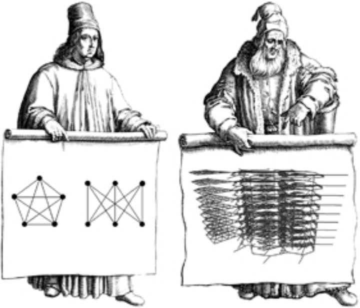
Research Topics in Graph Theory and Its Applications

Vadim Zverovich



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By

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In memory of my son, Vladik (1987 – 2000)

Contents

Preface

This book includes a number of research topics in graph theory and its applications. The topics are in the form of research projects developed by the author over the last 15 years. We discuss various research ideas devoted to α-discrepancy, strongly perfect graphs, the reconstruction conjectures, graph invariants, hereditary classes of graphs, embedding graphs on topological surfaces, as well as applications of graph theory, such as transport networks and hazard assessments based on unified networks. In addition to the original research ideas presented and methods to address them, there are also examples of impact statements, project resources required, support letters and work plans. The book has a free-form structure that allows the reader freedom, that is, the chapters are independent and can be read in any order.

Another important feature is the inclusion of reviewers' opinions in each chapter, which outline the strengths and weaknesses of various aspects of multiple research projects. The viewpoints of reviewers will be useful for recognising the typical mistakes authors make in research proposals. This book is ideal for developers of grant proposals, as well as for researchers interested in exploring new areas of graph theory and its applications. Advanced students in graph theory may use the topics presented in this book to develop their final-year projects, master's theses or doctoral dissertations.

It is the author's hope that this publication of original research ideas, problems and conjectures will instigate further research, or even a resurgence of interest, in the aforementioned important areas of graph theory.

I am very grateful to my wife and two daughters for their patience and support during the completion of this book.

Chapter 1

α-Discrepancy and Strong Perfectness

1.1 Background and Aims

Discrepancy theory, which originated from number theory, can generally be described as the study of irregularities of distributions in various settings. Classical combinatorial discrepancy theory is devoted to addressing the problem of partitioning the vertex set of a hypergraph into two classes in such a way that all hyperedges are split into approximately equal parts by the classes. That is, it is devoted to measuring the deviation of an optimal partition from a perfect partition when all hyperedges are split into equal parts. It should be noted that many classical results in various areas of mathematics (e.g. geometry and number theory) can be formulated in such terms. Combinatorial discrepancy theory was introduced by Beck in [3] and studied in [2]–[5] and [23]. Füredi and Mubayi [12] indicated that discrepancy theory had "developed into an elaborate field related ... to geometry, probability theory, ergodic theory, computer science, and combinatorics", while Tezuka [26] described its application to finance.

Among practical applications of the theory are image processing and the Monte Carlo methods for high dimensions. An important role in discrepancy theory is played by the fundamental "six-standard-deviation" result [25] and the discrepancy conjecture [4]. An interesting version of discrepancy is α-discrepancy, which occurs when success is measured by minimizing the imbalance of the vertex set while keeping the imbalance of each hyperedge at least α. The basic results in this context are devoted to 1-discrepancy and are concentrated on upper bounds and the Füredi–Mubayi conjecture [12].

The development of graph theory over the last five decades has been strongly influenced by the Strong Perfect Graph Conjecture and perfect graphs introduced by Berge in the early 1960s [6]. Perfect graphs are a fundamental concept in graph theory. This class of graphs has interesting applications, and there are books entirely devoted to perfect graphs (e.g. [7, 13]). The famous Strong Perfect Graph Conjecture, stated by Berge, had been open for about 40 years. Various attempts to prove it gave rise to many powerful methods, important concepts and interesting results in graph theory. Some of those methods affected the development of the theory of modular decomposition and Fulkerson's theory of antiblocking polyhedra. Chudnovsky, Robertson, Seymour and Thomas [9] relatively recently proved the Strong Perfect Graph Conjecture on 179 pages.

In 1978 at a Monday Seminar in Paris, Berge introduced another important class of graphs called strongly perfect graphs. It is a subclass of perfect graphs (see [8]). A graph is *strongly perfect* if every induced subgraph contains an independent set that meets all maximal cliques. The known results on strongly perfect graphs can be found in the survey paper [22]. Unlike perfect graphs, strongly perfect graphs do not have a conjecture similar to the Strong Perfect Graph Conjecture.

The class of absorbantly perfect graphs was introduced by Hammer and Maffray in [15]. A graph is *absorbantly perfect* if every induced subgraph has a minimal dominating set that meets all maximal cliques. Absorbantly perfect graphs are important because they form a superclass of strongly perfect graphs. The well-known classes of bipartite graphs and Meyniel graphs are subclasses of the class of strongly perfect graphs [16, 21]. In fact, we have the following chain of strict inclusions for these subclasses of perfect graphs:

$$\{\text{Bipartite}\} \subset \{\text{Meyniel}\} \subset \{\text{Strongly perfect}\} \subset$$
$$\{\text{Absorbantly perfect}\} \subset \{\text{Perfect}\}.$$

The basic aims of the proposed project are as follows (not in order of priority):

1. Find good bounds for α-discrepancy of hypergraphs.
2. Generalise the Füredi–Mubayi conjecture for α-discrepancy.
3. Formulate a characterisation conjecture on strongly perfect graphs and attempt to prove it.

Research conjectures are important for stimulating research and making progress in the corresponding areas. Any conjecture must ultimately be proved or disproved, but it is important to underline that the disproof of a conjecture does not necessarily mean a 'negative' result. It may yield insights into the problem and result in new conjectures and theorems.

To sum up, the basic idea of the proposed research project is to further develop discrepancy theory, and formulate the characterisation conjecture for strongly perfect graphs and then prove it theoretically. Note that the formulation of the conjecture includes both theoretical research and the development and application of scientific software, which is a new approach to the problem. Moreover, this research proposal meets the funder's mission and strategy because it will attract a talented post-doctoral researcher, stimulate adventure in developing the new approach and build collaboration between the investigator and the post-doctoral researcher.

1.2 α-Discrepancy

Let $\mathcal{H} = (V, \mathcal{E})$ be a hypergraph with the vertex set V and the hyperedge set $\mathcal{E} = \{E_1, ..., E_m\}$. One of the main problems in classical combinatorial discrepancy theory is to colour the elements of V by two colours in such a way that all of the hyperedges have almost the same number of elements of each colour. Such a partition of V into two classes can be represented by a function $f : V \to \{+1, -1\}$. For a set $E \subseteq V$, let us define the *imbalance* of E as follows:

$$f(E) = \sum_{v \in E} f(v).$$

First defined by Beck [3], the *discrepancy of* $\mathcal{H}$ *with respect to* f is

$$\mathcal{D}(\mathcal{H}, f) = \max_{E_i \in \mathcal{E}} |f(E_i)|$$

and the *discrepancy* of $\mathcal{H}$ is

$$\mathcal{D}(\mathcal{H}) = \min_{f\colon V \to \{+1,-1\}} \mathcal{D}(\mathcal{H}, f).$$

Thus, the discrepancy of a hypergraph tells us how well all its hyperedges can be partitioned. Spencer [25] proved the fundamental "six-standard-deviation" result that for any hypergraph $\mathcal{H}$ with n vertices and n hyperedges, $\mathcal{D}(\mathcal{H}) \leq 6\sqrt{n}$. As shown in [1], this bound is best possible up to a constant factor. More precisely, if a Hadamard matrix of order $n > 1$ exists, then there is a hypergraph $\mathcal{H}$ with n vertices and n hyperedges such that $\mathcal{D}(\mathcal{H}) \geq 0.5\sqrt{n}$. It is well known that a Hadamard matrix of order between n and $(1-\epsilon)n$ does exist for any ϵ and sufficiently large n. The following important result, due to Beck and Fiala [4], is valid for a hypergraph with any number of hyperedges: $\mathcal{D}(\mathcal{H}) \leq 2\Delta - 1$, where Δ is the maximum degree of vertices of $\mathcal{H}$. They also posed the following conjecture:

Discrepancy Conjecture [4] *For some constant* K*,*

$$\mathcal{D}(\mathcal{H}) < K\sqrt{\Delta}.$$

Another interesting aspect of discrepancy was discussed by Füredi and Mubayi [12]. A function $g : V \to \{+1, -1\}$ is called an *α-function* of the hypergraph $\mathcal{H}$ if

$$g(E_i) = \sum_{v \in E_i} g(v) \geq \alpha$$

for every hyperedge $E_i \in \mathcal{E}$, that is, each hyperedge has an imbalance at least α. The *α-discrepancy* of $\mathcal{H}$, denoted by $\mathcal{D}_\alpha(\mathcal{H})$, is defined in the following way:

$$\mathcal{D}_\alpha(\mathcal{H}) = \min_g g(V),$$

where the minimum is taken over all α-functions of $\mathcal{H}$. Thus, in this version of discrepancy, the success is measured by minimizing the imbalance of the vertex set V, while keeping the imbalance of every hyperedge $E_i \in \mathcal{E}$ at least α.

One of the basic results in α-discrepancy for $\alpha = 1$ is due to Füredi and Mubayi [12]:

Theorem 1.1 [12] *Let $\mathcal{H}$ be an n-vertex hypergraph with hyperedge set $\mathcal{E} = \{E_1, ..., E_m\}$ and suppose that every hyperedge has at least k vertices, where $k \geq 100$. Then*

$$\mathcal{D}_1(\mathcal{H}) \leq 4\sqrt{\frac{\ln k}{k}}n + \frac{1}{k}m.$$

This theorem can be easily re-formulated in terms of graphs by considering the neighbourhood hypergraph of a given graph. Note that $\gamma_s(G)$ is the signed domination number of a graph G, which corresponds to 1-discrepancy of the neighbourhood hypergraph of G.

Theorem 1.2 [12] *If G has n vertices and minimum degree $\delta \geq 99$, then*

$$\gamma_s(G) \leq \left(4\sqrt{\frac{\ln(\delta+1)}{\delta+1}} + \frac{1}{\delta+1}\right)n.$$

Moreover, Füredi and Mubayi [12] found quite good upper bounds for very small values of δ (for $\delta \le 19$) and, using Hadamard matrices, constructed a δ-regular graph G of order 4δ with

$$\gamma_s(G) \ge 0.5\sqrt{\delta} - O(1).$$

This construction shows that the upper bounds in Theorems 1.1 and 1.2 are off from optimal by at most the factor of $\sqrt{\ln \delta}$. They posed an interesting conjecture that, for some constant C,

$$\gamma_s(G) \le \frac{C}{\sqrt{\delta}} n,$$

and proved that the above discrepancy conjecture, if true, would imply this upper bound for δ-regular graphs. A strong result of Matoušek [19] shows that the bound is true, but he pointed out that the constant C is rather large: "I believe the constant shouldn't be astronomical — perhaps in the hundreds" (private communication on 15.01.2009). Notice that if $C > 100$, then the bound $\gamma_s(G) \le \frac{C}{\sqrt{\delta}} n$ is better than the bound of Theorem 1.2 only for huge values of δ. For example, if $\delta = 10^{12}$, then

$$4\sqrt{\ln(\delta+1)} = 21.03 << C,$$

that is, Theorem 1.2 yields a much better bound than the above conjecture. Hence the constant $C > 100$ in Matoušek's proof makes his result of rather theoretical interest.

Thus, when δ is not huge and at least 99, Theorem 1.2 gives the best upper bound, and for $19 < \delta < 99$, no good upper bound in known. Using the probabilistic method [1], we can prove the following result:

Theorem 1.3 *For any graph G with $\delta > 19$,*

$$\gamma_s(G) \le \left(1 - \frac{2\widehat{\delta}}{(1+\widehat{\delta})^{1+1/\widehat{\delta}}\, \widetilde{d}_{0.5}^{\,1/\widehat{\delta}}}\right) n, \qquad (1.1)$$

where $\widehat{\delta} = \lfloor 0.5\delta \rfloor$, $\widetilde{d}_{0.5} = \binom{\delta'+1}{\lceil 0.5\delta' \rceil}$, $\delta' = \begin{cases} \delta & \text{if } \delta \text{ is odd;} \\ \delta+1 & \text{if } \delta \text{ is even.} \end{cases}$

This theorem improves the result of Theorem 1.2 for 'relatively small' values of δ. For example, if $\delta(G) = 99$, then by Theorem 1.2, $\gamma_s(G) \leq 0.869n$, while Theorem 1.3 yields $\gamma_s(G) \leq 0.537n$. In fact, we can improve Theorem 1.2 for all values of δ. For instance, we can prove a result which is 1.63 times better than the bound of Theorem 1.2 for very large values of δ, and for $\delta = 10^6$ the improvement is 1.44 times.

Thus, using our technique, we would like to answer the following natural and important questions: What are good bounds for α-discrepancy of hypergraphs? Also, can we prove the aforementioned conjecture for a small value of C and generalise it for α-discrepancy?

1.3 Strongly Perfect Graphs

A graph G is a *perfect graph* if $\omega(H) = \chi(H)$ for every induced subgraph H of G, where $\omega(H)$ is the clique number of H and $\chi(H)$ is the chromatic number of H. A *hole* in a graph is an induced cycle C_n, $n \geq 5$. A hole is *odd* if it has an odd number of vertices. The complement of a hole is called an *antihole*, and the complement of an odd hole is called an *odd antihole*.

The Strong Perfect Graph Theorem [9] *A graph is perfect if and only if it does not contain any odd holes and odd antiholes as induced subgraphs.*

Let $\beta(G)$ denote the independence number of G and $\Gamma(G)$ denote the upper domination number of G. A graph G is called *upper domination perfect* (Γ-*perfect*) if $\beta(H) = \Gamma(H)$, for every induced subgraph H of G. The *prism* Pr_n ($n \geq 3$) consists of two disjoint cycles

$$C_1 = (u_1, u_2, ..., u_n), \quad C_2 = (v_1, v_2, ..., v_n),$$

and the remaining edges are of the form u_iv_i, $1 \leq i \leq n$. The *prism* Pr_1 is two loops connected by the edge u_1v_1, this is the only case where loops are permitted. The *Möbius ladder* Ml_n is constructed from the cycle

$$C = (u_1, u_2, ..., u_{2n})$$

by adding the edges $u_i u_{n+i}$ $(1 \leq i \leq n)$ joining each pair of opposite vertices of C. The odd prisms Pr_1 and Pr_3 and the even Möbius ladder Ml_4 are shown in Figure 1.1.

Two vertex subsets A, B of a graph G *independently match* each other if $A \cap B = \emptyset$, $|A| = |B|$ and all edges between A and B form a perfect matching in $\langle A \cup B \rangle$. A graph G of order $2k$ is called a *W-graph* if there is a partition $V(G) = A \cup B$ such that A and B independently match each other. Clearly, $|A| = |B| = k$. The sets A and B are called *parts*, and the graph G is denoted by $G(A, B)$. A graph H is called a *semi-induced subgraph* of a graph F if $H = H(A', B')$ is a W-graph with parts A' and B', and H is a subgraph of F such that the sets A' and B' independently match each other in the graph F. We say that a graph G is *2-homeomorphic to H* if G can be obtained from H by replacing edges of H by chains of even order $2k$, $k \geq 1$.

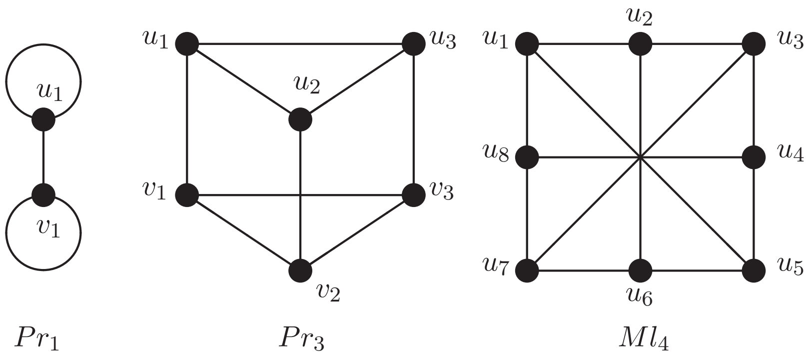

Figure 1.1: Odd prisms Pr_1, Pr_3 and even Möbius ladder Ml_4.

Theorem 1.4 [28] *A graph G is Γ-perfect if and only if G does not contain a semi-induced subgraph 2-homeomorphic to the odd prism Pr_{2n+1} $(n \geq 0)$ or the even Möbius ladder Ml_{2m} $(m \geq 1)$.*

Note that not all graphs described in the above theorem are W-graphs, for example the Möbius ladders are not. By the definition, such graphs cannot be semi-induced subgraphs, so there is no contradiction. However, some of graphs 2-homeomorphic to the Möbius ladders are W-graphs and they are 'really' forbidden. Note also that in such graphs the partition into parts is not fixed. The description of the 'pure' list of forbidden W-graphs can be found in [28].

Theorem 1.5 [14] *Any absorbantly perfect graph is Γ-perfect.*

Theorem 1.5 implies that any strongly perfect graph is Γ-perfect. A graph is called *strongly Γ-perfect* if it is both perfect and Γ-perfect. Since strongly perfect graphs are perfect, we obtain

$$\{\text{Strongly perfect}\} \subset \{\text{Strongly } \Gamma\text{-perfect}\} \subset \{\text{Perfect}\}.$$

The Strong Perfect Graph Theorem and Theorem 1.4 imply the following characterisation:

Theorem 1.6 *A graph G is strongly Γ-perfect if and only if*

1. *G does not contain odd holes and odd antiholes as induced subgraphs and*
2. *G does not contain a semi-induced subgraph 2-homeomorphic to the odd prism Pr_{2n+1} $(n \geq 0)$ or the even Möbius ladder Ml_{2m} $(m \geq 1)$.*

The class of strongly Γ-perfect graphs is 'much more narrow' than the class of perfect graphs because of forbidden semi-induced subgraphs 2-homeomorphic to the odd prism Pr_{2n+1} $(n \geq 0)$ or the even Möbius ladder Ml_{2m} $(m \geq 1)$. Theoretically, these forbidden semi-induced subgraphs can be replaced by an equivalent family $\mathcal{F}$ of forbidden induced subgraphs. This family would have an exponential growth of graphs. Indeed, $\mathcal{F}$ has 1 graph of order 6; 14 graphs of order 8 and 228 graphs of order 10 [14, 28].

Thus, we believe that the class of strongly Γ-perfect graphs is much 'closer' to strongly perfect graphs than to perfect graphs. Therefore, the missing link in the following conjecture, which is based on Theorem 1.6, is 'reasonably small' and can be determined by our approach.

Conjecture on Strongly Perfect Graphs *A graph G is strongly perfect if and only if*

1. *G does not contain odd holes and odd antiholes as induced subgraphs and*
2. *G does not contain a semi-induced subgraph 2-homeomorphic to the odd prism Pr_{2n+1} $(n \geq 0)$ or the even Möbius ladder Ml_{2m} $(m \geq 1)$ and*
3. *'Missing Link'.*

1.4 Computational Aspects

Besides graph-theoretic research, the proposed research project suggests developing and applying software to the hereditary class of strongly perfect graphs in order to obtain a characterisation conjecture for the class. Theoretically, each hereditary class can be characterised in terms of forbidden induced subgraphs. However, if the number of forbidden induced subgraphs is large, then it can be practically impossible to provide an explicit list of such subgraphs and a characterisation in different terms must be looked for. Another problem could be that the number of forbidden induced subgraphs is reasonable, but the method for obtaining the corresponding characterisation is difficult and cannot be implemented without specialised software.

Given a hereditary class $\mathcal{P}$, the first step in tackling the problem is to obtain the list of all minimal forbidden induced subgraphs of order at most 10. Let us denote this list by $\mathcal{F}$. This step is very important and crucial for choosing subsequent steps. In fact, the list $\mathcal{F}$ gives rise to various ideas and conjectures about the class. Moreover, it provides a characterisation of the class $\mathcal{P}$ for graphs of order at most 10. To fulfil the difficult task of finding the list $\mathcal{F}$ for the class $\mathcal{P}$, we will use our database of all non-isomorphic graphs of order at most 10. There are 12,293,434 graphs in the database. It is necessary to develop a program implementing the following procedure:

We put $\mathcal{F} = \emptyset$, where $\mathcal{F}$ is the list of minimal forbidden induced subgraphs for $\mathcal{P}$ of order at most 10.

Step 1 Take the next graph G from the database. If there are no more graphs, then exit.

Step 2 Verify whether G belongs to $\mathcal{P}$. If so, go to Step 1.

Step 3 Verify whether G contains one of the graphs from $\mathcal{F}$ as an induced subgraph. If it does contain any of the forbidden graphs, then go to Step 1.

Step 4 Update the list of minimal forbidden subgraphs $\mathcal{F}$ because a new minimal forbidden graph was found. Go to Step 1.

It is not difficult to see that this procedure correctly determines the list $\mathcal{F}$. Indeed, in Step 3 we only consider graphs not belonging to $\mathcal{P}$. If such a graph G is not minimal, then it must contain a minimal forbidden graph of smaller order from $\mathcal{F}$. Because our database contains all graphs of order up to 10, such a minimal graph of smaller order must be already in $\mathcal{F}$ and it will be found in G by the procedure. Thus, the procedure is correct. This procedure basically requires to develop and implement a recognition algorithm (in Step 2) for the class of strongly perfect graph and also a subgraph isomorphism algorithm (in Step 3). The latter algorithm was developed, implemented and successfully tested before. It is important to emphasise that the above procedure is feasible if we apply it to all graphs of order at most 10. For this database of graphs we ran various computer programs, which contained exponential algorithms, and running time was always very reasonable.

The conjecture will be thoroughly tested using random graphs of order between 11 and 20. Let $\mathcal{F}$ denote all graphs forbidden in the Conjecture on Strongly Perfect Graph (including the missing link). A graph G is called *$\mathcal{F}$-free* if it does not contain any member of $\mathcal{F}$. If the conjecture is not true, then there is an $\mathcal{F}$-free graph G which is not strongly perfect. We will generate a huge number of random $\mathcal{F}$-free graphs and verify whether they are strongly perfect:

Step 1 Take a random graph G of order 10 from the database of all graphs of order 10. If this graph is not $\mathcal{F}$-free, then do this step again.

Step 2 Generate a random number r between 1 and 10, and repeat the next step r times.

Step 3 Add a vertex v to G and randomly generate edges between v and the vertices of G. If the resulting graph is not $\mathcal{F}$-free, then remove the edges incident to v and generate them randomly again — do this until the resulting graph H is $\mathcal{F}$-free.

Step 4 Using the definition, verify whether H is strongly perfect. If so, go to Step 1. Otherwise, H is a counterexample to the conjecture.

Note that any $\mathcal{F}$-free graph of order between 11 and 20 can potentially be reached using the method of the above procedure. There are 3,063,185 strongly perfect graphs of order 10 and 12,005,168 graphs of order 10. Therefore, a strongly perfect graph of order 10 is randomly generated with the probability 0.255. Since the conjecture is true for all graphs of order at most 10, we conclude that an $\mathcal{F}$-free graph of order 10 in Step 1 is randomly generated with the same probability 0.255. It is very easy to see that Step 3 is feasible too. Thus, the above procedure will correctly verify the Conjecture on Strongly Perfect Graphs. If a counterexample H is found, then the conjecture must be rectified and the procedure must be run again.

There are no general methods for proving characterisation results. Therefore, it will be necessary to develop specific methods to prove the conjecture for some interesting cases and then try to prove the entire conjecture by further developing these methods. For example, it seems feasible to prove the conjecture for $K_{1,3}$-free graphs because in this case the families of forbidden graphs described in the second part of the Conjecture on Strongly Perfect Graphs can be replaced by three simple families of forbidden induced graphs.

Another approach would be to use the Strong Perfect Graph Theorem, whose proof provides a description of the structure of perfect graphs. Roughly speaking, a graph is perfect if and only if it either belongs to one of the four simple classes of graphs (bipartite graphs and their complements, line graphs and their complements) or can be obtained from these graphs by applying some operations. Using this structure and the families of forbidden graphs in the conjecture other than holes and antiholes, it might be possible to prove the entire conjecture. We are convinced that an essential progress in proving the conjecture will be achieved.

1.5 Beneficiaries and Dissemination

Füredi and Mubayi [12] indicated that discrepancy theory had "developed into an elaborate field related ... to geometry, probability theory, ergodic theory, computer science, and combinatorics", while Tezuka [26] describes its application to finance. Among other practical applications, image processing and the Monte Carlo methods in high dimensions can be mentioned. Ramirez-Alfonsi and Reed in their book [20] pointed out many important applications of perfect graphs and their links to other areas of mathematics. “These applications include frequency assignment for telecommunication systems, integer programming and optimisation” [10]. Among other applications, complexity and coding theories may be mentioned. For example, Diestel ([11], page 111) underlines that “the class of perfect graphs has duality properties with deep connections to optimisation and complexity theory, which are far from understood”.

The results of this project will be of benefit to the academic and technological organisations interested in graph theory, combinatorial methods and algorithms. The new results may be useful to solve various problems associated with graph and network structures. The algorithms developed will be used in a complex Windows application for graph theory, called GRAPHOGRAPH, which can be used as a research and learning tool for graph theory. This software will be freely available via Internet in future.

In order to reach the community of interested researchers, we will publish our results in an internationally recognised journal such as the *Journal of Graph Theory* or *Discrete Mathematics.* Moreover, we will present our ideas/results at the *International Workshop on Graph-Theoretic Concepts in Computer Science* (Europe) and the *British Combinatorial Conference.*

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1.7 Resources and Work Plan

Table 1.1 provides a summary of resources required for the project in terms of full economic costs.

Directly Allocated Costs

The duration of the project is 24 months, and the PI is anticipating actively researching approximately one day per week because of many additional commitments (e.g. research, teaching, grant applications, scientific software, editorship, supervision of a PhD student). The time frame of 24 months is appropriate for a project of this nature due to the volume of work to be undertaken.

Table 1.1: Summary of costs.

Type of Costs	**Sub-type**	**Full Economic Costs** (£)
Directly Allocated Costs	Investigators	20,694
	Estates Costs	15,473
Directly Incurred Costs	Staff	73,242
	Travel & Subsistence	1,700
	Other Costs (PC)	2,315
Indirect Costs		71,630
	Total	**185,054**

Directly Incurred Costs

Staff
The research proposed here requires considerable effort at a high technical level relying on expertise in graph theory and computer programming. The proposal for a post-doctoral researcher full time for 24 months is, in our view, a cost-effective approach to meeting these requirements. The starting salary of £29,704 (the start of the RF Grade) is necessary to attract a researcher at the desired level of expertise to ensure project success and high-quality research and implementation.

Travel and Subsistence
Travel expenses are needed to exchange ideas with British and foreign specialists in the field and to disseminate the work. The post-doctoral researcher will take part at the *International*

Workshop on Graph-Theoretic Concepts in Computer Science (Europe) and the *British Combinatorial Conference* (unknown location). The results of the research will be published in the internationally recognised *Journal of Graph Theory* or *Discrete Mathematics*.

Other Incurred Costs

A high-performance PC for a post-doctoral researcher will be essentially required to carry out the project. The PC must have specifications sufficient to carry out the project, taking into consideration that there will be many programming and testing activities that are resource intensive.

To carry out this research, the PI has all the resources and infrastructure at the University of the West of England, Bristol, and therefore does not require additional equipment for this project. The work is genuine and will be done using available infrastructure.

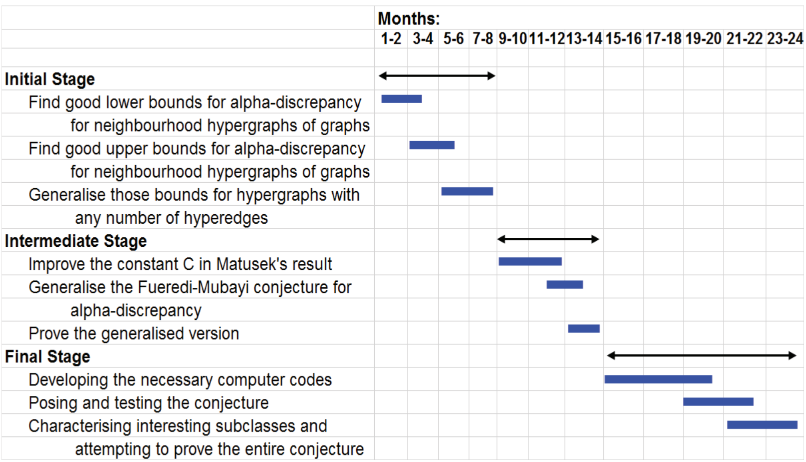

Figure 1.2: Work plan.

Work Plan

The plan of the work is shown in Figure 1.2. The first two stages are devoted to α-discrepancy, whereas the final stage to strongly perfect graphs.

1.8 Reviewers' Reports

The next four sections consist of full citations of four anonymous reviewers from their reports [27]. The reviewers' comments are highlighted in *italics*. Note that the review form of Reviewer 4 is different from the first three forms, probably because the Response Due Date for Reviewer 4 was 47 days later than the similar date for the first three reviewers.

Report 1

Response Due Date: 6 May

Significance and Potential of the Research
The proving of the Strong Perfect Graph Theorem in 2002 was certainly a landmark event, and the project described here is closely related to that work. The extensions of the concepts involved focussed on here are certainly interesting, and formulation and verification of appropriate conjectures would attract some attention.

Assessment: Unsatisfactory, Adequate, Good, Very Good, Outstanding

My confidence level: Low, Medium, High

Degree of Novelty or Risk
The ideas feeding into this project are new and interesting. The risk in this area is not high, since one can always expect partial results of some kind. On the other hand, the aim of deriving a suitable characterization conjecture for strongly perfect graphs, and proving it, is a challenging one.

Assessment: Unsatisfactory, Adequate, Good, Very Good, Outstanding

My confidence level: Low, Medium, High

People and Development

The proposed PI has a good track record, though there is no candidate named as post-doctoral researcher, which does slightly depress the grade.

Assessment: Unsatisfactory, Adequate, Good, Very Good, Outstanding

My confidence level: Low, Medium, High

Collaboration

Fine, as applicable.

Assessment: Unsatisfactory, Adequate, Good, Very Good, Outstanding

My confidence level: Low, Medium, High

Ability to Deliver the Proposed Research

One would anticipate a useful impact on the problems described, although a complete solution is not so likely. The usefulness of the computer-aided component of the research is not so clear to me.

Assessment: Unsatisfactory, Adequate, Good, Very Good, Outstanding

My confidence level: Low, Medium, High

Planning and Management

The planning and general arrangements for the project look very good.

Assessment: Unsatisfactory, Adequate, Good, Very Good, Outstanding

My confidence level: Low, Medium, High

Resources Requested

These are quite modest resources asked for, but certainly appropriate and justified.

Potential Contribution to Knowledge Transfer

The proposer points to some very relevant aspects of the research, citing the important role in recent years of discrepancy theory etc.

Assessment: Unsatisfactory, Adequate, <u>Good</u>, Very Good, Outstanding

My confidence level: Low, <u>Medium</u>, High

Your Conclusions

This is a well put together proposal by an able and inventive researcher. The project is certainly worth supporting, and one can be confident of useful outputs. For me, the absence of a proposed top-rate post doc researcher is a weakness, given that this is a quite competitive area.

The overall quality of the proposal: Unsatisfactory, Adequate, Good, <u>Very Good</u>, Outstanding

Recommendations

I believe this proposal:
Should not proceed as proposed, Could proceed as proposed, <u>Should proceed</u>

My confidence level: Low, <u>Medium</u>, High

Reviewer Expertise

Graph theory, combinatorics, theoretical computer science, computability theory.

Report 2

Response Due Date: 6 May

Significance and Potential of the Research

This proposal is divided into two parts, which are both related to domination in graphs, but which seem to be quite independent of each other. I am not familiar with the idea of alpha-discrepancy, *which the proposer presents as a generalization of signed domination in graphs. This part of the proposal looks interesting, but I am not qualified to judge its significance.*

The other part of the proposal is about strongly perfect graphs. These form a natural and interesting subclass of the class of perfect graphs. A characterization of perfect graphs, known as the Strong Perfect Graph Conjecture, was proposed by Berge in 1961 and proved by Chudnovsky, Robertson, Seymour and Thomas in 2002. In its time, it was regarded as possibly the most important unsolved problem in graph theory. It stimulated a great deal of work and gave rise to the development of many interesting concepts and methods. Now that it has been proved, it is natural to turn to the characterization of the related class of strongly perfect graphs. Such a characterization would be of great interest in its own right, and would be likely to stimulate the development of further important ideas and techniques.

The approach outlined in the proposal involves an interesting mix of computation and theoretical work, and it is entirely appropriate for the task of formulating a conjectured classification and testing it on important subclasses of graphs. This work could potentially have a significant impact in its contribution to the UK's world research standing and on the advancement of research knowledge for the benefit of the research community.

Assessment: Unsatisfactory, Adequate, Good, Very Good, Outstanding

My confidence level: Low, Medium, High

Degree of Novelty or Risk

The part of the proposal concerning alpha-discrepancy seems incremental in nature, but I cannot judge its significance. The part concerning strong perfectness is highly original in its proposed combination of theoretical work and computer testing. There is a good chance that this part will lead to the formulation of an interesting new conjecture, which will stimulate valuable further work in years to come.

Assessment: Unsatisfactory, Adequate, Good, Very Good, Outstanding

My confidence level: Low, Medium, High

People and Development

The project would support an RA for 24 months, doing quite high-level and innovative work.

Assessment: Unsatisfactory, Adequate, Good, Very Good, Outstanding

My confidence level: Low, Medium, High

Collaboration

No partnerships are proposed. This is reasonable.

Assessment: Unsatisfactory, Adequate, Good, Very Good, Outstanding

My confidence level: Low, Medium, High

Ability to Deliver the Proposed Research

The PI is well known and has a good range of publications (28 matches in MathSciNet). His published papers include several on domination theory, which are relevant to both parts of this proposal, and several more on perfect graphs. These include two in particular, both joint with I. E. Zverovich, that are particularly relevant to this proposal, in that the authors characterize classes of perfect graphs using a mixture of theoretical methods and specialized computer software written by the PI: domination perfect graphs in 1995, and basic perfect graphs in 2005. The PI therefore seems eminently qualified to carry out the research described in this proposal.

Assessment: Unsatisfactory, Adequate, Good, Very Good, Outstanding

My confidence level: Low, Medium, High

Planning and Management

The diagrammatic schedule in the proposal is difficult to interpret, since it is badly aligned. But it appears that the work on alpha-discrepancy will take up more than half the time, and that *the work on strong perfectness will not start until the second year. This is a pity, since it is the latter work that seems to me to be potentially more exciting. If it seems reasonable to do so (which may depend on the skills of the RA appointed), I would urge the PI to consider starting the work on strong perfectness earlier, so as to allow the possibility of spending more time on it if this seems appropriate. The plan is otherwise good, and should produce significant results. These will be presented at conferences and in international peer-reviewed journals, which is entirely appropriate.*

Assessment: Unsatisfactory, Adequate, Good, <u>Very Good</u>, Outstanding

My confidence level: Low, <u>Medium</u>, High

Resources Requested

The main direct costs are for an RA for 24 months, the PI's time for one day a week for 24 months, attendance at two conferences, and necessary computing equipment. These costs are all appropriate and justified.

Potential Contribution to Knowledge Transfer

The research described is potentially of interest to graph theorists and to anyone interested in combinatorial methods and algorithms. Discrepancy theory has applications in a wide range of fields including probability theory and computer science, and perfect graphs have applications in fields such as radio frequency assignment and integer programming. The research described is not aimed at such applications directly, but it could contribute to them indirectly. It will also contribute towards the GRAPHOGRAPH Windows application for graph theory.

Assessment: Unsatisfactory, Adequate, Good, <u>Very Good</u>, Outstanding

My confidence level: Low, <u>Medium</u>, High

Your Conclusions

I am not qualified to judge the part of the proposal concerning alpha-discrepancy, but the part concerning strong perfectness is *impressive. It has a high probability of producing significant results and the potential for having a major impact on the field. The PI is experienced in this area and is well placed to deliver these results.*

The overall quality of the proposal: Unsatisfactory, Adequate, Good, Very Good, Outstanding

Recommendations

I believe this proposal:
Should not proceed as proposed, Could proceed as proposed, Should proceed

My confidence level: Low, Medium, High

Reviewer Expertise

Graph theory, but from the mathematical rather than the computer science perspective.

Report 3

Response Due Date: 6 May

Significance and Potential of the Research

The PI has essentially chosen two topics: (1) inequalities on alpha-discrepancy, and (2) a characterization conjecture on strongly perfect graphs. Both topics are important, well-chosen and timely. I think that the second topic is especially very important and challenging. Any serious advances on either of the topics would be of great interest to the international combinatorics and graph theory community and would be a very positive contribution to the UK's world research standing.

Assessment: Unsatisfactory, Adequate, Good, Very Good, Outstanding

My confidence level: Low, Medium, High

Degree of Novelty or Risk

The following two topics are considered in the project: (1) inequalities on alpha-discrepancy, and (2) a characterization conjecture on strongly perfect graphs.

The first topic is very challenging and technically very difficult. It's unclear at the moment which techniques and approaches can help the researchers to advance on the matter. One promising avenue is probabilistic methods, where PI appears to be gaining strength recently. The idea to generalize the Fueredi–Mubayi conjecture to alpha-discrepancy is original. As the topic is certainly very difficult, I find it very adventurous with high potential to produce a high return in knowledge advances.

The second topic seems even more difficult as the PI has only some ideas on how the conjecture would look like and a partially-ready software tool to assist in tackling the problem. The idea to use software together with theoretical methods in this line of research is original. It's well-known that the Strong Perfect Graph Conjecture (a conjecture on perfect graphs) has led to a large body of high-quality research. It seems that, when formulated, a structural conjecture on strongly perfect graphs would lead to research of similar quality and quantity.

Assessment: Unsatisfactory, Adequate, Good, <u>Very Good</u>, Outstanding

My confidence level: Low, Medium, <u>High</u>

People and Development

The project is about solving challenging and technically very difficult combinatorial problems (including problems of finding a generalization of a known conjecture and formulating a new conjecture). Certainly, to work on the project the PI needs a very talented and able RA. Such an RA will get an excellent training and become a highly skilled researcher in the areas of combinatorics and graph theory.

Assessment: Unsatisfactory, Adequate, Good, Very Good, <u>Outstanding</u>

My confidence level: Low, Medium, <u>High</u>

Collaboration
No formal partnership is proposed and research of this nature is often carried out in small groups. Still, I'd expect there to be an informal collaboration with other researchers world-wide, but it is not key to the project.

Assessment: Unsatisfactory, Adequate, <u>Good</u>, Very Good, Outstanding

My confidence level: Low, Medium, <u>High</u>

Ability to Deliver the Proposed Research
The PI is well suited to carry out the proposed research as one can see from his research record on both theoretical studies and software development. Certainly, a strong RA is essential for the project to be a success. I'm quite confident that the PI will be able to find an appropriate RA (even though it is a non-trivial task).

Assessment: Unsatisfactory, Adequate, Good, <u>Very Good</u>, Outstanding

My confidence level: Low, Medium, <u>High</u>

Planning and Management
It's very difficult to plan a highly theoretical research, where one may obtain required results within a short period of time, or longer period of time, or never. Still, I'm sure the PI will deliver at least some significant results (in the very worst case) given his research record and the planning is aimed at achieving at least this outcome. Realistically, though, we can expect significant developments in both directions of the proposal. Dissemination is well thought through even though I'd plan more conferences for the RA to learn more on the state-of-the-art in the fields of study.

Assessment: Unsatisfactory, Adequate, Good, <u>Very Good</u>, Outstanding

My confidence level: Low, Medium, <u>High</u>

Resources Requested
The resources requested are very modest and they are appropriate and certainly justified.

Potential Contribution to Knowledge Transfer

One of the topics of the proposed is discrepancy theory and it has a host of applications in diverse areas and is of great interest in combinatorics. Significant results on the topic would advance our knowledge and, possibly, find new applications and, thus, researchers and practitioners interested in the research results. The software developed during the project will be freely available via Internet with great potential to assist many scholars in various fields of discrete mathematics and its applications.

Assessment: Unsatisfactory, Adequate, Good, Very Good, Outstanding

My confidence level: Low, Medium, High

Your Conclusions

I think it's a solid, technically difficult, risky and challenging proposal. The choice of topics to study is excellent. The PI is well suited to carry out the proposed research especially if assisted by an able RA. More conferences than planned would possibly provide a better training for the RA, but it is not critical.

The overall quality of the proposal: Unsatisfactory, Adequate, Good, Very Good, Outstanding

Recommendations

I believe this proposal:

Should not proceed as proposed, Could proceed as proposed, Should proceed

My confidence level: Low, Medium, High

Reviewer Expertise

Graph theory and algorithms.

Report 4

Response Due Date: 22 June

Quality

The proposed problems are new, and fairly natural to attack following the fairly recent proof of the perfect graph theorem. The proposed methods involve extensive use, and development, of general and special purpose graph software, for exploration on small graphs. The software will be useful for ground-clearing elsewhere in graph theory.

The excellence of this proposal has been demonstrated: Not at all, Adequately, Fully

Impact

The software should be useful quite widely. The mathematical part is less obviously of compelling interest and usefulness.

Potential impact has been demonstrated: Not at all, Adequately, Fully

Applicant

The applicant is well-placed for this project. He has worked in the area for some time, with several publications including some in good journals. Also, he has already put much effort into graph software.

The applicant's track record and ability to deliver this project is: Not appropriate, Adequate, Appropriate

Resources and Management

All seems appropriate for the proposed research.

The level of planning and justification of resources is: Unacceptable, Adequate, Good

Overall Assessment

The further development of good software for working with graphs would be useful, and the PI is well placed for this. The mathematical part of the proposal has some interest but it does not seem that a strong case has been made for this.

In terms of the numbers below, the proposal meets the assessment criteria but it is not clear to what extent the mathematical *part is attacking an important problem at a high level: the appropriate number should be 3 or 4, and on balance I will go for 4.*

My judgement: Option 4

1. This proposal is scientifically or technically flawed
2. This proposal does not meet one or more of the assessment criteria
3. This proposal meets all assessment criteria but with clear weaknesses
4. This is a good proposal that meets all assessment criteria but with minor weaknesses
5. This is a strong proposal that broadly meets all assessment criteria
6. This is a very strong proposal that fully meets all assessment criteria

My confidence level: Low, Medium, High

Reviewer Expertise
Discrete maths, algorithms and probability.

1.9 PI's Response to Reviewers' Comments

I would like to thank the reviewers for their comments, which are essentially very positive. The reviewers have kindly noted that the project "has a high probability of producing significant results and the potential for having a major impact in the field" and "this work could potentially have a significant impact in its contribution to the UK's world research standing", which is particularly important taking into account that this research area is dominated by the American mathematicians. I must concur with Reviewer 3 that the topics are "adventurous with high potential to produce a high return" and also that they are "well chosen and timely".

Concern has been voiced from Reviewer 1 regarding the absence of a named top-rate post-doctoral researcher. However, I have three strong candidates who expressed an interest in this project. Two candidates are from Canada and Europe, and the third is my current PhD student, who will successfully complete her PhD course soon. Under these circumstances, it seems very reasonable to organise an open competition in order to attract the strongest researcher available at the start of the project.

Reviewer 2 raised a minor issue about the schedule, saying: "I would urge the PI to consider starting the work on strong perfectness earlier...". We will try to finish the work devoted to the first part earlier, thus allowing more time for strong perfectness. However, we are also mindful of not rushing through the first phase of the work in order to commence the second and will, of course, devote appropriate time throughout the project plan.

Another issue was voiced from Reviewer 3, who said, "Still, I'd expect there to be an informal collaboration with other researchers world-wide, but it is not key to the project". I am always open for research collaborations, but instead of planning them in advance, I would prefer to contact 'right' mathematicians as soon as there is a need to discuss an issue, which potentially can result in joint research. This can be achieved through the conference attendance scheduled during the project. Indeed, the post-doctoral researcher has been specifically identified to attend those events in order to make necessary connections and begin to advance their own network of potential collaborators. On this note, in reply to the reviewer's comment regarding the relatively small number of conferences for the researcher, I would indicate that the project runs over 24 months and has an intensive research programme requiring a high calibre researcher. A rigorous schedule of conference attendance would severely disrupt the research programme itself, which is why I took the decision to limit this to two.

1.10 Summary of Key Lessons

Table 1.2 provides a brief summary of key lessons learnt from the proposal and the reviewers' reports. The comments given in this section might be viewed by the reader as straightforward, however they include some helpful hints for inexperienced developers of research bids.

Table 1.2: Key Lessons.

Strengths	**Weaknesses**
The topics are well chosen, timely and adventurous.	The case for the mathematical part is not strong enough.
Significant results are highly probable.	The usefulness of the computer-aided component is not well explained.
The second part is impressive and highly original.	The α-discrepancy part seems incremental.
There is potential for a major impact.	Absence of a named post-doctoral researcher.
New and interesting ideas.	Further collaborations are needed.
Solid, technically difficult, risky and challenging proposal.	The proposed schedule could be better developed and is difficult to interpret.
The PI is well suited to carry out the proposed research.	The small number of conferences.

The case for the scientific (e.g. mathematical) part of the project must be as strong as possible to satisfy reviewers, who want to see much more than just formulations of problems to be tackled and basic methodologies. The following points must be addressed: why the problem in question is important and interesting and how it will be tackled scientifically (e.g. mathematically) at a high level. The usefulness of the computer-aided and any other components must be addressed too. In some cases, it is impossible to avoid the 'incremental nature' of the proposal, but if you believe that your proposal is not incremental, explain this in the proposal.

If the project under development includes a research assistant/fellow or a post-doctoral researcher who are unnamed, then you might be criticised by reviewers. Whenever possible, try to indicate a <u>named</u> researcher, who must be a strong candidate. Otherwise, explain in the proposal why the researcher is unnamed — perhaps you have a number of candidates or there are ideas as to how to find them. For example, "There are three strong candidates who expressed an interest in this project. Two candidates are from Canada and Europe, and the third is my current PhD student, who will successfully complete her PhD course soon. Under these circumstances, it seems very reasonable to organise an open competition in order to attract the strongest researcher available at the start of the project."

Typically, funding bodies do not require to have collaborations, and it is up to the PI to decide on any collaborations within a given project. Yet, reviewers often want to see collaboration/partnership and they would welcome even a minor collaboration. However, instead of having many non-academic partners providing small in-kind benefits, it would be much better to have fewer partners with more meaningful contributions. A major collaboration with a Russell Group university would be a serious advantage that might make the difference. Anyway, all collaborations must be well justified.

The proposed schedule must be realistic and include specific milestones, and time frames for all parts of the project should be well justified. In addition, all diagrams must be accurate and unambiguous. For example, some activities in Figure 1.2 overlap on purpose, however the reviewer believes that they are "badly aligned".

There are no rules regarding the reasonable number of conferences to be attended annually. It seems that for a mathematical proposal, the reasonable number is one conference per year, however some reviewers expect more. The important thing it to justify your schedule of conference attendance and explain the choice of conferences. For instance, if relevant, you could explain that the project has an intensive research programme and a rigorous schedule of conference attendance would severely disrupt the research programme.

Chapter 2

Braess' Paradox in Transport Networks

2.1 Background and Aims

A network is one of the fundamental concepts in mathematics and their applications such as transport. The road transport network is a hierarchical network of classified and unclassified roads, broadly following the principle that, as vehicle traffic moves to a higher category of roads, it should be able to travel more quickly and efficiently and be better separated from pedestrians. This network is sub-divided into the Strategic Road Network (motorways and large-scale inter-urban roads, owned by the Secretary of State for Transport), the Primary Route Network (routes between major settlements and important destinations) and other classified roads (designating A roads, B roads, classified unnumbered roads and unclassified roads). This national system is focussed on getting the motorists from one destination to another. However, there is also a system of road categorisation run independently by local authorities, focussing on the needs of the local highway authority in managing the road. The issues of an integrated transport policy or transport planning introduce a change to the system and potentially unpredictable results in the network.

The well-known Braess' paradox [2] describes situations when adding a new link to a transport network might not reduce congestion in the network but instead increase it. This is due to individual entities acting selfishly/separately when making their travel plan choices and hence forcing the system as a whole not to operate optimally. Deeper insight into this paradox from the viewpoint of the structure and characteristics of networks may help transport planners to avoid the occurrence of Braess-like situations in real-life networks. The same paradox also applies to situations when an existing link is removed from a network. One potential example is the closing of 42nd street in New York City in 1990. Contrary to all expectations, the closing reduced the amount of congestion in the area [3], though it is not clear if this was due to Braess' paradox.

Although Braess' paradox has such a fundamental application to real-life networks, it is not a well-studied phenomenon. Pas and Principio [6] investigated the existence of Braess' paradox in his classical network configuration, where travel times of links are specified in such a way that the resulting network is symmetrical. Their important result describes the situations when Braess' paradox occurs in the aforementioned networks. However, this result is only limited to the type of network shown in Figure 2.1 and when the travel times of links are symmetrical. There is typically no symmetry in real-life networks, and we rather have a hierarchy of links (roads of class A, B, etc.). This implies a hierarchy of travel times. It is not known whether Braess' paradox can happen in a hierarchical network. One of the aims of this research project is to generalise Pas and Principio's result and find necessary and sufficient conditions for the existence of Braess' paradox with arbitrary travel times on the links in the network. In particular, we would like to figure out whether Braess' paradox can happen in a hierarchical network such as the road system in Britain.

Steinberg and Zangwill [7] investigated Braess' paradox from the probabilistic point of view for random networks. Their main conclusion was that in a random network, Braess' paradox is about as likely to occur as not occur under some assumptions. Notice that this result applies to random networks rather than real-life networks. Also, addition of any new link is allowed in their result while in practice it is often necessary to analyse adding a single link. Under other assumptions, Valiant and Roughgarden [9] proved that Braess' paradox is likely to occur in a natural random network model. More precisely, they showed that given an appropriate total flow, in almost all networks there is a set of links whose removal improves the travel time at equilibrium. In other words, Braess' paradox is widespread under those conditions. The main limitation of this result is that any set of links may be removed from a network, and it is not clear whether their conclusion is also valid in the situation when a single link is removed.

Nevertheless, the fundamental significance of Steinberg–Zangwill's and Valiant–Roughgarden's results is that the presence of Braess' paradox is not rare and exceptional, but rather widespread under aforementioned assumptions. Therefore, it might be reasonable to conclude that real-life networks must be analysed from the viewpoint of this phenomenon before adding/ constructing a new link/road. Unfortunately, practical efficient algorithms for finding Braess-like situations in real-life networks are not known. The so-called Best Response Dynamics Algorithm is rather of theoretical interest and it might not work for real-life networks as explained in the next section.

It may be mentioned that equilibrium flows naturally arise not only in transport networks, but also in networks where distributed delay-based routing protocols are used [1]. A number of papers (e.g., see [4, 5]) provide 'intuitive' strategies for allocating additional capacity to a network in such a way that Braess' paradox does not occur. However, those heuristics do not answer the question of whether adding a fixed link to a transport network (e.g. building a new road) results in Braess' paradox.

To answer this important question, we plan to devise an efficient algorithm for finding an equilibrium (or a pseudo-equilibrium) in a network and deciding whether Braess' paradox remains an emergent property of the system. Also, we plan to test the algorithm on real-life networks, for example the road network in Bristol. The method can also be used to decide whether any change in a network (e.g. in the classification of roads) results in a better or a worse performance (or congestion) in the network, which is particularly important in connection with the current Road Network Policy.

Another important goal is to investigate a network topology that may exhibit the existence of Braess' paradox, in particular a relationship between Braess' paradox in a given network and the existence of a local Braess' paradox in its subnetworks. We will determine small networks with Braess' paradox, which can be used as an early indication of the existence of Braess' paradox in a given large-scale network. This will provide simple strategies for adding a new link to a network in such a way that Braess' paradox does not occur. Such strategies can be used for a preliminary analysis of a network.

In contrast to road networks, it is not known whether an analogue of Braess' paradox can happen in a rail network. The concept of equilibrium in a rail network is different because rail entities do not act separately, but they basically follow a schedule and do not change routes. However, delays are quite common in rail networks and they play an important role because of their essential influence on the Public Performance Measure (PPM), travel times and other performance measures. As explained below, an analogue of Braess' paradox might exist in rail networks. This issue will be investigated and the corresponding methods will be developed and tested on a real rail network.

The basic aims of the proposed project are as follows:

(i) Generalise Pas-Principio's result and find necessary and sufficient conditions for the existence of Braess' paradox in its classical network configuration with arbitrary travel times on the links in the network. In particular, show that Braess' paradox can happen in a hierarchical network when adding a link of any hierarchy level. This is particularly important in case of typical real-life networks because they usually have a hierarchical structure.

(ii) Investigate network topologies that may experience Braess' paradox, in particular, the relationship between Braess' paradox in a given network and the existence of a local Braess' paradox in its subnetworks. Determine small networks with Braess' paradox, which can be used as an early indication of the existence of Braess' paradox in a given larger network. This will provide simple strategies for adding a new link to a network in such a way that Braess' paradox does not occur.

(iii) Develop an efficient method for finding an equilibrium (or a pseudo-equilibrium) in a network and deciding whether there exists Braess' paradox when adding/deleting a link.

(iv) Practically implement the above method (using AMPL, Gurobi/CPLEX, C/C++) in such a way that it could be adopted in relevant organisations, test the method on real-life road networks (e.g. the road network in Bristol) and carry out simulations to investigate whether Braess' paradox is widespread in real-life networks.

(v) Investigate the concept of equilibrium in rail networks and a direct analogue of Braess' paradox in such networks from the mathematical viewpoint.

(vi) Determine the most realistic formulation of the equilibrium in a rail network from the practical viewpoint with the help of industrial experts.

(vii) Develop and practically implement the method for finding Braess-like situations in rail networks and test it on a real rail network.

2.2 Braess' Paradox and Methodology

Let N be a network with total flow $Q > 0$, one source and one sink. The network N is said to be at *equilibrium* if

(a) The travel time on paths with non-vanishing flow is the same (it is denoted by T_{eq}), and

(b) The travel time on paths with no flow is at least T_{eq}.

If a network is not at equilibrium, then some users of the network (e.g. drivers) can switch routes in order to improve their travel times. However, if a driver decides to switch to a better route, then the travel time for this route increases and, after a certain period of time, it will become impossible to improve drivers' travel times by switching the routes. Thus, the equilibrium describes 'stable state' behaviour in a network, and no driver has any incentive to switch routes at equilibrium because it will not improve their current travel times.

Now let us add a single link to the network N and denote the resulting network by N^+. Assuming that the network N^+ has the same total flow Q, let T^+_{eq} be the travel time at equilibrium in N^+. *Braess' paradox* is said to occur in N/N^+ if

$$T^+_{\text{eq}} > T_{\text{eq}}.$$

Thus, Braess' paradox describes a situation when adding a new link to a network makes a general performance worse.

The classical network configuration introduced by Braess [2] is shown in Figure 2.1. This network is denoted by N^+. The network N is N^+ with link (b, c) removed. The link volume-travel time functions are defined as follows:

$$\beta_1 f_{ab} \text{ for link } (a, b),$$

$$\alpha_1 + \beta_2 f_{bd} \text{ for link } (b, d),$$

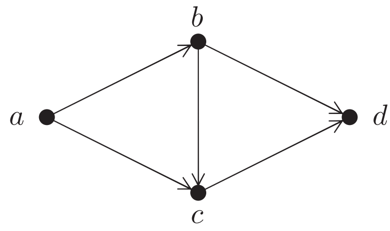

Figure 2.1: The network N^+.

$$\alpha_2 + \beta_2 f_{bc} \text{ for link } (b, c),$$

$$\alpha_1 + \beta_2 f_{ac} \text{ for link } (a, c),$$

$$\beta_1 f_{cd} \text{ for link } (c, d),$$

where $\alpha_i > 0$ is the *free flow travel time* for the corresponding link, $\beta_i > 0$ is the *delay parameter* for the corresponding link and f_{ij} is the flow on link (i, j). As can be seen above, this classical problem is symmetric because the time functions for links (a, b) and (c, d) are similar as well as the time functions for links (b, d) and (a, c). Let Q denote the total flow in N/N^+.

The network configuration of Figure 2.1 is of fundamental significance. Valiant and Roughgarden [9] proved that "the 'global' behaviour of an equilibrium flow in a large random network is similar to that in Braess' original four-node example" (i.e. in the network of Figure 2.1). This is why theoretical results devoted to this classical example are so important.

Assuming that $\alpha_1 > \alpha_2$ and $\beta_1 > \beta_2$, Pas and Principio [6] determined the situations when Braess' paradox occurs in N/N^+ in terms of the five parameters $\alpha_1, \alpha_2, \beta_1, \beta_2$ and Q:

Theorem 2.1 [6] *Braess' paradox occurs in N/N^+ if and only if*

$$\frac{2(\alpha_1 - \alpha_2)}{3\beta_1 + \beta_2} < Q < \frac{2(\alpha_1 - \alpha_2)}{\beta_1 - \beta_2}.$$

The proof of Theorem 2.1 in [6] is rather lengthy and confusing. By using a different approach, the proof of Theorem 2.1 can be considerably simplified. In fact, when dealing with flows, Pas and Principio [6] considered flows for each route along the road network as separate flows. In contrast, we consider a flow for each link separately so that there are five unknown variables associated with flows on five links. Then, applying so-called conservation-of-flow constraints, the number of such unknown variable is reduced to two. This approach allows us to formulate the following lemma, whose proof is relatively straightforward and short:

Lemma 2.1 *In the network N, the travel time at equilibrium is*

$$T_{\text{eq}} = \alpha_1 + Q(\beta_1 + \beta_2)/2.$$

In the network N^+, the travel time at equilibrium is as follows:

$$(a)\;\; T^+_{\text{eq}} = \alpha_2 + \beta_1 Q + \frac{2(\alpha_1 - \alpha_2) + Q(\beta_2 - \beta_1)}{\beta_1 + 3\beta_2}(\beta_1 + \beta_2)$$

$$if \quad \frac{\alpha_1 - \alpha_2}{\beta_1 + \beta_2} < Q < \frac{2(\alpha_1 - \alpha_2)}{\beta_1 - \beta_2};$$

$$(b)\;\; T^+_{\text{eq}} = \alpha_1 + Q(\beta_1 + \beta_2)/2 \quad if \quad Q \geq \frac{2(\alpha_1 - \alpha_2)}{\beta_1 - \beta_2};$$

$$(c)\;\; T^+_{\text{eq}} = \alpha_2 + Q(2\beta_1 + \beta_2) \quad if \quad Q \leq \frac{\alpha_1 - \alpha_2}{\beta_1 + \beta_2}.$$

Now, the result of Theorem 2.1 follows directly from Lemma 2.1 by considering three cases formulated in the lemma and comparing the corresponding travel times at equilibria. The proposed approach not only provides a much shorter proof of Theorem 2.1, but also makes the result and its proof easier to understand. Moreover, it enables us to generalise Theorem 2.1

by allowing arbitrary volume-travel time functions on the links in the above network. Note that the resulting networks with arbitrary time functions are not necessarily symmetrical. This makes such a generalisation particularly important because it might lead to a similar result for hierarchical networks. More precisely, we assume that the volume-travel time functions of links in the network N^+ are as follows:

$$\alpha_1 + \beta_1 f_{ab} \text{ for link } (a, b),$$

$$\alpha_2 + \beta_2 f_{bd} \text{ for link } (b, d),$$

$$\alpha_3 + \beta_3 f_{bc} \text{ for link } (b, c),$$

$$\alpha_4 + \beta_4 f_{ac} \text{ for link } (a, c),$$

$$\alpha_5 + \beta_5 f_{cd} \text{ for link } (c, d),$$

where α_i (free flow travel time) and β_i (delay parameter) are arbitrary positive numbers, $1 \leq i \leq 5$. The problem of occurrence of Braess' paradox in this generalised network becomes much more complex because there are eleven parameters (Q, α_i and β_i). However, we can solve it because our approach uses only two unknown variables associated with the link flows; this was discussed above. The *Braess numbers*, which are functions of the above parameters, will be introduced, and the general solution will be formulated in terms of the Braess numbers.

Thus, the first important goal is to provide necessary and sufficient conditions for the existence of Braess' paradox in the network N with arbitrary travel times on links when the link (b, c) is added. This would not only generalise the aforementioned result of Pas and Principio, but could also lead to a similar result for hierarchical networks, where each link (i, j) belongs to a particular hierarchy h and the corresponding travel time is specified as

$$\alpha k^{h-1} + \beta l^{h-1} f_{ij}.$$

Hierarchical networks are very interesting from the practical point of view because many real-life networks are indeed hierarchical. However, it is not known yet whether Braess' paradox can occur in this kind of networks.

In a similar way, we will determine small networks experiencing Braess' paradox, which can be used as an early indication of the existence of Braess' paradox in a given network. Moreover, we will investigate network topologies that may show evidence of Braess' paradox, in particular the relationship between Braess' paradox in a given network and the existence of a local Braess' paradox in its subnetworks. This should provide simple strategies for adding a new link to a network in such a way that Braess' paradox does not occur.

2.3 Computational Aspects of Braess' Paradox

Another important issue is how to practically determine whether adding a new link to a given network results in Braess' paradox? Because Braess' paradox occurs if $T^{+}_{\mathrm{eq}} > T_{\mathrm{eq}}$, we must be able to efficiently calculate the travel times at equilibria in a given network with and without the new link. Let $t_{ij}(f_{ij})$ denote the *linear travel time* of link (i,j) with flow f_{ij}. The *energy of link* (i,j) is defined as follows:

$$t_{ij}(1) + t_{ij}(2) + ... + t_{ij}(f_{ij}).$$

The *total energy of a network* is the sum of the energies of all the links in the network.

The well-known Best Response Dynamics Algorithm for finding an equilibrium in a network is shown below. An important issue in the context of this algorithm is its running-time complexity. It may happen that the number of routes described in the second 'for each' operator is an exponential function of the number of nodes, and some simple examples of networks with a relatively small number of links show that this is the case. To calculate the total number of steps in the worst case scenario, the exponential number of steps in the second 'for each' operator must be multiplied by the number of drivers in the first 'for each' operator and then multiplied by the number of runs of the 'while' cycle, which is unknown and might be very large as well. Thus, it seems that this algorithm is of exponential-time complexity and hence it will only work for networks with a small numbers of links. The Best Response Dynamics Algorithm can be transformed into a 'simulated annealing' heuristic, which would produce near-optimal solutions. However, near-optimal travel times at equilibria will not help to decide whether there is Braess' paradox because we need to know exactly whether the time at equilibrium increases when adding a new link. Thus, heuristics are not suitable for our purpose.

On the one hand, we need an exact algorithm for calculating travel times at equilibria. On the other hand, the number of paths in the definition of the equilibrium can be an exponential function of the number of nodes, but exponential algorithms do not work for large-size instances. The idea is to sacrifice memory in order to produce a 'near-polynomial-time' algorithm. More precisely, we will investigate the possibility to formulate the problem of finding the travel time at equilibrium as a Linear Programming (LP) problem (perhaps with some binary variables), which can be solved efficiently for instances of reasonable size. The number of constrains, however, can be exponentially large because we have to take into account all the aforementioned paths, that is, it is likely that the memory usage would be exponential as well. If all the paths have non-vanishing flows, then the formulation of the corresponding LP problem is relatively straightforward. However, if there are paths with vanishing flows, then such a problem formulation becomes more challenging, but we believe that it is still feasible. Since the number of constraints is large, we will use the AMPL language to state and formalise the corresponding LP

Algorithm 1: Best Response Dynamics Algorithm

Input: A road network N with a specified flow.
Output: Equilibrium in the network N.

```
begin
    while true do
        Compute the total energy E of N
        foreach vehicle v in N do
            foreach alternative route R for v do
                Calculate the total energy E' of N when
                 v chooses route R
                if E' < E then
                    Update the flow with v taking route R
                    continue while
                end
            end
        end
        halt    /* N is at equilibrium because no
         vehicle has a better route */
    end
end
```

problem. The advantage of this programming language is that an LP problem is specified algebraically, so that the large number of similar constraints can be easily incorporated into the problem formulation.

The next step is to test the resulting LP formulation on small instances to validate it and then on real-life networks. Since the number of constrains can be large, a high-specification PC is needed, and in some cases we would need to use the 'number crunching' server available at UWE. We believe that the LP formulation will be feasible for real-life networks, and a powerful PC could, to some extent, process the exponential input size. However, there is a chance of the worst-case scenario when the LP formulation would not be feasible for real-life networks. In this case we will apply a different approach.

The first idea is to apply a statistical technique to calculate the probability of Braess' paradox occurring in a network. More precisely, instead of dealing with all the aforementioned paths, we would generate large samples of such paths for which the LP formulation is feasible. This would allow us to determine the confidence intervals for the travel times at equilibria in N and N^+. By comparing the confidence intervals, it may be possible to determine the probability of occurrence of Braess' paradox.

The second idea is to introduce a relaxed version of the concept of equilibrium, which should be based on empirical study of flows in networks. The advantage of such a pseudo-equilibrium is that it would reflect the real behaviour of users in a network and its calculation could be done in polynomial time. Therefore, it will be feasible to determine the occurrence of Braess' paradox in real-life networks using the concept of pseudo-equilibrium. In order to implement these ideas, we will use AMPL, Gurobi/CPLEX, C/C++. The corresponding expertise is available because the PI has been frequently employing them in research and teaching. The aforementioned method will be used to carry out simulation to determine whether Braess' paradox is widespread in real-life networks.

2.4 Braess-like Situations in Rail Networks

At present, it is not known whether an analogue of Braess' paradox can happen in a rail network. However, the paradox is known to occur in different networks, for example road networks and electric circuits. The latter are particularly interesting because the equilibrium in such networks is not the classical Nash equilibrium. In fact, electrons in a circuit do not act selfishly/separately, and the equilibrium follows Kirchhoff's laws and Wardrop's principle. This is similar to rail networks, where rail entities do not act separately. Therefore, we believe that an analogue of Braess' paradox can happen in a rail network and we would like to prove this statistically using in-depth simulation techniques.

The first step is to look, from the mathematical viewpoint, at a simplified version of the equilibrium in a rail network and at a direct analogue of Braess' paradox there. More precisely, it will be assumed that there is one input variable, 'delays', and the equilibrium is one performance measure based on average travel times when the network is at a 'stable state'. We will try to find an example of Braess' paradox in a hypothetical rail network, similar to the classical example of Braess, and then the real occurrence of the paradox in the British rail network.

The next step would be to determine most realistic formulation of the equilibrium in a rail network from the practical viewpoint with the help of industrial experts. This concept is not the classical Nash equilibrium because rail entities do not act separately, but they basically follow a schedule and do not change routes. Assuming there are no delays, the equilibrium is stable and it could be any general performance of the network, for example the average travel time taken over all trains in the network. However, delays are quite common in rail networks and they play an important role because of an essential influence on PPM, travel times and other performance measures, and there are other factors playing a similar role.

Therefore, the equilibrium will describe the 'real state' behaviour in the network in the form of basic performance measures. In order to calculate it practically, we will apply the appropriate sampling and randomisation testing technique to analyse those factors statistically and decide what probability distributions describe them. Using Simul8 and C++, we will then carry out an in-depth simulation analysis taking into account how the factors affect the basic performance measures, and probability distributions for those measures will be generated. The basic statistics (e.g. means) of the resulting distributions will give us the equilibrium for the current state of the network.

Thus, we can determine the equilibria in the current network and in the updated network with a link added/removed, or in a set of updated networks. Next, we can use a multi-attribute rating technique (e.g. the Analytic Hierarchy Process) to decide whether the general performance is better or worse, the latter would mean the existence of Braess' paradox. This technique can be used not only when adding/removing a link, but also for any reasonable change in a network, for instance a change in the schedule. The method will be practically implemented and tested on a real rail network.

2.5 References

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2.6 Beneficiaries, Resources and Work Plan

The results of this project will be of benefit to the academic and technological organisations interested in transport networks. In particular, Organisations A, B and C are partners of this project, and Organisation D has expressed an interest in this research. The new methods will be useful for solving various problems associated with network structures, for example the problem of reducing congestion in a network.

Table 2.1 provides a summary of resources required for the project in terms of full economic costs.

Directly Allocated Costs

The duration of the project is 48 months, and both the PI and the co-I are anticipating actively researching approximately two hours per week. This time frame has been carefully thought over while preparing the work plan in order to take into account two things: additional commitments and the need to successfully carry out the project. Using a simple decision-making technique, it has been decided that two hours per week per investigator is the most appropriate schedule to make a meaningful

Table 2.1: Summary of costs.

Type of Costs	**Sub-type**	**Full Economic Costs** (£)
Directly Allocated Costs	Investigators	24,536
	Estates Costs	20,030
Directly Incurred Costs	Staff	145,412
	Travel & Subsistence	4,070
	Other Costs	3,317
Indirect Costs		129,854
	Total	**327,219**

contribution to the project. One hour per week per investigator would be not enough to make such a contribution, while three hours per week per investigator would jeopardise the additional commitments of the investigators. The time frame of 48 months is appropriate for a project of this nature due to the volume of work to be undertaken.

Directly Incurred Costs

Staff

The research proposed here requires considerable effort at a high technical level relying on expertise in networks, graph theory and computer science. The proposal for two RAs full time for 24 months each is, in our view, a cost-effective approach to meeting these requirements. The starting salary of £29,099 (F28 Grade) is necessary to attract a researcher at the desired level of expertise to ensure project success and high-quality research and implementation. The work plan suggests that the first RA is managed by the PI (Years 1 and 2) and the second by the co-I (Years 3 and 4). There is a possibility that the first RA will be recruited as the second RA, so the starting salary for the second RA is £29,972 (F30 Grade) to ensure a continuity of the recruitment.

Travel and Subsistence

Travel expenses are needed to exchange ideas with British and foreign specialists in the field and to disseminate the work, for example the *British Combinatorial Conferences* will be attended and talks will be given. The results of the research will be published in internationally recognised journals (e.g. *Discrete Mathematics* and *Transportation Science*).

In addition to the planned conference presentations, the impact plan envisages that the PI (and/or Co-I) will attend events of Organisation E and make informal visits to UK institutions and companies active in this field. Resources are requested to cover the estimated costs of rail fares, local transfers, subsistence and occasional overnight accommodation for more distant trips. It is difficult to predict exactly where these visits will take place, so a budget of £1,000 was included on the basis of five trips at an average cost of £200. It is expected that seven meetings with representatives of Government organisations will be held at UWE, Bristol. A modest budget of £30 per meeting (£210 in all) is requested to provide refreshments. Possible collaborators are: Organisations A, B, C and E.

Other Incurred Costs

A high-performance PC (a standard workstation) for RAs will be essentially required to carry out the project, including standard software, the AMPL package and an upgrade of the Simul8 license to the Professional version. The PC must have specifications sufficient to carry out the project, taking into consideration that this requires many programming and testing activities that are resource intensive. Recruitment costs, including advertising, are also necessary to find RAs with a desired level of expertise. To carry out this research, the PI and the co-I have all the resources and infrastructure at UWE, Bristol, and therefore do not require additional equipment for this project. The work is genuine and will be done using available infrastructure.

Work Plan

The plan of the work is shown in Figure 2.2.

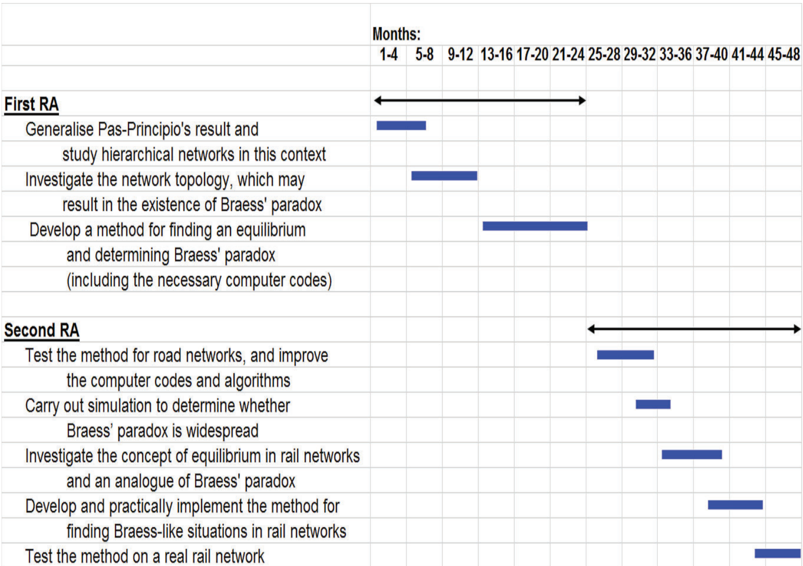

Figure 2.2: Work plan.

2.7 Impact Statement

Economic Impact

The economic impact of the methods developed will be significant assuming that the relevant organisations adopt it (please see the section below devoted to the practical implementation of the methods). If building a new road results in a worse congestion in the network, then perhaps such a road should not be built. One relevant example is a demolition of a motorway section in Seoul in 2003, which improved travel time in the local network and resulted in a restoration of a river under the motorway (see Figure 2.3). The demolition and restoration costs were £201 million, not including very high construction expenses and upkeep of the river. In similar situations, a huge waste of money might be prevented if an analysis of all performance measures in a network is done before the construction of a road.

Figure 2.3: The Cheonggyecheon river: before and after.
(Streetsblog: https://usa.streetsblog.org/2006/12/08/seouls-new-heart/)

Sustainability (Environmental and Social Impact)
"There was a time when we could say that there was either a complete lack of knowledge, or at least room for doubt, about the consequences for our planet of our actions. That time has gone. We now know all too clearly what we are actually doing and that we need to do something about it urgently." (His Royal Highness The Prince of Wales, The Accounting for Sustainability Project, 2010).

The methods developed will improve decision-making and prevent building unnecessary roads, railway lines, etc. They may also result in demolition of existing links in a network if such links are a cause of Braess' paradox. This will improve the environment (or will not make it worse) and simultaneously reduce CO/CO_2 emission and congestion in networks, thus making both environmental and social impact. By reducing emissions, improving congestion and the environment, the project will make a meaningful contribution to sustainable transport systems.

Practical Implementation for Widening the Impact
The methods developed will be practically implemented in such a way that users will not need any preliminary knowledge in order to use the software package. A user-friendly interface will allow easily making any changes in a network and seeing what is happening with general performance/congestion in the network for different values of the total flow. The ease of use of the package will widen the impact of the method and the chance that it will be adopted in relevant organisations for deciding whether a change in a network increases or decreases congestions in the network.

Dissemination
In order to disseminate the work as wide as possible and increase the impact of the proposed research, we will take part in prestigious conferences, give a number of talks and organise seven meetings with our partners (Organisations A, B, C) and

other interested parties. Moreover, the results of the project will be published in internationally recognised journals to reach an even wider audience.

The Prince's Accounting for Sustainability Project is to help, in particular, in developing tools for embedding sustainability into decision-making. Our method is one of such tools and it will be reported to The Prince's Project as a case study for further dissemination.

Putting the Project on the 'Rail Research Map'

The PI is an institutional member of Organisation E, which encourages and provides a forum for collaboration and interaction between the rail industry and academia. This membership is an important route to dissemination, for example through participating in events of Organisation E and making contacts with industrial experts. It will be used to let the rail industry know how they can use the results of the research and what their benefits are. Thus, the research project will be put on the 'rail research map'.

Meetings with Relevant Organisations for Industrial Adoption of the Methods

There will be seven meetings with representatives from various organisations, for example Organisation A (Team X), Organisation B, Organisation C (Team Z) and Organisation D (Team Y), to make sure that the methods developed are applicable in these organisations and will be used to reduce congestion in networks and solve similar problems.

Follow-up Activities

There will be follow-up activities (e.g. further research proposals) to extend the results of the present project to other networks, for example oil pipelines and natural gas pipelines, and to practically implement them in relevant organisations.

2.8 Letters of Support

Letter from Organisation A

I am writing this letter to register my support for your research project on "Braess' Paradox in Transport Networks". Team X in Organisation A would like to participate in this project as a partner because we do appraisal and evaluation of road schemes and highway projects, and the results of the project can be used to reduce congestion in road networks.

We are particularly interested in the results of this project connected with practical testing of the method on road networks, and also in the method itself for deciding whether any change in a network might result in Braess' paradox. These results and their practical implementation may have significant impact on our modelling and appraisal policy and our approach to determining whether any change in the classification of roads results in a better or worse congestion in the entire network.

Our contribution towards the project will comprise:

- Attending bi-annual meetings (including a representative from Team Y of Organisation D). There will be seven meetings over the course of the project, which will have a resource cost to us of £2,100 per attendee from Organisation A or Organisation D.
- Giving recommendations and providing useful insights into the implementation issues on the ground and decisions made on the categorisation of roads.
- Providing datasets (if confidential, then a confidential report is expected).
- Advising which road networks should be tested and what are anticipated changes to those networks.

I look forward to seeing the results of the project and contributing to it.

Letter from Organisation B

Further to your enquiry and our subsequent correspondence, I would like to express my enthusiasm for involvement with your project.

Organisation B is a cross-industry body, with particular interest in issues affecting the whole rail system. We see ourselves as part of the wider transport system and are very aware of the interdependences between rail and other systems. Braess' paradox is an area which we are aware and have considered whether we might think about it in a rail context. The proposal fits well with our concept of examining the use of mathematical modelling to address intractable problems regarding the rail network. This research could provide a major contribution to the understanding of transport networks.

Organisation B will be happy to support this research by attending bi-annual meetings as you describe where we can offer a useful insight into the implementation issues.

Please let me know if we can assist in any other way.

Letter from Organisation C

Organisation C is happy to provide you with assistance, as discussed by e-mail.

Why we want to be a partner in this research project

I understand that the second part of your project is devoted to "Braess' paradox" in the particular case of railway networks. More specifically, Organisation C is interested in whether general network performance (in terms of, say, punctuality and/or reliability of services) becomes better or worse if one additional link is added/removed. This issue, however theoretically specified, is of particular practical relevance to the remit of Organisation C in the realm of transport economics.

What we hope to obtain from the completion of this research project

We assume that the researchers will develop a practical method for deciding whether an incremental addition or removal to a railway network results in better or worse basic performance measures. We assume that the method will not be confined to an ideal (or virtual) "green field" network model, but that it will also be tested in a real railway network. Real-world testing might have interesting implications on policy and regulation.

What we intend to contribute towards this research project

We do not plan to contribute in terms of direct funding, but to provide "in kind" contributions such as attendance at six-monthly meetings (either in City 1 or City 2); the provision of non-confidential datasets if available and relevant; the provision of practical and policy-related recommendations; the provision of insights on applied transport and railway economics, and of feedback as required.

Please let me know if you need any clarification regarding the contents of this letter. I look forward to working with you on your research project.

2.9 Reviewers' Reports

This section includes four reviewers' reports [8], where some text was replaced by comments about the type of removed information in square brackets. The final conclusions in subsections "Overall Assessment" of the reports are based on six predefined statements, which can be found at the end of Section 1.8. The reviewers' comments are in *italics*.

Report 1

Response Due Date: 15 June

Quality

The "Braess' Paradox" in transport networks was first observed relatively recently, and is not well understood. Clearly there is a need to understand it better and so the application is very timely. The possible effects in designing transport networks may be marked. The initial effort has to be more theoretical and mathematical: this is what is to be concentrated upon in the first two years. The second two year period is to be devoted to developing software and algorithms for easy practical application. (I am not experienced enough to judge this part, although it does seem perfectly good).

Impact

The very existence of "Braess' Paradox" in "real life" wrong-headed expensive road projects shows that understanding the paradox should lead to substantial savings in the construction of transport projects. Thus the potential impact of the project is very considerable in money terms; as to its mathematical impact, this is hard to say, but it certainly won't be negligible, and it could be considerable. The dissemination and knowledge exchange has been well thought through, with regular meetings with several interested parties already planned and via publication in professional journals.

Potential impact has been demonstrated very well.

Applicant

The track records of the two applicants are very appropriate. [The PI] *has solved a number of well-known mathematical problems, using considerable ingenuity and* [the co-I] *has appropriate background in transport network theory. Their backgrounds blend together ideally as directors of this proposed project.*

Resources and Management

The scheme seems to be very well worked out and the plans well explained. Of course, discoveries may disarrange the plans somewhat, but one would not expect any such disarrangement would be catastrophic. The requested resources are well justified.

Overall Assessment
The "Braess' Paradox" has recently been noticed and is not well understood. Further investigation is needed for both practical as well as theoretical reasons. The two proposers are both well able to direct the research, [the PI] *more from the mathematical point of view and* [the co-I] *more from the practical.*

My judgement:
This is a very strong proposal that fully meets all assessment criteria.

My confidence level: Low, Medium, High

Reviewer Expertise
Graph Theory and Combinatorics.

Report 2

Response Due Date: 15 June

Quality
The Braess paradox is a classical result in the theory of flows on networks — e.g. road networks, or indeed any commodity flow where the packets operate selfishly and where there is a congestion effect so that the price of travel increases with flow. Networks with the Braess paradox have the property that it is possible to add a link in a particular way, thus at face value "improving" the network — yet the consequent redistribution of flows, due to the equilibration of selfish individuals, results in an increase in all users' costs/delays. Equivalently, one may have a network where removing a link causes costs to go down. There have allegedly been several real world examples of the Braess paradox (e.g. the effect of the 1990 closure of 42nd street in New York). However – in my view, it is not clear whether these examples are genuinely "Braess" or whether they are due to the independent effect of (negative) trip generation — where the change in the level of network provision changes the demand to use it. The balanced conclusion is that it is not really known

whether the Braess paradox occurs in real-world road networks — and it is not something of which transport planners have any kind of general understanding.

*The Braess paradox is usually illustrated in text books via a classic "diamond-shaped" network — with a single origin-destination flow — and, at least for the case of affine cost-flow functions, this example is well understood. However, even in terms of their mathematical idealisation (e.g. ignoring messy stuff like trip generation) not very much is known about whether large-scale networks exhibit Braess — the handful of studies so far have analysed families of random graphs with computational techniques. The promise of (the theoretical component of) this proposal is to develop a better *understanding* of the occurrence of the Braess property on large networks. In full generality, this seems an impossibly hard problem — except the investigators propose to simplify it by analysing small-scale network "motifs" in some depth, then consider whether hierarchical embedding of these structures inside larger networks preserves the Braess property. This is a fresh idea and seems pretty exciting to me — it's risky in the best sense — in that I am not sure whether it can be done, but even in the case of failure I think we will discover a lot of stimulating ideas along the way.*

The second component of the (theoretical) research is concerned with efficient numerical methods for detecting Braess — I have to say that I am not an expert on numerical methods for solving complementarity problems — it's a highly technical area — and I can't assess the validity of the proposed approach. However the goal is certainly the right one — namely that we shouldn't for large-scale networks be looking to iterate UE problems through to convergence but instead should be seeking some concept of "nearby-ness" to the final solution — achieved at much lower computational cost.

Finally, the proposal is to demonstrate all this in action on (i) road networks (entirely credible, using some of the famous examples in the literature, or developing new networks e.g. from *open street map) and (ii) on rail networks. The application of Braess to rail seems far-fetched, and as the authors admit, it is not a UE problem. Instead, it is really "system optimal" (SO) — if indeed optimisation does take place in the UK (!) — and as such, SO solutions can never get worse as links are added because the case of zero flow on the new link is feasible. This is a fundamental point — the proposal promises vaguely to "determine most realistic formulation of the equilibrium in a rail network... with the help of industrial experts", but I don't see much prospect for this. Unfortunately, this means the last 18 months of the project must be considered very risky (in a negative way).*

Impact

There are 3 non-academic partners providing small in-kind benefits in terms of attendance at meetings, advice etc. They are [three organisations]. *It's nice to see this kind of small-scale engagement in what is essentially a mathematics proposal — although it falls far short in my view of the sort of collaboration which would be required to see some of this theory through in to practice.*

I find it a bit curious that the rail industry is signing up to this — and there's not sufficient detail in their letters to justify why, or indeed to show that they themselves understand why. As I've said elsewhere, I think the proposed application of this theory to rail is at best highly speculative — and at worst (at least it seems to me) tenuous and likely to fail.

These things said, the Impact Plan — which is mostly about exploiting these three collaborators for dissemination through to industry — is good for a Mathematics proposal — and the appropriate small-scale resources have been put in place to support this.

Applicant

It's a two man team — [the PI] *is the mathematician and the* [the co-I] *is a transport modeller, with a focus in travel be-*

haviour. In rough terms, this is a good balance for this proposal — with a natural tension between the theory and the application — although it should be pointed out that this seems to be a new collaboration, so is not proven to work. In any case, [the co-I] *is very mathematically oriented for a transport modeller, so I don't foresee any fundamental communication problems.*

In terms of their individual attributes — I know the work of [the co-I] *very well and in my view he is a top-class international researcher. However, I don't know* [the PI] *and I can't vouch for him. My feeling from his publication record is that his previous focus is rather "purer" than this project — so I have some doubts whether he can deliver what is proposed here. However, in fairness, I should say that although I am a user of graph theory, I don't do fundamental graph theory research — and so I cannot accurately judge the quality of* [the PI's] *publications.*

Finally — based on an inspection of [database] *— it seems* [the PI] *has no previous* [funder] *track record. The balanced conclusion is that his management track record is unproven.*

I have dealt with the appropriateness of industrial partners in the Impact section.

Resources and Management

There's no largesse here in resources. In my view, the investigators are asking for the bare minimum (4 years RA) to give any realistic chance of seeing this programme through. The small items have all been precisely and carefully costed and justified. Indeed, my main criticism is that the investigators are not asking for enough of their own time (only 2 hrs per week each) to support this project and develop the RAs. I didn't spot any mention of named candidates for RAs, but I think a lot of this would be very tough for a new RA hire to deliver.

An unusual feature of this proposal is the way it is split in to 2 x 2 year RAs that follow each other sequentially. Actually, I thought this was well justified with [the PI] *directing the work in the 1st half and* [the co-I] *in the 2nd half — and as the proposal says, they can always use the same person all the way through.*

In terms of management process — it has only been described with the broadest brush — for me, there is a risk here given the inexperience of the PI.

Aside from my concerns about the rail work (which anyway comes at the end of a 4 year project and therefore could be seen as part of the adventure) — the workplan generally seems credible. But it is also entirely sequential and there is nothing in the proposal to demonstrate risk management, i.e. what will be done in the project if some of the earlier packages tasks cannot be delivered.

Overall Assessment

I think this is basically a very good proposal — some of the basic theoretical ideas are highly innovative — and even failure to deliver the major goals is still likely to stimulate many interesting knock-ons — in the interface of (pure-ish) mathematics and the theoretical end of transport modelling. The pull-through to industry is more speculative, but the right mechanisms have been put in place to give it a chance.

For me there are really two main negatives: (i) I don't see how the proposed application to rail can work out. The proposal is really rather vague about this. (ii) I am uneasy about [the PI] *leading this — he doesn't have a track record in managing grants and for him, this seems to be much more "applied" work than in his track record.*

But for these points I would be straying towards the "6" box.

My judgement:
<u>This is a strong proposal that broadly meets all assessment criteria.</u>

My confidence level: Low, Medium, <u>High</u>

Reviewer Expertise

Mathematical Modelling; Transport Studies.

Report 3

Response Due Date: 28 June

Quality

Understanding Braess' paradox is a central topic in network theory, and there is a significant literature attempting to understand it. Identifying cases where the paradox appears in real-life networks is clearly very important. The main idea, to formulate it as a Linear Programming problem, does not strike me as theoretically innovative. The design of software that can be used by network planners is absolutely vital (and timely in these resource-constrained times) but I'm too far from the applications to know what the current state-of-the-art is, or to judge whether this project will contribute significantly to this. The study of which small sub-networks tend to give rise to Braess' paradox does seem to be a sensible first step in the design of software that performs well in real world situations.

The proposal also explores the possibility that Braess' paradox could appear in rail networks. This is against my intuition: trains do not run independently and selfishly, which seems to be a precondition for the paradox to occur. Of course, relying on intuition is extremely dangerous in this area! But I'd have liked to have seen a (maybe theoretical) example of when it might occur in this context.

Impact

Links with transport planners are excellent. If network planning software is enhanced as a result of this project, the impact will be large.

I'm surprised in the choice of the British Combinatorial Conference as the venue for dissemination of the theoretical part of this work. I would have thought that a theoretical computer science conference might be a more appropriate venue.

Applicant
The PI has produced high quality research in combinatorics, and I am sure that more such research will emerge from the funding of this project.

I am unable to assess the [co-I's] *track record, as I'm too far from his field.*

The combination of the discrete mathematics skills of the PI with the more applied focus of the [co-I] *makes a nice team, with significant potential for useful cross-fertilization of ideas.*

Resources and Management
The resources look appropriate (though see my comments above on the choice of conferences).

Overall Assessment
An interesting proposal. I don't see much theoretical appeal here, but the excitement of the proposal comes from the applications.

I'm not quite convinced about the applicability of Braess' paradox to rail networks. The most interesting aspect is the intention to develop good software for network planners, but unfortunately I'm too far from the area to assess this well.

My judgement:
This proposal meets all assessment criteria but with clear weaknesses.

My confidence level: Low, Medium, High

Reviewer Expertise
Discrete Mathematics.

Report 4

Response Due Date: 28 July

Quality
Braess' Paradox is an phenomenon in transportation science modelling, whereby the addition of a new route between two points, A and B, in a transport network results in an increase in

travel time for all those travelling between A and B. The history of the problem is as follows. Braess' paper appeared in 1968, in which he provided a very simple example that this paradox occurs under the standard model of transport networks. This resulted in a flury of papers giving other example networks in which the paradox can occur.

No one at this point asked the reasonable question: Is it possible that occurrence of this paradox in the mathematical model is due to the fact that the mathematical model does not realistically capture all the important features of an actual transport network? Anecdotal evidence was cited to give support to the belief that this paradox occurs in practice, e.g., in Stuttgart, in New York, etc. However, no one considered the possibility that the opening of a new route resulted in increased travel time having nothing to do with Braess paradox, but rather to entirely different, conflating factors. For example, it is well-known to transport engineers and city planners that opening up a new route in a transport network can increase demand, simply due to the existence of the new route. If that is the cause of the increased travel time, there is no paradox occurring.

The first-listed objective of the proposed research is to generalise the result of Pas and Principo (1997). The paper of Pas and Principo is almost 15 years old, and it has been cited over 50 times. A credible proposal would summarise in a concise fashioni all the literature that has built on Pas and Principo (1997), and then explain how and why this proposal is doing something new and substantial. I do not see such a literature review in the proposal before me.

The literature review that has been provided does not instill confidence. For example, we read [in Section 2.1] *'Background and Aims', paragraph 4: 'Steinberg and Zangwill [7] investigated Braess' paradox from the probabilistic point of view for random networks.' I was surprised to read this, since from what I recalled about that paper, the Steinberg and Zangwill model was deterministic, not probabilistic. In preparing this review,* *I returned to the paper and saw that, indeed, the applicants were wrong: Steinberg and Zangwill investigated Braess' Paradox from a deterministic point of view, just as Braess had done. Their main contribution was to give necessary and sufficient conditions for Braess' Paradox to occur in general networks.*

Later in the same paragraph, the applicants discuss the paper of Valient and Roughgarden [9]. We read this critique: 'The main limitation of this result is that any set of links may be removed from a network, and it is not clear whether their conclusion is also valid in the situation when a single link is removed.' This sentence shows that, like the Steinberg and Zangwill paper, the applicants have missed the main point of the paper. Space does not allow a detailed discussion of why the applicants' statement is misleading, but I refer them to Section 1.2 of the Valient and Roughgraden paper to see the main contribution of that paper.

Further, I was disappointed to see that some important papers on Braess' paradox were not discussed at all, including [two papers are cited: one is devoted to queuing networks and another to loss networks].

Impact

The proposal is accompanied by letters of support from [two organisations]. *However, both organisations are interested in the practical side of this issue. I have respect for the fact that these organisations are interested and supportive. However, they have made it clear that they are looking for *practical results*. Such results might be better provided by a proposal from engineers who are aware of the Braess Paradox, rather than mathematicians.*

Applicant

With respect, I do have a concern with the Principal Investigator's ability to deliver the proposed project. His background is primarily in graph theory, rather than transport networks. Knowledge of perfect graphs is unlikely to be very helpful to someone attempting to model rail networks. But even his work *in graph theory does not appear to be to the highest standard. He has published primarily in the 'Journal of Graph Theory' and 'Discrete Mathematics' which, while good, are not the best journals in graph theory. I would have wanted to see at least one paper by him in 'Combinatorica' or the 'Journal of Combinatorial Theory (B)', for example.*

The co-investigator has a mixed record of publication. If this project results in publications in 'Transportation Science' or in 'Transportation', as was the case in [year], *then that would be a good result. However, if this project results in publications along the lines of what he has published since, e.g., 'Transportation Research Record' or 'Transportmetrica' then, like the Principal Investigator, the co-investigator is not achieving research at the highest, world-class, standard.*

Resources and Management

I do not believe this to be relevant, in light of my other responses.

Overall Assessment

Before the [funder] *considers spending* [the funder's contribution] *supporting a project on Braess' paradox, the* [funder] *should ask what they expect to land up with. One possible outcome would be new and significant research results on Braess' paradox. This seems optimistic, given that (i) there is by now an enormous literature on Braess' paradox, with well over a thousand publications on this specific topic in the literature, (ii) the Principal Investigator does not yet have a strong track record — or any record, for that matter — in modeling transport networks, and (iii) the proposal does not indicate a good understanding of the extant literature.*

Rather than new research results, an alternative possible outcome would be a practical application of Braess' paradox to road and rail networks. The proposal provides evidence that the [two organisations] *are both interested. My suggestion is that they apply for funding directly to these organisations, and see whether they find that they can make a contribution to either* *or both of them. I do not think that new academic research is required for this. However, the experience the applicants gain in working with these organisations —* [organisation] *especially — might very well lead them to new and relevant research ideas on Braess' paradox. At that point, the applicants should consider re-applying to the* [funder] *for funding.*

My judgement:
This proposal does not meet one or more of the assessment criteria.

My confidence level: Low, Medium, High

Reviewer Expertise
My main research interests are in networks and optimization, and I am especially interested in applications to the design and control of networks.

2.10 PI's Response to Reviewers' Comments

We would like to thank the first three reviewers for their comments, which are essentially very positive. Concern has been voiced with low level of confidence from Reviewer 3 regarding some weaknesses: "The main idea, to formulate it as a Linear Programming problem, does not strike me as theoretically innovative... I don't see much theoretical appeal here, but the excitement of the proposal comes from the applications." The Linear Programming formulation is not the main idea, just a technical step to achieve the basic aims, and it is theoretically challenging as described in the proposal. Also, we believe that there is a considerable theoretical appeal in investigating large-scale and complex networks topologies. This might be one of the reasons why the investigation of Brass' paradox has been so far limited to simple small-scale networks. The PI's experience of working with substructure patterns in graphs is essential in analysing subnetwork patterns in the context of Braess' paradox.

Reviewer 3 also said, "I am surprised in the choice of the British Combinatorial Conference..." A large part of this project deals with flows and graph-theoretic properties of combinatorial objects (networks). We therefore consider this conference to be relevant. Also, we plan to attend specialist conferences devoted to transport, APMOD conference and events of Organisation E.

"The proposal also explores the possibility that Braess' paradox could appear in rail networks. This is against my intuition: trains do not run independently and selfishly. . . ", said Reviewer 3. Also, Reviewer 2 commented that "System optimal solutions can never get worse as links are added because the case of zero flow on the new link is feasible... I don't see how the proposed application to rail can work out." We generally agree with both reviewers that an analogue of Braess' paradox in rail networks is contra-intuitive. Rail services are scheduled and coordinated, and the optimisation of rail networks does take place, but it is rather global, while the focus of Braess-like situations is on a local subnetwork. A certain level of heuristics and sub-optimal solutions, mainly at the local level of the rail network, might be involved in decisions about the topology and operations of such a network (for example in situations of extreme supply and demand levels). Such decisions may have an essential influence on the flow and performance measures in local networks. It would be therefore relevant and valuable to explore the possible emergence of Braess-like situations in subnetworks of what might be seen as a perfectly optimised global rail network. In discussions that we had with rail stakeholders (Organisations B and C), they have expressed their strong interests in the results of this research. For example, Organisation B said, "The proposal fits well with our concept of examining the use of mathematical modelling to address intractable problems regarding the rail network. This research could provide a major contribution to the understanding of transport networks".

Reviewer 2 has indicated the need to demonstrate risk management in the proposal. The proposal consists of two main 'work packages', one is focusing on the theoretical investigation of Braess' paradox and the other is devoted to its applications in both road and rail networks. Although ideally performed sequentially (as illustrated in the work plan), the work packages and some of their elements are largely independent. Not all deliverables must be produced in a specific order or time, reducing the risk of not meeting the project's objectives even if some of the early tasks are delayed. Preliminary steps for each work package are moderately challenging and they will be definitely delivered at a certain level. The risk management of the most challenging step (developing the algorithm) is described in the final part of the proposal, where we discuss different approaches in tackling the problem.

Concern has been voiced from Reviewer 2 regarding the inexperience of the PI in applied research and his management skills. The PI has successfully completed a research project of his two-year post-doctoral fellowship from the Alexander von Humboldt Foundation and a challenging project for providing a complete characterisation of domination perfect graphs with the help of specially developed software tools. His recent experience deals with more applied research, for example practical efficient randomised algorithms in networks. The co-I has experience in managing grants. He will be involved in all parts of the project, provide advice and support to the PI and lead "more applied work".

Response to the Fourth (Late) Reviewer's Report 4

In our view, this report contains clearly twisted and misleading arguments trying to diminish the value of the project and to misrepresent its description and goals. There are no serious arguments in support of the final conclusion that the proposal does not meet the assessment criteria. In light of the above mentioned, we would be grateful if you could kindly consider disregarding this report. The detailed discussion of the reviewer's remarks is given below.

Section "Quality"
In the first two paragraphs, the report discusses Braess' paradox in general, paying particular attention to examples. Even though it seems that the occurrence of Braess' paradox in some examples has not been proved yet mathematically, the reviewer's conclusion that there is no paradox in those examples is wrong. We agree that "opening up a new route in a transport network can increase demand", that is, the total flow can increase after adding a new link. However, when deciding whether Braess' paradox occurs in a network, the total flows in the two networks must be the same. More precisely, it is necessary to compare times at equilibria in the original network and the network with a new link when the total flows in these networks are equal. Thus, one does not 'compare' behaviours of the networks with observed total flows, but rather applies modelling to decide if Braess' paradox occurs. Also, contrary to the reviewer's assertion, the proposal never cited Stuttgart as an example.

Further, the report criticises the lack of a literature review. However, because of space limitations, we did not attempt to provide such a review describing "50" papers. We have briefly discussed the most relevant papers and our statements are not "misleading". For example, the report says, "the Steinberg and Zangwill model was deterministic, not probabilistic", but we did not discuss the model. We said, "Their main conclusion was that in a random network, Braess' paradox is about as likely to occur as not occur". In other words, the probability of Braess' paradox to occur in a random network is about 0.5 under some assumptions. Therefore, it is correct to say that they "investigated Braess' paradox from the probabilistic point of view". In the next paragraph, the reviewer said, "the applicants have missed the main point of the paper" ([9]). We have actually formulated the main conclusion of this paper and said that it does not imply a similar result when a single link is removed, which is relevant to our proposal. Also, the papers mentioned in the final paragraph are not cited because they deal with queuing/loss networks, which are irrelevant in the context of the proposal.

Section "Impact"

In this section, the main and only point of the reviewer is that practical results "might be better provided by a proposal from engineers". However, practical results are only one component of the project. Another, equally important component is to develop purely mathematical results and algorithms. As explained in the proposal, the existing algorithm might not work for large networks. Thus, without mathematicians, this project will not be successful and so the reviewer's suggestion is controversial.

Unfortunately, the report discusses neither "the relevance and appropriateness of any beneficiaries" nor "appropriate routes for dissemination", which makes this part of the report very poor.

Section "Applicant"

We believe that the reviewer exaggerates things here, since a transport network is a directed graph (i.e. a particular case of graphs). "Knowledge of perfect graphs" is given as an example of unhelpful knowledge, but the review fails to notice that the PI has good experience in dealing with subgraph structures, which is necessary for analysing subnetwork structures in networks. Also, the PI has developed various algorithms, for example randomised algorithms in graphs and networks (see, e.g. "Randomised algorithms and upper bounds for multiple domination in graphs and networks", *Discrete Applied Mathematics,* **161** (2013), 604–611). Further, the report discusses who has published where, not the depth of our research results, which is very unusual. In our view, the *Journal of Graph Theory* is one of the best in its area, and the PI's choice of journals has been purely motivated by the relevance of his research topics to those journals.

Section "Resources and Management"
The reviewer provides no comments, thus raising a question about the quality of this report.

Section "Overall Assessment"
The reviewer contrasts research results and a practical application of the paradox. This is highly controversial because these two components are inextricably linked — the practical implementation is based on research results and algorithms, which must be developed before the implementation. Also, the reviewer exaggerates things in items (i)–(iii):

(i) There are not "thousand publications" devoted to Braess' paradox. Searches in ScienceDirect and SCOPUS reveal roughly 100 papers on this topic.

(ii) The PI's track record is very relevant for this project because the project deals with an analysis of networks (i.e. directed graphs).

(iii) There was no objective to provide a literature review as part of the proposal, and both the PI and co-I have good understanding of the subject area.

2.11 Summary of Key Lessons

In this section, we provide a brief summary (Table 2.2) of key lessons learnt from the proposal.

Any example in a proposal is supposed to illustrate some property under discussion, and this property should either be proved by you or be well known from other sources. If the property is controversial or unproven, do not use such an example. If you are going to tackle a problem and you feel that it is rather 'far-fetched', then the reviewers will spot it too. Do not pursue such a problem unless you have a very good example or any other explanation to convince reviewers that this problem is well defined and worth investigating.

Table 2.2: Key Lessons.

Strengths	**Weaknesses**
Potential impact was demonstrated very well.	Unreliable/controversial examples were used.
The two proposers are both well able to direct the research.	The application of Braess' paradox to rail seems far-fetched; the proposal is vague about this.
Some of the basic theoretical ideas are highly innovative.	Absence of a named research assistant.
Links with transport planners are excellent.	The partners' contribution is rather small.
The most interesting aspect is the intention to develop good software.	PI's previous research is rather "purer"; his management track record is unproven.
The dissemination and knowledge exchange has been well thought through.	Research time requested is not enough.
The potential impact of the project is very considerable in money terms.	The management process is not described well; no risk management.
The application is very timely.	Wrong choice of a conference.
The track records of the two applicants are very appropriate.	The list of references could be better thought through.

You have to explain the quality and relevance of your publications and why you can deliver the proposed project. Also, explain your management track record — perhaps you have had projects, not necessarily funded, where your management skills were developed. The planned research time per investigator must be realistic and sufficient. The requested two hours per week per investigator are indeed not enough, this really should be one day per week. The management process should be described in detail, and there must be a short section describing risk management.

It is a bad idea to criticise anything (e.g. a paper) in the proposal. The reviewer might be the author of that paper or just irritated because of the criticism. Try to be neutral in your writing, for example "The main limitation of this result..." can be replaced in this proposal by "The assumption of this result...".

In conclusion, it is not completely clear why Reviewer 4 was so biased and unfair in their report, perhaps the key could be found in the comment, "I was disappointed to see that some important papers on Braess' paradox were not discussed at all". In general, reviewers can be irritated because their important papers were not cited. A crucial lesson is to be more careful when preparing a list of references.

Acknowledgement

The author is grateful to Prof E. Avineri for his contribution to the development of this project.

Chapter 3

Hazard Assessment of Infrastructures for Smart Evacuation Based on Unified Networks

3.1 Background

When human-induced or natural disasters take place in complex infrastructures, a short delay in response to an incident or a major event may mean a significant change in the disaster environment from which trapped people have to escape. This often results in a loss of human lives because the prescribed evacuation procedures do not take into account unpredicted hazardous conditions. It is well known that the frequency of extreme events is increasing and their impact on the world both in terms of human lives and economic costs is becoming more dramatic. This conclusion is confirmed by the datasets from the Emergency Events Database (EM-DAT) created by the Centre for Research on the Epidemiology of Disasters [2].

We will investigate the potential for developing an intelligent decision-support system for smart evacuation and will develop a number of important elements of this system, which will be able to quickly and autonomously analyse and respond to possible hazards within infrastructures. The above development will allow collection and processing of information in real time from various sensors and data sources. This will help to locate epicentres of hazard, classify the types of hazard and estimate the hazard intensity in different epicentres, as well as to determine the distribution of people in the infrastructure. Using advanced decision-making and mathematical techniques, the system will automatically propagate hazard in the infrastructure and calculate the optimal evacuation flows, thus saving as many people as possible. Rescue teams will not be required to make challenging decisions in real time regarding optimal evacuation flows of people and their role will be reduced to acceptance/rejection of a proposed evacuation solution and helping in its practical implementation. Thus, this system will benefit the social environment and public safety because it will reduce the severity of impact of terrorist attacks and other disasters upon citizens and national infrastructure.

Furthermore, the developed technology can be used for safety assessment of new construction projects at the design stage, resulting in higher resilience to disasters and more secure infrastructures such as transport hubs, airports, stadiums and buildings. Such technology can be applied by organisations dealing with safety issues in public places and in the construction industry, especially where hazardous conditions may result in catastrophic consequences. The modelling methods and tools developed in this project will improve the safe design of public places and reduce the cost of future infrastructure redevelopment aimed at increasing public safety. Moreover, better understanding of hazard influence on a whole infrastructure may have further social impact because it may lead to improved safety policies in public places.

This project will enable Company A and Company B to develop state-of-the-art decision-support systems for smart evacuation in infrastructures under hazardous conditions and make them the leading providers of such systems and services.

Related Past and Current Work in the UK and Abroad

The typical research in emergency response is devoted to evacuation and rescue, with a focus on indoor navigation and route finding. For example, Kisko and Francis [6] proposed the EVACNET+ software application for evacuation scenarios planning. Other alternative solutions are available as well: Sun and Wu [13] recently developed an advanced configurable crowd model for different behaviours and scenarios and implemented it for the simulation of evacuation in a building. Some important surveys in the area of evacuation are worth mentioning. A review of 16 evacuation models was given by Gwynne et al. [5], and Kuligowski [7] reviewed 28 egress models. Further reviews of fire and evacuation models can be found in [4, 9, 16]. (We only mention review papers here.)

In general, applications for emergency situations in infrastructures have been restricted to simulations and network approaches. These types of applications often failed to account for the complexity of hazard dynamics and trade-offs needed to select optimal egress routes during an emergency situation under hazardous conditions. To overcome these issues, an intelligent decision-support system for smart evacuation must be created.

The nearest state-of-the-art system for emergency response, CUBER, has been recently proposed by the CUBER team. They have developed a new automated method for deriving a navigable network in a 3D indoor environment, including a full 3D topological model, which may be used not only for standard navigation but also for finding alternative egress routes and simulating phenomena associated with disasters [1]. However, the method only works for buildings and it should be further developed for other infrastructures and outdoor spaces. The CUBER team proposed a step change for finding indoor optimal routes for search and rescue teams [17, 18]. The developed algorithm is based on a pioneering approach integrating many advanced techniques. Its main limitation is that it is designed for a single agent only, and there is a need to develop a similar technique for evacuation flows (i.e. many agents). Also, the proposed mathematical method for hazard propagation is rather generic and it should be further improved.

Another algorithm depending on hazard locations was proposed by Park et al. [11], who developed a time-dependent optimal routing algorithm based on a 2D network representing the building configuration. The focus of their research is on computing optimal routes leading rescue personnel to disaster locations, taking into account the location of evacuees and smoke density. Even though the algorithm had a considerable potential, the authors concluded that the method needs "further improvements to fully apply to real-time evacuation systems".

Any algorithm for indoor navigation is based on a spatial model. A good example of such a model was given by Kwan and Lee ([8], Figure 5). The structure of a building is represented as a logical network, where the nodes represent spatial objects such as rooms, corridors and other navigable areas. The edges represent navigable connections between adjacent objects. The network can be further extended to a geometric network in order to model precise geometric properties (e.g. distance between nodes and their locations) and provide real navigation routes. Boguslawski et al. [1] recently developed a 3D building model, called a *navigable unified network*, which is an integration of Building Information Modelling (BIM) technology and Geography Information Science analysis. An example of the navigable unified network of a building is shown in Figure 3.1.

Another relevant area is multi-objective shortest path (MOSP) problems, which might be important for developing fast heuristics for flow algorithms. There are interesting approaches for MOSP problems, for example an Ant Colony Op-

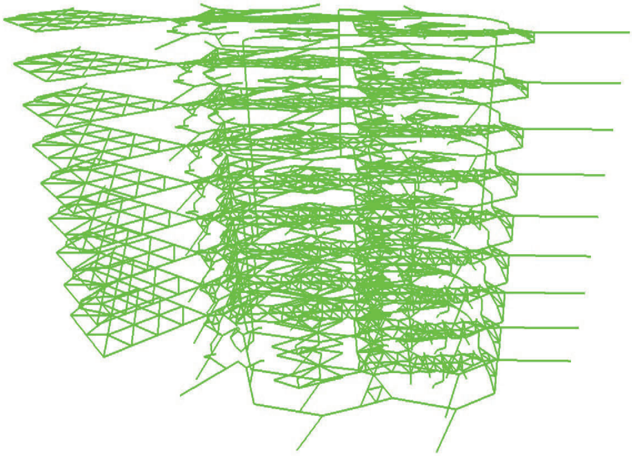

Figure 3.1: Example of navigable unified network.

timisation (ACO) algorithm has been successfully applied for finding evacuation routes during a tsunami [3]. This algorithm is an ACO heuristic for the MOSP problem. Such heuristics are important because standard MOSP problems are computationally harder than ones with a single objective. Some surveys in this area are worth mentioning. Pangilinan and Janssens [10] gave an overview of the MOSP problems and a review of essential issues for their solution. Skriver [12] as well as Ulungu and Teghem [14] reviewed the existing literature on MOSP problems.

The following projects can be helpful for different aspects of the proposed research: "Measurement and Prediction of Fire Smoke Toxicity of Materials in Enclosures"; "Adaptive Co-ordinated Emergency Response to Rapidly Evolving Large-Scale Unprecedented Events (REScUE)"; "Real Fires for the Safe Design of Tall Buildings". Further relevant projects are mentioned in the next section.

3.2 National Importance

The proposed project deals with infrastructures under hazardous condition, and its objectives align well with the research area in infrastructure and urban systems because the main goals of the project are to improve resilience of national infrastructures to disasters, increase security of the infrastructures and provide public safety. It is well known that the frequency of extreme events is increasing and their impact on the world both in terms of human lives and economic costs is becoming more dramatic. This conclusion is confirmed by the datasets from the Emergency Events Database (EM-DAT) created by the Centre for Research on the Epidemiology of Disasters [2].

The development of the proposed technology will greatly benefit the social environment in the UK, as its goal is to reduce the severity of impact of terrorist attacks and other disasters upon UK citizens and national infrastructure. As long as no such system is yet developed or deployed within the sector, this technology will also greatly improve upon the UK economy because it will be developed and sold by Company A and Company B. This will increase their strategic role in the UK market. Moreover, better understanding of hazard influence on a whole infrastructure may have further social impact because it may lead to improved safety policies in public places in the UK.

This project would complement the portfolio of existing projects in infrastructures, risk assessment, disaster management and resilience in different contexts:

- “Disaster Management and Resilience in Electric Power Systems”, University of Manchester, Prof P. Mancarella.
- “Emergency Resource Location-Allocation and Deployment (eROAD) Tool”, Newcastle University, Dr S. Dunn.
- “Soil-Value: Valuing and Enhancing Soil Infrastructure to Improve Societal Sustainability and Resilience”, Lancaster University, Dr J. Davies.

- “CRUST: Cascading Risk and Uncertainty Assessment of Earthquake Shaking and Tsunami”, University of Bristol, Dr K. Goda.

3.3 Academic Impact

To reach the academic community, we will take part in high-quality conferences, give a number of talks and publish main scientific results of the project in the top-ranked journal *Computer-Aided Civil and Infrastructure Engineering*, where the UWE team has recently published two papers. The results of this project will be of benefit to the academic and technological organisations interested in emergency response and evacuation procedures. Research institutes dealing with infrastructure risk assessment might be interested in simulations of hazard spread within infrastructures.

One of the goals of the project is to reduce the severity of impact of disasters upon citizens and national infrastructure, hence there will be academic beneficiaries in the areas of social environment and emergency response. The architecture community and organisations dealing with designs of buildings and disaster management would be interested in the developed methods because they can be used for safety assessment of new construction projects at the design stage, resulting in higher resilience to disasters and more secure infrastructures such as transport hubs, airports, stadiums and buildings.

Further, the proposed technology can be applied by academic organisations dealing with safety issues in public places and in the construction industry, especially where hazardous conditions may result in catastrophic consequences. The system can be used by different organisations to improve the safe design of public places and reduce the cost of future infrastructure redevelopment aimed at increasing public safety. Better understanding of hazard influence on a whole infrastructure may have further social impact because it may lead to improved safety policies in public places. Therefore, further academic beneficiaries include organisations dealing with safety policies. In a long term, the developed techniques can be used in military applications, which would be of interest to research departments of the Ministry of Defence (MOD) and relevant organisations.

3.4 Research Idea and Objectives

Main Idea and General Goal

The proposed project deals with infrastructures under hazardous condition and its main goals are to improve resilience of infrastructures to disasters, increase security of infrastructures and provide public safety. The general goal of the proposed project is to develop techniques, which can be used for smart evacuation in infrastructures under hazardous conditions. In particular, these techniques include identification of hazard epicentre locations, type of hazards, hazard intensity and optimal evacuation flows. They must be applicable to different infrastructures with the basic objective to save as many people as possible during evacuation.

We will investigate the potential for an advanced system — an intelligent decision-support system for smart evacuation that will be able to quickly and autonomously analyse and respond to possible hazards within infrastructures and will efficiently deliver the optimal egress routes to the evacuees with minimal involvement of rescue teams. Because no such system is yet developed or deployed within the sector, this technology will greatly improve upon the UK economy. It is anticipated that the system will be further developed and commercialised in the future by UWE and its partners: Company A and Company B.

Novelty and Timeliness

The focus of the innovation in this project will be on the development of new methods and techniques in the area of evacuation, which are based on the expertise in mathematics, computer science, decision analysis, modelling, ICT in built environment and other fields. The main features of the techniques, which would constitute innovative challenging solutions, are as follows: fully dynamical, real-time method, realistic propagation of hazard, reliable and robust, based on all relevant criteria. Currently, there exists no system that would take into account the aforementioned aspects. Hence the proposed project is timely, taking also into account that its goal is to reduce the severity of impact of disasters upon citizens and national infrastructure.

There must be a realistic propagation of hazard (e.g. smoke and heat) in an infrastructure from multiple sources, which is based on expertise in differential equations, numerical methods and forecasting techniques, and such that it works in real time. This is important for determining the optimal evacuation flow. All relevant decision-making criteria must be taken into account when choosing the optimal evacuation flow, for example hazard proximity of the flow, its evacuation time and complexity. (It may be pointed out that efficient delivery of the optimal egress routes to the evacuees is a work package in a separate three-year project).

A suite of algorithms for optimal flows must be developed because the standard flow algorithms do not take into account hazard and they do not work if link capacities are exceeded. Bespoke real-time heuristics will be developed, which would take into account the dynamics of hazard, people densities and all relevant criteria. Some reliable techniques will be used and further developed for this purpose, for example the Analytic Network Process (ANP), the ACO and MOSP algorithms. In addition, the entire system must be reliable and robust, which will be guaranteed by using reliable techniques and thorough testing and validation.

Measurable Objectives

1. Create a model of an artificial infrastructure including tools for rapid model modifications. Such a platform will include a spatial model (a priori information) and incorporate data from sensors and data sources (posteriori information), and it will be used for automatic generation of a navigable network suitable for propagation of hazard from multiple sources and for flow algorithms.

2. Develop methods for identifying critical spaces and potential hazard sources taking into account different types of infrastructures. Propose methods for locating epicentres of hazard, classifying the types of hazard and detecting the hazard intensity in different epicentres.

3. Develop hazard propagation simulator for single- and multi-source disasters. Based on mathematical methods, the system will automatically propagate hazard in the infrastructure, taking into account building properties of the infrastructure (e.g. fire ratings of materials), the location of hazard epicentres, the types of hazard and the hazard intensity in different epicentres.

4. Investigate the potential for developing smart evacuation tools, which would constitute an intelligent decision-support system for optimal escape routes under hazardous conditions. Using advanced decision-making techniques and operational research methods, we will develop and test a suite of algorithms for calculation of optimal evacuation flows.

3.5 Programme and Methodology

To successfully deliver this research project, it is split up into the following work packages:

WP1 "Platform"

(Month 1 – Month 3; Lead: co-I1)

The aim of this task is to develop a platform for spatial model representation and visualisation. The geometry, topology and semantics will be represented in a unified model and used to reconstruct navigable networks. An artificial infrastructure will be modelled and used as a case study. The platform will be developed as an in-house computer application. Essential functionality will be adapted from previous research done by the applicants. This includes BIM model reconstruction using the dual half-edge data structure, which integrates geometry, topology and semantics in one consistent 3D model. The data structure is accompanied by navigation and construction operators, which essentially facilitate user interface at a programming level. The topology (i.e. a logical building model) is automatically generated from the geometry and semantics of the original model and then used for generation of a navigable model as well as for effective model traversal without geometry-based search operations. Such a solution is suitable not only for indoor navigation purposes, but also for simulation of various hazard phenomena such as smoke and fire spread and their influence on the building. These issues are essential in the proposed project.

Some of the aforementioned aspects were investigated in the CUBER project, for example navigable networks with variable density and their generation. They will be adjusted and improved in order to meet the new objectives. New functionality will be introduced in order to allow intuitive data incorporation from other WPs, for instance information about hazard locations. This will also include a new mechanism for generation and storage of various scenarios and case studies used in the proposed project. Information about the model and its state will be visualised in a 3D engine based on OpenGL.

WP2 "Hazard Propagation"

(Month 1 – Month 10; Lead: co-I2)

Based on mathematical methods, the system should automatically propagate hazard in the infrastructure, taking into account building properties of the infrastructure, the location of hazard epicentres, the types of hazard and the hazard intensity in different epicentres. The focus of hazard propagation will be on heat and smoke spread. One approach will be to develop an air flow network model to predict smoke and heat movement at specific node points in the network within the designated evacuation period. A set of flow equations will be established, which take into account the specific geometry of the building, the thermal properties of the building as well as the initial temperature and heat release rate of the fire. The numerical solution of these equations will provide data about the temperature, pressure and smoke concentration at all points along the navigable networks. These results will need to be considered alongside other flow models, such as the fastest flow, so that temperature and smoke concentration can be used for better prediction of optimal evacuation routes.

High parametrisation of the model is anticipated, which allows for development of various scenarios. For example, the location and intensity of the fire epicentre as well as flammability of the building materials will be taken into consideration. The air flow network model will be compared to the field model, whereby we will consider a computational fluid dynamics model which solves the fundamental equations describing the fluid flow and heat transfer phenomena associated with fires. Whilst the field model is likely to provide significantly more accurate detail of the flow structure, the cost of computation is likely to prohibit its practical development for real-time hazard simulation.

WP3 "Simulator"

(Month 4 – Month 10; Lead: RF)

In this task, information about critical infrastructures, occupancy, hazard locations and hazard type, intensity and influence will be incorporated into the platform. A hazard propagation simulator for single- and multi-source disasters will be developed. Due to the limited number of possible egress routes in the indoor environment, hazards and their locations have a great impact on safety of building occupants. In the applicants' recent research, it was shown that hazard could make predefined egress routes too dangerous for their use by an unprotected person, and hence alternative routes have to be found. It is especially important in critical spaces, from where a large number of people are evacuated, for example emergency staircases, main corridors and lecture theatres.

In the proposed project, various types of hazard, hazard intensity and hazard influence on the building will be investigated, in particular smoke and fire spread. A special focus will be put on critical infrastructures, which include critical spaces, flammable gas installations, storage rooms with chemicals and other sensitive locations, where presence of unpredicted hazard may cause its intensification leading into a serious disaster and panic situations. Such locations pose a risk to evacuees if the hazard influence is high. In addition, evacuation cannot be properly planned and managed if at least approximate occupancy is not known. A high density of people affects people's speed of travelling and may cause congestions, which can be treated as a specific type of hazard, especially in a panic situation. Empirical data, provided by the project partner (Company B), will be used for estimated occupancy distribution. All these elements having influence on the evacuation analysis and safety of occupants will be incorporated in the model. Data input and management will be provided through an intuitive user interface.

WP4 "Optimal Flow"

(Month 1 – Month 10; Lead: PI)

This work package will be used to calculate optimal and safe evacuation flows/routes taking various groups of people into consideration. Using advanced decision-making techniques and operational research methods, we will develop a suite of algorithms for calculation of optimal evacuation flows/routes. One of the challenges is to develop a mathematical method to measure the proximity of a flow to hazard epicentres, which will be called a flow proximity index. This parameter is needed to measure how dangerous different flows are. For example, it might happen that the fastest flow (i.e. with the best evacuation time) is very dangerous and hence it is not suitable for evacuation. Thus, the main method is a combination of flow algorithms/heuristics and a decision-making technique for choosing the optimal flow, and hence this problem is subdivided into two sub-problems.

The first task is to generate in real time a set of feasible flows, which would include the fastest flow, the safest flow and further flows with different weights of decision-making criteria described below. We will develop and test a suite of algorithms for generating the set of feasible flows. Note that the standard flow algorithm do not take into account hazard and they do not work if link capacities are exceeded. Hence bespoke real-time heuristics for equilibrium flows will be developed, which would take into account the dynamics of hazard, people densities and all relevant criteria. Some reliable techniques will be used as building blocks for flows and further developed for this purpose, for example the ACO and MOSP algorithms.

The second task would be to develop a real-time version of a reliable multi-attribute decision-making technique (e.g. the ANP) to rank the flows and choose the optimal one. All relevant decision-making criteria must be taken into account when choosing the optimal evacuation flow, for example hazard proximity of the flow, its evacuation time and complexity. Thus, the optimal flow will be reasonably safe, fast and simple.

WP5 "Tests"

(Month 11 – Month 12; Lead: RF)

In order to validate the methods and algorithms included in the platform, several disaster scenarios will be designed and used in evacuation simulations to test the entire system. The results of the tests will be used to update the methods developed in the previous work packages.

Role of the Researchers in the Project

The proposed research requires considerable effort at a high technical level relying on expertise in computer science and modelling of infrastructures. The proposal for a researcher, full time for 12 months is, in our view, a cost-effective approach to meeting these requirements. The starting salary at G33 Scale is necessary to attract a Research Fellow (RF) at the desired level of expertise to ensure project success and high-quality research and implementation (e.g. of a hazard simulator). The detailed work plan provides a realistic plan of work for the RF. It may be pointed out that the named RF has the necessary expertise, which was confirmed by his recent successful work in the three-year CUBER project. The team for this project is multi-disciplinary because of the wide scope of problems to be analysed. The following list explains the expertise of other team members necessary for the proposed project: PI – network algorithms and decision analysis; co-I1 – ICT in built environment and Building Information Modelling; co-I2 – numerical methods for prediction and partial differential equations.

Project Management

This research project will fully exploit the benefits of collaboration between academia and industry. Company A, Company B and UWE will have an open flow of knowledge among the parties and will correspond regularly regarding technical and industrial advancements to ensure the continuous exchange of ideas, critique and building on each other's work. Regular meetings will be put in place to ensure a high level of innovation and creativity. Consequently, in addition to regular informal meetings of sub-groups, more formal meetings of the partners will take place on a regular basis. UWE is responsible for the overall management of the project.

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3.7 Resources and Work Plan

The following table provides a summary of resources required for the project in terms of full economic costs.

Table 3.1: Summary of costs.

Type of Costs	**Sub-type**	**Full Economic Costs** (£)
Directly Allocated Costs	Investigators	44,430
	Estates Costs	13,028
Directly Incurred Costs	Staff	45,449
	Travel & Subsistence	1,050
	Other Costs (PC)	1,852
Indirect Costs		63,720
	Total	**169,529**

Directly Allocated Costs

The duration of the project is 12 months, and both the PI and the co-Is are anticipating actively researching one day per week. In addition, the PI will spend 2.5 hours per week for management activities. Thus, the following numbers of hours per week are requested: PI – 10 hours; co-I1 – 7.5 hours; co-I2 – 7.5 hours. This time frame has been carefully thought over while preparing the work plan in order to take into account two things: additional commitments at the University of the West of England, Bristol, and the need to successfully carry out the project according to the proposed plan. Using a simple decision-making technique, it has been decided that one day per week per investigator is the most appropriate schedule to make a meaningful contribution to the project. Less than one day per week per investigator would be not enough to make such a contribution, whereas more than one day per week per investigator would jeopardise other commitments of the investigators. It has been calculated that the time frame of 12 months is appropriate for a project of this nature due to the volume of work to be undertaken.

The team for this project is multi-disciplinary because of the wide scope of problems to be analysed. The following list explains the expertise of the team necessary for the proposed project:

- PI: network algorithms and decision analysis.
- Co-I1: ICT in built environment and Building Information Modelling.
- Co-I2: numerical methods for prediction and partial differential equations.
- RF: computer science and modelling.

It may be pointed out that three researchers from the above list were a team in the very successful three-year project CUBER, and the main results of this project were published in two papers in the top-ranked journal *Computer-Aided Civil and Infrastructure Engineering*, which has Impact Factor 5.288 and is the best journal in 4 areas according to ISI Journal Citation Reports © Ranking, 2016. Also, the PI will be responsible for the general management of the project; he has had an experience of acting as a PI for the recent successful project "Autonomous Decision-Making for Maximising Security in Defence Sensor Networks".

Directly Incurred Costs

Staff

The proposed research requires considerable effort at a high technical level relying on expertise in computer science, BIM, spatial modelling and analysis. The named RF has the desired level of experience to ensure project success, high-quality research and implementation. He has the necessary expertise, which was confirmed by his recent successful work in a research project at UWE. The archived results and methodology are essential input into the proposed project, which will allow for immediate research development without any delay necessary for investigating fundamental ideas related to the topic. The proposal for the RF, full time for 12 months is, in our view, a cost-effective approach to meeting these requirements. The starting salary of £35,566 (G33 Scale) is necessary to attract the named researcher at the desired level of expertise to ensure project success and high-quality research and implementation.

Travel and Subsistence
Travel expenses are needed to exchange ideas with British and foreign specialists in the field and to disseminate the work. A talk will be given at the *3D GeoInfo Conference* (location: Europe; cost: £1,050). Funds for meetings between partners are not requested because they are available at the University of the West of England, Bristol.

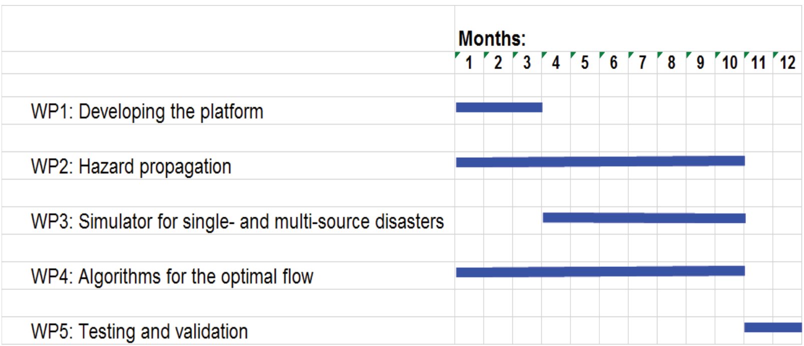

Figure 3.2: Work plan.

Other Incurred Costs
A high-performance laptop (a standard workstation) for the RF will be essentially required to carry out the project, including standard software. The laptop must have specifications sufficient to carry out the project, taking into consideration that this requires many programming and testing activities that are resource intensive.

To carry out this research, the PI and the co-Is have all the resources and infrastructure at the University of the West of England, Bristol, and therefore do not require additional equipment for this project. The work is genuine and will be done using available infrastructure.

Work Plan

The plan of the work is shown in Figure 3.2.

3.8 Impact Statement

Beneficiaries, Users of Research and Their Engagement
This research in general is for the benefit of the social environment, the UK economy, the public sector and the academia; and in particular this project will be of benefit to UK companies and academic and technological organisations interested in emergency response, risk assessment, evacuation procedures and safety policies. The following companies and organisations are main beneficiaries and users of research:

- Company A and Company B.
- Company B's user base: Companies C and D.
- Research organisations interested in military applications.
- Academic and technological organisations interested in emergency response, disaster management, resilience to

disasters, evacuation procedures, infrastructure risk assessment, social environment, public safety and safety policies.

Companies A and B have been engaged in the discussion of the ideas and work plans of this project. Both companies are very interested in developing these ideas further. Jointly with the UWE team, they will pursue funding to develop a state-of-the-art decision-support system for [topic of the joint project], which will be based on methods developed in this project. It may be pointed out that the UWE team has a clear understanding of the needs of Companies A and B within this subject area. The companies will be informed of the progress of the project on a regular basis to further impact upon their understanding of the area.

Social and Economic Impact

The development of the proposed technology will greatly benefit the social environment, as its goal is to reduce the severity of impact of disasters upon citizens and national infrastructure, including some terrorist attacks resulting in fire/smoke. As no such system is yet developed or deployed within the sector, this technology will also improve upon the UK economy because it will be developed and sold by Companies A and B. A direct economic impact should be observed by the aforementioned companies, as well as in the construction industry in general, where crisis management will be improved. The system can be used for safety assessment of new construction projects at the design stage, resulting in higher resilience to disasters and safer infrastructures such as transport hubs, airports, stadiums and buildings.

There are also beneficiaries within the public sector because the proposed technology can be applied by organisations dealing with safety issues in public places, especially where hazardous conditions may result in catastrophic consequences. The modelling methods and tools developed in this project will be available in the aforementioned system and they will improve the safe design of public places and reduce the cost of future infrastructure redevelopment aimed at increasing public safety. Moreover, better understanding of hazard influence on a whole infrastructure may have further social impact because it may lead to improved safety policies in public places.

Reaching End Users

To bring this product to market, Companies A and B will exploit direct routes to market such as exhibiting at various shows, press release distribution, emergency response networking and government/federal networking groups. Companies A and B will intend to distribute this innovative technology through their global networks. This will enable the companies to bring innovations, knowledge and experience worldwide and apply these to specific local needs and situations.

It is anticipated that Company B will distribute this technology through its global network of industry partners and prime contractors such as Companies C and D. Also, Company B will target the security market in Country X, where the company is well established and has sold products and service to government agencies for over 10 years. [Description of the market.] Company B will also expand its global arm by targeting the market of Country Y. Integration of the developed technology into Company B's software suite is the route-to-market strategy — features and functions outlined in this proposal have been requested by Company B's user base. Both Company A and Company B will also offer the technology as an 'add-on' to third-party emergency response tools.

To reach the academic, research and technological organisations, the UWE team will take part in high-quality conferences and give a number of talks at seminars. For example, a talk will be given at the *3D GeoInfo Conference*. Also, the main results of the project will be published in the top-ranked journal *Computer-Aided Civil and Infrastructure Engineering*, where the team has recently published two articles.

Long-Term Collaboration and Existing Engagement
This research project will fully exploit the benefits of collaboration between academia and industry. Companies A and B and the UWE partner will have an open flow of knowledge among the parties (subject to IP rules) and will correspond regularly regarding technical and industrial advancements. The both companies have previous successful experience in developing and exploiting a similar technology in collaboration with academia. Moreover, in order to widen collaboration and make sure it will become long-term, the UWE partner is actively searching for PhD students to work on the topic of the project and provide a continuous link between the proposed research and follow-up projects.

It may be pointed out that the UWE partner and Company B have a long-term collaboration. Companies A and B are about to implement a pilot study for a joint project. Thus, the proposed project constitutes an important phase to establish a long-term collaboration among three parties: UWE, Companies A and B. It will be the foundation for a three-year joint project with three parties for the development of the state-of-the-art decision-support system for [topic of the joint project].

3.9 Reviewers' Reports

This section includes four reviewers' reports [15], where some text was replaced by comments about the type of removed information in square brackets. The final conclusions in subsections "Overall Assessment" of the reports are based on six predefined statements, which can be found at the end of Section 1.8. The reviewers' comments are in *italics*.

Report 1

Response Due Date: 24 July

Quality

(1) The project is certainly timely as illustrated by recent terrorist attacks in London and Manchester and especially by the Grenfell Tower fire. While the project is a continuation of the successful previous project [CUBER], *it has several new elements as the project team suggests to tackle a harder problem, in particular, when not just one agent (a rescue team or a small group of evacuees) but many agents are to be evacuated. This raises significantly the "dimensionality" of the evacuation problem, and thus requires a number of new ideas to deal with the problem. I do appreciate the short period of time, just a year, for the project as its outcomes are especially useful in the circumstances this country finds itself recently.*

(2) I think that despite being a continuation of the successful previous project CUBER, the new project is sufficiently ambitious and adventurous due to the significant raise in the "dimensionality" of the evacuation problem mentioned above. This is amplified given a short period of theme for the project.

(3) I think the methodology is appropriate. In particular, the use of heuristics such as Ant Colony Optimisation is fully justified for tackling the multi-objective shortest path problems (MOSPs), to which the evacuation problem can be reduced, since MOSPs are intractable optimisation problems.

Importance

(1) I like the fact that the proposal combines tools and ideas from different disciplines. (Only this way the mathematical model for the evacuation problem becomes quite close to the real-life problem.) In particular, the project will contribute to the area of combinatorial optimisation by testing efficiency and effectiveness of certain approaches such as Ant Colony Optimisation. I should admit that I have been sceptical of Ant Colony Optimisation to tackle such classical combinatorial optimisation problems as TSP, but I think the approach will likely to prove highly appropriate for the evacuation problem. In such a case, researchers in Ant Colony Optimisation will realise even further

that the approach should be used for appropriate optimisation problems rather than "universally".

Terrorism and disasters such as the Grenfell Tower fire are currently among the most challenging UK societal problems. The proposal aims at reducing the human cost of terrorism and disasters. The authors of the proposal have identified companies which whom they will carry on developing the results of the project, which is likely to lead to UK economic advance in the area of security technology.

(2) It appears the project team has already established itself as world leaders on the very important evacuation problem. The proposal will maintain and strengthen this position.

[The reviewer lists the projects mentioned in Section 3.2]

Impact

(1) The authors have identified companies and organisations which are main beneficiaries and users of proposed research: [names of companies], *academic and technological organisations interested in emergency response, disaster management, resilience to disasters, evacuation procedures, etc. and research organisations interested in military applications. This covers a lot of potential "customers" and it looks complete to me. I think the impact is realistic given the way they model the real-life problem and the approaches they will employ to solve it.*

(2) I think the author have listed realistic activities to help realise the impacts. In fact, the main routes are via their industrial partners whose expertise makes realisation of the impacts realistic.

(3) I think the authors have identified appropriate and relevant beneficiaries or collaborators.

Applicant

(1) The applicants are certainly experts in their research areas. In particular, almost all applicants have successfully carried out related research project CUBER.

(2) The balance of skills is very good as each member of the

team is a specialist in his/her area of research and each of them is vital to successfully carry out the project.

Resources and Management
The applicants require only one year of funding and their requests are very moderate, i.e. a very small amount of money to travel. I was surprised to see that they plan to complete the project just within a year, but given their commitment to the project, I believe it's realistic. They do not require much of equipment, just a high spec laptop.

Overall Assessment
I have found the project very timely, of high quality, of sufficient degree of novelty especially for just a year project. It's ambitious and adventurous. The proposed methodology is appropriate. The project is certainly important and of high potential impact to the UK economy and society; the applicants are recognized specialists in their areas of research; they require a very limited amount of resources for just one year. I do not see any problems with the project.

My judgement:
This is a very strong proposal that fully meets all assessment criteria.

My confidence level: Low, Medium, High

Reviewer Expertise
Combinatorial Optimisation and its Applications.

Report 2

Response Due Date: 26 July

Quality
(1) The project develops simulations tools to model hazard propagation in built-environments during disaster scenarios and

provide optimal evacuation routes. The novelty of the work appears to be the propagation of smoke and heat hazards into a unified topological/geometrical/semantic model of the environment (a-priori defined), and the design of optimal flow algorithms.

The applicants claim that they plan to take into account sensors and other data sources in the model. This would be highly novel and relevant in light of the large number of sensors nowadays in building and even on people. However the workplan does not detail this aspect and it is not clear to which extent sensors would be used in the simulation. This could be clarified in a response. I believe that the inclusion of sensors (and ideally sensors on people, such as mobile phone data indicating movement and location) would make this project stand out.

The project is very timely from an application perspective, in light of the recent terrorist attacks and the Grenfell disaster.

(2) The project will make a significant improvement beyond the current state of the art, although it falls short of being transformative as it is an extension of prior work (which used single agent and didn't model hazard propagation). Nevertheless this work will bring highly important improvement, esp. the inclusion of hazard propagation and use of sensors. This could lead to a transformation in the way in which disasters are managed, with information potentially being delivered to people at the hazard site autonomously.

(3) The methodology appears appropriate at a high level. I have limited ability to judge the realism of the hazard propagation models. I found the proposal to lack inclusion of any human factors (considering agents as having own internal decision process) in the simulation model. Indeed, cognitive aspects related to trust (to information received, peers, friends), fear, etc often drive the way people behave in emergency situations. An optimal route recommended by the system may not be followed by people due to these factors. The proposal appears to treat humans as a flow of "inert particles" and there may be limits to the realism of such an approach. I was not able to evaluate the

appropriateness of the approach to include sensors data in the simulation, as this was not explained in the workplan.

Importance

(1) The project would help the UK address key societal challenges, especially in light of recent terrorist attacks and the Grenfell disaster. It is well possible that this project would help first responder manage such situations much more rapidly and effectively.

There are two companies associated to the project [names of companies]*, which both appear to be active in the UK and would benefit from this research. This project could help support the UK economy.*

I would recommend the applicants to seek patent protection for some of the key ideas if the goal is to eventually aim at commercialisation.

(2) [Name of company] *appears to be a leading provider of* [area of expertise] *and could increase its competitive advantage through this project.*

(3) The proposal matches well the [funder's research area]*. The applicants indicated a number of projects in the UK which relate to this proposal, so this work is not going to be carried in isolation and has the potential to feed into other research groups.*

Impact

(1) The impacts are clear, realistic and well identified. They lie primarily in improved response to disasters with immediate societal benefits. They potentially also lie with economic impact through the two companies affiliated with the projects.

(2) The project does not seem very ambitious along the activities to achieve impact. Only costs for one conference travel at 3D GeoInfo Conference are planned. The proposal seems to hang on [names of companies] *bringing the impact, but these two companies did not put in matching resources into the project so it is not clear to which extent they take this project at heart.*

I would recommend the applicants to seek patent protection *of some findings if they seek to eventually commercialise and/or benefit from technology transfer to a company.*

(3) The key beneficiaries appear to be the two companies [names of companies]*. These appear to be active in the sector and therefore appropriate and relevant.*

Applicant

(1) The applicants have collaborated together during [the CUBER project] *addressing the foundations on which this project is built (i.e. single agent only, no hazard propagation model). The project outcomes were 2 publications published in a high impact journal. I was surprised that such a large project led to such small number of publications. However the Research Fellow who will be working full time on the project shows a promising track record, with an H-index of 6 with publications starting from 2011. This is higher than many postdocs of similar academic age.*

(2) The team has the right mix of expertise to deliver the project: graph theory and decision making, decision support tools for built environment, optimal transport methods, 3D spatial modelling.

Resources and Management

The project is of short duration with a team that knows each other well from participating in a previous 3-year long project, and therefore the nimble management structure in place, with regular meetings (although of unspecified frequency) appears appropriate for this project.

The resources requested are modest and appropriate to conduct the research. I found that there has been only limited ambition to participate to conferences (only one costed), however as the applicants seem to follow a high impact journals strategy that might be adequate.

Overall Assessment

Overall, I find the project objectives, workplan to achieve these objectives, and team in place to be capable of delivering the out-

comes, which will have scientific impact, highly relevant societal impact, and potentially as well economic impacts through uptake via two companies affiliated with the project.

The societal impacts along better management of emergency/ disaster situation is highly relevant in the current climate with terrorist attacks and the Grenfell disaster, and this project puts the science into place which will allow better management of such situations. In the future, this may lead even to autonomously provide information directly on user's mobile device of evacuation routes.

On the negative, and this can be clarified by the applicants:
- The proposal mentions that sensors will be used but the workplan does not explain how sensors (and which sensors) will be integrated into the simulation models.
- The project aims at impact solely through publications. It would be interesting of open-sourcing some of the simulation tools (or have a public interface to server-hosted algorithms for other groups to use).
- The two companies that are affiliated with the project did not put matching funds into the project and therefore it is not clear if the project is only of passing interest or if they have plans to exploit the project results.

My judgement:
This is a strong proposal that broadly meets all assessment criteria.

My confidence level: Low, Medium, High

Reviewer Expertise

My background is in pervasive/ubiquitous computing, particularly in the use of sensors embedded in the environment or in wearable devices and the interpretation of the resulting signals through machine learning techniques to infer user context and activities. [Applications of this area.]

Report 3

Response Due Date: 11 September

Quality

This proposal aims to develop a Decision Support System for evacuating different types of infrastructure under different types of hazardous conditions. The proposed system has four major constituent components that will be developed by the corresponding work packages: i) platform for spatial modelling for representation and visualization, ii) hazard propagation, iii) simulator, and iv) optimal flow. The proposal sets a number of ambitious objectives. However, from the technical description of the methodology of the project, and the associated project plan, it is not clear how the project objectives will be achieved. It is worth stating that this is a multidisciplinary proposal and that the expertise of this reviewer relates mostly to the optimal flow, and the development of Decision Support Systems (DSS) part of the proposed research.

The description of the methodology for the optimal flow part of the project is provided at a very macroscopic level and lacks specificity regarding the types of models that will be used to mathematically represent pedestrian flows in different types of buildings including transport terminal facilities such as airports, railroad stations, etc. Furthermore, the proposal does not provide sufficient reference to previous research related to the modelling of pedestrian flows. Reference is made to a method (ANP) for ranking alternative flows, however there is no justification why the proposed method is the most appropriate to be used to address the problem at hand, and how ANP will be operationalized.

Other methodological issues include: i) the lack of a workpackage that will integrate the constituent components of the proposed DSS, ii) the lack of a feedback loop between WP4 (Optimal Flows) and WP2 (Hazard Propagation) and WP3 (Simulator). Although the proposal identifies five work packages it *does not explain sufficiently the relationship among them, iii) testing and validation is a very important component of the development of any DSS. However, the description provided for this work package is very brief and provides limited technical information regarding how the proposed system will be validated, and how the acceptability of the system by the end-users will be evaluated, iv) there is no sufficient information regarding the implementation of the ranking method (ANP).*

Importance

The proposal addresses key UK societal challenge related to public safety and security, and the management of infrastructure. The proposal addresses issues relevant to the [funder's research area]. *The proposed research has the potential to improve public safety and security against major hazards. However the proposal, at least the part related to optimal flow, does not include sufficient technical detail that will provide the information needed to judge the potential of the proposed research to establish/maintain a unique world leading activity in this sector. The proposed work is complementary to* [funded projects] *related to risk assessment, disaster management, and infrastructure resilience.*

Impact

The proposal has identified the potential impacts of the project. Dissemination activities and the communication media that will be used to disseminate the results to the broader professional and academic communities are not presented with the required level of detail. Furthermore, the project plan does not include a work package dealing with the communication and dissemination activities of the project. The beneficiaries and collaborators of the project are appropriate and their commitment to the project is clearly stated.

Applicant

The research team has adequate and appropriate track record regarding hazard modelling and hazard propagation. However, the *area of pedestrian flow modelling is not adequately represented. In particular, expertise for modelling pedestrian flows in major transport hubs, and expertise for modelling the interactions of operations of major transport terminals, e.g. airports, railroad stations, etc., with pedestrian flows needs to be enhanced.*

Resources and Management
The description of the project management activities is very brief. The Gantt Chart is presented at a very aggregate level and specific milestones are not identified. The project plan does not include any explicit WP dealing with the dissemination and communication activities of the project, and the system integration. The project management structure and procedures are not adequately described. The proposed project duration is rather tight for meeting the project objectives.

Overall Assessment
This proposal seeks to address a problem of growing concern related to public safety, security, and infrastructure resilience. The proposal meets the assessment criteria but it has the following limitations: i) the level of detailed used to describe pedestrian flow modelling is not adequate, ii) the selection of the methodology for ranking flows is not adequately explained and there is no description how the ANP method will be operationalized in the context of the proposed research, iii) there is no adequate description of the validation of the proposed system, iv) there is no an explicit Work Package dealing with the dissemination and communication activities of the project, v) the proposed time-frame is rather tight.

My judgement:
<u>This is a good proposal that meets all assessment criteria but with minor weaknesses.</u>

My confidence level: Low, Medium, <u>High</u>

Reviewer Expertise
Operations Research, Transportation, Multi-Criteria Decision Making.

Report 4

Response Due Date: 27 June

Quality

The proposal is concerned with developing methods to optimize evacuation from infrastructures during emergencies involving hazardous conditions.

Detail is not provided at an appropriate level to properly identify novelty, and therefore I would have to say the novelty is low. The review of related work is not current (majority > 10 *years old, with some much older). In a research proposal I would expect the state-of-the-art to be compared, with the majority of papers reviewed from the past 3 years, leading to one or two very specific problems to be identified. This leads to objectives which are too vague.*

The methodology is very high level. For example, it refers to use of "mathematical models" and "advanced decision-making techniques and operational research methods", without really giving details. I would expect to see much more detail provided in a research proposal. Validation is left to the very end of the project, which does not leave time for further improvements. Again, insufficient detail is provided on how validation will be performed. The workplan provides very little additional detail (no sub-tasks, milestones, etc.).

Importance

Efficient evacuation during major incidents is of course of great national importance. Other work has been carried out in this area in the UK, and this is an area funded by the [funder]. *Outputs from this project appear to be relevant to another project planned by the team. It would have been helpful to see a description of activities of how this could support other researchers in this area (especially UK), other than just publications, e.g. provision of open-source software/data-sets, training activities, etc.*

Impact
The proposal discusses impacts to [name of company] *and its customers, the UK economy, and the construction industry generally. I think it is very unrealistic that a project of this scale/ duration will have a large impact on this areas. Indeed much of the* [Impact Statement] *was spent redescribing what the benefits are, rather than providing detail on the activities that would be undertaken to realise the impacts. Two appropriate project partners are listed, however they appear to be more beneficiaries than actually contributing to the project. Indeed, no contribution was given in the proposal, even indirect contributions such as staff-time, use of facilities, etc.*

Applicant
The applicants have publications and recent funding in this area, and their skills cover a range of areas. For evacuation studies, some expertise from social science would have been welcome. Overall though, the project scope matches the experience of the applicants.

Resources and Management
Very few resources are requested as the project is only for 12 months. As well as usual PI/Co-I time, a named researcher is requested for 12 months.

Funds are requested to attend a specific conference in 2018, as well as for a laptop for the researcher. Both of these are appropriate.

I would have expected resources requested for pathways to impact activities.

A description of project management is provided but I would have expected more detail (how regular are meetings, how will code be managed, etc.)

Overall Assessment
Although the applicants have recent publications in this important research area, overall I found quality to be a concern in this proposal. I would have expected very specific problems to be

identified from a review of the state-of-the-art, leading to better formulated objectives. The workplan and work-packages did not provide enough detail, especially for a 12 month project. I do not recommend this proposal to be funded.

My judgement:
This proposal does not meet one or more of the assessment criteria.

My confidence level: Low, Medium, High

Reviewer Expertise
Optimization, machine learning, agent-based simulation (of emergency response).

3.10 PI's Response to Reviewers' Comments

We thank the reviewers for their comments. Concern has been voiced from Reviewer 1 regarding the project duration: "*I was surprised to see that they plan to complete the project just within a year, but given their commitment to the project, I believe it's realistic*". The duration of the project has been carefully thought through, taking into account the amount of work for this project and the pace of work. The same research team has recently had the successful project CUBER. Considering this experience, good personal relationships and good understanding of the project, we can work very effectively within our research team.

Reviewer 2

The applicants claim that they plan to take into account sensors and other data sources in the model. This would be highly novel and relevant in light of the large number of sensors nowadays in building and even on people. However the workplan does not detail this aspect and it is not clear to which extent sensors would be used in the simulation. This could be clarified in a response.

I believe that the inclusion of sensors (and ideally sensors on people, such as mobile phone data indicating movement and location) would make this project stand out.

Indeed, we plan to use sensors such as smoke/CO/temperature sensors to help with accurate hazard propagation and will investigate the potential of using smart phones and motion sensors for location of people, which is important for finding optimal flows. The optimal distribution of sensors in an infrastructure is a mathematical problem that has to be solved yet, including a forecasting technique for prediction of missing data in case when some sensors are faulty or have been destroyed by an extreme event.

I found the proposal to lack inclusion of any human factors (considering agents as having own internal decision process) in the simulation model. Indeed, cognitive aspects related to trust (to information received, peers, friends), fear, etc often drive the way people behave in emergency situations. An optimal route recommended by the system may not be followed by people due to these factors. The proposal appears to treat humans as a flow of "inert particles" and there may be limits to the realism of such an approach.

Human factors are outside the scope of this project because its focus is on hazard propagation and (optimal) controlled evacuation. In fact, human factors resulting in (partially) uncontrolled evacuation will be investigated in a large follow-up project jointly with our industrial partners (Company A and Company B).

The project does not seem very ambitious along the activities to achieve impact. Only costs for one conference travel at 3D GeoInfo Conference are planned. The proposal seems to hang on [names of companies] *bringing the impact, but these two companies did not put in matching resources into the project so it is not clear to which extent they take this project at heart.*

We plan to take part in other relevant conferences/meetings (e.g. *GeoAdvances*, research seminars); the funds for those events are available at UWE, Bristol. Also, this project initiates the long-term collaboration among three parties: UWE, Company A and Company B. The support letters by Company A and Company B clearly show that they are very interested in this project. If it is successful, then they will put necessary resources in the large follow-up joint project.

The project outcomes were 2 publications published in a high impact journal. I was surprised that such a large project led to such small number of publications. ... The project aims at impact solely through publications. It would be interesting of open-sourcing some of the simulation tools (or have a public interface to server-hosted algorithms for other groups to use).

Actually there were 10 research outcomes for the CUBER project. Also, open-sourcing is a good idea; we will definitely consider implementing it.

Reviewer 3

The proposal sets a number of ambitious objectives. However, from the technical description of the methodology of the project, and the associated project plan, it is not clear how the project objectives will be achieved.

Based on our expertise in the field, we have a clear vision and a plan to successfully achieve the objectives.

The research team has adequate and appropriate track record regarding hazard modelling and hazard propagation. However, the area of pedestrian flow modelling is not adequately represented. In particular, expertise for modelling pedestrian flows in major transport hubs, and expertise for modelling the interactions of operations of major transport terminals, e.g. airports, railroad stations, etc., with pedestrian flows needs to be enhanced.

We have already made a contact with [name of a known specialist and a company], who will help with providing solutions for people's movement (e.g. pedestrian and crowd simulations).

The proposal meets the assessment criteria but it has the *following limitations: i) the level of detailed used to describe pedestrian flow modelling is not adequate, ii) the selection of the methodology for ranking flows is not adequately explained and there is no description how the ANP method will be operationalized in the context of the proposed research, iii) there is no adequate description of the validation of the proposed system, iv) there is no an explicit Work Package dealing with the dissemination and communication activities of the project, v) the proposed time-frame is rather tight.*

Indeed, we have tried to describe general ideas because many technical details will emerge during actual research. All necessary expertise is available for achieving successful research, impact and publications in top-ranked journals. We agree with the reviewer that those shortcomings are minor and the proposal meets all assessment criteria. The proposed time-frame is explained in the first paragraph of this section.

Reviewer 4

Detail is not provided at an appropriate level to properly identify novelty, and therefore I would have to say the novelty is low. The review of related work is not current (majority >10 years old, with some much older). In a research proposal I would expect the state-of-the-art to be compared, with the majority of papers reviewed from the past 3 years, leading to one or two very specific problems to be identified. This leads to objectives which are too vague. The methodology is very high level. ... Validation is left to the very end of the project, which does not leave time for further improvements.

Our research in this area is world leading with publications in high-impact journals. As described in the previous paragraph, we believe that these minor shortcomings will not affect the successful completion of this project. Also, there was no objective to provide a current literature review as part of the proposal. The reference list includes the most important/relevant research papers, regardless of their 'age'. In our view, the objectives are clearly formulated in the proposal. Validation is typically done at the very end of the project; we have successful experience in validating our previous results and algorithms.

It would have been helpful to see a description of activities of how this could support other researchers in this area (especially UK), other than just publications, e.g. provision of open-source software/data-sets, training activities, etc. ... A description of project management is provided but I would have expected more detail (how regular are meetings, how will code be managed, etc.)

The idea of providing an online testing tool, available to other experts in the field, will be implemented in due course, whereas open-sourcing will be considered. Also, some findings of this project will be included in the masters course 'Networks and Graphs' at UWE, Bristol. We will have regular bi-weekly brainstorming meetings within our research team and every few months with the partners. This approach has worked well in the CUBER project.

I think it is very unrealistic that a project of this scale/duration will have a large impact on this areas.

The proposed research is rather fundamental/exploratory — the best solution will be further developed in the large follow-up project in collaboration with our industrial partners. The total expected impact will be large.

3.11 Summary of Key Lessons

In this section, we provide a brief summary (Table 3.2) of key lessons learnt from the proposal.

Although there is typically a page limit, all parts of the methodology must be clearly explained, including sufficient reference to previous research, work packages and the relationship among them, testing and validation (if relevant). If your method is only based on some factors, criteria, etc., you have to clarify why other factors, criteria, etc. are outside the scope of the project. The proposal in this chapter is an example of

Table 3.2: Key Lessons.

Strengths	**Weaknesses**
The methodology is appropriate; the use of heuristics such as ACO is fully justified.	The methodology is described at a macroscopic level (e.g. the use of sensors, the ANP, testing and validation). Human factors are not included.
The project is certainly timely and it is sufficiently ambitious and adventurous.	Insufficient reference to previous research; the literature review is not current.
The impacts are clear, realistic and well identified.	The relationship among work packages is not well explained.
Very good balance of skills.	The objectives are too vague.
This project could help support the UK economy.	The previous project (CUBER) is not well described.
The project will make a significant improvement beyond the current state of the art.	Expertise for modelling pedestrian flows needs to be enhanced, as well as expertise from social sciences.
The inclusion of sensors would make this project stand out.	The partners did not put matching funds into the project.
The proposal matches well the funder's research area.	Open-sourcing for simulation tools was not promised.
The project team has already established itself as world leaders in the area.	Some work packages are missing (for communication and dissemination activities; for the DSS).
The project would help the UK address key societal challenges.	The Impact Statement could include more activities and necessary resources.
The beneficiaries and collaborators of the project are appropriate.	The project management could be better described (e.g. the frequency of meetings is not specified).
The commitment of beneficiaries and collaborators to the project is clearly stated.	The project duration (one year) is rather tight. Only one conference is planned.

the methodology presented at a macroscopic level. (Another example is Chapter 7.)

Reviewers expect to see a literature review of recent papers. The idea to provide most relevant review/survey papers, which was used in this chapter, does not work because many of those papers are rather old. If you mention a past research project in your proposal, briefly provide all 'positive' information about it (e.g. the total number of research outputs). In addition, it is necessary to identify which aspects of expertise are not available or have to be enhanced, and who will provide such expertise.

For projects with algorithmic components, it is a good idea to promise "open-sourcing" at least some of computer codes, data-sets, etc. Another good idea would be to have "a work package dealing with the communication and dissemination activities". Also, the Impact Statement must include information on the activities that would lead to the impacts (as opposite to the description of benefits to the partners), and it is a good idea to request necessary resources for such activities. If there are interesting applications with potential impact, then explain them, but do not make up unrealistic impacts. The dissemination arrangements must be reasonably ambitious (e.g. a leading international conference, etc.). In addition, the frequency of all planned meetings must be specified. Also, it appears that reviewers dislike one-year projects, perhaps developers are trying to plan too many objectives within just 12 months.

Acknowledgement

The author is grateful to Dr E. Walsh and Dr P. Boguslawski for their help in the development of this project.

Chapter 4

The Probabilistic Method for Domination Parameters

4.1 Aims and Background

Both classical domination and multiple domination are important areas of graph theory with interesting applications. One of the fundamental results in domination theory is due to Arnautov, Lovász and Payan [1, 8, 9]. In 1974 and 1975, they proved that the domination number of an n-vertex graph with minimum degree δ is at most

$$\frac{\ln(\delta+1)+1}{\delta+1}n.$$

There are generalisations of this result for double domination, triple domination, k-tuple domination and k-domination. The strongest (unproved) generalisation is given in the Rautenbach–Volkmann conjecture on the k-tuple domination number (see next section).

We plan to prove the Rautenbach–Volkmann conjecture and then extend this result to the parametric domination number. More precisely, the main aims of the proposed project are:

- Prove the Rautenbach–Volkmann conjecture.
- Introduce the parametric domination number, which naturally generalises classical domination, k-domination, k-tuple domination and k-total domination.
- Generalise the proof of the aforementioned conjecture for parametric domination.

To sum up, the basic idea of the proposed research project is to prove the Rautenbach–Volkmann conjecture and then generalise the result for parametric domination. Thus, analogues of the Arnautov–Lovász–Payan bound will be obtained for the k-domination number, the k-tuple domination number and the k-total domination number.

4.2 The Classical Bound and Related Results

The concept of domination is fundamental in graph theory. Interesting applications of domination can be found in the books [6, 7]. One of the important directions of research in this area is search for various bounds of domination parameters.

If G is a graph, then $V(G) = \{v_1, v_2, ..., v_n\}$ is the set of vertices in G, d_i denotes the degree of v_i and

$$d = \sum_{i=1}^{n} d_i/n$$

is the average degree of G. Let $N(x)$ denote the neighbourhood of a vertex x. Also, let

$$N(X) = \cup_{x \in X} N(x)$$

and $N[X] = N(X) \cup X$. Denote by $\delta(G)$ and $\Delta(G)$ the minimum and maximum degrees of vertices in G, respectively. Put $\delta = \delta(G)$ and $\Delta = \Delta(G)$.

A set X is called a *dominating set* if every vertex not in X is adjacent to a vertex in X. The minimum cardinality of a dominating set of G is the *domination number* $\gamma(G)$. A set X is called a *k-tuple dominating set* of G if for every vertex $v \in V(G)$,

$$|N[v] \cap X| \geq k.$$

The minimum cardinality of a k-tuple dominating set of G is the *k-tuple domination number* $\gamma_{\times k}(G)$. The k-tuple domination number is only defined for graphs with $\delta \geq k-1$. It is easy to see that $\gamma(G) = \gamma_{\times 1}(G)$ and

$$\gamma_{\times k}(G) \leq \gamma_{\times k'}(G)$$

for $k \leq k'$. The 2-tuple domination number $\gamma_{\times 2}(G)$ is called the *double domination number*, and the 3-tuple domination number $\gamma_{\times 3}(G)$ is called the *triple domination number*.

Arnautov, Lovász and Payan independently proved the following fundamental result:

Theorem 4.1 [1, 8, 9] *For any graph G,*

$$\gamma(G) \leq \frac{\ln(\delta+1)+1}{\delta+1}n.$$

This upper bound is actually the strongest one for general graphs. Since $\gamma(G) = \gamma_{\times 1}(G)$, we have

$$\gamma_{\times 1}(G) \leq \frac{\ln(\delta+1)+1}{\delta+1}n.$$

Harant and Henning found an upper bound for the double domination number:

Theorem 4.2 [5] *For any graph G with $\delta \geq 1$,*

$$\gamma_{\times 2}(G) \leq \frac{\ln\delta + \ln(d+1)+1}{\delta}n.$$

Rautenbach and Volkmann posed the following interesting conjecture:

Conjecture 4.1 [10] *For any graph G with $\delta \geq k-1$,*

$$\gamma_{\times k}(G) \leq \frac{\ln(\delta-k+2)+\ln\left(\sum_{i=1}^{n}\binom{d_i+1}{k-1}\right)-\ln(n)+1}{\delta-k+2}n.$$

For $m \leq \delta$, let us define the m-degree $\widehat{d_m}$ of a graph G as follows:

$$\widehat{d_m}=\widehat{d_m}(G)=\sum_{i=1}^{n}\binom{d_i}{m}/n.$$

Note that $\widehat{d_1}$ is the average degree d of a graph and $\widehat{d_0}=1$. Also, we put $\widehat{d_{-1}}=0$.

Since

$$\binom{d_i+1}{k-1}=\binom{d_i}{k-1}+\binom{d_i}{k-2},$$

we see that the above conjecture can be re-formulated as follows:

Conjecture 4.1′ *For any graph G with $\delta \geq k-1$,*

$$\gamma_{\times k}(G) \leq \frac{\ln(\delta-k+2)+\ln(\widehat{d_{k-1}}+\widehat{d_{k-2}})+1}{\delta-k+2}n.$$

It may be pointed out that this conjecture, if true, would generalise Theorem 4.2 and also Theorem 4.1 taking into account that $\widehat{d_{-1}}=0$. Rautenbach and Volkmann proved the above conjecture for the triple domination number:

Theorem 4.3 [10] *For any graph G with $\delta \geq 2$,*

$$\gamma_{\times 3}(G) \leq \frac{\ln(\delta-1)+\ln(\widehat{d_2}+d)+1}{\delta-1}n.$$

Furthermore, this bound was generalised by Gagarin and Zverovich:

Theorem 4.4 [4] *For any graph G with $3 \le k \le \delta + 1$,*

$$\gamma_{\times k}(G) \le \frac{\ln(\delta - k + 2) + \ln\left(\sum_{m=1}^{k-1}(k-m)\widehat{d}_m - d\right) + 1}{\delta - k + 2} n.$$

However, the bound of Theorem 4.4 is still far from the bound of Conjecture 4.1$'$ because of the sum under the second logarithm.

4.3 Proof Technique of the Conjectures

We strongly believe that Conjecture 4.1$'$ is true and have several ideas how to prove it. The best idea is to develop the following probabilistic method.

Let A be formed by an independent choice of vertices of G, where each vertex is selected with probability p, $0 \le p \le 1$. For $m = 0, 1, ..., k-1$, let us denote

$$B_m = \{v_i \in V(G) - A : |N(v_i) \cap A| = m\}.$$

Also, for $m = 0, 1, ..., k-2$, we denote

$$A_m = \{v_i \in A : |N(v_i) \cap A| = m\}.$$

For each set A_m, we form a set A'_m in the following way. For every vertex in the set A_m, we take $k - m - 1$ neighbours not in A. Such neighbours always exist because $\delta \ge k - 1$. It is obvious that

$$|A'_m| \le (k - m - 1)|A_m|.$$

For each set B_m, we form a set B'_m by taking $k-m-1$ neighbours not in A for every vertex in B_m. We have

$$|B'_m| \le (k - m - 1)|B_m|.$$

We construct the set D as follows:

$$D = A \cup \left(\bigcup_{m=0}^{k-2} A'_m\right) \cup \left(\bigcup_{m=0}^{k-1} B_m \cup B'_m\right).$$

The set D is a k-tuple dominating set. The expectation of $|D|$ is

$$\begin{aligned}
\mathbb{E}[|D|] &\leq \mathbb{E}\left[|A| + \sum_{m=0}^{k-2} |A'_m| + \sum_{m=0}^{k-1} |B_m| + \sum_{m=0}^{k-1} |B'_m|\right] \\
&\leq \mathbb{E}\left[|A| + \sum_{m=0}^{k-2} (k-m-1)|A_m| + \sum_{m=0}^{k-1} (k-m)|B_m|\right] \\
&= \mathbb{E}[|A|] + \sum_{m=0}^{k-2} (k-m-1)\mathbb{E}[|A_m|] \\
&\qquad\qquad + \sum_{m=0}^{k-1} (k-m)\mathbb{E}[|B_m|] \\
&\leq pn + (k-1)\mathbb{E}[|A_0|] + (k-2)\mathbb{E}[|A_1|] \\
&\quad + (k-3)\mathbb{E}[|A_2|] + \sum_{m=3}^{k-2} (k-m-1)\mathbb{E}[|A_m|] \\
&\quad + k\mathbb{E}[(B_0|] + (k-1)\mathbb{E}[|B_1|] + (k-2)\mathbb{E}[|B_2|] \\
&\quad + \sum_{m=3}^{k-1} (k-m)\mathbb{E}[|B_m|].
\end{aligned}$$

We have

$$\begin{aligned}
\mathbb{E}[|A_m|] &= \sum_{i=1}^{n} \mathbb{P}[v_i \in A_m] \\
&= \sum_{i=1}^{n} p \binom{d_i}{m} p^m (1-p)^{d_i - m} \\
&\leq p^{m+1}(1-p)^{\delta - m} \sum_{i=1}^{n} \binom{d_i}{m} \\
&= p^{m+1}(1-p)^{\delta - m} \widehat{d}_m n
\end{aligned}$$

and

$$\mathbb{E}[|B_m|] = \sum_{i=1}^{n} \mathbb{P}[v_i \in B_m]$$

$$
\begin{aligned}
&= \sum_{i=1}^{n}(1-p)\binom{d_i}{m}p^m(1-p)^{d_i-m} \\
&\leq p^m(1-p)^{\delta-m+1}\sum_{i=1}^{n}\binom{d_i}{m} \\
&= p^m(1-p)^{\delta-m+1}\widehat{d}_m n.
\end{aligned}
$$

The next step is to provide accurate upper bounds for each term of the above bound for $\mathbb{E}[|D|]$. The problem here is that we can do it in many different ways. For example, for the second term of the above bound for $\mathbb{E}[|D|]$, we could use the following upper bound:

$$(k-1)\mathbb{E}[|A_0|] \leq (k-1)p(1-p)e^{-p\mu}n,$$

where

$$\mu = \delta - k + 2.$$

The most difficult part of the proof will be to prove that

$$\mathbb{E}[|D|] \leq pn + e^{-p\mu}n\Psi,$$

where

$$\Psi = \widehat{d}_{k-1} + \widehat{d}_{k-2}.$$

We hope that we will be able to prove this bound. Let us denote

$$f(p) = pn + e^{-p\mu}n\Psi.$$

For $p \in [0,1]$, the function $f(p)$ is minimised at the point $\min\{1, z\}$, where

$$z = \frac{\ln\mu + \ln\Psi}{\mu}.$$

If $z > 1$, then the result easily follows. If $z \leq 1$, then

$$\mathbb{E}[|D|] \leq f(z) = \left(z + \frac{1}{\mu}\right)n = \frac{\ln\mu + \ln\Psi + 1}{\mu}n.$$

Since the expectation is an average value, there exists a particular k-tuple dominating set of order at most $f(z)$, as required.

Any research conjecture must ultimately be proved or disproved. It is important to underline that the disproof of a conjecture does not necessarily mean a 'negative' result — it may result in new conjectures and/or theorems and yield insights into the problem.

It is very unlikely that Conjecture 4.1′ is not true because it naturally generalises the known bounds for the domination number, the double domination number and the triple domination number. However, if it turns out that Conjecture 4.1′ is not true, then we will rectify it and prove the modified version of the conjecture.

4.4 Generalisation of the Conjecture for Parametric Domination

We introduce the parametric domination number, which naturally generalises the classical domination, k-domination, k-tuple domination and k-total domination. It seems that many results for these types of domination can be unified and formulated for the parametric domination number.

More precisely, a set X is called a (k,l)-*dominating set* $(k, l \geq 1)$ if for every vertex v not in X,

$$|N[v] \cap X| \geq k$$

and for every vertex v in X,

$$|N[v] \cap X| \geq l.$$

The minimum cardinality of a (k,l)-dominating set of G is the *parametric domination number* $\gamma_{k,l}(G)$.

It is not difficult to see that $\gamma_{1,1}$ is the domination number, $\gamma_{k,1}$ is the k-domination number, $\gamma_{k,k}$ is the k-tuple domination number (in particular, $\gamma_{2,2}$ is the double domination number and

$\gamma_{3,3}$ is the triple domination number) and $\gamma_{k,k+1}$ is the k-total domination number (in particular, $\gamma_{1,2}$ is the total domination number).

We plan to generalise Conjecture 4.1′ for parametric domination and prove it.

4.5 References

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4.6 Resources and Work Plan

The following table provides a summary of resources required for the project in terms of full economic costs.

Table 4.1: Summary of costs.

Type of Costs	**Sub-type**	**Full Economic Costs** (£)
Directly Allocated Costs	Investigators	4,414
	Estates Costs	440
Directly Incurred Costs	Staff	0
	Travel & Subsistence	1,350
	Other Costs	57
Indirect Costs		3,555
	Total	**9,816**

The duration of the project is 9 months, and the PI is anticipating actively researching approximately one day per week. This time frame is suitable for the project because the PI has many additional commitments (e.g. research, teaching, grant applications and scientific software). Moreover, this type of research usually requires some everyday reflection (between the research days) over possible ideas about how to prove or disprove a particular result or conjecture. Therefore, it would be inappropriate to plan this project over a shorter period of time.

Travel expenses are needed to exchange ideas with British and foreign specialists in the field and to disseminate the work, for example the PI plans to present his results at the *British Combinatorial Conference*, which is a prestigious international conference, and the *Oxford University One-Day Combinatorics Colloquium*. The results of the research will be published in the internationally recognised *Journal of Graph Theory*. Also, he will give a talk and discuss his results at RUTCOR (Rutgers Center for Operations Research, USA) with the intention of establishing collaborative associations. RUTCOR is a well-known international centre for operational research, and a number of researchers in RUTCOR are prominent graph theorists.

To carry out this research, the PI has all the resources and infrastructure at the University of the West of England, Bristol, and therefore does not require additional equipment for this project. The work is genuine and will be done using available infrastructure.

The first stage of the project is to prove the Rautenbach–Volkmann conjecture, and the second stage is to generalise this result for the parametric domination number. Both the first stage and the second stage will last four months. The final third stage will last two months and will be devoted to writing up. The measurable objectives of the project are:

Stage 1: Proof of the Rautenbach–Volkmann conjecture.

Stage 2: Generalisation of the proof for the parametric domination.

Stage 3: Scientific paper submitted to the *Journal of Graph Theory*.

4.7 Reviewers' Reports

This section includes three reviewers' reports [12]. Their comments are in *italics*.

Report 1

Response Due Date: 18 August

Research Quality
The proposed research is moderately interesting (it is at the good end of a generally uninteresting area).

Methodology
Seems feasible – the approach might well work.

Adventure in Research
Random methods are quite standard nowadays. The method might well work — but I would not say that 'a major advance in knowledge would result'.

Assessment of the overall research quality: Unsatisfactory, Adequate, Good, Outstanding
My confidence level: Low, Medium, High

Research Impact
The results, if proved, would be interesting to one part of the graph theory community, namely the domination community — but that is one of the weakest parts of the graph theory community.
Assessment: Low, Limited, Significant, Major
My confidence level: Low, Medium, High

Academic Collaboration
[Crossed over]

Non-Academic Collaboration
[Crossed over]

Dissemination
The usual means are proposed – very sensible.

Assessment of the collaboration and dissemination arrangements: Unsatisfactory, Adequate, Good, Outstanding

Management and Resources
It seems very well thought-out and organised.

Assessment: Unsatisfactory, Adequate, <u>Good</u>, Outstanding
My confidence level: Low, Medium, <u>High</u>

Your Conclusions
The proposal is about 'r-tuple domination numbers', but nowhere is it explained why this is an interesting or worthwhile notion (as opposed to a generalization for its own sake of 'domination number').

The overall quality of the proposal: Unsatisfactory, <u>Adequate</u>, Good, Outstanding

I believe this proposal:
Should not proceed as proposed, <u>Could proceed as proposed</u>, Should proceed

Reviewer Expertise
Graph theory, combinatorics.

Report 2

Response Due Date: 18 August

Research Quality
The research quality is high. Domination is a subject in which there is a lot of interest, and the research proposed is very up-to-date.

Methodology
The methodology is sound, moreover the proposer has a lot of imaginative techniques at his disposal.

Adventure in Research
There is a certain amount of risk in trying to solve a conjecture as the conjecture may turn out not to be true. However, even so, the knowledge accrued will be valuable. In any case, tackling this conjecture does seem to be the natural next step in this particular topic.

Assessment of the overall research quality: Unsatisfactory, Adequate, Good, Outstanding (two options chosen)

My confidence level: Low, Medium, High

Research Impact

The impact on the academic community interested in domination will be high.

Domination does have natural applications outside academic mathematics; whether this particular project will have much impact outside academia I could not say, but quite likely it will have an impact.

Assessment: Low, Limited, Significant, Major

My confidence level: Low, Medium, High

Academic Collaboration

Part of this proposal involves a visit to Rutgers, and contacts there will be very valuable for [the PI], *and for West of England more generally.*

Non-Academic Collaboration

N/A

Dissemination

Dissemination through talks and journal articles is ideal.

Assessment of the collaboration and dissemination arrangements: Unsatisfactory, Adequate, Good, Outstanding

Management and Resources

The timescales and milestones are realistic.
The research objectives do justify the requested resources.

Assessment: Unsatisfactory, Adequate, Good, Outstanding

My confidence level: Low, Medium, High

Your Conclusions

This is a worthwhile project which deserves support.

The overall quality of the proposal: Unsatisfactory, Adequate, Good, Outstanding

I believe this proposal:
Should not proceed as proposed, Could proceed as proposed, Should proceed

Reviewer Expertise
Combinatorics and graph theory.

Report 3

Response Due Date: 18 August

Research Quality
Sound work advancing graph theory. The proposer will attack a neat conjecture of some interest to researchers in the area.

Methodology
Sensible and uncontroversial.

Adventure in Research
Evolutionary research rather than high adventure.

Assessment of the overall research quality: Unsatisfactory, Adequate, Good, Outstanding
My confidence level: Low, Medium, High

Research Impact
The research should be of significant interest to other workers on graph domination and related topics.
Assessment: Low, Limited, Significant, Major
My confidence level: Low, Medium, High

Academic Collaboration
Contacts with Rutgers should be beneficial for the PI.

Non-Academic Collaboration
n.a.

Dissemination
Discussing his work and presenting it at for example the BCC next year, and then publishing it in the Journal of Graph Theory — that all seems a very sensible way to reach the academic community.

Assessment of the collaboration and dissemination arrangements: Unsatisfactory, Adequate, Good, Outstanding

Management and Resources
Have appropriate project management plans been specified, including realistic timescales and milestones? *Yes, all sensible.*
Do the research objectives justify the requested resources? *Yes, for the directly incurred costs.*
Assessment: Unsatisfactory, Adequate, Good, Outstanding
My confidence level: Low, Medium, High

Your Conclusions
This is a perfectly good and well-presented project, though not of the most compelling nature for support.
The overall quality of the proposal: Unsatisfactory, Adequate, Good, Outstanding
I believe this proposal:
Should not proceed as proposed, Could proceed as proposed, Should proceed

Reviewer Expertise
Discrete mathematics and probability.

4.8 PI's Response to Reviewers' Comments

The PI thanks the reviewers for their comments, which are basically very positive. The PI was thrilled to read that the proposed research "is at the good end of a generally uninteresting area".

Concern has been voiced from Reviewer 1 regarding the importance of multiple domination: "The proposal is about 'r-tuple domination numbers', but nowhere is it explained why this is an interesting or worthwhile notion (as opposed to a generalisation for its own sake of 'domination number')". It seems that this reviewer overlooked the reference to two books (in the beginning of Section 4.2) regarding applications of domination theory.

Domination is one of the fundamental concepts in graph theory with various applications in wireless and ad hoc networks, social networks, biological networks and distributed computing. Dominating sets are used as models for facility location problems in operational research. Multiple domination plays an important role too, for example it is used for modelling the optimal location of electric vehicle charging stations in road networks. Also, k- and k-tuple dominating sets are used for balancing efficiency and fault tolerance in wireless sensor networks. Such networks are applied in military applications, for example for detecting moving military objects.

Wireless sensor networks and ad hoc mobile networks are natural examples of applications of multiple domination. A wireless sensor network usually consists of several hundred small autonomous devices to measure some physical parameters. Each device contains a processing unit, a limited memory and power battery as well as a radio transmitter and a receiver for communications with its neighbours. A routing algorithm allows the sensor nodes to self-organise into a wireless sensor network. To maximise the functional lifetime of such a network, it is important to select some sensor nodes to behave as a backbone set to support routing communications in an efficient and fault tolerant way.

The backbone set can be considered as a multiple dominating set in the corresponding underlying graph of the network. Dominating sets of several different kinds have proved to be useful and effective for modelling backbone sets. In the recent

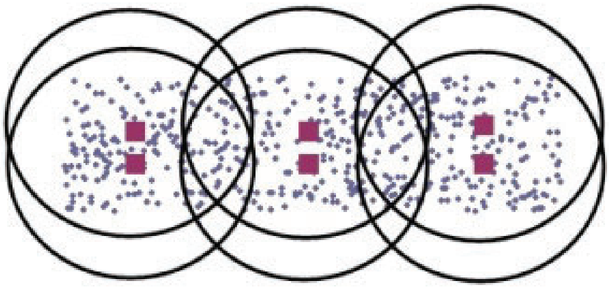

Figure 4.1: A double dominating (backbone) set of size 6 in a wireless sensor network.

literature, particular attention was paid to construction of m-connected k- and k-tuple dominating sets in wireless sensor networks. An example of a double dominating (backbone) set of size 6 in a random wireless sensor network is shown in Figure 4.1, where the sensors are connected by links (not shown) if they are within a certain distance. This set can be used for collecting information from all sensors and sending it to the base station.

4.9 Summary of Key Lessons

Key lessons learnt from the proposal are summarised in Table 4.2. It may be pointed out that making references to practical applications of a theory (e.g. in the beginning of Section 4.2) does not 'work' for reviewers. Such applications must be explicitly described and their importance explained (e.g. see Section 4.8); not doing this in a proposal is a serious mistake. Also, the choice of the topic was wrong — domination theory is not popular in the UK and hence it was labelled as "uninteresting".

Table 4.2: Key Lessons.

Strengths	**Weaknesses**
The methodology is sound, and also sensible and uncontroversial. The proposer has a lot of imaginative techniques at his disposal. The research quality is high. Sound work advancing graph theory. Domination is a subject in which there is a lot of interest, and the research proposed is very up-to-date. The research should be of significant interest to other workers on graph domination and related topics. The impact on the academic community interested in domination will be high; quite likely there will be an impact outside academia. Domination does have natural applications outside academic mathematics. Part of this proposal involves a visit to Rutgers, and contacts there will be very valuable. Dissemination through talks and journal articles is ideal. The timescales and milestones are realistic. The research objectives do justify the requested resources. Management and resources seem very well thought-out and organised. This is a perfectly good and well-presented project.	The proposed research is moderately interesting. Evolutionary research rather than high adventure. The project is not of the most compelling nature for support. It is not the case that 'a major advance in knowledge would result'. The results would be interesting to one part of the graph theory community. Nowhere is it explained why this type of domination is an interesting or worthwhile notion.

One of the reviewers of this book said that “the author’s belligerent tone in the PI’s Response to Reviewers’ Comments sections could annoy a panel”. For example:

- “The PI was thrilled to read that the proposed research ‘is at the good end of a generally uninteresting area’ ”.
- “We would be grateful if you could kindly consider disregarding this report”.

In what follows, the comments made in Sections 1.10, 2.11 and 3.11 will not be repeated in this and similar sections. For instance, the comment in Sections 3.11 about a literature review is relevant for this chapter too.

Chapter 5

Decomposition Methods for the Reconstruction Conjectures

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5.1 Background and Aims

In mathematics, the problem of relations among structures of an object and its subobjects is traditional. The main question here is in what proportion the structure of an object is determined by the structures of its parts? Actually, the question about the possibility of reconstructing the object from its parts is fundamental not only in mathematics, but in other fields of science. A well-known example is the reconstruction of the 3D structure of biomolecules using X-ray crystallography.

To understand all substructures of an object it is often sufficient to know the maximal ones. If the structure under consideration is a graph G, then its maximal substructures may be proper maximal induced subgraphs of G, that is, $G - v$ for all vertices v of G. In this case, the corresponding question about reconstruction is the famous Kelly–Ulam Conjecture (or the Reconstruction Conjecture).

Kelly–Ulam Conjecture [9, 19] *If two graphs with at least three vertices have the same families of maximal induced subgraphs, then these graphs are isomorphic.*

Since every maximal induced subgraph of a graph could be obtained by deleting some vertex of the graph, it is natural to consider the edge analogue of the Kelly–Ulam Conjecture with subgraphs obtained from the graph by deleting one edge. This conjecture, posed by Harary, is called the Edge Reconstruction Conjecture.

Edge Reconstruction Conjecture [6] *If two graphs with at least four edges have the same families of edge-deleted subgraphs, then these graphs are isomorphic.*

The development of graph theory has been strongly influenced by the Reconstruction and Edge Reconstruction Conjectures. Among the names of mathematicians who have been engaged in these conjectures are Bollobás [2], Lovász [10], Nash-Williams [12] and Tutte [16]. An outstanding result, which must be mentioned here, is the fundamental Nash-Williams Lemma dealing with the Edge Reconstruction Conjecture. The lemma has been used by many authors to obtain deep results in the area.

Despite the simplicity of the formulations, the aforementioned reconstruction conjectures have remained open for more than 50 years. This caused some pessimism about the possibility of proving those conjectures by existing methods. For example, Harary in his book [7] wrote: “The reader is urged not to try to settle this conjecture since it appears to be rather difficult”. In the paper [13], the author noted that “even though it is one of the foremost unsolved problems in graph theory, work on it has slowed down, may be due to the general feeling that existing techniques are not likely to lead to a complete solution”.

The aim of this project is to apply decomposition methods to both reconstruction conjectures. Our main decomposition tool is the so-called *operator decomposition* of graphs. This decomposition consists in the representation of graphs as elements of some semigroup and the consideration of a graph in question as a result of action of that semigroup on the set of all graphs. This approach seems to be fruitful because it has shown itself as an effective instrument for problems connected with the notion of isomorphism. In particular, using such a decomposition, the problem of characterisation of unigraphs was completely solved in [17]. Moreover, we are encouraged by the relatively recent proof of the famous Strong Perfect Graph Conjecture. This problem was solved by Chudnovsky, Robertson, Seymour and Thomas using decomposition methods [5]. In fact, they proved that every Berge graph either admits some prescribed decomposition or belongs to the class of basic graphs. Hence, the Strong Perfect Graph Conjecture is true. It may be pointed out here that the class of basic graphs was characterised in terms of forbidden induced subgraphs in [21].

We strongly believe that a similar idea based on decomposition methods could work for the reconstruction conjectures. Thus, the basic aims of the proposed project are as follows:

1. Prove the Reconstruction Conjecture and the Edge Reconstruction Conjecture for substantial classes of graphs using decomposition methods.

2. Find new types of decompositions that can be used for the reconstruction conjectures and prove 'corresponding' theorems.

3. Using our decomposition technique, formulate characterisation conjectures implying reconstructibility or edge reconstructibility of substantial classes of graphs.

It is important to underline that this research proposal meets the funder's mission and strategy because it will:

- Attract a talented post-doctoral researcher.
- Stimulate adventure in developing the new approach.
- Build collaborations between the investigator and the post-doctoral researcher.

5.2 Reconstruction and Decompositions

All graphs considered are finite, undirected, without loops and multiple edges. The vertex and edge sets of a graph G are denoted by $V(G)$ and $E(G)$, respectively. We write $u \sim v$ ($u \not\sim v$) if vertices u and v are adjacent (non-adjacent). For the subsets $U, W \subseteq V(G)$ the notation $U \sim W$ means that $u \sim w$ for all vertices $u \in U$ and $w \in W$; $U \not\sim W$ means that there are no adjacent vertices $u \in U$ and $w \in W$. To shorten notation, we write $u \sim W$ ($u \not\sim W$) instead of $\{u\} \sim W$ ($\{u\} \not\sim W$). The subgraph of G induced by a set $A \subseteq V(G)$ is denoted by $G[A]$.

Let G be a graph and $M \subseteq V(G)$. The set M is called a *module* of G if $v \sim M$ or $v \not\sim M$ for every vertex $v \in V(G) - M$. If M is a module, then $V(G)$ is naturally partitioned into three parts:

$$V(G) = A \cup B \cup M, \quad A \sim M, \quad B \not\sim M, \tag{5.1}$$

where A or B might be empty. The partition (5.1) is *associated* with the module M.

For every graph G, the set $V(G)$, one-vertex subsets of $V(G)$ and the empty set are modules. The module M with $1 < |M| < |VG|$ is called a *non-trivial module*, or a *homogeneous set*, or an *interval.* A graph is *decomposable* if it contains a homogeneous set, otherwise it is called *prime* or *indecomposable.*

Let G be a graph. The collection

$$D = (G_v)_{v \in V(G)}$$

of vertex-deleted subgraphs of the graph G is called a *deck* of G. A graph H with the deck $D' = (H_u)_{u \in V(H)}$ is called a *recon-*

struction of G if there exists a bijection

$$f : V(G) \to V(H)$$

such that $G_v \cong H_{f(v)}$. The graph G is *reconstructible* if it is isomorphic to any of its reconstructions. The famous Kelly–Ulam Conjecture claims that every graph with at least three vertices is reconstructible. The Edge Reconstruction Conjecture, posed by Harary, is formulated analogously. In this case, the deck $D = (G_e)_{e \in E(G)}$ of a graph G consists of subgraphs of G obtained by deleting one edge of G. Note that if a graph is reconstructible, then it is edge reconstructible (see [3]).

We will apply decomposition methods to the reconstruction conjectures using the following basic idea. One of the possible ways to prove that a class of graphs is reconstructible is to define a decomposition of graphs under consideration such that:

(a) A graph is determined by this decomposition up to isomorphism;

(b) The parts of this decomposition and connections among them could be reconstructed from the deck of the graph.

For this purpose, the so-called operator decomposition of graphs may be used. Let us outline the basic concepts and results related to this decomposition.

We consider a *triad* $T = (G, A, B)$, where G is a graph and (A, B) is an ordered partition of the set $V(G)$ into two disjoint subsets (a *bipartition*). The sets A and B are called the *upper* and *lower parts* of the graph G (triad T), respectively (one of the parts may be empty). Let

$$T_i = (G_i, A_i, B_i),\ i = 1, 2$$

be two triads. An isomorphism $\beta : V(G_1) \to V(G_2)$ of the graphs G_1 and G_2 preserving the bipartition (i.e. $\beta(A_1) = A_2$ and $\beta(B_1) = B_2$) is called an *isomorphism* of the triads $\beta : T_1 \to$

T_2. We write $T_1 \cong T_2$ if and only if there exists an isomorphism $\beta : T_1 \to T_2$.

Let us denote the set of all triads (graphs) distinguished up to isomorphism of triads (graphs) by Tr (Gr). The triads from Tr are considered as left operators acting on the set Gr, and the action of the operators is defined by the following formula:

$$(H, A, B)G = G \cup H + \{ax \mid a \in A, x \in V(G)\}. \tag{5.2}$$

Hence, on the set Tr, the action (5.2) induces a binary algebraic operation called the *multiplication of triads*:

$$(G_1, A_1, B_1)(G_2, A_2, B_2) = ((G_1, A_1, B_1)G_2,\ A_1 \cup A_2,\ B_1 \cup B_2). \tag{5.3}$$

Proposition 5.1 *The set Tr is a semigroup with respect to multiplication (5.3). Formula (5.2) determines the action of Tr on Gr. In other words, Tr is a semigroup of operators on Gr, that is,*

$$(T_1T_2)T_3 = T_1(T_2T_3) \quad \textit{and} \quad (T_1T_2)G = T_1(T_2G)$$

for all $T_i \in Tr$, $i = 1, 2, 3$, $G \in Gr$.

A triad T is called *decomposable* if it can be represented as a product of two triads. Otherwise, it is *indecomposable* (or *prime*). It is evident that every triad T can be represented as a product

$$T = T_1T_2 \ldots T_k,\ k \geq 1, \tag{5.4}$$

of indecomposable triads T_i. Such a representation is called a *decomposition of T into indecomposable parts.* An indecomposable part T_i with empty lower (upper) part is called an *A-part* (a *B-part*). A-parts T_i and T_j, $i < j$, are called *non-separable* if every indecomposable part T_k, $i < k < j$, is also an A-part. *Non-separable* B-parts are defined analogously. The most interesting fact about decomposition of triads is the following theorem:

Theorem 5.1 [14] *The decomposition of a triad into indecomposable parts is determined uniquely up to permutation of non-separable A-parts or non-separable B-parts.*

Multiplying all non-separable A-parts as well as all non-separable B-parts in (5.4), one can obtain a *canonical decomposition* of T:

$$T = C_1C_2 \dots C_r, \; r \geq 1. \tag{5.5}$$

Note that (5.5) does not contain non-separable A-parts and non-separable B-parts. The components C_i in decomposition (5.5) are called *canonical parts* of the triad T.

Theorem 5.1 implies the following statement:

Corollary 5.1 *The canonical decomposition of a triad is determined uniquely, that is, two triads $T = C_1C_2 \dots C_r$ and $R = S_1S_2 \dots S_q$ are isomorphic if and only if $r = q$ and $C_i \cong S_i$ for all i.*

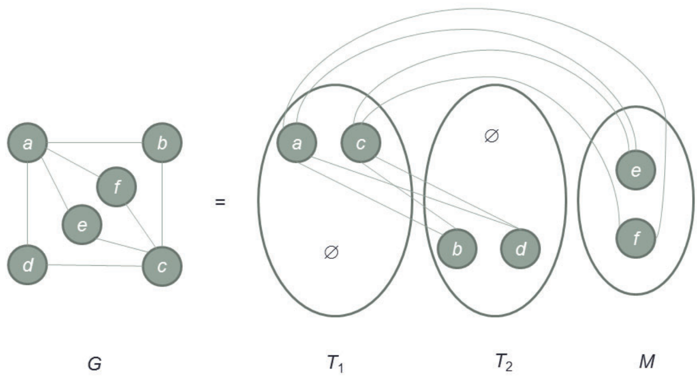

Figure 5.1: Example of graph decomposition.

Now let us turn to graphs. It is obvious that every decomposable graph G can be represented in the form $G = TG_0$ with a non-trivial indecomposable G_0. The part G_0 is called an *indecomposable part* of G, whereas T is called an *operator part*. Further, if (5.4) is the decomposition of T into indecomposable parts, then the representation

$$G = T_1 T_2 \dots T_k G_0 \tag{5.6}$$

is called an *operator decomposition* of G. If (5.5) is a canonical decomposition of T, then the representation

$$G = C_1 C_2 \dots C_r G_0 \tag{5.7}$$

is called a *canonical operator decomposition* of G.

Corollary 5.2 *Every minimal non-trivial module M of a graph G determines a unique canonical operator decomposition.*

5.3 Reconstructible Graphs Admitting Decompositions

Taking into account Corollary 5.2, it is obvious that a graph may have several operator decompositions. However, when solving a particular problem, it may be useful to admit only the decompositions whose operator parts satisfy some conditions essential for the problem.

Let P and Q be two non-empty hereditary classes of graphs. A graph G is called (P,Q)*-split* if there exists a triad (G, A, B) in Tr with $G[A] \in P$, $G[B] \in Q$ ((P,Q)*-triad*). Denote by $(P,Q)Split$ and $(P,Q)Tr$ the sets of all (P,Q)-split graphs and (P,Q)-triads, respectively. The pair (P,Q) is said to be a *closed hereditary pair* if the following conditions hold:

(i) The class P is closed with respect to the join of graphs.

(ii) The class Q is closed with respect to the disjoint union of graphs.

In what follows, (P, Q) is a closed hereditary pair. A graph G is called (P, Q)-*decomposable* if G can be represented in the form

$$G = TH, \quad T \in (P, Q)Tr, \quad H \in Gr. \tag{5.8}$$

Otherwise, G is (P, Q)-*indecomposable.*

If $T \in (P, Q)Tr$ and $T = T_1T_2$, then both T_1 and T_2 belong to $(P, Q)Tr$ as well. Therefore, if the graph H in (5.8) is (P, Q)-decomposable and (5.4) is a decomposition of the triad T into indecomposable parts, then

$$G = T_1T_2 \dots T_kH, \quad T_i \in (P, Q)Tr. \tag{5.9}$$

Decomposition (5.9) is called a (P, Q)-*decomposition* of G. A *canonical* (P, Q)-*decomposition* is defined analogously to the canonical decomposition of the triad T.

Theorem 5.2 (Uniqueness Theorem) *A canonical (P, Q)-decomposition of non-(P, Q)-split graphs is determined uniquely. More precisely, let*

$$G = C_1 \dots C_kH \quad \textit{and} \quad G' = C'_1 \dots C'_lH'$$

be canonical (P, Q)-decompositions of graphs $G, G' \notin (P, Q)Split$, respectively. Then $G \cong G'$ if and only if the following conditions hold:

(i) $k=l$;

(ii) $C_i \cong C'_i$, $i = 1, \dots, k$;

(iii) $H \cong H'$.

The simplest non-trivial case of (P, Q)-decomposition is the case when P is the class of all complete graphs and Q is the class of all empty graphs. This type of decomposition is useful for characterisation and enumeration of different graph classes (e.g. see [11, 17]).

By Theorem 5.2, the (P, Q)-decomposition of non-(P, Q)-split graphs is determined by this decomposition up to isomorphism. Also, it was proved in [14] that the (P, Q)-decomposition of non-(P, Q)-split graphs satisfies the following: the parts of this decomposition and connections among them could be reconstructed from the deck of the graph. So, the following theorem is true.

Theorem 5.3 [14] *Let G be (P, Q)-decomposable but not (P, Q)-split for some closed hereditary pair of classes (P, Q). Then G is reconstructible.*

In other words, without using the notion of (P, Q)-decomposition, this result can be re-formulated as follows:

Theorem 5.4 [14] *Let G be a graph having a homogeneous set M with an associated partition $V(G) = A \cup B \cup M$ such that for some closed hereditary pair of classes (P, Q), we have $G[A] \in P$, $G[B] \in Q$ and $G[M]$ is not (P, Q)-split. Then G is reconstructible.*

This theorem can be used for proving the Reconstruction Conjecture or the Edge Reconstruction Conjecture in the following way. Let us consider the simplest case of a (P, Q)-decomposition when P is the class of complete graphs and Q is the class of empty graphs; such a (P, Q)-decomposition is called a *1-decomposition*. In this case, even the stronger result than the statement of Theorem 5.2 holds: *Every graph (not necessarily non-(P, Q)-split) has a unique 1-decomposition* [18].

Using this property, it was proved in [15] that all 1-decomposable graphs are reconstructible.

Let us consider, for example, the well-known class of split graphs. It could be shown that for every split graph, one of the following conditions holds:

1) G is 1-decomposable.
2) G has some prescribed structure, which allows us to prove the edge reconstructibility of G.

3) For every edge reconstruction H of G, there exist edges $e \in E(G)$ and $w \in E(H)$ such that $G_e \cong H_w$ and the isomorphism $f : G_e \to H_w$ can be extended to the isomorphism between G and H.

Therefore, split graphs are edge reconstructible.

We will apply similar methodology to other classes of graphs. It should be mentioned that there are different types of decomposition, which can be considered as a result of multiplication of elements of some semigroup. In particular, the decompositions used in the proof of the Strong Perfect Graph Conjecture are of this type. Thus, we will develop a number of decompositions, obtain results similar to Theorem 5.2 and, with their help, prove the Reconstruction Conjecture or the Edge Reconstruction Conjecture for different classes of graphs.

5.4 References

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5.5 Resources and Work Plan

The results of this project will be of benefit to the academic and technological organisations interested in graph theory, combinatorial methods and algorithms. The new decomposition technique may be useful to solve various problems associated with graph and network structures including the information and communications technologies.

Table 5.1 provides a summary of resources required for the project in terms of full economic costs.

Table 5.1: Summary of costs.

Type of Costs	**Sub-type**	**Full Economic Costs (£)**
Directly Allocated Costs	Investigators	8,709
	Estates Costs	7,718
Directly Incurred Costs	Staff	34,331
	Travel & Subsistence	700
	Other Costs (PC)	800
Indirect Costs		33,367
	Total	**85,625**

Directly Allocated Costs

The duration of the project is 12 months, and the PI is anticipating actively researching approximately one day per week because of many additional commitments, for example research, teaching, grant applications, scientific software, editorship and supervision of a PhD student. The time frame of 12 months is appropriate for a project of this nature due to the volume of work to be undertaken.

Directly Incurred Costs

Staff

The proposed research requires considerable effort at a high technical level relying on expertise in graph theory. The proposal for a post-doctoral researcher full time for 12 months is, in our view, a cost-effective approach to meeting this requirement. The starting salary of £28,290 is necessary to attract a researcher at the desired level of expertise to ensure project success and high quality research.

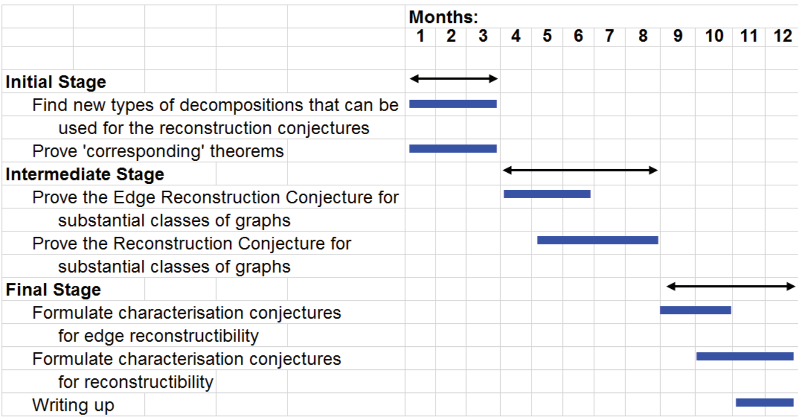

Figure 5.2: Work plan.

Travel and Subsistence

Travel expenses are needed to exchange ideas with British and foreign specialists in the field and to disseminate the work, for example the post-doctoral researcher will take part in the *British Combinatorial Conference*. The results of the research will be published in the internationally recognised *Journal of Graph Theory* or *Discrete Mathematics*.

To carry out this research, the PI has all the resources and infrastructure at the University of the West of England, Bristol, and therefore does not require additional equipment for this project. The work is genuine and will be done using available infrastructure.

Work Plan

The plan of the work is shown in Figure 5.2.

5.6 Reviewers' Reports

This section includes three reviewers' reports [20]. Their comments are in *italics*.

Report 1

Response Due Date: 30 May

Significance and Potential of the Research
The proposed research addresses a very well-known old problem, the reconstruction conjecture for graphs, about which there are numerous partial results, many of great interest in themselves. The approach suggested is novel, but seems likely to yield some new and interesting classes of graphs for which the reconstruction conjecture is true. It is not suggested that their approach will lead to a resolution of the whole conjecture (though I suppose that that is a possibility). The research could well stimulate an upsurge in activity on the conjecture. This novel approach stems from some work by some of the proposer's former colleagues in the Soviet Union, and it would be useful, and would indeed contribute to the UK's world research standing, if it could be extended and developed here.

Assessment: Unsatisfactory, Adequate, Good, Very Good, Outstanding (two options chosen)

My confidence level: Low, Medium, High

Degree of Novelty or Risk

(1) *The research is novel, but the originality of the basic idea has to be shared with some of the proposer's former colleagues in the Soviet Union.*

(2) *The research (if successful) seems likely to stimulate further work in this area, and would be "money well-spent". It is an adventurous and stimulating proposal.*

(3) *Although there is a substantial body of knowledge about vertex- and edge-reconstruction, the approach is entirely novel, and so it should not be thought of as being incremental.*

Assessment: Unsatisfactory, Adequate, Good, Very Good, Outstanding

My confidence level: Low, Medium, High

People and Development

The proposal should contribute to the further development of a good quality mathematician at the start of his/her career.

Assessment: Unsatisfactory, Adequate, Good, Very Good, Outstanding

My confidence level: Low, Medium, High

Collaboration

The likely value would be the continuing collaboration which may well be engendered between the proposer and post-doctoral researcher.

Assessment: Unsatisfactory, Adequate, Good, Very Good, Outstanding

My confidence level: Low, Medium, High

Ability to Deliver the Proposed Research

For such an interesting proposal, a one year time frame may be too short. It would depend very much on how long the post-doctoral researcher takes to familiarize him/herself with all the background, before she/he can start to think constructively about the problem in hand. The proposer, however, clearly has a good notion of how to make progress on the problem.

Assessment: Unsatisfactory, Adequate, Good, Very Good, Outstanding (two options chosen)

My confidence level: Low, Medium, High

Planning and Management

The methodology is original, but seems to be well worked out. The management is simple – that is all that is required – but good. My only concern is that the time requested (one year) may not be enough.

Assessment: Unsatisfactory, Adequate, Good, Very Good, Outstanding (two options chosen)

My confidence level: Low, Medium, High

Resources Requested

Are the resources requested appropriate and justified? *Yes.*

Potential Contribution to Knowledge Transfer

I don't think this has any particular contribution to make to knowledge transfer. In fact, I don't understand what this section is all about.

Assessment: Unsatisfactory, Adequate, Good, Very Good, Outstanding (not chosen)

My confidence level: Low, Medium, High (not chosen)

Your Conclusions

This is an excellent proposal which I wish every success. My only concern is with the time frame, which may not (or may) prove sufficient.

The overall quality of the proposal: Unsatisfactory, Adequate, Good, Very Good, Outstanding

Recommendations
I believe this proposal:
Should not proceed as proposed, Could proceed as proposed, Should proceed

My confidence level: Low, Medium, High

Reviewer Expertise
Combinatorics, Design Theory and Graph Theory.

Report 2

Response Due Date: 30 May

Significance and Potential of the Research
The graph reconstruction conjectures belong to the notoriously hardest problems in combinatorics. They have eluded all attempts for the last 50 years and many researchers now accept that this problem requires fundamentally new insights. Progress is most likely to come from algebra and will probably involve the classification of finite simple groups. Significant progress in this prestigious area will certainly contribute to the standing of UK research and form a valuable knowledge advance for the research community.

In this proposal algebraic decomposition techniques based on the work of Tyshkevich and others are examined. In recent work of these authors it has been shown that the decomposition methods are sufficient to prove reconstructibility for graphs which have particularly simple but still non-trivial properties under decomposition. It would probably be useful to look also at Formann's work on algebraic properties of the finite-type invariants.

The aim of the proposal under review is to extend the decomposition methods to other classes for which the conjectures are open. For instance, it is suggested that the method is sufficient to show that split graphs are edge reconstructible. While this claim still requires some work it is reasonable to expect that this result can be proved. This is the first key step where one *can observe how far the new techniques can be expanded. This does look promising.*

Assessment: Unsatisfactory, Adequate, Good, Very Good, Outstanding

My confidence level: Low, Medium, High

Degree of Novelty or Risk

The standard form of the vertex reconstruction conjecture has proved so intractable that promising new methods deservedly attract immediate interest. The idea of applying factorization methods of this kind to reconstruction is new and therefore this project deserves to be taken further.

If the method is successful to deal with sufficiently large classes of graphs then progress can be quite essential.

Assessment: Unsatisfactory, Adequate, Good, Very Good, Outstanding

My confidence level: Low, Medium, High

People and Development

The subject area is important and to develop expertise at this high level would be beneficial to the community of people working in graph reconstruction theory.

The proposal would introduce a postdoc to advanced techniques in algebraic graph theory and semi-groups. The proposed duration seems entirely adequate for the purpose.

Assessment: Unsatisfactory, Adequate, Good, Very Good, Outstanding

My confidence level: Low, Medium, High

Collaboration

NA (The 'good' rating is subject to the NA comment)

Assessment: Unsatisfactory, Adequate, Good, Very Good, Outstanding

My confidence level: Low, Medium, High

Ability to Deliver the Proposed Research

The proposer is an experienced researcher in graph theory and computation.

Recent progress on the graph decomposition methods which are to be studied in this project give this a good chance to be successful.

Assessment: Unsatisfactory, Adequate, Good, Very Good, Outstanding

My confidence level: Low, Medium, High

Planning and Management

Planning: Fine;

Methodology: Fine, an new idea worth pursuing;

Risk: Is not an issue;

Dissemination: Perfectly adequate.

Assessment: Unsatisfactory, Adequate, Good, Very Good, Outstanding

My confidence level: Low, Medium, High

Resources Requested

Yes, they are appropriate and justified.

Potential Contribution to Knowledge Transfer

This is theoretical research for which applications and exploitation outside pure mathematics is not discernible to me at this moment.

(The 'good' rating is subject to this comment)

Assessment: Unsatisfactory, Adequate, Good, Very Good, Outstanding

My confidence level: Low, Medium, High

Your Conclusions

The proposal has a good chance to make significant progress on the graph reconstruction conjectures. It makes use of recent techniques that have led to interesting new results in related areas.

The project is exploratory in nature — to find out how far the decomposition techniques will extend — and the duration of 12 months seems exactly right for this purpose. The training aspect is good, it could lead a postdoctoral co-worker into an interesting area of combinatorics.

The overall quality of the proposal: Unsatisfactory, Adequate, Good, Very Good, Outstanding

Recommendations
I believe this proposal:
Should not proceed as proposed, Could proceed as proposed, Should proceed

My confidence level: Low, Medium, High

Reviewer Expertise
Algebraic Combinatorics, Graph Theory, Group Theory.

Report 3

Response Due Date: 4 July

Significance and Potential of the Research
This research, if successful, would have a high impact, as the Reconstruction Conjectures are deep and important open questions in discrete mathematics. Indeed, if solved for graphs, the solutions might lead on to solutions of reconstruction questions in many other settings (abelian groups, for example).

Assessment: Unsatisfactory, Adequate, Good, Very Good, Outstanding

My confidence level: Low, Medium, High

Degree of Novelty or Risk
The ideas involved are moderately novel. Using decomposition methods has taken off nicely since the work of Seymour et al. on the strong perfect graph conjecture. The particular ideas here, involving viewing a graph as an operator (via a multiplication defined on 'triads'), are new.

Assessment: Unsatisfactory, Adequate, Good, Very Good, Outstanding

My confidence level: Low, Medium, High

People and Development

The PI will gain in understanding of reconstruction, and the other researcher will gain from looking at the triads and the associated algebraic structure.

Assessment: Unsatisfactory, Adequate, Good, Very Good, Outstanding

My confidence level: Low, Medium, High

Collaboration

The usual academic partnerships (within department, at conferences, etc.) will be useful. It is particularly nice that the Reconstruction Conjectures are known to just about all combinatorialists, so that at any gathering there will be interested people.

Assessment: Unsatisfactory, Adequate, Good, Very Good, Outstanding

My confidence level: Low, Medium, High

Ability to Deliver the Proposed Research

The applicant has written many papers, in several different areas. He has shown his ability to take on baord new ideas and concepts. However, he had not yet proved a 'big' result like Reconstruction, and there is no evidence that he is able to attack successfully a major open problem in combinatorics like this.

Assessment: Unsatisfactory, Adequate, Good, Very Good, Outstanding

My confidence level: Low, Medium, High

Planning and Management

All standard, and very tried and tested – this is the standard model in mathematics.

Assessment: Unsatisfactory, Adequate, Good, Very Good, Outstanding

My confidence level: Low, Medium, High

Resources Requested

Yes, the resources are sensible and entirely justified.

Potential Contribution to Knowledge Transfer

The knowledge transfer upon successful completion of this project would be the usual maths one, namely that other researchers would be interested and would read the paper.

Assessment: Unsatisfactory, Adequate, <u>Good</u>, Very Good, Outstanding

My confidence level: Low, <u>Medium</u>, High

Your Conclusions

I think that the project would be great if successful, but I see no reason to suspect that it would be. In particular, there is no track record of major breakthroughs or deep innovations, which would surely be needed for a project on these very tough open questions.

The overall quality of the proposal: Unsatisfactory, <u>Adequate</u>, Good, Very Good, Outstanding

Recommendations

I believe this proposal:
<u>Should not proceed as proposed</u>, Could proceed as proposed, Should proceed

My confidence level: Low, Medium, <u>High</u>

Reviewer Expertise

Combinatorics, Graph Theory.

5.7 PI's Response to Reviewers' Comments

I would like to thank the reviewers for their comments, which are essentially very positive.

All reviewers have kindly noted that the approach we propose to undertake is both novel and innovative for this area, and success in this project would have a major impact on UK research in this field stimulating further activities. All reviewers have also noted that this research is adventurous because work on the reconstruction conjectures for graphs has thus far provided only partial results.

Concern has been voiced from Reviewer 1 regarding the time frame proposed for this work, although Reviewer 2 specifically mentioned that the time frame is sufficient. We anticipate that the research work can be successfully performed in the time frame because of the calibre of researcher we intend to employ on this project and due to the PI's recognised capability in this area and close supervision of the work.

Two reviewers have expressed concern about the possibility of successfully achieving the aims of the project. The adventurous nature of the project in tackling the reconstruction conjectures can carry with it the risk of only partial success. However, the PI's track record in the area clearly shows an aptitude and capability including many successes, and as all three reviewers note, on successful completion of this research project a significant contribution to UK's world research standing in this field will have been made, instigating a resurgence of interest and research in this important field.

5.8 Summary of Key Lessons

Key lessons learnt from the proposal are summarised in Table 5.2. This is a very strong project with few shortcomings. One of the mistakes was to plan it over just one year, this was discussed in Section 3.11. Also, the project did not promise to completely solve the Reconstruction Conjectures; this could be made crystal clear in the proposal because it appears that Reviewer 3 misunderstood this point. It may be argued that the report of Reviewer 3 is another example of unfair judgement.

Table 5.2: Key Lessons.

Strengths	**Weaknesses**
The approach suggested is novel, but seems likely to yield some new and interesting classes of graphs for which the conjecture is true.	The PI had not yet proved a 'big' result like Reconstruction.
Significant progress in this prestigious area will certainly contribute to the standing of UK research and form a valuable knowledge advance for the research community.	There is no evidence that he is able to attack successfully a major open problem in combinatorics like this.
The research could well stimulate an upsurge in activity on the conjecture. If the method is successful to deal with sufficiently large classes of graphs, then progress can be quite essential.	I think that the project would be great if successful, but I see no reason to suspect that it would be.
The particular ideas here, involving viewing a graph as an operator, are new. This technique does look promising. The methodology is original, but seems to be well worked out. The approach is entirely novel, and so it should not be thought of as being incremental.	In particular, there is no track record of major breakthroughs or deep innovations, which would surely be needed for a project on these very tough open questions.
The proposed duration seems entirely adequate for the purpose.	A one year time frame may be too short.
This research, if successful, would have a high impact.	
The research seems likely to stimulate further work in this area, and would be "money well-spent".	
The proposal should contribute to the further development of a good quality mathematician.	
The likely value would be the continuing collaboration which may well be engendered between the proposer and post-doctoral researcher.	
Planning and methodology are fine. Dissemination is perfectly adequate. The resources are sensible and entirely justified. The resources requested are appropriate and justified.	

Chapter 6

Embedding Graphs on Topological Surfaces

V. Zverovich and A. Gagarin[†]

[†]Cardiff University, UK

6.1 Aims and Background

One of the classical problems in graph theory and combinatorics is the problem of embedding graphs on surfaces. Given a graph, it is important to have an algorithm to decide whether it is possible to draw the graph on a surface without any edge crossings. This problem is related to VLSI design problems, in which an embedding can be used to facilitate an appropriate circuit representation. It can also be addressed from the viewpoint of hereditary classes of graphs because a class of graphs embeddable on a given surface contains all minors of members of this class.

The structural characterisation of planar graphs through excluded subdivisions of K_5 and $K_{3,3}$ by Kuratowski [25] is one of the fundamental results of modern graph theory. Moreover, Wagner [38] proved that a graph G is non-planar if and only if it contains $K_{3,3}$ or K_5 as a minor. These results imply that a planar graph can be detected by a polynomial-time algorithm. However, efficient computer-implementable algorithms that were capable of recognising whether a graph is planar appeared much later. For example, the popular linear-time planarity testing algorithm by Hopcroft and Tarjan [19] appeared only in 1974. In general, the problem of finding the genus of a graph was proved to be #P-complete by Thomassen [35] in 1989.

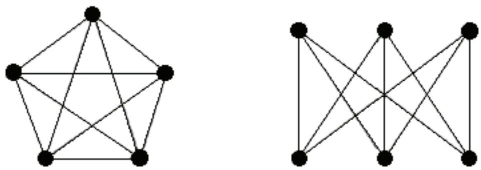

Figure 6.1: The graphs K_5 (left) and $K_{3,3}$ (right).

Kuratowski's result was generalised for an arbitrary topological surface — for any surface of fixed genus, the number of excluded minors (and subdivisions) is finite. A proof, specifically for orientable surfaces, was independently given by Vollmerhaus [37] as well as Bodendiek and Wagner [4]. For nonorientable surfaces, this was proved by Archdeacon and Huneke [2]. The Robertson and Seymour theory [34] provided a proof for an arbitrary fixed surface. This implies that for a given topological surface, the question of whether a graph is embeddable on the surface may be decided by a polynomial-time algorithm. Moreover, Mohar [27] announced a series of linear-time algorithms to answer the question. Unfortunately, these linear-time algorithms appear to be very challenging to implement and are of theoretical, rather than practical, interest.

The projective plane and the torus are topological surfaces closest to the plane. One of the known implemented efficient algorithms is the projective-planarity testing algorithm by Myrvold and Roth [32]. This algorithm runs in $O(n^2)$-time. Gagarin and Kocay [8] devised an implementable linear-time algorithm to detect a projective-planar graph; this algorithm can be regarded as a generalisation of the Hopcroft–Tarjan planarity testing algorithm [19]. However, its implementation is still not complete because it requires complex computer data structures. A simpler version of this linear-time projective-planarity testing algorithm was presented in [24], but it runs in $O(n^2)$-time. Implementable polynomial-time algorithms for other surfaces of non-zero genus are still unknown. However, Gagarin and Kocay [9] devised a simple linear-time algorithm that detects some toroidal graphs containing a K_5-subdivision, otherwise indicating that the input graph is not toroidal or finding a $K_{3,3}$-subdivision in the graph. This partial toroidality testing algorithm is based on structural properties and decompositions of graphs containing a K_5-subdivision as a subgraph and it is a reduction to the aforementioned linear-time planarity testing.

Considering topological surfaces of non-zero genus, the precise lists of all forbidden minors and subdivisions are only known for the projective plane — there are 103 forbidden subdivisions and 35 forbidden minors. Glover, Huneke and Wang [18] first showed that these are obstructions for the projective-planar graphs, and Archdeacon [1] later proved that their list is complete. The precise complete lists of obstructions for the torus (both forbidden minors and subdivisions) are still unknown. However, Gagarin, Myrvold and Chambers [16] found that 4 forbidden minors and 11 forbidden subdivisions are sufficient for toroidal graphs containing no $K_{3,3}$-subdivision.

The results in [11, 12, 14] explained the structure of projective-planar and toroidal $K_{3,3}$-subdivision-free graphs in terms of substitutions of two-pole planar networks into edges of some special non-planar graphs called *projective-planar* and *toroidal cores*, respectively. The characterisations in [11, 12, 14] allow us to count labelled and unlabelled projective-planar and toroidal graphs without $K_{3,3}$-subdivisions by using counting techniques similar to Walsh's methods [39, 40].

The basic aims of the proposed project are:

(i) To finish the implementation of the linear-time projective-planarity testing algorithm from [8] and, if possible, to simplify the algorithm.

(ii) To devise a computer implementable polynomial-time algorithm to detect a toroidal graph containing a subdivision of $K_{3,3}$ or to solve the toroidality testing problem for tractable subclasses of toroidal graphs. For example, we plan to consider graphs containing no K_5-minors first and then graphs containing both minors $K_{3,3}$ and K_5.

(iii) To devise and implement efficient algorithms similar to those in [8, 9] for the Klein bottle (testing embeddability on the Klein bottle by avoiding $K_{3,3}$-subdivisions).

(iv) To find forbidden minors and subdivisions for different subclasses of toroidal graphs and to try to find new types of structural characterisations for toroidal graphs in subclasses.

(v) To describe the projective-planar and/or toroidal graphs with prescribed properties, for example graphs having a unique embedding on the projective plane and/or the torus.

(vi) To enumerate some subclasses of graphs embeddable on the aforementioned surfaces. Labelled and unlabelled enumerations will be considered separately.

(vii) To carry out a feasibility study into generalisations of the algorithmic results for graphs embeddable on surfaces of higher genera, for example on the Klein bottle and/or the two-holed torus.

6.2 Devising an Implementable Polynomial-Time Toroidality Testing Algorithm

The history of devising correct and implementable graph embedding algorithms is very complicated. The original version of the Hopcroft–Tarjan planarity testing algorithm [19] does not find some planar graph embeddings. This was discovered and fixed by Kocay [23], who used a refinement procedure for the ordered adjacency lists of graph vertices, initially obtained by using the depth-first search procedure. The very first polynomial-time algorithm for the projective plane [33] is known to be incorrect [29].

More recently, Myrvold and Kocay [30] showed that polynomial-time algorithms in [6, 7] for the torus and other surfaces are not correct, and they normally would require exponential running time for some graph inputs. The linear-time algorithms by Mohar et al. [26, 20, 27] are not known to be implemented to run on a computer and they appear to be of theoretical, rather than practical, interest. The descriptions in [20, 26, 27] are missing too many details necessary for an implementation. However, as mentioned in [42], the description in [26] gives some insights into the problem.

Note that the *implementability* of an algorithm means that:

- It is evident which data structures to use and how to implement the algorithm as a computer programming code, so that it is available as an executable computer code (ready to use).
- Its executable files (application) are readily available via the Internet and can be independently tested for correctness.
- Preferably, the algorithm runs in polynomial time (efficient).
- It works reasonably fast for inputs of a reasonably large size.

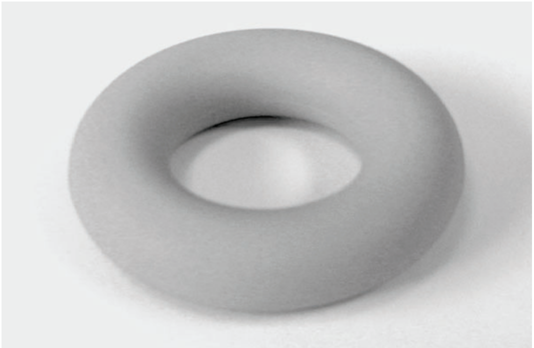

Figure 6.2: The torus.

The research in [8, 9, 10] focused on devising correct implementable linear-time algorithms to determine if there exists an embedding of a graph on the projective plane and/or the torus. These two topological surfaces are closest to the plane. Known algorithms for these surfaces (e.g. [20, 26, 32]) often begin with the Kuratowski subgraph K_5 or $K_{3,3}$ in a graph G and try to extend one of the embeddings of K_5 or $K_{3,3}$ to an embedding of the whole graph G on the corresponding surface.

In [9], certain types of graphs containing a subdivision of K_5 were completely characterised and linear-time algorithms to decide on their embeddability on the projective plane and/or the torus were provided. Given a non-planar graph G with a subdivision of K_5 as a subgraph, we can either transform the K_5-subdivision into a $K_{3,3}$-subdivision or partition the vertices of $G \backslash K_5$ into disjoint sets. As a result, a linear-time algorithm is obtained, which either reduces a projective-planarity checking (or toroidality checking) algorithm to a small constant number of planarity tests or finds a subdivision of $K_{3,3}$ in the input graph G. This significantly simplifies algorithms presented in [20, 26] and [32]. Then, it is necessary to consider only the embeddings of a $K_{3,3}$-subdivision on the projective plane or the torus, which are much less numerous than the corresponding embeddings of K_5. Notice that in some particular cases, toroidal graphs found by the torus embedding algorithm in [9] may contain a $K_{3,3}$-subdivision too. This provides some additional insight into the problem. Therefore, we hope to simplify and modify the algorithm in [9] for the torus, so that it will detect a larger class of toroidal graphs. This may lead to additional ideas and directions to deal with graphs containing a $K_{3,3}$-subdivision on the torus in general.

In [8], the structure of the unique embedding of a $K_{3,3}$-subdivision in a given graph on the projective plane was investigated with respect to other parts of the graph. The structure of the embedding combined with some ideas of the Hopcroft–Tarjan planarity testing algorithm provide a projective-planarity testing algorithm for graphs containing a subdivision of $K_{3,3}$. As a result, this gives an implementable linear-time algorithm for determining whether a graph is projective planar or not, which is described in [8]. However, the projective-planarity testing algorithm in [8] is quite complicated and involved with details. We hope that programming and analysing this algorithm will lead to a simpler linear-time algorithm for the projective plane and give additional ideas to work with graphs containing $K_{3,3}$-subdivisions on the torus.

The results of [5] suggest an efficient method to compute the orientable genus for a graph embedded on the projective plane. Our projective-planarity testing algorithm can be used as a preliminary step in the approach of [5] to compute the orientable genus of projective-planar graphs.

The structural and algorithmic results of Kézdy and McGuinness [21] suggest a possibility to treat graphs containing a $K_{3,3}$-subdivision and avoiding K_5-minors by analogy with [9]. Therefore, we will try to generalise the approach of [9] for graphs containing $K_{3,3}$-subdivisions. This should lead to the last stage of the toroidality testing algorithm that would consider graphs containing both minors $K_{3,3}$ and K_5.

Figure 6.3: The Klein bottle.

Thus, the research will follow the five main steps:

1. Efficient use and development of computer data structures to complete the implementation of the linear-time projective-planarity testing algorithm from [8].

2. Simplifying and developing ideas used for the partial toroidality testing algorithm in [9] and for the projective-planarity testing algorithm in [8]. Carrying out research into graph structures embeddable on the Klein bottle by analogy with [8] and [9].

3. Devising an implementable polynomial-time algorithm to decide on embeddability of graphs containing a $K_{3,3}$-subdivision and avoiding K_5-minors on the torus. Solving the graph embeddability problem for the torus in well-defined subclasses of graphs.

4. Devising an implementable polynomial-time algorithm to decide on embeddability of graphs containing both minors $K_{3,3}$ and K_5 on the torus.

5. Feasibility study into possible generalisations of the algorithmic results for the Klein bottle, the two-holed torus and surfaces of higher genera.

6.3 Structural Properties of Graphs Embeddable on Surfaces

A graph may have many different embeddings on a particular topological surface. It is interesting to know when embeddings are pairwise distinct and how to distinguish them. Each embedding can be represented combinatorially as a *rotation system*, which is a set of cyclically ordered adjacency lists of the graph vertices. When a nonorientable topological surface is used, a rotation system also includes edge signatures. The *signature* of an edge is $+1$ or -1: it is negative when the edge goes 'over the boundary' and positive otherwise.

Two embeddings of a graph on a surface are said to be *isomorphic* if there exists a one-to-one mapping of the vertices preserving the cyclically ordered adjacency lists. Whitney's theorem [41] implies that a 3-connected graph can have at most one planar embedding (up to orientation of the plane). As the results in [10] show, there may be many 2-cell embeddings of a 3-connected graph on the torus. The book of Mohar and Thomassen [28] contains some interesting information about properties of embeddings and possible analogues of Whitney's theorem for the torus.

We will try to find an analogue of Whitney's theorem for the projective plane and the torus. In other words, we will try to describe all graphs having a unique embedding on the projective plane and/or the torus. Whitney's theorem can be proved by using non-separating cycles of a graph (e.g. see Theorem 12.20 in [24]). Therefore, we wonder what are equivalents of the non-separating cycles on other surfaces.

In general, it is interesting to know how to obtain all possible 2-cell embeddings of a graph on a given surface and how many different embeddings exist. As described in [10], we can distinguish orientable and nonorientable embeddings on the orientable surfaces. A regular embedding provides a tiling of the plane. We are interested in possible classifications of the embeddings and their characterisations. Also, it is interesting to consider dual graphs and automorphism groups of the embeddings.

Thus, the next steps of this research will be devoted to developing methods and algorithms to construct and distinguish all embeddings of a particular graph on a given topological surface and to analyse properties of the embeddings. Characterisations by forbidden minors and subdivisions should also be considered at this stage of research. As soon as we have enough structural information, it might be possible to completely characterise a particular hereditary class of graphs in terms of forbidden substructures; the results of [15] and [16] can serve as initial examples.

6.4 Enumeration and Automorphism Groups

The results of Gagarin, Labelle and Leroux (e.g. see [11, 12, 14]) describe counting of projective-planar and toroidal graphs without $K_{3,3}$-subdivisions. The results of [9] provide a criterion of projective planarity for graphs without $K_{3,3}$-subdivisions and give some ideas about the structure of toroidal graphs without $K_{3,3}$. The algorithmic results of [9] are based on a decomposition into planar components. By using enumeration techniques similar to Walsh's methods [39] and results for 2-connected planar graphs from [3], it is possible to devise methods to count labelled projective-planar and toroidal graphs without a $K_{3,3}$-subdivision (see [11, 12, 14]). Notice that structural results of [12, 14] are based on a refinement of the algorithmic and structural results of [9] and they also provide an implementable linear-time algorithm to detect a $K_{3,3}$-subdivision-free toroidal graph.

A much more subtle and complicated problem of counting isomorphism classes of $K_{3,3}$-subdivision-free projective-planar and toroidal graphs (i.e. unlabelled graphs from the corresponding classes) is solved in [13, 14]. The counting technique used in [13, 14] is based on the counting technique of Walsh [40] and on counting unlabelled 2- and 3-connected planar graphs. Notice that unlabelled enumeration of planar graphs is still in need of efficient methods and approaches.

Future research in this area can be focused on developing methods to count graph embeddings (i.e. maps without loops and multiple edges) on the projective plane and the torus and to count the projective-planar and toroidal graphs containing a $K_{3,3}$-subdivision. The surfaces of higher genera can be considered later.

6.5 Beneficiaries, Applications and Dissemination

The results of this project will be of benefit to the academic and technological organisations interested in computer science, graph theory, combinatorial methods and algorithms. The new results may be useful for addressing various problems associated with graph and network structures including the information and communications technologies, VLSI design and statistical mechanics.

A graph embedding can be used to design a VLSI layout. Given a VLSI circuit to design, we can represent its elements and wire connections by vertices and edges of a graph. Since connections between elements should not cross, we are interested in a drawing of the graph without edge crossings (i.e. an embedding). In practice, the situation can be more complicated because some ‘bridges’ are allowed, resulting in various generalisations. This provides a practical motivation to obtain a graph embedding with particular properties. Graph embeddings are also used for modelling in statistical mechanics — particles interacting in space can be considered as moving and interacting on a topological surface (e.g. on the two-holed torus). The well-known software *Groups & Graphs* [22] is in need of algorithms detecting embeddability of a given graph on a fixed surface and providing embeddings themselves. The enumerative problems and methods are motivated by problems of classification and enumeration of molecular structures in chemistry, and the corresponding techniques can be used for creation of molecular catalogues.

An implementable polynomial-time algorithm for the torus will be a very important tool for many researchers working in graph theory and related fields. The algorithm is supposed to be implemented in two operating systems. The Mac OS X version of the algorithm will be incorporated into the software *Groups & Graphs*, while another version will be implemented as a Windows application. Both versions will be available via the Internet.

In order to reach the community of interested researchers, we will publish our results in internationally recognised journals such as *Discrete Applied Mathematics, Discrete Mathematics, Journal of Graph Algorithms and Applications* and *Ars Combinatoria*. We also plan to attend several scientific workshops, meetings and conferences to present our work and to obtain feedback from the scientific community. For example, we intend to take part in the *British Combinatorial Conference* and the *International Workshop on Graph-Theoretic Concepts in Computer Science.*

6.6 Programme of Work

The proposed research plan is focused on design and analysis of graph embedding algorithms, graph embeddings themselves and related areas. Both the PI and the RF have necessary expertise in order to successfully complete the project. The PI is particularly strong in structural properties of hereditary classes and developing scientific software, whereas the RF has excellent expertise in toroidal and projective-planar graphs as well as in programming languages.

We plan to accomplish the main steps of the project within two years. The initial work for the project will consist in encoding and analysing some of the known algorithms similar to those described in [8, 9, 23] and [24]. However, some pure theoretical work will be done at this stage too. For example, we will work on a torus embedding algorithm for graphs avoiding K_5-minors by analysing the approach of [21]. This initial stage of the project can take up to seven months.

The main stage of the research will consist in devising and encoding a polynomial-time algorithm to detect toroidal graphs. Note that the algorithm will be implemented in two operating systems: Windows XP and Mac OS X. This stage will be the most complicated and subtle part of the research and it may require tricky scientific programming. We hope to complete this stage during a nine-month period.

The next stage of the project will be concentrated on analysing structural results obtained during the first two steps. We will try to generalise our results and ideas for other surfaces and consider the corresponding structural and enumerative problems. This can take as long as eight months.

The proposed project requires high-performance computers. The data structures and the algorithmic methodology for graphs of Gibbons [17] and Kocay and Kreher [24] will be followed. The linear-time planarity testing algorithm of the software *Groups & Graphs* will be used to detect non-planar graphs. The planarity testing algorithm of *Groups & Graphs* returns a subdivision of K_5 or $K_{3,3}$ in a non-planar graph, which will be used in our project. Since the full version of *Groups & Graphs* was developed for Apple Macintosh computers, the main programming and computational work should be performed on these machines. We also need a high-performance computer with Windows XP for testing and implementing our algorithms. The computer programming will require corresponding software necessary to write and compile the codes.

A part of the funding is required for a PhD student, who is expected to have a good knowledge of graph theory and object-oriented programming. The proposed research plan is based on implementing and devising graph embedding algorithms for several topological surfaces (the projective plane, the torus and the Klein bottle). The research will also require pure graph-theoretic studies. It is expected that new algorithms and methods will be incorporated into the existing software *Groups & Graphs*. Thus, the research student is expected to make a meaningful contribution both to implementing algorithms in the C language and analysing graph structures for the design of efficient embedding algorithms.

The programme of study for the PhD student has been carefully thought over and discussed. The first six months of PhD studies will be spent with the student familiarising her/himself with the known approaches for implementable and theoretical graph embedding algorithms and implementing and simplifying the linear-time projective-planarity testing algorithm. This stage will also require a good understanding of computer data structures and methods necessary to implement efficient graph embedding algorithms. In the next six–eight months, the student will thoroughly investigate graph structures and approaches for efficient toroidality testing algorithms and will work on design and implementation of some partial toroidality testing algorithms (e.g. for graphs without K_5-minors).

During the second year, the student will be involved with design and implementation of a general toroidality testing algorithm and, possibly, embedding algorithms for some other surfaces. Structural and enumerative problems can be considered at this stage too. This will enable the student to learn many things about methods for an efficient algorithm design. In the first part of the final year, before writing up, the student will be involved with implementation of algorithms in an existing computer system. This part of the programme is optional for the student and strongly depends on the student's progress in the previous stages.

The specification of a training plan is a formal part of the student registration process at UWE, Bristol. Training in research methods and issues is provided locally by the Faculty of Environment and Technology and, in generic and transferable skills, by the University's Centre for Research, Innovation and Graduate Studies. Progress against the training plan is monitored through a student's progression examination (normally at the end of the first year) and throughout a student's programme. The student will also be encouraged to attend relevant workshops and conferences.

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6.8 Resources

Table 6.1 provides a summary of resources required for the project in terms of full economic costs.

Directly Allocated Costs

The duration of the project is 36 months, and the PI is anticipating actively researching approximately one day per week. This time frame is suitable for the project because the PI has many additional commitments (e.g. research, teaching, grant applications and scientific software). Moreover, this type of research usually requires some everyday reflection (between the research

Table 6.1: Summary of costs.

Type of Costs	**Sub-type**	**Full Economic Costs (£)**
Directly Allocated Costs	Investigators	22,817
	Estates Costs	10,922
Directly Incurred Costs	Staff	67,226
	Travel & Subsistence	4,510
	Other Costs (PCs)	3,457
Indirect Costs		88,162
PhD Student		47,334
	Total	**244,428**

days) over possible ideas about how to advance the project. Therefore, it would be inappropriate to plan this project over a shorter period of time.

Directly Incurred Costs

Staff
The proposed research requires considerable effort at a high technical level relying on expertise in graph theory and computer programming. The proposal for an RF for two years is, in our view, a cost-effective approach to meeting these requirements. The starting salary matches the RF's outstanding education, experience and sound research.

Travel and Subsistence
Travel expenses are needed to exchange ideas with British and foreign specialists in the field and to disseminate the work. For example, we will present our results at the *British Combinatorial Conference*, *International Workshop on Graph-Theoretic Concepts in Computer Science* and attend the *Oxford University One-Day Combinatorics Colloquium* (or similar meeting if

not in Oxford). The results of the research will be published in internationally recognised journals (e.g. *Discrete Mathematics*). The RF plans to visit the University of Manitoba to give a talk and have consultations with the principal developer of the software *Groups & Graphs*, who is a recognised specialist in the design and implementation of embedding algorithms. Moreover, it will be necessary to discuss practical steps for incorporating the developed algorithms into this software.

Other Incurred Costs

Two high-performance computers and C++ software will be essentially required to carry out the project because the algorithms must be implemented in two operating systems: Windows XP and Mac OS X. The computers must have configurations necessary to carry out the project taking into account that it suggests considerable efforts in applied practical algorithmic work and code compilation and testing.

PhD Student

A part of the funding is required for a PhD student, who is expected to have a good knowledge of graph theory and object-oriented programming. The programme of study for the PhD student and the specification of a training plan are described in Section 6.6.

To carry out this research, the PI has all the resources and infrastructure at the University of the West of England, Bristol, and therefore does not require additional equipment for this project. The work is genuine and will be done using available infrastructure.

6.9 Reviewers' Reports

This section includes three reviewers' reports [36]. Their comments are in *italics*.

Report 1

Response Due Date: 7 February

Research Quality
The quality is high.

Methodology
It is interesting to see the mistakes made in the past in Section 6.2 (at the beginning) and a description of the difficulties involved. [The RF] *has co-operated with one of the leading authorities (* [name]*) in this area. The methodology is sound.*

Adventure in Research
This research is more incremental than risky. It answers a need and is essential for further advance in this area.

Assessment of the overall research quality: Unsatisfactory, Adequate, Good, Outstanding (two options chosen)

My confidence level: Low, Medium, High (two options chosen)

Research Impact
The results will be of considerable interest to the academic community. It is quite possible it will have a major impact in the non-academic sector — but I am not well-qualified to judge that.

Assessment: Low, Limited, Significant, Major

My confidence level: Low, Medium, High

Academic Collaboration
[Empty]

Non-Academic Collaboration
[Empty]

Dissemination
The dissemination arrangements are very good — publication in journals and improvement to widely available computer packages.

Assessment of the collaboration and dissemination arrangements: Unsatisfactory, Adequate, Good, Outstanding

Management and Resources

Have appropriate project management plans been specified, including realistic timescales and milestones? *The management plans are good.*

Do the research objectives justify the requested resources? *They do justify the requested resources.*

Assessment: Unsatisfactory, Adequate, Good, Outstanding

My confidence level: Low, Medium, High

Your Conclusions

The high standards needed to measure up to the proposer's definition of "Implementability of an algorithm" *in Section 6.2 bode well for very useable algorithms to come out of this research.*

The overall quality of the proposal: Unsatisfactory, Adequate, Good, Outstanding

I believe this proposal:

Should not proceed as proposed, Could proceed as proposed, Should proceed

Reviewer Expertise

Combinatorics, graph theory, design theory.

Report 2

Response Due Date: 7 February

Research Quality

This is a high quality research project that will continue and extend recent interesting work of the proposer and others.

Methodology

The proposal contains a detailed discussion of the scientific methodology. It is clear that the expertise of the PI and RA will allow them to make progress towards their goals.

Adventure in Research

The project builds on a body of existing work. The degree of risk is quite small, but there is a need for the advances in knowledge that can be expected to be gained.

Assessment of the overall research quality: Unsatisfactory, Adequate, Good, Outstanding

My confidence level: Low, Medium, High

Research Impact

The primary interest will be for other academic researchers, both theorists and those interested in mathematical software. It is possible that there will also be benefits for those working on technological applications such as VLSI circuit design.

Assessment: Low, Limited, Significant, Major

My confidence level: Low, Medium, High

Academic Collaboration

The collaboration with [name], *the developer of Groups and Graphs, should yield considerable benefits to the development of the software.*

Non-Academic Collaboration

n/a

Dissemination

The plans for dissemination are standard for a project of this kind.

Assessment of the collaboration and dissemination arrangements: Unsatisfactory, Adequate, Good, Outstanding

Management and Resources

If the proposal were only for the PI and RA, then the plans would be excellent. What is unclear though, is how the work of the PhD student and RA will fit together. As a period of training is needed, the project will be well under way before the student is able to fully participate. My concern is whether he *will be able to have full "ownership" of a part of the project as will be required for him to be awarded a doctorate.*

The requests are reasonable. In fact, the travel plans are modest for a project of this duration.

Assessment: Unsatisfactory, Adequate, Good, Outstanding

My confidence level: Low, Medium, High

Your Conclusions

As suggested above, without the inclusion of the studentship, I would have recommended the proposal without reservation. As it is, I have slight doubts about whether the different parts of the project fit together.

The overall quality of the proposal: Unsatisfactory, Adequate, Good, Outstanding

I believe this proposal:
Should not proceed as proposed, Could proceed as proposed, Should proceed

Reviewer Expertise

Graph theory.

Report 3

Response Due Date: 7 February

Research Quality

This proposal is focused on the design and implementation of efficient algorithms for the embedding of graphs on surfaces, together with associated structural issues in graph theory. The proposal could be viewed as being on the boundary of mathematics and computer science.

Graph embedding problems have a long history, and have attracted a good deal of attention over the years, mainly from a theoretical standpoint. The proposal is primarily concerned with graphs embeddable on the torus, in particular with designing and implementing practical algorithms for embedding subclasses of *such graphs. These are challenging, if rather specialised, questions in graph theory.*

Progress in these directions would be interesting and the availability of implementations of efficient algorithms would be worthwhile. On the other hand, no major breakthroughs are envisaged, expected progress being of an incremental nature, and it is questionable whether the research will achieve a major advance towards a full understanding of toroidal graphs.

It could be argued that the part of the proposal that relates to implementation of existing algorithms is not high quality research that is worthy of special funding and that will lead to significant publications. This is particularly true when the algorithms are overwhelmingly of theoretical interest.

Methodology

Both the principal investigator and the named researcher have strong records of research in the relevant fields and are well equipped to make significant further contributions. There seems to be a reasonable prospect that they will be able to build on existing work of [the RF] *and others, as outlined in Section 6.2 of the proposal, to devise, for example, efficient algorithms to detect larger classes of toroidal graphs.*

Adventure in Research

There is inevitably some risk associated with this kind of research — risk that relevant structural results and efficient algorithms turn out to be more elusive than expected, and the achievements fall short of what had been hoped. It is perhaps not reasonable to expect major advances in knowledge from such a project, incremental advance is more likely. The proposal is not particularly 'adventurous', more a continuation of lines of research that have met with some success in the past — but lack of adventure need not be viewed as a shortcoming.

Assessment of the overall research quality: Unsatisfactory, Adequate, <u>Good</u>, Outstanding

My confidence level: Low, <u>Medium</u>, High

Research Impact

Results accruing from this research will be of interest to those working in the immediate area, and in structural and algorithmic graph theory in general. Any impact outside these areas will be very limited, and it seems unlikely that the work could have any impact at all in the industrial or non-academic sector. This is pure mathematical research, and should be judged on that basis, not in the light of imagined applications.

Assessment: Low, <u>Limited</u>, Significant, Major

My confidence level: Low, <u>Medium</u>, High

Academic Collaboration

Some limited collaboration is proposed with the University of Manitoba, at least involving one visit to that institution. This seems appropriate, given the obviously successful working relationship between [the RF] *and* [researcher] *(at Manitoba), and the relevant expertise of the latter.*

Non-Academic Collaboration

Not applicable.

Dissemination

The dissemination arrangements seem broadly as one would expect, though perhaps in some respects lacking in ambition. One might hope that research results arising from a funded project of this kind would merit presentation at higher profile meetings than, for example, a one-day meeting at Oxford/UCL, and indeed would merit submission to one or more of the leading international conferences in the field.

Assessment of the collaboration and dissemination arrangements: Unsatisfactory, <u>Adequate</u>, Good, Outstanding

Management and Resources

The project management plans are somewhat vague, though this is not unusual in work of this kind, where the progress of the project depends on so many uncertainties.

It does not seem to me that a convincing case has been made for a PhD student on this project. The precise role of the student has not been clearly spelt out.

The objectives of this proposal convey the feel of a two year, rather than a three year project, perhaps recognised by the suggested duration of the post doc position.

Assessment: Unsatisfactory, <u>Adequate</u>, Good, Outstanding

My confidence level: Low, <u>Medium</u>, High

Your Conclusions

Overall, the proposed research has a highly specialised feel, and although the researchers have a good track record in the area, and positive results could be expected, the breadth of the impact of such results would be limited.

The overall quality of the proposal: Unsatisfactory, Adequate, <u>Good</u>, Outstanding

I believe this proposal:

Should not proceed as proposed, <u>Could proceed as proposed</u>, Should proceed

Reviewer Expertise

Design and analysis of algorithms.

6.10 PI's Response to Reviewers' Comments

We are very pleased with the reviewers' reports, which are basically very positive.

Reviewer 2

If the proposal were only for the PI and RA, then the plans would be excellent. What is unclear though, is how the work of the PhD student and RA will fit together... without the inclusion of the studentship, I would have recommended the proposal without reservation.

The programme of work for the PhD student is described in detail in Section 6.6. However, since the main research load can be carried out by the PI and RA, we agree to remove the PhD studentship from the project and reduce the duration of the project to two years.

Reviewer 3
Progress in these directions would be interesting and the availability of implementations of efficient algorithms would be worthwhile. On the other hand, no major breakthroughs are envisaged, expected progress being of an incremental nature, and it is questionable whether the research will achieve a major advance towards a full understanding of toroidal graphs. It could be argued that the part of the proposal that relates to implementation of existing algorithms is not high quality research that is worthy of special funding and that will lead to significant publications. This is particularly true when the algorithms are overwhelmingly of theoretical interest.

Currently, no implementable algorithms for topological surfaces other than the plane and the projective plane are known. Also, many graph problems that are hard in the general case can be efficiently solved for planar graphs and graphs embeddable on some fixed topological surfaces, for example the torus. This gives another incentive for detecting graphs embeddable on the torus and the Klein bottle. Going from the non-existence of implementable algorithms to the existence of readily available ones is a major advance of knowledge in the field, which should lead to significant publications and practical results. For example, the algorithms are planned to be implemented in two graph software packages. The last statement "the algorithms are overwhelmingly of theoretical interest" is confusing — the emphasis of the project is actually on practical polynomial-time (i.e. implementable) algorithms and their software implementation.

One might hope that research results arising from a funded project of this kind would merit presentation at higher profile meetings than, for example, a one-day meeting at Oxford/UCL.

The reviewer must have overlooked our plans to present project results at the *British Combinatorial Conference* and the *International Workshop on Graph-Theoretic Concepts in Computer Science*. These two conferences are among best European conferences in the field.

It does not seem to me that a convincing case has been made for a PhD student on this project. The precise role of the student has not been clearly spelt out. The objectives of this proposal convey the feel of a two year, rather than a three year project...

We agree to remove the PhD studentship from the project and reduce the duration of the project to two years.

6.11 Summary of Key Lessons

Key lessons learnt from the proposal are summarised in Table 6.2. Similar to the previous chapter, it is another strong project with some shortcomings. Because the research area is very challenging, the project was aiming at realistic objectives and hence it has an "incremental nature". Also, its duration could be better thought through. Another important lesson is that the programme of work for each researcher must be provided, clarifying all interactions between the researchers.

Table 6.2: Key Lessons.

Strengths	**Weaknesses**
The quality is high. The methodology is sound.	The breadth of the impact of such results would be limited.
This is a high quality research project that will continue and extend recent interesting work of the proposer and others.	It does not seem to me that a convincing case has been made for a PhD student on this project.
The proposal contains a detailed discussion of the scientific methodology.	What is unclear though, is how the work of the PhD student and RA will fit together.
The RF has co-operated with one of the leading authorities in this area. The collaboration with the developer of Groups and Graphs should yield considerable benefits.	Any impact outside these areas will be very limited, and it seems unlikely that the work could have any impact at all in the industrial or non-academic sector.
The results will be of considerable interest to the academic community. It is quite possible it will have a major impact in the non-academic sector.	The part of the proposal that relates to implementation of existing algorithms is not high quality research that is worthy of special funding.
Progress in these directions would be interesting and the availability of implementations of efficient algorithms would be worthwhile.	No major breakthroughs are envisaged, expected progress being of an incremental nature. This research is more incremental than risky.
Both the PI and the named researcher have strong records of research in the relevant fields and are well equipped to make significant further contributions.	It is perhaps not reasonable to expect major advances in knowledge from such a project, incremental advance is more likely. The proposal is not particularly 'adventurous'.
The dissemination arrangements are very good — publication in journals and improvement to widely available computer packages.	The objectives of this proposal convey the feel of a two year, rather than a three year project, perhaps recognised by the suggested duration of the post doc position.
The management plans are good. The research objectives justify the requested resources.	The project management plans are somewhat vague.

Chapter 7

Development and Mining of a Comprehensive Database of Graph Invariants

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7.1 Aims, Objectives and Methodology

The primary purpose of this proposal is to develop and then data mine a comprehensive database of graph invariants with the aim of producing new insights and theorems in graph theory. The specific objectives are:

Extend GRAPHBASE We will extend GRAPHBASE to include a large sample of graphs of order 11 (about 25 million non-isomorphic graphs). Notably, counterexamples to conjectures and 'exceptional' graphs in various proofs very often have a small number of edges. Our sample of graphs of order 11 will include all such graphs and their complements, thus enhancing the value of the database. Further examples can be added at a later date by the user community.

Determine graph invariants For each graph in the extended GRAPHBASE, all invariants described below will be calculated and held in GRAPHRESOURCE. Additional invariants will also be considered and determined. Our ambition is that GRAPHRESOURCE will hold exact values of the invariants in all cases but, if for some invariants this proves computationally infeasible, bounds will be given.

Establishment of a database of known results Known results and conjectures in graph theory will be collected and held in GRAPHRESULTS, conjectures flagged as such. This database will be used to steer the search away from known results. The graph theory community will have access to GRAPHRESULTS and be asked to contribute.

Discovery of relationships between graph invariants This objective will be achieved using two approaches. Firstly, a comprehensive statistical analysis of the data and invariants in GRAPHRESOURCE will be undertaken. This alone may yield novel results. The second approach is the application of a variety of data mining techniques, including rule induction, clustering and feature construction using genetic programming. These results will be held in GRAPHRESULTS.

Analysis of relationships between graph invariants Patterns emerging from the discovery process, representing relations between invariants, will be analysed theoretically to establish new graph-theoretic results. In addition, we will test the validity, or otherwise, of conjectures resulting from the Graffiti studies on all the graphs in our extended database.

Website development and population A website will be developed and maintained, allowing access, firstly to GRAPHRESOURCE, then to GRAPHRESULTS, containing the results of the statistical and data mining analysis. Registered users of the site will thus be granted access to the code used to compute the invariants and to the database of known and conjectured results.

The key deliverables will be as follows:

1. GRAPHRESOURCE, an extended version of the database, GRAPHBASE, containing all non-isomorphic, undirected, simple graphs of order at most 10 plus a large sample of non-isomorphic graphs of order 11 in the form of encoded adjacency matrices, together with over 60 invariants evaluated for each graph.

2. A database, GRAPHRESULTS, containing known results and conjectures in graph theory, together with results of the statistical analysis and data mining of graph invariants in GRAPHRESOURCE.

3. A website allowing access by the research community to GRAPHRESOURCE and GRAPHRESULTS.

4. A library of code to compute invariants.

5. Additional invariants will be considered for inclusion in our study and added to the database. Detailed descriptions of these new invariants and the updated database will then be available on the web.

6. Statistical and data mining techniques to search for relationships between the invariants in GRAPHRESOURCE.

7. A number of papers will be written describing the statistical analysis of the invariants and the data mining methods used. The latter will include exploitation of some of the most recent developments in metaheuristics.

8. Hitherto unknown relationships between graph invariants found in the data mining phase will be scrutinised and, if considered of interest, we will decide if their general validity can be established. If so, these results will represent new theorems in graph theory and will be published. If not, the patterns will only hold as theorems for graphs of order at most 10. We will attempt to find counterexamples among larger graphs but, in either case, will publish our results.

The first year of the project will concentrate on extending the database and on determining the invariants of the graphs. This work will be split between UEA and UWE, with both post-doctoral RAs acquiring and/or developing algorithms for this purpose. During this first year, decisions will be made with respect to additional invariants that we wish to include in our study.

During the second year, the UWE post-doctoral RA will continue the work of determining the remaining invariants and populating the database, whilst also statistically analysing the data in GRAPHRESOURCE. During the second and third years, the UEA post-doctoral RA will apply statistical and data mining techniques to the data held in GRAPHRESOURCE. The results from these experiments will be analysed and made public, both through normal publication routes (i.e. journals) and the website (via GRAPHRESULTS).

Further analysis on the rules discovered will be carried out in the context of graph theory. It is envisaged that a number of new results in graph theory will emerge from this work. This theoretical analysis will be undertaken mainly by UWE, with considerable contributions from UEA.

The key tasks of the UEA post-graduate RA will be to develop the databases GRAPHRESOURCE and GRAPHRESULTS, collect known graph results and conjectures to populate the latter and develop and maintain the website for access to all resources. This website will provide information about the project, its objectives and progress. It will also give access to the graph database and to the invariants and results as they are determined/discovered.

7.2 Development of the Database

As previously stated, the primary purpose of this proposal is to develop and then data mine a comprehensive database of graph invariants with the aim of producing new insights and theorems in graph theory. As such, it contributes to a growing body of research which uses computers to aid the discovery of new results in graph theory [16].

We have a database, GRAPHBASE, that contains all non-isomorphic graphs of order at most 10. These graphs are all undirected and contain no loops nor multiple edges. There are 12,293,434 graphs in GRAPHBASE and they are stored as encodings of the adjacency matrix. An initial task for this project is to extend this database to include a sample of the graphs of order 11.

In [5, 6], a computer-based system called INGRID (Interactive Graph Invariant Delimiter) was described. This system took known relationships between graph invariants that had been published in the literature and used these to deduce bounds for unspecified variables once bounds for specified variables are given. As with this study, research was focussed on undirected graphs with no loops nor multiple edges. The original paper incorporated 262 relationships.

The second stage of our project is to take the extended GRAPHBASE and, for each graph, compute the invariants used by [5] (see Tables 7.1 and 7.2). Some of these are trivial to compute (e.g. *order*) and some require the application of well-known polynomial-time algorithms (e.g. *planar graphs*). Moreover, the Boolean-valued invariants in Table 7.2 can easily be determined using the invariants in Table 7.1.

Table 7.1: Twenty seven numerical graph-theoretic invariants [5].

Type	*Symbol*	*Description*	*Symbol*	*Description*
Integer	p	*order* (the number of vertices)	q	*size* (the number of edges)
	δ	*minimum degree*	Δ	*maximum degree*
	d	*diameter*	r	*radius*
	κ	*vertex connectivity*	κ_1	*edge connectivity*
	g	*girth*	c	*circumference*
	β_0	*independence number*	β_1	*edge independence number*
	χ	*chromatic number*	χ'	*edge chromatic number*
	γ	*domination number*	k	*number of connected components*
	α_0	*vertex covering number*	α_1	*edge covering number*
	Θ_0	*vertex clique cover number*	Θ_1	*edge clique cover number*
	ω	*clique number*	B	*bandwidth*
	Υ	*arboricity*	ν	*crossing number*
	γ_g	*genus*	θ	*thickness*
Real	λ	*spectral radius*, i.e. the largest eigenvalue of the adjacency matrix		

However, for many graph invariants, their computation for an arbitrary graph is NP-hard (e.g. *chromatic number*, *bandwidth*). In such situations, since the graphs are relatively small, we expect that it will be feasible to use exact algorithms for most cases. For many NP-hard invariants we will use so-called return algorithms, which have already been successfully tested for some of the invariants. We already calculated the *domi-*

Table 7.2: Seven Boolean-valued graph-theoretic invariants [5].

bipartite	connected	forest	tree	regular	planar	Hamiltonian

Table 7.3: Additional invariants.

Symbol	*Description*	*Symbol*	*Description*
ξ	*coarseness*	η	*Hadwiger number*
ψ	*achromatic number*	i	*independent domination number*
$\overline{d}$	*average degree, i.e.* $\frac{1}{p}\sum_{i=1}^{p} deg(v_i)$	Γ	*upper domination number*
γ_c	*connected domination number*	γ_t	*total domination number*
b	*binding number*	ϵ	*energy*

nation number and other domination-related invariants for all graphs in GRAPHBASE (not extended). Although their computation is NP-hard, the running time was quite reasonable because all graphs are of order at most 10.

A more challenging problem is calculating the topological invariants (*genus*, *crossing number*, *thickness*), which require a different approach. Although the *genus* problem is NP-complete, it is the case that for every fixed surface there exists a polynomial-time algorithm for embedding graphs on that surface (see, e.g. [26, 15]), however it is not clear whether the algorithms in [15] are implementable. Because the *genus* of the complete graph of order 11 is 5, we need to use or develop five algorithms for surfaces of *genus* 0, 1, 2, 3 and 4 to efficiently solve the *genus* problem for small graphs. Note that if a graph from GRAPHBASE does not have *genus* less than 5, then it is automatically of *genus* 5.

The NP-complete *crossing number* problem turns out to be particularly difficult. In fact, no practically efficient exact algorithm for this problem is known, but we might be able to use the planarisation method taking into account that we deal with graphs of small order. For the *thickness* of a graph, we can use the fact that the *thickness* of the complete graph with 11 vertices is only 3. Because we can efficiently test whether a graph is planar, that is, whether it has *thickness* 1, it remains to determine all the graphs in GRAPHBASE with *thickness* 2. In other words, we need to recognise graphs which are non-planar and can be split into two planar subgraphs. This problem is much easier than the general *thickness* problem. However, where our ideas for the topological invariants prove to be infeasible, we will have to resort to heuristics and then may only be able to produce (bounded) approximations.

Further invariants will be considered for inclusion in our study and we will add them during the project. Table 7.3 provides an initial list of invariants, all of which will be included.

At the completion of this stage of research, we will have a database, GRAPHRESOURCE, of several million non-isomorphic graphs, each with 50–60 associated invariants. The database will be made publicly available from a website developed and maintained as part of the project and will include a detailed data dictionary for the database that will describe each field. This alone is of enormous value to the research community. Researchers will be able to use it to find examples of, or counter-examples to known or hypothesised results. Within this project, we will use the extended version of the database to check out the conjectures made within the Graffiti project, see [14] for example. Some of these conjectures were checked for graphs of order up to 10 [4] but not for graphs of order 11.

7.3 Mining of the Database

The third phase of the project is to use statistical and data mining techniques to search for relationships between the invariants that hold within the database. The Graffiti system is similar in that it is designed for the automated generation of conjectures.

This has proved successful to some extent and resulted in scores of papers [38]. However, the relationships generated by Graffiti are constrained to have very simple formats, and the database of graphs used was extremely small. For a number of conjectures, graphs of small orders were counterexamples. In this project, not only do we use a very much larger database but we also aim to provide a general framework capable of discovering more complex patterns, albeit with greater user guidance.

As to analysis, firstly, we shall generate descriptive statistics of the invariants, including ranges and distributions for each invariant. These statistics will be held in a separate database, also published on our website. The next step will be to investigate 'one-ways', that is, simple relationships between pairs of fields. This will include the use of visualisation techniques to recognise any obvious patterns. Even this simple analysis may provide new results. The famous observation *planar* $\Rightarrow \chi \leq 4$, first noticed in the mid nineteenth century, led to the *Four Colour Theorem*, a grand challenge in graph theory, which was not proved to hold in general until 1976 and then only with computer assistance [2, 3].

The main challenge to the data mining phase of the project is to discover patterns involving constraints and/or arithmetic expressions constructed from several variables. As an example, the following is a general result proved in [27]:

$$\delta \geq 4 \text{ and } g \geq 5 \Rightarrow c \geq (g-2)(\delta-2)+5, \tag{7.1}$$

where δ is the *minimum degree*, g is the *girth* and c is the *circumference.* This is an example of a relationship between just three of the invariants, which we would expect to be able to find.

Formally, let I be the set of all invariants. We will seek patterns involving some subset $I' \subset I$, and usually this subset will be constrained to be quite small. The simplest pattern that we can seek is a simple equality or inequality that holds for all graphs, for example [9]:

$$\chi \geq p/(p - \lambda),$$

where χ is the *chromatic number*, p is the *order* and λ is the *spectral radius.* We will search for general patterns of the form

$$X_1 \; R \; f(X_2, \ldots, X_n) \tag{7.2}$$

where

$$\{X_1, X_2, \ldots, X_n\} \subset I$$

and X_1 is the *target* variable, R is an element of

$$\{=, \neq, >, \geq, <, \leq\}$$

and $f(X_2, \ldots, X_n)$ is an arithmetic function over the invariants $X_2, \ldots, X_n$ and natural numbers. In practice, $\{X_1, X_2, \ldots, X_n\}$ will contain only a small number of carefully selected variables, which are deemed to be of interest. Similarly, the natural numbers will be constrained to be small.

Restrictions may also be used to reduce the number of graphs for which the result holds. These restrictions will be defined in terms of the non-target variables. In the general form, they will be a conjunction of expressions of the form

$$f(X_2, \ldots, X_n) \; R \; 0.$$

Thus, we can describe patterns such as

$$p - 4 > 0 \;\text{ and }\; q - 2p + 4 > 0 \;\Rightarrow\; c \geq 2g - 2, \tag{7.3}$$

where p is the number of vertices, q is the number of edges, g is the *girth* and c is the *circumference.*

In data mining terms, we are searching for *rules of association.* Assume that we have a database D of d records. Each record has n attributes (fields), f_i, defined over domain Dom_i. We are searching for a rule $\alpha \Rightarrow \beta$ that appears to hold for some records in D. If r denotes a particular record in D, then there are three sets of records associated with any rule $\alpha \Rightarrow \beta$:

- $A = \{r \mid \alpha(r)\}$ is the set of records matching the condition part of the rule.

- $B = \{r \mid \beta(r)\}$ is the set of records matching the consequent part of the rule.

- $C = \{r \mid \alpha(r) \text{ and } \beta(r)\}$ is the set of records matching both the condition part and the consequent part of the rule, that is, those records accurately classified by the rule.

There are a number of performance measures that determine the usefulness of any given rule:

Coverage is the number of records satisfying the condition part of the rule, usually expressed as a proportion of the whole database (i.e. $|A|/|D|$).

Confidence is a measure of how often the rule is correct, often measured as a proportion of the number of records matching the condition part (i.e. $|C|/|A|$).

Support is the product of *coverage* and *confidence* (i.e. $|C|/|D|$) and is a measure of the proportion of records in the whole database accurately classified by the rule.

Thus, we are searching for rules with 100% confidence and, in order that they have some interest, we will need a set of support exceeding some threshold. If the precondition becomes too prescriptive, the set of support will be reduced. Thus, we will limit the format of the precondition and the number of invariants used.

The search for association rules of the form (7.2) will be achieved using a variety of methods already tested successfully at UEA, as well as others as necessary. The former include the use of genetic algorithms [19, 20], with which the data mining group at UEA has been successful in searching for rules in commercial databases, and simulated annealing used within the commercial data mining package, Witness MINER, developed by the UEA team within a Teaching Company Scheme programme. Genetic algorithms and simulated annealing are examples of heuristics used in search.

In [43], we developed more effective heuristics requiring no parameter settings and yet outperforming the more established heuristics on many problem instances in classical operational research problems such as TSP and QAP. We plan to incorporate these new heuristics into a rule induction platform to search for the rules of association. Such heuristics can also be used to facilitate the search for graphs to provide counterexamples to conjectures. Moreover, if a counterexample is found, then it may lead to a new (rectified) conjecture. The AutoGraphiX system [17] uses variable neighbourhood search to achieve this.

The search for association rules is complicated by the need to search among arithmetic expressions. Most rule induction algorithms, such as tree induction techniques, are restricted to axis-parallel conditions, for example $p \leq 10$ and $q \geq 15$. However, we expect to be finding rules containing non-linear expressions such as in equation (7.1).

One approach that can be used to search for non-linear expressions is genetic programming [23]. This technique is used to evolve non-linear expressions from the set of attributes and a specified set of operators suitable for the attribute set. In [24, 25], we exploited the nonlinearity of genetic programming in *constructive induction*, in which highly predictive non-linear combinations of attributes are evolved as a pre-processing, feature construction phase. The enhanced attribute set, originals plus evolved, are then fed into a tree or rule induction algorithm to generate non-linear rules. For two of the data sets used, a single evolved attribute in each case led to a classifier that attained 100% accuracy over the entire data set, that is, 100% confidence and 100% support. The team is therefore confident of developing successful approaches to determining rules of association. The search space will include rules of varying support but always aiming at 100% confidence.

We will be particularly interested in searching for patterns involving combinations of invariants not explored in detail in the literature. To determine in which areas to focus search, we will undertake preliminary research to determine promising subsets of invariants. We will develop software to allow the user to steer the search towards interesting relations by specifying the target variable and the other variables, and to constrain the format of any arithmetic expressions.

Another challenge is to avoid regenerating the wealth of results already known or conjectures already made. Not only do we want to avoid known results, but we also wish to avoid results deduced from known results by simple arithmetic manipulation or by logical inference. We will maintain a database, GRAPHRESULTS, of known results and conjectures and steer the search away from them. This database will be publicly available and updated throughout the project.

Some results will be of more interest than others. This may be because some invariants are better known but is more generally motivated by the need to bound a difficult-to-compute invariant in terms of simpler-to-compute invariants. By focusing on target invariants that are hard to compute and using other invariants that are easier to compute, we hope to produce interesting conjectures which, if proved, would have some applicability. At the very least, any result we produce can be turned into a theorem by adding 'for graphs of order at most 10' as an additional precondition.

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7.5 Resources and Work Plan

The following table provides a summary of resources required for this three-year project in terms of costs.

Table 7.4: Summary of costs.

Type of Costs	**Sub-type**	**Costs (£)**
Directly Incurred Costs	Staff	256,811
	Travel & Subsistence	17,880
	Consumables	17,885
	Equipment	18,589
Indirect Costs		118,134
	Total	**429,299**

The proposed research requires considerable effort relying on expertise in various fields and of differing technical requirements. Our proposal for a post-doctoral RA at UEA (graph theory, algorithm design, statistics, data mining) and a post-doctoral RA at UWE (graph theory, C++ programming) to fulfil the higher technical level requirements and a post-graduate RA at UEA (database/website design, development and population) to concentrate on the lower technical requirements is, in our view, a cost-effective approach to meeting these requirements. The RA at UWE is expected to have a strong background in mathematics at a post-doctoral level, a basic knowledge of graph theory and experience in C++ programming. To attract a person of such a qualification, the support is requested at scale LCIG RB8 (non-JNCHES).

In addition, due to the size of the data sets and the tasks required, we seek a high performance workstation for each researcher for the duration of the project and a high performance server to host the database and serve the website, as we expect the resulting site to have to deal with many thousands of hits per day, once established. Finally, at UEA, we seek a contri-

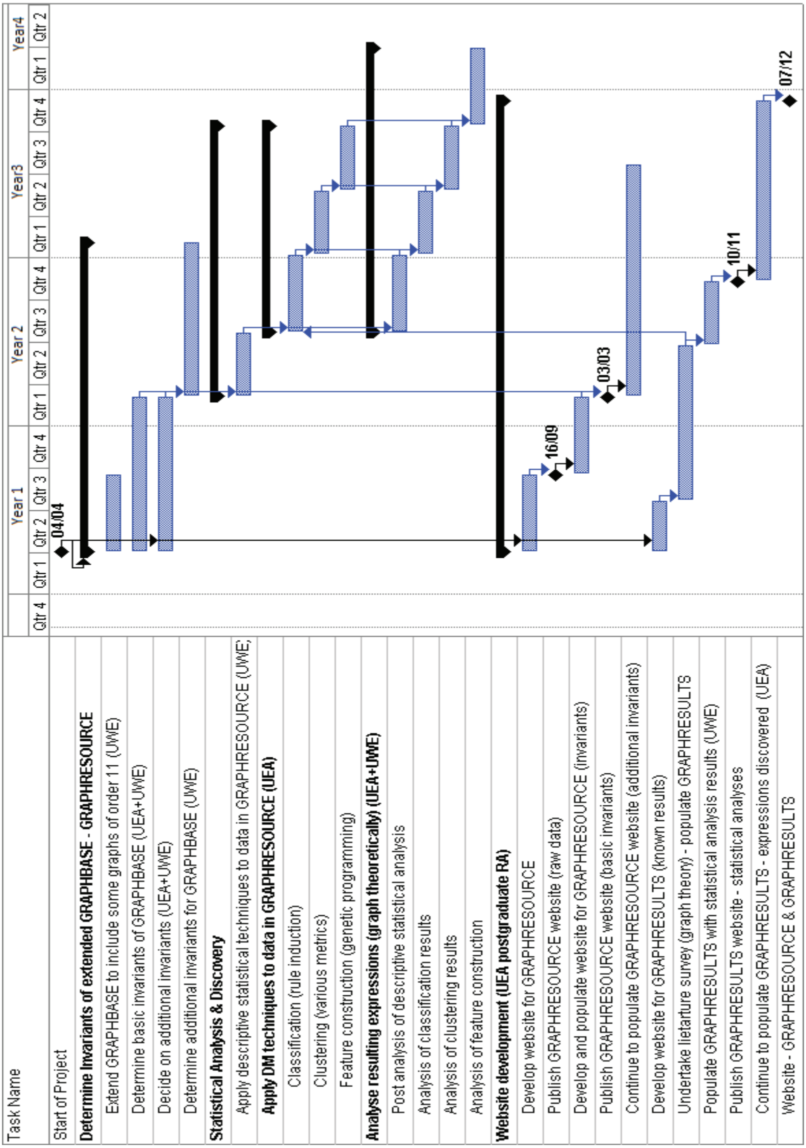

Figure 7.1: Work plan.

bution to secretarial support to aid in the administrative tasks of managing the project and also to technical support in the setting up and maintenance of the database and the website.

The plan of the work is shown in Figure 7.1.

7.6 Reviewers' Reports

This section includes three reviewers' reports [37]. Their comments are in *italics*.

Report 1

Response Due Date: 7 February

Research Quality
This proposal is high quality, both from the point of view of the originality of the proposal, the expertise of the proposers, the care with which it has been thought through, and the likely outcomes.

Methodology
The methodology is well explained, and although it is complicated, it is feasible.

Adventure in Research
There is little risk, but much originality.

Assessment of the overall research quality: Unsatisfactory, Adequate, Good, <u>Outstanding</u>

My confidence level: Low, Medium, <u>High</u>

Research Impact
The impact on the academic community should be high. I would be surprised if it did not also have a considerable impact industrially.

Assessment: Low, Limited, Significant, <u>Major</u>

My confidence level: Low, Medium, <u>High</u>

Academic Collaboration

In this case, [the PI] *appears to have a greater theoretical background in graph theory, but all parties have considerable and varied experience in handling large quantities of data. Both departments should benefit from this collaboration.*

Non-Academic Collaboration

[Crossed over]

Dissemination

The dissemination by the development of an extremely useful website, and by publications in journals is ideal.

Assessment of the collaboration and dissemination arrangements: Unsatisfactory, Adequate, Good, <u>Outstanding</u>

Planning and Project Management

The management appears to be well thought out, and the timescales are realistic.

Staff Resources, Equipment and Consumables

In a complicated project like this, there is a need for substantial staff resources, but the potentially useful outcome justifies the resources.

Assessment of the management arrangements: Unsatisfactory, Adequate, Good, <u>Outstanding</u>

My confidence level: Low, <u>Medium</u>, High

Your Conclusions

This unusual proposal deserves support.

The overall quality of the proposal: Unsatisfactory, Adequate, Good, <u>Outstanding</u>

I believe this proposal:

Should not proceed as proposed, Could proceed as proposed, <u>Should proceed</u>

Reviewer Expertise

Graph theory and combinatorics.

Report 2

Response Due Date: 7 February

Research Quality
The quality is not high — this will not generate "interesting conjectures".

Methodology
The methodology for the (very uninteresting) goals is fine.

Adventure in Research
No risk. More importantly, it is absurd to think that looking at graphs of order ≤ 11 will generate interesting maths: that is not how graph theory works.

Assessment of the overall research quality: Unsatisfactory, Adequate, Good, Outstanding (not chosen)
My confidence level: Low, Medium, High (not chosen)

Research Impact
No impact. People make (non-trivial) conjectures by thinking about things. More importantly, conjectures are usually supposed to be natural/interesting.
Assessment: Low, Limited, Significant, Major
My confidence level: Low, Medium, High

Academic Collaboration
Looks good: will develop clear links between UEA and UWE.

Non-Academic Collaboration
[Crossed over]

Dissemination
Seems fine.

Assessment of the collaboration and dissemination arrangements: Unsatisfactory, Adequate, Good, Outstanding

Planning and Project Management
Milestones: yes, specified.

Staff Resources, Equipment and Consumables
The research objectives certainly do not justify any resources.

Assessment of the management arrangements: Unsatisfactory, Adequate, Good, Outstanding
My confidence level: Low, Medium, High

Your Conclusions
This is no way to do graph theory. The project might be fine for computer-science. But its own stated aim, namely "aim of producing new insights and theorems in graph theory", has no chance of being achieved by this sort of small-graph stuff.

The overall quality of the proposal: Unsatisfactory, Adequate, Good, Outstanding
I believe this proposal:
Should not proceed as proposed, Could proceed as proposed, Should proceed

Reviewer Expertise
Graph theory.

Report 3

Response Due Date: 7 February

Research Quality
This proposal is, briefly, as follows. In phase 1, compute a whole variety of invariants for all graphs of order at most 11 (some of which may be NP-hard to determine in general). In phase 2, confirm or refute conjectures from the Graffiti project for graphs of order 11 (as opposed to order 10 which has been done). In phase 3, use data mining techniques to generate new conjectures. These techniques will include genetic algorithm, simulated annealing and genetic programming. Throughout the project, a web interface will be developed to make public the results of the project. Whilst the above may seem a very cursory description of the project, that is essentially all that is provided

by the applicants. No technical details are given. For example, which algorithms will be used to compute the graph invariants and why should they work for NP-hard problems, even on graphs of order at most 11? How will the data mining techniques, which are all established and standard, be applied to yield new conjectures? One is drawn to the opinion that there are no real new insights in this research proposal, just a brute force approach towards a worthy goal.

Methodology
As I mentioned above, "how" things will be done is not really discussed. The applicants appear to wish me to take it on trust that, e.g., suitable invariant-computing algorithms are available/will be found and that suitable genetic algorithms can be concocted. Either that or there are no real computational difficulties inherent in the proposed work, in which case the work might be described as development rather than research.

Adventure in Research
There is no risk at all associated with this proposal. It is very incremental in nature and I am still undecided as to whether this proposal is simply developmental. I would have liked to have been much better informed as to the viability of certain aspects of the project and the technical difficulties to be overcome. Instead, all I got was a list of occasions when the team had had success apply certain techniques in the past.

Assessment of the overall research quality: <u>Unsatisfactory</u>, Adequate, Good, Outstanding

My confidence level: Low, Medium, <u>High</u>

Research Impact
What cannot be doubted is that the outcomes of a successful conclusion to this research would be of merit to the graph theory community and would be seen as a very useful testing-board for graph theoreticians. New theorems would undoubtedly be generated (but I make no comment on the "usefulness" of these theorems, even from a graph-theoretic perspective).

Assessment: Low, Limited, Significant, Major
My confidence level: Low, Medium, High

Academic Collaboration
The collaboration between UEA and UWE is to be welcomed.

Non-Academic Collaboration
N/A

Dissemination
Dissemination plans are appropriate.

Assessment of the collaboration and dissemination arrangements: Unsatisfactory, Adequate, Good, Outstanding

Planning and Project Management
The planning is only acceptable as the interaction between the teams at UEA and UWE has not been described in detail, from a technical perspective. That is, I'am not too clear as to exactly what each of the post-doc researchers will be doing and how they will liaise. The roles of the researchers (apart from the postgraduate researcher, who is to develop the web-interface) have not been described in sufficient detail by any means.

Staff Resources, Equipment and Consumables
The resources requested for an undertaking of this research are way too much, bordering on the ludicrous. The applicants wish for 3 members of research staff when, as I have mentioned, it has not been well argued that this "research" goes beyond development. Also, UEA have not explained why they need secretarial and technical support. Which "administrative tasks" will need managing so that it takes 5% of a Clerical Pt 4 person? Why will technical support be needed to maintain the database and the web-site? Won't this be done by the postgraduate researcher?

Assessment of the management arrangements: Unsatisfactory, Adequate, Good, Outstanding
My confidence level: Low, Medium, High

Your Conclusions

Whilst the intentions of this proposed research are worthy and whilst a successful conclusion would benefit the graph theory community, I am not convinced that the proposed research has any real technical merit associated with it. This, allied with a vastly overinflated request for resources, makes this research unfundable. Nevertheless, I would still encourage the proposers to develop such a web-site as that proposed, although I understand that finding funding for such a developmental activity will be difficult. Perhaps they might do it via a series of third-year undergraduate/masters student projects.

The overall quality of the proposal: Unsatisfactory, Adequate, Good, Outstanding

I believe this proposal:
Should not proceed as proposed, Could proceed as proposed, Should proceed

Reviewer Expertise

Theory of computing.

7.7 PI's Response to Reviewers' Comments

Speechless...

7.8 Summary of Key Lessons

Key lessons learnt from the proposal are summarised in Table 7.5. Similar to Chapter 3, the proposal of Chapter 7 is presented at a macroscopic level, where there are not enough technical details. This project is regarded as "incremental" by the reviewer, but it seems that the proposal is rather adventurous, perhaps too adventurous. Another reviewer calls the approach "absurd", which is a very harsh assessment; however many conjectures and theorems in graph theory were first formulated by looking at small graphs. Nevertheless, some good examples could be given in the proposal to illustrate this point. The inclusion of secretarial and technical staff should be avoided, if possible, and consumable costs must be reasonable.

Table 7.5: Key Lessons.

Strengths	**Weaknesses**
This proposal is high quality (from the point of view of the originality, the expertise of the proposers and the likely outcomes).	The quality is not high — this will not generate "interesting conjectures". "How" things will be done is not really discussed.
The methodology is well explained. There is little risk, but much originality.	Very uninteresting goals. It is very incremental in nature.
The impact on the academic community should be high.	The work might be described as development.
The outcomes would be of merit to the graph theory community and would be seen as a very useful testing-board for graph theoreticians.	No impact. It is absurd to think that looking at graphs of order ≤ 11 will generate interesting maths.
Both departments should benefit from this collaboration.	There are no real new insights in this research proposal.
The dissemination by the development of an extremely useful website, and by publications in journals is ideal.	The aim of the project has no chance of being achieved by this sort of small-graph stuff.
The management appears to be well thought out, and the timescales are realistic.	The interaction between the teams at UEA and UWE has not been described in detail.
The potentially useful outcome justifies the resources.	The research objectives certainly do not justify any resources. The resources requested for an undertaking of this research are way too much.

Chapter 8

Graph-Theoretic Conjectures and the Computer System GRAPHOGRAPH

8.1 Aims and Background

It is generally accepted nowadays that various software packages are applied in advanced research. There are methods for solving some particular problems that cannot be implemented without using specialised software. Within the last thirty years, a number of computer systems for dealing with graphs have been proposed: INGRID by Brigham and Dutton [2]–[5], AutoGraphiX by Caporossi and Hansen [6], Autoson and Nauty by McKay [18, 19], GRAPH THEORIST by Epstein [10], GRAPH by Cvetković [7, 8], Graffiti by Fajtlowicz [11]–[15], Groups & Graphs by Kocay [17]. Nevertheless, there are still many graph-theoretic problems to which these computer systems cannot be applied. Also, some systems are not readily available or are designed for a narrow range of specialised problems.

Scientific conjectures, obviously, express highly interesting theoretic statements which have been neither proved nor disproved. They are basically posed in order to draw the attention of the scientific community and advance progress in the corresponding field. We plan to consider conjectures about the coefficients of the chromatic polynomial and the number of forests of a graph, Bartels–Welsh's problem on the expected number of colours used in a random colouring as well as a number of well-known problems connected with the domination parameters and the conjectures about the independence number generated by the well-known computer program Graffiti.

Once posed, a conjecture must ultimately be proved or disproved. It is important to underline that the disproof of a conjecture does not necessarily mean a 'negative' result. It often yields insights into the problem and can result in new conjectures and/or theorems. Moreover, it can be the case that a disproof of a conjecture gives an important constructive theorem. For example, in 1990, Sumner [26] conjectured that the class of domination perfect graphs cannot be characterised in terms of a finite number of forbidden induced subgraphs. However, with the help of specially developed software, a characterisation of domination perfect graphs in terms of 17 forbidden subgraphs was obtained in [29], thus disproving Sumner's conjecture.

The main aim of the proposed project is to investigate various open problems and conjectures with the help of GRAPHOGRAPH, a Windows application for graph theory. For example, we intend to solve a number of well-known problems connected with the domination parameters, to study a problem of Bartels and Welsh [1] on the expected number of colours used in a random colouring, to verify unimodal and log-concavity conjectures about the coefficients of the chromatic polynomial [23] and the number of forests [28] of a graph, and to check whether a certain graph polynomial does not distinguish trees [21]. Moreover, we plan to verify some of the conjectures generated by the well-known computer program Graffiti (see [11]–[15]), especially those connected with the concept of independence in graphs.

The second aim is to produce a user-friendly Windows application GRAPHOGRAPH, in particular to develop and implement a wide range of algorithms so that GRAPHOGRAPH may be applied to a variety of graph-theoretic conjectures. In order to support and advance research in graph theory, the software will be freely available to the community of interested researchers through the Internet. Finally, besides the extensions of the existing database of non-isomorphic graphs, we plan to carry out a feasibility study into the construction of a database of graph parameters and its analysis using data mining techniques.

To sum up, the basic idea of the proposed research project is the application of scientific software and databases associated with it to graph-theoretic problems and conjectures. It is important to underline that we already exploited this idea and successfully solved some open problems in graph theory.

8.2 Graph Polynomials and the Unimodal Conjectures

In [1], Bartels and Welsh posed the following three problems concerning the expected number $EC(G)$ of colours used in a random proper n-colouring of a graph G with n vertices:

1. For any graph G with n vertices $EC(G) \geq EC(O_n)$, where O_n is the empty graph with n vertices.

2. For any graph G there exists an edge $e \in E(G)$ such that $EC(G) \geq EC(G - e)$.

3. For any graph G and for all edges $e \in E(G)$, $EC(G) \geq EC(G - e)$.

The first problem was shown to be true [9], and the third is known to be false [20]. However, the second remains open. We will study this problem using GRAPHOGRAPH. An expression for $EC(G)$ is given by

$$EC(G) = n\left(1 - \frac{\chi(G; n-1)}{\chi(G; n)}\right),$$

where $\chi(G;\lambda)$ is the chromatic polynomial of G. Consequently, finding $EC(G)$ will require evaluating the chromatic polynomial. Although this is a #P-hard problem, techniques for doing this in a reasonable time exist, using the deletion-contraction method and exploiting the fact that many of the graphs arising in this procedure are isomorphic, see for instance [24]. The counterexample to the third statement has only six vertices, so it is reasonable to hope that if the second statement has a negative answer then there will be a relatively small counterexample.

Using the aforementioned methods to evaluate the chromatic polynomial will allow to compute its coefficients using Lagrange interpolation. This will enable us to study the conjectures due to Read [23] and Welsh [28], respectively, stating that the coefficients of the polynomial $(-1)^n\chi(G; -x)$ form a unimodal or even log-concave sequence. Welsh makes a further conjecture that for any graph the number of forests of size k (where $k = 0, 1, \ldots, n-1$) forms a log-concave sequence. This conjecture can be tested by finding the specialisation of the Tutte polynomial of a graph to the line $y = 1$. The techniques that we will use for the chromatic polynomial can easily be modified to apply here. Although these conjectures are long-standing, no extensive investigation using modern fast computers has been carried out.

The U polynomial of a graph was introduced in [7]. This polynomial generalises the Tutte polynomial and includes many graph invariants as special cases, for instance the matching polynomial and the 2-polymatroid polynomial of Oxley and Whittle [22]. Furthermore, for trees, it is equivalent to the symmetric function generalisation of the chromatic polynomial introduced by Stanley [25], in the sense that the coefficients of one function may be obtained from the coefficients of the other. In [25], Stanley asked whether his symmetric function generalisation of the chromatic polynomial distinguishes trees.

Stanley's problem has so far been verified in the affirmative for trees with up to 11 vertices. Using GRAPHOGRAPH, we will extend this search by studying the equivalent problem of whether the U polynomial distinguishes trees. For trees, there is a polynomial-time algorithm to compute U and it is a simpler function to deal with computationally than Stanley's symmetric function. There is good cause to believe that examining graphs of this size will be worthwhile because the smallest non-isomorphic graphs with the same U polynomial have 11 vertices.

8.3 Domination Parameters of Graphs

The concept of domination is fundamental in graph theory. Let $ir(G)$, $\gamma(G)$, $i(G)$, $\beta_0(G)$, $\Gamma(G)$, $\Gamma^-(G)$ and $IR(G)$ be the irredundance number, the domination number, the independent domination number, the independence number, the upper domination number, the upper minus domination number and the upper irredundance number of a graph G, respectively. Also, let $\gamma_s(G)$ and $\Gamma_s(G)$ be the signed domination number and the upper signed domination number of G. A *cubic graph* is a 3-regular graph. The class of cubic graphs plays an important role both in graph theory and computer science. Cubic graphs were formally introduced by Petersen in 1891, although informally they were considered by Tait as early as 1880. Since then this class has been widely studied by many researchers.

The following interesting problems on the domination parameters of cubic graphs were posed in the well-known book by Haynes, Hedetniemi and Slater [16]:

Problem 8.1 *Does there exist a cubic graph G satisfying*

$$\Gamma^-(G) < \Gamma(G)\,?$$

Problem 8.2 *Is it true that if G is a cubic graph, then*

$$\Gamma^{-}(G) \leq \gamma_s(G)\,?$$

Problem 8.3 *For a cubic graph are Γ^{-} and Γ_s comparable? If so, how do they compare?*

Problem 8.4 *Does there exist a cubic graph for which*

$$ir < \gamma < i < \beta_0 < \Gamma < IR\,?$$

Why are cubic graphs of particular interest? Firstly, many graph-theoretic problems admit transformations reducing problems on general graphs to problems on cubic graphs. Such transformations permit one to make use of the properties of cubic graphs and, in some cases, solve the problems. On the other hand, many problems are no easier to solve when restricted to the class of cubic graphs, although this class seems to be very simple. Studying problems restricted to cubic graphs, however, often yields insights into the general problems.

It should be pointed out that we already solved a number of open problems connected with the above parameters. For example, we investigated the difference between the domination and independent domination numbers for cubic graphs with given connectivity [30]. In particular, a 92-vertex counterexample to Barefoot–Harary–Jones' conjecture was found. Also, we proved a known conjecture on the difference between the upper irredundance number and the independence number for graphs with maximum degree 3 [31].

8.4 Conjectures Generated by Graffiti

The development of the computer program Graffiti was initiated by Fajtlowicz in 1984. This program generates graph-theoretic conjectures basically in the form of inequalities. The most interesting conjectures generated by Graffiti can be found in [11]–[15], while the most complete list of about 900 conjectures is available via the Internet. In fact, this list contains about half of the Graffiti conjectures. We are mainly interested in verifying the Graffiti conjectures devoted to the independence number because we believe that some of them are not true. We mention just a few conjectures.

For an edge e and a vertex v, e is called a *v-horizontal edge* if the distance from v to both end vertices of e is the same. Let $d_o(v)$ denote the number of vertices at odd distance from v, and let $h_o(v)$ be the number of v-horizontal edges whose both end vertices are at odd distance from v.

Conjecture 766 *If a graph G is cubic, then the independence number of G is greater than or equal to*

$$\min_v d_o(v) - \min_v h_o(v).$$

Let $d_e(v)$ denote the number of vertices at even distance from v, and let $h_e(v)$ be the number of v-horizontal edges whose both end vertices are at even distance from v.

Conjecture 767 *If G is cubic, then the independence number of G is greater than or equal to the average value of $d_e(v)$ minus the average value of $h_e(v)$.*

Conjecture 768 *If G is cubic, then the independence number of G is greater than or equal to the average value of $d_e(v)$ minus* $\min_v h(v)$, *where $h(v)$ is the number of v-horizontal edges.*

For regular graphs of degree d, the *residue* is defined as

$$\rho = \lceil n/(d+1) \rceil.$$

Conjecture 773 *The radius of a cubic connected graph is not more than* $0.5(n - \rho)$.

Note that the smallest counterexample to any of these conjectures, if it exists, must have at least 21 vertices, as shown by Caporossi, Hansen and Pujol.

8.5 GRAPHOGRAPH and Graph-Theoretic Properties

The main purpose of the GRAPHOGRAPH software is to check the validity of various graph-theoretic properties for different sets of graphs and to search for graphs satisfying certain properties. GRAPHOGRAPH comprises several parts described below and incorporated into the Graphical User Interface Shell. This software will be particularly useful for finding counterexamples to conjectures, obtaining characterisation results and evaluating properties involving graph-theoretic parameters. However, it is still in the draft state and further development is needed for the first release.

The Property Editor and the Graph Language GRALE
Properties are specified in the Property Editor using a high-level language that is very close to the terminology of graph theory. This language is called GRALE (GRAph LanguagE). Here is an example of a simple property in GRALE:

```
ConnectedGraph[G]
DominationNumber[G] < IndependentDominationNumber[G]
ChromaticNumber[G] = 3
```

GRAPHOGRAPH must be able to ‘understand’ properties in GRALE — this is achieved by the compiler that analyses and translates them into structures recognisable by the software. Thus, the development of GRALE also suggests the construction of the corresponding compiler. The above is a relatively simple property ‘A connected graph with the domination number less than the independent domination number and having the chromatic number 3’. Some ideas from [2, 3] can be used and further developed for compilation of simple properties. For many graph properties, however, it is not straightforward to define the corresponding GRALE instructions. An example of such a property is as follows: 'A graph possesses a clique covering $C_1, C_2, ..., C_q$ such that some C_i has property P'. There are other 'difficult' properties. Thus, for many graph-theoretic properties to be translatable into GRALE, it is necessary to define appropriate terms and/or functions for GRALE and develop corresponding algorithms.

The Graph Database GRAPHBASE and the Graph Editor

The software has a large built-in database of pairwise non-isomorphic graphs. This database, called GRAPHBASE, contains over 16 million graphs and includes all graphs of order up to 10. All those graphs are stored as encodings of the adjacency matrix. Properties specified in the Property Editor can be evaluated on:

- The whole of the built-in database.
- Subsets of the built-in database.
- Sequences of randomly generated graphs with specified properties.
- Arbitrary user-defined sets of graphs.

The first two options utilise GRAPHBASE, which must be extended by adding graphs of orders between 11 and 20 belonging to well-known classes of graphs, for example cubic graphs, planar graphs, chordal graphs and bipartite graphs. It should be noted that this problem is difficult because it suggests an 'algorithmic solution' of the isomorphism problem for those graphs. To generate sequences of random graphs with prescribed properties, it is necessary to develop algorithms randomly generating graphs from many well-known classes of graphs with the number of vertices/edges in a specified range. For the last option, we plan to create the Graph Editor that will enable a user to easily define different practically-useful sets of graphs using predefined operations.

The Bank of Algorithms

At present, the weakest part of the software is its bank of algorithms. It is necessary to develop hundreds of algorithms, test them and incorporate into GRAPHOGRAPH. The algorithms are subject to classification according to the input/output. One important class of algorithms includes computations of integer-valued graph-theoretic parameters. Further important algorithms are those connected with graph polynomials as well as algorithms for random generation of graphs from various classes.

Feasibility of the Database of Graph Invariants

The existing GRAPHBASE can be used to create a database of graph invariants. Such a database would be a considerable asset to the research community. Using data mining techniques, it could be used in future research to spot patterns and equations that might not be known. For example, techniques based on genetic programming, evolutionary computation and rule induction can be applied to generate valid patterns holding in the database of graph invariants. Moreover, this database could considerably reduce the running time for many properties because, as a rule, they involve different graph-theoretic invariants. Within the present research project, we plan to carry out a feasibility study into the construction and analysis of the database of graph invariants.

8.6 References

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8.7 Beneficiaries, Resources and Work Plan

The results of research will be of benefit to all academic and technological organisations interested in theoretical computer science, especially in graph theory and combinatorial optimisation. It is important to underline that there are two distinct beneficial aspects of this research: theoretical results and software (GRAPHOGRAPH). This software will be of benefit to researchers both in graph theory and computer science, by enabling them to readily test new or known conjectures. It can also be used, as part of the learning process, by students specialising in mathematics and related fields. To make the dissemination of GRAPHOGRAPH as wide as possible, it will be freely available through the Internet.

Table 8.1 provides a summary of resources required for this three-year project in terms of costs.

Besides pure graph-theoretic research, the proposed plan is essentially based on further developing and applying GRAPHOGRAPH. The majority of the funding is required for a RA who will be responsible for the development of this software. It is necessary to stress that the success of the entire project strongly depends on successful development of GRAPHOGRAPH.

Table 8.1: Summary of costs.

Type of Costs	**Sub-type**	**Costs** (£)
Directly Incurred Costs	Staff	90,269
	Travel & Subsistence	5,490
	Other Costs	8,305
Indirect Costs		41,524
	Total	**145,588**

The RA is expected to have a strong background in mathematics and great deal of experience in C++ programming. Moreover, the RA must have a basic knowledge of graph theory, combinatorial optimisation and compiler theory. All these elements are necessary because the RA will make an essential contribution to developing GRAPHOGRAPH. In particular, the RA will take part in developing graph-theoretic algorithms and implementing them using C++ as well as developing GRALE and creating the compiler. An ideal RA would be a highly experienced C++ programmer having a strong background in mathematics. To attract a person of such a qualification, the support is planned at spine point 8.

The programme of work for the RA has been carefully thought over and discussed. The first two months will be spent with the RA familiarising her/himself with the GRAPHOGRAPH program and the necessary background graph theory involved in the problems to be studied. During the first year, the RA must essentially contribute to the development of the Graph Editor, the Property Editor, GRALE and the compiler, and the facilities for generating graphs randomly. The second year will be devoted to implementation of various algorithms, for instance for evaluating the chromatic polynomial and domination-related numbers. Since GRAPHOGRAPH is designed for a wide range of graph-theoretic problems, it is necessary to develop many algorithms and incorporate them into the Bank of Algorithms.

The final year will be spent for testing and improving all the modules of GRAPHOGRAPH and preparing the first release of this software.

The proposed project essentially requires high-performance PCs with dual high-speed processors. Indeed, almost all algorithms to be incorporated in GRAPHOGRAPH are exponential (i.e. inefficient), since no analogous polynomial algorithms are known. Hence such algorithms work slowly and require much time to process sequences of graphs. On the other hand, it is often necessary to process hundreds of million graphs. Thus, high-performance PCs play a crucial role in corresponding calculations. It is extremely important that each member of the project has such a PC, to be used exclusively for running and testing various GRAPHOGRAPH programs, and these PCs must be considered as equipment additional to computers provided by the universities. We also need C++ Builder for developing GRAPHOGRAPH as well as a number of books on C++ and graph theory.

In order to reach the community of interested researchers, we will publish our results in internationally recognised journals such as the *Journal of Graph Theory; Combinatorics, Probability and Computing* and *Discrete Mathematics*. Moreover, we plan to attend scientific meetings and conferences to present our work and obtain feedback from the scientific community. We will attend the *British Combinatorial Conference* and also an American conference such as the *Southeastern International Conference on Combinatorics, Graph Theory and Computing.*

Travel expenses are needed to exchange ideas with British and foreign specialists in the field and to disseminate our work. We plan to attend two conferences mentioned above. To maintain collaboration between co-investigators, it is necessary to have visits between University A and University B twice a year. Moreover, in connection with the feasibility study into construction and analysis of the database of graph invariants, we plan two visits between University C and University A. The aim of the visits is to receive expert advice from one of the leading scientists in data mining and metaheuristics.

The programme of work is based on developing the computer system GRAPHOGRAPH and applying it to various graph-theoretic conjectures and problems. The theoretical part consists of three basic directions: graph polynomials and the unimodal conjectures, the domination parameters of graphs and the graph-theoretic conjectures generated by the computer program Graffiti. The development of GRAPHOGRAPH suggests the creation or revision of the following: the Graphical User Interface Shell, the Property Editor, the Graph Language GRALE and the corresponding compiler, the Graph Editor, the Graph Database and the Bank of Algorithms.

8.8 Reviewers' Reports

This section includes three reviewers' reports [27]. Their comments are in *italics.*

Report 1

Response Due Date: 26 March

Research Quality
The proposed research is of a reasonable quality; the graph-theoretic problems would be of some interest to other researchers in the immediate community.

Methodology
There does seem to be a potentially significant problem with feasibility. The proposal stresses that the algorithms are mainly exponential in their complexity and that it is necessary to process "hundreds of million graphs" (sic). It would be interesting to know how long the proposers think all this would take. I may be missing something here but, as stated, this does not seem to be tractable with the existing algorithms.

It may be that something can be done but the case, as made, is not convincing; this aspect needs to be more carefully explained.

Adventure in Research

The software proposed would be potentially useful although there is the usual problem with proposals of this kind. Production of high-quality software is a very skilled task and it is hard to recruit developers with sufficiently high ability, not to mention ones with the other areas of expertise outlined in the proposal as well.

In general, there is a great deal of money expended by [the funder] *funding proposals to produce stand-alone software systems which do not reach complete fruition in the timescale of the project and then never reach full maturity. One sometimes wonders whether efforts would be often better directed towards collaborating with the developers of existing well developed and successful packages.*

Of course, these reservations may be unfair in this particular case, but it is hard to be completely convinced that this sort of relatively stand-alone project is the best way forward. However, given some degree of uncertainty about this judgement, I have ticked "Adequate" rather than "Unsatisfactory" below.

Assessment of the overall research quality: Unsatisfactory, <u>Adequate</u>, Good, Outstanding

My confidence level: Low, <u>Medium</u>, High

Research Impact

As stated above, the graph-theoretic problems are of some interest in the immediate research community. Given the nature of the research, there would be no real interest in the industrial/non-academic sector; this is not a criticism, just a consequence of this type of research being proposed.

Assessment: Low, <u>Limited</u>, Significant, Major

My confidence level: Low, <u>Medium</u>, High

Academic Collaboration
The case for the involvement of Professor [name] *is not made in any detail at all and, as it stands, is not at all convincing. In order for this request to be justified, some more details as to precisely how he would be involved (and, in particular, how his expertise would help the project at hand) should be provided.*

These worries are the cause of the "Unsatisfactory" rating below.

Non-Academic Collaboration
Not applicable.

Dissemination
The arrangements here are standard and perfectly satisfactory.

Assessment of the collaboration and dissemination arrangements: Unsatisfactory, Adequate, Good, Outstanding

Planning and Project Management
The timescales and milestones are not completely convincing (although I do accept that there is a fair degree of uncertainty in a project of this nature).

Staff Resources, Equipment and Consumables
These do not seem to be unreasonable given the proposed project.

Assessment of the management arrangements: Unsatisfactory, Adequate, Good, Outstanding

My confidence level: Low, Medium, High

Your Conclusions
As seen by the above comments, I do have some significant reservations about this proposal. In some cases this may be due to a lack of clarity on the part of the proposers and they obviously have the opportunity to respond to my comments.

On balance I'm prepared to give them the benefit of the doubt and say that the proposal is "Adequate" and that it "Could proceed as proposed" but it might have a better chance of success if it was rewritten with these points in mind. The feasibility of *the algorithms, the need to develop yet another system, the involvement of Professor* [name]*, and so on, are all issues where the case (as it stands) is not convincing.*

The overall quality of the proposal: Unsatisfactory, <u>Adequate</u>, Good, Outstanding

I believe this proposal:
Should not proceed as proposed, <u>Could proceed as proposed</u>, Should proceed

Reviewer Expertise
Graph theory and combinatorics; theoretical computer science.

Report 2

Response Due Date: 26 March

Research Quality
The most important part of the proposal is the further development of GRAPHOGRAPH, but the mathematic problems are also interesting and important. I rate the research quality as extremely high, as GRAPHOGRAPH seems to be very well thought out and should be of very great use to a very wide range of users. A real credit to its originators.

Methodology
The methodology is tried and tested, and is sound.

Adventure in Research
As the proposal is to develop further something that already exists, perhaps it is not so adventurous. The adventure lies in developing GRAPHOGRAPH to the point that it can become of use to many different users. There is adventure in the consideration of the mathematical conjectures.

Assessment of the overall research quality: Unsatisfactory, Adequate, Good, <u>Outstanding</u>

My confidence level: Low, Medium, <u>High</u>

Research Impact

The research impact on the graph theory community should, in the long term, be very great. I have little idea about its industrial impact, but it would not surprise me greatly if it were of great use there too.

Assessment: Low, Limited, Significant, Major

My confidence level: Low, Medium, High

Academic Collaboration

The collaboration is between [two universities]*. The collaboration will be good for both institutions. The further collaboration of both with* [another university] *will also be valuable.*

Non-Academic Collaboration

Not applicable.

Dissemination

GRAPHOGRAPH will be available on the internet, and, I presume, the mathematical results will be published in the normal way in journals. At some point, GRAPHOGRAPH will be developed sufficiently for some further publicity to be warranted, but there is no discussion of this in the proposal.

Assessment of the collaboration and dissemination arrangements: Unsatisfactory, Adequate, Good, Outstanding (not chosen)

Planning and Project Management

Timescales and Management seem to me to be realistic. Of course further development of GRAPHOGRAPH will take place over time.

Staff Resources, Equipment and Consumables

[No comments]

Assessment of the management arrangements: Unsatisfactory, Adequate, Good, Outstanding

My confidence level: Low, Medium, High

Your Conclusions
This is an excellent project, and should be supported.

The overall quality of the proposal: Unsatisfactory, Adequate, Good, Outstanding

I believe this proposal:
Should not proceed as proposed, Could proceed as proposed, Should proceed

Reviewer Expertise
Graph theory, combinatorics.

Report 3

Response Due Date: 26 March

Research Quality
This proposal combines high quality theoretical research and software development.

Methodology
The proposed methodology is correct.

Adventure in Research
The research risk is minimal as the researchers have shown ability to tackle problems similar to ones suggested in the proposal.

Assessment of the overall research quality: Unsatisfactory, Adequate, Good, Outstanding

My confidence level: Low, Medium, High

Research Impact
The results, both theoretical and software, will have major impact on the academic community. The impact to non-academic sector is less significant.

Assessment: Low, Limited, Significant, Major

My confidence level: Low, Medium, High

Academic Collaboration
The proposed collaboration will be very helpful.

Non-Academic Collaboration
N/A

Dissemination
The journals and conferences are chosen correctly.

Assessment of the collaboration and dissemination arrangements: Unsatisfactory, Adequate, Good, <u>Outstanding</u>

Planning and Project Management
Appropriate project management plans have been specified.

Staff Resources, Equipment and Consumables
Do the research objectives justify the requested resources? *Yes.*

Assessment of the management arrangements: Unsatisfactory, Adequate, Good, <u>Outstanding</u>
My confidence level: Low, Medium, <u>High</u>

Your Conclusions
[Empty]
The overall quality of the proposal: Unsatisfactory, Adequate, Good, <u>Outstanding</u>
I believe this proposal:
Should not proceed as proposed, Could proceed as proposed, <u>Should proceed</u>

Reviewer Expertise
Graph theory, combinatorial optimisation.

8.9 PI's Response to Reviewer 1's Comments

Based on misinterpretations of minor issues, Reviewer 1 jumps to negative conclusions about some aspects of the entire project. In our view, this style is inadequate and it can be used to sink any research proposal.

There does seem to be a potentially significant problem with feasibility. . .

There is no problem with feasibility at all because we deal with graphs of small order. The methodology has been successfully tested and used to obtain some interesting results described in the proposal. The important thing is to have high-performance PCs to reduce running time. This is based on our experience in testing the system.

Production of high-quality software is a very skilled task...

We have all the necessary expertise, for example the PI has been teaching and using in research many programming languages (C, C++, Assembler, PASCAL, Java) since 1992. Furthermore, we have a preliminary agreement with a software developer who has the necessary abilities.

One sometimes wonders whether efforts would be often better directed towards collaborating with the developers of existing well developed and successful packages.

Although there are a few well-developed and successful packages, they all have a different philosophy and some of them are not freely available.

Of course, these reservations may be unfair in this particular case...

The reviewer demonstrates uncertainty and lack of understanding in simple issues. He/she rated the overall research quality as "Adequate" and failed to see in the proposal that we successfully carried out similar research and published it in first-class journals.

The case for the involvement of Professor [name] *is not made in any details at all...*

This professor is supposed to be a visiting researcher for two short visits without salary, thus suggesting a relatively small contribution. Indeed, the funding of this collaboration constitutes a very small part of the overall project, namely 540 pounds. In Section 8.7, we say, "... in connection with the feasibility study into construction and analysis of the database of graph invariants, we plan two visits...". Perhaps the reviewer overlooked a subsection "Feasibility of the Database of Graph

Invariants", which explains the need for these visits.

The overall collaboration and dissemination arrangements are: unsatisfactory.

This is beneath all criticism. The reviewer says nothing about the main collaboration between Universities A and B described in the proposal, and he/she is only unhappy about the minor collaboration between Universities A and C discussed above. The other reports describe the collaboration as "very helpful" and "valuable". Concerning dissemination, the reviewer says, "The arrangement here are standard and perfectly satisfactory". What is the logic to conclude "unsatisfactory" for the overall collaboration and dissemination?

On balance I'm prepared to give them the benefit of the doubt.

The reviewer has a great deal of doubt and mainly discusses minor issues. No proposal with a page limit can explain all simple questions. Our proposal describes general ideas that have been successfully tested. Some of our ideas resulted in publications in the internationally recognised journals such as the *Journal of Graph Theory* and *Discrete Mathematics.*

8.10 Summary of Key Lessons

Key lessons learnt from the proposal are summarised in Table 8.2. Unfortunately, the report of Reviewer 1 is another example of very poor practice. For example, the statements made in the Adventure in Research section of this report could be said for any proposal with the objective to develop a computer system. This reviewer's style can be used to reject practically any research proposal.

It is the author's hope that reviewers will be much more accurate in their assessments of research bids. Perhaps the next step in improving the reviewing process would be to introduce a 'scientific arbiter' who has the right to nullify biased reports.

Table 8.2: Key Lessons.

Strengths	**Weaknesses**
The research quality is extremely high.	Reasonable quality.
The methodology is tried and tested, and is sound. The proposed methodology is correct.	There does seem to be a potentially significant problem with feasibility.
There is adventure in the consideration of the mathematical conjectures. The research risk is minimal.	The proposal is not so adventurous.
The graph-theoretic problems are of some interest in the immediate research community. The research impact on the graph theory community should, in the long term, be very great. The results, both theoretical and software, will have major impact on the academic community.	Given the nature of the research, there would be no real interest in the industrial/non-academic sector.
The dissemination arrangements are standard and perfectly satisfactory. The collaboration will be good for both institutions.	The case for the involvement of a professor is not made in any detail at all.
Timescales and management seem to me to be realistic. Appropriate project management plans have been specified.	The timescales and milestones are not completely convincing.

Acknowledgement

The author is grateful to Dr S. Noble for his contribution to Section 8.2.

Chapter 9

Man-Machine Approach to Investigation into Hereditary Classes of Graphs

9.1 Aims and Background

A hereditary class of graphs is one of the fundamental concepts in graph theory. Hereditary classes have been widely studied in the literature, and a number of classical results have been proved for them. Many well-known classes of graphs are hereditary, for example planar graphs, perfect graphs and bipartite graphs. Given a hereditary class of graphs, one of highly interesting problems would be to provide a characterisation of the class. Such a characterisation may give insights into the structure of graphs in the class and in many cases provide a recognition algorithm, in some cases an efficient one. A number of important classes of graphs have been characterised, for example the fundamental Kuratowski theorem characterises planar graphs, and the classical König theorem characterises bipartite graphs. Chudnovsky, Robertson, Seymour and Thomas [6] proved the famous Strong Perfect Graph Conjecture, thus providing a characterisation of perfect graphs. While some classes are well studied and their characterisations are known, other classes remain uninvestigated.

The basic aims of the proposed project are as follows:

(i) To develop a man-computer technique for dealing with hereditary classes of graphs from the viewpoint of obtaining and proving characterisation results. For this purpose, we will produce a user-friendly module for processing large sets of graphs possessing prescribing properties. This module will be part of a complex Windows application for graph theory, which will be developed in the subsequent years.

(ii) To apply our technique to various hereditary classes of graphs, for example 2-dominable graphs, pentagraphs, blue graphs and locographs, in order to obtain characterisation results for these classes.

(iii) To formulate characterisation conjectures about the aforementioned hereditary classes of graphs.

Nowadays, various software packages are applied in advanced research in graph theory. The reason being that there are methods for solving some particular problems that cannot be implemented without using specialised software (e.g. see [1]–[5], [8]–[15] and [19]–[21]). On the other hand, there are still many graph-theoretic problems to which the above software cannot be applied, for instance problems connected with characterisations of hereditary classes of graphs.

Using the proposed man-machine technique, we plan to formulate a number of conjectures about the aforementioned hereditary classes and prove some of them. In general, scientific conjectures express highly interesting theoretic statements which have been neither proved nor disproved. They are basically posed in order to draw attention of scientific community and advance progress in the corresponding field.

Thus, the basic idea of the proposed research project is the application of scientific software to hereditary classes of graphs in order to obtain characterisation results for the classes. It is important to underline that we already exploited this idea and successfully solved some open problems in graph theory.

9.2 Applying Software for Preliminary Analysis of the Classes

As previously stated, the basic idea of the proposed research project is the application of scientific software to hereditary classes of graphs in order to obtain characterisation results for the classes. Theoretically, each hereditary class can be characterised in terms of forbidden induced subgraphs. However, if the number of forbidden induced subgraphs is large, then it may be practically impossible to provide an explicit list of such subgraphs. Another problem could be that the number of forbidden induced subgraphs is reasonable, but the method for obtaining a corresponding characterisation is difficult and it cannot be implemented without specialised software.

Given a hereditary class P, the first step in tackling the problem is to obtain the list of all minimal forbidden induced subgraphs of order at most 10, let us denote this list by L. This step is very important and crucial for choosing subsequent steps. In fact, the list L gives rise to various ideas and conjectures about the class. Moreover, it provides a characterisation of the class P for graphs of order at most 10.

To fulfil the difficult task of finding L for the class P, we will use our database of all non-isomorphic graphs of order at most 10. There are over 12 million graphs in the database. Thus, it will be necessary to develop a computer program implementing the following procedure:

We put $L = \emptyset$, where L is the list of minimal forbidden induced subgraphs for P.

Step 1 Take the next graph G from the database. If there are no more graphs, then exit.

Step 2 Verify whether G belongs to P. If so, go to Step 1.

Step 3 Verify whether G contains one of the graphs from L as an induced subgraph. If it does contain any of the forbidden graphs, then go to Step 1.

Step 4 Update the list of minimal forbidden subgraphs L because a new minimal forbidden graph was found. Go to Step 1.

It is not difficult to see that this procedure correctly determines the list L. Indeed, in Step 3 we only consider graphs not belonging to P. If such a graph G is not minimal, then it must contain a minimal forbidden graph of smaller order from L. Because our database contains all graphs of order up to 10, such a minimal graph of smaller order must be already in L and it will be found in G by the procedure. Thus, the above procedure is correct.

9.3 Formulating Conjectures

Having obtained the list of minimal forbidden subgraphs of order at most 10, we can analyse it in order to decide what subsequent steps should be undertaken. The simplest situation is when the list L has a small number of graphs of order not exceeding 8. This situation would result in a conjecture that L is the list of all minimal forbidden subgraphs, and the next step would be to prove this conjecture. This case can be illustrated by basic graphs described in the next section.

A more complicated situation is when L has a large number of graphs of order not exceeding 8. In the same fashion, we can pose a conjecture that L is the list of all minimal forbidden subgraphs, but it might be difficult to prove it because of a large number of possible 'cases' to consider.

Another 'good' situation is when the list L has a small number of graphs for each order and 'patterns' of forbidden graphs can be easily identified from L. For example, if we apply the above procedure to perfect graphs, then the list L would contain the cycles C_5, C_7, C_9 and their complements. It is straightforward to see the pattern and pose a conjecture (in fact, the Strong Perfect Graph Conjecture), even though we know that it is extremely difficult to prove it.

The most difficult situation would be that L has a large number of graphs and the function $L(n)$ is similar to an exponential function, where $L(n)$ denotes the number of graphs in L of order n. For example, suppose that

$$L(7) = 100, \quad L(8) = 200, \quad L(9) = 400 \quad \text{and} \quad L(10) = 800.$$

What can we do in this situation? The best solution would be to obtain a characterisation in different terms, not in terms of forbidden induced subgraphs. Unfortunately, rarely can we find an alternative characterisation. For instance, if we applied the above procedure to planar graphs, then the corresponding list L would contain a large number of graphs. However, a careful consideration of those graphs (perhaps with the help of a computer) could reveal that every graph in L contains a subgraph which is homeomorphic to either K_5 or $K_{3,3}$, resulting in a conjecture similar to the Kuratowski theorem.

Another interesting example, which could illustrate the last situation, is our characterisation of upper domination perfect graphs in terms of forbidden *semi*-induced subgraphs [36]. If we apply the above procedure to upper domination perfect graphs, then L contains a large number of graphs, and we believe that it is impossible to provide an explicit list or a reasonable description of all minimal forbidden induced subgraphs for this class. However, we were able to provide the list of all forbidden semi-induced subgraphs [36]. (See Theorem 1.4.)

The above discussion describes rather extreme situations, but there may be further ‘intermediate cases’. They must be considered on an individual basis.

Thus, if the above procedure does not lead to a conjecture (or conjectures) for a particular class of graphs, then we should apply something else, even though the procedure yields an important result giving us an explicit list of all minimal forbidden graphs of order at most 10 for the class. One of possible ideas often used in practise is to restrict our characterisation results to some subclasses. For example, if we consider a hereditary class P and the list L has too many graphs, then we could try to characterise bipartite graphs from P (or other subclasses which could be of interest). After eliminating from L all non-bipartite graphs, we may obtain an updated list L' containing a small number of graphs, so that it would be possible to formulate the corresponding conjecture. In the same fashion, we can investigate the class P further.

9.4 Proving Conjectures

While the previous stages ‘admit’ some general rules, this stage is more complicated. There are no general methods for proving conjectures. They all must be investigated on an individual basis, and particular methods must be developed for each conjecture, not excluding a possible need to develop specialised software. Another problem is that we do not know the exact formulations of those conjectures until we actually run the proposed project.

Moreover, while applying software is always a success, resulting in an explicit list of all minimal forbidden graphs of order at most 10 for a particular class of graphs, plus possible conjectures, there is no guarantee that a particular conjecture will be proved during this stage. First of all, the project is only one year long. Secondly, some conjectures might be very difficult to prove. However, it is very important to stress that good conjectures are of great value in mathematics. Many fundamental results were first known as conjectures.

In order to illustrate possible ideas that can be applied to prove conjectures, we give a couple of examples below.

Basic Graphs

A subclass of perfect graphs called basic graphs plays an essential role in the proof of the Strong Perfect Graph Conjecture by Chudnovsky, Robertson, Seymour and Thomas [6]. The *line graph* $L(G)$ of a graph G is defined as the intersection graph of edges of G, that is,

$$V(L(G)) = E(G)$$

and two distinct vertices e and e' are adjacent in $L(G)$ if and only if the edges e and e' in G have a common vertex.

The following notation is used: $\mathcal{B}$ is the class of all bipartite graphs, $\overline{\mathcal{B}}$ is the class of complements of all bipartite graphs, $\mathcal{LB}$ is the class of line graphs of all bipartite graphs and $\overline{\mathcal{LB}}$ is the class of complements of line graphs of all bipartite graphs. Conforti, Cornuéjols and Vušković [7] defined the class of basic graphs:

Definition 9.1 *The class of* basic graphs *is defined as follows:*

$$\mathcal{BASIC} = \mathcal{B} \cup \overline{\mathcal{B}} \cup \mathcal{LB} \cup \overline{\mathcal{LB}}.$$

The well-known König theorem states that a graph is bipartite if and only if it does not contain any odd cycles as an induced subgraph. It follows that a Berge graph is bipartite if and only if it is C_3-free. Accordingly, a Berge graph is cobipartite if and only if it is O_3-free. Hemminger and Beineke [18] as well as Staton and Wingard [23] independently proved that the class $\mathcal{LB}$ coincides with the class of $(\text{Claw}, \text{Diamond}, \text{Odd Holes})$-free graphs. Therefore, the class $\mathcal{LB}$ is exactly the class of $(\text{Claw}, \text{Diamond})$-free Berge graphs, and $\overline{\mathcal{LB}}$ is exactly the class of $(\text{coClaw}, \text{coDiamond})$-free Berge graphs.

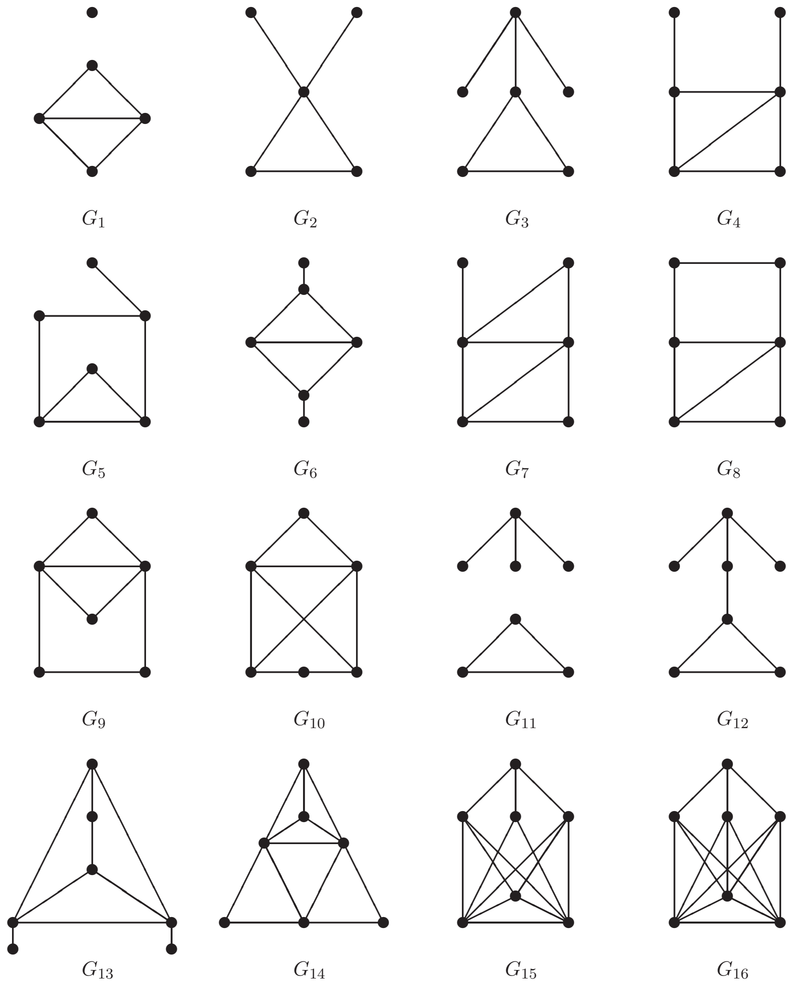

Figure 9.1: Minimal forbidden induced subgraphs for basic graphs within Berge graphs. (Reproduced from [37] with the permission from Elsevier.)

By applying the computer program to basic graphs, we found that besides odd holes and odd antiholes, all other minimal forbidden subgraphs of order at most 10 for this class are 16 graphs $G_1, G_2, \ldots, G_{16}$ shown in Figure 9.1. An immediate conjecture is that these 16 graphs, odd holes and odd antiholes are all minimal forbidden graphs for basic graphs:

Conjecture 9.1 *A graph is basic if and only if it does not contain any of the graphs $G_1, G_2, \ldots, G_{16}$, odd holes and odd antiholes as induced subgraphs.*

Omitting technical details, we proved in [37] that for basic graphs, the maximum order of a minimal forbidden graph, which is not an odd hole or an odd antihole, is nine. Because all such minimal graphs were found, Conjecture 9.1 is proved, thus providing a characterisation of the entire class of basic graphs:

Theorem 9.1 *A graph G is basic if and only if it does not contain any of the graphs $G_1, G_2, \ldots, G_{16}$, odd holes and odd antiholes as induced subgraphs.*

Domination Perfect Graphs

Our next example is the known class of domination perfect graphs. A graph G is *domination perfect* if for each induced subgraph H of G, the domination number of H is equal to the independent domination number of H. The first result on domination perfect graphs was obtained by Gupta in 1969, even though the formal definition of the class was given by Sumner and Moore in 1979 [24]. Since then, many graph theorists provided a number of partial characterisation results for the class. Moreover, in 1990, Sumner [25] posed a conjecture that the entire class of domination perfect graphs cannot be characterised in terms of a finite number of forbidden induced subgraphs.

We proved in [35] that a graph is domination perfect if and only if it does not contain the graphs $F_1, F_2, \ldots, F_{17}$ in Figure 9.2 as induced subgraphs, thus disproving Sumner's conjecture. This characterisation implies a number of important results, for

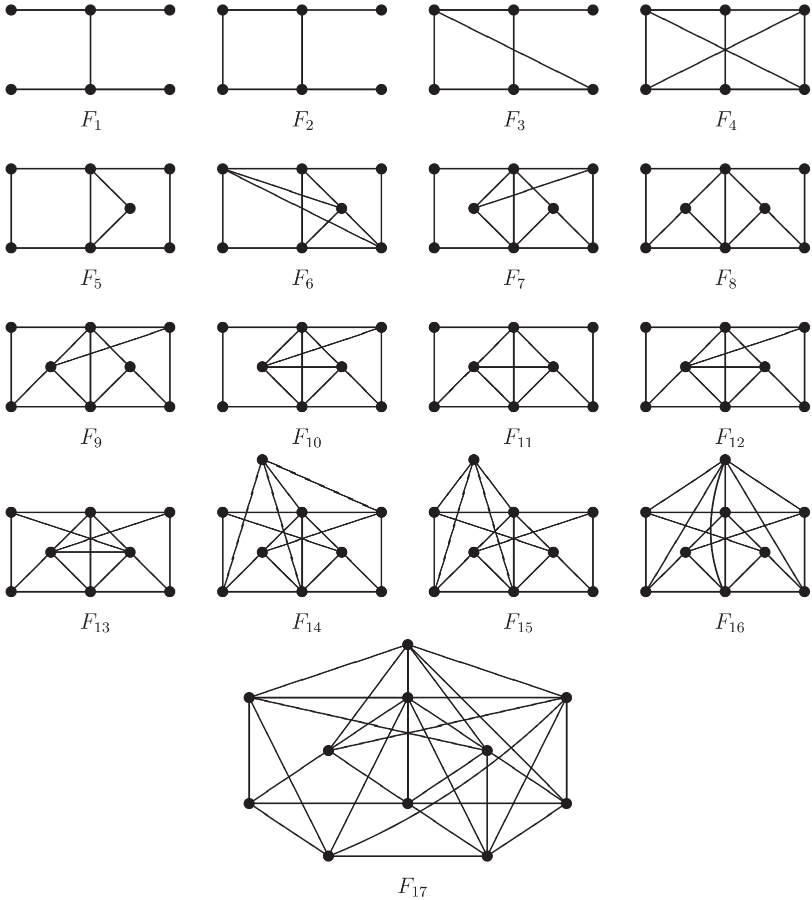

Figure 9.2: Minimal forbidden domination imperfect graphs $F_1 - F_{17}$. (Reproduced from [35] with the permission from John Wiley and Sons.)

example a polynomial-time recognition algorithm for domination perfect graphs and a result on the computational complexity of the domination number and the independent domination number for various classes of graphs.

To obtain the above characterisation, we developed a method of eliminating a dominating pair. Without mentioning any technical details, this method was impossible to implement manually. Hence we developed specialised software for our method and successfully applied it.

It is important to emphasise that the final proof has no reference to computer programs. Although the proof is very difficult, it can be verified without any use of a computer. To explain this, let us imagine that in a graph of order 15 with 50 edges we want to find an induced subgraph isomorphic to a particular graph of order 9 with 21 edges. And suppose that we want to do this for many graphs of order 15. Of course, it cannot be done by hand. However, if a computer produces a printout showing the corresponding bijections, it is not so difficult to verify that those bijections indeed give the necessary induced subgraphs.

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9.6 Resources and Work Plan

Table 9.1 provides a summary of resources required for this one-year project in terms of costs.

Table 9.1: Summary of costs.

Type of Costs	**Sub-type**	**Costs** (£)
Directly Incurred Costs	Staff	10,110
	Travel & Subsistence	2,300
	Other Costs	5,988
Indirect Costs		4,651
	Total	**23,049**

Besides pure graph-theoretic research, the proposed plan is essentially based on further developing and applying scientific software. The PI will be responsible for this stage because of his expertise in programming languages. The visiting researcher (VR) will be responsible for analysing the results of the first stage and posing conjectures about hereditary classes under consideration. It is important that the both researchers take part in developing/applying software and analysing the results because these stages are closely related to each other. The final stage also involves the both researchers and it will be devoted to proving some of the above conjectures and writing up. Because the duration of the project is only one year, some of the conjectures might remain unproven, however all such conjectures will be published.

The proposed project essentially requires high-performance PCs. Indeed, some of the algorithms used are exponential (i.e. inefficient), since no analogous polynomial algorithms are known. Hence such algorithms work slowly and require much time to process sequences of graphs. On the other hand, it is often necessary to process millions of graphs. Thus, high-performance PCs play a crucial role in corresponding calculations. It is extremely important that each member of the project has such a PC, to be used exclusively for running and testing various programs related to the project, and these PCs must be considered as equipment additional to computers provided by the universities. We also need C++ Builder for developing the module as well as a number of books on C++ and graph theory.

In order to reach the community of interested researchers, we will publish our results in internationally recognised journals such as the *Journal of Graph Theory* and *Discrete Mathematics*. Moreover, we plan to attend scientific meetings and conferences to present our work and obtain feedback from the scientific community. We will attend the *British Combinatorial Conference*, the *University of Reading One-Day Combinatorics Colloquium* and also an American conference such as the *Southeastern International Conference on Combinatorics, Graph Theory and Computing*. Travel expenses are needed to exchange ideas with British and foreign specialists in the field and to disseminate our work. We plan to present our results at the aforementioned conferences, and the PI will give a talk at RUTCOR (USA).

To carry out this research, the VR will be visiting UWE, Bristol, during January, June and December. These months correspond to the most crucial periods of the project: the beginning of Stage 1, the beginning of Stage 2 and the end of the final Stage 3. A summary of activities for carrying out the project can be found in Figure 9.3. Note that some activities mentioned in the figure are described in the previous sections.

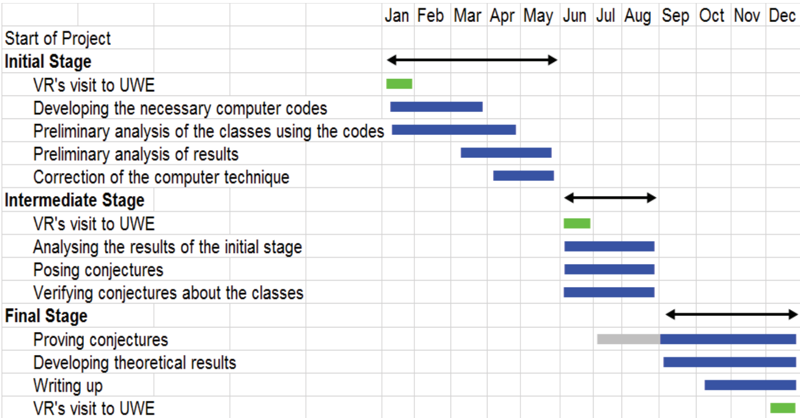

Figure 9.3: Work plan.

The measurable objectives of the project are:

Stage 1: Computer codes; analyses of the classes.

Stage 2: Conjectures; additional codes (optional).

Stage 3: Proofs of the conjectures; papers.

9.7 Reviewers' Reports

This section includes three reviewers' reports [28]. Their comments are in *italics*.

Report 1

Response Due Date: 14 February

Research Quality
The research quality is high standard. The proposed research will definitely advance the study of graphs and their application. Both PI and VR are well-equipped to carry out the task.

Methodology
Both PI and VR have already produced joint research of highest quality using man-machine approaches. The proposed research is certainly feasible even though the project will last only one year.

Adventure in Research
The research risk is minimal (see above), only a handful of researchers in the world could undertake the proposed research. For a one year project, the proposed research will make significant advances in the area of graphs. As hereditary classes of graphs play very important role in graph theory and its applications, it is important to carry out research in the field.

Assessment of the overall research quality: Unsatisfactory, Adequate, Good, Outstanding
My confidence level: Low, Medium, High

Research Impact
The impact to the academic community working in graph theory will be significant-to-major. The research may well contribute to potential applications.
Assessment: Low, Limited, Significant, Major
My confidence level: Low, Medium, High

Academic Collaboration
RUTCOR, Rutgers University is a major site for the study of discrete mathematics and operational research. The collaboration with [the VR] *is very important in carrying out the proposed research.*

Non-Academic Collaboration
N/A

Dissemination
The arrangements for dissemination are well-thought; some key conferences in the field as well as important journals are listed.

Assessment of the collaboration and dissemination arrangements: Unsatisfactory, Adequate, Good, Outstanding

Planning and Project Management
The project management is well-planned; the timescales and milestones are realistic.

Staff Resources, Equipment and Consumables
The requested resources are justified; the project is definitely value for money.

Assessment of the management arrangements: Unsatisfactory, Adequate, Good, Outstanding

My confidence level: Low, Medium, High

Your Conclusions
This is an outstanding project that will produce research of highest quality using a very moderate [funder's] *funding.*

The overall quality of the proposal: Unsatisfactory, Adequate, Good, Outstanding

I believe this proposal:
Should not proceed as proposed, Could proceed as proposed, Should proceed

Reviewer Expertise
Graph theory, combinatorics, research software.

Report 2

Response Due Date: 14 February

Research Quality

The nature of the proposed research is slightly unusual within the mathematics domain. The intention is to develop computer programs that will spawn conjectures concerning the characterisation of certain classes of hereditary graphs. Attempts will then be made to convert these conjectures into theorems, with or without further computer support.

The principle is a perfectly reasonable one, and there is evidence to suggest that the prospects of establishing original results by this means are good. All the same, there is a question mark over the likely significance and importance of such results outside a fairly narrowly focused research community in graph theory. And it is certainly somewhat odd and disconcerting to encounter a proposal that states, effectively, "we are going to formulate some conjectures, and then try to prove them, although we don't yet know what these conjectures will be".

Methodology

The methodology does seem feasible. Indeed evidence is presented, in the [proposal], *to suggest that some success has already been achieved using just such methodology.* [In Section 9.4], *it is stated that "By applying the computer program to basic graphs, we found that...". This raises the question as to how much of the computer software that the project is supposed to develop in fact already exists, and this point requires clarification.*

Adventure in Research

There is inherent risk here. It cannot be known in advance whether useful conjectures will emerge from the application of the proposed software, and certainly when the nature of such conjectures is unknown there can be no guarantee that proofs of the conjectures will be forthcoming. However, regardless of the success or otherwise of the proposed methodology, it is hard to envisage an outcome that could be objectively described as a 'major advance in knowledge'.

Assessment of the overall research quality: Unsatisfactory, Adequate, Good, Outstanding

My confidence level: Low, Medium, High

Research Impact

Results that are likely to emerge from the proposed research would be of interest to pure mathematicians working in a relatively specialised area of graph theory. The impact on the wider academic community would be slight, and on the industrial / non-academic sector negligible. The proposer claims that the results would benefit "all academic and technological organisations interested in computer science", and "may well be useful in solving industrial problems...". These are frankly ridiculous overstatements of the likely impact. There is little or no computer science involved here, and absolutely no evidence of applications to real world problems.

Assessment: Low, Limited, Significant, Major

My confidence level: Low, Medium, High

Academic Collaboration

The proposal is centred around collaboration between the proposer and [the VR], *who is currently located at RUTCOR in the U.S. There has clearly been successful collaboration between them in the past, and it is likely that future collaborations would be equally productive. There is, however, no evidence that the proposed collaboration would bring in its wake any broader ties involving the departments/institutions concerned or other academics based at these institutions.*

Non-Academic Collaboration

Not applicable.

Dissemination

Proposed dissemination arrangements are satisfactory, comprising the usual submission of papers to reputable journals and presentation at national and international conferences (though the specific conferences mentioned are not necessarily of the highest rank).

Assessment of the collaboration and dissemination arrangements: Unsatisfactory, Adequate, Good, Outstanding

Planning and Project Management

The project management issues involved here are limited. The programme of proposed visits, and the timescale of the various activities seem reasonable.

Staff Resources, Equipment and Consumables

The proposed salary level for the visiting researcher seems fair, under the assumption that he would have no source of funds from elsewhere during the three one month visits to the UK. Travel and subsistence costs are not unreasonable. Consumable costs seem relatively high — for instance two printers seems excessive, and it is not clear what happens to the visiting researcher's high specification workstation during the time that he is not present.

Assessment of the management arrangements: Unsatisfactory, Adequate, Good, Outstanding

My confidence level: Low, Medium, High

Your Conclusions

It is not clear to this reviewer whether the proposal is to be considered by an ICT panel or a Mathematical Sciences panel. In my view the latter would be more appropriate. There is little in the proposal that could be described as computer science research.

The overall quality of the proposal: Unsatisfactory, Adequate, Good, Outstanding

I believe this proposal:
Should not proceed as proposed, Could proceed as proposed, Should proceed

Reviewer Expertise

Graph theory and graph algorithms.

Report 3

Response Due Date: 14 February

Research Quality

There are two aspects of this research. One is the plan to develop software tools to solve graph theoretical problems of a certain type, namely to find forbidden subgraph characterizations of hereditary classes of graphs. The other is to find, or conjecture about, classifications for some specific such classes.

The first aspect seems to be novel, and potentially of value if done well. However, none of the specific graph classes that the researchers propose to address are of much interest to the top researchers in the field.

Methodology

In the first phase, the applicants propose to use high-performance computers and purpose-built software to analyze all small graphs (up to 10 or so vertices) in the given class: this is surely feasible.

In the second phase, the applicants will try to make sense of the results and attempt to guess the characterizations on the basis of the data (and also, no doubt, of their understanding of the classes concerned). I am sceptical about how fruitful this will be in practice: the applicants cite a number of solved problems where the full characterization would have been revealed by their approach, but I strongly suspect these examples are atypical.

Proving any conjectures that suggest themselves is likely to remain challenging, although of course knowing what to try and prove is a crucial first step.

Adventure in Research

In terms of what might be done with any one pre-specified graph class, I think there is rather a small chance of learning anything significant; even if this could be done, it would not be regarded as a major advance.

The value is in designing research methodology that can be adapted to cover many different problems of this nature. Something important might well be achieved, either as part of this research or in the future.

Assessment of the overall research quality: Unsatisfactory, Adequate, Good, Outstanding

My confidence level: Low, Medium, High

Research Impact

As I've commented elsewhere, I don't think this research is likely to have much impact by itself in the graph theory community.

I found it striking that the applicants mention four candidates for classes of graphs to investigate (2-dominable graphs, pentagraphs, blue graphs, locographs), but they do not define any of these anywhere, let alone motivate their study, or refer to any previous interest in them.

Potentially the most useful output would be a software package written in such a way that, for any cleanly-specified hereditary graph class, information about small members of the class can be generated easily. This is indeed a natural first step.

Assessment: Low, Limited, Significant, Major

My confidence level: Low, Medium, High

Academic Collaboration

The proposal is to continue an existing successful collaboration, and the value is clear.

Non-Academic Collaboration

None proposed.

Dissemination

Publishing results in academic journals is normal and proper. I would hope that any software developed would be made available to the academic community; I don't see this mentioned in the proposal.

Assessment of the collaboration and dissemination arrangements: Unsatisfactory, Adequate, Good, Outstanding

Planning and Project Management
Have appropriate project management plans been specified, including realistic timescales and milestones? *Yes.*

Staff Resources, Equipment and Consumables
The sum requested seems realistic for the proposed research.

Assessment of the management arrangements: Unsatisfactory, Adequate, <u>Good</u>, Outstanding
My confidence level: Low, <u>Medium</u>, High

Your Conclusions
Both applicants are very prolific in terms of numbers of papers written.

The overall quality of the proposal: Unsatisfactory, <u>Adequate</u>, Good, Outstanding
I believe this proposal:
Should not proceed as proposed, <u>Could proceed as proposed</u>, Should proceed

Reviewer Expertise
Combinatorics and graph theory.

9.8 PI's Response to Reviewers' Comments

Reviewer 2
And it is certainly somewhat odd and disconcerting to encounter a proposal that states, effectively, "we are going to formulate some conjectures, and then try to prove them, although we don't yet know what these conjectures will be".

The reviewer uses the quotation marks, but the proposal does not contain the above statement. Although the exact formulation of the conjectures is not known yet, it follows easily from the proposal that the conjectures are of the following form: if a graph G does not contain induced subgraphs $H_1, H_2, \ldots, H_k$, then G belongs to one of the classes. The graphs $H_1, H_2, \ldots, H_k$

must be found by a computer. As described in the proposal, the similar research has been successfully carried out and the results have been published in highly reputable journals.

It cannot be known in advance whether useful conjectures will emerge from the application of the proposed software, and certainly when the nature of such conjectures is unknown there can be no guarantee that proofs of the conjectures will be forthcoming.

The nature of the conjectures is known, see above. Also, as discussed in Section 9.4, there is no guarantee that all formulated conjectures will be proved, taking into account that the duration of the project is only one year.

There is little or no computer science involved here, and absolutely no evidence of applications to real world problems.

Developing computer codes is one of the measurable objectives of the project. This will include developing various graph algorithms, for example for isomorphism testing.

It is well known that hereditary classes are important and they can be used, for example, in VLSI design. Of course, when a theory is developed some particular results may not have obvious direct practical applications. The famous Kuratowski's characterisation of planar graphs is not directly used in printed circuit design, but this result can be used, for instance, for developing important algorithms for recognising planar or toroidal graphs.

Reviewer 3

However, none of the specific graph classes that the researchers propose to address are of much interest to the top researchers in the field.

The reviewer speaks on behalf of all the top researchers. However, there are prominent researchers who are interested in some of those classes. (The name of such a researcher was given in the original document.)

I am sceptical about how fruitful this will be in practice:

the applicants cite a number of solved problems where the full characterization would have been revealed by their approach, but I strongly suspect these examples are atypical.

They are not atypical. The problem is that such characterisations require much effort including development of computer codes, and hence few full characterisations are known.

I found it striking that the applicants mention four candidates for classes of graphs to investigate (2-dominable graphs, pentagraphs, blue graphs, locographs), but they do not define any of these anywhere...

We agree with the reviewer that the definitions had to be included, despite the guidance notes clearly saying that the proposal should be "uncluttered with technical jargon".

Potentially the most useful output would be a software package written in such a way that, for any cleanly-specified hereditary graph class, information about small members of the class can be generated easily.

We actually generate small members not belonging to the class.

I would hope that any software developed would be made available to the academic community; I don't see this mentioned in the proposal.

Such software is not among the objectives of this project because we will develop computer codes for specific classes, and new codes will be required for each new class. Therefore, we will only publish corresponding methodology/technique, but the module developed will be part of a Windows application for graph theory in future.

9.9 Summary of Key Lessons

Key lessons learnt from the proposal are summarised in Table 9.2. It should be taken into account that the reviewers do expect to see "a major advance in knowledge", so if a proposal promises to tackle rather minor and unknown problems, it has no chance for funding. Another mistake is not providing all necessary definitions of basic terms, etc. If a proposal promises to develop a computer system or something similar and some part of the system already exists, then this must be carefully explained.

Table 9.2: Key Lessons.

Strengths	**Weaknesses**
The research quality is high standard.	There is a question mark over the likely significance and importance of such results outside a fairly narrowly focused research community.
The first aspect (software) seems to be novel, and potentially of value if done well.	None of the specific graph classes that the researchers propose to address are of much interest to the top researchers in the field.
Both PI and VR are well-equipped to carry out the task.	The applicants cite a number of solved problems where the full characterizations were revealed by their approach, but these examples are atypical.
The proposed research is certainly feasible even though the project will last only one year. The methodology does seem feasible.	How much of the computer software that the project is supposed to develop in fact already exists? The applicants mention four candidates for classes of graphs to investigate, but they do not define any of these anywhere.
The research risk is minimal. The value is in designing research methodology that can be adapted to cover many different problems of this nature.	There is inherent risk here. It is hard to envisage an outcome that could be objectively described as a 'major advance in knowledge'.
The impact to the academic community working in graph theory will be significant-to-major.	The impact on the wider academic community would be slight, and on the industrial / non-academic sector negligible. There is absolutely no evidence of applications to real world problems.
The collaboration with the VR is very important in carrying out the proposed research.	There is no evidence that the proposed collaboration would bring in its wake any broader ties.
The arrangements for dissemination are well-thought.	Will any software developed be made available to the academic community?
The project management is well-planned; the timescales and milestones are realistic. The requested resources are justified; the project is definitely value for money. The salary for the VR seems fair.	Consumable costs seem relatively high — for instance two printers seems excessive, and it is not clear what happens to the visiting researcher's high specification workstation during the time that he is not present.

List of Abbreviations

2D	Two Dimensional
3D	Three Dimensional
ACO	Ant Colony Optimisation
AMPL	A Mathematical Programming Language
ANP	Analytic Network Process
APMOD	Applied mathematical programming and modelling (series of conferences)
BCC	British Combinatorial Conference
BIM	Building Information Modelling
CO	Carbon monoxide
CO_2	Carbon dioxide
Co-I	Co-Investigator
CPLEX	Optimisation Software Package
CUBER	Critical Urban Buildings Emergency Response (research project)
C/C++	Programming Language
DSS	Decision Support System
EM-DAT	Emergency Events Database
FET	Faculty of Environment and Technology (UWE)
GRALE	Graph Language
Gurobi	Commercial Optimisation Solver
HEA	Higher Education Academy
ICT	Information and Communications Technology
INGRID	Interactive Graph Invariant Delimiter
IP	Intellectual Property
LP	Linear Programming
MOD	Ministry of Defence
MOSP	Multi-Objective Shortest Path (problem)
OpenGL	Open Graphics Library
OR	Operational Research
PhD	Doctor of Philosophy

PI	Principal Investigator
PPM	Public Performance Measure
QAP	Quadratic Assignment Problem
RA	Research Assistant
RF	Research Fellow
RGMA	Research Group in Mathematics and its Applications (University of the West of England, Bristol)
RUTCOR	Rutgers Center for Operations Research, USA
ScienceDirect	Database of scientific and medical research
SCOPUS	Elsevier's abstract and citation database
Simul8	Simulation Software
SO	System Optimal
TSP	Travelling Salesman Problem
UE	User equilibrium
UEA	University of East Anglia
UWE	University of the West of England, Bristol
VLSI	Very Large-Scale Integration
VR	Visiting Researcher
WP	Work Package

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