2 Summary of my past research achievements

Ever since the early stage of my academic career, my research had been motived by the most important problems of the homotopy theory at the time. For instance, contrary to the common belief at the time, I gave a supporting evidence that only finitely many the Kervaire invariant one elements exist, for the first time in the world. And I had generalized this phenomenon to a much more general finiteness conjecture about the stable homotopy groups of the sphere, or more generally those of connected ring spectra, which I christed "New Doomsday Conjecture,"generalizing not only the finiteness of the Kervaire invariant one elements, but also the famous finiteness theorem of Adams' Hope invariant one theorem. Furthermore, I also worked with Hopkins' chromatic splitting conjecture, for which I made some achievement.

However, for both cases, I arrived at the conclusion that the traditional homotopy theory is completely useless to attack these deep problems of homotopy theory.

Having realized powerlessness of the traditional method of homotopy theory in these ways, I sailed out to search for any possible clue in other ares of mathematics. One such an example of the study of $\mathbb{F}_1$-scheme, invented by people like Connes-Concani, Koyama-Kurokawa, in order to approach the Riemann hypothesis. Concerning this, having settled a conjecture of Koyama-Kurokawa, I wrote some articles. However, I realized some essential difficulty for further advance.

On the other hand, I had also worked with study of four manifolds, making use of the Bauer-Furuta-Seiberg-Witten invariant having its value in the equivariant stable cohomology group. Although I wrote some papers with Furuta, Kametani and Matsue, my ultimate conclusion was that, as far as the traditional homotopy theory concerns, no matter how we apply the Seiberg-Witten equation, we can never solve Professor Yukio Matsumoto's 11/8 conjecture, the most important unsolved problem in the study of four manifolds. Rather than a despair of essential difficulty of any further advance, this was really shocking for me who beleived "The classical homotopy theor should be very deep"for many years (of course, assuming the validity of Professor Yukio Matsumto's 11/8 conjecture).

However, even if the traditional homotopy itself is useless for some problems, it is not necessary the case that the "abstract homotopy theory,"which was constructed in analogous framework with the classical homotopy theory, is useless. In fact, Voevodsky, together with Morel, constructed "motivic homotopy theory,"which is an abstruct homotopy theory of algebraic geometry and contains all the information of the classical homotopy theory when the case field is a subfield of the complex numbers. What I woudl like to aim in this setting is, rather than local applications like extracting information for some classical homotopy group of a specific dimension out of the motivic homotopy theory, but to extract more glocal structore. For this purpose, we should look after glocal algebro-geometric properties which yield glocal properties of the motivic homotpy theory.

And, I have now realized what I have to put all of my effort:

Investgate hierarchy structures in algebraic geomety

I depcited this in an article published in the Ohkawa memorial proceedings.