ERC Starting Grant
Research proposal (Part B section 2 (B2))

"Novel numerical approaches to quantum gravity, holography and extra dimensions"
NumGrav

PI: Toby Wiseman
Institution: Imperial College London, UK
Duration: 60 months

Section 2 a. **State of the art and objectives**

In the past three decades gravity has played an ever more prominent role in fundamental physics. My aim is to introduce novel numerical methods to tackle the key questions in *quantum* and *classical* gravity applied to fundamental theory, where previously only analytic methods were available and failed to give answers.

My proposal is divided into two related research topics, Topic A `Quantum gravity from holography' and Topic B `Classical gravity in fundamental physics'. Both topics are related in their application to fundamental theory as well as by the methodology involving numerical computation to problems previously only treated analytically. In addition there is also specific cross over of results between the two topics. Within each topic I have specific research objectives to pursue. For each topic I will now review the background, motivation and state of the art. In the later *Section 2 b* (Methodology), I will detail the directions to be tackled, giving the methodology to be employed and justification of the requested resources.

State of the art and objectives for Topic A*: `Quantum gravity from holography'*

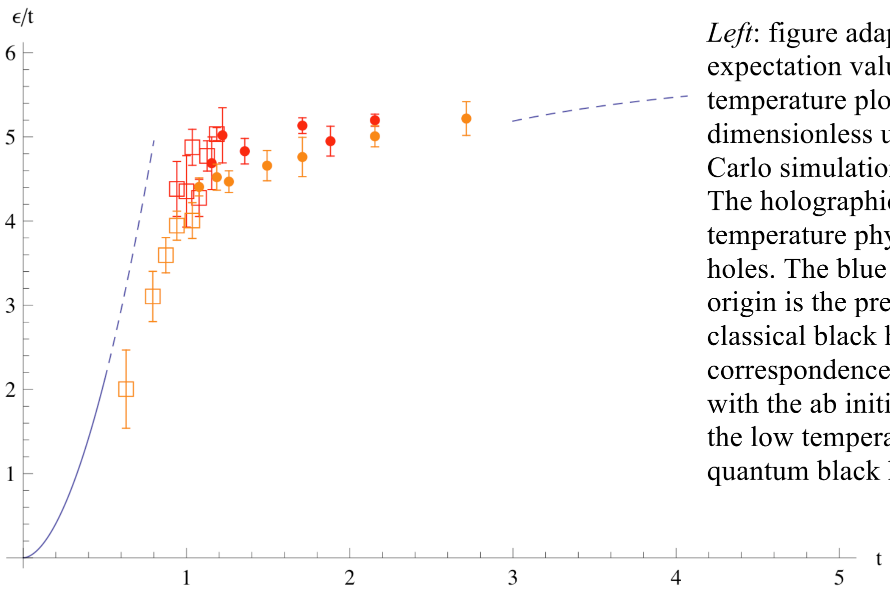
Background and motivation: For many decades up until the mid 1990's the combination of quantum mechanics and classical gravity into a theory of quantum gravity had been the `holy grail' of fundamental physics. Ideally this theory would be the quantum gravity of our world, but in the absence of that any consistent theory of quantum gravity would be a tremendous breakthrough. The problem of quantum gravity is one where genuinely new physical ideas concerning the nature of space and time are required, in an analogous manner to the revolution in thinking that ushered in quantum mechanics in the 1920's. A decade or so ago modern string theory provided the first ever candidate for a fully quantum description of certain theories that contain gravity. While not phenomenologically viable for our universe, this represented a stunning breakthrough. It makes the remarkable conjecture that certain string theories, which reduce to quantum gravity in particular limits of their parameters, are completely equivalent to specific quantum field theories (theories of quantum matter) but without gravity. These are of the `*gauge theory*' type which is a class that includes QCD, the theory of the Strong nuclear force, and are perfectly well defined theories. This equivalence is called the `*holography correspondence',* referring to the fact that the gauge theory lives in at least one dimension less than the equivalent quantum gravity theory. The fundamental object in the gravitational side of the correspondence, the black hole, is viewed as a thermal plasma in the gauge theory side. Hawking and others in the 1970's famously understood that black holes had a temperature and an associated thermodynamics. However, with no quantum description of gravity it had been unclear what microscopic degrees of freedom would underlie this thermodynamics. This culminated in Bekenstein and Hawking's 'information loss paradox', which describes the tension between our classical and quantum understanding of black holes, and specifically concerns how the information in the quantum degrees of freedom is released as a black hole in empty space evaporates by thermal 'Hawking radiation'. To this day it remains the key unresolved question in quantum gravity. *In principle*, using this holographic correspondence we understand quantum black holes. *In practice*, despite valiant work by many theorists, like QCD these gauge theories and their thermal plasma are strongly coupled systems and appear to be analytically intractable. It is the aim of this proposal to develop the necessary numerical methods to solve them.

State of the art: The gauge theories conjectured to be holographically equivalent to quantum gravitational theories (or more precisely string theories which contain quantum gravitational limits) are in various cases very explicitly known. The simplest well understood examples are those that derive from certain objects in string theory called 'D-branes' [25]. In these holographic equivalences the gauge theories are maximally supersymmetric gauge theories. For quantum gravity to be described we should work at large N, where N is the number of 'colours' in the gauge theory (for QCD N = 3). In order to describe equilibrium black holes we should work at finite temperature, and study the gauge theory thermal plasma. There is a correspondence where the gauge theory is 1-dimensional, so it simply becomes a quantum mechanical model (deriving from D0-branes). For numerical calculation it is by far the simplest starting point.

I pioneered finite temperature numerical studies of such gauged quantum mechanics theories. Using numerical lattice field theory methods I performed the first studies of the quenched version (meaning ignoring fermions) of this theory in the large N limit [2]. In later work together with the well known lattice field theorist Simon Catterall (Syracuse), himself a world leader in supersymmetric lattice methods, we employed state of the art lattice field theory techniques using the `Rational Hybrid Monte Carlo' (RHMC) method together with supersymmetric lattice formulations to perform the first simulations of the large N theory including the necessary fermions [3]. For the first time we showed that the thermal plasma behaviour of the gauge theory from direct computation were consistent with the predictions from black hole thermodynamics. This was a crucial test of holography, and opened up the new research area of studying quantum black holes using these methods. Our studies were the first of their kind, with independent, concurrent and consistent results produced by a group in Japan lead by Jun Nishimura (KEK) [4].

Past Impact: Having shown that direct simulations of black hole physics can indeed be performed, and are consistent with predictions from gravity, these results attracted considerable interest from the international string theory community, as well as the field theory community. Currently these initial papers have > 50 citations each even though they are only a few years old and the number of people working in this numerical field is very small -- essentially my group with Catterall and the Japanese group at KEK.

Perhaps better evidence of the international impact of these new ideas is that myself, Jun Nishimura together with David Bekenstein and Lawrence Yaffe are organizing an 8 week program at the KITP (Kavli Institute for Theoretical Physics) in UC Santa Barbara (US) on the use of novel numerical methods in quantum gravity and string theory. Such programs must pass a stringent refereeing process and be deemed of sufficient potential international interest and impact to warrant funding. Our application was strongly supported and is due to go ahead in Jan 2012.



Left: figure adapted from [10] showing expectation value of energy divided by temperature plotted against temperature in dimensionless units. Data points are from Monte Carlo simulation of gauged quantum mechanics. The holographic correspondence claims the low temperature physics is that of classical black holes. The blue curve on the left starting at the origin is the prediction at low temperature from classical black holes in the gravity side of the correspondence. We see very nice consistency with the ab initio quantum simulation, suggesting the low temperature physics is indeed describing a quantum black hole near its classical limit.

My objectives for Topic A of this proposal are divided into two complemenary research directions, A.1 and A.2, which I will now detail below;

Objective A.1: Solving quantum black holes by ab initio thermal simulation

I will directly solve the gauge theories that via holography govern the physics of quantum gravity. By using thermal lattice field theory methods I will simulate ab initio the gauge theory in the large N regime that describes black holes. I will use brute force methods, and in addition refined algorithms to achieve this, tackling first the quantum mechanical case, and after the higher dimensional gauge theories. The first goal is to test the holographic conjecture is true by precision comparison with the expected classical thermodynamic limit predicted by the gravity side of the correspondence. The second is then to study the thermal behaviour of quantum gravity, its phase structure, and in particular the behaviour of fully quantum black holes.

Objective A.2: Ab initio calculation of the spectrum of quantum gravity

Thermal lattice methods are ideally suited to studying quantum black holes in large N gauge theories. However to understand the real time dynamics which govern Hawking's famous `information loss paradox' a Hamiltonian approach is most appropriate. I will perform ab initio simulation of the large N quantum mechanical gauge theory corresponding to quantum gravity. I will numerically compute the spectrum and eigenstates, allowing me to study the quantum states that compose black holes. I will attempt to use this to address the question of time dependence of fully quantum black holes, to observe Hawking radiation and if possible, gain insight into the information loss paradox.

Future Impact: As discussed above, my numerical approach to direct computation in holographic theories of quantum gravity has already demonstrated significant international impact. The resources requested in this proposal would enable me to pursue the two objectives A.1 and A.2. The impact among the string theory and quantum gravity community would be very significant, as these precision tests of holography would be the most non-trivial direct tests to date. In the unlikely event that holography was seen to fail, this would of course overturn a huge body of work, and have massive international significance. In the more likely event that the correspondence is seen to hold, it will still by highly significant, and I expect it would open up a large research field internationally where many lattice field theoriests would begin to study quantum gravity using the numerical methods I have pioneered. I should stress that in particular for A.2 any progress at all on the issue of information loss would be of huge significance. This paradox has existed for decades and no approach to quantum gravity has yet made any quantitative statements regarding it. For these methods to enable holography to make any statements – even if they do not completely resolve the issues – would be deeply significant for the string theory and quantum gravity fields, and would likely have great international impact.

State of the art and objectives for Topic B: `*Classical gravity in fundamental theory'*

Background and motivation*:* Of course in the end we are not just interested in *a* quantum theory of gravity, but *the* quantum gravity describing our world. Unified models of fundamental physics have involved extra spatial dimensions since the work of Kaluza and Klein in the 1920's. String theory, the most promising candidate, requires 10 or 11 such dimensions. The famous 'large extra dimension' scenario which may solve the 'hierarchy problem' (why the fundamental quantum scale - the Planck scale - and Standard model scale are so disparate) employs extra dimensions whose size may be shockingly large, up to ~ 0.1mm. If such large extra dimensions exist there is the tantalizing possibility of producing black holes in LHC, and actually revealing the true fundamental theory - string theory or otherwise - directly in this experiment. Once extra dimensions are introduced the geometries that describe the vacuum and black holes are usually not known. Analytic methods are unable to find them since the extra dimensions and their associated boundary conditions reduce the symmetry of the problem. The black holes that could be formed at LHC are of at least two varieties, those of spherical and those of ring-like topology. Only the former are known explicitly, and whilst the latter are conjectured to exist (in dimensions six and above), their details and properties are completely unknown. If we are to search for black hole collisions in LHC, which will remain a major endeavor over the coming 10 years, we must understand the properties of these black holes. I aim to develop these necessary numerical techniques and systematically compute these solutions. Astrophysical black holes provide another arena to test theories of extra dimensions. For example the widely studied `Randall-Sundrum II' model (> 5000 citations) was famously conjectured to have very different behaviour for astrophysical black holes than usual Einstein gravity, and for a decade the model was assumed to be constrained by astrophysical observations. Using numerical methods I recently computed these black holes [1] showing the conjecture was wrong, and the models could not be tested this way. More generally very little is known about what black hole solutions exist in realistic phenomenological models of extra dimensions. Such models are extremely widely studied and it is essentially we gain control over the physics of black holes in them in order to understand how to test them.

A further research direction is to understand the exotic classical physics of black holes in the holographic correspondence. Aside from the problem of ab initio solution of the gauge theory to compute the full quantum black holes, even on the gravitational side of the correspondence which we do understand classically (although not quantum mechanically) we know little about the black holes that exist. This classical limit corresponds in the gauge theory to a large number of colours - the 'large N limit'. It is critical to understand these classical black holes in order to properly test the holographic conjecture, as we must ensure that the quantum objects we find from ab initio study of the gauge theory do indeed behave as these classical black holes in this limit. Furthermore such classical black holes in this correspondence can be used to understand certain properties of strongly coupled gauge theory physics that are inaccessible by thinking in terms of the gauge theory directly, such as the dynamics of plasma formation relevant for understanding heavy ion collider physics.

State of the art: In my earliest work I pioneered the use of numerical calculation to find solutions such as black holes in higher dimensional theories of gravity where typically they cannot be analytically found. Over the last decade I have (I believe it is fair to say, singlehandedly) driven that field forward to the point where now we have a framework of numerical techniques designed to elegantly solve the problem, and furthermore have demonstrated that the application of the method can yield interesting and important physical results.

Finding equilibrium or time independent solutions in gravity, known as *stationary* solutions, as in electromagnetism should be an elliptic partial differential equation (p.d.e.) problem, where equations with the character of the Laplace equation should be solved as a boundary value problem. Understanding how to implement this generally in gravity is always complicated by the coordinate invariance (or gauge symmetry) of the theory. However, the state of the art is that developed in [13,14,15] where I finally have a method to phrase the stationary gravitational problem as an elegant and geometric elliptic problem, and furthermore have algorithms to solve this. The local relaxation algorithm usually applied to elliptic numerical problems has various important subtleties associated to interesting physical properties of black holes, and furthermore has beautiful links to the geometric Ricci flow that has played a prominent role in contemporary mathematics.

It should be emphasized that this formulation as an elliptic problem is absolutely critical so that standard numerical apparatus developed to solve elliptic p.d.e.s can be employed. The aim is not to develop new methods to solve elliptic p.d.e.s - that would be reinventing the wheel. The problem instead is to pose the problem so that it looks like a `wheel' in the first place. In fact one does not solve the Einstein equations themselves, but actually must modify the equations to massage them into elliptic character. A key point is understanding that whilst one solves modified equations, the solutions one finds are those to the original problem. This turns out to be a very interesting geometric problem, related to the existence of Ricci solitons (studied extensively by mathematicians in geometry), and we recently made much progress in analytically proving that in many cases of interest, solving the modified equations implies a solutions of the actual gravity ones we are interested in [16].

Past Impact: This research program has had considerable impact. Perhaps the cleanest application was the use of these methods to elucidate for the first time the behaviour of black holes in the simplest model of extra dimensions, the theory of Kaluza and Klein from the 1920's, namely gravity with one extra circle dimension. While this gravitational theory, the toy model for all extra dimensions, has existed for nearly a century, the rather basic problem of what black holes look like was only answered recently using these new state of the art numerical methods, where the properties of two entirely new types of black holes were found (the localized and inhomogeneous black string solutions). These results have been an important driver for the research field of exploration of higher dimensional gravity. In particular the last decade of research has culminated in a new beautiful pedagogical textbook, “Higher dimensional black holes”, which is the first in this subject and is edited by the internationally renowned string and gravity theorist Gary Horowitz (UC Santa Barbara, US). One of the leading chapters is on black holes in the Kaluza-Klein model, and is written by Horowitz and myself [17]. I also have written a chapter outlining the new elliptic numerical techniques mentioned above [15].

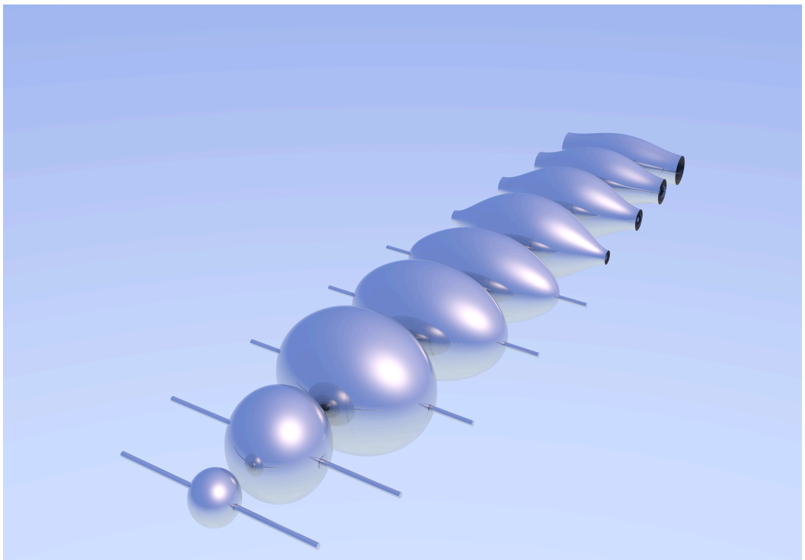


Figure depicting actual numerical solutions for black holes in the Kaluza-Klein theory (adapted from [17])

Another measure of impact is that recently there have been several high level international meetings (Denver, Edinburgh, Madeira all in 2010-11) specifically directed at using numerical methods in gravity beyond astrophysics. I was a key invited speaker at these and the use of my elliptic methods to directly find stationary solutions has been a focus of these meetings.

Beyond black holes I pioneered the use of numerical calculation to compute the simplest vacuum geometries (so called 'Calabi-Yau's' geometries) that arise in String theory phenomenology. With Headrick (Brandeis) we computed the first known geometries for the simplest Calabi-Yau called 'K3' [18]. After that work the Field's medal prize winning mathematician Donaldson (Imperial) has discussed complementary numerical methods [19] and several string theory groups in the US have started to try to extract detailed particle physics phenomenology using these numerical constructions (see for example [20]). Since our pioneering proof of principle calculation it is now clear this problem may be tackled in its simplest form, and using the tool box we have developed the challenge is now to identify the model of interest and submit it to calculation.

My objectives for Topic B of this proposal are in two research directions which I label B.1 and B.2, and will now detail below.

Objective B.1: Black holes at LHC and in realistic theories of extra dimensions

I will develop my numerical approach to study the properties of black holes in realistic theories of extra dimensions. Most importantly I will implement methods to determine the dynamical stability of the stationary solutions that are found numerically. I will apply these methods to systematically find the black holes that could be formed in LHC and determine their stability, to allow detailed predictions for LHC large extra dimension scenarios to finally be made. I will also study the phenomenology of black holes in realistic theories of extra dimensions (eg. Randall-Sundrum with radius stabilization), and determine whether astrophysics can give a window into testing these.

Objective B.2: Classical black holes and the holographic correspondence

I will use my numerical approach to find classical black holes in the gravitational side of the holographic correspondence. Firstly I will use this to determine the classical limit that should be reproduced by the ab initio calculations on the gauge theory side in Topic A. Secondly I will use these calculations to shed light on the dynamics of heavy ion collisions in QCD, by finding equilibrium black holes in holographic theories thought to have analogous behaviour to QCD. Thirdly I will compute black holes on the gravitational side of the correspondence suitable to describe *fully out of equilibrium heat flow* in large N strongly coupled gauge theories. Direct calculations in gauge theory are impossible, and I will study the physics that can be computed from this holographic reformulation in terms of black holes.

Future Impact: As discussed above my numerical approach to computing classical black hole solutions relevant for theories of extra dimensions and holography has already made significant international impact. I believe that with the resources to pursue B.1 and B.2 above, this approach will become increasingly significant in the study of gravity in fundamental theory. B.1 will have a huge impact on the interpretation of data for LHC, and indeed without the approach of B.1 the community will be very limited in its analysis of potential black hole formation, being restricted to studying only one type of black hole that is known (the Myers-Perry solution) without knowing if others might form (such as black rings etc…). So little is known about black hole physics in realistic theories of extra dimensions it is possible that exotic phenomena occur that would allow the theories to be tested or even ruled out based on astrophysical measurements of black holes or other experimental black hole observations (eg. limits on primordial black holes). In the event that I discover such an effect, B.1 would have immense international impact, redefining how we think about testing for the existence of extra dimensions. In the event that B.1 uncovers that no exotic behaviour occurs, it will still have very significant impact, finally resolving the nature of black holes in realistic theories with extra dimensions, and showing such exotic phenomena cannot occur. The direction B.2 is crucial for the field of ab initio study of holographic gauge theories (topic A) to allow quantum calculations to be compared to black hole calculations and hence holography to be tested. Since this is a growing field with growing significance in string theory, lattice field theory and quantum gravity, so B.2 will have important impact. As discussed above for topic A, if discrepancies are seen this has potential to have enormous international significance. The use of classical black holes to deduce properties of strongly coupled gauge theory has already had huge impact, and without the work of B.2 the potential of these ideas cannot be realized. Hence I believe B.2 will have deep impact on the string theory and field theory communities which I expect in the future will widely adopt the numerical gravity tools I will develop in B.2.

Section 2 b. **Methodology**

I will now detail the methodology I propose to employ to achieve the objectives of the two topics detailed above.

Methodology for Topic A*: `Quantum gravity from holography'*

Resources

Two of the postdocs I have requested will work on this topic. The first 3 year postdoc will be hired at the start of the grant. The second will be hired in year 3 allowing an overlapping period where accumulated knowledge can be effectively transferred. I expect these postdocs will have a background in lattice field theory, and also knowledge of string theory and holography. An example of such a person is Anosh Joseph who recently completed his PhD with Catterall in Syracuse. I will work closely with these postdocs on the research directions I will now outline. The cluster I am requesting (200 cores) will be critical in providing the necessary computational resources for myself and my postdocs to carry out the research in this topic.

Methodology for A.1: Solving quantum black holes by ab initio thermal simulation

Studying the quantum mechanical holographic correspondence

Using the supersymmetric lattice methods so far developed, and the new methods to be discussed below, the aim is to study the large N quantum mechanical gauge theory, and to gain precision understanding of it. The thermal properties of the theory will be determined using large scale simulations run on the dedicated cluster I have requested. In the large N limit the behaviour of the thermodynamics should reproduce the predictions from classical black hole thermodynamics on the gravitational side of the holographic correspondence. My previous results with Catterall and those of Nishimura's group have seen consistency with this [3,4] but my aim will be to obtain high precision data to allow a strong test that the holographic correspondence works as conjectured. Beyond confirming black hole behaviour is seen at large N, the next step is to deduce the quantum corrections to this black hole physics. In some sense this is easier, as it is moving to finite N that `turns on' these corrections. However, the challenge is to quantify what such corrections look like, and then measuring them. It is well known in Euclidean semiclassical quantum gravity how to compute semiclassical corrections to black holes using 1-loop path integral techniques. Doing this (in principle) straightforward calculation for the black holes on the gravity side of the correspondence and then comparing the answer quantitively with the lattice simulation finite N corrections to the large N limit would give a fascinating window into quantum gravity. Finding agreement with these semiclassical gravity expectations would provide a highly non-trivial confirmation of these old Euclidean gravity methods - which incidentally are employed in all manner of places, such as cosmology to make predictions about the quantum origin of the universe, even though there are no actual tests of the voracity of these semiclassical methods. Going to small N would allow one to access the truly quantum regime. From the point of view of the lattice simulations this is the easiest regime to tackle since, of course, N and so the number of degrees of freedom, is small. The challenge here is to identify the most meaningful quantities to measure. Since this is so far from the regimes where we have understood gravity, not much is known concretely here. Of course this means there is huge potential for discovery and opening up new research possibilities.

As one raises the temperature in this theory one can see a transition from physics that is described by quantum gravity and that where the full string theoretic nature of the equivalent theory is revealed. At high temperatures the system corresponds to a hot gas of strings (as well as the `D-branes'). The transition between these two behaviours is a fascinating subject. From our previous studies we have seen that the transition appears to be quite smooth, but we were unable to test whether there is a weak first order phase transition or a second or higher order transition. Determining this would be an important goal for the research project. This is a particularly interesting question as I expect it is a universal feature of quantum theories of gravity, and may not be tied to the particular version of the holographic theory that one studies, or indeed possibly the theory of quantum gravity one takes. In the context of the production of black holes at LHC this transition from black holes to a hot gas of strings is of critical importance. Whether the transition is smooth or there is a phase transition may have significant impact on what is observed, particularly if only low mass black holes are formed which are near to the transition temperature and quickly cool to it.

Quantum field theory correspondences

Moving beyond simulation of the quantum mechanics introduces spatial dimensions and the problem is that of gauge field theory. This clearly makes moving to large N more challenging, since to maintain resolution on the lattice more degrees of freedom are needed. In addition, the approach to the correct supersymmetric continuum theory is increasingly subtle in higher dimensions, and full supersymmetric lattice formulations of the theory must be employed. However we have already demonstrated this can be done, even at moderate N [5] in the case of the theory with one compact spatial dimension. The advantage to higher dimensions is that the range of physical phenomena that can occur is even richer. In particular as one varies the temperature and the size of spatial dimensions then phase transitions can occur in the large N limit. This was first understood in my papers [2,6] where such phase transitions were related to phase transitions between different types of black hole in the gravity side of the correspondence. Our recent simulation [5] saw evidence of an expected large N phase transition, but to properly confirm this, to determine its order and properties we require much more study of the theory. One of the most interesting features of this is that the quantitative behaviour of the classical black holes on the gravity side is actually not known as they are of an exotic variety. This is a key element of **Objective B.2** where these solutions will be determined by numerical classical gravity methods.

Detailed Methodology

My work with Catterall to date involved a modest ~ 20,000 processor hours (performed on the LQCD cluster at Fermilab; approx 40 days with 20 processor cores running in parallel). They showed the feasibility of seeing and studying black hole behaviour in the lattice simulations. However these first simulations where too crude to accurately test the black hole behaviour, and far too crude to extract novel quantum physics of black holes. The goal now is to significantly improve the simulations. One approach is simply to move to larger scale parallelization, and proceed in a brute force manner with increasing computational resources. With the computational resources I am requesting in this proposal this is a direction I will employ. However based on much experience with different numerical problems I believe that one should always also try to develop the algorithmic approach to a problem, one that builds in as much physical insight as possible. An improved algorithm typically will always beat brute force scaling up of resources, certainly in the long term. Furthermore we have not exploited key physical aspects of the problem.

There are two physical aspects to our problem that are novel and do not arise in usual (eg. QCD) lattice simulations. Firstly we are interested in the large N limit. Since the theory is strongly coupled this is not a trivial semiclassical limit, but nonetheless quantum fluctuations while important in determining the configurations that dominate the path integral, become relatively suppressed about those configurations. This appears to lead to a significant simplification in the behaviour of the theory, but one that is not taken advantage of in the numerical approach. Secondly due to the supersymmetry enjoyed by our theories they may have a moduli space even at the quantum level, or at least some anomalously light degrees of freedom. The physical degrees of freedom in the theory can then be thought of as the `fast' ones, the usual strongly coupled ones in gauge theory, together with these `slow' ones.

In order to address the first issue I will explore whether there is an improved representation of the theory to work with. In particular I expect the collective field representation may provide such a radically new approach to these problems [7]. Such representations have the advantage that certain simplifications can be explicitly seen at large N. Related methods were utilized a few years ago by Leigh et al [8] to claim an exact *analytic* calculation of the vacuum wavefunction of large N pure gauge theory in 3 dimensions. In fact it did not give the precise answer, but follow up lattice work showed it did give a surprisingly accurate one. I believe by working with such representations we will be able to develop an algorithm where the simplification evident in the semiclassical large N limit can be directly implemented. The main drawback of collective field methods is their non-locality, but dynamical fermions introduce such non-locality anyway, so in our context this does not pose such an obstacle. The strategy will be to test collective field methods in the quenched theory first. This is straightforward to simulate accurately using conventional methods even at large N. Assuming these collective field method give significant improvement, we would then implement them for the full supersymmetric theory.

The second issue, that of light degrees of freedom, has a simple physical origin, namely the slow modes describe the dynamics and shape of the black hole, and the fast modes describe its constituent degrees of freedom. I propose to use the BMN mass term in the quantum mechanical theory to ameliorate this issue. The resulting quantum mechanics is refered to the `BMN plane wave matrix theory' [9]. The big advantage of this regulation, as first discussed in [10], is that even with the regulator the equivalent gravity theory still exists and is known. Thus this mass isn't a regulator to be worked with and then removed. It is intrinsically interesting even for non-zero values, representing a new parameter by which to deform, test and probe the holographic correspondence. An important point is that the classical black hole solutions are not yet known on the gravitational side of the correspondence deformed by this BMN mass term, and it is an important direction addressed later in **Objective B.2** that these will be found.

While this mass term will control the light modes, by lifting the quantum moduli space, it cannot remove the separation of scale between light and heavy modes. Real progress may be made by separating the light and heavy degrees of freedom at the level of the path integral, and then simulating the two with different timescales. A good guess (in an appropriate gauge which can be fixed in the quantum mechanics) for the light degrees of freedom is to take them to be the diagonal components of the various matrices of the gauge and matter fields. In principle one can directly split the path integral up into first an integral over the off-diagonal components of the fields, and then these diagonal components. This is likely to be far more efficient if the lattice equilibration time for the off-diagonal modes is considerably quicker when the light degrees of freedom are fixed. These effects can be studied to some extent already in the quenched theories. I propose to study this separation of degrees of freedom and determine the viability of the method. It is possible that improved guesses for splitting the degrees of freedom may be required, and this is something that would also be studied, using physical input and insight gained from simulation. It is also worth noting that the split between dynamics/shape degrees of freedom and internal constituent degrees of freedom is fascinating from the perspective of the corresponding black hole physics and insight gained will also have impact in the theoretical understanding of the holographic correspondence.

For the gauge theory with a compact space circle one of the black holes in the gravity side of the correspondence (the `localized' one) has a profile depending on the spatial circle direction in a specific way. On the lattice the physics will not naively reflect this localization as all possible positions of the black hole are summed over. However, if one can successfully separate the light and heavy degrees of freedom in the theory as discussed above, it should be possible to 'freeze' the one degree of freedom corresponding to the centre of mass of the gauge theory plasma on the circle. In this case, computing one point functions of the stress tensor may allow a very explicit comparison of the profile of this plasma with the corresponding gravity black hole prediction and open up new research possibilities into understanding the relation between locality in the field theory and the dual gravity theory.

Another important issue that affects Monte Carlo simulation for these theories is the `sign problem', namely the Euclidean action used to perform the Monte Carlo simulation is not necessarily real. Our work and that of the Japanese group employs the method of `reweighting' to tackle this sign problem. In our recent work [10] we have carefully computed the complex phase of this action and indeed its fluctuations away from zero are on average small, justifying this reweighting approach. However, we have little understanding why this phase appears to play little dynamical role, where in other theories with fermions (such as QCD) it can be a major obstacle to Monte Carlo methods. I intend to investigate whether we can gain analytic understanding for this phase by thinking about its origin in the gravitational side of the correspondence. Furthermore, I intend to see whether the isolation of the slow degrees of freedom from the fast ones can be used to control this phase.

Methodology for A.2: Ab initio calculation of the spectrum of quantum gravity

Thermal lattice formulations are well suited to studying the finite temperature behaviour of these field theories in the large N limit. However, another important area that we would like to learn about in quantum gravity is the spectrum of states in the theory, and in particular the nature of the spectrum in the energy range where the states account for the microphysics of black holes. There are various conjectures for the behaviour of the energy levels of these type, and it is an important challenge to confirm whether these hold.

In principle knowing the spectrum gives one the detailed quantum dynamics of the theory, not just its thermal equilibrium behaviour. Whilst realistically I would not expect to be able to extract the complete spectrum, determining some features of it would allow us to probe the dynamics. The key driving questions would be whether one can use this to understand *black hole evaporation* and *information loss*. These are incredibly profound questions originating in Hawking's work in the 1970's that sadly so far have received little input from holography even though it represents the first controlled theory of quantum gravity. Any progress in these directions would be huge breakthroughs in the string theory and quantum gravity fields.

I will pursue a numerical Hamiltonian approach to solving the gauge theories of interest. Whilst it is well known how to describe gauge theories in a Hamiltonian setting, certainly the simplest example of this is the quantum mechanical case, where the Hamiltonian is well understood and various formal mathematical properties of it have even been studied in the 1990's in the context of M-theory (for example, it is known to have a continuous spectrum). A few years ago there has was numerical work (using only desktop computing) looking at the low lying spectrum of this theory with N=2 [11]. Being at such small N these do not tell us about quantum gravity. In the different setting of the light-cone formulation impressive numerical work with a Hamiltonian formulation has been employed using much greater computing resources [12]. However this was a two dimensional theory where the Hamiltonian methods were used in the context of Discrete Lightcone Quantisation (which is formally only approximate as it ignores certain zero modes) which is a rather complicated setting. No information about quantum gravity and black hole physics was extracted, although an impressive check of holography was performed by studying vacuum correlators. This work is encouraging in that Hamiltonian methods in related theories to ours have been seen to work, and I believe it is now a case of using them to ask the physically interesting questions for quantum gravity. Another very attractive feature of the Hamiltonian approach is that the `sign problem' of the thermal lattice methods do not arise.

Detailed Methodology

In the modern setting of holography the aim would be to focus on the quantum mechanical version of the holographic equivalence and expand the wavefunction in an appropriate finite basis (for example a Fock basis). The Hamiltonian operator would then be diagonalized using standard numerical methods (for example the Arnoldi methods). Systematically increasing the size of the basis then allows the true Hamiltonian spectrum to be constructed. I believe that if one can appropriately take into account the global symmetries in the theory, which simply relate various states, then given the progress made in [11] and the scale of the numerical problem tackled in [12] I think it is realistic to assume significant progress can be made into determining the spectrum of the quantum mechanical theory at large N where it will be possible to see black holes physics.

Methodology for Topic B: *`Classical gravity in fundamental theory'*

Resources:

The third postdoc I am requesting will work on this topic. They will be hired at the start of the grant. They should have detailed knowledge of gravity and also some experience of programming and ideally numerical methods. Perfect examples of such an individual are my collaborator Pau Figueras (Cambridge) or Jorge Santos (UCSB). In addition to this postdoc, the PhD student I am requesting will also work on this topic. They will begin in the second year of the grant. This will ensure that there will be well posed problems that a starting student can immediately engage with. I envision the student would be strongly guided by the postdocs, and interact closely with them. This will enable the postdocs to develop leadership experience, and allow the PhD student to be bought rapidly up to speed on the technical side of the problems.

Methodology for B.1: Black holes at LHC and in realistic theories of extra dimensions

LHC black hole physics

For LHC to produce black holes there must be at least two large additional dimensions. These dimensions would be so large that whilst formally they have finite size, for the black holes produced they are effectively infinite. Hence understanding black hole formation at LHC requires one to understand black hole formation in pure gravity in 6 or more spacetime dimensions with asymptotically flat boundary conditions. Only one type of black hole is explicitly known, the Myers-Perry solution. However it is conjectured that various other more exotic solutions also exist (black rings, Saturns, deformed Myers-Perry etc). Of course if we are to make predictions for LHC, or analyse LHC data, then it is imperative that we determine whether these solutions exist, what their stability properties are and hence whether they can form. Using my stationary numerical framework and the tools to test stability I will develop I will begin a systematic computation of these higher dimensional solutions. With the LHC upgrade to its full 14 TeV energy occurring over the next 5 years, this is a time critical project that is likely to have considerable impact in detailed LHC black hole production analysis.

Black holes in realistic theories of extra dimensions

In some theories of extra dimensions, for example Kaluza-Klein theory, it is well understood how the transition from higher dimensional behaviour to 4 dimensional behaviour works. However in more phenomenologically realistic models this is poorly understood. As mentioned in the introduction, in the Randall-Sundrum II model it was widely believed for a decade that astrophysical black holes behaved completely differently to those in usual 4 dimensional Einstein gravity, prompting strong constraints to be imposed on the model. However, by finally numerically finding these solutions I very recently explicitly proved this thinking wrong [1].

There are two important sources of uncertainty that mean the behaviour of black holes in realistic theories of extra dimensions is very poorly understood. Firstly in any realistic theory one must stabilize the size of the extra dimensions. There have been simple proposals to do this, for example the Goldberger-Wise mechanism, where certain matter is added that acts like a spring tensioning the extra dimension to a set radius. It is very unclear what effect this has on black hole solutions and indeed it is a numerical problem just to correctly include the backreaction of this stabilzation field even in the vacuum. Secondly, one should include the tension of any branes present in large extra dimensions, and this has the effect of 'warping' the geometry of the extra dimensions. For example, in the canonical model of this warped geometry, the Randall-Sundrum I model (very widely studied with > 5000 citations) actually the phenomenology of black holes is not at all understood. Whilst a homogeneous warped black string solution does exist (in the absence of a stabilization mechanism at least) which would reproduce 4 dimensional physics, it is also expected that in the stabilized theory black holes may exist on the IR brane of the `plasmaball' type [21] (these plasmaball solutions will be discussed more below in objective B.2). It is unclear which large black hole - the string or the plasmaball - is relevant for astrophysics. I will extend my numerical framework to include matter and study black hole solutions in these phenomenologically viable models of extra dimensions, deducing whether they can be used to constrain or even rule the models out based, for example, on astrophysical black hole observations.

Detailed Methodology

The numerical framework developed so far has focussed on finding stationary solutions to pure gravity. However the physical applications of this problem require us to solve gravity coupled also to matter fields. A first step in this research program is therefore to generalize the framework to include the various matter types we expect to be relevant. For pure gravity one may use Ricci flow as a tool to find stationary solutions. However Ricci flow can be derived from a gradient flow of the Einstein-Hilbert action - the action for pure gravity. Therefore I believe once other matter fields are introduced, the general framework I have developed will extend by constructing the elliptic problem for gravity plus these matter field and solving it as a gradient flow (or using Newton's method) on an appropriate action. For gauge fields a gauge fixing must be introduced to ensure ellipticity, and I expect this to take the similar form to the 'De Turck' term in the gravity context. We must also undertake the formal task of constraining the existence of 'Ricci solitons' (or their generalization with matter), and I expect that we will be able to prove their non-existence in many cases.

The numerical methods developed so far have been focussed on finding stationary solutions by phrasing the gravity equations as a standard elliptic problem. However the physical relevance of a time independent solution is determined by its stability. Whilst unstable solutions are interesting at a formal level, they will play no role in dynamics unless the instability timescale is extremely long. Thus after finding a numerical solution it is important to determine its stability. If stable, an important property is the lowest quasi-normal mode of the solution - the linear response that governs the approach to the stable equilibrium solution. It should be emphasized that computing the stability of even analytically known solutions, such as Myers-Perry, is challenging and has only been done very recently. In principle it is clear how to proceed. I will construct the linear numerical problem of perturbations about the numerically known solution. Then we can assume the canonical time dependence of an instability, $\exp(\omega\, t)$, and search for real positive values of ω, or alternatively solve the evolution of the time dependent linear problem starting from some initial perturbation and watch to see if an instability occurs. I propose to test both these methods.

However there is also a more subtle approach. Whilst computing the spectrum of a linear operator that is only known numerically is complicated, really we simply wish to answer a yes/no question -- is the solution stable or not? Hence the subtle approach is to try to find an analytic way to write the perturbation equations on the numerically known background so that one might show stability or instability by computing certain properties of the background, rather than the details of the whole spectrum of perturbations. Due to the well known difficulty in constructing perturbations of general higher dimensional black holes it is highly unlikely that a single quantity will be found that if positive implies stability **and** if negative implies instability. However, it is likely that *some* quantity(ies) may be found whose positivity implies stability **or** whose negativity implies instability. Hence one might not be able to get sharp bounds on the stability or instability regions of the space of solutions, but one might nonetheless get clean determinations of regions that are unstable or stable. The recent work of Reall et al [24] provides some hint that such methods may be found - these authors considered tools to demonstrate instability of an analytic solution without solving the full perturbation problem, but rather using a variation approach to give a trial perturbation with overlap with an instability. This involved solving an elliptic equation which can be done on a numerical background, but is certainly not optimal. However, it does indicate that such approaches have potential. I believe significant progress will be made by considering the perturbation equations and using the recent progress that has been made in understanding their structure.

Moving to higher dimensions, and including dependence on various dimensions implies the p.d.e. problem depends on many variables. Whilst elliptic p.d.e.s in two and three variables are straightforward, in four they become hard, and in more than five, very problematic. The essential problem is maintaining numerical resolution which requires increasing amounts of memory as one moves to higher dimensions. We have already made some progress in this direction, moving from real space discretization to pseudo spectral implementation of solution of the elliptic systems [16]. However the essence of our approach is to treat the metric components as functions of the coordinates and then represent these functions using standard p.d.e. method (such as the pseudo spectral representation).

The numerical work on Calabi-Yau geometries has hinted that one may be able to do better. In this problem the geometries are of a very special type - so called Kahler - which hugely simplifies the problem. In Donaldson's method of numerically finding solutions he uses a beautiful representation of the geometry as an `algebraic metric'. This may be thought of as a preferred spectral basis (derived from embedding the Calabi-Yau as an algebraic curve in CP^N) which is tailored to the geometry in question. Using this Headrick has recently shown that even for 6 dimensional Calabi-Yau's the geometries may be computed straightforwardly with high accuracy and modest resources. The question is then whether there are preferred spectral representations of more general metrics. The most obvious approach is to follow that of Donaldson's algebraic metrics. If one has a black hole of interest, one should try to embed this into some simple higher dimensional space where the eigenfunctions of the Laplacian are known explictly. This embedding would not induce the correct black hole geometry that solves the Einstein equations, but rather just some smooth geometry with the correct topology. These eigenfunctions may then be pulled back onto the embedding to give an over-complete basis of functions which one can use to represent the metric solution of the black hole. One can try to reduce these to a complete basis, or alternatively simply work in the over-complete basis. If this generalization of Donaldon's approach to general geometries is effective it has the potential to vastly open up the space of problems that one might tackle.

Methodology for B.2: Classical black holes and the holographic correspondence

Testing holography by comparision with ab initio calculation

In order to test the idea of holography, we need to know the properties of the black holes which describe the thermal behaviour of the gauge theories that we hope to compute using ab initio methods of Topic A. As discussed above the gauge theories that are most likely to be cleanly numerically solved to high precision first are the gauged quantum mechanics with supersymmetric 'BMN' mass deformation and the gauge field theory on a spatial circle. It is sobering to think that in neither example are the classical black hole solutions on the gravitational side of the holographic correspondence known, as these black holes lack the symmetry to allow analytic or simple numerical (by which I mean solving o.d.e.s) construction. Once I have added matter fields to my numerical approach, the tools will then be in place to tackle the problem of the black holes corresponding to the thermal physics of these theories. Computing this thermal physics directly in the gauge theory in the large N limit using the numerical lattice methods of **objective A.1** will allow me to directly compare with the classical black hole predictions, to give highly non-trivial tests of the conjectured holographic correspondence. Assuming it holds true, these will be among the most non-trivial tests of holography to date. Of course in the unlikely event I see deviations that suggest the correspondence does not hold, this would be a huge discovery with tremendous impact in the string theory community.

Application to QCD

A huge amount of research has been invested in trying to understand QCD heavy ion collision using insight gained from gauge theories at large N, which are similar to QCD although not identical, but have a classical holographic gravity description. An important early result I obtained using my numerical framework was the discovery of metastable states in these large N confining gauge theories that model QCD which I called "plasmaballs" [21]. In the gravity side of the holographic correspondence these are black holes which I explicitly constructed in a certain large energy limit. These are tremendously important in understanding the dynamics of plasma in confining gauge theories such as QCD using holography. The thermalization of plasma can be thought of in the gravity side of the correspondence as formation of a black hole from a high energy collision and the end state of this process is the `plasmaball' black hole. As I discussed in [21] in order to address relaxation and then subsequent hadronization in these models of heavy ion physics, the properties of these plasmaballs must be understood. In particular, the quasinormal mode spectrum of these objects will govern the relaxation to quasi-equilibrium. My original calculation of the plasmaballs was only in the large energy limit, and in order to develop the connections to heavy ion physics one would need these solutions at finite energies. I will use the numerical framework to solve for these solutions, and in addition their quasi-normal mode excitations. With the intense interest internationally in the dynamics of black hole formation in holography and its application to QCD physics, this research is likely to have great impact.

Strongly coupled field theory in curved spacetime

A new emerging and exciting area is learning about strongly coupled gauge theories by studying their holographic gravity description and deforming the spacetime the gauge theory lives on. The physics of free field theories in curved spacetime has been extensively studied in the 1970's, with famous results such as Hawking's that theories put in black hole spacetimes thermally radiate, indicating that black holes have a temperature. By studying certain classical gravity solutions with appropriate boundary conditions we may also understand properties of holographic strongly coupled gauge theories on curved spacetimes of our choosing. It is worth emphasizing that these calculations would be essentially impossible to do directly in the gauge theory due to its strong coupling, and lattice methods are not suited to solve gauge theory on curved spacetimes. Initial work exploring features of gravity solutions describing field theory on curved spacetime in a thermal vacuum was introduced in [22]. However, for interesting curved spacetimes it was clear that analytic methods could not be used to find the required gravity solutions, and that paper only conjectured their existence and postulated what their properties might be. Recently in [16] using the new numerical techniques I found the first gravity solution corresponding to a field theory on a curved spacetime that asymptotically was in the zero temperature vacuum. In fact we found the solution for the gauge theory in a black hole background in the Unruh vacuum. This solution surprised many leading researchers in the field who had previously presumed it could not exist - its existence was thought to be ruled out by the same logic that implies black holes in Randall Sundrum II had peculiar properties. This solution was therefore the first step in showing that line of reasoning was quite incorrect, and its impact on the community has been significant because of this. Whilst we found the dual for zero temperature asymptotic boundary conditions, placing the theory at finite temperature is thought to yield many more interesting solutions and behaviours and I will study these.

I will pursue the fascinating recent claim [23] that stationary black holes exist in the gravity side of the correspondence with non-Killing horizons that describe the gauge theory on spacetimes with two black holes at different temperatures. In the gauge theory picture energy flows from the hot black hole to the cooler one. In the gravity side the black hole horizon describing this flowing plasma has a velocity which is not in a symmetry direction (hence the horizon is not Killing). Studying linear response theory in holography has had a tremendous impact, for example with the discovery of the very low shear viscosities. These new solutions give the exciting opportunity *to go far beyond linear response*, to allow highly non-trivial transport behaviour of stationary flows that are far out of equilibrium to be studied. I will find them numerically and use them to examine the transport of heat from a hot to cold source in these strongly coupled gauge theories, where there is no possibility to address this out of equilibrium physics without using these holographic tools. It is likely very interesting results and physics will be seen, and there is much room for surprises.

Detailed Methodology

The methodology for this problem is similar to that of problem 3. However there is important additional technology to develop. The techniques I have up to now to find stationary black hole solutions depend on an important assumption, namely that the black hole has a Killing horizon with the rigidity property, so that they have angular (or linear) momentum only in directions that are associated to symmetries. This is physically important to understand why the problem remains elliptic even in the presence of ergo-regions [15]. However, in the heat flow part of this problem we are interested in certain equilibrium black holes whose horizons are *not* Killing horizons. We may say that while the solution as a whole is a stationary one, with no time dependence, the horizon has some velocity in a direction that is not a symmetry. Understanding how to extend the numerical framework to this case is an important goal of this problem. The main obstacle is how to understand boundary conditions at the horizon that are compatible with ellipticity. I believe that by studying local frames near such a horizon which move with the horizon, similar methods to those I developed for Killing horizons can be employed. There are no examples of such exotic black holes known, and this makes it even more important to develop tools to study them, given that they are likely to play a very interesting role in the physics of holography. Finding such solutions would open up entirely new lines of research in black hole physics with qualitatively different questions and behaviour to conventional solutions which all have Killing horizons.

Section 2 c. **Resources**

The major factor limiting the development of the numerical approaches to tackling these questions is the lack of a group. For the initial exploratory development of the methods it has been sufficient for me to work with my students (Kitchen, Adam), my long term collaborator Catterall (Syracuse), a variety of other collaborators (although not necessarily all involved in the numerical side of projects) and most recently Figueras (a postdoc in Cambridge). However now to develop the numerical program past this exploratory phase into a mature field that is capable of realizing the results that it clearly has the potential to do, it is necessary for me to build a dynamic group locally at Imperial College dedicated to these research directions. Whilst I have a clear picture of where the research must go over the next 5 years, I would be incapable of implementing a fraction of it without the support of such a dedicated research team. Beyond requesting half of my salary, I therefore request funds to support 3 postdoc positions (each 3 years), together with a PhD studentship (a 3.5 year position). In addition to the salaries for these posts, I am requesting reasonable travel expenses for myself and my group, to enable visits to my current collaborators in the US and elsewhere, to allow new collaborations to be built, particularly with the group at KEK, Japan, and to allow travel to high profile international meetings (such as the various string theory and gravity conferences held each year). I am also requesting funds for equipment to buy the necessary specification desktop computers needed for numerical work, and also laptops for the postdocs to enable them to work effectively while traveling.

In the past I have run simulations largely on desktop computers for problems in numerical classical gravity. I have also used the Imperial college High Performance Computing (HPC) center cluster. For numerical quantum gravity simulation I have used the LQCD cluster in Fermilab (US) which I have a (relatively modest) time allocation on for collaboration with Catterall. However, for the group I will build to function well, and in particular to be able to develop code and access the much greater resources required now in this phase of the research, it is essential to have dedicated time on a cluster. I will achieve this by buying a 200 core cluster dedicated to my group. These will be administered by the Imperial HPC center, although I emphasize my team would have sole access to these processors.

Another important aspect of the program is to organize 3 workshops during the 5 year period, each lasting 5 days. The aim of these is to bring the relevant international researchers to London in order to foster collaboration, and allow effective dissemination of our results. During the past 5 years, using the money from my Halliday award I organized 2 such meetings, each with ~ 30-40 high profile international participants (~1/3 from the US and Japan, ~2/3 from Europe), one on higher dimensional black holes, the other on novel numerical gauge theory methods and holography. I found these tremendously useful for my own research, but also for promoting and establishing the use of my novel numerical methods. Since this proposal is roughly divided in half between numerical approaches to quantum and classical gravity, I propose to have a meeting on each topic in years 2 and 3 and then one more meeting in year 4 on the topic that I believe at that point will most benefit. I have found that ~30-40 international participants is a good number to allow invitation of the key leaders in relevant research fields, and also the most promising junior researchers, and yet have a group that is small enough to have genuinely interactive discussion and close participation. These workshops can be organized for approximately £15,000 each at Imperial college, including travel expenses for some speakers, hotel accommodation, conference dinner, room booking and conference fee expenses. I therefore am requesting £45,000 to fund the 3 proposed meetings.

In order to build existing and new collaborations I am requesting money to support a visitor program. I am requesting funds to support approximately one month of stay in London per year, which may be taken by a single visit, or possibly up to 4 one week visitors. I have estimated this will require £3,000 per year. Amongst the visitors I will invite are Catterall (Syracuse, US - numerical lattice simulation), Nishimura (KEK, Japan - numerical lattice simulation), Berenstein (UC Santa Barbara, US - numerical holography), Pretorius (Princeton, US - numerical GR), Lehner (Perimeter Institute, Canada - numerical GR).

Summary of the requested resources for the 5 year duration of the grant;

PI (Wiseman): 50% salary, £3,000 travel/year, £10,000 equipment (£7.5k initially, and £3k in year 3 to update equipment)

3 postdocs: each for 3 year positions ; two start in year 1, the other in year 3, expected to have completed PhD and one previous postdoc. In addition to salary, £3,000 travel/year and £2,500 equipment each.

1 PhD student: a 3.5 year position, to start in year 2. In addition to salary, £2,000 travel/year and £1,000 equipment.

Cluster Computing: £50,000 to purchase 200 cores dedicated to my group. To be administered by the Imperial HPC Center cluster. (current cost; £12,000 per 48 cores - using Intel Xeon 6 core processors, each with 4Gb RAM)

3 one week workshops: ~£42,000. Workshops will have ~30-40 international and UK invited participants; (each estimated from previous experience at £14,000)

Visitor program: £15,000. 1 month/year (probably divided between several visitors, (estimated £3,000 per year, including accommodation and travel)

	Cost Category	Year 1	Year 2	Year 3	Year 4	Year 5	Total (Y1-5)
Direct Costs:	*Personnel:*						
	PI						
	Senior Staff						
	Post docs						
	Students						
	Other						
	Total Personnel:						
	Other Direct Costs:						
	Equipment						
	Consumables						
	Travel						
	Publications, etc						
	Other						
	Total Other Direct Costs:						
	Total Direct Costs:						
Indirect Costs (overheads):	Max 20% of Direct Costs						
Subcontracting Costs:	(No overheads)						
Total Costs of project:	(by year and total)						
Requested Grant:	(by year and total)						

For the above cost table, please indicate the % of working time the PI dedicates to the project over the period of the grant:	**50%**

Summary

This is an interdisciplinary proposal between string theory, lattice field theory and numerical General Relativity. It will develop existing and novel numerical methods to address key gravitational issues in fundamental theory, namely how to perform computations with the holographic formulations of quantum gravity, and how to understand the phenomenology of extra dimensions and test theories such as string theory using LHC and astrophysical observation. I have a proven track record in pioneering a novel numerical approach in both fields. Now is exactly the time when I require the postdoc, PhD student and computing resources to ensure that I can maintain my international lead in these exciting high impact areas. These are high risk/high gain directions, and my methodology is far from incremental, being focused on using new insights and ideas to push forward well beyond the current state of the art. The risk is ameliorated by my flexible approach, in particular having two main topics of focus so that if one falters, the other may be expanded. In addition I have outlined a variety of approaches that will be used to make progress. From my considerable experience with numerical problems I am confident great progress will be made with the resources requested. I expect that both topics will flourish into high impact research areas.

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Section 2d. **Ethical and Security sensitivity Issues**

ETHICS ISSUES TABLE

	Research on Human Embryo/ Foetus	YES	Page
	Does the proposed research involve human Embryos?		
	Does the proposed research involve human Foetal Tissues/ Cells?		
	Does the proposed research involve human Embryonic Stem Cells (hESCs)?		
	Does the proposed research on human Embryonic Stem Cells involve cells in culture?		
	Does the proposed research on Human Embryonic Stem Cells involve the derivation of cells from Embryos?		
	I CONFIRM THAT NONE OF THE ABOVE ISSUES APPLY TO MY PROPOSAL	YES	

	Research on Humans	YES	Page
	Does the proposed research involve children?		
	Does the proposed research involve patients?		
	Does the proposed research involve persons not able to give consent?		
	Does the proposed research involve adult healthy volunteers?		
	Does the proposed research involve Human genetic material?		
	Does the proposed research involve Human biological samples?		
	Does the proposed research involve Human data collection?		
	I CONFIRM THAT NONE OF THE ABOVE ISSUES APPLY TO MY PROPOSAL	YES	

	Privacy	YES	Page
	Does the proposed research involve processing of genetic information or personal data (e.g. health, sexual lifestyle, ethnicity, political opinion, religious or philosophical conviction)?		
	Does the proposed research involve tracking the location or observation of people?		
	I CONFIRM THAT NONE OF THE ABOVE ISSUES APPLY TO MY PROPOSAL	YES	

	Research on Animals[1]	YES	Page
	Does the proposed research involve research on animals?		
	Are those animals transgenic small laboratory animals?		
	Are those animals transgenic farm animals?		
	Are those animals non-human primates?		
	Are those animals cloned farm animals?		
	I CONFIRM THAT NONE OF THE ABOVE ISSUES APPLY TO MY PROPOSAL	YES	

[1] The type of animals involved in the research that fall under the scope of the Commission's Ethical Scrutiny procedures are defined in the Council Directive 86/609/EEC of 24 November 1986 on the approximation of laws, regulations and administrative provisions of the Member States regarding the protection of animals used for experimental and other scientific purposes Official Journal L 358 , 18/12/1986 p. 0001 - 0028

	Research Involving non-EU Countries (ICPC Countries[2]) [3]	YES	Page
	Is the proposed research (or parts of it) going to take place in one or more of the ICPC Countries?		
	Is any material used in the research (e.g. personal data, animal and/or human tissue samples, genetic material, live animals, etc) : a) Collected in any of the ICPC countries?		
	b) Exported to any other country (including ICPC and EU Member States)?		
	I CONFIRM THAT NONE OF THE ABOVE ISSUES APPLY TO MY PROPOSAL	YES	

	Dual Use	YES	Page
	Research having direct military use		
	Research having the potential for terrorist abuse		
	I CONFIRM THAT NONE OF THE ABOVE ISSUES APPLY TO MY PROPOSAL	YES	

[2] In accordance with Article 12(1) of the Rules for Participation in FP7, 'International Cooperation Partner Country (ICPC) means a third country which the Commission classifies as a low-income (L), lower-middle-income (LM) or upper-middle-income (UM) country. Countries associated to the Seventh EC Framework Programme do not qualify as ICP Countries and therefore do not appear in this list.

[3] A guidance note on how to deal with ethical issues arising out of the involvement of non-EU countries is available at: ftp://ftp.cordis.europa.eu/pub/fp7/docs/developing-countries_en.pdf