First Paper:
GT2004-53033
“*Constraint on Aeroengine Design by Loss of Grounding in Vibration Physics*”

Second Paper:
GT2004-53034
‘Prediction of Turbofan Vibrations via Distinct Waves”

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My early work on design of jet engines impressed on me that success often depended on taming vibrations. But even today, practical design analysis that would help to propel the now mature industry to greater advancements is still missing.

Concern Over Engine Vibrations

“.... forced response prediction ...... to avoid the **late discovery of High Cycle Fatigue problems**. Indeed, in the current design process, engine manufacturers know the vibration levels only at the very end of the development, during engine testing, or sometimes in operation.”

GT2004-53372, “**Forced Response Prediction - Methodology For The Design Of HP Compressor Bladed Disks,”** Eric Seinturier and team from SNECMA Moteurs

If we look at the abstracts of papers that have been offered to this Conference, we see that the old problem of engine vibrations still poses much mystery and concern. Actually, that concern may be growing, as evidenced by this published abstract from Eric Senturier and his team from Snecma: *“engine manufacturers know the vibration levels only at the very end of the development, during engine testing, or sometimes in operation.”* The Snecma paper is to be presented later this morning.

The two papers I am introducing today provide a way for design teams to clear up mystery that shrouds the physics of overlooked mechanisms, mechanisms that designers need to account for in their analysis of engine vibrations.

Researchers are still trying understand these vibrations through the mystery they call “mistuning.” This mystery of “mistuning” rose out of a puzzling test result way back in 1955 -- the test that uncovered the “split peak.” Wow, that’s almost half a century without a clear result.

As a designer, I understood “mistuning” as just the inevitable nonuniformity in blade–to-blade response in bladed disks, nonuniformity common to every real engine. Recently, I have become curious about all the dire warnings about “mistuning.” At Turbo Expo 2001 I offered an explanation of the “split peak,” the mysterious mechanism that seems to have triggered the quest called “mistuning.”

”Rogue Blades”

”Rogue” blades in a compressor are very occasional rotor blades which appear typical in their aerodynamic and mechanical characteristics and yet fail under fatigue when all the other blades are scarcely fatigued at all,”

B. S. Stratford, 1966, “**Rogue Blades**,” *Rolls-Royce Internal Report*, 1966 MCR90011

Since that 2001 paper, I discovered mechanisms stemming from the principle of “split peak” that are real and relevant, particularly because of recent adoption of the shroudless fan. In 1966 Rolls-Royce found this mystery in what they called the “***rogue blade***.”

This year’s offered abstracts indicate that some in industry have become convinced of the need to predict the effect nonuniformity in blade-to-blade response as a way to help avoid what Eric Senturier and team call *“late discovery of High Cycle Fatigue problems.”*

The recent shift to blisks introduces a new challenge, because the inherent damping at blade attachments is lost.

The mysterious "worst" consequence of "Mistuning"

"the modal amplification factor may take on very large values"

Marc Mignolet and team from Arizona State University
GT2004-54030 "**On the Maximum Amplification of Blade Response Due to Mistuning and the Whitehead Limit**,"

Today many see the one-size-fits-all, *"maximum amplification"* factor as a way for designers to avoid risks. Researchers have responded to this new challenge by intensifying the half century quest to capture the "worst" effect of "mistuning," most recently through "Monte Carlo" analysis."

Many researchers are still focusing on the long-sought maximum *"amplification factor,"* addressed yesterday by Prof. Mignolet and team. That research seeks to develop design analysis to predict the maximum possible peak amplitude as a multiple factor of ideal (tuned) amplitude. Verification of that factor remains elusive because demonstrating base-line amplitude is impracticable; a physical model will never replicate mathematical perfection.

That maximum *"amplification factor"* is calculated by arbitrary assumption of damping, as Pertov and Ewins acknowledge; otherwise we would have to divide infinity by infinity. Nevertheless, Prof. Mignolet and team report that the "**amplification factor may take on very large values.**" Numbers as high as 3 have been mentioned.

Here is a 2001 paper, one that claims success in developing an analysis to account for the "worst" effect of "mistuning." These analytical results by Petrov and Ewins of *"engine order type"* excitation in each blade of a shroudless fan show *"many closely-spaced resonance peaks."* The analysis appears to be one of the first order response - at 2/rev, as 1/rev does not occur in a spinning bladed disk. Finding the actual "worst fan" would still require much Monte Carlo dice rolling through their opaque process.

"*Near resonance peaks, the amplitude levels of systems* ***with small damping*** *(which is typical for bladed discs) vary abruptly...* ***many closely-spaced resonance peaks***."

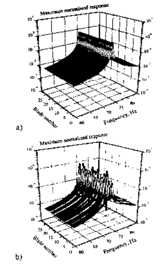


Fig. 1 Forced response of each from 26 blades of the fan bladed disc: a) the case of tuned blades; b) the case of mistuned blades

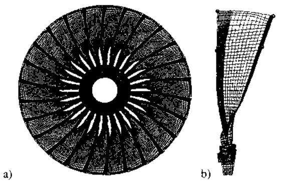


Fig. 2. Finite element model and active nodes: a) a bladed disc; b) a sector of the bladed disc

E. P. Petrov and D. J. Ewins , 2001
"**Analysis of the Worst Mistuning Patterns in Bladed Disc Assemblies**,"
Proceedings of ASME TURBO EXPO 2001,
Paper No 2001-GT-0292

However, the principle of the "split peak," which I will explain, requires only two *"closely-spaced resonance peaks."* Designers should seek explanation of the apparent violation of basic mechanics before committing to opaque processes such as suggested by Petrov and Ewins.

In my experience, the separation between exciting and resonant frequency is a primary concern of designers. If resonance is safely avoided in operation, is the magnitude of that avoided resonance response in the "worst fan" a key to taming vibration?

These plots by Petrov and Ewins of response in each blade of their trial, **real** (by which they mean “mistuned”) fan, show that the uneven peaking seems to vanish quickly with separation as low as 2%. We may conclude that the speed of the peaking is crucial, not the unevenness of the peaks. Every designer works hard to avoid those “*many closely-spaced resonance peaks,*” or more correctly, two “*closely-spaced resonance peaks.*”

Papers at this Conference show that there is concerned interest in understanding the challenging, unpredicted vibrations in bladed disks. Unfortunately, they also convey very, very little from researchers that would set designers on-track to account for the mysteries in those vibrations.

How did “mistuning” become the only sanctioned search for solution today?

Timoshenko on Campbell

The principal results described in the present paper can be best described as follows: The importance of obtaining data on full-sized wheels under actual operating conditions has been fully appreciated by the author.

.....The author shows that the type of vibration responsible for practically all serious wheel failures consists of a train of backward-traveling waves whose ***backward speed in the wheel exactly equals the forward speed*** *of wheel rotation.”*

Discussion on

Wilfred Campbell, “**Protection of Steam Turbine Disk Wheels from Axial Vibration**,” Paper No. 1920, TRANS. ASME, 46, 1924, pp. 31-160

In seizing on the 1955 puzzle of the “split peak,” which surfaced in an experiment on a stationary, oversimplified model, experts missed important mechanisms that had been shown in previous experimental data. Researchers failed to look beyond what their distinguished predecessor, Timoshenko - who was then still at Westinghouse – identified as important in 1924. “*…a train of backward-traveling waves whose backward speed in the wheel exactly equals the forward speed of wheel rotation.*” Those waves are now the “*engine order type*” excitations that are addressed by Petrov and Ewins.

Timoshenko did not mention Campbell’s demonstration of the one-nodal-diameter backward traveling wave due to elliptical precession at the disk/shaft juncture. That wave travels at exactly twice spin speed and contains forces of the same frequency as in first wave embedded in the train of backward-traveling waves recognized by Timoshenko as “*responsible for practically all serious wheel failures.*” But after 80 years it is time to include the wave overlooked in Campbell’s work, the wave that travels at twice the speed of that train.

In looking beyond what Timoshenko identified as important, researchers might have uncovered another one-nodal-diameter wave traveling at exactly twice spin speed, the wave that arises because the first order response is inevitably asymmetric. The ideal of perfect symmetry does not exist in an operating engine.

If this twice-spin-speed wave due to asymmetry is ignored, prediction of the two-nodal-diameter fan-alone resonant speed, which determines the first order resonance in operation, can be dangerously misleading. In an actual fan that resonant speed is always lower than ideal.

How is this ignored one-nodal-diameter wave the cause of the 1955 puzzle? If we accept processes such as are being offered by researchers, for example by Petrov and Ewins, how do we integrate the results to account for bladed disk flexing/engine whirl interaction?

This figure from my second paper shows an asymmetric standing wave, the principal component wave of the full traveling wave that is forced by inlet distortion in a shroudless fan. The timing and the axis location of this component wave are chosen so as to contain the full response at the "rogue blade," the blade closest to its condition of resonance.

One-nodal-diameter wave lowers the resonant frequency of the fan-alone

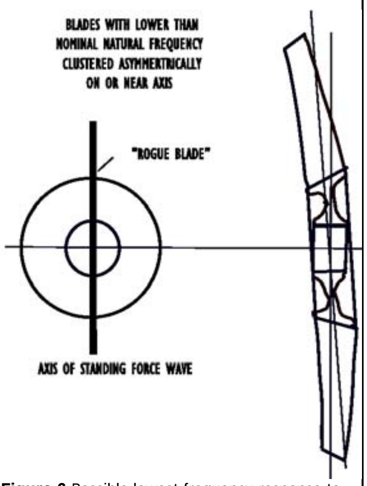


Figure 6 Possible lowest frequency response to inlet flow distortion of a shroudless fan with one-nodal-diameter natural frequency only slightly higher than 2/rev

This is an oscillating wave fixed to the spinning disk. There is a similar in-phase wave fixed to the perpendicular axis and two more similar phase-orthogonal waves fixed to a reference frame at 45 deg. An observer here at the "rogue blade," where highest amplitude occurs, would see the full wave coming and going, but those other component waves do not affect his own motion.

This wave lowers the resonant frequency of the fan-alone from the resonant frequency the symmetric, two-nodal-diameter wave. None of papers at this Conference address this lowering. A serious "design system miss" might accrue if the design system does not allow for the lowered resonant frequency.

You should notice that in this wave, which is a component of the full traveling wave, there is an oscillating moment imposed on shaft. There is no way to isolate a specific mechanism like this moment without accounting for all interacting mechanisms. My own work, which I began publishing thirty years ago, shows that once one begins to study the net effect of interactive mechanisms, the need for modeling the whole engine becomes obvious.

By jumping to real solutions that include the one-nodal-diameter wave, designers can leapfrog the swamp of research into "mistuning" simply by using the tools available to them, as my second paper shows.

I will now show many of the nuances of vibration in the high-bypass turbofan through the example of "prop-rotor whirl flutter," the phenomenon widely accepted as the probable cause of the Lockheed Electra crashes in 1959-1960.

The turboprop a good starting place, easier to understand and without what they now call "baggage." We can begin with actual observations and prevailing theories, rather than having to address decades of heavily defended, but so far fruitless, research.

"*Additional important factors are almost certain to arise in the treatment of more complex systems. One might find cases, for example, where added degrees of freedom, such as* ***propeller blade flexibility****, produce significant changes in the stability of the system.*"

J. C. Houbolt and W. H. Reed, III, "**Propeller-Nacelle Whirl Flutter**," Journal of the Aerospace Sciences, Vol. 29, No. 3, March, 1962, pp.333-346

The first recognized occurrence of "prop-rotor whirl flutter," first raised as a theory in a 1938 paper, was when on September 29, 1959, a Lockheed Electra crashed, resulting in the loss of the aircraft and all persons aboard. The same loss was sustained in the second crash six months later. The probable cause in both crashes was listed as, "*In-flight separation of the right wing because of flutter induced by oscillation of the* ***outboard*** *nacelles.*"

The physics of "prop-rotor whirl flutter" are elusive; the literature still refers to the 1938 paper for the theory. But if you try to grasp that theory from the paper, you begin to see that the 1938 authors envisaged a germ of load-induced whirl, as we will see.

Today's experts rely on wind-tunnel tests rather than theory to validate new designs. However, Donald Kunz's 2002 survey paper indicates that there seems to be consensus that "prop-rotor whirl flutter" in the whole propeller-gearbox-engine-nacelle assembly (the "propeller-nacelle" in short) is backward whirl of which amplitude increases with increasing power.

I have not found any studies that incorporate propeller blade flexibility, even though this 1962 NASA paper, published in the wake of the Electra crashes, acknowledges: "*One might find cases, for example, where added degrees of freedom, such as* ***propeller blade flexibility****, produce significant changes in the stability of the system.*"

We will now examine the effect of *"added degrees of freedom,"* in the case of the Electra crashes, effects that have not yet even been discussed literature. When we grasp the inevitable interaction of mechanisms acting simultaneously in the whirl of the turboprop, the course to design analysis of the shroudless high-bypass turbofan should become clear; the principles stay the same, only the details in modeling the hardware become more complex.

We will follow the engineer's way of learning about mechanics, the way Timoshenko taught. We will start with a simple model to establish the basic principles that we dare not violate in any realistic solution. Then, we will be ready to grapple with a complex engine model. At the least we will become aware of the principles that must be satisfied.

We adopt the way of the conceptual designer: begin with the broad picture and seek a set of details that merge into the picture, all the while staying alert to the possibility that well accepted assumptions may no longer be sufficient as designs and operating conditions keep changing. One might say, we start by surveying the forest, not staring at a tree.

I know that this way of thinking is different than represented in the Structures and Dynamics sessions. Those researchers are still reacting to the half-century puzzle of the "split peak." Let's see if "prop-rotor whirl flutter" might be accounted for in the integration of a turboprop into an airframe.

I was very fortunate last fall to reach Willis M. Hawkins, principal designer on the Lockheed Electra, and I received more details on the crashes of 1959-1960 than have previously been published. A fatigue fracture of a gearbox mount lug was the first event in each crash. There had been prior incidents of failure in that lug, on inboard propeller-nacelles, failures that did not result in catastrophe. Each time the engine was safely shut down on detection of vibration.

In the crashes, such fracture in an outboard propeller-nacelle not only triggered similar vibration, but also precipitated the catastrophic "*flutter induced by oscillation of the outboard nacelles*" when the engine speed dropped to idle.

Strengthening the gearbox mount structure was the effective fix. So far as I could determine, there has not been a failure that would test the wing stiffening and addition of bumpers intended to limit the whirl amplitude in the event of recurring mount failure. I have read in the popular press that Electras and military derivatives are being flown as fire-fighting tankers, and have been breaking off wings, but without technical literature, the cause is not clear.

Clearly, Allison's design and development missed at least one important mechanism that did not show up until commercial flight. The problem was solved easily by the traditional way of development, but not before two planeloads of people were killed and the race to establish the turboprop took a hit.

One missed mechanism may well be the effect of propeller inlet flow distortion, as might be evidenced by the greater susceptibility of the inboard nacelles. At the inboard nacelles we should expect more flow distortion because of nonuniformity of flow near the fuselage, especially during flight maneuvers.

Prop-Rotor Whirl

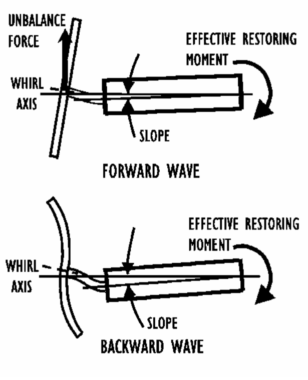


Figure 2 Unbalance response as forward and backward waves

Let's first look at another mechanism that Allison might have missed but that was foreseen in 1938: backward whirl in the propeller-nacelle.

We need to examine this simple physical model. The bulk of the propeller-nacelle may be represented as a rigid body, with only the rotor, the propeller and the drive shaft being treated as flexible. Mounting in the wing is at a pivot with asymmetric angular stiffness representing the restoring moments of the wing.

Unbalance response is contained in two traveling waves, the primary, forward wave forced by the unbalance and the secondary, backward wave induced as compensation for the asymmetry of the primary restoring moment at the wing. The effective restoring moment in each wave is to be determined by analysis of the coupling between the waves.

The propeller is assumed to flex as a one-nodal-diameter wave at frequency above its propeller-alone natural frequency.

For NASA's 1962 assumption - which Donald Kunz suggests is still the standard in the literature - that the propeller is rigid, the gyroscopic moment for backward whirl is three times as large as for forward whirl and is opposite in direction.

If the propeller is flexible, the effective gyroscopic moment depends on whether twice spin speed is below or above the one-nodal-diameter propeller-alone natural frequency.

For frequency of gyroscopic forcing due to whirl above the propeller-alone natural frequency, the direction of the effective gyroscopic moment is reversed, becoming opposite of the forcing.

In this definition the effective gyroscopic moment is the total inertial moment due to precession at the drive shaft. The one-nodal-diameter propeller-alone natural frequency is determined for the propeller constrained to zero precession at the drive shaft juncture.

Campbell called this reversal of the effective gyroscopic moment a “minor resonance.”

For our assumption that gyroscopic forcing of the propeller is supercritical, the gyroscopic moment is a positive restoring moment. In other words, the effective gyroscopic moment acts to resist the slope at the drive shaft juncture.

This diagram shows the slope for the backward wave higher than for the forward wave. Even if it is smaller, it does not really matter to this explanation.

For simplicity of illustrating the effective forward and backward restoring moments and the moments of reaction, the pitch stiffness is assumed as zero. This assumption is reasonable in an initial design analysis of a propeller-nacelle.

Coupling at Wing

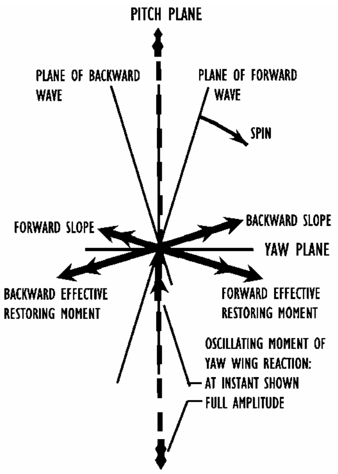


Figure 3 **Coupling of response waves at wing juncture**

The timing between the forward and backward waves is depicted in this diagram as a view looking forward, toward the propeller. The traveling moments, which are the effective restoring moment in each wave, are shown in this end view as vectors of the conventional right hand rule.

The waves are opposed at an instant of coincidence in the pitch plane. This opposition is indicated by opposite direction of slope. The waves are unidirectional at an instant of coincidence in the yaw plane. At coincidence in the pitch plane, the effective restoring moments are in balance. At coincidence in the yaw plane, the sum of the effective restoring moments is opposed by the oscillating moment of reaction at the wing.

The motion at any location is predominantly in backward-tracking elliptical orbits. These orbits have higher deflection at the yaw plane, which is the plane in which the waves are unidirectional. The tips of the propeller blades move essentially in a backward traveling, one-nodal-diameter wave.

Thus far we see how propeller unbalance drives backward whirl in the whole propeller-nacelle. The consensus on “prop-rotor whirl flutter” goes further than this response. We still have to show that the whirl is load-sensitive, with amplitude increasing with increasing power. Is there relevant historical precedence for such sensitivity?

> for line shafting, either a first possibility where “**the shaft must go on permanently whirling in a bent form** ... to the injury of itself and of the adjoining machinery and framing: a kind of motion which may be called centrifugal whirling,”
>
> or a second possibility where *“**centrifugal whirling is impossible.**”*
>
> W. J. Macquorn Rankine, **“On the Centrifugal Force of Rotating Shafts**,” *The Engineer*, April 9, 1869, pp. 249

The effect of propeller drive torque on the unbalance response of the propeller-nacelle has not been addressed in the literature. We should expect a result similar to observations that Rankine reported 135 years ago.

Here the vector of the resisting torque is reduced into a "tangential" torque load and an external moment. That external moment is the torque-induced moment that is imposed at the propeller/shaft juncture in the backward wave.

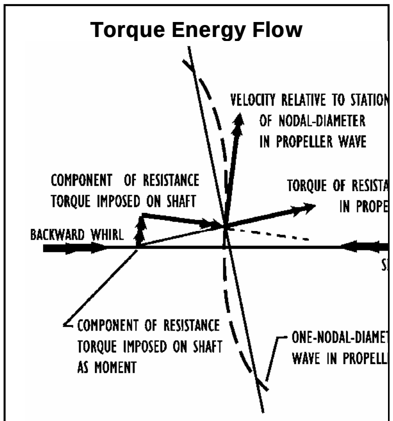


Figure 4 **Energy flow from spin to backward precession of propeller**

The vector of angular velocity relative to the stationary frame of reference is coincident with the moment vector. This coincidence means that the resulting energy flows from spin to backward precession of propeller.

For our simple model, this flow represents the net flow into the whirl of the propeller-nacelle. The small energy flow due to the torque transmittal in the flexing shaft is neglected.

That net flow of energy to backward precession of propeller may explain why Allison's design and development underestimated the vibration.

In the explanation, choosing to show the propeller-alone natural frequency lower than 2/rev illustrates Rankine's first possibility. If we stiffen the propeller, so that the propeller-alone natural frequency is only slightly lower than twice operating speed, the inherent damping may not be sufficient to safely limit the amplitude of the "prop-rotor whirl flutter" at full power.

This observation by Taylor and Browne, in the 1938 paper that is still cited for the theory of "prop-rotor whirl flutter": *"when the axis of the propeller is displaced to make a small angle with the direction relative to the wind, air forces generate a* ***moment tending to rotate the propeller at right angles to this displacement,"*** describes an external moment and the angular velocity that are shown as vectors in the previous figure.

propeller is displaced to make a small angle with the direction relative to the wind, air forces generate **a moment tending to rotate the propeller at right angles** to this displacement"

E.S. Taylor and K.A. Browne, "**Vibration Isolation of Aircraft Power Plants,**" Journal of the Aeronautical Sciences, Vol. 6, No. 2, December 1938, pp. 43-49

Calculation of the moment that *"air forces generate"* probably would account for only secondary effects if propeller flexibility is neglected.

Rankine's second possibility, stabilization due to torque, would prevail if we stiffen the propeller so the propeller-alone natural frequency becomes higher than twice operating speed. Some of the energy of propeller backward whirl is then extracted to drive the propeller.

In the first paper, I conclude that successful high-bypass turbofans benefit from amplitude modulation by the action of torque on whirl. This benefit is secured through traditional development, not by design. Experts have not yet recognized even Rankine's first possibility, the possibility that this action may be destabilizing.

From "day one," the academic community missed Rankine's insight into whirl instability. Designers who attempt to see beyond the dogma of "today's science" when seeking to understand puzzling observations are often written off with that same academic myopia which accepted only the dogma of "Rankine's Mistake" instead of working to understand his paper.

Virtual Energy Flow: key to the mystery?

"investigating the relation between blade response and **vibration energy flow** in lumped parameter models."

Christophe Pierre and team from University of Michigan "**Intentional Mistuning Design Space Reduction Based on Vibration Energy Flow in Bladed Disks**," GT2004-53873,

"For the disk-dominated modes, the sharing of **modal energy** with the blades can lead to the disk being excited by aerodynamic loading."

James Kenyon from the Air Force Research Laboratory and colleagues from Carnegie Mellon University, "**Sensitivity of Tuned Bladed Disk Response to Frequency Veering,**" GT2004-53280

"[Klompas'] argument is conditional on the disc having a passive effect on the response (i.e. the flexibility of the disc serves only to distribute the mistuning influence); it does not consider the **redistribution of forcing**."

Reviewer # 3 on 2003 draft paper, "**Significance of Interblade Nonuniformity in Engine Vibrations**:"

Those who attended yesterday's session on bladed disks, heard in the presentation of these two papers, about an energy flow that does not fit within the mechanics that explain Rankine's historical observation.

As I understand, that energy flow occurs only in the "modal domain." Engines do not operate in such a virtual domain. This "v*ibration energy flow"* is not the same as the flow I have shown through the simple model of the turboprop.

Because they address only conservative systems, the assumption of *"modal energy,"* which *"can lead to the disk being excited by aerodynamic loading,"* can only be cyclic. We can imagine this flow as the cyclic conversion of energy between kinetic and potential in the response of the spring-mass; the net energy flow is zero.

This is very different from "prop-rotor whirl flutter," where the net energy flow is constant for steady whirl.

Even though the response of a blisk to a traveling wave of force exhibits blade-to-blade variations, there is still only the cyclic conversion of energy.

My second paper addresses inconsistencies in the assumptions of modal analysis, but today it seems impossible for a designer to successfully challenge long-defended academic theories. However, this review comment on my last year's offering seems to uncover the gist of those assumptions.

Reviewer #3, while acknowledging that the standing waves defined in my paper represent clearly *"the mistuned response of a bladed disk,"* insisted that any valid analysis must consider "***redistribution of forcing.***" On the face of it, this second comment makes no sense because forcing is a given for any particular solution of response.

In "connecting the dots," I conclude that "***modal energy,"*** or ***"vibration energy flow***" is the cause of "***redistribution of forcing,"*** which in turn leads "***to the disk being excited by aerodynamic loading."***

Clearly, "***redistribution of forcing."*** has no basis in the physics of fan response to inlet flow distortion; all blades pass through the same stationary wave of force. One wonders how something that has no basis in physics appears to have become the basis of all expectations in researchers' papers.

Allison may have missed also the excitation backward whirl of the propeller-nacelle by distortion of flow through the propeller. Examination of that mechanism will pass through an explanation of the "split peak."

Extracting Distinct Standing Waves

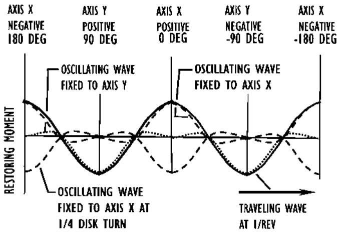


Figure 1 Component of ideal two-nodal-diameter traveling wave transformed into two in-phase standing waves

This traveling wave represents the first order excitation – excitation by a two-nodal-diameter stationary wave of force - due flow distortion in the propeller. The ideal response, in which the response of each blade is identical, is represented by the axial restoring moment alternating at the juncture of the blades to the disk. The moment is mathematically represented by a two-nodal-diameter, 1/rev backward traveling wave, which is viewed as two standing waves (fixed with respect to the disk). Both of the standing waves oscillate at 2/rev, but their oscillation is phase-orthogonal - separated in phase by 90 deg corresponding to 1/8 turn of the disk.

This graph shows the traveling wave coinciding with the standing wave fixed to the X-Y reference. The phase–orthogonal wave, which is not shown, is fixed to axes at 45deg and coincides with the traveling wave 1/8 turn later.

The Electra propeller has four blades. One of the standing waves is timed so that all blades are at maximum amplitude in at time zero, reverse at every ¼ turn, and complete a cycle in ½ turn. The complementary orthogonal component does not register in the four-bladed propeller; it represents zero amplitude at all blades.

For the fan, however, both orthogonal components are necessary to represent the traveling wave at all blades.

The first standing wave, the one that contains the response of the four blades, is in turn reduced into two standing waves, each fixed to one axis of the reference frame. The opposite limit of the oscillation, at ¼ turn of the propeller, is shown for the reduced wave that is fixed to the 0-180 deg (X) reference axis.

For the four-bladed propeller, the two standing reduced waves, each fixed to the axis through two diametrically opposite blades, are sufficient; the portion of the waves between the axes may be erased.

An observer on a blade sees only the same motion on the opposite blade, and the reversed motion at the blades on the sides.

Now that we have visualized the first order excitation in the four-bladed propeller we are set to visualize the half-century mystery of the "split peak."

Let's look at the oscillating motion in the propeller, assuming purposeful nonuniformity of blade-to –blade response.

To begin, we denote a blade of lowest blade-alone natural frequency, which we call the "most flexible" blade.

When we excite the propeller by a two-nodal-diameter wave of force at a frequency lower than the corresponding natural frequency of the ideal propeller-alone, the inertial forces on the "most flexible" blade and its opposite blade, the blade on the same axis, become increasingly mismatched with increased exciting frequency. Flexing of the disk as a one-nodal-diameter wave tends to even out the moments at the root of the blades.

The "most flexible" blade remains closer to the condition of resonance. Resonance is the condition where its elastic restoring moment is insufficient to balance sum of the external force and the inertial force associated with the flexing.

If we increase the propeller excitation frequency to approach the condition of resonance in the "most flexible" blade, the amplitude of both the "most flexible" blade and its opposite blade tend toward infinity. The amplitude of the opposite blade stays lower than the "most flexible" blade. The amplitude of the other two blades, those on the perpendicular axis to the most flexible blade, rises steeply but remains bounded.

Let's now start again with the same model, but begin with excitation of the propeller at higher frequency than the two-nodal-diameter natural frequency of the ideal propeller-alone.

We assume that for the two perpendicular blades the blade-alone natural frequency for both is higher than the blade-alone natural frequency for the "most flexible" blade, and that the blade-alone natural frequency for at least one of them is higher than for the blade opposite to the most flexible blade.

At that exciting frequency, the response of all four blades is supercritical. In other words, the direction of deflection is reversed and the inertial forces are opposed to the applied forces.

As the excitation frequency is decreased, the perpendicular blade with the higher blade-alone natural frequency is first to enter the condition of resonance. This resonance occurs at a slightly higher frequency that that entered by increasing the frequency of excitation.

Inevitable nonuniformity in blade-to-blade response splits the natural frequency of the propeller-alone into two closely-spaced frequencies, each associated with a condition of resonance in only one blade. In an actual propeller the action of inherent damping allows the infinite resonant amplitudes to be discernable as two peaks with the highest amplitude at two perpendicular blades. Together these peaks are called the "split peak."

As a reminder, the "split peak" was first reported in the 1955 experiment by Armstrong on a stationary, simulated bladed disk. Subsequent similar experiments, as recent as in 2000, have established the "split peak" as the inevitable response of a bladed disk passing through a resonance. To this point, no researcher has given such an explanation for this phenomenon as the explanation I just gave you. Instead, the "split peak" often seems to disappear from any explanation of what they call "mistuning."

Even though a fan has many blades, and the nonuniformity is random, nature splits the resonance response into two closely-spaced peaks, each associated with resonance in a blade approximately 90 deg apart. A resonant blade need not be either the blade of lowest nor of highest blade-alone natural frequency. This split of response to "*engine order type*" excitation led to observation of the "split peak."

The "split peak" by itself is of no concern to designers; designers have always worked to avoid possible interference of engine order exciting frequency with bladed disk natural frequency of the same order. None base their design criteria on possible response at resonance.

So long as the "design system" does not account for the existence of the "spilt peak," the natural frequency that corresponds to the "*engine order type*" excitation could be missed. This miss can create a serious risk of catastrophic design error.

Response of a shroudless fan to two-nodal-diameter excitation could be dangerously miscalculated.

The two-nodal-diameter natural frequency of an ideal integral shroudless fan-alone - the impracticable perfect blisk-alone I had mentioned earlier - usually falls slightly higher than 2/rev at operating speed. We may easily be led to accept the separation between that natural frequency and the frequency of "*engine order type*" excitation by a two-nodal-diameter wave of force as sufficient for safety. But as we have seen, there is really more to the response than the two-nodal-diameter deflection shape that is associated with that ideal natural frequency.

As a worst case, let's cluster blades of blade-alone natural frequency lower than nominal and place a reference axis through the cluster. The axis then becomes similar to the axis through the "most flexible" blade in the propeller. There is mismatch between the moment at the roots of these blades and the opposite blades near the same axis. Compensation for the mismatch occurs by oscillation of disk rim at 2/rev, the frequency of excitation.

This figure, which I have shown earlier, shows that the principal response to inlet distortion is an asymmetric standing wave, which interacts with whirl of the rotor.

If design analysis were based strictly on the deflection shape as a two-nodal-diameter wave, the implicit assumption of symmetry in the deflection shape would possibly produce a large error. This assumption of symmetry fails to satisfy equilibrium.

One-nodal-diameter wave lowers the resonant frequency of the fan-alone

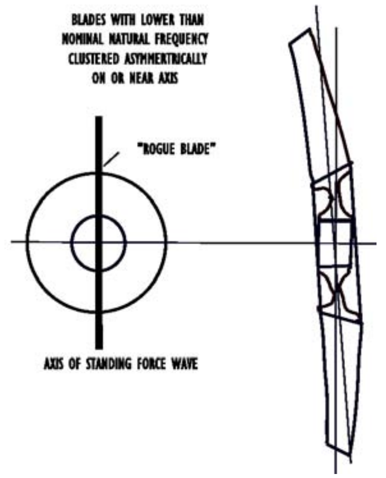


Figure 6 Possible lowest frequency response to inlet flow distortion of a shroudless fan with one-nodal-diameter natural frequencyonly slightly higher than 2/rev

In calculating the response of an actual fan equilibrium is satisfied by superposing a one-nodal-diameter standing wave on the symmetric wave. Both waves oscillate at 2/rev. The resulting asymmetry of disk flexing lowers the frequency of resonant response, reducing the separation between the exciting frequency and resonance.

In our assumed worst case, where blades of blade-alone natural frequency lower than nominal are clustered, this reduction of separation between the exciting frequency and resonance may be missed during rig testing, only to be discovered in operation as surprising vibration.

With this reduced separation between the exciting frequency and resonance in engine operation, the "rogue blade," that is the blade closest to its condition of resonance, may begin to show signs of High Cycle Fatigue earlier than any other blade.

Eric Senturier and team call such signs "*late discovery of High Cycle Fatigue problems,*" and agree that designers must be aware of this type of possible surprise in operation.

If this separation between the exciting frequency and resonance is reduced to the point that the first resonance of the "split peak" is entered, the stabilizing effect of fan resisting torque begins to reverse, as our examination of "prop-rotor whirl flutter" shows. That stabilizing effect would tilt fully into a destabilizing effect when the second resonance is reached.

Before that tilt, engineers would certainly hear what my Whittle trained mentors called "expensive noises."

The oscillation as a one-nodal-diameter wave produces an oscillating moment in the shaft.

In this diagram, that moment is reduced into a forward traveling moment at 3/rev and a backward traveling moment at 1/rev, both with respect to the stator.

Interaction with Shaft Precession

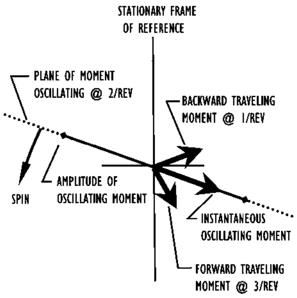
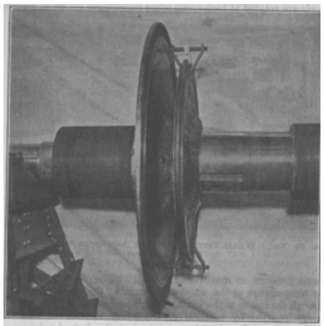


Figure 4 Reduction of disturbing moment into two traveling moments

The backward moment acts to force backward whirl of the shaft. The gyroscopic forces resulting from that whirl induce a one-nodal-diameter wave in the fan. This induced wave can produce stresses that are additive to stresses due to "*engine order type*" excitation.

Campbell demonstrated the one-nodal-diameter forward traveling wave at 3/rev by applying intentional excessive unbalance to his *Testing Machine Shaft* even before 1924. This demonstrated forward whirl, however, is unlikely to be significant in an engine.

Here "R" is evidence of 2/rev vibration in the bladed disk and "S," which is the response in the stator, is evidence of the forward traveling wave.



IG. 61 TESTING-MACHINE SHAFT SHOWING EXPLORING COILS CARRI
ON AN ADJACENT AND RELATIVELY STIFF WHEEL

vibration, is shown in
the shaft oscillate mostly in one plane, the horizontal

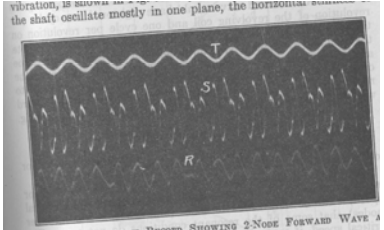


FIG. 91 OSCILLOGRAPH RECORD SHOWING 2-NODE FORWARD WAVE A
FIRST MINOR RESONANT SPEED

Wilfred Campbell, "**Protection of Steam Turbine Disk Wheels from Axial Vibration**," Paper No. 1920, TRANS. ASME, 46, 1924, pp. 31-160

Campbell envisaged also the one-nodal-diameter backward wave, but realized that his setup allowed for recording only the forward wave. The backward wave is significant in an engine. We have seen this wave as part of the effective gyroscopic moment in "prop-rotor whirl flutter."

Campbell called the condition where the effective gyroscopic moment in backward whirl shifts from a negative to a positive restoring moment a "minor resonance". We have seen this condition while examining "prop-rotor whirl."

The second paper shows how the 3/rev response can be induced by asymmetry in the effective gyroscopic moment. The paper cites occurrence of the 3/rev response in Campbell's experiment as corroborating evidence of presented mechanics.

The backward-traveling wave that is a direct result of unbalance has been shown to cause energy flow in "prop-rotor whirl flutter." Researchers have never addressed this wave in association with unbalance response or discussed the results of this energy flow.

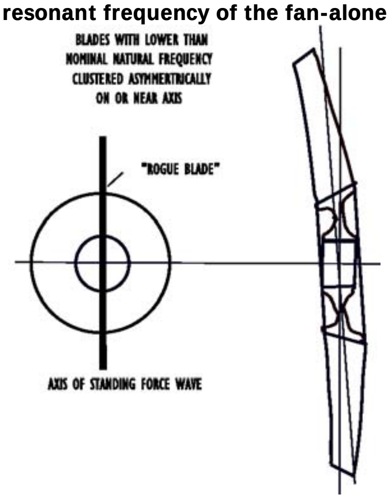

One-nodal-diameter wave lowers the resonant frequency of the fan-alone

Figure 6 Possible lowest frequency response to inlet flow distortion of a shroudless fan with one-nodal-diameter natural frequencyonly slightly higher than 2/rev

The paper shows how a design team can characterize the response of the fan-alone to "*engine order type*" excitation and to gyroscopic forces. As the first step, a probable worst distribution of blade-alone natural frequency may be assumed, as I have assumed in showing you why this asymmetric deflection shape is inevitable in an actual blisk.

The design analysis outlined in the paper incorporates fan-alone dynamic characteristics, into a solution of whirl of the whole engine. These characteristics may be secured analytically or experimentally.

The approach to this analysis is different than used by researchers. Researchers are trying to secure practical design analysis out of finite-element models with millions of degrees of freedom, as in the suggested analysis by Petrov and Ewins of "*engine order type*" excitation in each blade of a shroudless fan.

The design analysis outlined in the paper is a practical way to account for the inevitable interaction of mechanisms acting simultaneously in the whirl of the whole engine. The offered analysis is based on previously published principles and procedures. For example, the basic bladed disk flexing/shaft whirl interaction was first published in 1974.

For years, researchers have attempted to address the unwieldy challenge of ordering a finite-element model into a usable solution. Thus far, none have demonstrated a solution that can be integrated into the "design system."

Design teams need practical answers now. We must stop waiting for a decades-delayed design analysis from researchers. It is time to face up to our own challenges in creating advanced engines and in fixing operational problems.

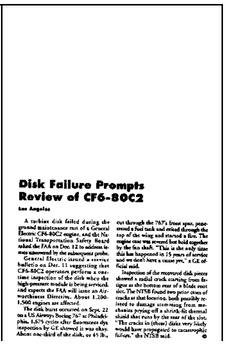
Researchers cannot deliver needed analysis until they focus on understanding real life problems and letting those problems inform their models, rather than refining complicated, isolated models and then failing to find a way to apply those models to real-life problems. Let me give you an example of a real life problem that was raised in this forum but has not yet been addressed.

At Turbo Expo 2001, I highlighted the September 2000 high-pressure turbine disk fracture. *"A piece of the disk cut through the 767's front spar, penetrated a fuel tank and exited through the top of the wing and started a fire,"* reported Aviation Week. The failure occurred during ground maintenance, thank God. I suggested that researchers look at bladed disk flexing/engine whirl interaction as the possible cause.

2000 HPT Disk Fracture

About one-third of the disk, or 45 lb., cut through the 767's front spar, penetrated a fuel tank and exited though the top of the wing and started a fire…

" the NTSB



Disk Failure Prompts Review of CF6-80C2

Los Angeles

A turbine disk failed during the ground maintenance run of a General Electric CF6-80C2 engine, and the National Transportation Safety Board asked the FAA on Dec. 12 to address issues uncovered by the subsequent probe.

General Electric issued a service bulletin on Dec. 11 suggesting that CF6-80C2 operators perform a one-time inspection of the disk when the high-pressure module is being serviced, and expects the FAA will issue an Airworthiness Directive. About 1,200-1,500 engines are affected.

The disk burst occurred on Sept. 22 on a US Airways Boeing 767 at Philadelphia, 1,675 cycles after fluorescent dye inspection by GE showed it was okay. About one-third of the disk, or 45 lb., cut through the 767's front spar, penetrated a fuel tank and exited through the top of the wing and started a fire. The engine case was severed but held together by the fan shaft. "This is the only time this has happened in 15 years of service and we don't have a cause yet," a GE official said.

Inspection of the recovered disk pieces showed a radial crack starting from fatigue at the bottom rear of a blade root slot. The NTSB found two prior cases of cracks at that location, both possibly related to damage stemming from mechanics prying off a shrink-fit thermal shield that runs by the rear of the slot. "The cracks in (these) disks very likely would have propagated to catastrophic failure," the NTSB said.

There was no response from the audience. In the spring of 2002, CNN reported that the manufacturer has completed their investigation, and found no fault in the design.

Due to the effective gyroscopic moment in the second stage bladed disk, the rim of the first stage disk must transfer a traveling wave of axial inertial force into the main shaft. It seems obvious the first stage disk would sustain highest cyclic tangential stresses at the sides of the rim, the reported location of crack initiation.

High stresses can be caused by exceptional conditions of engine whirl. Conditions such as unchecked unbalance, loosening of joints and tip rub may combine interactively so as to cause whirl amplitude to progressively grow. These conditions might be rarely seen in catastrophic combination, even in operation. A probable result may be a bump in a protective squeeze-film damper, precipitously changing the dynamic characteristics of the engine and causing these rare but deadly stresses.

To an old designer, this disk fracture -- which I am sure Eric Senturier would agree was a case of **extremely** *"late discovery of a High Cycle Fatigue problem"* -- was a fortuitous wake-up call. Somehow institutions that claim to spearhead the search for the insight that designers need to boldly and confidently create amazing new engines have missed this wake-up call.

Until researchers provide useful insight into existing problems, insight which is vetted by the "design system," designers cannot be bold. So long as puzzling observations remain unexplained, designers remain ill served.

Has NASA Heard The Wake-Up Call?

"*Rotor crack problems present a* ***significant safety and loss hazard*** *…. particularly in the power generation industry, but not only.*"

GT2004-54095, **Coupled Lateral and Torsional Vibrations of Cracked Rotor**, Jerzy T. Sawicki, from Cleveland State University and collaborators from NASA Glenn Research

There may be a glimmer of hope. On Tuesday, this paper, based on an acknowledgement that rotor crack problems present a *"**significant safety and loss hazard,**"* was presented. According to their abstract, Prof. Sawicki and his NASA collaborators are not yet grappling with the vibrations of the whole engine. They seem to assume that realistic insight can be garnered from interaction between torsional and lateral vibrations in an isolated rotor.

Our examination of "prop-rotor whirl flutter" shows that we need a comprehensive design analysis that accounts for the inevitable interaction of mechanisms acting simultaneously in the whirl of the whole engine. We will all benefit if this NASA study of a specific mechanism in an isolated rotor is but an initial step in a search into unexplained operational fractures and the tools designers need to avoid them.

I hope that you will agree that means a comprehensive approach to engine dynamics is long overdue.