DEAD	+11.5mm
in 0.5ml	96	21	20	+7.5	29	YES	SICK - DIED	5/5 METAS	
(5x/wk IP)	118	44	19	+11.5	64	YES	SICK - DIED		

(≈ 333 mg/kg)

* DR. OLD - ONLY THE MICE DONE LAST HAD ACTUALLY COLIATED METASTASES.

+ initial size < 16 mm

** NO CORRELATION BETWEEN TUMOR SIZE AND METASTASES

*** HIGH DOSE (100 MG/ML) SEEMS TO KEEP THE MICE HEALTHIER AND ALIVE LONGER WHILE DECREASING THE INCIDENCE OF LUNG METASTASES.

4. Minutes of Therapy Field meeting of December 1, 1972 showing extensive plans for amygdalin testing at SKI. Ten SKI doctors in attendance. (Abridged)

Dr. Old opened the discussion by pointing out the need to examine the usefulness of old drugs or forms of therapy, such as Laetrile, so that the Institute would be in a position to take a stand on certain controversial agents....

Normal dogs. Dr. Hardy gave 1000 mg/kg orally to three normal dogs which became sick after three hours and died on day 2 when the dose was repeated. The dogs were cyanotic and died of hemorrhage in the gut. No convulsions were observed indicating that the deaths were probably not caused by cyanide.

A special release by the McNaughton Foundation stated that the tolerated oral dose in their IND was incorrect and that it is 100 mg/kg instead of 1000 mg/kg. Dr. Hardy used 100 mg/kg in normal dogs without toxicity after four days and will continue the pharmacology at this dose level. If the study goes forward 40 to 60 dogs with cancer will eventually be treated for a period of 30 days.

Preparations. The McNaughton preparations of Amygdalin are good according to the precise information included in their IND....

In summary, it was agreed that the following should be done:

1. The authenticity of Amygdalin will be checked by comparison with a sample obtained from Sigma Chemical Company if their preparation is synthetic.

2. Dr. Hardy will continue oral therapy in "out-patient" dogs with cancer but will switch to intravenous administration if permission can be obtained from the owners for hospitalization.

3. Dr. Sugiura will repeat the experiments in mice with spontaneous tumors.

4. Dr. Teller will be asked to try it in his Huggin's rat system.

5. Dr. Tarnowski will test at least one spectrum tumor with the drug given orally.

6. Dr. Schwartz will make β-glucosidase determinations on tumor tissues and will examine urines for products of enzymatic action.

7. Dr. Philips will review the McNaughton pharmacologic data to determine what additional work needs to be done.

8. No further clinical review is needed. Dr. Krakoff has reviewed the literature thoroughly and finds no evidence that Amygdalin has any effect in humans.

9. A decision with respect to clinical trials will be made after further work is done.

5. Letter to Daniel Martin from NCI official showing that government considered value of his mouse colony "obscure" in 1973:

DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE
PUBLIC HEALTH SERVICE
NATIONAL INSTITUTES OF HEALTH
BETHESDA, MARYLAND 20014

August 7, 1973

NATIONAL CANCER INSTITUTE

Daniel S. Martin, M.D.
The Catholic Medical Center
of Brooklyn and Queens, Inc.
Department of Surgery-Research
89-15 Woodhaven Blvd.
Woodhaven, New York 11421

Dear Dr. Martin:

As I discussed with you on the telephone recently, your correspondence with Dr. Terry about the possibility of contract support for renovating your animal quarters was recently discussed with the Committee on Cancer Immunotherapy.

Unfortunately, as I told you, the Committee did not feel the information as to the scientific need for the expansion of the mouse colony carrying spontaneous mammary tumors has been fully established. Thus, they were in the position of having to consider support of a resource whose potential value is obscure. There was no hesitation on the part of the Committee to say that contract funds in specific contracts could be spent to buy the mice that you propose to provide. This mechanism would then provide the necessary element of review of specific projects for scientific merit.

I recognize that this approach does not solve your immediate problem in terms of the funding needed for renovating the animal quarters, and I have great sympathy for your concern about this resource as a model with value for studies in immunotherapy. I sincerely hope that you will find a mechanism to accomplish your goal.

Yours truly,

Dorothy Windhorst

Dorothy Windhorst, M.D.
Executive Secretary
Tumor Immunology Contracting Program

Laetrile at Sloan-Kettering: A Chronology

According to the SKI report, "The scientific caution that led us to establish as clearly as possible the facts on the degree of activity of amygdalin in the CD8F1 system led to baseless charges of cover-up by those who either were unaware of, or chose to ignore, the existence of negative results."

Second Opinion has been among those claiming there is a "cover-up" of facts about amygdalin at Sloan-Kettering Institute. We have done so not out of ignorance or because we chose to ignore negative results. On the contrary, we favor the publication of all results on amygdalin, pro or con. That is why we ourselves are publishing for the first time a report of a negative study with 30B12 transplantable tumor, and we point out the possibility that amygdalin might interfere with the treatment of cancer by other agents (e.g. "M").

Our charge of cover-up is based on the contradiction between the public statements made by MSKCC officials over the last four years and the facts as we know them and present them in this monograph. In addition, we feel that the unnecessary delay in publishing and the subtle but real intimidation of scientists not to publish politically sensitive results is a form of cover-up.

The SKI report is noticeably deficient in chronology; in fact, it is almost impossible to tell from the text itself when experiments took place and in which order. Below is a chronology of amygdalin testing at SKI and related events, as best as we could reconstruct it:

Mid-1972: Experiments with amygdalin begun at SKI under the direction of Lloyd J. Old and C. Chester Stock, and at the behest of Mr. Benno Schmidt, MSKCC vice chairman and head of the President's Cancer Panel.

Summer, 1972: Essentially negative results achieved in transplantable tumor systems (SKI paper #1; see above, reports these tests).

Autumn, 1972: Kanematsu Sugiura, member emeritus of SKI, begins studies with amygdalin in CD8F1 mice. Reports first positive results in inhibiting the spread of metastases, stopping the growth of small tumors and improving the health and well-being of animals.

December 1, 1972: Meeting of the "Therapy Field". Decision to go full steam ahead with amygdalin testing. SKI chemotherapist opposes clinical trials.

February-May, 1973: Sugiura's 2nd, 3rd, and 4th CD8F1 experiments. All positive with respect to metastases and other anti-cancer effects noted above.

June, 1973: Sugiura issues report to SKI leadership summarizing his experiments to date.

July 23, 1973: "First collaborative experiment" between SKI and Dr. Daniel S. Martin of the Catholic Medical Center begins. SKI team, including Sugiura, travels to Queens weekly to take part in test.

August 7, 1973: Daniel S. Martin informed that the National Cancer Institute considers his CD8F1 mouse colony of "obscure" value. Turned down in request

for funds.

Summer, 1973: Copy of Sugiura's internal memorandum "leaked" from SKI to a lawyer for Dr. John Richardson, doctor on trial for using Laetrile to treat cancer in California. Cited in court

September, 1973: "First Collaborative experiment" brought to a close prematurely. Test invalidated because too few mice had developed metastases.

October, 1973: Lewis Thomas, newly appointed president of MSKCC, says, "These are bad times for reason, all around. Suddenly, all of the major ills are being coped with by acupuncture. If not acupuncture, it is apricot pits"[i.e. amygdalin].

Late fall, 1973: Positive amygdalin experiments performed in the laboratory of Dr. Elizabeth Stockert of SKI by Stockert, Schloen and Jacobs.

December, 1973: Science magazine publishes first full account of amygdalin testing at SKI. Thomas says, "This institute can answer the Laetrile question fairly quickly." Robert A. Good, newly appointed president of SKI, says, "We have evidence on both sides of the fence on this."

January, 1974: "At this moment there is no evidence that Laetrile has any effect on cancer." Robert A. Good, Los Angeles Times (1/10/74)

February, 1974: Sugiura's 5th CD8F1 treatment experiment. Results same as in previous experiments, this time with some "bioassays".

March 15, 1974: MSKCC administration issues statement that "at this time, we have no information that amygdalin is useful in the treatment of human cancer." No mention of Sugiura or Stockert's experiments.

March 25, 1974: Dr. Arthur Holleb, vice president of the American Cancer Society, tells Daily News (N.Y.), that positive SKI tests with Laetrile had "no confirmation from later work."

March 22, 1975: Franz Schmid begins first experiment with amygdalin.

Late March, 1975: Stockert completes first negative experiment with amygdalin, using only macro-visual method of detecting metastases. Alters Sugiura's protocols; test invalidated.

April 2, 1975: "Laetrile has shown after two years of tests to be worthless in fighting cancer," Lewis Thomas. "Details of the study will be published within a few weeks. He declined details." --Associated Press release, April 3, 1975.

Spring, 1975: Daniel Martin receives $1,000,000 from the National Cancer Institute to breed CD8F1 mice.

May 27, 1975: Schmid completes second CD8F1 experiment. One-fortieth the dose of Sugiura's experiments is used: treated animals have more metastases, but live 50 percent longer.

February 8, 1975: Sugiura reports positive results with amygdalin

in Swiss Albino spontaneous mammary tumors. Results "essentially the same" as in CD8F1 experiments and Stockert's unpublished Swiss Albino tests.

November, 1974- August, 1975: Sugiura performs eight experiments with amygdalin in AKR leukemic mice. "Amygdalin had a certain inhibitory action on the growth of leukemia." (Sugiura memo of August 1, 1975)

July, 1975: SKI leaders tell the New York Times that Sugiura's positive results are "spurious" and the result of "vagaries of experimental variation and unfamiliarity with the animals used." (July 21, 1975)

August 11, 1975: "We have found amygdalin negative in all the animal systems we have tested," C. Chester Stock (Medical World News).

Ibid.: Clinical trials? "No ways," says Benno Schmidt. "There's no way, I believe, that they can convince the people at Sloan-Kettering there's any basis for going further."

Late August, 1975: Laboratory notes and memos from Sugiura's CD8F1 and Swiss Albino experiments 'leaked' to the press. Note, on SKI stationery, claims, "Due to political pressure these results are being suppressed."

October, 1975: "Most of the time when other people repeat my experiments they confirm them- especially in the chemotherapy of cancer. I don't remember ever doing experiments that were later not confirmed. It is still my belief that amygdalin cures metastases." - Kanematsu Sugiura(Medical World News, October 6, 1975)

Ibid.,: Plans announced for a joint Sugiura-Schmid tie-breaker experiment.

November, 1975: Schmid confirms Sugiura's findings that amygdalin inhibits the formation of metastases in CD8F1 mice. Schmid refuses comment. SKI calls for a "blind" experiment.

Fall, 1975- Spring, 1976: Prolonged negotiations between SKI and the Catholic Medical Center over design and location of "blind" study. SKI finally agrees to let study be done at CMC, under Martin's direction.

January, 1976: Sugiura performs still unpublished positive experiment with prunasin (compound related to amygdalin) in CD8F1 mice.

January, 1976: Sugiura submits completed paper, in publishable form, on nine amygdalin experiments to Stock and Old. SKI declines to publish this independently.

May, 1976: Martin addresses scientific meetings on subject of the "Laetrile hoax." Publishes his own anti-Laetrile pamphlet.

June, 1976: "Blind" experiment begins at Catholic Medical Center. "Blindness" lost midway for undetermined reasons. Stock attributes failure to "clumsy injection procedures" (Science, September 10, 1976)

October, 1976: Final "blind" experiment begins at SKI. Completed in

December, it shows no meaningful difference between treated and controls with respect to metastases. Sugiura rejects validity of test when over 40 percent of the control mice show initial tumor stoppages.

November, 1976: First issue of Second Opinion accuses SKI of Laetrile cover-up, particularly of facts about the first "blind" experiment.

February, 1977: Second Opinion publicizes unpublished work on the biochemistry of amygdalin and prunasin which appeared in the SKI 1974 Annual Report. (Sugiura's work not included in the same report).

June 15, 1977: Press conference at MSKCC releases pre-publication copies of amygdalin papers to the press. "Laetrile was found to possess neither preventive, nor tumor-regressant, nor anti-metastatic, nor curative anticancer activity."

January, 1978: Projected date for publication of SKI amygdalin experiments in the Journal of Surgical Oncology (Buffalo, N.Y.)

References

"Anatomy of a Coverup: Successful Sloan-Kettering Amygdalin (Laetrile) Animal Studies." Commentary and analysis by Ernst T. Krebs, Jr. Committee for Freedom of Choice in Cancer Therapy, Los Altos, 1975

Anderson, Joan C., Fugmann, Ruth A., Stolfi, Robert L., and Martin, Daniel S. Metastatic Incidence of a Spontaneous Murine Mammary Adenocarcinoma. Cancer Res. 34, 1916-1920, August 1974.

Attiyeh, Fadi F., Jensen, Michael, Huvos, Andrew G., and Fracchia, Alfred. Axillary Micrometastases and Macrometastases in Carcinoma of the Breast. Surgery, Gynecology & Obstetrics, 144:6, June, 1977.

Carmel, Richard J. and Brown, J. Martin. The Effect of Cyclophosphamide and Other Drugs on the Incidence of Pulmonary Metastases in Mice. Cancer Res. 37, 145-151, January 1977.

DeWys, William D. A quantitative model for the study of the growth and treatment of a tumor and its metastases. Cancer Res. 32, 367-373, 1972.

Franks, Leonard M., Carbonell, Anthony W., Hemmings, Valerie J., and Riddle, Peter N, Metastasizing Tumors from Serum-supplemented and Serum-free Cell Lines from a C57BL Mouse Lung Tumor, Cancer Res. 36, 1049-1055, March 1976.

Harada, Yoshio. Pituitary Role in the Growth of Metastasizing MRMT-1 Mammary Carcinoma in Rats. Cancer Res. 36, 18-22, January 1976.

Krebs, Ernst T., Jr. The Laetrile-Nitrilosides in the Prevention and Control of Cancer. McNaughton Foundation, Montreal, Canada.

Kassel, Robert, Hardy, W., Day, N.K., Complement in cancer, In Biological Amplification Systems in Immunology (Day, N.K. and Good, Robert A., eds.) New York, Plenum, 1977, pp. 277-294

Liotta, Lance Allen, Kleinerman, Jerome and Saidel, Gerald M. The Significance of Hematogenous Tumor Cell Clumps in the Metastatic Process. Cancer Res. 36, 889-894, March 1976.

Martin, Daniel S., Hayworth, Peggy and Fugmann, Ruth A. Enhanced Cures of Spontaneous Murine Mammary Tumors with Surgery, Combination Chemotherapy and Immunotherapy. Cancer Res. 30, 709-716, March 1970.

Martin, Daniel S., Fugmann, Ruth A., Stolfi, Robert L. and Hayworth, Peggy E. Solid Tumor Animal Model Therapeutically Predictive for Human Breast Cancer. Cancer Chemotherapy Reports, Part 2 Vol. 5, No. 1, December 1975. P 89

Martin, Daniel S. A Review of Amygdalin-Laetrile [mimeographed], New York, 1976.

Poggi, Andreina et al, Blood Coagulation Changes in Mice Bearing Lewis Lung Carcinoma, a Metastasizing Tumor. Cancer Res. 37, 272-277, January 1977

Dubois, Jean B. and Serrou, B. Treatment of the Mouse Lewis Tumor by the Association of Radiotherapy and Immunotherapy with Bacillus Calmette-Guerin. Cancer Res. 36, 1731-1734, May 1976

Stolfi, R.L., Martin, D.S. and Fugmann, R.A. Spontaneous Murine Mammary Adenocarcinoma: Model System for Evaluation of Combined Methods of Therapy. Cancer Chemotherapy Reports 55: 239-251, 1971.

Sugiura, K. Effects of intravenous injection of yeast and barley extracts and L. casei factor upon spontaneous mammary adenocarcinoma in mice. in Moulton, F.R., ed.: Approaches to Tumor Chemotherapy, Amer. Assoc. Adv. Sci., 1947.

Sugiura, K. The Publications of Kanematsu Sugiura: Memorial Edition (4 vols.) New York, 1965.

Sugiura, K. Reminiscence and Experience in Experimental Chemotherapy of Cancer. in Medical Clinics of North America, Vol. 55, no. 3, May 1971.

Tarnowski, George S., Kassel, Robert L., Mountain, Isabel M. Blackburn, Peter, Wilson, Glynn, and Wang, Dalton. Comparison of Antitumor Activities of Pancreatic Ribonuclease and its Cross-Linked Dimer. Cancer Res. 37 July-August, 1977.

Ushio, Yukitaka, Chernik, Norma L., Shapiro, William R. and Posner, Jerome B. Metastatic Tumor of the Brain: Development of an Experimental Model. Annals of Neurology, vol. 2, no. 1, July 1977.

Wexler, H. Accurate Identification of Experimental Pulmonary Metastases. J. Nat. Cancer Inst. 36: 641-645, 1966.

Yuhas, John M. and Ullrich, Robert L. Responsiveness of Senescent Mice to the Antitumor Properties of Corynebacterium parvum. Cancer Res. 36, 161-166, January 1966.

Political Analysis

Why would the leaders of Memorial Sloan-Kettering misrepresent the results of their Laetrile tests? Second Opinion would briefly like to present its thinking on this question. Readers who do not share our political perspective are asked not to reject our scientific critique because of ideological differences.

We start with basics: ours is a capitalist country, in which the profit motive is king. We are taught to prize individualism above all virtues--each one making it for himself in a competitive, dog-eat-dog world. This spirit also pervades the world of medicine.

The mythology of medicine states that the doctor is dedicated above all to the good of the patient. While some doctors are truly devoted to their patients' welfare, all too often the doctor's concern for the sick loses out to his own self-interest. The medical profession as a whole is organized to defend its own narrow economic interests. It does this by resisting any fundamental change in a system of health-care delivery which is highly inadequate from the patient's point of view, but profitable for the doctors as a group.

The profession, and especially its main spokesman, the American Medical Association, has waged a long and bitter rearguard action against such basic reforms as Medicare, Medicaid, group plans, etc. It generally opposes preventive medicine, nutrition, health education, restriction of cancer-causing industries (although there are some exceptions).

In fact, the medical profession reacts with knee-jerk rapidity against anything which could overturn the applecart and bring about sweeping changes in the way medicine is practiced, or which would undermine the almost complete monopoly which the profession has in all matters of health.

Such a posture is not conducive to the development of new therapeutic ideas and philosophies, to say the least. In fact, the history of medicine from Paracelsus to Pasteur, from Semmelweis to the "Pap smear" has been the history of struggle against ridicule, skepticism and suppression. Eventually new, correct ideas win out over prejudice.

This general atmosphere in medicine helps to explain what is happening to Laetrile. This is not to say that Laetrile is an effective anti-cancer agent, much less that it is necessarily in the same category as Pasteur's discoveries. But no one can honestly deny that the theory behind it is not provocative and challenging.

This newness, and the vigor with which it was pushed, especially by those outside the medical profession, was enough to damn it in the eyes of the medical leadership. They simply shut their ears to it and from that point on there was more of a "war" than a debate.

This closed-minded attitude can be illustrated by an incident that took place at Sloan-Kettering several years ago. A person prominent in the Laetrile movement was quietly invited to speak to a select group of doctors. After he finished presenting his "trophoblastic theory of cancer", which holds that cancer is a single disease rather than many different diseases and that Laetrile is a sweeping preventative and cure, a leading SKI chemotherapist exclaimed: "Well, if that's the case, there isn't a need for Memorial Sloan-Kettering Cancer Center any more, is there?" He proceeded to storm out of the room.

But doctors alone are not responsible for Laetrile's suppression. For

with the passage of the National Cancer act and the disbursement of billions of dollars in research funds an extensive cancer bureaucracy has grown up to administer those funds. These bureaucrats at the National Cancer Institute, the American Cancer Society, the Food and Drug Administration and Sloan-Kettering itself are enmeshed in their own continual, narrow power plays and jealously guard their own ground.

It is sometimes said that bureaucrats are deliberately sitting on a cure for cancer because their own jobs, paying up to $100,000 a year, would be eliminated by such a cure. We do not believe, however, that anybody is deliberately and maliciously sitting on a cure. Rather, bureaucracy engenders a generally stupefying conservatism. Issues are avoided and no one wants to be the "bearer of ill tidings."

There is an unhealthy skepticism and even cynicism about anyone else's claims to progress, especially if it relates to therapy. This is understandable in bureaucrats and fund-raisers who must promote their own institution's work above all others. Even about your own work, it is far safer to talk in general terms about the progress that is being made than to make specific claims that can be proven or disproven.

Rather than a conscious conspiracy you often find a blanket of apathy or fear about new things. The chronology of the Laetrile tests illustrates this, we think. Why, for example, did it take five years to publish this paper (which still isn't officially published, as of this writing)? Why did it take six months for two experienced researchers (Stock and Martin) to design the first "blind" test?

Sadly, this foot-dragging and fearfulness, vacillation, cynicism and delay are all too common at Sloan-Kettering and, we suspect, at other large, centralized research centers as well. It is an inherent feature of our basically undemocratic economic structure.

Scientists suffer in this situation. There are "electric fences" which surround them, whether they realize it or not. So long as a scientist sticks to a safe topic, on well-trod ground, and develops cozy relationships with his peers, he is left alone. If he does good work, he will be rewarded with grants and contracts.

When a lab's work progresses to the point where it has medical implications the difficulties really begin. The safe thing is to draw back, go back to your test tubes and "further studies". If the scientist persists with a new therapeutic idea he is bound to run into heavy opposition from those who already occupy the space he is vying for. He finds he cannot get patients to try his new compound on, even if they themselves are willing. He cannot get cooperation from other departments in the center. He cannot get funds, laboratory space, even government grants!

There are a number of exciting, imaginative and worthwhile therapeutic ideas which are presently "kicking around" Sloan-Kettering. Experience has shown that few of them will ever be given a fair test, much less become established forms of treatment.

A third force which makes a fair test for Laetrile almost impossible is also the most powerful: the Board of Trustees. The meetings of the Board are closed to the public and, in fact, no notes are kept of what transpires. A look at the composition of the Board, however, shows an awesome concentration of power. These are among the richest and most powerful men in the world.

Of course, most of the members of the Board are basically ornaments: big donors or once-a-year fund-raisers. A handful are really active in

the affairs of the Center and personally direct the Administration: they include Laurance S. Rockefeller, chairman of the board, Benno Schmidt, vice chairman, and a few others.

These men are all investment bankers. Their primary business interest may therefore seem to be divorced from the realms of cancer research, but actually the two can be closely related. Making profitable investments is often dependent on knowing the latest developments in technology and Laurance Rockefeller in particular has made much of his money by investing at the initiation of technologically oriented businesses (Eastern Airlines, McDonnell Douglas, Mallinckrodt Chemicals, etc.) "In venture capital investment, the main line of Mr. Rockefeller's activities has involved new or young enterprises operating on the 'frontiers of technology'" according to his official biography.

There is nothing illegal about this, but being on the MSKCC Board clearly gives these men access to some of the best scientific talent and ideas in the country, in addition to whatever benefits the position confers.

If we trace the history of the Memorial board, in fact, we find it intertwined with the personal and business needs of the men on the board and the general needs of their class.

The Astors founded Memorial Hospital largely because two members of their own family were dying from cancer. James Douglas, the president of Phelps-Dodge mining company was fascinated by radium, as a miser is enthralled by gold. He set up an elaborate mining and marketing scheme under government auspices to handle radium, which was then selling for $150,000 a gram.

We are told that he did this for the sake of poor cancer victims, but he himself wrote, "All this story about humanity and philanthropy is foolish. I want it understood that I shall do what I like with the radium that belongs to me." (H.H. Langton, James Douglas: A Memoir, Toronto, 1940, p.118)

At the time of the Depression, the Rockefellers took over control of Memorial. John D. Rockefeller then entrusted his interest in the hospital to Frank Howard, vice president of research at Standard Oil Co. Howard, in conjunction with "Dusty" Rhoads, began to envision a great research institute attached to the hospital which would seek out a chemical cure for cancer, analagous to the newly discovered "sulfa" drug Prontosil, the first modern antibiotic.

There was a catch to Howard's "humanity," however, just as there was to Douglas's. Drug companies reap super-profits through the perfectly legal expedient of patents. In the eyes of Howard, the cure for cancer had to be a patentable cure or it simply was not worth the effort.

"To undertake a costly industrial research or development project," he wrote, "without inquiring into the patent situation is like drilling an exploratory oil well without finding out who owns the property on which you drill." (lecture at George Washington University, December 5, 1956)

Upon the founding of SKI, Howard drew up a legal agreement called the "Standard Form", under which SKI agreed to test various compounds for the drug companies, to keep completely silent about its research in progress, to give the companies the right to review all papers about its product, and to provide patents or free licenses to the company should the product turn out to be valuable. Methotrexate, one of the most widely used anti-cancer agents, emerged from this program as the possession of Lederle Laboratories, a division of American Cyanamid chemical company (A director of American Cyanamid, James Fisk, sits on the MSKCC Board and is, in fact, Chairman of the Board of Sloan-Kettering Institute.)

What's wrong with that? you might ask. At least they're developing a cure for cancer! What is wrong is that the promotion of one kind of cancer

therapy has brought with it the suppression of other kinds. In this case, a chemical cure for cancer was promoted to the rafters, while most other approaches were ignored or suppressed.

The most glaring and tragic example has been the suppression of the field of cancer prevention. According to various estimates 50-90 percent of all cancers are environmentally caused. To this day, however, SKI has only the most paltry program in cancer prevention: only three or four individuals out of several hundred are seriously working on the preventive approach.

Is it accidental that a research center which has on its board the president of Exxon, a director of American Cyanamid chemical company and the Philip Morris tobacco company has no serious program to study the environmental origins of cancer? We don't think this is a coincidence.

In fact, we feel that it is inherent in the nature of our entire economic and political system that threatening and revolutionary scientific ideas can be and are suppressed. There is a "good" reason for this.

In huge corporations, enormous sums of money are invested in new plants. These plants are supposed to last a certain number of years before they are obsolete. But science knows no bounds: new inventions, unbridled, can lay low a factor as effectively as a missile! Look at what the transistor did to the vacuum tube business, or what the calculator did to the adding machine.

Certainly no one can suppress a good idea forever. But the modern day monopoly capitalists have such power over their industries and over the economy as a whole that they can delay for many years the appearance of revolutionary techniques which threaten their profits.

"The process takes two forms," wrote the great British scientist, J.D. Bernal. "The stifling of existing invention and the choking of new invention by restricting research." (The Social Function of Science, Cambridge, 1967, p.141)

This suppression is not limited to cancer, nor is it a recent thing. It dates from the origin of the modern corporation. Justice Louis Brandeis pointed out over fifty years ago that the gas companies tried to suppress the electric light, the electric industry then suppressed the development of neon lighting, Western Union fought against the telephone, and then both Western Union and the telephone company opposed radio.

In 1937, the F.C.C. found that Bell Telephone had bought up and locked in its vaults 3,400 useful patents for fear that a competitor would get ahold of them!

A big businessman of the 1930s put it in a nutshell when he wrote:

> "I have even seen the lines of progress that were most promising for for public benefit wholly neglected or positively forbidden just because they might revolutionize the industry. We have no right to expect a corporation to cut its own throat." (quoted in Bernal)

Unless one sees the difficulties of Laetrile and other such therapies or approaches as part of this objective process of our society one is left with the "devil" or "conspiracy" theory. Unfortunately, at the present time there are some people in the "Laetrile movement", even in its leadership, who take this view of things.

We reject all such narrow conspiracy theories, which basically exonerate the real culprit: the profit system and especially its twentieth century form,

monopoly capitalism.

Our interest in Laetrile has always been to have it be adequately tested and to have all those research results released. If it is indeed a useful agent (whether as preventive, palliative or cure) all patients should have access to it, including the poor. If it is useless, as determined by fair and extensive tests, we would oppose its use.

We cannot accept the slogan "freedom of choice in cancer therapy." First, as Dr. Virginia Livingston points out, "freedom of choice" is not very meaningful to the poor, who cannot afford any decent cancer treatment, much less private care in a "metabolic therapy sanitorium."

Second, we think that the "freedom of choice" slogan is directed against the existence of the Food and Drug Administration. We recognize that the F.D.A. has played a suppressive role in the Laetrile controversy and that some of its leaders epitomize a kind of arrogance and pigheadedness which is simply incompatible with good science.

On the other hand, we think that the F.D.A. provides some (although hardly enough) protection against the introduction of poisons and especially carcinogens in our food and drugs.

"Freedom of choice" is not the issue. The focus of the Laetrile movement should be to mobilize large numbers of people to demand the truth from the scientific establishment about this agent, and all issues relating to cancer.

The exposure of the Laetrile coverup has already been an eye-opener for tens of thousands around the country. It could become a revelation for millions about the real nature of this system.

Second Opinion is the voice of rank-and-file employees of Memorial Sloan-Kettering Cancer Center. It presents news and opinions of the Center and the cancer field from the employee's point of view.

The paper is distributed free, bimonthly, to the Center's 4600 employees.

We believe that very basic changes must be made at MSKCC and in the "war on cancer" in general.

We favor the best possible working conditions for all employees. Our basic aim is to help all MSKCC employees get organized, since nothing can be accomplished without organization. We oppose everything that keeps us divided, such as racism and male chauvinism.

In cancer, we believe in putting prevention first; making research relevant to human diseases; an open-minded policy toward new and unorthodox methods; making the best treatment available to all people; taking the profit out of cancer.

If you agree with us, we need your help.

Since we distribute the paper free, the cost must be met through the contributions of our staff and supporters.

But our support must continue unabated, if Second Opinion is to grow and achieve its goals.

Please help us in whatever way you can.

SEND IN YOUR SUBSCRIPTION. This will assure you of receiving each issue and will give us your support.

WRITE: Send in your letters, stories and ideas. Tell us what is happening in your area, what needs exposing. We welcome your criticism.

S.O.

SECOND OPINION
c/o Alec Pruchnicki
Box 548
Bronx, New York 10468

( ) Enclosed is $1 for a one-year employee subscription
( ) Enclosed is $5 for a one-year non-employee subscription
( ) Enclosed is $2 for Special Report "Laetrile at SKI"
( ) Enclosed is $_____ to support your efforts
( ) I would like to help distribute S.O.
( ) I would like to write for S.O.

PLEASE MAKE ALL CHECKS PAYABLE TO SECOND OPINION