

Chapter 2

The Father of Biophysics in China

Abstract This chapter intends to elucidate the academic heritage and intellectual background of the father of biophysics in China. My central argument is that if we want to learn about biophysics in China, we have to learn more about Bei Shizhang; if we want to understand Bei, we have to pay a visit to his intellectual world. But this world is complex and many-faceted, for Bei is not just an institution builder and a visionary leader, he is also a charismatic leader and a controversial scientist. This chapter explores these various facets of Bei Shizhang by chronicling his early life in China, the correlation between the formation of his scientific worldview and the *naturphilosophie* of his neo-Lamarckian German teacher, Wilhelm Harms. I also consider Bei's contested scientific pursuit—the theory of cell reformation which had some parallels with O.B. Lepeshinskaya's now discredited studies of “the origins of life” in the Soviet Union—within the larger sets of issues involving Soviet-styled biology, the perception of Marxist philosophy, and political patronage of cytology in twentieth-century Communist states.

Keywords Bei Shizhang • Cell reformation • Father of biophysics in China • Tübingen • Wilhelm Harms

When Bei Shizhang passed away in Beijing in 2009, there were two major details that captured the media's attention.¹ First, at the age of 106, he was the oldest academician in modern China, being among the first batch of scientists to be elected to the *Academia Sinica* in 1948, right before the Nationalist government retreated to Taiwan. He was the last academician whose scientific membership straddled across the Nationalist and Communist regimes, and after 2004, Bei became the only surviving academician who witnessed the transformation of modern China from the “Republic of China” to the “People's Republic of China.” The second detail was that he was lauded as the founding father of biophysics in China. People were told that biophysics could not have materialized in China without him. Biophysics and

¹There were many news reports, videos, and articles between October and November 2009 on Bei's death and legacy. See the following for some of the most representative ones: Committee of the Funeral Service of Bei Shizhang (2009), Zhao and Chen (2009), *China News* (2009), *Xinhua News* (2009), *Science Times* (2009), *People's Daily* (2009).

Bei Shizhang were synonymous in China, and this was represented through Bei's pioneering efforts in synthesizing physical and biological sciences (Wang 2009; *Science Times* 2010).

In 2003, a *festschrift* commemorating his centennial birthday entitled *Bei Shizhang and Biophysics* was released. Several of his students and colleagues enumerated his tremendous contribution to laying the groundwork of biophysics in China. The biographical narrative did not merely center around the personal virtues of Bei as an indefatigable scientist, but also as a deep thinker, an institution builder, and a visionary disciplinary leader. These various facets of his life painted a human picture of Chinese biophysics, as Bei's intellectual commitment and political conviction personified the biophysics program in China.

I assume most English readers have never heard of Bei Shizhang, let alone his contribution to biophysics. Therefore, this chapter will give a general biographical sketch of Bei Shizhang stressing his intellectual commitment and academic heritage. It is my assertion that if we want to learn about biophysics in China, we have to learn more about Bei Shizhang. In what follows, I focus on the academic background and scientific contributions of Bei Shizhang—the first and arguably the most important biophysicist in China—in order to investigate the epistemic origins of biophysics in contemporary China.

2.1 Bei Shizhang: the Early Years

The early life of Bei Shizhang, as chronicled by Bei's student Ying (1992) spanned from his birth in 1903 to his return to China from Tübingen in 1929.²

The first stage of Bei's life stretched from his childhood to his undergraduate education in China and graduate training in Germany, which was chronicled by Ying Youmei, while Zhang Jianjun (张坚军) presented a much simplified version of Bei's pre-schoolings and familial life intended for juvenile readers (Zhang 2002). Both accounts were written in Chinese. For the purpose of my analysis, the most important round of events in this stage was his graduate experience in Germany. Below I will reiterate only important milestones in his pre-German phase.

In 1903, Bei Shizhang was born to a fisherman's family in the coastal village named Zhenhai (镇海) in Zhejiang province. As the name of the fishing village implies—Zhenhai means literally to conquer (*zhen*) the sea (*hai*)—it lies along the seaside frontier facing the South China Sea, overlooking the Formosa Strait. Bei came into this world at an unpropitious time, for China was steeped in the

²In chronicling Bei's life and work, Bei's student Ying Youmei divided his timeline into three stages: the first stage spanned from his pre-German schooling and German education to his return to China where he was appointed as chair of the biology department at Zhejiang University (*Zheda*), a position he held from 1930 to 1949. The *Zheda* period was stage two. In 1950, he moved to Beijing to establish and organize the Institute of Biophysics of the Chinese Academy of Sciences. That was stage three.

interregnum period after the Boxer Uprising and before the establishment of the Republic of China. Bei was the first one in his family to get formal schooling. As much as he enjoyed eating salted fish and toying with herring, he knew from an early age that getting good grades were far more important than knitting a tight seine. He had witnessed how hard his parents toiled day and night, on and off the boat, making barely enough money to make ends meet. Life staged on the maritime and wartime theatres was harsh and demanding.

Although he had to swallow more pain than food at an early age, he had always believed in the promising engine of education fueled by science. The primary and secondary schools he had attended, namely the Jin Xiu School (进修学堂), Bao Shan School (宝善学堂), and De Hua School (德华学校) were German-run charities in China, so he had studied the German language for several years before going off to college. His father encouraged him to further study foreign language, but Bei was certain that his real interest was in life science. In 1919 he was admitted to the Tongji Technical College of Medicine and Engineering, TTCME (同济医工专门学校), formerly the German-founded Tongji School of Medicine and Engineering (同济医工学堂) and graduated with a pre-medical degree in 1921.

His college completion coincided with the defeat of Germany in World War I. The currency depreciation after the war made graduate education in Germany affordable for Bei. It was estimated that his tuition fee in post-WWI Germany was almost the same as that of TTCME. Along with his two friends from TTCME, he boarded the ocean-liner *Amazon* bound for Marseilles in the summer of 1921. Their travels into inland France finally landed them on a train bound for Freiburg (Wang 2010a, pp. 19–25).

The three Chinese students chose the Universität of Freiburg because their pre-medical credentials from TTCME were accepted by Freiburg. Bei's TTCME companions Yu Hongkang (余鸿康) and Li Yuanshan (李元善) applied immediately to the School of Medicine at Freiburg but Bei applied to the School of Natural Philosophy instead. Although he held a pre-medical degree, he had always been interested in a broad-based education in natural science. He took advantage of this opportunity to take various classes by renowned scholars in their fields, including zoology with Nobel-prize winner Hans Spemann, botany with Friedrich Oltmann, physics with Franz Himstedt, chemistry with the Nobel laureate Heinrich Otto Wieland, anatomy with Eugen Fischer, and pathology with Ludwig Aschoff.

In 1922, he moved to Munich hoping to study with the famous zoologist Richard Hertwig. He continued his multifarious coursework at the Universität der München, taking chemistry with Nobel-prize winner Richard Willstätter, physics with Wilhelm Wien, botany with Karl von Göbel, paleontology with Ferdinand Broili, and geology with Emmanuel Kayser. Years later, his multi-disciplinary transcript would prove useful for building the Institute of Biophysics in China as the next chapter will disclose.

Bei's original plan was to undertake a doctoral project under the supervision of Hertwig. As Hertwig was about to retire, he referred Bei to Friedrich Blochmann at Tübingen. So, in 1923, Bei left the cosmopolitan city of Munich for the small university town of Tübingen. Yet it turned out that Blochmann was going to retire

too. Bei was directed to conduct experiments on the cell constants (*zellconstanz*) of the parasites *oxyuris obvelata* and *o. tetraptera* under the supervision of Richard Vogel for two years until the arrival of Wilhelm Jürgen Heinrich Harms.

2.2 Wilhelm Harms

It was a long journey before Bei could embark on his PhD research. From Freiburg to München to Tübingen, from Spemann to Hertwig to Vogel, he finally settled down with Wilhelm Harms as his advisor. According to Ying's documentation, Harms was one of the earliest surgeons in sex reassignment operations. Along with Voronoff and Steinach, Harms was famous for their pioneering work on "rejuvenation" research. Ying drew upon his writings on various methods of "rejuvenation" from his two-volume *Body & Germ Cells (Körper und Keimzellen)* published in 1926. In the first volume, Harms reviewed the nature and history of germ cells and their interconnections with the body.

A germ layer is a group of germ cells formed during embryogenesis. The germ layer theory was the cornerstone of Ernst Haeckel's biogenetic laws, in which he maintained that only after the germ layers were formed in the gastrula was there structural differentiation. The biogenetic theory was controversial partly because it dealt with the relationship between individuals and groups (or ontogeny and phylogeny). Haeckel saw individual organisms repeating the ancestral reminiscence in the course of development, which was known as "recapitulation theory." The theory was chiefly about to what extent features were inherited from our ancestors and which were epigenetic adaptations to external environment.

Maienschein (1978) has assessed the opposing views expressed by Wilhelm His, E.B. Wilson, E.G. Conklin, and F.R. Lillie as to whether "ontogeny recapitulates phylogeny." According to Maienschein, Wilhelm His subscribed to hereditarian thinking in explaining embryonic development, while E.B. Wilson held a more balanced view of the relative roles played by hereditary and epigenetic factors. Both His and Wilson denied a "true recapitulation" of ontogeny by phylogeny.

More relevant to Harms' proposition are E.G. Conklin and F.R. Lillie. E.G. Conklin shared Harms' interests in the origin and formation of the germinal layers. Like his contemporaries, Conklin (1897) was drawn to the developmental history of fertilized eggs. But his investigative lens was placed under the cytological changes that occur during cellular differentiation. The significance Conklin attached to cleavages stemmed from his assertion that a comparative inquiry of forms and causes of cleavages in closely related organisms would elucidate the homologies of blastomeres in different animals. Conklin distinguished between different types of cleaving ova among various types of organisms. He considered the various types of cleavages (determinate and indeterminate cleavages) as a reflection of diversity in the natural world, for "Nature is continually performing some very remarkable experiments in her own way." (Conklin 1897, p. 4). Conklin found E.B. Wilson's sweeping generalization of the "hereditary tendency" to all organisms questionable.

Conklin showed that a comparative study of cell lineages in closely and not-so-closely related organisms was valuable to clarify the ontogeny-phylogeny relations. The last one in Maienschein's cell-lineage circle was F.R. Lillie. Lillie also objected to E.B. Wilson's omission of cleavage heterogeneity at the early dividing stages. Drawing from his studies on the life history of the mussel *Unionidae*, Lillie argued that the early cleavage stages were not simply ancestral reminiscences but adaptive features to the later adult stages. Both Conklin and Lillie insisted that the early differences in cleavage were critical to later development. Maienschein carefully showed that despite their minor disagreements, these four biologists found Haeckel's biogenetic laws inadequate to explain the ontogeny-phylogeny relations. Ontogeny did not just "recapitulate" phylogeny. As Lillie summarized, "The ontogeny is inherited no less than the adult characteristics, and is subject to precisely the same laws of modification and variation." (quoted in Maienschein 1978, p. 153)

Against this historical backdrop of ontogeny-phylogeny debate, we can now proceed to consider some of Harms' legacies. Compared to the American orthodoxy on ontogeny, Harms' takes on the phylogenetic issues were somewhat heretical. To begin with, Harms was an outspoken "neo-Lamarckian" when it came to "*des Erwerbs neuer adaptiver Eigenschaften*" (the acquisition of new adaptive features). Hull (1984) has pointed out that it was inadequate, sometimes misleading and even defamatory, to call someone a "Lamarckian" not only because "nearly every type of hereditary phenomenon has been termed at one time or another Lamarckian," but also because "at times Lamarckian was a pejorative term to be used to characterize the views of one's opponents." (Hull 1984, p. xliii). In the case of Harms though, the label "neo-Lamarckian" was given to Harms not by me but by others (Potthast and Hoßfeld 2010). But I agree with Hull that what was more meaningful than borrowing the name of Lamarck was an elaboration of the specific "Lamarckian" or "neo-Lamarckian" notion being discussed.

In essence, the following excerpt exemplifies Harms' "neo-Lamarckian" philosophy (my translation)³:

The more we immerse ourselves into the thinking of Darwin, which includes the non-vitalist Lamarckism, the more we recognize that it has also nowadays unlimited validity: the formation of species and with it evolution is in its course mechanistic, it is determined by the environment [...]. At first, it is of little importance whether the resulting, newly adapted form is only a permanent modification (somatic mutation) or a heritable one (idiomatic modification). Under correspondingly enduring and stable environmental conditions, the former will lead to genetic changes or the formation of new genes and radicals (Potthast and Hoßfeld 2010, p. 442).

Harms' attempt to reconcile the Lamarckian and Darwinian positions was quite clear in the above quote. In this sense, Harms fit the archetypal "neo-Lamarckians" in Hull's schema, in which "the neo-Lamarckians did not form a group but worked largely in isolation from each other. Instead of viewing themselves as renegades,

³I thank Raffael Himmelsbach for his help with the German translation.

they tended to see themselves as conservatives championing a wider view of evolution against the overly restricted position of the neo-Darwinians.” (Hull 1984, p. 1). Harms saw the great Darwinism–Lamarckism divide in terms of relative differences rather than absolute contradiction. As he framed it, although the Darwinian theory of natural selection may never be discarded, Lamarckian theory was indispensable for explaining the emergence of functionally adaptive traits. Despite the fact that Harms was an adherent of the doctrine of “the inheritance of acquired characteristics” (*Vererbung erworbener Eigenschaften*), he was more articulate about the interplay of Darwinian and Lamarckian paradigms than about the exact mechanisms by which the acquired characteristics were inherited.

Like most neo-Lamarckians, Harms was a synthesizer who set himself the task of harmonizing seemingly contradictory systems. He wanted to bring the plausibility of environmental modification into the Darwinian program. Not just because Darwinian evolution was “cruel, wasteful, and opportunistic,” but also because it ignored the possibility that evolution might not be as gradual as Darwin thought it was (Hull 1984, p. iv). Harms followed the Lamarckian convention to call upon at least considering environmental influences in addressing how species change rather than just natural selection (Burkhardt 1984). For Harms, recording adaptive responses to the environment and its philosophical implication for the Darwinian framework preceded answering specific questions in Lamarckian inheritance such as what the “*langandauernden und gleichmäßigen Umweltbedingungen*” were and how mutation can be organically passed on to the next generation.

Harms’ take on the ontogeny-phylogeny relationship and his developmental thesis were inseparable from his Lamarckian worldview. For the purpose of illustration, I will briefly discuss two of his seminal works: cytoplasmic investigation of *Unionidae* development and the theory of germ cells.

The postembryonic developmental phases had always intrigued Harms, and he was attentive to recording the general conditions under which regeneration occurred. One of his best-known studies concerned the parasitic development of glochidia on *Unio* (Harms 1909). Glochidium was a parasitic larva feeding on freshwater mollusks. When attaching segments of glochidia to the host, the epithelium cells of the mussel around the glochidium began to proliferate. Harms suggested that the proliferation of epithelial cells was caused by the external implantation of glochidium upon the host cells. This was evident in the vigorous multiplication of epithelial cells just below the glochidium. Harms further described a slight growth of the shell of *Unio* as an indicator of external changes of the glochidia, although his interpretation was disputed by other researchers (Young 1911).

In *Körper und Keimzellen*, Harms discussed the origin of germ cells from the dynamics of post-embryonic differentiation and particularly the likelihood of regeneration of germ cells from somatic cells. He bypassed the Weismann-Roux “*Descendenztheorie*” which relied on the preformation of germ-plasm to explain regeneration. Instead, Harms drew upon his studies on the formation of germ cells of the opposite sex from the peritoneal epithelium (abdominal lining) to build an alternative theory of germ cells. He highlighted the acquired ability of regenerative capacities of germinal epithelium from which germ cells were derived. Noteworthy

was the way in which he attributed the regeneration of germ cells to epigenetic manipulation (from transformed somatic cells) rather than pre-existing conditions (preformed germ-plasm). From Harms' perspective, the origin of germ cells was an affirmative example in favor of the environmental cause of genetic changes. It was changes in the organism's somatic environment that triggered the proliferation of epithelial cells that lined up the sex glands.

Does inheritance from changes of somatic environment count as an occasion of Lamarckian inheritance? David Hull wrestled with this question skillfully by demonstrating that it was semantics that was really at play. Here "semantics" was used to underline the careful use of terms and definitions for making necessary distinctions among closely related phenomena. By "Lamarckian inheritance," one could refer to the transcription of adaptive changes in the organism's body into hereditary materials and the subsequent passing on of these materials to the offspring. Or, one could consign "Lamarckian inheritance" to the surmise that phenotypical traits from one organism could transmit directly to another organism without touching upon any genetic materials. These were two very different ideas that were entangled under the "Lamarckian" rubric. Harms' proposition resembled the first variant more, but as Hull interpreted the issue, the crux here was not whether adaptive characters could be transmitted at all, but over the origin and the specificity of the transmission of those adaptive traits. Even if Harms made a convincing case that the "transformed somatic cells" could be transmitted to the germ cells which later transmuted the reproductive organs, he did not prove that the environment was the principal force for heredity and evolution because those changes in the somatic cells were very likely the result of a selection process rather than environmental induction. In plain language, our bodies and genes change over time as we adapt to the environment, but these adaptations cannot pass over to the next generation immediately. As a species, we can only change slowly and over a long course of natural selection. We modify gradually and somewhat accidentally, not inductively.

2.3 Harms and the Nazis

For my purposes, it is important to distinguish what Harms had achieved from what he left open. His objection to Weismann's presumed separation of germ cells and somatic cells proved prescient. Reproductive cells and hereditary materials were not independent from the rest of the body. Those who subscribed to a rigid specialization of cells would be taken aback by the later technology of cellular reprogramming. A case in point was the successful induction of pluripotent stem cells from adult somatic cells by a Japanese team. The lead scientist, Shinya Yamanaka, was awarded the 2012 Nobel Prize in Medicine.

What Harms did not do was to give an adequate elaboration of the mechanics for Lamarckian inheritance, let alone the relative contribution of ancestral, inherited, and adaptive factors in shaping ontogenetic development. No one, not even J.B.

Lamarck himself, has accounted for the mechanisms of inheritance of acquired characteristics in persuasive and conclusive details (Burkhardt 1984). Recently, some evolutionary biologists have argued that it was unfair to reduce the scope of Lamarck's thinking to merely the inheritance of acquired characteristics. Writing slightly before he died, Stephen Jay Gould (2002) championed a more broadly conceived landscape of Lamarck's evolutionary theory.

So why didn't Harms straighten out the record once and for all? Why didn't he go ahead and lay out the mechanisms of Lamarckian inheritance? To address these questions, it is helpful to turn to David Hull, who has tried to explain why the Lamarckian theory of heredity and evolution was difficult to illustrate experimentally. It was not that Lamarckians were less scientifically rigorous than Darwinians, but to sufficiently demonstrate the vast environmental impacts on short-lived organisms was very difficult, if not inconceivable. Environmental variations are simply too immense and enduring to be internalized by the genetic capacity of an organism within a limited life span. But demonstrable falsifiability was not the main reason for the widespread unpopularity of Lamarckism among evolutionary biologists. Both David Hull and Richard Burkhardt have pointed out that the unjust condemnation Lamarckian advocates received had more to do with the power dynamics and social divides in the scientific community than Lamarckism's empirical deficiency. Darwinian champions won along with Mendelian genetics and the discovery of DNA while Lamarckian followers lost with the Kammerer and Lysenko affairs. Scientific controversy was never just about science; it was also about the social and political phenomena in the wider culture and how people treated each other.

As it turned out, Lamarckism was not just a disreputable position among scientists, it was perceived as a political "crime" in the eyes of the beholder. Harms' "neo-Lamarckian" way of thinking was to cause him troubles with the bureaucrats of the Third Reich.

In the winter of 1935, Harms was appointed to the renowned position of "Haeckel Chair" of the department of zoology at the University of Jena. Connected to the directorate of the Zoological Institute at Jena was that of the Phylogenetic Museum. For a while, Harms enjoyed dual positions of privilege. But Harms' Lamarckian view clashed with the ideology of the Nazis. In 1938, Harms was relieved from his directorate of the Phylogenetic Museum because of his Lamarckian beliefs. Harms' antagonistic relationship with the political authority intensified when his former assistant at Tübingen—Gerhard Heberer—was appointed to the directorship of the Phylogenetic Museum at the behest of SS (*Schutzstaffel*) commander Heinrich Himmler. As Heberer was promoted to SS-First Lieutenant (*SS-Obersturmführer*), Harms was forced to resign from his Haeckel professorship. Harms was rehabilitated after World War II in 1946 and became the rector of the University of Jena but his petition to return to the Zoology Institute at Tübingen was denied. As Potthast and Hoßfeld (2010) lamented, zoology at Tübingen was in free-fall afterwards as the scientific team was squeezed out by the Nazis cliques. Both now and then, scientific conviction has never been completely isolated from political intervention. Harms' case was no exception.

The biographical profile of Harms is explored at length here because of the significant impact of Harms on Bei's scholarly development. It is impossible to grasp the host of scientific and political issues that puzzled Bei throughout his life without immersing ourselves first into Harms' intellectual world. To assess Harms' lasting influence on Bei, we now turn to Bei Shizhang and his theory of cell reformation.

2.4 Bei Shizhang and His Theory of Cell Reformation

Despite much respect to Bei Shizhang's lifelong dedication to furthering the development of scientific enterprise in modern China, academic renown was still missing from his list of accomplishments. Bei's biggest regret was that he didn't succeed in advancing the cell reformation theory that he proposed in the early 1940s. After more than seventy years, Bei's attempt at giving an alternative interpretation of cell differentiation is still met with resistance from the mainstream scientific community. The lack of international recognition of Bei's scientific achievement led some Chinese scientists to mock him as a "centenarian" (百岁老人), insinuating that Bei was just an old codger without much scientific credibility other than his longevity.

I will first describe the content of his theory of cell reformation, followed by explanations on what motivate his research inquiry. These background understandings, in my opinion, are helpful for making sense of the controversy surrounding his theory.

Bei's theory of cell reformation casts doubt on the prevailing cytological paradigm exemplified by the Virchowian motto *omnis cellula a cellula* (all cells come from pre-existing cells) by drawing on the result of his empirical study of the developmental morphology of an autochthonous prawn in China.

The story of Bei's iconoclastic research typically took us back to 1932 (Bei 1992). The official story began with his finding of an intersex strain of an arthropod in the swampy field of Songmuchang on the outskirts of Hangzhou. Indigenous to the Chinese southeastern shores, its unusual reproductive process captured the curiosity of this German-returned zoologist. *Chirocephalus nankinensis* (南京丰年虫),⁴ as it was called, was a fascinating shrimp for Bei because of its unusual mechanism of cellular proliferation during sex change. The sample breed under his investigation was of hermaphrodite nature, setting *c. nankinensis* apart from other sexually dimorphic arthropods. Microscopic observations showed that characteristics of both sexes co-existed in a typical *c. nankinensis* strain. Although *c. nankinensis* combined

⁴The English scientific name was coined to capture the fact that this insect is a Nanjing (aka Nanking)-originated species (*nankinensis*) in the family of *chirocephalus*. The Chinese name was given by a folklore belief that the appearance of this shrimp in wintertime, along with heavy snowstorms, is a harbinger of a productive year as summarized in the Chinese proverb "heavy snow forecasts a good harvest year" (瑞雪兆丰年).

within one organism both male and female characteristics, the distribution and proportion of these sex features were not uniform among all types. According to the relative sexual traits from the specimens he collected, Bei divided them into male and female intersex types. He noted that at certain developmental stages, both sexes underwent sex reversal. The female intersex transformed into male and male into female. He further sub-divided the female intersex into weak, middle, and normal female intersexes while the male intersex was divided into weak and normal male intersexes by virtue of their secondary morphological features. Thus, there were altogether five types of intersex strains of *c. nankinensis*. When the weak male intersex (弱勢雄原中间性) underwent gonad reversal, the germ cells disaggregated their cellular contents into yolk granules or substances similar to yolk—a process which he called “cell deformation” (细胞解形)—then re-aggregated into an adult cell incrementally from these chromatin-bearing entities during the transformation of germ cells from weak female intersex (弱勢雌原中间性) to weak male intersex. Bei argued that the yolk granules outside the cytoplasm of the oocytes “reformed” from yolk granules in this manner, which he called “cell reformation.” The new germ cells were formed not by cytokinesis but rather generated from the corpuscular substances, i.e. the yolk granules. His conclusion was that it was possible for cells to reproduce by means other than cell division. “Cell reformation” was thereafter hailed as an alternative way by which cells multiplied.

The above episode was probably the most well-known part of Bei's work on the theory of cell reformation. It was published in a Chinese reminiscence article by Bei himself in 2003, nearly seventy years after he had first conducted the research (Bei 2003a). After Bei's death, his long-time assistant and one of his vocal supporters—Wang Guyan wrote a short English sketch in remembrance of Bei's cell reformation theory. Published in 2010, Wang's cameo stood as the only existing writing in the Anglophone community about Bei Shizhang and his work on cell reformation (Wang 2010a, b). A lack of publications in English journals was a blot on Bei's escutcheon. Those who denounced Bei and the theory of cell reformation held that if his theory was really credible, it should have appeared in either *Nature* or *Science*, if not both. The absence of an imprimatur from the mainstream Anglo-American authorities was iterated as indicative of the inferior quality of his work. One whistle-blower who styled himself as the “science cop” in China went as far as calling the theory of cell reformation “pseudoscience” and Bei a “third-rate scientist,” an accusation that I will explore later.

My purpose here is not to rehabilitate Bei's theory or reputation, but I think it is necessary to consider the entirety of his work before jumping to any conclusions. For better or worse, the familiar story was an over-simplified tale of a man with a complex and eventful life. The official saga portrayed Bei as a genius who challenged a cherished foreign theory with an indigenous Chinese organism. Bei and his admirers also perpetuated this myth of a scientific hero by emphasizing his ambition to undercut existing views on cell differentiation. Ever since Bei wrote the first research article on *c. nankinensis* in German, he had relentlessly cast his theory as an alternative to the traditional viewpoint held by Rudolph Virchow. He was determined to refute Virchow's claim that cell division was the only way by which

new cells generated by upholding his cell reformation theory as undermining the *philosophia perennis* in cell biology (Bei [1943a], [1943b] 1992).

Among the many important questions obscured by this abridged story was what inspired Bei to pursue an alternative cell theory. His eureka discovery of *c. nankinensis* in Hangzhou was one thing, yet how to make sense of what he saw under the microscope was another. It was not just a matter of instrumental conditions. Translating empirical findings into a theory did not follow a linear pattern because theorizing cellular activities required an agenda to determine the proper relationship among cellular and subcellular organelles. It entailed much more than the ability to optimize the quality of pictures and the extent of visualization. With advanced microscopes and the right techniques of preparation, a set of working assumptions is required in order to interpret the biological phenomena in a petri dish. In other words, what matters most is not how much one can see, but where to look for what one assumes is being seen and how to decide what to make out of it. In Bei's study, why did he focus on the sexual metamorphisms and not other structural and functional aspects of *c. nankinensis*? What drove him to divide *c. nankinensis* into five categories? Where did he get the ideas of "deformation" and "reformation?" In short, what led him to such a particular cellular interpretation?

The *naturphilosophie* of Wilhelm Harms, which informs his research on the relationship between the endocrinal secretions of sex glands and the whole organisms, holds the key to the above questions. Bei's formulation of the cell reformation theory reflects the developmental worldview espoused by Harms (1939).

According to Ying Youmei, Harms' emphasis on the entire developmental cycle made a permanent impression on Bei. To probe into the regenerative power of the labile tissues and organs such as the primordial germ cells, Harms divided the life cycle of a typical organism into three stages: *progressive periode*, *stationäre zu-stand*, and *regressive periode*. Ying remarked that "Bei appreciated Harms' philosophy of addressing a problem comprehensively from a developmental point of view." (Ying 1992, p. 5)

Ying's analysis of Harms' influence on Bei's intellectual evolution captures the correlation between Harms' developmental philosophy and Bei's framework on cell reformation. Harms stressed sexual physiology and regeneration of germ cells whereas Bei studied morphological changes of germ cells during sexual metamorphosis; Harms looked at growth and developmental processes whereas Bei examined the cytoplasmic changes during cell differentiation. More strikingly, Bei adopted Harms' terms to illustrate the core concepts in the theory of cell reformation. Bei attributed the transition from male to female intersex to the "*regressive periode*" (退行性) while the female to male intersex to the "*progressive periode*" (前进性). Not only were "regressive period" and "progressive period" a direct importation of Harms' language; the ideas were the primary conceptual grounds for building Bei's theory. The "regressive period" corresponds to the "deformation" (解体) of germ cells and the "progressive period" to the "reformation" (形成) of reproductive cells. Harms' work was the key to unlock Bei's philosophical foundation.

Even before Bei postulated the theory of cell reformation, Harms' scientific style already colored the topic, driving questions, and analysis of Bei's doctoral thesis. Using the nematode *anguillula aceti* (a type of vinegar eel) as the research material, Bei (1928) embarked his doctoral research on an "experimental-morphological investigations on nematodes" (*experimentell-morphologische untersuchungen an nematoden*). He proposed to study the complete life cycle rather than just the embryonic stage. One of the objectives of his dissertation was "to follow the entire life cycle" (*den gesamten Lebenscyclus zu verfolgen*) of the nematode. His analysis of the development of *a. aceti* followed the idioms of *progressive*, *stationäre*, and *regressive phasen* introduced by Harms. In addition, Harms' abiding interest in sex determination and germ layers left an indelible mark on Bei's discussion of the findings. Bei's dissertation incorporated a number of handwritten drawings on "the growth of sex cells and germ cells" (*wachstum der geschlechtzellen und keimzellen*). "Germ-line and the determination of somatic cells in the formation of the germ layers" (*keimbahn und die determination der somazellen bei der bildung der keimblätter*) was a major theme in Bei's mapping of the morphological differentiation of *a. aceti*.

In short, the reason Bei focused on sexual metamorphisms rather than other physiological aspects of *c. nankinensis* is a legacy of Harms' enduring interest in sex transformation and reassignment. Harms' tripartite analytical structure also exhibited a discernable influence on Bei's framework and his choice of words in categorizing the intersex strains of *c. nankinensis*. Harms and Bei had a lot in common as both were attracted to the developmental mechanics of living organisms, with a special emphasis on the differentiation of germ cells.

The intellectual continuity between Harms and Bei is important for considering the kind of oppositions against Bei. The objectionable nature of neo-Lamarckian cause of inheritance is an underlying factor, but the controversy of the theory of cell reformation is complicated by the political interests revolving around the issue of cell origin.

2.5 Bei Shizhang, O.B. Lepeshinskaya, and the Controversy of Cell Theory

On 24 August 1964, during his meeting with the Japanese physicist Shoichi Sakata in the company of Chinese physicist Zhou Peiyuan, Mao Zedong enunciated his intellectual interest in the origins of cells: "We should study the origins of cells. The cell has its nucleus, a mass of protoplasm, and a membrane. The cell is organic, and so there must have been non-cellular forms (cytooes) before there was the cell. What was there before the cell was formed? How was the non-cellular form changed into the cell?" (Mao 1964).

Apparently, his interest in this biological area was partly triggered by the work of a Soviet biologist: "there is a woman scientist in the Soviet Union who has been

studying this problem, but no result has been reported.” (Mao 1964). The “woman scientist in the Soviet Union” in Mao’s speech was Olga Borisovna Lepeshinskaya. At that time, Mao probably did not know that before his declaration of interests and even before China was governed by his regime, Bei Shizhang was already intrigued by Lepeshinskaya’s work.

In 1943, Bei published a Chinese research paper entitled “Yolk Granules and the Reformation of the Cell” (卵黄粒与细胞之重建). It was contained in the inaugural issue of the Chinese journal *Science*. It was also the first place in which his theory of “cell reformation” was introduced to the scientific community to explain the process of cell differentiation at a time when the world was set ablaze by the flames of WWII.

Bei’s 1943 paper started off by reviewing the research efforts made by the Soviet biologist O.B. Lepeshinskaya between 1936 and 1937. Her name is now infamous among historians of science, and for good reasons. In May 1950, O.B. Lepeshinskaya rose to superstardom in Stalinist Russia with her “new cell theory.” As an anti-Virchowian, Lepeshinskaya claimed to have proved that the basic properties of an organism were contained not in cells but in some amorphous “vital substance.” It was later revealed that the alleged bearer of all vital processes and materials for producing cells—“vital substance”—was founded upon Stalin’s blessing rather than replicable results. Her reputation was completely destroyed as her experimental results failed to stand the test of other scientists (Zhinkin and Mikhailov 1958). Her coronation as a famous scientist by her receipt of a Stalin Prize and election to the Soviet Academy of Medical Sciences were glacially described by a former Soviet physician as representative of “a carefully staged farce of collective ecstasy for the ‘great discovery.’” (Rapoport 1991, p. 266)

Although Lepeshinskaya’s meteoric rise did not last long, the political repercussions were more far-reaching. Her “inglorious demise” was usually cited not as a reflection of her individual character but as a general problem inherent in the philosophy of “dialectical materialism.” For instance, the exposé article from *Science*, which revealed the irreplicable results of various sorts reported by O.B. Lepeshinskaya, began with the characterization of her theory as “a new dialectical-materialist cell theory.” (Zhinkin and Mikhailov 1958, p. 182). Loren Graham, the award-winning historian of Soviet science and philosophy, also denounced Lepeshinskaya’s cell theory scornfully, but Graham did not regard her theory as discrediting dialectical materialism writ large. Graham considered Lepeshinskaya an unbridled careerist who rigged the political system for her own gains. But it was her lack of personal integrity, not dialectical materialism as a philosophy of science that was to blame. Beneath her character flaws, Graham suggested that there was very little connection between her cell theory and the intellectual content of dialectical materialism (Graham 1974).

The denigration of Lepeshinskaya forms the backdrop against which we can assess Bei and his reputation because of his close affinity to Lepeshinskaya. As a matter of fact, Lepeshinskaya was Bei’s Soviet analogue: Both were prominent biologists living at the revolutionary heights in their home countries (Bei during Mao’s regime and Lepeshinskaya during Stalin’s). Both came up with alternative

interpretations on cell proliferation and had contemplated on the question of the origin of life. Both wrote papers in German in addition to their native languages. They met twice in Moscow to exchange views and papers. Since they were acquaintances, and since Lepeshinskaya was a sloppy charlatan, for many observers it followed that Bei was probably a fraudulent scientist too.

It is worth knowing that the correspondence of Bei and Lepeshinskaya ran deeper than their social acquaintance and comparable theoretical contentions. Bei was intrigued by Lepeshinskaya's work before China turned communist and before Lepeshinskaya became one of Stalin's favorites. Prior to Lepeshinskaya's political debut in 1950, Bei had already studied and made reference to her research papers. This brings us back to Bei's 1942 paper which first appeared in the Chinese magazine *Science*. The papers Bei had access to were written in German and published in the Japanese journal, *Cytologia* in 1936 and 1937. Considering the chronology of events, it was highly unlikely that Bei paid attention to Lepeshinskaya because he was yielding to political pressures. If Bei was a "Chinese Lepeshinskaya," the parallel was not political service to the parties—led by the Bolsheviks or the "Chinese Bolsheviks." (Luk 1990). The facts call into question the stereotypical thinking that lumps Bei and Lepeshinskaya together simply because they were both biologists in the Red East.

Nor was Bei an uncritical supporter of Lepeshinskaya. Bei was aware of the shortcomings in her papers. What drew Bei's attention was Lepeshinskaya's conjecture of "the formation of cells from yolk spheres in chicken embryos" (*bildung von zellen aus dotterkugeln beim Hühnerembryo*). Bei was attracted by her revolutionary thesis that yolk spheres could generate cells after "remaking" (改塑). But at the same time he cautioned that Lepeshinskaya's discovery exceeded the general principle of cell generation, and thus in need of a careful examination (Bei 1943a, p. 111). Bei's skepticism about Lepeshinskaya appeared in print before Lepeshinskaya was formally ostracized by the Western scientific community. Although Bei did not disprove Lepeshinskaya's claims, he did not unconditionally accept her arguments either. His mindfulness and independent thinking had more to do with his fascination with cell studies rather than path-dependency upon the West.

Bei's interest in and skepticism of Lepeshinskaya prior to her political fame and scientific infamy should not be underrated. The cytological connection between Bei and Lepeshinskaya had been pre-fabricated before the arrival of political intervention from Kremlin and organized skepticism from America. It was not political struggles or scientific networking that cut across the Bei–Lepeshinskaya line. Rather, it was a shared intellectual curiosity in the past and future of cells that was the common thread. However, the common interests in examining the development of cells in relations to their surrounding fluids should not conceal their differences. Not only was Bei skeptical of Lepeshinskaya's paradigm-shattering contention; he held a different interpretation of the cellular phenomenon under investigation. Bei also disagreed with the specific use of terminology in Lepeshinskaya's papers (my translation):

Lepeshinskaya's view that yolk spheres from chicken embryos could be remade into cells, in my speculation, was the well-nigh reformation of yolk spheres. As the meanings of remaking and reformation differ. Remaking means creation from anew, but reformation simply means revitalization. The latter must be easier to accomplish as the ingredients were all there (as inherited) from the historical background (Bei 1943a, p. 115).

The research materials of Lepeshinskaya's 1936 and 1937 papers differ quite significantly from these of Bei's 1942 study. But the meaning of the Bei–Lepeshinskaya interface, at least from Bei's point of view, was the connotation and interpretation of the keywords. He interpreted Lepeshinskaya's investigation of “*die entstehung von zellen aus dotterkugeln*” as “remaking” cells from anew. The “remaking” (改塑) of new cells from yolk spheres in chicken embryos created an image of the de novo generation of cells which undercut the historical continuity of cells. Bei was unsure of this “ahistorical” implication signified by the word “改塑.” He did not overrule Lepeshinskaya's choice of the word, but he was confident that the word “reformation” (重建) was more appropriate. Although there were other English equivalences to the Chinese word “改塑” such as remodel, reconfigure, or recreate, the bone of contention was the historicity of cells. Bei was in favor of seeking words that would preserve the undertone of the historical continuity of cells. He was concerned that a complete severing from the genetic past would make regeneration difficult to occur. Reforming cells were more plausible and believable than remaking cells. Semantics was at the center of Bei–Lepeshinskaya scholastic exchange.

Bei's concern with the choice of word was wise, as the issue would become the center of disagreement in contemporary debate surrounding his objectionable cell theory. On 8 November 2005, a Chinese blogger sent an excerpt of Bei's cell reformation theory to Fang Zhouzi (方舟子), who is known for writing science popularization literature and exposing academic misconduct in China (Nature 2012). The sender was hoping that Fang would publicly denounce Bei's theory as fraudulent. Fang re-posted the excerpt to the website <http://www.xys.org> but did not add further comment.

Registered and based in the US, <http://www.xys.org> (the URL was derived from its Roman initials of its Chinese name 新语丝, *The New Threads*) is a Chinese website launched and maintained by Fang. The website provides mainland and overseas Chinese a virtual space to share and discuss topics related to science and society in China. After the post appeared on *The New Threads*, a web user picked up the excerpt and waged an online war with Fang over the credibility of Bei's theory. Under the alias *Tomoe*, this user did not regard Bei's cell reformation theory as bogus. *Tomoe* argued that some aspects in Bei's theory were consistent with the embryonic development in *Drosophila*. *Tomoe* pointed out that the embryos of *Drosophila* do not undergo transcription and cytokinesis in the first two hours but the nucleus divides once every nine minutes and results in hundreds of nuclei after the ninth division. The nuclear DNA comes from cell division but the nucleus constituents such as proteins and nuclear membranes come from material deposit. The enzymes, energy, and nucleic acids necessary for replicating DNA also come from the mother cells. The newly formed cells are built on the basis of cytoplasm

from these mother cells. The phenomenon is consistent with one of the tenets in Bei's cell reformation theory, which states that new cells are generated on the basis of existing materials from old cells. *Tomoe* suggested that "a special form of division" would be a more appropriate term to describe this mechanism rather than "cell reformation." But whatever it is called, *Tomoe* claimed that there is a considerable overlap between the developmental embryology of *Drosophila* and Bei's theoretical assertion. What *Tomoe* objected to was a premature and sweeping denunciation of Bei's cell reformation theory.

Tomoe benchmarked Bei's theory against external criteria in developmental genetics; he did not uncritically glorify Bei, but he found Bei's vision both prescient and precious as he stated, "What I admire is the phenomenon he has observed some seventy years ago" and concluded that "his theory is now common sense, but it was a breakthrough at that time." Yet Fang and his followers were not ready to grant Bei this special acclaim of prophetic insight. Fang maintained, "All, not just some, viewpoints of old Bei are 'mistaken'." Fang regarded Bei as a vitalist and interpreted Bei's theory as advocating the *de novo* generation of new cells by denying the historical continuity of cells: "regardless of what materials are utilized, 'cell reformation' denies the genetic continuity of cells. It suggests that environmental condition alone (without the need of genetic order of the old nucleus) is enough to generate new cells and nucleus from scratch..." (*The New Threads*, 2005)

At the heart of this debate lies the question regarding the historical continuity of cells. Fang rejected Bei's theory on the grounds that cell reformation defies the historical continuity of cells, in contrast to *Tomoe* who suggested otherwise. For Fang, what was at stake was not Bei's experimental sophistication or his choice of research substrates; nor did Fang display much interest in the cytomolecular similarities between *Drosophila* and *c. nankinensis*. Fang was skeptical of Bei's theory because he considered the theory of cell reformation a heresy against the doctrine of cell continuity.

The paradigm of cell continuity is a core theme that has run through much of the history of biology in twentieth-century China. Scientists have been concerned with the lineage of cells ever since Virchow, if not earlier. Western scientists had been preoccupied with the question of how new cells were formed and where cells came from ever since the establishment of cells as the fundamental unit of life (Harris 1999). In Republican China, Bei dedicated much time and energy into investigating the formation, constitution, and interpretation of how cells arose. By advocating a cell-from-yolk cause of cell generation, Bei was among many scientists of the world who offered a cell-from-X hypothesis in the early twentieth century. Specifically, Bei Shizhang postulated "reformation" as a theoretical explanation of some of the transformation of cells. While once sympathetic to Lepeshinskaya's cytological theory, Bei was insistent upon choosing the right term in order to not sound ahistorical.

Under Mao, the "origins of life" were placed in the framework of dialectical materialism. To demonstrate relevance of his work to state-approved agendas at the height of the Cultural Revolution, Bei aligned his theory on cell lineage to Mao's known interest in cell origins. What Mao offered was a justification for Mao-era

scientists to work on issues related to cell lineage. In the post-Mao era, science writers and informed netizens shared the same concern over the historicity of cells. Both before and after Mao, Chinese scientists and critics have cared much more about the concept of maintaining biological continuity with the past than with the exact mechanisms of how this was achieved.

Was Bei's study a Chinese scam that resembled the Soviet-style pseudo-science presented by O.B. Lepeshinskaya? This question was contemplated by Hu Wengeng (胡文耕), one of the most widely-cited philosophers of biology in contemporary China. In a chapter devoted to the topic "cell origins and cell reformation," he dealt with the objection that Bei's work was dubious simply because it looked too much like Lepeshinskaya's. A popular attitude held that since cell reformation was similar to Lepeshinskaya's work, and since she failed, therefore it was difficult for the work of cell reformation to obtain positive results. Hu's tactic was to highlight the differences between Bei and Lepeshinskaya in terms of research materials, theoretical emphases, and experimental conditions. Hu's point was that although there were some overlaps between Bei and Lepeshinskaya, their differences should not be overlooked. Their dissimilarities were more substantive and suggestive than their superficial similarities. For example, one of the marked Lepeshinskaya–Bei discontinuities, according to Hu, was that Lepeshinskaya purported to completely replace Virchowian paradigm with her "vital" theory while Bei was merely offering an alternative way to broaden the cytoscape dominated by Virchow. Moreover, Bei did not commence his study of the developmental cycles of *c. nankinensis* with the intention to root out advocates of Virchowianism and Weismanism the way Lepeshinskaya did. He was not a political demagogue manipulating science for his personal gain. Unlike Lepeshinskaya, Bei did not start from a shaky foundation that rested on an unempirical, quasi-supernatural belief in "vital substances." He never claimed that cells, or any fundamental units of life, could come from mysterious non-living matter *a priori*. He never wanted to dismiss rigorous evidence in favor of an inexplicable vitalistic cause of life (Hu 1982).

As far as I know, Hu's vindication is probably the only existing writing that appraised the theory of cell reformation from a philosophical perspective. Hu addressed "the possibility of the theory of cell reformation" in a historiography paper (Zhang 1996). Situating the theoretical plausibility of the theory of cell reformation within the context of the natural history of cells, Hu concluded that Bei's theory should not be dismissed of its philosophical plausibility simply because it looked similar to the failed attempt of Lepeshinskaya. Hu called for further experimental corroboration to clarify unclear points in Bei's theory.

While Hu gave Bei's theory the benefit of the doubt, not all Chinese reviewers shared Hu's sympathy. The Chinese neurobiologist, Rao Yi (饶毅), recently expressed his distrust of Bei and his work. Rao was trained at Harvard and UCSF. He had worked at the School of Medicine at Washington University in St. Louis before serving as the head of a scientific institute at Northwestern University. In 2010, his high-profile decision to give up not just his tenured professorship but also his American citizenship to return to China put him in the media spotlight

(FlorCruz 2010; LaFraniere 2010). He is now the dean and chair professor of the School of Life Sciences at Peking University.

Rao (2009) authored a collection of essays expressing his views on a number of popular issues surrounding science and science management in China. The range of topics appearing on his narrative radar included backyard stories of Marie Curie, gender inequity in science, tips on how to read scientific literature, lists of scientists whom he thought deserve Nobel accolades for their achievements but hadn't won yet (many of the scientists on his list later won the Nobel awards, making him a stunningly accurate predictor of future Nobel prize-winners). The point is, Rao's compilation was not as philosophically oriented or intellectual minded as Hu's monograph. Rao's anthology was intended for general Chinese readers rather than philosophers of science. One of the essays was a short eulogy he wrote to commemorate the late biochemist Zou Chenglu (邹承鲁) when Zou died in 2007. Entitled "Zou Chenglu: The Good People Loved Him, and the Bad Hated Him," Rao walked through the major milestones in Zou's righteous life while venting his frustration and grievances against a certain "centenarian" and his theory of cell reformation:

The "centenarian" has made contribution to the development of Chinese science. However, compared to his contemporaries such as Feng Depei in physiology and Wang Yinglai in biochemistry, the quality of his academic achievement was less outstanding, and even quite below that of his contemporaries'. When I first came to visit the newly established research center of his, I was brought to the exhibition room in which his "cell reformation" results were on display. I said his research was inappropriate.....It was inappropriate as anyone can take a gander and see his limited "achievement".....When I expressed my concern, I did not know that academician Zou Chenglu had raised similar objections in the past and had paralyzed their interpersonal relationships...Perhaps because the issue is now timeworn, I have not been reprimanded. But things could not have been easy for Zou as Zou used to work under him...Those who did not know about developmental biology might regard Zou's objection against him as groundless. Reportedly Zou said his research in China was quite inactive over the years. He seldom published papers. Readers can check out bibliography to evaluate Zou's comment on their own. There is no need for other people to explain (Rao 2009, p. 159).

When Rao's article was first published in 2007 in a Chinese periodical, Bei Shizhang was still alive. This might explain Rao's indirect reference to him as the "centenarian" rather than spelling out his name. Yet there is no doubt that Rao was talking about Bei Shizhang. Besides invoking Bei's personal trademark—"cell reformation"—Rao's speculation on the glacial relationship between Bei and Zou was situated when Zou Chenglu was a biochemist at the Institute of Biophysics, thus explaining why Zou was "uneasy" when he worked with Bei at the same institute. Also, Feng Depei and Wang Yinglai were both scientists in Bei's cohort. All the signs point to Bei as the target figure in Rao's essay. If we accept that Bei Shizhang was indeed the target of allusion in Rao's essay, the next question is: why did such a young and brilliant scientist choose to publicly denounce Bei?

One of the main reasons for a lack of approbation of Bei's theory of cell reformation among the mainstream scientific community is his insufficient explanation of details. What Bei called "cell reformation" encompassed primarily the

principle that cells could be “reformed” from the yolk granules that enveloped the cells under suitable conditions. But “deform,” “reform” are vague terms in a scientific study, nor is the phrase “suitable condition” specified with enough details. The biggest problem is that even if one observes an abnormal pattern of cell differentiation in *c. nankinensis*, is it justified to generalize from this one isolated occurrence to the overall cytoscape? Even more so, since Mao’s declaration of interest in the origins of cells, Bei began to sell his theory as offering a potential explanation for cell origins and even the evolution of life. The problem is that issues as complicated as the origins of cells are very difficult, if not impossible, to illustrate in an experimental setting. It is not that Bei was less attentive to scientific rigor but that sufficiently demonstrating the vast array of environmental and hereditary factors on short-lived organisms in a laboratory was almost inconceivable.⁵

In sum, cell reformation theory is hardly an undisputed set of ideas. The controversy has its root in its Lamarckian view of life, of which Wilhelm Harms was an adherent. Even though Harms was sympathetic to the Lamarckian cause of inheritance, the truth is that he did not give sufficient details and evidence to support the Lamarckian interpretation. Harms’ lifelong emphasis on the environmental conditions and organic development shaped Bei’s formulation of scientific theory. Apparently, this “intellectual trait” of dedication to knowledge synthesis but failure to account for internal mechanics also inadvertently passed on to Bei. On the one hand, Bei was cultivated to synthesize specialties with divergent methodological approaches and theoretical worldviews. On the other hand, Bei’s humble academic record also stems from the same intellectual commitment: he saw the big picture and the connection among previously separated fields, but did not attach enough weight to details and intricacies, and the details are important, especially when attempting to communicate the merits of a new theory to detail-oriented people.

2.6 Concluding Remarks

This chapter gives an introduction to the early life and academic disputation of Bei Shizhang—the founding father of biophysics in China. Controversy over Bei’s theory is of direct relevance to the historiographical character of biophysics as the theoretical viewpoint upheld by a disciplinary founder is indicative of its epistemic

⁵Between 1988 and 2003, two Chinese volumes by the title of *Cell reformation I and II* were published. The twin volumes contain follow-up and new research on cell reformation under the laboratory guidance of Bei Shizhang. In addition to cell reformation of *c. nankinensis* yolk granules, sources of experimental data in these studies were broadened to include self-assembly of reforming cells in the early development of chicken embryos, nucleus reformation of cultured bone marrow cells of adult mice, cell proliferation of *chlamydia trachomatis*, and reconstruction of cells in *Rhizobium japonicum* in *glycine gracilis*. However, none of these experiments have been externally reviewed and/or testified by a third party. The figures and data published in these volumes remain less than satisfactory. See Bei, ed. (1988, 2003b).

content. The extent to which Bei's conception of biophysics is embodied in his theoretical outlook behooves us to study their correlation and broader impacts.

To investigate the formation of Bei's scientific worldview, I chronicled Bei's early schooling that led to his graduate study with Wilhelm Harms in Germany. From there, I explored the scholarship of Harms with the intention of highlighting Harms' intellectual influence on Bei's academic pursuits, in both a positive and negative light. I suggest that the disagreement on the credibility of the theory of cell reformation was ensnared in the larger sets of debate involving Soviet-styled biology, the perception of Marxist philosophy, and political patronage of cytology in twentieth-century Communist states.

The story of Harms and Bei stayed mostly in Germany. Since Bei returned to China in 1931, his cytological research would lie dormant for the next 30 years, during which he put the state-assigned administrative and organizational tasks before his own academic agenda. In the post-1949 period, Bei would have to put his scholarly interests on hold, as his duties would compel him to act as a discipline builder that would ultimately define him as a "founding father" of Biophysics in China. It is to these institutional building efforts we now turn.

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