Flavour physics: summary and outlook

Flavour physics has played a key role in the development of our knowledge of fundamental interactions, leading to several remarkable "New Physics" predictions, ranging from the existence of the charm quark to the presence of a third generation of quarks, as well as to very stringent constraints on new interactions, at the level of 10^5 TeV for $\mathcal{O}(1)$ New Physics (NP) flavour couplings. It is therefore of fundamental importance for the future development of the field of elementary particle physics to continue improving the theoretical and experimental tools needed to study flavour physics. These improvements should guarantee the achievement of a twofold goal: on one hand, increasing the reach of indirect searches for new physics, in order to be able to probe higher scales in the event that no NP is discovered by direct detection; on the other hand, providing a precise determination of the NP Lagrangian if any new particle is produced in direct search experiments.

From a theoretical point of view, particle physics is confronted with what can be called the "flavour paradox". On one side, the Higgs discovery and the related measurements are showing the physical reality of the relation $m_i = \lambda_i v$ between the fermion masses and their couplings to the Higgs boson. On the other side we have no clue whatsoever on the pattern of the λ_i themselves. The consideration of this "paradox" makes the program of testing BSM contributions to the CKM picture from the $20-30\%$ level, as it is now, down to $1-0.1\%$ precision a fundamental goal of all particle physics. This goal might be achieved in the next 10-15 years if appropriate steps are taken to put together a comprehensive program of flavour physics experiments and theory.

Considering the goals outlined above, the flavour physics What Next GdL has investigated future theoretical and experimental prospects, with particular emphasis on new experimental ideas that could represent a breakthrough in the development of this field, and in which INFN could play a key role.

- **Kaon Physics** In the very near future, the NA62 and KOTO experiments are expected to probe NP through the $K^+ \to \pi^+ \nu\bar{\nu}$ and $K_L \to \pi^0 \nu\bar{\nu}$ decays respectively. The expected data samples for the two experiments correspond to $\sim$ 100 events for the charged kaon decay and to a few events for the neutral kaon. While these results will already considerably improve our knowledge of the effective $\bar{s}d\nu\bar{\nu}$ interaction, the ultimate experimental goal should be a precise measurement of BR$(K_L \to \pi^0 \nu\bar{\nu})$, since the theoretical uncertainty in this mode is much smaller than the one for the charged kaon decay. This goal might be achieved either by an upgrade of the KOTO experiment or by a K_L experiment based on the NA62 infrastructure at CERN. In both cases, one aims at obtaining a sample of $\sim$ 100 SM events. It is worth mentioning that even LHCb or an extreme flavour experiment (see below) could contribute to experimental progress in K physics by providing precise measurements of rare K_S decays.
- **Charm physics** Given the very small rate of CP violation in charm mixing expected in the SM, the recent experimental improvements in the measurement of mixing-related observables has lead the NP-constraining power of charm physics very close to the one of kaon physics. Belle II and LHCb upgrade will provide more precise data, probing CP violation in charm mixing to the degree level. SM theoretical uncertainties might start playing a role at this level of accuracy, and new ideas are being put forward on how to reduce them with data-driven methods. More precise experimental data, as well as data-driven theoretical improvements, will shed light on the amount of CP violation in singly Cabibbo-suppressed D decays and on its compatibility with the SM. Improved data on very rare decays will probe NP contributions that might produce large enhancements of the very suppressed SM short-distance contributions. A further order-of-magnitude experimental improvement of several key observables might come from an extreme flavour experiment (see below).
- B **physics, CP violation and the Unitarity triangle** The next decade will witness substantial improvements in the study of B physics, both from the experimental point of view (with Belle-II and the LHCb upgrade) and from the theoretical one (with the expected progress in Lattice

calculations of matrix elements and with the calculation of higher-order corrections to several interesting observables). With these improvements, a determination of the CKM parameters with 1% accuracy will become feasible, as well as an improvement of the bounds on NP contributions and a clarification of a few tensions between theory and experiment, in particular in rare $B_{(s)}$ decays.

- **flavour and CP violation in the lepton sector** Searching for flavour- and CP-violating processes involving charged leptons is an excellent probe of NP, since an observation of these processes would constitute a clean signal of NP. The complementarity of measurements of τ and μ FCNC transitions in decays to different final states and in conversions calls for an experimental improvement in all sectors. While Belle II, the MEG upgrade, Mu2e and COMET will be the players in the next decade, it is certainly worth exploring the possibility of further improvements by new techniques. Concerning CP violation, the feasibility study of a magic ring for electrons could open up the interesting possibility of performing electron EDM measurements at accelerators.
- **an extreme flavour experiment** In order to push to the extreme the intensity frontier, the very interesting possibility of exploiting the full luminosity provided by the HL-LHC (or by any future hadron collider) for flavour physics has been considered. This poses severe experimental and theoretical challenges: can a sample of 10^{14} bottom and 10^{15} charm mesons be fully exploited? Can theoretical uncertainties be kept below the projected experimental ones for a large number of interesting observables? While a full answer to these questions requires a much more detailed study than what was possibly achievable in the What Next WG, at present the necessary technical developments do not seem unfeasible. From the experimental point of view, storing and processing such data samples is a huge technological challenge, even if projected a decade into the future, thus requiring a paradigm shift by which data analysis will be performed in real time, rather than off-line. A detector with strong tracking capability at high luminosity seems technologically within reach, while a readout at 40MHz will be doable thanks to the foreseen progress of telecom technology. Specialized processors are needed to perform real-time event reconstruction and get tracks and other complex primitives straight out of the detector. The expected progress in CPU processing power would allow offline-grade reconstruction and calibration in real-time. Physics analysis *in real time* requires the ability to do precision measurements from reduced-size stored samples, the need to overcome the *event* concept by saving statistical summaries only, the usage of well-chosen control samples and special methods to control systematic effects. The resulting physics potential is impressive, leading to a determination of the angles of the unitarity triangle at the 0.1° level and of the CKM parameters at the 0.1% level, corresponding for example to a theoretical uncertainty on $BR(B_d \to \mu^+\mu^-)/BR(B_s \to \mu^+\mu^-)$ below 2%, to be compared with the expected experimental uncertainty of $\sim 4\%$; CP violation in charm mixing will be probed at the 0.1° level, comparable to the expected SM uncertainty. These are just a few examples of an extremely rich physics program that is still under investigation.