

Theory Panel Remarks

Maxim Perelstein, Cornell/LEPP
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- Modern collider experiments face a huge challenge: the event rate is too high to even record them all, let alone analyze them carefully
- In search for “new” (defined as “interesting”) physics, experiments need to constantly prioritize the resources (trigger bandwidth, GRA-hours, etc.)
- Theorists have traditionally played a major role in this prioritization: i.e. all new particle discoveries of the last 30 years - W/Z, top, Higgs - have been enabled (or at least greatly accelerated) by precise, almost-unambiguous (≤ 1 free parameter) predictions of the SM
- With the completion of the SM, reasons for more new particles are not as water tight (e.g. naturalness), and theory predictions far less specific
- The challenge for pheno and exp community has been to develop strategies for setting course in this new situation
- Lack of success so far may be a sign that that the challenge has not yet been met, or simply lack of new physics within reach ?????

- Back in the 1990s (grad school for me), BSM theory guidance was strongly dominated by a single hypothesis: constrained MSSM, a.k.a. mSUGRA
- Searches that were not powerful in constraining the parameter space of this model were often neglected (example: monojet+MET at the Tevatron run I)
- Explosion in model-building with branes/extra dimensions in the late 1990s motivated new searches (e.g. monojet+MET as a search for large extra dim.)
- More models fueled more searches, and sometimes motivated development of new broadly useful experimental techniques (e.g. boosted top tagging from KK gluon searches in RS models)
- Perhaps more importantly, proliferation of models brought a powerful dose of humility: we devised quasi-model-independent search strategies (e.g. WIMP searches in gamma/jet+MET), as well as “modules” or simplified models (e.g. “natural SUSY” stop/neutralino module)
- Another humble approach is to construct models motivated explicitly by providing framework for interesting/unusual signatures, rather than by the traditional “big ideas” motivation (e.g. hidden valley)

- All these ideas originated with the theory/pheno community and had a real, strong impact on how experiments go about searching for new physics
- Incidentally, all ideas I mentioned originated in the US theory community, (very likely) giving the US experimental groups an edge in the hyper-competitive world of CMS and ATLAS
- While we cannot claim “success” (BSM discovery) at the LHC so far, I believe that our chances of success have been improved by these efforts
- However, we’re not done till we’re done: lack of success motivates further broadening of our search strategies, and new ideas are needed!
- Luckily, BSM/collider physics attracted a wave of first-rate young physicists over the past decade, ensuring the vibrancy of the field; many are now in junior faculty positions and need adequate funding to build up their research programs