We are in a turning point of particle physics. The Standard Model (SM) has been confirmed as the correct effective theory at the electroweak scale by the discovery of the Higgs particle at the LHC in 2012. This makes even more pressing the longstanding fundamental questions that remain unanswered in the SM. Here belong 1) the question about the dynamical origin of the Fermi scale 2) about the origin of dark matter 3) about the origin of the flavour structure and CP violation 4) about the origin of the baryon-antibaryon asymmetry in the universe 5) about the unification of elementary forces.

There is no doubt that the SM is not the final theory of elementary interactions as each of those questions has to be answered for a deeper understanding of the structure of matter and the history of the universe. There must exist a deeper theory, an extension of the SM and finding it is now the main challenge and the main goal of particle physics. Paving the way towards this goal is the main objective of this project.

In the last several decades, particle physics has been progressing with a clear path of "planned" discoveries which could, at each step, always be identified. Examples are the discovery of the W and Z bosons, of the top quark and of the Higgs particle. Now, we are however in a turning point of particle physics since, contrary to certain logical continuity of the theoretical research leading to the completion of the SM, there is neither unique theoretical framework for extending the SM, nor clear indication at what energy scale new physical effects should appear. We are facing the situation that is not unusual in basic science whose purpose is the exploration of unknown territories, with high risk but high reward possibilities.

So far, the experiments and in particular the LHC runs have given null results in searching for beyond the SM phenomena. Given the complexity of the experiments, that search is most often guided by a number of theoretical ideas and concrete models that have been proposed as extensions of the SM. The null results give important hints for further systematic exploration. That theoretical exploration is of course not totally random. It has important guidelines and constraints. The obvious one is to be consistent with the existing experimental data. The other one is to be motivated by the mentioned above puzzles of the SM. The third one is to be mathematically consistent. It may look surprising how strong are those simple and natural constraints in limiting the number of plausible theoretical ideas to be explored. Fortunately, and this we would like to stress, a systematic research can be conducted in those territories and this is the purpose of the present project. Our research program is centered at the interface of collider phenomenology and low-energy precision physics and it aims at the synthesis of theoretical lessons learned from both. As a consequence we will be able to probe new territories in the broadest range of ways and with the best theoretical tools that we can design. Ultimately we will be in a position to make whatever discoveries Nature places within our reach.