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TECHNICAL REPORT No. 1 (ISAAC-TR 1)

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*Modified and extended version of blog posts "On Kamefuchi's essay about Heisenberg and Yukawa (1)–(6)," *IDEA & ISAAC: Femto-Essays* (Aug. 20–Dec. 6, 2020).

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ISAAC-TR 1
Heisenberg, Yukawa, and Nambu: Commentary on, and Thoughts Derived from, Kamefuchi's Essay

In commemoration of the 100th anniversary of
Yoichiro Nambu's birth

Abstract

Kamefuchi wrote an essay about Heisenberg's and Yukawa's tragic incidents in their later years. We have studied facts related to these incidents by looking up other references and found that Kamefuchi's suppositions at three minor points about Heisenberg's case are wrong. We have reviewed the literature evaluating the studies of the above two physicists in those years. The result has revealed that Heisenberg's work influenced Nambu for finding spontaneous symmetry breaking in subatomic physics. On the other hand, Yukawa's work has not had much impact on other researchers. We have also compared Kamefuchi's classification of research methods of theoretical physics with Nambu's two versions. The descending type method defined by Kamefuchi gives a convincing reason for the two physicists' failures in finishing the later studies. To remove faults in Nambu's revised version, we propose a new, two-aspect scheme, which indicates that the esthetic, bottom-up mode kept by Nambu made him a long-time productive theorist. In Appendix B, we describe Kamefuchi's classification of theoretical physicists given in another essay and suggest a possible extension of our proposal.

Contents

1 Introduction

KAMEFUCHI'S essay [1] treats the tragic incidents in their later years of "two giants in theoretical physics," Werner Heisenberg and Hideki Yukawa (the expression in the quotation marks are from Kamefuchi's words). Though he first worried about degrading the heroes by writing these matters, Kamefuchi came to think it essential to convey the truth and published the essay as "witness testimony."

Heisenberg's tragedy occurred at the International Conference on High Energy Physics held at CERN in July 1958. The chairperson Pauli began to attack violently against his lecture [2], denying his study altogether. Yukawa's sad situation happened at the International Conference on Particles and Fields held at the University of Rochester in August 1967. He was chairing a session, and his collaborator K was going to give a talk on the paper [3] co-authored with the former. Then, most of the audience stood up from their seats and left the room.

In the present report, we describe some facts learned from other sources concerning the tragedies. We also review the literature evaluating the research results around those occasions of the two physicists. Then, we compare Kamefuchi's classification of research methods of theoretical physics, used to explain their failures in completing the later studies, with Nambu's original and revised ones. To remove faults in Nambu's revised version of the classification, we propose a new scheme consisting of two aspects.

2 Heisenberg's Tragedy

2.1 Description in Polkinghorne's Book

Kamefuchi begins the stories writing [1, p. 18],

> Because these seem not much talked about, [...][1]

However, Polkinghorne writes the same story about Heisenberg in a book [4], which treats twenty international conferences on high energy physics from 1950 to 1980 and is a unique historical record in this research field. He writes the story of Heisenberg's tragic scene in more detail than Kamefuchi, citing from the proceedings Pauli's words of the attack on Heisenberg's speech. The book also has a photo of Pauli at the 1958 conference. Its caption includes his remark, "No credits for the future," directed to all the speakers of the session "Fundamental theoretical ideas" he chaired [5]. The story ends with the following words ([4, p. 77]):

> It was a scene at once farcical and sad. Justification lay with the sceptical Pauli but Heisenberg was one of the greatest physicists of the twentieth century who should have been able to enjoy a more dignified close to his career.[2]

Kamebuchi writes in the penultimate paragraph [1, p. 22]:

[1] All the English translations of quotes from Japanese references are of the present author's.

[2] These are emotional words, which readers might think out-of-place in a book on the history of physics. However, Ref. [4] is neither a textbook nor a scholarly book, but a book for the general public. Thus, the passages light-heartedly convey the atmosphere of conferences and the personalities of famous scholars.

> They published well-edited proceedings for the Geneva conference. The Heisenberg section has the footnote, "This part of the discussion has not been reported verbatim but is based on a paper sent by Heisenberg after the Conference." So, we cannot see from the record that there was turmoil.

The footnote mentioned above refers not to all the Discussion section for Heisenberg's talk but only to his first reply in that section. This is clear from the position of the footnote symbol * in the text [2]. However, the record of that section probably does not fully contain the words exchanged at the conference. Nonetheless, Pauli's long, professional comment and many short ones fully convey his fierce criticism of Heisenberg's talk.

2.2 Chair Pauli's Attack

Kamefuchi describes the chair Pauli's behavior as follows [1, p. 20]:

> [...] he stood up, spoke in place of the speaker, behaved more and more violently, and got so abusive as I had never heard in a physics conference.

Some may wonder: Why did no participants protest the chair's behavior? We can imagine the answer from the following. Before the above description, Kamefuchi writes [1, p. 20]:

> In the beginning, the chair Pauli, who was well known for his harshness, expressed the following introductory words, "I don't think there are any new ideas. Nevertheless, I'll open the session."

Polkinghorne quotes these Pauli's words in more detail from the proceedings [5] and adds the following opinion [4, p. 77]:

> It would be no use waving your hands in front of him and expressing the hope that it would all work out right in the end.

At this point, Pauli had already brought the entire audience to his knees, and no one could dare to advise him, who was famous for his effective spiciness.

2.3 Publicizing of Heisenberg's Research at That Time

2.3.1 Not Press Conference Announcement

Kamefuchi calls Heisenberg's research at that time *monistic field theory of elementary particles* and explains it as "a big idea to derive all elementary particles starting from a single field (or equation)." The same research is also called *Heisenberg's non-linear unified field theory program* [6, p. 283]. Hereafter, we call it simply Heisenberg's unified field theory. Kamefuchi continues [1, p. 19]:

> I first learned of this in a newspaper, so he probably made a press conference and announced it. At that time, he might have used the adjective 'universal' for the basic equation, and they erroneously reported it as "the equation of the cosmos" in Japan.

A little before graduation, the present author also saw the newspaper article about this research and wrote it down in the diary. The diary reads [7]:

February 27, 1958

I have found the following article in the Asahi Shimbun:

> [Göttingen (West Germany) 25th UP=Kyodo] At the University of Göttingen on the 25th, Professor Heisenberg, Nobel Prize winner in physics of West Germany, gave a lecture entitled "Advancement of elementary-particle theory." He announced that the research group led by him studied "unified field theory" and found a basic equation explaining all the physics laws without exception. The theory was the one about which Dr. Einstein also thought. [. . .]

March 13, 1958

[*Note on copying the diary to the website:* Here is the clipping of the Asahi Shimbun article entitled "This Is Equation of Cosmos." It showed the basic formula of elementary particles found by Heisenberg and his coauthors.]

After posting the copy of the diary on his website, the present author destroyed the original. So, he now does not have the clipping mentioned above but will show the equation quoted from another source later.

According to the first newspaper article, Heisenberg did not hold a press conference, in contradiction to Kamefuchi's supposition, but newspaper reporters listened to Heisenberg's lecture at the University of Göttingen and wrote about it. This fact is also clear from the following description in the biography of Heisenberg written by Cassidy [8, p. 542]:

> The distribution [of the preprint on the work made by Heisenberg and Pauli] was set for February 27, 1958. [. . .] Three days before the preprint was to be distributed, Heisenberg announced the new formula in a lecture at the University of Göttingen physics institute.[3]

2.3.2 Equation of Cosmos: Not Mistranslation

The description in Cassidy's book continues as follows [8, p. 542]:

> An eager reporter in the audience relayed word of a sensational new "world formula" around the world. One enthused press agent proclaimed, "Professor Heisenberg and his assistant, W. Pauli, have discovered the basic equation of the cosmos!"

The above quote indicates that the name of the equation in the Asahi Shimbun was not a mistranslation, contradictory to Kamefuchi's supposition.

The Asahi Shimbun separately reported the equation later than the news of the lecture at Göttingen University. The sentences that follow the above in Cassidy's book explain this delay [8, p. 542]:

> Two months later, more than 1800 listeners turned out to hear Heisenberg reveal the secret of the cosmos in the same auditorium on the occasion of Max Planck's one-hundredth birthday.[4] During his highly technical talk, Heisenberg carefully wrote

[3]"Three days before" means that the lecture date was the 24th, inconsistent with the date of the 25th in the Asahi Shimbun. The difference might have occurred because Asahi Shimbun missed correcting the time difference for the news distributed by "UP=Kyodo."

[4]Planck's birthday is April 23 [9], consistent with "Two months later." However, the Asahi Shimbun of March 13 cannot report the contents of the lecture made in April. They might have had this lecture to celebrate Planck's birthday about 40 days earlier than the birthday. Despite this, Cassidy seems to have imagined that it should have been around his birthday.

his new equation on the overhead projector in the darkened room:

$$\gamma_\mu \frac{\partial \psi}{\partial x_\mu} \pm l^2 \gamma_\mu \gamma_5 \psi (\bar{\psi} \gamma_\mu \gamma_5 \psi) = 0$$

Heisenberg did not reveal the formula in his lecture at the University of Göttingen in February but only its name. He wrote the equation on another occasion mentioned in the above quote. The equation quoted by Cassidy is the same as the one in a paper co-authored by Heisenberg and young researchers [10].

Polkinghorne describes Heisenberg's speech at the conference in a little more technical words than Kamefuchi as follows [4, p. 77]:

> [...] [Heisenberg] had conjectured a 'non-linear spinor equation', whose solutions he thought would correspond to the structure of matter as it was then known. Not only was his equation hard to work with, but in the course of the attempt use was made of the dangerous concept of an infinite metric, something which could result in the appearance of unphysical ghosts. [...]

Heisenberg confidently wrote his equation before the large audience, but the above quote tells us that it had some problems. We give a further explanation about the equation in §A.1.

2.3.3 Coworker Not until "Three Months" Before

Pauli was a collaborator in Heisenberg's research before the tragedy, as mentioned in the quotes at the end of §2.3.1 and the beginning of §2.3.2 from Cassidy's book. Kamefuchi writes [1, p. 20]:

> Pauli was a close friend of Heisenberg's since student days and coworker of this problem until three months before. Why did the former go on such outrage in the place where prominent researchers in particle physics lined up?

The words "three months before" in the above quote probably come from the fact that Pauli had been to the United States for three months before this international conference. According to Heisenberg's autobiography [11], however, Pauli departed for the United States a week plus a few weeks after Christmas 1957, namely around late January 1958. Further, Pauli's decision to withdraw from the joint research with Heisenberg was in the earliest period of his visit to the United States, i.e., within January, as we see below, and the conference started on July 1, 1958. Thus, their cooperation ended about five months before their meeting, and Kamefuchi's supposition "until three months before" is incorrect.

2.4 Descriptions by Criticized Side

Heisenberg writes about the 1958 Conference in his book [11, p. 235]. The description is short, as quoted below, but well conveys Pauli's intense criticism.

> I did not meet Wolfgang [Pauli] again until July 1958, when both of us attended a congress in Geneva. I was due to give a report on the current state of research into the disputed field equation, and Wolfgang's attitude to me was almost hostile. He criticized many details of my analysis, some, I thought, quite unreasonably, and he could barely be persuaded to discuss matters with me at greater length.

At the place corresponding to the end of the above quote in the Japanese version of Heisenberg's book, the translator Yamazaki attaches the note quoted below [12]. There he includes the atmosphere before the session of Heisenberg's presentation. Yamazaki's description is also a view by the criticized side because he was Heisenberg's coworker for more than ten years and was even the last coauthor of the technical paper (See Ref. [13]).

> For the 8th International Conference on High Energy Physics held in Geneva, I accompanied Heisenberg with Mr. Dürr and others from Göttingen. Heisenberg had lunch with Pauli, Feynman, and others during the break before the official discussion at the conference. Already from that time, the air was terrible. Mr. Dürr and his colleagues were worried about saying, "Poor Heisenberg." Pauli severely criticized Heisenberg's talk in the afternoon, and there was a fierce verbal exchange with Heisenberg. Most of the others were also critical of Heisenberg's approach. Being surrounded by enemies on all sides, however, Heisenberg adamantly pushed his theory through.

Mr. Dürr in the above quote is the first author of the paper [10] of Heisenberg's group mentioned in §2.3.2.

2.5 Reasons for Pauli's Rebellion

2.5.1 Criticism from U.S. Physicists

Kamefuchi answers the question about the reason of Pauli's rebellion by quoting the explanation (the part in the quotation marks below) given to him by K. Broiler (Professor at the University of Bonn) and attaches a little satirical comment [1, p. 20].

> "In the United States, Pauli perhaps proudly spoke about their research but got strong objections from young American geniuses, coming to think that it was a difficult study. Thus, he would have wanted to express openly to the excellent people at the conference that he no longer believed in their theory." This view seems to mean that Pauli sacrificed his friend's honor for his own.

Broiler's explanation is a presumption, but there is a book that gives a similar, assertive statement of the reason as follows [14, p. 1120][5]:

> Although Pauli drafted the first preprint, entitled 'On the Isospin Group in the Theory of the Elementary Particles,' he withdrew from further collaboration in January 1958, after he encountered severe criticism and opposition to the theory from the U.S. physicists at the American Physical Society meeting in New York; thus Heisenberg was left to work out the details of the theory with younger collaborators (Dürr *et al.*, 1959).

The reference "Dürr et al., 1959" at the end of the above quote looks like the source of this entire description but is not such. It is the paper [10] of the result of Heisenberg's continued research with young collaborators. Thus, the quote does not specify the source that Pauli received severe criticism from U.S. physicists. However, it indicates that Pauli decided to quit the collaboration with Heisenberg within January 1958.

[5] Reference [14] is academic, unlike Polkinghorne's book, and the quote here is in a footnote.

2.5.2 Criticism from Bohr

Not only American physicists but also an important person severely criticized Pauli's lecture in the United States. In a collection of essays by Dyson, we see this description [15, pp. 105–106]:

> Pauli happened to be passing through New York, and was prevailed upon to give a lecture explaining the new idea [of Heisenberg and him] to an audience that included Niels Bohr, who had been mentor to both Heisenberg and Pauli [. . .]. Pauli spoke for an hour, and then there was a general discussion during which he was criticized sharply by the younger generation. Finally, Bohr was called on to make a speech summing up the argument. "We are all agreed," he said, "that your theory is crazy. The question which divides us is whether it is crazy enough to have a chance of being correct. My own feeling is that it is not crazy enough."

The statement here that Pauli "was criticized sharply by the younger generation" underscores Broiler's presumption as well as the description in Ref. [14]. Moreover, Pauli's teacher, Niels Bohr, criticized Pauli. It is difficult to understand that Bohr's words, "Not crazy enough," are a harsh criticism, but Dyson adds the following explanation in his next paragraph [15, p. 106].[6]

> When the great innovation appears, it will almost certainly be in a muddled, incomplete, and confusing form. To the discoverer himself it will be only half-understood. To every body else it will be a mystery. For any speculation that does not at first glance look crazy, there is no hope.

Concerning Pauli's withdrawal from the joint research with Heisenberg, the former wrote to the latter during the former's stay in the United States, as described in Heisenberg's autobiography as follows [11, p. 235]:

> Then we were divided by the Atlantic, and Wolfgang's letters came at greater and greater intervals. I thought I noticed signs of fatigue and resignation in them, but otherwise Wolfgang was apparently still determined to see our common project through. Then, quite suddenly, he wrote me a somewhat brusque letter in which he informed me of his decision to withdraw from both the work and the publication.

2.5.3 After Rebellion

The above story is in the chapter "The unified field theory" of the autobiography, which concludes by the following passage [11, p. 236]:

> Toward the end of 1958 I received the sad news that he [Wolfgang] had died after a sudden operation. I cannot doubt but that the beginning of his illness coincided with those unhappy days in which he lost hope in the speedy completion of our theory of elementary particles. I do not, of course, resume to judge which was the cause and which the effect.

If you read the above statement only, you would feel sad. However, as Kamefuchi mentions referring to the Japanese translation of Heisenberg's autobiography, there was the

[6] The present author tried to shorten the paragraph, but it was impossible to do so because Dyson's text was like a polished jewel.

following fact before Pauli's death. A few weeks after the conference, Heisenberg and Pauli were guests at a summer school in Varenna on Lake Como, Italy. There, Pauli was again friendly to Heisenberg and said to the latter, "I think you are doing right to continue working on these problems. [...] As for me, I have had to drop out, [...]" This gives us a feeling of relief.

2.6 Impact of Heisenberg's Unified Field Theory

About the research of Heisenberg and Yukawa at the time of the tragic incidents, Kamefuchi writes [1, p. 19]:

> [...] the two persons, unfortunately, failed to complete their studies.

We see later what he wrote as a common reason for their failures. Even though Heisenberg's unified field theory was incomplete in itself, the concepts used in it seem to have had a considerable positive effect on other researchers. Concerning this, we can quote from Cao [6, p. 283].

> At the 1958 Rochester Conference on high-energy nuclear physics held in Geneva, Heisenberg invoked the idea of a degenerate vacuum to account for internal quantum numbers, such as isospin and strangeness, that provide selection rules for elementary particle interactions (1958).*
>
> In an influential paper submitted in 1959,† Heisenberg and his collaborators used his concept of a degenerate vacuum in QFT [quantum field theory] to explain the breaking of isospin symmetry by electromagnetism and weak interactions. [...]
>
> Heisenberg's degenerate vacuum was at the time widely discussed at international conferences. It was frequently quoted, greatly influenced field theorists, and helped to clear the way for the extension of SSB [spontaneous symmetry breaking] from hydrodynamics and condensed matter theory to QFT. [...]

The term "degenerate vacuum" repeated in the above quote is closely related to spontaneous symmetry breaking.

Table I. Citations of Heisenberg's papers in earlier and later years, taken from Google Scholar (as of February 1, 2021).

Period	Publication year	Topic	Ref	Citations
Earlier	1925	Quantum mechanics	[16]	1803
	1927	Uncertainty principle	[17]	4924
Later	1960	Unified field theory	[2]	15
	1959	Unified field theory	[10]	226

In Table I, we compare the citations of Heisenberg's papers in earlier and later years. The earlier ones chosen are the Nobel Prize-winning paper on the formulation of quantum mechanics based on matrices [16] and the paper on the uncertainty principle [17]. Later year papers are on the unified field theory, i.e., [2] and [10] (cited at the places of the symbols * and † in the above quotes). Cao uses the words "frequently quoted" for the latter papers. However, citations of these are much less than those of the famous earlier

*Ref. [2] of the present report is cited here.
†Ref. [10] of the present report is cited here.

works. We can guess the reason for the small citation numbers of the later work is that it was incomplete as a whole theory.

Speaking of the application of spontaneous symmetry breaking to particle physics, we know that the reason for receiving the Nobel Prize by Yoichiro Nambu was discovering the mechanism of spontaneous symmetry breaking in subatomic physics [18]. So, we apt to think that it was almost Nambu's originality. However, Heisenberg's research had an impact on him. About this, we again make quotes from Cao [6, pp. 286, 287].

> Nambu's work on superconductivity led him to consider the possible application to particle physics of the idea of non-invariant solutions (especially in the vacuum state). [...]
>
> It is of interest to note the impact of Dirac and Heisenberg on Nambu's pursuing this analogy. First, Nambu took Dirac's idea of holes very seriously and viewed the vacuum not as a void but as a plenum packed with many virtual degrees of freedom. This plenum view of the vacuum made it possible for Nambu to accept Heisenberg's concept of degeneracy of the vacuum, which lay at the heart of SSB. Second, Nambu was trying to construct a composite particle model and chose Heisenberg's non-linear model, 'because the mathematical aspect of symmetry breaking could be mostly demonstrated there', although he never liked the theory or took it seriously. [...]

3 Yukawa's Tragedy

3.1 Yukawa's Research at That Time

Kamefuchi writes [1, p. 21]:

> The lectures progressed, and Yukawa called up K to present the paper "Space-time picture of elementary particles," of which they were the co-authors.

K is Yasuhisa Katayama (1926–1978), familiar to the people who know about Yukawa in his later years. Their research belongs to the elementary domain theory that Yukawa worked on with coworkers those days. About this work, Yukawa wrote [19, p. iii]:

> I was able to formulate a theory in 1967 with Mr. Yasuhisa Katayama's much effort.

Yukawa continued somewhat proudly,

> The following year, I published a paper co-authored with Mr. Katayama and another with the additional coworker, Mr. Isao Umemura.

The papers mentioned in the above quotes are Refs. [20] and [21].

Three years later, Yukawa wrote "Preface" and "Part V. Unified theory of elementary particles" as the supervisor of Ref. [22]. In them, he frankly described the reaction of academia to the elementary domain theory and his thought as follows [22, p. vii; pp. 608–609]:

> In Part V, we decided to follow a path towards a unified theory. It will not be the only way, nor is it guaranteed to reach its goal. On the contrary, it is the path that many researchers consider to be the farthest from the legitimate one.

> If we proceed in this direction, we may, in the end, have to run into the problem of the quantization of space-time itself in some sense. The concept of the elementary domain itself may still be incomplete in that it assumes the Minkowski space, i.e., the four-dimensional continuum, as the background frame. However, all the elucidation remains in the future.

We give a short description of the concept of the elementary domain theory in §A.2.

3.2 Impact of Yukawa's Elementary Domain Theory

Table II. Citations of Yukawa's papers in earlier and later years, taken from Google Scholar for the former and DOI pages for the latter[a] (as of February 1, 2021).

Period	Publication year	Topic	Ref	Citations
Earlier	1935	Meson theory	[23]	2617
Later	1968	Elementary domain theory	[20]	39
	1968	Elementary domain theory	[21]	20

[a]Citations of the papers [20] and [21] counted by Google Scholar are the mixture of the two under the latter title. So, we have used the numbers from DOI pages instead. These numbers consist of the sum of the earlier ones in Prog. Theor. Phys. and the later ones counted by Crossref.

In Table II, we compare the citations of Yukawa's papers in earlier and later years. The earlier one chosen is the Nobel Prize-winning paper on the meson theory [23]. Later ones are on the elementary domain theory [20, 21]. Compared with paper [23], citation numbers of [20] and [21] are small. However, there might be a possibility that the elementary domain theory will make new contributions to the development of particle physics in the future. We can quote experts' views on this point.

Kemmer [24, p. 666] describes Yukawa's research after the 1940s as follows.

> Yukawa devoted the greater part of his subsequent life as a research worker to the quest for a better, deeper fundamental theory. He published over twenty papers spanning a period of twenty years developing various approaches to this goal. Central to his thinking was the belief that the association of any elementary particle with a single geometrical point in space was in some deep sense mistaken; the key concept in many of his publications is the 'non-local field'. [...] We cannot see into the future and say with confidence that all the ideas presented in these papers are lacking in any grains of deeper truth that we do not yet perceive. And we cannot measure the stimulation that readers of his papers on the way to developing ideas of their own may have received. Even so it is a fact that in present day work one would be hard put to find reference to or influence of his later publications.

Kemmer's words seem to be a modest statement that Yukawa's second-half research was barren.

Brown [25] states as follows.

> The idea of nonlocal fields (which is to be distinguished from the idea of local fields having nonlocal interaction) gradually became a theory of elementary particles

> with internal structure. By the late 1960's it was superseded by Yukawa's concept of "elementary domain", based upon the quantization of the classical continuously deformable body. These fundamental ideas do not play a major role in current theoretical physics but may well be vindicated in a future physics.

Here, the last words "may well be vindicated in a future physics" give Yukawa fans hopes. However, Brown seems to have added these words, similarly to Kemmer's passage "We cannot see into the future ... we do not yet perceive," in honor of Yukawa, who had established meson theory and the method of particle physics at a young age.

Tanaka [26, pp. 57–58, 309–359] mentions Japanese-born researchers' evaluations of Yukawa's postwar research together with his view. From those evaluations, we quote below Nambu's one, which seems to be the outspoken and sharpest criticism (via [26, p. 311]; originally in [27]).

> Unfortunately, [Yukawa's postwar research] was not very fruitful. Aside from the relentless efforts he made for understanding elementary particles as things with a geometric spread, the content and method seem too naive. With the development of the gauge field theory, the geometrical view has become very important, and there is a possibility that we might reduce the internal quantum numbers to geometry. However, we cannot say that his idea was a seed of these developments. The influence he had on younger Japanese scholars since the theory of mesons was indirect.

Tanaka [28] points out that the D0-brane of string theory, about which we explain in §A.3, is close to the idea of Yukawa's elementary domain. However, the relationship between the two has not gone beyond Tanaka's speculation.

4 Research Methods of Theoretical Physics

4.1 Kamefuchi's Classification

Kamefuchi divides the research methods of theoretical physics into the *ascending* and *descending types* to consider why Heisenberg's and Yukawa's later studies remained uncompleted. He explains the two types as follows [1, p. 18]:

Ascending type One builds up the theory from the fundamental points.

Descending type One puts a hypothetical principle at a level far higher than the existing theoretical system and goes down from there to deduce all the physical laws.

Kamefuchi adds the following [1, p. 19] to the explanation of the latter type and makes the use of this one the reason for the two physicists' failures:

> For putting the hypothetical principle, one has to rely on intuition or analogy. Neither of these has objectivity or inevitability. So, one mostly goes astray.
>
> Both Heisenberg and Yukawa achieved results by the ascending type in the first half of their career. However, they turned to the descending one in the second half. [...] Thus, the two persons, unfortunately, failed to complete their studies.

The above conclusion becomes much persuasive when we consider the failures of the two physicists together with the case of another great theoretical physicist Einstein, about whom Kamefuchi writes [1, p. 19]:

> Frankly, Einstein was the same. He wasted thirty years of his later life with the far-reaching goal of unifying the gravitational and electromagnetic fields. Indeed, this kind of tendency might be the fate of physics heroes.

Kamefuchi earlier wrote about another classification of research in theoretical physics from a different aspect. We introduce it in Appendix B.

4.2 Nambu's Classifications

4.2.1 Original Version

Nambu [29, 30] also proposed classifications, similar to Kamefuchi's, of research methods in theoretical particle physics. Kamefuchi thinks that the research type can change between the first and the second half of a researcher. Nambu names his categories by the proper names of famous physicists as if a researcher uses a single method throughout her/his life. However, we should understand the naming refers to one of the successful studies done by the physicist of that name. Nambu calls his categories *Yukawa* and *Dirac modes* in [29]. We quote his explanations of these modes below, attaching more concise ones taken from Kaku and Thompson [31, p. 85] in curly brackets.

Yukawa mode The pragmatical one of trying to divine what underlies physical phenomena by attentively observing them, using available theoretical concepts and tools at hand. This also includes the building and testing of theories and models. Examples; Yukawa's meson theory, quark theory, GUTs (grand unified theories), [...].

{Deeply rooted in experimental data — Yukawa was led to his seminal idea of the meson as the carrier of the nuclear force by closely analyzing the data available to him.}

Dirac mode To invent a new mathematical concept or framework first, and then try to find its relevance in the real world, with the expectation that a mathematically beautiful idea must have been adopted by God. Examples; magnetic monopole, non-Abelian gauge theory, supersymmetry.

{The wild, speculative leap in mathematical logic that led to astonishing discoveries, such as Dirac's theory of antimatter or his theory of the monopole (a particle that represents a single pole of magnetism). Einstein's theory of general relativity would fit into the Dirac mode.}

Kaku and Thompson's explanation of Yukawa mode "Deeply rooted in experimental data" seems to have a nuance different from Nambu's "trying to divine what underlies physical phenomena by attentively observing them." However, we cannot say that these two expressions are inconsistent.

For Dirac mode, Kaku and Thompson cite Dirac's theory of antimatter and Einstein's theory of general relativity as its examples. This citing contradicts Nambu's additional description that these theories are rare examples of the two modes becoming a single, combined mode.

Kaku and Thompson also mention a combined mode, but its application target is Nambu's research. Namely, Nambu's friends considered a research type that combined the two. Then, they named it *Nambu mode* in commemoration of his 65th birthday in 1985. The related passages are as follows [31, p. 85]:

> This mode combines the best features of both modes of thinking and tries carefully to interpret the experimental data by proposing imaginative, brilliant, and even wild mathematics. The superstring theory owes much of its origin to the Nambu mode of thinking.
>
> Perhaps some of Nambu's style can be traced to the clash of Eastern and Western influences represented by his grandfather and father. [...]

We write about Nambu mode again later.

Nambu gave another name of *bottom-up*, similar to ascending type in Kamefuchi's classification, to Yukawa mode in his revised version. However, the original version lacks the category that contrasts with this. Dirac mode is close to Kamefuchi's descending type but different from it (the difference becomes clear when we see the revised version in §4.2.2). Nambu's purpose of the classification in Ref. [29], presented[7] at Kyoto International Symposium, The Jubilee of the Meson Theory, Kyoto, Japan, August 15–17, 1985, was to trace how the mainstream method of theoretical particle physics, established by Yukawa, has changed in modern times. Considering this, it is natural that we cannot apply Nambu's original version directly to explaining Heisenberg's and Yukawa's later failures.

4.2.2 Revised Version

Later, Nambu modified the original classification by adding another mode. The explanation for the revised version is as follows [30, pp. 31–33]:

Einstein mode (top-down) To create a theory by assuming that nature should follow a definite principle. Example: Einstein's theory of gravity (general theory of relativity), made under the assumption that generally, space may be curved.

Yukawa mode (bottom-up) To start from the working hypothesis that apart from deep reasons, some field or particle exists behind the new phenomenon. Examples: Yukawa's meson theory and Pauli's neutrino hypothesis.

Dirac mode (from heaven) To assume that a mathematically beautiful theory should be correct. Examples: Dirac's monopole theory; supersymmetry and string theories currently being explored.

Nambu gives further explanation of Dirac mode [30, p. 33]:

> The monopole's existence is now the natural consequence of the quantum field theory, but we still need to confirm it by observation.
>
> There is no experimental evidence that there is supersymmetry among various elementary particles. However, theorists hope that mathematically beautiful theories include some truth and are pursuing such [as supersymmetry and string theories]. So we can say that it is the heyday of Dirac mode nowadays.

By the way, the success or failure of supersymmetry and string theories is still unclear. Contrary to the theorists in the above description, Penrose [36], Woit [37], and Smolin [38] cast doubts on string theory, and Hossenfelder [39] criticizes theoretical physicists' obsession with beauty.

[7] The present author listened to Nambu's talk at the symposium.

In this version of Nambu's classification, the Einstein and Yukawa modes have become equivalent to Kamefuchi's descending and ascending types, respectively. However, we find the following faults in this version:

1. Dirac mode is out of place. Compared with the other two, this mode seems to refer to a different aspect. Further, another name for this mode *from-heaven* obscures its difference from Einstein (top-down) mode.

2. The general theory of relativity is not a suitable example of the top-down mode. We give the reason for this below.

Einstein used a thought experiment to come up with the general theory of relativity as explained by his own words (via Holton [32, p. 78]; the first two sentences also quoted in [33, p. 382]; the original, [34]):

> Just as in the case with the electric field produced by electromagnetic induction, the gravitational field has similarly only a relative existence. *For if one considers an observer in free fall, e.g., from the roof of a house, there exists for him during his fall no gravitational field*—at least in his immediate vicinity. For if the observer releases any objects they will remain relative to him in a state of rest, or in a state of uniform motion, independent of their particular chemical and physical nature. (In this consideration one must naturally neglect air resistance.) The observer therefore is justified to consider his state as one of "rest."

Namely, Einstein began the formulation of the general theory of relativity based on *an experiment*, and the method was, partially at least, Yukawa mode in Nambu's original classification. Kamefuchi means that Einstein turned to use the descending type (equivalent to Nambu's top-down) not from the study of the general theory of relativity but in his later years, as quoted in §4.1. This view is consistent with our thought but in disagreement with Nambu's.

The earliest paper in which Nambu's three-component classification mentioned here appears is Ref. [35]. There he uses the name *Yukawa–Sakata mode* in place of Yukawa mode. In another paper, the name *Heisenberg mode* replaces it, and we make quotes from this paper in Appendix B.

4.3 Our Proposal

Now, we consider removing the above faults in Nambu's revised classification and making it consistent with Kamefuchi's. We propose a new scheme for classifying the methods of theoretical physics. The basis of our idea is that Dirac mode is an aspect different from that of top-down and bottom-up ones. The scheme is as follows:

Aspect 1. Research standard

- α. **Esthetic** (Nambu's Dirac mode)
- β. **Realistic** This attaches importance to experimental results (including thought experiments) or theories already confirmed.

Aspect 2. Research directions

- A. **Top-down** (Nambu's Einstein mode)

B. Bottom-up (Nambu's Yukawa mode)

In the above, α, A, and B are the equivalents of the three modes in Nambu's revised version of classification as shown in parentheses. Aspects 1 and 2 are independent of each other. So, we consider combinations between them to get the following research modes:

Research modes

- αA. Esthetic, top-down mode Examples, the monopole, supersymmetry, and string theories
- αB. Esthetic, bottom-up mode (Nambu mode considered by his friends) Examples, Dirac's antimatter theory, Nambu's most studies.
- βA. Realistic, top-down mode Example, Einstein's general theory of relativity
- βB. Realistic, bottom-up mode Example, Yukawa's meson theory.

The use of two aspects and the name esthetic for Nambu's Dirac mode has removed the first fault of Nambu's revised version. We now attribute Einstein's general theory of relativity to mode βA (realistic, top-down) by considering his thought experiment at the start. Thus, the second fault has also disappeared. Note that the use of either category in research standards is possible at any time during a project.

Nambu mode considered by his friends corresponds to αB (esthetic, bottom-up) in our classification. This mode is irrelevant to the top-down one (Kamefuchi's descending type). Nambu had kept it all through his research life, and this fact should have been the reason for him to have been a productive researcher in particle theory for a long time (As for the summary of Nambu's research results, see Refs. [40, 41]).

5 Concluding Remarks

We have compared Kamefuchi's description of Heisenberg's tragic incident with other sources and found that Kamefuchi's suppositions at three minor points are wrong, as described in §2.3. On the other hand, we regard that Kamefuchi's explanation of Heisenberg's and Yukawa's failures in completing their theories in later years is persuasive.

Our literature survey has also revealed this: Heisenberg's work around the time of the tragedy, i.e., his unified field theory, affected another great physicist, Nambu, for finding spontaneous symmetry breaking in subatomic physics (Nobel-Prize winning work). However, Yukawa's later work has not had much impact on other researchers.

Kamefuchi used a scheme of classifying the research methods for explaining the great physicists' failures. Nambu also proposed similar ones. We have compared these classifications and found faults in Nambu's revised version. To remove them, we have proposed a new scheme, which separates two aspects of research. Using this scheme, we can say that Nambu's long-time productivity owes to his having kept esthetic, bottom-up mode.

Young theorists in the field of particle physics might want to be productive all through their research life. For them, it would be profitable to learn not only from Nambu's success at this point but also from the failures of Heisenberg, Yukawa, and Einstein.

Nambu says that now it is the heyday of the Dirac mode of theoretical physics in pursuing supersymmetry and string theories, while some other researchers express doubts about string theory and obsession with beauty. It would be interesting to keep an eye on what comes to theoretical particle physics in the future.

Acknowledgement

I heartily thank Naoki Toyota, Professor Emeritus, Tohoku University, for the following: He told me that there is a story in Ref. [15] about Bohr criticizing Pauli's lecture on the work with Heisenberg. He also gave me many helpful comments and suggestions throughout my way from preparing to finishing this report.

Appendix A Explanation of Technical Matters

In this appendix, we give a brief explanation about technical matters for the laypersons who want to know a little about them. The topics treated are Heisenberg's equation of cosmos, Yukawa's elementary domain theory, and D0-Brane of string theory mentioned in §2.3.2, §3, and at the end of §3.2, respectively.

A.1 Equation of Cosmos

We write here Heisenberg's equation of cosmos again:

$$\gamma_\mu \frac{\partial \psi}{\partial x_\mu} \pm l^2 \gamma_\mu \gamma_5 \psi (\bar{\psi} \gamma_\mu \gamma_5 \psi) = 0 \tag{A.1}$$

The technical name of this equation is the wave equation of the non-linear spinor theory. The meaning of the functions and parameters in Eq. (A.1) are as follows: ψ is a wave function (in the spinor form) of a particle, and $\bar{\psi}$ is its adjoint spinor. γ_μ's ($\mu = 0$, 1, 2, 3) are the Dirac matrices also appearing in the Dirac equation for all the spin-$\frac{1}{2}$ massive particles (we can find definitions of them in books on quantum mechanics). γ_5 is a matrix defined by

$$\gamma_5 = i\gamma_0\gamma_1\gamma_2\gamma_3. \tag{A.2}$$

The factor l^2 is a constant with the dimension of a square of a length.

Heisenberg thought that the solutions of the equation would "correspond to the structure of matter as it was then known," as explained in the quote from Polkinghorne in §2.3.2. In other words, Heisenberg expected that the theory would describe the properties and behavior of all the known elementary particles (including the numerical values of coupling constants involved [14, p. 1120]). This expectation came from the fact described by Heisenberg [11, Ch. 19]:

> [...] besides the space-time structure of relativity theory, it also contained the proton–neutron symmetry, [...], to put it in more mathematical terms, it contained not only the Lorentz group but also the isospin group — in other words, it seemed to account for a great many symmetries found in nature.

However, the equation had defects; (1) mathematical ones and (2) the contradictions found later between the physical evidence and prediction of the theory. The description of (1), as Pauli mentioned at the conference, appears in the quote from Polkinghorne in §2.3.2 ("Not only ... "). In Ref. [14, p. 1120], we see that an example of (2) is the coupling constant of the η-meson.

A.2 Elementary Domain Theory[8]

In introducing the concept of the elementary domain, Yukawa uses the local field $\phi(x)$ in the conventional, continuous Minkowski space x as the background and puts an infinite number of those domains in this space. Each elementary domain D has the characteristics of (1) taking different shapes and (2) being the smallest finite volume that does not allow further division. Then, D has different excited states due to the diversity of its available shape, and these states are the fields corresponding to various particles. The operator $\phi(D)$ describes these fields, replacing the former quantized local ones. The following equation defines $\phi(D)$:

$$\phi(D) \equiv \int_D \phi(x) d^4x, \tag{A.3}$$

where $\phi(x)$ is assumed to follow the 4-dimensional quantization.

The excitations realized for each elementary domain depends exclusively on its shape. This fact makes it an important issue how to determine many parameters to characterize the domain shape. Next, it becomes a problem to construct a new field equation to define the mutual relationship between the fields $\phi(D)$ in adjacent elementary domains. Because of the finiteness of D, this equation becomes the "difference" type instead of the conventional differential one. Further development of the theory beyond this stage remained unfinished.

A.3 D0-Brane of String Theory

First, we make a simple description of string theory. Looking back at the birth of this theory, we again find the name of Yoichiro Nambu. Kaku and Thompson [31, pp. 87–88] write about the circumstances that triggered the birth of the theory and the concept of strings as follows:

> Nambu originally proposed the idea of the string to make sense out of the chaos of the hundreds of hadrons being discovered in the nation's laboratories. Clearly, these hadrons could not be viewed as "fundamental" in any sense of the word. The disarray of strong interaction physics, Nambu thought, must be a reflection of some underlying structure. [...] Nambu's seminal idea was that the hadron consisted of a vibrating string, with each mode of vibration corresponding to a separate particle. [...] Strings come in two types: open strings (which have ends) and closed strings (which are circular).

Nambu proposed the above idea in "a well-known, unpublished (not contradictory) work [44]" as mentioned by Itoyama [45].

Next, let us see the D0-brane of string theory. Some years after Nambu and Gotō [46] independently used strings to describe hadrons (strongly interacting particles), different researchers began to apply it to all the particles. Then, a new version of the theory appeared. This version (also called superstring theory) has the symmetry that relates the fermions (particles with fractional spin) to the bosons (particles with integral spin).[9]

[8] In writing this subsection, we referred to Refs. [26, pp. 350–352] and [42], the first of which in turn relies on Ref. [43].

[9] One of the good books for lay readers to learn the history and physics of superstring theory is Ref. [47].

Further, the following situation has come. The string theory is no longer just the theory of strings extending in one spatial direction but also the branes (membrane-like objects) in the higher-dimensional space. D-brane is a particular type of brane corresponding to the allowed locations for open string ends. Physicists attach the dimension p of the D-brane after D to express the p-dimensional D-brane as Dp-brane. Thus, D0-brane is a particle extending in zero dimension, i.e., a point-like particle. Physicists do not know whether branes exist in the real world but know the types of branes that string theory says are possible [48].

Tanaka notices that the concept of D0-branes as the fundamental constituent of superstring is in close accord with Yukawa's idea of the elementary domain, despite the former's similarity to a point-like particle. The reasons are as follows [28]:

1. D0-brane is defined as a constraint on the ends of an open string and thus inseparable from its background.

2. In the N-D0-brane system, the position coordinates as a whole become $N \times N$ non-commutative matrices.

These two facts, Tanaka thinks, lead one to the idea of non-commutative space-time as supposed for the elementary domain.

Appendix B Kamefuchi's Earlier Classification of Research Styles

In this appendix, we introduce Kamefuchi's earlier three-component classification of theoretical physicists' research styles. The present author once wrote about it in a blog post [49], and the following description is an adaptation from it.

Kamefuchi published an essay on the different research styles of Japanese theoretical physicists [50]. At its beginning, he quotes the following words of Gramsci [51]:

> Passage from knowing to understanding and to feeling and vice versa from feeling to understanding and to knowing

Kamefuchi likens the three elements in the above quotes, i.e., feeling, understanding, and knowing, to the three stages of research in theoretical physics: (I) practitioner's stage, (II) theorist's stage, and (III) natural philosopher's stage. Then, he thinks in which stages Hideki Yukawa, Sin'itiro Tomonaga, and Shoichi Sakata were good at working or liked to work, to classify them into corresponding three types, I, II, and III, of physicists.

His friends called Sakata a person of methods, and his successful studies, i.e., the two-meson theory and the Sakata model of elementary particles, were phenomenological. From these facts, Kamefuchi classifies Sakata as type I.

Tomonaga had an excellent mastery of mathematics and expertise in constructing theories based on different physical requirements. These characteristics made him possible to produce "the super-many-time theory," leading to the renormalization method and winning him the Nobel Prize. From these, Kamefuchi classifies him as type II.

Yukawa's work to create a comprehensive theory of particles starting from "nonlocal fields" or "elementary domains" corresponded to the process of "going from knowing to understanding and to feeling," but was unfinished. However, Yukawa said in his later year, "Such a fundamental theory was my ultimate purpose. The meson theory was a byproduct on my way." Yukawa often expressed his opinions about various cultural problems such as creativity, genius, learning, and peace. This fact showed that he was an excellent thinker not only in physics but also in culture. Considering these, Kamefuchi classifies Yukawa as type III.

Kamefuchi's essay concludes as follows:

> It was lucky for particle theory in Japan that Yukawa, Tomonaga, and Sakata belonged to the three different types. The three leaders played the role of antithesis against each other so that the study of particle physics in our country made balanced progress. [...] I believe that such a history was the basis of the Nobel-prize-winning works by the physicists of the next generation, Yoichiro Nambu, Masatoshi Koshiba, Toshihide Maskawa, and Makoto Kobayashi.

Kamefuchi's classification scheme of physicists reminds me of a similar classification proposed by Nambu. His classification summarized by him is as follows [44, p. 10]:

> Once I classified theoretical physicists into three types according to their different styles of approach, and called them Heisenberg (H), Einstein (E) and Dirac (D) modes, referring to their most characteristic contributions respectively, i.e., quantum mechanics, theory of gravitation and the Dirac equation. Heisenberg's is heuristic, bottom-up and inductive. Einstein's is axiomatic, top-down and deductive. Dirac's is abstract, revolutionary and esthetic.

As for the modes to which Yukawa and Tomonaga belong, Nambu writes as follows [52, p. 10]:

> It would be safe to say that Yukawa belonged to H when he proposed the meson. He failed in E when he tried his hand at nonlocal theory. I have a bit of difficulty applying this to Tomonaga, but I will assign him to E. Most theorists belong to H or E. But, when it comes to contrasting Yukawa and Tomonaga, it may be appropriate to use the analogy to designer vs. craftsman.

Kamefuchi's type II and III seem to correspond to Nambu's H and E modes, respectively. However, when we consider the corresponding categories identical, it causes inconsistency between Kamefuchi's and Nambu's classifications of Yukawa and Tomonaga. The inconsistency comes from the difference in the viewpoint between Kamefuchi and Nambu. Namely, Kamefuchi attaches importance to the physicist's preference of a principle, especially for the application to Yukawa, and Nambu, on a physicist's approach to his successful work.

On the other hand, Kamefuchi's type II and type III seem to correspond to Nambu's category of craftsman and that of designer, respectively. Here, we can regard the corresponding categories as nearly equal without causing inconsistency. The consistency arises because Nambu's craftsman–designer classification is from the physicists' methodologies, similar to Kamefuchi's.

From the above description, we notice that we can add one aspect or two, i.e., either Kamefuchi's *research styles* or the aspects of *research principle* and *research methodology*, to our classification scheme in §4.3. We expect that some other person would consider the proper extension of our classification and its application to historical studies of theoretical particle physics.

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Corrigendum to 'Heisenberg, Yukawa, and Nambu: Commentary on, and Thoughts Derived from, Kamefuchi's Essay' [ISAAC-TR 1 (2021)]

Tatsuo Tabata

May 16, 2021

The author regrets that there are terminological errors on page 15 (§A.1) and give corrections below.

Place	Now reads	Should read
l. 1 below Eq. (A.1)	wave equation	field equation
ll. 2–3 below Eq. (A.1)	a wave . . . particle	the field operator (in the spinor form)