ELEMENTS OF A PHYSICS CASE FOR A HIGH-ENERGY LHC

James D. Wells, Cavendish Laboratory, University of Cambridge, United Kingdom

Abstract

I outline the elements of a physics case for a high-energy upgrade to the LHC. The motivations are centered around the perspective of "blue chip" ideas that solve the hierarchy problem: technicolor-like theories, supersymmetry, and extra dimensions. In each case there is the prospect that going to higher energies is not only desirable but needed for discoveries. Nevertheless, the results from experiment over the next few years, most especially at the LHC, will sharpen the arguments and enable a more enlightened decision between the various experimental options for the future. (Based on an October 14, 2010 presentation at the Malta HE LHC meeting.)

PRINCIPLES OF MOTIVATION

It is not possible to say with precision what physics ideas we will wish to study more than a decade from now, especially given that the LHC has just begun and we do not know what surprises it has in store for us. When discussing motivations for a collider experiment that is to begin decade(s) from now, there is the risk that everything said will be of little value in the future.

However, one thing is clear, and that is the energy frontier has been kind to us historically. We go up in energy, with appropriate luminosity gains, and we find new things. The first element of any physics case for the ramping up of energy is precisely the issue that gave us anxiety in the paragraph above: we do not know what is there, so let's go there.

One is tempted to end there. However, there is a second level of motivation needed beyond just increasing the energy. We need to ask ourselves what positive contributions could this new collider make if one of our main ideas of today is correct, and LHC does its job splendidly. Of course it is possible that none of our "ideas of today" are correct, but there are at least four good reasons for applying this approach anyway.

First, any other attitude (e.g., "who knows, let's see what happens without pre-conceived prejudices") is too speculative to support. Second, the knowledge gained through studying "theories of the day" likely will transfer to the study of the emergent theories refined by discoveries of tomorrow. Third, detectors and accelerator R&D must be guided by our best physics ideas now, with an eye toward inclusiveness to cover the possibilities. And fourth, this approach is a stable "physics case" strategy which by its formulation can change in the details and take into account further insights from theory and discoveries by experiment along the way, including those results that will come from LHC operation over the next few years.

HIERARCHY PROBLEM AS GUIDE

What ideas or "theories of today" shall we consider when discussing the case of the high-energy LHC (HE LHC)? There is subjectivity in that, and the answer will depend at least in small part on the person you are listening to. But you have me here, and I shall give my views, accompanied by a discussion somewhat centered on work I have done, yet which I believe are not out of step with the community's collective sensibilities.

There is no better place to look than in the proposed answers to the biggest question of our time because it is deep, important and ripe for answering: How did elementary particles get their masses at a scale so much lower than the Planck scale? This is the mass problem and the hierarchy problem jointly stated. The simplest idea for mass generation, the Standard Model (SM) Higgs boson, does not answer the question because it yields a weak scale that is unstable to quantum corrections.

Corrections to the Higgs mass are quadratically divergent according to $\delta m_h^2 \sim \Lambda^2$, and thus is highly unstable to the existence of a high scale that couples to the Higgs. Gravity, with its intrinsic large scale $M_{Pl} \sim 10^{19}\,\mathrm{GeV}$, couples to the Higgs boson and the problem is laid bare. Our most important theories of the day attempt to rectify this problem and give a full answer to the question above. The three main directions our efforts have taken us are

- Technicolor: disallow all scalars in the theory ("Higgs vacuum expectation value" is $\langle\psi_L\psi_R\rangle$).
- Supersymmetry: cancel quadratic divergence through symmetry ($\delta m_h^2 \sim \tilde{m}^2$).
- Extra Dimensions: disallow higher mass scales ($\Lambda \sim \mathrm{TeV}$).

The basic point I would like to make is that the HE LHC has the prospect of playing a decisive role in each of these three theory directions. There is no guarantee at this point that the HE LHC would be needed even if we knew that nature has chosen one of these three directions – there are too many free parameters of the theory that can be adjusted in and out of HE LHC relevance – but there is a strong plausibility argument that HE LHC could be needed, and the LHC results will likely tell us if that is indeed so.

TECHNICOLOR

Let us take first the idea of technicolor. I use the term "technicolor" very broadly here to mean any theory with strong dynamics that induces electroweak symmetry breaking and has no inherit hierarchy problem. The quintessential example is that of a bilinear operator of technifermions condensing to break electroweak symmetry in a manner similar to ordinary quark bilinear condensation breaking chiral symmetry (and also electroweak symmetry, albeit very weakly). In the past, when the community discussed the potential need for a very high energy hadron collider, often this was the primary case it made. However, given developments of experiment over the last decade, there is an argument that today it no longer should be considered the leading motivation for a higher energy hadron collider.

First, traditional technicolor ideas suffer from some standard problems such as how to get all the fermion masses out of the theory without causing problem in flavor changing neutral currents. Another challenge is what to do about the non-discovery of pseudo-Nambu-Goldstone bosons expected in the symmetry breaking. Yet another challenge is the precision electroweak constraints, which do not suggest correct values for the S parameter. These challenges, and potential solutions, are discussed in [1].

Regarding precision electroweak, there is an additional point that steers us away from traditional technicolor theories and their cousins. Back some time ago, there was very little experimental input to the question of electroweak symmetry breaking (EWSB). For example, if we assume a simple SM Higgs boson explanation for EWSB there were not even decent range limits to what this mass could be. Thus, EWSB could be just as much of a "strong" phenomenon (i.e., Higgs boson, or equivalent dynamics, quite massive) as a "weak" phenomenon (i.e., Higgs boson mass around m_Z). Today we know it is a weak phenomenon – the best fit Higgs boson mass is around m_Z with upper limit not more than about $2m_Z$ at 95% CL. This, I believe, is telling us that whatever is accomplishing EWSB it is less likely to be a strongly coupled theory at the weak scale.

The traditional argument for a very high-energy hadron collider machine was to first state that EWSB is completely unknown, and then to suggest that if it is "strongly coupled" then unitarization of the longitudinal W scattering, for example, would manifest itself by wiggles and wobbles in the very high energy scattering of those states. Perhaps a ρ-like resonance would come in at a TeV or two to save the unitarity of the theory, and the high-energy collider would see it. Today, that motivation is less appealing for the reasons given above.

Nevertheless, there can be mild conspiracies with precision electroweak, and proponents like to suggest this direction is no worse than others when it comes to making a full theory of the weak scale. These protestations might even be fair, and so it behooves us to at least state that if nature chooses this path it will be crucial to have HE LHC. It is obvious that to find a very heavy ρ-like resonance a dramatic increase in energy and/or luminosity would be needed. But energy is more important. I do not go into it in more detail here but to highlight this fact by an extended quote from the Barklow et al. report [2], where these issues have been studied:

> There has been some discussion of upgrading the LHC in luminosity and energy after the $300\,\mathrm{fb}^{-1}$ run is complete. A possible (though unlikely) doubling of the energy has been considered along with a tenfold increase in instantaneous luminosity. Since the LHC detectors were not designed for these conditions only jet and muon information is likely to be useful. Such an upgrade could double the reach for a Z' ($m_{Z'} \simeq 10$ TeV) and compositeness ($\Lambda \simeq 80$ TeV), and significantly increase the sensitivity for excited quarks ($m_{q*} \simeq 9$ TeV) and the scale of WW scattering available ($\sqrt{\hat{s}} \simeq 1.5\,\mathrm{TeV}$, assuming that forward jet tagging is still possible). *Unfortunately, most of these gains come from the energy increase* which is less plausible than a simple luminosity upgrade. [italics are mine]

My summary: inasmuch as strong dynamics ideas are worth pursuing, higher energy may be critical for success.

SUPERSYMMETRY

The second approach to discuss is supersymmetry. Supersymmetry solves the hierarchy problem via a posited symmetry between fermions and bosons (for a review see [3]). The quadratic divergence of a top quark loop in the self energy of the Higgs boson, $y_f^2\Lambda^2/4\pi$, is exactly cancelled by a top squark loop, $-y_f^2\Lambda^2/4\pi$, in the supersymmetric limit. For softly broken supersymmetry this cancellation is not exact, but effective up to supersymmetry breaking masses $\delta m_h^2 \propto \tilde{m}_t^2$, where $\tilde{m}_t$ is the supersymmetry breaking mass contribution to the top squark. For the hierarchy problem to be solved, the masses of the superpartners of the Standard Model states need to be in the neighborhood of the weak scale.

I cannot be anything more than vague about the expectations of supersymmetry partner masses. Some people make admirable and non-frivolous attempts to quantify the finetuning of the hierarchy when supersymmetry masses get heavier than the weak scale [4], but I have a difficult time taking any precise criteria seriously. Nevertheless, I do take the hierarchy problem seriously. What to do?

In the case of supersymmetry, we can confidently say that the lighter the superpartner masses are, the larger role supersymmetry plays in stabilizing the hierarchy. Whether the maximum tolerable superpartner masses should be 1 TeV, 10 TeV, or 1000 TeV, I do not know. I am not sure our finetuning sensibilities are accurate enough to strongly discount any of these scales. Furthermore, there is some advantage to having superpartner mass scales climb to larger values. In particular, there are advantages to having all the scalar superpartner masses be very heavy [5, 6, 7]. The reason is that their large masses can squash unwanted contributions to flavor changing neutral currents and CP violating observables, such as electric dipole moment of the neutron.

On the other hand, gauge coupling unification and dark matter considerations prefer the fermion superpartner masses to be much smaller. The lightest neutralino, if a wino or a higgsino (i.e., superpartner of W boson or Higgs boson), can be an excellent dark matter candidate with mass as high as 2 TeV but not higher. Restrictions on the bino (i.e., superpartner of the hypercharge gauge boson) are even tighter. Thus, a $\sim \mathrm{TeV}$ limit on the fermion superpartners is a reasonable assumption. In most models of supersymmetry breaking the gluino (i.e., superpartner of the gluon) is a factor of ~ 2 to 10 higher in mass than the LSP. Thus, the gluino mass gets restricted to less than about 15 TeV by these considerations.

We have already established that a liberal attitude toward the hierarchy problem enables scalar superpartner masses to be well above LHC energy reach, and even a 33 TeV HE LHC collider reach. We must focus on the fermion superpartners, which have a more restricted range of possibilities. It is well-known that at the LHC with several tens of fb^{-1} of integrated luminosity, none of the fermionic superpartners over a TeV in mass has a chance of being found directly except the gluino. The limit on the sensitivity to the gluino is around 2.5 TeV with less than $50\,\mathrm{fb}^{-1}$ of data [8] in this scenario.

Given the dark matter considerations stated above and the usual limit of $m_{\tilde{g}} < 10 m_{LSP} \simeq 15\,\mathrm{TeV}$, the LHC sensitivity is far below the range of mass that would cover the "full parameter" space of these ideas. A 33 TeV HE LHC clearly will do better, all other considerations equal, and that is the crux of the supersymmetry argument: deeper exploration into the high-mass lands. Determining precisely how much better the HE LHC can do over LHC, and over a high-luminosity LHC, when the parameters of the collider luminosity and detector performance are better understood, would contribute an important element to the case for the HE LHC. It should be noted that a high-energy e^+e^- collider may very well enable the complementary probing of the lighter electroweak superpartner fermions, in which case it could compete well with a HE LHC for discovering supersymmetry at the highest mass scales.

EXTRA DIMENSIONS

We now come to a third motivation which is extra dimensions. My discussion will be about the flat extra dimensions of Arkani-Hamed, Dimopoulos and Dvali (ADD) [9], but there are analogous and extended arguments one could make with the Randall-Sundrum case of warped extra dimensions [10]. The warped case even has some phenomenological overlap with the technicolor theories, which can be understood qualitatively through the AdS/CFT correspondence (AdS is the warped extra dimension theory, and CFT is the walking technicolor theory). Due to lack of time I will forego that interesting discussion and focus on the flat extra dimensions of ADD, where the value of higher energy is immediately transparent.

If we assume that there exists n extra spatial dimensions compactified on a torus of radius R, the relationship between the fundamental scale of gravity M_D ($\sim$ TeV scale) and the ordinary Planck scale as measured by Newton's constant for gravitational attraction of bodies separated by a distance much greater than R is $M_{Pl}^2 = R^n M_D^{n+2}$. The graviton is allowed to propagate into the extra dimensional space, and since it is a compact space, the momentum components in the extra dimensions are quantized. Momentum in extra dimensions looks like mass in our ordinary $3+1$ dimensions, and thus the graviton looks like a series of Kaluza-Klein excitations with masses $m_{\vec{n}}^2 = \vec{n}\cdot\vec{n}/R^2$.

The details of this theory can be found in many review articles (e.g., see [11]). Many observables in this game are not calculable, but only qualitatively given with ignorance parametrized. A good example of that is virtual graviton exchange. When contemplating the effects of low-scale gravity contributions to Drell-Yan scattering for example, one must sum over the infinite tower of KK states in $q\bar{q} \to G^{(\vec{n})} \to e^+e^-$ which is generally divergent. The divergence can be regularized arbitrarily and the amplitude can be represented by energy momentum tensor squared with a coupling constant of Λ_T^{-4} to get the dimensionality correct. The value of Λ_T is expected to be nearly the value of M_D but the precise numerics are unknowable.

However, there are two observables that are calculable in this framework. One is the rate of external graviton emission in the limit of $E \ll M_D$, and the other is the eikonal regime of very high energy $E \gg M_D$ elastic scattering. The HE LHC has much to offer in both of these limits.

Let's take graviton emission to begin with. The cross-section to produce one KK graviton in a production cross-section such as $q\bar{q} \to G^{(n)} g$ is $\sigma_{KK} \sim 1/M_{Pl}^2$. It would take many orders of magnitude beyond the lifetime of the universe to produce even one of these KK states with energy above a GeV. However, there are very many of these gravitons spaced closely to each other. Below the energy E there are $(ER)^n$, a truly staggering number of gravitons when one realizes how large R must be to seesaw M_{Pl} down all the way to $M_D \sim \mathrm{TeV}$. The probability of producing *any* one graviton then goes up to $\sigma_{any\,KK} \sim (ER)^n/M_{Pl}^2$. But with $R^n = M_{Pl}^2/M_D^{2+n}$, the total summed cross-section is

$$\sigma_{any\,KK} \sim \frac{1}{M_D^2}\left(\frac{E}{M_D}\right)^n. \tag{1}$$

Note that in the equation above the cross-section climbs steeply with energy. This is in contrast to most other high-energy cross-sections that usually decrease with energy $\sigma \sim 1/E^2$. This is one of the core reasons why it is sometimes stated "energy is everything" for extra-dimensional theories. Large increases in luminosity pale in comparison to what can be accomplished by even moderate increases in energy of the collider. The high-power scaling of this observable with respect to energy means that as one dials energy up it can be the case that nothing is seen, until a small turn of the energy knob yields an explosion of events. The HE LHC is just such an energy knob that could possibly do this for us.

The above scenario presupposes that the LHC finds nothing, and that as we increase the energy for HE LHC a signal develops. However, it could be the case that the LHC does find a signal already for external graviton emission. Perhaps it will not know with certainty that it is graviton emission, and perhaps it does not have other phenomenological handles to pin down more details of the theory. How could going to higher energies help?

In that case going to higher energies enables us to reach another perturbative regime of the scattering. Seeing a signal of gravition emission at the LHC means that M_D is not more than a few TeV. Scattering at 33 TeV center of mass energy at the HE LHC then would enable us to probe center of mass collisions with energy much greater than M_D. At small momentum transfer, the glancing blows of partons scattering at energies well above M_D is a computable, classical amplitude. The two-parton to two-parton eikonal approximation is used for this kind of analysis [12, 13].

The corrections to this eikonal amplitude scale as $-\hat{t}/\hat{s}$ and $(M_D^2/s)^{1+2/n}$, and thus serve as expansion parameters for the eikonal resumation perturbation theory. When the expansion parameter $-\hat{t}/\hat{s}$ is small that is correlated with the impact parameter being less than the Schwarzschild radius, thereby avoiding the risk of producing a black hole [14]. When the expansion parameter M_D^2/s is small that is correlated with the impact parameter remaining in the classical regime, with minimal quantum corrections. There may be model-dependent string corrections as well, or other new physics contributions, but we do not consider them here as we are dealing only with the well-defined gravity scattering amplitude.

To give a visual representation of the computability of two-to-two scattering in the high-energy eikonal regime, we introduce the parameter ϵ, defined to be

$$\epsilon = \left|\frac{\hat{t}}{\hat{s}}\right| + \left(\frac{M_D^2}{s}\right)^{1+2/n}, \tag{2}$$

and then compute this ϵ and the scattering rates for LHC at 14 TeV and 33 TeV [13]. For two-to-two scattering, there is a direct correspondence between $-\hat{t}/s$ and $\Delta\eta = \eta_1 - \eta_2$, the difference in rapidities of the two jets:

$$\frac{-\hat{t}}{\hat{s}} = \frac{1}{1+e^{\Delta\eta}}. \tag{3}$$

The larger the $\Delta\eta$ separation of jets the smaller $-\hat{t}/\hat{s}$ and thus the more accurate the eikonal computation.

In Fig. 1 we have plotted the differential two-jet cross-section $d\sigma/d\Delta\eta$ as a function of $\Delta\eta$ for three different values of the fundamental gravity scale $M_D = 1.5\,\text{TeV}, 3\,\text{TeV}$ and $5\,\text{TeV}$ in $n = 6$ extra dimensions. The background is also shown, here calculated from the leading order $2 \to 2$ QCD scattering processes. The plot was made for the dijet invariant mass greater than $M_{jj} > 9\,\text{TeV}$, which means that for all collisions $\hat{s} > M_{jj,min}^2 = (9\,\text{TeV})^2$. In addition, $p_T > 100\,\text{GeV}$ and $|\eta| < 5$ are required for acceptance of each jet. The signal lines have three colors, green (light solid) line meaning the most calculable region with $\epsilon < 0.15$, blue (dashed) line for $0.15 < \epsilon < 0.3$ and red (dotted) for $0.3 < \epsilon < 0.5$. We do not extend the lines any further leftward for $\epsilon > 0.5$ as there is no reliability to speak of for that region. We see that for very high $\Delta\eta$ the signal is computable but the background dominates, and for very low $\Delta\eta$ the signal computation is not reliable. Thus, an intermediate region of $2 < \Delta\eta < 6$ is ideal from the standpoint of calculable signal to background advantage. Note, the $M_D = 5\,\text{TeV}$ signal line never has a green (light solid) line component since the M_D^2/s correction takes $\epsilon > 0.15$ always.

At higher center of mass energy afforded by the HE LHC, we can set the dijet invariant mass cut to be much higher while at the same time boosting the total rate for the signal. We illustrate that in Fig. 2 which is the same plot as Fig. 1 except the center of mass energy of the collider is 33 TeV and the dijet invariant mass has been raised to $M_{jj} > 15\,\text{TeV}$. We see that not only has the event rate increased while keeping the signal to background similar, but the $M_D = 5\,\text{TeV}$ line has now "turned green", meaning that we have trust in the eikonal amplitude's appropriateness for the computation, and thus the result is calculable.

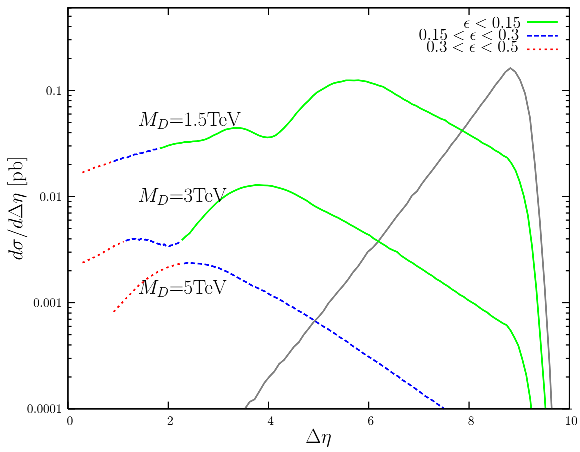

Figure 1: The differential two-jet cross-section $d\sigma/d\Delta\eta$ at 14 TeV LHC as a function of $\Delta\eta$ for three different values of the fundamental gravity scale M_D. The dijet invariant mass cut is $M_{jj} > 9\,\text{TeV}$.

It is unlikely that the first discovery of physics beyond the Standard Model would come through high energy two-to-two eikonal scattering well above the Planck mass. Instead, the example here serves to illustrate just one of the many ways that building a much higher energy collider can lead to complementary information inaccessible to what came before. LHC is good for cis-Planckian and perhaps Planckian physics, and the HE LHC could then access the Planckian and trans-Planckian regions to teach us more about the underlying theory of gravity, and perhaps fill in the phase diagram of gravitational scattering [15].

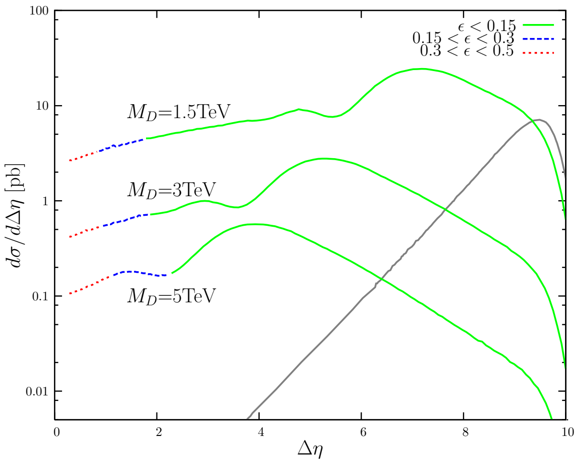

Figure 2: The differential two-jet cross-section $d\sigma/d\Delta\eta$ at 33 TeV LHC as a function of $\Delta\eta$ for three different values of the fundamental gravity scale M_D. The dijet invariant mass cut is $M_{jj} > 15\,\mathrm{TeV}$.

TOMORROW'S WORLD

What I have presented are some elements of a physics case based on what we know today. That case can be refined by more detailed statements of collider performance and would-be detector characteristics. Simulations can be done, and cost-benefit plots can be made. Comparisons can and should be made between a HE LHC option and other options that are before us as a community: ILC, CLIC, high-luminosity LHC, eLHC, muon collider, VLHC, etc.

However, it is equally obvious and important to make another point. It may be unlikely that any of the details of the justification that we can make today will be the reason why physicists will be happy to throw the on switch for HE LHC. The results of the LHC will change everything, one way or another. There will be a new "theory of the day" at each major discovery, and the arguments will sharpen in some ways and become more divergent in other ways. Yet, the need to explore the high energy frontier will remain. We will always be able to make that case, today and tomorrow.

REFERENCES

[1] K. Lane, "Two lectures on technicolor," [hep-ph/0202255].

[2] See section III.C "Super-LHC" in T. L. Barklow, R. S. Chivukula, J. Goldstein, T. Han, "Electroweak symmetry breaking by strong dynamics and the collider phenomenology," Snowmass 2001 Proceedings [hep-ph/0201243].

[3] S. P. Martin, "A Supersymmetry primer," In *Kane, G.L. (ed.): Perspectives on supersymmetry* 1-98. [hep-ph/9709356].

[4] For one of the most recent examples, see S. Cassel, D. M. Ghilencea, G. G. Ross, "Testing SUSY at the LHC: Electroweak and Dark matter fine tuning at two-loop order," Nucl. Phys. B835, 110-134 (2010). [arXiv:1001.3884 [hep-ph]].

[5] J.D. Wells, "Implications of supersymmetry breaking with a little hierarchy between gauginos and scalars," hep-ph/0306127; J. D. Wells, "PeV-scale supersymmetry," Phys. Rev. D71, 015013 (2005). [hep-ph/0411041].

[6] N. Arkani-Hamed, S. Dimopoulos, "Supersymmetric unification without low energy supersymmetry and signatures for fine-tuning at the LHC," JHEP 0506, 073 (2005). [hep-th/0405159];

[7] G. F. Giudice, A. Romanino, "Split supersymmetry," Nucl. Phys. B699, 65-89 (2004). [hep-ph/0406088].

[8] H. Baer, V. Barger, A. Lessa *et al.*, "Supersymmetry discovery potential of the LHC at s**(1/2) = 10-TeV and 14-TeV without and with missing E(T)," JHEP 0909, 063 (2009). [arXiv:0907.1922 [hep-ph]].

[9] N. Arkani-Hamed, S. Dimopoulos, G. R. Dvali, "The Hierarchy problem and new dimensions at a millimeter," Phys. Lett. B429, 263-272 (1998). [hep-ph/9803315].

[10] L. Randall, R. Sundrum, "A Large mass hierarchy from a small extra dimension," Phys. Rev. Lett. 83, 3370-3373 (1999). [hep-ph/9905221].

[11] G. D. Kribs, "TASI 2004 lectures on the phenomenology of extra dimensions," [hep-ph/0605325].

[12] G. F. Giudice, R. Rattazzi, J. D. Wells, "Transplanckian collisions at the LHC and beyond," Nucl. Phys. B630, 293-325 (2002). [hep-ph/0112161].

[13] W.J. Stirling, E. Vryonidou, J.D. Wells, in progress.

[14] S. B. Giddings, S. D. Thomas, "High-energy colliders as black hole factories: The End of short distance physics," Phys. Rev. D65, 056010 (2002). [hep-ph/0106219].

[15] S. B. Giddings, "Beyond the Planck scale," [arXiv:0910.3140 [gr-qc]].