Responses to the comments made by Prof. John Young

I would like to begin by thanking Prof. Young for taking the time and effort required read and asses this theses. As per the recommendations made by Prof: Young major revisions have been made in this revised submission.

Overall the shortcoming of the thesis pointed out by both examiners was the clarity of the presentation. Thus, most of the chapters were re-written to make the arguments more clear. Chapter 5 of the previous submission was merged with Chapter 4 of the same submission and re-written. The typographical errors have been fixed.

**Abstract**

“Mixing past and future … Language needs to be corrected throughout”

It is conceded that the writing of the previous version of this thesis was not satisfactory. The thesis has been extensively restructured and re-written. The language throughout has been improved in an effort to convey the ideas of the thesis as clearly as possible.

**Table of contents**

2.2.7 "summery" should be "summary".

The typographical error was corrected

**Dedication**

"myslef should be "myself"

The typographical error was corrected

**Nomenclature**

“Seems out of order - why are c\* and Cy not placed between c and D near the top of the nomenclature? Normal practice is to group all English, then all Greek, in their own alphabetic orders.”

The nomenclature table was re-ordered in English and Greek alphabetical order.

**Chapter 1 – Preliminary Remarks**

p. 2: "interactions occurs".

The error was corrected.

“Vortex induced vibration (VIV) contrasted with fluid-elastic galloping. VIV is resonance when vortex shedding frequency aligns with structural natural frequency (lock-in). Does not sufficiently draw out the differences between VIV and FEG, reader would need to consult the quoted references to understand. If FEG is to be more fully explained later, this should be noted.”

It has now been noted in the preliminary remarks section of the thesis that FEG will be explained in the literature review section. The mechanism of FEG is fully explained in the literature review section. Kindly refer to page 3.

p.3 last full sentence: grammatically incorrect.

Phrase removed after rewriting

**Chapter 2 - Literature survey**

“why does the fluid have to be Newtonian? ”

Of course, the fluid does not need to be Newtonian. However, only Newtonian flows have been considered as part of this thesis.

p.5 Section 2.2.1: "crated" should be "created".

The error was corrected.

“ When a bluff body moves along the transverse direction of the fluid flow, it generates a force along the transverse direction. This force, also known as the induced lift" - wouldn't this be drag, not lift? Do you mean a force normal to the transverse direction, not along it? ”

As illustrated in figure 2.1 the cross section under the influence of galloping generates a force perpendicular to the fluid flow. In order to generate this transverse force the body should be oscillating in the transverse direction of the fluid flow. For bodies under the influence of galloping this force is in the same direction as the transverse velocity. Here, the transverse direction is defined as the direction perpendicular to the direction of the flow.

p.6 Figure 2.1: If the body is moving downwards (ydot positive, as stated in the text), then wouldn't the effective angle of attack on the body (which is an angle by definition measured in the reference frame of the body) have the opposite sign to that shown in the figure, and the lift would oppose the motion (i.e. the lift force would be 180 degrees out of phase with the motion, not in phase with it as stated in the text)?

Taking the downward direction as positive means that a positive angle of attack is given by a clockwise rotation (pointing the front of the body “up”).

As illustrated in figure 2.1 the cross section under the influence of galloping generates a force perpendicular to the fluid flow. In order to generate this transverse force the body should be oscillating in the transverse direction of the fluid flow. For bodies under the influence of galloping this force is in the same direction as the transverse velocity. Therefore, for galloping to occur, pointing the front of the body up needs to produce a lift force down, which is the opposite direction to what would be expected for an airfoil – hence why airfoils are not susceptible to transverse galloping. Hence the downward direction is taken as positive.

Section 2.2.1: Galloping is not adequately explained here, and the differences between galloping and VIV are not further developed

Further explanation on the mechanism of galloping has been added in the amended thesis kindly refer to section 2.1.1 (page 5). Further explanation on the difference is provided in section 2.1.3 (page 13)

p.7 Section 2.2.2: Jumps to a reference to Figure 2.2 with no explanation of what is being compared, why, and how the results were obtained. Needs to be made much clearer that this is a figure reproduced from Parkinson and Smith, for the purpose of showing that the quasi-steady model can achieve results close to experiments under certain conditions.

I thank the examiner for identifying this error. The error was identified and an explanation for the results of Parkinson and Smith was added, kindly refer to page 7.

p.7: "...stationary Cy data (which consists of both lift and drag components)": this statement is difficult to understand, given that equation 2.1 is specifically modelling motion in the y direction only. Are lift and drag measured in directions that are normal and parallel to the free stream velocity, or to the instantaneous effective velocity experienced by the body? This needs to be made clear.

The Quasi-Steady State model assumes that the instantaneous induced lift force of the oscillating body is equal to that of the lift force generated by the same body when static at the same induced angle of attack.

Even though the body is moving transversely (in the y direction), the frame of reference changes as the lift force of the corresponding static condition is considered. Hence, Cy consists of both lift and drag data.

p.7: How is the Cy data (which is to be interpolated) obtained? Equation 2.3 just creates a form of interpolating function, but does not define the data itself (i.e. how are coefficients a1 to a7 determined)? Use of the term "interpolating" is probably not appropriate here, this appears to be a 7th order polynomial curve fit to the data, which will not pass through every data point exactly (which the term "interpolating" implies).

The raw Cy data is obtained through simulations or experiments of the static body at incrementing angle of attack. The examiner is correct, the polynomial is not interpolating, but is a curve of best fit in the least-squares sense. The term "interpolation polynomial" was changed to “Curve fit ” kindly refer to page9

p.9: "vortex shedding will be correlated well": correlated with what? Do you mean well developed?

Vortex shedding is correlated along the span at the laminar region. This was not clearly written the in initial submission and was corrected. Kindly refer to page 9 of the amended thesis.

“p.9: What is hysteresis in a galloping context? What is its significance, i.e. why should the reader be interested in whether or not hysteresis occurs? This sort of background must be explained in a thesis, not left as assumed knowledge as may be done in a journal article.”

Two outputs can be produced for the same input in the hysteresis region. Thus this fact is quite vital for energy harvesting as two values of energy levels can be present for the same reduced velocity. The examiner is correct the lack of information of this section was identified and an explanation was added in the amended thesis please refer to page 10.

p.11 Section 2.2.3 "Galloping is governed by the the shear layers created at the leading edge due to flow separation on the top and bottom corners of the bluff body." (note two "the" in the text): this sort of explanation of the phenomenon should be up front in Chapter 1, not buried down in the literature survey. How is this different from VIV, which is also governed by the shear layers leading the formation and shedding of an alternating vortex wake?

VIV and galloping are both governed by the shear layers. However, VIV is sustained through an alternating rolling up and shedding of the shear layers, where galloping is sustained through the difference of proximity of the *mean* shear layers to the body occurred when the body is in motion.

The point regarding the placement of this explanation is acknowledged; however, the author feels that the explanation of this phenomenon is more suitably presented in the literature review to maintain the overall flow of the thesis. Similarly, the QSS model and the underlying theories are introduced with literature in the literature review section.

“p.11 Figure 2.4: is this based on your own results (if so how were they obtained), or is it extracted from the literature (in which case it should be referenced)? If these are your own results, then they are out of place as you have not yet described your methodology, they belong somewhere other than in the literature survey.”

The illustrations which now has been changed to contours of shear strain rate magnitude, was used for illustration purposes to show the wall jets created in the leading edge of the body, to present the reader a better understanding on how the shear layer behaves.

The image is taken from simulation results from the author, as no pre-existing image in the literature completely conveyed the overall concept being discussed. The figure is now correctly attributed, please see page12.

p.12 Section 2.2.4 "It is clear that the cyclic motion of the shear layer harmonize with the mechanical system.": Under what conditions is this statement true? For all flows, body shapes, Reynolds numbers, structural parameters? Just the ones shown in Figures 2.3 and 2.4? This needs to be explained.

Galloping occurs when the transverse forcing Fy is in phase (in the same direction) with the motion of the body. The transverse force is generated through the proximity of the shear layers to the body. The proximity of the shear layers alternates in harmony with the motion of the body. Thus for any system that is galloping the cyclic motion of the shear layer has to be in harmony with the mechanical system of the body.

The statement is changed to “It is clear that the cyclic motion of the shear layer will harmonize with the mechanical system of a body under the influence of galloping.” Please refer to page 14 **.**

p.12: "galling" should be "galloping"

p.12: "hight" should be "height".

p.13: "Anther" should be "Another".

All three typographical were corrected.

Figure 2.5: reproduced from Paidoussis (2010), but with different terminology to that used here - CFy instead of Cy, alpha instead of theta (presumably), should be explained in the caption.

The examiner is correct. This shortcoming was identified and an explanation is added in the caption of the figure in the amended thesis (figure 2.6, page 8)

Section 2.2.6: The findings of the two main references cited here (Barrero-Gil, Vicente-Ludlam) are not discussed in sufficient detail.

An explanation is added to mitigate this shortcoming in section 2.17 in the amended thesis kindly refer to page18.

p.16: "equation 3.1" possible misprint, should be "equation 2.1"? If so, I cannot see how this equation could be used to draw conclusions about delayed shear layer reattachment. If not, it is not appropriate to use an equation that has not yet been seen by the reader as justification for an objective of the thesis, it must have been discussed in the introduction or literature survey.

The examiner is correct. This equation reference was incorrect and has been fixed please refer page 46.

**Chapter 1 and 2 overall comments:**

These two chapters do not adequately set the scene for the remainder of the thesis, as they do not fully explain the background to the project and galloping to a reader unfamiliar with the phenomenon. Rather it is discussed piecemeal. For example, Figure 2.5 appears to give the criterion under which galloping can be expected (dCFy/dalpha > 0) for a range of shapes, yet this criterion is not mentioned or explained in the text in Chapters 1 or 2. The objectives (Phase 1) use parameters (damping ratio, reduced velocity) that have not yet been introduced in the literature survey, so a case that their use for fluid-elastic galloping is not appropriate, cannot possibly have been made.

The author thanks the examiner for pointing out the shortcomings of the two chapters. Overall the explanation in these two chapters was inadequate and necessary corrections have been made.

For example, the criterion dCFy/dalpha > 0 has explained as this explanation was lacking in the initial submission (refer page 15 and 16).An introduction to the classical governing parameters has been added (page 13 and 14).A sketch of the shear layers and their relation to the geometry has been added (figure 2.4 page 12) for better illustration of a galloping system.

**Chapter 3 - Methodology and Validation**

p.17: "two directional" should be "two dimensional"?

The examiner is correct. The phrase is two dimensional this phrase was removed after rewriting.

“p.18: Re = 200 was selected for the low Re regime, based on the 3-dimensional transition in the wake of a square cross section being at Re = 160. This implies that the flow regime being studied is 3-dimensional, but on p.17 the statement is made that the low Re range was intended to be where the flow was laminar and 2-dimensional (at least that is what I assume, see previous comment). These two statements seem to be contradictory.”

The author agrees with the comment from the examiner; a clearer explanation is needed here. The following explanation was added in the amended thesis.

“Leontini et al. (2007) concluded that the oscillation of the bluff body essentially stabilizes the wake, for example the the limit of three-dimensional transition of an oscillating circular cylinder can be as high as Re = 280, compared to the transition Reynolds number of Re≃190 for a stationary cylinder. As the essential flow physics such as the formation of the Karman vortex street is common for both a circular and square bluff body, it can be assumed that the wake is also stabilised for oscillating square cross sections. Thus, Re = 200 was selected as the Reynolds number for the “low” Reynolds number region as a compromise between keeping the flow strictly two-dimensional, and providing a high enough lift to generate vigorous galloping.”

p.18: Parameters such as Re, mass ratio and U\* are chosen to be the same as previous works in the literature, but what are the physical justifications for these parameter choices? Why did these previous authors choose these parameter values?

Considering previous studies (Robertson et al., 2003; Joly et al., 2012) m∗was kept at m∗= 20 which was a level of inertia not so high as to suppress galloping and not so low for vortex shedding to dominate and weaken galloping as observed by Joly et al. (2012). The reduced velocity U∗was kept U∗≥ 40 to keep the natural frequency of the system far from the frequency of vortex shedding to ensure that the primary mode of flow-induced vibration was galloping as opposed to vortex-induced vibration (VIV).

The above explanation was added in the amended thesis kindly refer to page 23.

p.21: "and the dynamic by" should be "and the dynamic viscosity by", "velocity vector filed" should be "velocity vector field" (this error is made multiple times in the following paragraphs).

The author thanks the examiner for pointing out this error. The error has been corrected.

p.24: "The velocity of the cylinder is in advance by half a time-step of the position of the cylinder", and "However, both the cylinder positions and velocities are located at the same discrete times". I'm not sure I understand what these two sentences are saying, as they seem to be contradictory.”

The sentence has been removed as it created unnecessary confusion, and was not strictly accurate. The body position is strictly at some time between the start and the end of a step, as it is calculated as part of the convection substep equation (equation 3.8, page 28). A fuller explanation of the time splitting scheme leading to this difference has been added, including a new figure (figure 3.1 page 29).

“p.24 Eqn 3.13: What is N in this equation? It is not in the nomenclature, or in the text prior to the equation. Same question for Eqn 3.16.”

N is the non-linear convection term in the Navier-Stokes equations. The examiner is correct, this term was not mentioned in the nomenclature or prior to the equation and the term has been added in the nomenclature.

p.25: "carried ou" should be "carried out", "subsetep" should be "substep".

The above errors were fixed.

Section 3.4.2 Convection substep: the entire process described here would benefit from a graphic showing the various steps.

An illustration of the sub-steps has been added to gain a clear picture of the numerical scheme in section 3.4.2, kindly refer to page 29.

p.26: "na d" should be "and"?

p.27: "spacial" should be "spatial".

Both typographical errors were fixed.

p.28: "special coordinate" should be "spatial coordinate"?

The examiner is correct. The error was fixed.

p.28 onwards: "could be" probably better replaced by "may be" or "can be", which does not have the implication of speculation. This change should be made throughout the thesis.

The author thanks the examiner for bringing this error to notice and it is rectified throughout the amended thesis.

“p.31: Neumann boundary condition applied for velocity at the downstream exit, assumes flow does not spatially evolve while exiting the domain. It has been noted that this is not true for a vortex wake generated aft of a bluff body where there is significant spatial and temporal evolution, but that low Re and distant boundaries ameliorate the problem. Do you have any evidence or literature references to support this statement? While I am confident that the values used (20D upstream and laterally, 60D downstream) for domain are sufficient, I would like to see some verification in the form of a domain size variation study to back it up with evidence.”

The author agrees with the examiner. Although the physical validity of the outlet boundary condition is not quite true, this does not turn out to be a significant problem provided that the domain outflow is sufficiently far away from the body.

Leontini and Thompson (2013) was used as the primary reference for the numerical domain in the present study where the trailing part of the domain was increased to capture the long wave lengths associated with the low flow frequencies of galloping. This selection was done for two reasons. The first reason was that both Leontini and Thompson (2013) and the present study were carried out using the same numerical solving code. The second reason was that the cross sections used in both studies are similar. Thus, further optimisation of the domain need not be carried out as Leontini and Thompson (2013) has already shown this domain to be adequate for this class of flows.

The explanation was not provided in the initial submission and it is added in section 3.4.3 of the amended thesis kindly refer to page 38.

“p.32 Convergence: this section aims to establish the grid independence of solutions, however only p convergence has been conducted, not h-p convergence as discussed earlier in the text. How did varying the spatial refinement of the grid vary the results? What evidence do you have that you have adequately captured the details of the shear layer both spatially and temporally, given the importance of predicting the separation point and subsequent formation and evolution of vortices convecting into the wake? The time step is reduced in conjunction with increasing order p to keep the Courant number low (a numerical stability criterion), but what evidence do you have that this time step is small enough to capture the temporal flow physics? Overall the validation section seems rather cursory.”

The author apologizes for clearly mentioning this point in the initial submission. As mentioned earlier the current domain and the spatial and temporal parameters were obtained from Leontini and Thompson (2013). Thus, in that study it was established the domain size was adequate. However, what the author has tried for the current study is to obtain a converged value for the average velocity amplitude. This fact is quite vital as the velocity directly affects the mean power and a difference of less than 1% was achieved for both mean velocity amplitude of the body and galloping frequency using these spatial and temporal parameters.

The author agree with the examiner more DNS simulations would be preferable, however, as galloping is a low frequency phenomenon, a longer time is taken to achieve the steady oscillating state. Furthermore, as galloping is dependent on the initial excitation of the flow, the initial development of galloping takes a significant amount of time. Both of these factors result in long computation times in excess of 2 weeks per run.

These explanations have been added in the amended thesis kindly refer to pages 38-41.

**Chapter 4 - Governing Parameters of Fluid-Elastic Galloping**

Figure 4.1: Cy data here is time-averaged, given that the flow is unsteady (oscillatory) even for a stationary body. What are the implications of this?

The quasi-steady state model assumes the instantaneous induced lift force of the oscillating body is equal to that of the lift force generated by the same body when static at the same induced angle of attack. Thus, stationary Cy data are used as the inputs for the QSS model. The main implication of this is that there needs to be a distinct separation of timescales – the galloping motion needs to be much slower than the vortex shedding. This is typically the case, as long as the system is not to stiff, meaning high U\*, or low Π1.

Multiple spelling mistakes, e.g. "dimenasional", "ussed".

The typographical errors were corrected.

p.40: "It is assumed that the stiffness plays a minor role" Explain why.

As explained above, “pure” galloping is low frequency compared to the vortex shedding, and therefore has long time periods. This essentially results from a low value of the spring stiffness. Therefore, it can be assumed that stiffness is low, and the spring forces do not play a controlling role in the dynamics.

p.42: Argument presented in Section 4.3.1 seems somewhat simplistic to say that PI\_1 "does not have a significant influence on the behaviour of the system". The amplitude of the oscillation A/D has not collapsed against PI\_2 in the same way the velocity and power have, in Figure 4.2, but this seems to have been glossed over in the discussion in the text.

The displacement amplitude does collapse with a scaling parameter consists of both PI\_1 and PI\_2. This scaling parameter essentially reduces to the inverse of the damping ratio. The comment by the examiner’s was taken into account and the discussion regarding amplitude of the system has been added in 4.13 (refer page 49).

p.43: "An example case is presented in Figure 4.2", do you mean Figure 4.3?

The examiner is correct. The error was corrected.

p.45 Figure 4.5: This figure plots Pd and Pt. Neither of these quantities have been described in the text in any manner before this figure, just in the nomenclature. How are they calculated? Both of these should be explained in the background.

The author thanks the examiner for pointing out this shortcoming. The explanation regarding Pt and Pd were added kindly refer to page 51 of the amended thesis.

p.47 Figure 4.6: Caption states that "mass ratio does not have an effect on PI\_1 even at low PI\_1". Do you mean does not have an effect on the power Pm?

The examiner is correct. The corrections has been made to figure 4.7 in the amended thesis (refer page 55).

p.51: "Indecates" should be "indicates".

The error was corrected

p.51: Regarding figure 4.10 and the associated discussion, it might be worthwhile adding another figure plotting Cy and its spectrum, to draw out the difference between the occurrence of vortex shedding (shown in the Cy plots) versus the influence it has on the body (shown in the V/U plots) as the mass ratio increases. Figure 4.10 caption should explain terms fg and fs

The plots of Cy and its spectrums were added taking the examiner’s comment (refer page 61). This figure also clearly shows how the influence of vortex shedding decreases as PI\_1 increases.

The captions were also amended explaining fg and fs (figure 4.11 page 60) which are the galloping and vortex shedding frequencies respectively.

p.51: "relative intensity of the component at the vortex shedding frequency to the component at the galloping frequency" - firstly what do you mean by "experimentally" shown? Is this based on experimental data, or on your DNS data? Secondly, use of the word "component" is confusing here - this implies a component of a force or other vector, I suggest rewording to make clearer.

The word “experimentally” was an error it has been corrected to “explicitly”. The phrase "relative intensity of the component at the vortex shedding frequency to the component at the galloping frequency" was changed to “relative contribution of vortex shedding in the galloping system” to imply the influence of the vortex shedding in the galloping system. (page 62)

p.53 Figure 4.11: I am not completely convinced that you have plotted what the vertical axis says, i.e. the relative power of the vortex shedding, as power is force by velocity and the force has not entered into the calculations here, just the velocity (see previous comment regarding Figure 4.10). However this figure does seem to show convincingly that the behaviour of the error in Figure 4.9 is closely matched by the behaviour of the quantity plotted here, i.e. as the motion of the body shows increasing high frequency motion on top of the low frequency galloping, the error increases.

The author agrees with the examiner the term “relative power” is not suitable in this context. Thus, the phrase was to changed to “relative contribution of vortex shedding” which is essentially the ratio between the intensity of the vortex shedding frequency and the intensity of the galloping frequency in the power spectrum plot. (refer page 62)

p.53: "The relative strength of the vortex shedding" - again, I believe it is not the strength of the vortex shedding that you are referring to here, but rather the strength of the response of the body to that vortex shedding.

The examiner is correct. The sentence reworded “magnitude of any vortex shedding correction term that might be added to the QSS model in an effort to decrease the discrepancy between it and the DNS simulations.” Kindly refer page 62

p.53: "Though it is unequivocal" - I think you mean "not unequivocal"?

The author is correct. The error was corrected.

p.54 Figure 4.12: are vorticity levels non-dimensionalised? If so how? This figure serves to show that vortex shedding is just as prevalent at PI\_1 = 1000 as it is at PI\_1 = 10, reinforcing the previous comments that what you have plotted in Figure 4.10 with V/U time histories is not the strength of the vortex shedding itself, but the response of the body to that shedding.

The vorticity levels are non dimensionalised using U/D. The examiner is correct. Indeed, V/U time histories is not the strength of the vortex shedding itself, but the response of the body to that shedding. This figure gives an indication of the influence of vortex shedding on a galloping system as PI\_1 is varied.

**Chapter 5 - Frequency Response of the System**

As the examiner has pointed out this chapter was very short. Essentially the frequency study was carried out to study the influence of PI\_1 and PI\_2 on the galloping frequency. Thus, the data and the analysis of this chapter was merged with the chapter “**Governing Parameters of Fluid-Elastic Galloping”**

As the examiner has also pointed out, the analysis of this section was not adequate. Thus, a clearer presentation of data and discussion was added.

The major objective of this section was to investigate the influence of PI\_1 and PI\_2 on the galloping frequency. Thus an expression was obtained for the frequency in terms of PI\_1 and PI\_2. The frequency data obtained using the QSS model, linear frequency and DNS simulations were compared.

Kindly refer to pages 64-73 for the results and discussion of this section.

p.60: "Early deviation of f\_DNS could be due to..." - has this been further investigated or is this speculation? What other forms of forcing?

The chapter has been significantly rewritten as section 4.2 to be clearer – this particular argument is no longer present.

Figure 5.1: Caption says PI\_1 = 0.15, should this be PI\_2 = 0.15?

The figure has been significantly updated (now figure 4.15) and the caption is now accurate.

Figure 5.2: It is a minor point, but why change the x-axis notation compared to Figure 5.1? They are both on a log scale, and that of Figure 5.1 is probably preferable. In trying to reconcile Figure 5.2 with the data presented in Figure 5.1, I was confused - Figure 5.2 shows that f\_lin becomes much smaller then f\_QSS, but this is not the case in Figure 5.1 (hence the previous question about which value of PI\_2 is shown in Figure 5.1).

Figure 5.2 and 5.3, and 5.4 should be plotted on the same vertical scale.

Figure 5.3 shows that for PI\_2 = 0.15 and Pi\_1 = 1.0, f\_lin/F\_DNS = 0.93, i.e. f\_lin < f\_DNS. However Figure 5.1 shows f\_DNS dropping off much lower than f\_lin. Similarly, in Figure 5.2 the contour scale is from 0.05-0.95, but in Figure 5.3 it is 0.91-0.99. These figures seem difficult to reconcile with the behaviour shown in Figure 5.1. Please provide some additional commentary in the text to explain.

Could the presentation of the data be made more uniform by having all of Figures 5.1 through 5.4 plotted with a common denominator, i.e. the natural frequency f, rather than a mix of f, f\_QSS and f\_DNS?

All of the above points have been addressed by restructuring the data. The new figures 4.19, 4.20 and 4.21 present the various frequencies nondimensionalised by the inverse timescale m\*U/D. No ratios are presented to show the true trend of the frequencies as a function of PI\_1 and PI\_2. They are presented on commonly scaled axes and use a very similar colour map. In this way, the contour plots are much easier to reconcile with the data of the original figure 5.1 (now figure 4.15), and to cross-compare the results of the various frequencies. Figure 4.22 then presents the ratio of f\_lin/f\_DNS to give some indication of how well the simple linear frequency predicts the true measured frequency.

p.62: "grater" should be "greater".

The spelling has been fixed during the rewriting of this section.

Figure 5.5: Caption grammar needs to be corrected.

This figure has been removed.

p.64: "an in-depth analysis should be carried out" - I believe that analysis is within the scope of this thesis, and should be carried out by the author rather than remaining a recommendation for future work. This section presents data, but does not provide significant analysis. What are the implications of the results presented, relating back to the literature survey? This chapter is very short and would likely not constitute a journal article in its own right.

The new section 4.2 goes to some effort to try to extend the analysis from what was originally presented. The comparison of the various frequencies gives some indication of where nonlinearities are playing a role by showing where the linear frequency does not match closely with the frequency measured during DNS.

The new section is now able to show the parameter values where the use of the linear prediction is valid – this is a useful conclusion from a design perspective, as it shows where this very lightweight approach can be used to predict the galloping behaviour of the system.

**Chapter 6 - Influence of Fluid Dynamics of the System on the Extracted Power**

p.66: The reasons for the choice of the cross section geometry are not really explained. Why a hybrid square and triangle, and not some other shape? How does the presence of a sharp corner at the join of the square and triangle affect the behaviour of the shear layer? Does this fix the separation point, and thus limit the variation in behaviour that may be observed?

The examiner is correct. This information was lacking in the initial submission. The key facts which lead to the cross section geometry were identified and presented in section 5.2 (page 80). Basically the body needs a bluff front face with sharp corner to force separation, an initially straight or streamlined section to begin to bring the separated shear layer towards the body, and then a surface the moves towards the centre to delay the reattachment of the shear layer. The presence of the sharp corner at the join of the square and triangle does have an effect in inhibiting the shear layer reattachment, but not on the separation point (this is fixed to the sharp corner at the leading edge).

p.66: In what sense is the cross section identified from the QSS power data optimum? Best of all possible cross sections? Or best of the ones tested?

The optimum cross section was identified as the best of the cross sections tested. However, the main objective of this chapter was to test the possibility of achieving higher power output through inhibition of the shear layer re-attachment (refer page 95). This was shown only for the family of cross sections tested, but it is hoped that this hypothesis is generalizable to other body types.

p.67 Section 6.3; What Reynolds number was used to obtain the DNS results? Presumably the DNS results would not have been steady, but the flow would be oscillatory (due to vortex shedding), so are the stationary Cy results time averaged? If so what are the implications of using such time averaged data?

The Reynolds number was not mentioned in the introduction of this chapter in the previous submission. The DNS results were obtained at Re=200. This information was added in section 5.1 (page 77) in the amended thesis.

The examiner is correct, the stationary body Cy data are time averaged in order to filter out the effects of vortex shedding. This is consistent with the quasi-steady assumption.

p.68: Where the Cy values become negative, will this result in a transfer of energy from the body to the fluid and reduce mean power, as suggested, or would this in fact preclude galloping altogether (i.e. it would not be able to establish itself under these conditions) leading to zero power output?

In fact, both statements from the examiner are correct. When the range of angles over which Cy is negative is large, the mean power is reduced as shown in the results of section 5.4. If this range becomes so large that most angles give a negative Cy, galloping will not be established.

p.70: The discussion regarding Figure 6.3 states that decreasing d/l delays the shear layer reattachment. Do the DNS simulations support this statement? Again presuming that there is vortex shedding occurring (at Re = 200?) is this referring to in a time-averaged sense? What would the physical reasons be for the delay in reattachment be as d/l is decreased? Are there situations where the shear layer does not reattach to the body?

The examiner is correct. DNS simulations support this statement. The phrase “delaying shear layer reattachment” creates some confusion. This phrase was changed to “inhibit the shear layer reattachment” (refer page 91).

Yes, this refers to in a time averaged sense. When shear layers of a galloping system is discussed it is generally referred to a time averaged sense where the vortex shedding is filtered off. Galloping occurs on frequencies far lower than vortex shedding frequency. Therefore, the vortex shedding is generally filtered out when the shear layer behaviour of a galloping system is discussed. The correct phrasing should be “inhibition of the shear layer reattachment”, this was corrected throughout chapter 5.

p.71: The last sentence on this page is arguable at best. The pressure at a given point in the flow will depend on the static pressure, dynamic pressure (i.e. the velocity) and where it is being measured. So raising the velocity can easily result in a higher pressure at some locations (e.g. a stagnation point). You should be much clearer what you mean by this statement. Do you mean that within a given flow, if the pressure is lower then the velocity would be expected to be higher, via Bernoulli's principle? As you have alluded to, this would not necessarily apply in shear layers where there is significant viscous dissipation negating the assumptions of Bernoulli.

This section was re written as previous discussion was not clear. Kindly refer to section 5.52 (pages 85-97) in the revised thesis.

p.72 Figure 6.4: While these surface pressure plots are useful, it would also be illuminating to see the entire pressure field (along with velocity fields as vectors or streamlines, and vorticity fields), for these same conditions, to build up a clearer idea of the flow physics in each case. Also why look only at the triangle here? Are the reasons for negative Cy on the triangle the same as the reasons for negative Cy on a square?

The examiner is correct in that pressure fields and the velocity fields will provide more data. However, even after extensive experimenting with contour levels, colour and line types, etc. an image that clearly showed the required features near the body could not be gained. Therefore, it was decided that the best way to present this argument is through the surface pressure on the cross section itself.

The triangle was taken as it contains the largest initial range of θ over which cy is negative. An initial negative Cy region of a square cross section is not present (refer figure 5.2 (a) page 6.2). As the discussion is about the initial negative region of the Cy curve the cross section which had the largest negative region was chosen for analysis.

p.73 Figure 6.5: this figure could be made clearer (e.g. indicate the flow direction).

The figure was made clearer by indicating direction of the flow. (refer page 87)

p.73: Here we have the first reference in this chapter to the fact that the DNS data (at least the velocity profiles) have been time averaged. See earlier questions on p.67 and p.70 about this, and the need to explain it earlier. Why was it necessary to time average (presumably because of vortex shedding)? Over what period of time was the data time averaged? What are the implications for the discussion up to this point in the chapter about the shear layer behaviour, and is it meaningful to only talk about time-average behaviour and not time-accurate vortex behaviour?

As mentioned previously time averaging was carried out over a vortex shedding cycle in order to filter out the effects of vortex shedding. Time averaging was not mentioned in the previous sections, however this has now been amended.

p.74 Figure 6.6: As for Figure 6.4, while these plots are interesting, I think far more information could be gleaned by also presenting the entire flow fields, not just velocity along single lines. Either velocity vectors (preferably) or velocity magnitude would be useful.

As mentioned earlier in response to the point regarding the surface pressure plots, it was not possible to gain a clearer picture through the entire flow fields. The author feels that the arguments are best presented through the current figures but with a better explanation which was done in the amended thesis, kindly refer to page 85.

p.75 Figure 6.7: These figures appear to show the limiting time-averaged streamlines that pass either side of the body from the stagnation point. More streamlines would be useful to interpret the flows here. Do they show that the shear layer on the top of the body does not ever reattach, and the one on the bottom reattaches close to or at the back of the body? It is difficult to understand your argument about delaying reattachment when the flow appears to be separated over essentially the entire body. I would like to see the entire wake, where I would expect to see the recirculation bubbles aft of the body in their entirety.

The author agrees that the argument was not clearly mentioned in this section. The intention of this section was to show the significant deviation of the QSS model and FSI simulations at lo d/1. According to the assumptions of the QSS theory the mean flow-fields between the stationary and FSI cases at the same induced angles should be approximately the same. It was shown even at 0°the two approximations are non-identical and hence the large deviation between the QSS and FSI results. The author feels that this can be clearly seen in figure 5.11 (page 95) hence, the figure was not changed.

p.76: "1 < d/l < 0.25" does not make sense, should be "0.25 < d/l < 1"? p.77: "flow-filed" should be "flow-field" throughout.

The errors were identified and corrected.

p.77: Why was the decision made to time-average the flow-field data over a vortex shedding cycle? Why not just present instantaneous results? This needs to be explained in the text.

The instantaneous results consist of effects of vortex shedding and galloping. In order to analyze the flow behaviour of galloping vortex shedding has to be filtered out and hence, the time-averaging over a vortex shedding cycle. This point was added in the amended thesis (refer page 92).

p.78 Figure 6.10: should have notation on the horizontal and vertical axes.

The horizontal and vertical axes were added refer figure 5.10 (page 93)

p.78: " This indicates that there is significant non linear forcing present as d/l decreases which could be a result of the higher induced angles and correspondingly larger transverse velocities involved." This is a speculative statement, and it would be more appropriate to determine definitively whether this is the case, by further investigation and analysis.

The author agrees with the examiner that the statement was speculative. As further investigations were not carried out due to time constrains of the research this argument was removed. This section was rewritten (refer section 5.62).

p.79: "It is clear that delaying reattachment of the flow would lead to higher energy output. " Please see earlier comments about what delaying reattachment actually means in your flows. Figure 6.11, like 6.7, does not to my mind clearly show the behaviour that you are trying to describe in the text, as there are separation bubbles that are the same size as the body. You should annotate the figures appropriately to explicitly point out the behaviour you are describing in the text, in both these figures (6.7, 6.11) at a minimum.

The intended message in this section was not clearly given in the initial submission. The section has been rewritten with better clarity kindly refer to section 5.7 in the amended thesis.

p.82 Table 6.1: last column (angles) is missing a heading.p.83 Figure 6.11 caption: time values should be presented non-dimensionally.

The column angles were added (refer page 82) and the time values were presented non-dimensionally (refer page 94).

**Chapter 7 - Conclusions**

p.87: " Understand the governing fluid mechanics of the system and to optimise and control these mechanics in order to obtain a higher power transfer." I do not believe it is accurate to say that you have optimised and controlled the fluid mechanics of the system. In Chapter 6 p.81 you state "Thus as a result an optimum d/l should be obtained in order to get a balance between the negative and positive regions which leads to an optimal galloping energy harvesting system." but you have not made a clear statement of what the optimum d/l value is, and there is no control exerted over the flow behaviour.

The author agrees with the examiner. This study did not obtain an optimum cross section but showed that a higher power could be gained through inhibition of the shear layer reattachment. The best cross section of those tested was identified (d/l=0.25). The conclusions chapter was re written to incorporate these points and all the other amendments made in this thesis. Kindly refer to Chapter 6 of the amended thesis (pages 98-101).