The SN 1987A Link to Others and Gamma-Ray Bursts

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ABSTRACT

Early measurements of SN 1987A can be interpreted in light of the beam/jet (BJ), with a collimation factor >10⁴, which had to hit polar ejecta (PE) to produce the “Mystery Spot” (MS), some 24 light-days distant. Other details of SN 1987A strongly suggest that it resulted from a merger of two stellar cores of a common envelope (CE) binary, i.e. a “double degenerate” (DD)-initiated SN. Without having to blast through the CE of Sk -69° 202, it is likely that the BJ would have caused a full, long-soft gamma-ray burst (ℓGRB) upon hitting the PE, thus DD can produce ℓGRBs. Because DD must be the overwhelmingly dominant merger/SN mechanism in elliptical galaxies, where only short, hard GRBs (sGRBs) have been observed, DD without CE or PE must also produce sGRBs, and thus the pre-CE/PE impact photon spectrum of 99% of all GRBs is known, and neutron star (NS)-NS mergers may not make GRBs as we know them, and/or be as common as previously thought. Millisecond pulsars (MSPs) in the non-core-collapsed globular clusters are also 99% DD-formed from white dwarf (WD)-WD merger, consistent with their 2.10 ms minimum spin period, the 2.14 ms signal seen from SN 1987A, and sGRBs offset from the centers of elliptical galaxies. The many details of Ia’s strongly suggest that these are also DD initiated, and the single degenerate total thermonuclear disruption paradigm is now in serious doubt as well. This is a cause for concern in Ia Cosmology, because Type Ia SNe will appear to be Ic’s when viewed from their DD merger poles, given sufficient matter above that lost to core-collapse. As a DD-initiated SN, 1987A appears to be the Rosetta Stone for 99% of SNe, GRBs and MSPs, including all recent nearby SNe except SN 1986J, and the more distant SN 2006gy. There is no need to invent exotica, such as “collapsars,” to account for GRBs.

Subject headings: cosmology:observations–gamma-rays: bursts–pulsars:general—white dwarfs—stars: Wolf-Rayet—supernovae: general—supernovae: individual (SN 1987A)
1. Introduction

In Supernova 1987A (87A), Nature has provided an unparalleled opportunity to learn the details of one of the most frequent, and violent events in the Universe. Although confirming some early expectations of theorists (Chevalier 1992), even from the first, features which would never have been seen at ordinary extra-galactic distances, appeared in the early light curve, which at that time defied easy explanation.

The most remarkable feature of 87A was the “Mystery Spot” (MS), with a thermal energy of $10^{49}$ ergs, even 50 days after the core-collapse (CC) event (Meikle et al. 1987; Nisenson et al. 1987) and separated from the SN photosphere “proper” (PP) by some 0.06 arc s, with about 3% of this energy eventually radiated in the optical band. The possibility that this enormous energy implied for the MS might somehow link it gamma-ray bursts (GRBs) generally went unnoticed at the time.

GRBs, particularly long, soft GRBs ($\ell$GRBs), appear to be the most luminous objects in the Universe, occurring at the SN rate of one per second, given a collimation factor near $10^5$, yet we still know very little about them (see, e.g., Mészáros 2006 and references therein). Although some have been found to be associated with SNe, others, mostly those lasting only a fraction of a second, with slightly harder spectra (sGRBs), produce only “afterglows,” sometimes extending down to radio wavelengths. A large number of models have been put forth to explain GRBs, including NS-NS mergers for sGRBs, and exotic objects such as “collapsars” (MacFadyen & Woosley 1999) for $\ell$GRBs. The prime physical motivation for these is the enormous energy of up to $10^{54}$ ergs implied for an isotropic source. However, given that the data from 87A presented herein support a beam/jet (BJ) collimation factor (CF) $>10^4$ in producing its MS (see §3), there is no need for such a high energy.

This letter offers a simple explanation for 99% of SNe, MSPs, and GRBs\(^2\) in the context of the DD SN 1987A, its BJ and MS (Middleditch 2004, hereafter M04). It further argues that these start as sGRBs, and only later are some modified to $\ell$GRBs (and one other type – see §4), by interaction with the common envelope (CE) and/or polar ejecta (PE). It also argues that many, possibly all SNe Ia are caused by DD (merger-induced) CC, the single

\(^1\)Not counting, for the moment, the 2.14 ms pulsed optical remnant, which also revealed a $\sim1,000$ s precession (Middleditch et al. 2000a,b – hereafter M00a,b). Since a prototypical, dim, thermal neutron star remnant (DTN) has been discovered in Cas A (Tananbaum et al. 1999), representing what PSR 1987A will look like after another 300 years, and other pulsars have since been observed to precess (Stairs et al. 2000), this candidate is no longer controversial.

\(^2\)All except Soft Gamma Repeater [SGR] GRBs, which are estimated to amount to less than 5% of sGRBs and 1.5% of the total (Palmer et al. 2005).
degenerate (SD) paradigm (total thermonuclear disruption) being now admittedly in serious doubt (Siegfried 2007). Thus Ia Cosmology has not yet successfully challenged the Standard Model, and the burden of proof, for an accelerating expansion of the Universe, lies with the challenging model, the convenience of Concordance Cosmology amounting to only that.

2. The SN 1987A Bipolarity and “Mystery Spot”

SN 1987A is clearly bipolar (NASA et al. 2007; Wang et al. 2003). A “polar blowout feature” (PBF – a needed candidate for the r-process, e.g., Arnould et al. 2007) approaches at $\sim 45^\circ$ off our line of sight, partially obscuring an equatorial bulge/ball (EB), behind which part of the opposite, receding PBF is visible. The 87A PBFs and EB are approximately equally bright, in contrast to what polarization observations imply for Type Ia SNe (see §5).

A binary merger of two electron degenerate stellar cores (DD – in isolation these would be white dwarfs [WDs]) has been proposed for 87A (Podsiadlowski & Joss 1989), and the triple ring structure has recently been calculated in this framework (Morrin & Podsiadlowski 2007). Many other details of 87A, including the mixing (Fransson et al. 1989), the blue supergiant progenitor, the early polarization (Schwarz & Mundt 1987; Barrett 1988), and the 2.14 ms optical pulsations (M00a,b), strongly support this hypothesis.

The first clear evidence for DD-formed MSPs coincidentally came in the birth year of 87A, with the discovery of the 3 ms pulsar, B1821-24 (Lyne et al. 1987), in the non-core-collapsed (nCCd) globular cluster (GC) M28. Subsequently many more were found in the nCCd GCs, such as 47 Tuc, over the next 20 years, and attributing these to recycling through X-ray binaries has never really worked (Chen et al. 1993), by a few orders of magnitude.

The 0.059 arc s offset of the MS from the PP coincides with the PBF bearing of 194° (and thus along the axis of its DD merger), some 45° off our line of sight, corresponding to 24 light-days (ℓt-d), or 17 ℓt-d in projection, it taking light from 87A only eight extra days to reach the Earth after hitting the MS, and there is evidence for exactly this delay (see below). In addition, the typical 0.5° collimation for an ℓGRB, over the 24 ℓt-d from 87A to its PE, produces ~100 s of delay, within the range of the non-prompt components of ℓGRBs.

3Relatively slowly rotating, recycled pulsars weighing 1.7 M⊙, in the CCd GC, Ter 5 (Ransom et al. 2003), have removed high accretion rate from contention as a alternative mechanism to produce the MSPs in the nCCd GCs. The three MSPs in Ter 5 with periods < 2 ms, Ter 5 O, P, and ad (Hessels et al. 2006), may have been recycled starting with periods near 2 ms. There are four in this sample with periods between 2.05 and 2.24 ms, and another, the first from Arecibo ALFA, at 2.15 ms (Champion 2007).

4The far-side (southern) minor axis of the equatorial ring has a bearing of 179°.
3. The Early Luminosity History of SN 1987A

The early luminosity histories of 87A taken with the Cerro Tololo Inter-American Observatory (CTIO) 0.41-m [Hamuy & Suntzeff 1990] and the Fine Error Sensor (FES) of the International Ultraviolet Explorer (IUE) [Wamsteker et al. 1987], both show such evidence of the BJ and MS (Fig. 1)\(^5\).

Following the drop from the initial flash, the luminosity rises again to a maximum (‘A’ in Fig. 1) of magnitude 4.35 at day 3.0, interpretable as the hotter, more central part of the BJ shining through/running ahead of the cooler, roughly cylindrical outer layers which initially shrouded it. This declines (‘B’) to magnitude 4.48 around day 7.0, interpretable as free-free cooling of, or the loss of the ability to cool for, an optically thin BJ. The initial flash should scatter in the PE at day 8, and indeed ‘C’ shows ∼2×10\(^{39}\) ergs/s in the optical for a day at day 8.0, and a decline consistent with the flash after that, indicating a CF >10\(^4\) for this component (beam). A linear ramp in luminosity starting near day 10 indicates particles from the BJ penetrating into the PE, with the fastest traveling at >0.9 c, and a particle CF >10\(^4\). A decrement\(^6\) of ∼5×10\(^{39}\) ergs s\(^{-1}\) appears in both data sets near day 20 (‘D’). The CTIO point just before the decrement can be used as a rough upper limit for the MS luminosity, and corresponds to an excess above the minimum (near day 7.0) of 5×10\(^{40}\) ergs s\(^{-1}\), or magnitude 5.8, the same as that observed in H\(\alpha\) for the MS at days 30, 38, and 50.

4. The SN 1987A link to GRBs

Without the H and He in the envelope of the progenitor of 87A, Sk -69°202, the collision of the BJ with its PE (which produced the MS) might be indistinguishable from a full \(\ell\)GRB [Cen 1999]. This realization, together with the observation that no \(\ell\)GRBs have been found in elliptical galaxies, and the realization that the DD process must dominate (as always, through binary-binary collisions) by a large factor the NS-NS mergers in these populations, even when requiring enough WD-WD merged mass to produce CC, leads to the inescapable conclusion that the DD process produces sGRBs in the absence of CE and PE, the means

\(^5\)The CTIO V band center occurs at 5,500 Å, as opposed to 5,100 Å for the FES, and in consequence, the FES magnitudes have been diminished by 0.075 in Fig. 1 to account for the resulting luminosity offset.

\(^6\)This is preceded by a spike of up to 10\(^{40}\) ergs s\(^{-1}\) in the CTIO data, with the unusual colors of B, R, and I, in ascending order. Optical pulsations were not seen during this early period (R. N. Manchester, private communication, 2007). The possibility of less-than-coherent pulsations, though, is harder to eliminate.

\(^7\)Otherwise it would just beg the question of what distant, on-axis such objects would look like.
by which they would otherwise become ℓGRBs. Given that the sGRBs in ellipticals are due to mergers of WDs, we can conclude that: 1) the pre-CE/PE impact photon spectrum of ℓGRBs is known, 2) sGRBs are offset from the centers of their elliptical hosts because they are WD-WD mergers in their hosts’ GCs (to produce most of their MSPs – Gehrels et al. 2005), and 3) NS-NS mergers may not make GRBs as we know them, and/or be as common as previously thought a disappointment to gravitational observatories.

Through their interaction with the overlaying CE and/or PE, BJs produce the wide variation in GRB/X-ray flash properties observed from DD SNe of sufficiently low inclination to the line of sight, and the flavors of the 99% of GRBs due to DD depend only on observer inclination, CE and/or PE mass, extent, and abundance. Of the three different classes of GRBs, ℓGRBs, sGRBs, and the intermediate time, softest GRBs (iGRBs), as recently classified by Horváth et al. (2006, see also Middleditch 2007), most sGRBs occur from DD WD-WD merger without CEs or PE, ℓGRBs pass through at least the PE (necessary for small angle deviations to produce ~100 s of delay), and usually the CE (which, in addition to the PE, can soften the burst), while iGRBs pass through red supergiant (RSG) CEs, but little or no PE, possibly the result of a merger of two stars with very unequal masses, the possible cause of SN 1993J, which had an RSG progenitor (Podsiadlowski et al. 1993). The ~10 s limit for $T_{90}$, and its substantially negative slope (tradeoff) with $H_{32}$ for the iGRBs, are consistent with an RSG CE, but no PE. As in the case of ℓGRBs, the pre-CE impact photon spectrum of iGRBs is also known.

5. DD in Type Ia/c SNe

The list of good reasons against SD for Ia’s is long: (1 & 2) no SN-ejected or wind-advected H/He (Marietta et al. 2000; Lentz et al. 2002), (3) ubiquitous high velocity features (Mazzali et al. 2005a), (4 & 5) SiII/continuum polarization (CP) both inversely propor-

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8Thus sGRBs may not flag NS-NS mergers, which may last only a few ms, the same timescale as the 30-Jy DM=375 radio burst (Lorimer et al. 2007), far shorter than sGRBs (Hansen & Lyutikov 2001).

9At 1.6% and 1.0% (Trammell et al. 1993) the early polarization of SN 1993J was twice that of the 0.9% and 0.4% observed from 87A, consistent with even more axiality than that of 87A.

10The fluence of both the non-prompt and prompt parts of off-axis ℓGRBs are suppressed, the first by scattering in the PE, the second by being off axis by the time it emerges from the CE, frequently leaving both roughly equally attenuated. This scenario also explains why the two ("precursor" and "delayed") have similar temporal structure (Nakar & Piran 2002). Negligible spectral lag for late (~10–100 s) emission from "spikelike" bursts (Norris & Bonnell 2006) can be explained in terms of small angle scattering off the PE, without invoking extreme relativistic $\Gamma$’s.
tional to luminosity (IPL – Wang et al. 2006; Middleditch 2006), (6) no radio Ia SNe (Panagia et al. 2006), (7) four Ia’s within 26 years in the merging spiral/elliptical galaxies comprising NGC 1316 (Immler et al. 2006), (8) >1.2 M⊙ of 56Ni in SN 2003fg (Howell et al. 2006), (9) cataclysmic variables are explosive (Scannapieco & Bildsten 2005), and (10) DD SNe are needed to account for the abundance of Zinc (Kobayashi et al. 2006).

No observation of any recent SN other than SN 1986J and SN 2006gy, including all ever made of Type Ia SNe, is inconsistent with the bipolar geometry of 87A. Thus, especially in the light of SD’s serious problems, it seems likely that Ia’s are also DD-initiated SNe, of which some still produce TN yield, but with all producing weakly magnetized MSPs.

Further, it seems likely that Ia’s and Ic’s form a continuous class, classified as Ic’s when viewed from the merger poles, if sufficient matter exists, in excess of that lost to CC, to screen the Ia TN products (a rare circumstance in ellipticals), because this view will reveal lines of the r-process elements characteristic of Ic’s. All this complicates the use of SNe Ia in cosmology, because many Ia/c’s in actively star-forming galaxies (ASFGs) belong to the continuous class, and Ia’s in ellipticals (and some in ASFGs) may not produce enough 56Ni to be bolometric (Pinto & Eastman 2001), lying as much as two whole magnitudes below the width-luminosity (W-L) relation (the faint SNe Ia of Benetti et al. 2005).

A “missing link” of Ia’s must exist, more luminous than ‘faint’ SNe, which fall below the W-L relation by a tenth to a whole magnitude, may still be largely absent from the local sample, but may not be easily excluded by the TiII λλ 4,000-4,500 Å shelf. There is a more luminous class of Ia’s, found almost exclusively in ASFGs, that may be attributed to CE Wolf-Rayet stars (see, e.g., DeMarco et al. 2003, Howell et al. 2001, and the data in Górný & Tylenda 2000), and a less luminous “leaner” class, found in both ellipticals and ASFGs (Hamuy et al. 2000, Sullivan et al. 2006, Wang et al. 2006), which can be attributed to CO-CO WD mergers. In the DD paradigm, the Ia mass, above the 1.4 M⊙ lost to CC, determines the optical luminosity. Since optical afterglows have been found in sGRBs with no

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11This SN, in the edge-on spiral galaxy, NGC 0891, exceeds the luminosity of the Crab nebula at 15 GHz by a factor of 200 (Bietenholz et al. 2004), and thus is thought to have occurred because of iron photodissociation catastrophe (FePdC), producing a strongly magnetized NS (the origin of magnetic fields in NSs is still poorly understood, though it is believed that thermonuclear [TN] combustion in a massive progenitor to an Fe core is related).

12As with 87A-like events, it would again beg the question of “What else they could possibly be?,” and “delayed detonation” (Khokhlov 1991), or “gravitationally confined detonation” (Plewa et al. 2004), do not produce the IPL polarization. And unless the view is very near polar, this geometry can produce split emission line(s) on rare occasions, as was seen in SN 2003jd (Mazzali et al. 2005b), and thus again there is no need to invoke exotica, or an entire population (III) to account for GRBs (Conselice et al. 2005; M04).
SNe (Gal-Yam et al. 2006; Fynbo et al. 2006; Della Valle et al. 2006; Gehrels et al. 2006), DD Ia’s can be very lean indeed. It is not at all clear if SD ever happens.

If Ia/c’s are indeed the result of the same process that underlay 10–15 $M_\odot$ in 87A, but which instead only underlay 0.5 $M_\odot$, the outcome will be even more extreme than the geometry of the SN 1987A remnant. The PBFs will have higher velocities, and the equatorial/thermonuclear ball (TNB) will be much brighter, due to the greater concentration of $^{56}$Ni. Thus PBFs form linearly extended structures, whose brightness pales in comparison to that of the spheroidal TNBs, which explains why Ia continuum and SiII polarization are both IPL (Wang et al. 2006; Middleditch 2006), and also indicates that part of these lines must originate from the sides of the Ia/c PBFs.

Ia/c PBFs depart and/or thin out quickly because of their high velocities and limited masses, potentially exposing a fraction of the TNBs during the time interval when $\Delta m_{15}(B)$ is measured. Ia/c’s with PBFs initially showing r-process lines, because of views closer to the poles of the DD merger, are frequently excluded from the local sample as part of overdiligent attempts to select a “pure” sample of Ia’s. This selection doesn’t work as effectively on the distant sample, and the result will be distant SNe which are too faint for the redshift of their host galaxies. Figure 3 of Middleditch (2007) shows how this effect could spuriously produce half of $\Omega_\Lambda=0.7$ for a co-inclination (co-i) of 30° and a PBF of half angle of 45°. More realistic TNBs which begin as toroids could produce a big effect even for low co-i’s.

6. Conclusion

We have argued that the DD SN 1987A, its beam/jet, Mystery Spot, and possible 2.14 ms pulsar remnant, are intimately related to as many as 99% of GRBs, MSPs, and other SNe, including all Type Ia SNe, a grave concern for Ia Cosmology. The time lags, energetics, and collimation of $\ell$GRBs are consistent with those of 87A’s BJ and MS, and there is no need to invent exotica, such as collapsars, to satisfy them, the expansion of the Universe may not be accelerating, and there may be no Dark Energy. Recent observations have also cast significant doubt on the existence of dark matter as well (Nelson & Petrillo 2007).

Given this new, very complex picture of Ia’s, any sample, with a very low dispersion in magnitude, is hardly reassuring. A rigorous treatment of Ia data rules out all cosmologies (Vishwakarma 2005). A straightforward argument indicates that NS-NS mergers may not make any GRBs as we know them, and/or occur nearly as frequently as previously thought. Models of SNe to date are flawed because neither the DD process, nor strong magnetic fields have been included, developments that may still be at least a decade away. Certainly, no
relatively nearby FePdC SN has been well studied, SN 1986J having occurred during 1983.

The DD mechanism ensures that nearly all SNe are born from a maximally rotating, post-merger WD with a rotation period near 1.98 s, thus rapid rotation can not be invoked as an unusual circumstance, for the case of SN 2003fg, to justify “super-Chandrasekhar-mass” WDs (SCMWDs). The \( > 1.2 \, M_\odot \) of \(^{56}\text{Ni}\) it produced may only mean that CC underneath mixed TN fuel can initiate very efficient combustion/detonation,\(^{13}\) the paltry amounts of \(^{56}\text{Ni}\) associated with Ib’s and at least 90% of IIs being the result of dilution of their TN fuel with He and/or H due to the DD merger process. Thus SN 2006gy may not be a pair-instability SN (Smith et al. 2006),\(^{14}\) only a massive FePdC SN, which may have produced \(~ 20 \, M_\odot\) of \(^{56}\text{Ni}\), and a strongly magnetized NS remnant, a prediction which can be tested soon.

Although it would appear that a Universe without collapsars, pair instability SNe, SCMWDs, and frequent NS-NS mergers which make sGRBs, is much less “exotic” than previously thought, SNe themselves are plenty exotic enough, with 1% producing a strongly magnetized NS remnant/pulsar, and the remaining 99% caused by DD, producing \(~ 2 \, \text{ms}\) pulsars, and BJs which can incinerate half the planet from a great distance with little or no warning. This is what we will spend a good deal of the first half of this century figuring out.

I am extremely grateful to CCS-3 for supporting me during an interval when I was without funding. I would like to thank Drs. Aaron Golden, Geoffrey Burbidge, Falk Herwig, Peter Nugent, and an anonymous referee for useful suggestions which helped me to improve an earlier version of this manuscript. I would also like to thank Jerry Jensen for conversations and bringing this issue to my attention. This research was supported in part by LDRD grant DR2008085 and performed under the auspices of the Department of Energy.

\(^{13}\)The spectroscopic demands of a significant mass of unburned fuel, such as O, being invalid because of the invalid paradigm under which such estimates were made.

\(^{14}\)The inner layers of all FePdC SNe, possibly many \( M_\odot \) of Si, Ne, O, and C, have not been diluted with H and/or He by DD, and thus may ignite/detonate upon CC, and burn efficiently. SN modelers therefore face the unenviable choice of calculating FePdC SNe, which involve strong magnetic fields, or DD SNe, which involve a great deal of angular momentum and demand GRBs as an outcome (see \(^{[4]}\)).
7. Appendix I: The Primal Scream Rejection

Prologue: The test of a good review is whether the arguments in it could be sustained in a public forum. Neither the very long review presented immediately below, nor the very short one that follows as Appendix II, pass this test:

Dear Jon,

Enclosed please find the EDITED referee’s report on your submission to the ApJ entitled “The SN 1987A Link to Others and Gamma-Ray Bursts” (MS# 72836).

Please don’t let the occasional harsh words in the report distract from the point that the referee identifies many critical flaws in the manuscript. It seems to me that withdrawal would be the best option. If you feel that the referee does not understand your work well enough, you could request a new and independent referee. I would support that, BUT if the 2nd report is negative as well I would have no choice but to reject the paper.

Please let me know asap whether or not you agree with my suggestion to withdraw the submission.

If you have any questions, feel free to contact me.

I am sorry the news did not turn out to be more favorable. With best regards, Dieter

*************** Referee Report (with reply):

Whoa! A 2nd report?! That means I can get another gem like this one?!

Well duh, Dieter. This person is obviously not objective about a paper that turns much of that reviewer’s research into vapor. “This is not exactly a review, more of a primal scream.” – a colleague. I am surprised that ApJL even considers this valid.

Many of the reviewer’s objections are seriously out of date (Thielemann 1990?), and the rest have valid rebuttals. These issues are addressed in full detail below (the specific small changes made to the manuscript are not detailed herein.

This is my answer to the points made by this referee:

A common thread runs through the criticism of #72836. That thread is that calculations, or the lack thereof, trumps straightforward interpretations of observations. It is far too early, and the potential calculations far too complicated, to use their absence as criticism of observational interpretation. An example is the collapsar computations of MacFadyen and Woosley, which allow astronomers to continue on and claim that they are actually doing real science. I have a lot of respect for those who chose to hack away at such a difficult endeavor,
but it’s a perversion of the science to use them to filter out unpopular interpretations of observations. Calculations are basically theory injected into computers, and will NEVER fail to err without rigorous observational constraint, and I’m not talking about just a single split emission line from one SN.

It is indeed unfortunate, possibly even tragic, that an entire generation of young astronomers were misled by their mentors, because those mentors rushed to ignore the implications of valid observations of SN 1987A, even those which had been reproduced by more than one competent group of observers. Now a large segment of this generation is IN THE WAY, using up large telescope time in the fruitless search for Dark Matter and Dark Energy, neither of which will ever be found to exist (see Nelson & Petrillo 2007 on the absence of Dark Matter). “What they should be doing is studying the nearby SNe, on the smaller telescopes, but they don’t want to do that.” – a colleague. In the meantime, SN 1987A, the one nearby SN in nearly 400 years, has not been observed in high time resolution by anyone for over 11 years, and that’s criminal. The equatorial ring will be a factor of 10 brighter in 10 years, and yet another factor of 10 in the next 10. Time is running out. Far worse still, is that by holding out hope for new physics, this frenzy may be distracting political leaders from taking steps to mitigate global warming, and in that sense it is irresponsible for them to remain in their state of denial.

In this paper, John Middleditch proposes, using SN 1987A as his prime example, that essentially all supernova-like events are caused by the double-degenerate merger of two white dwarfs, presumably CO white dwarfs, leading to the formation of a rapidly spinning millisecond (ms) pulsar. The author suggests this as a unifying scheme that can explain all these events, which he finds intellectually attractive, although this is an aguable [sic] point, considering the diversity of observed explosions.

While the paper makes a couple of interesting points that could stimulate discussions in the field, some of the main claims are either wrong or unproven and can only be put forward by ignoring the wealth of detailed scientific literature (see below for more details). Because of this, I do not think that this paper can be published. It would be acceptable to have a speculative paper that stimulates ideas in a new area of research, but ignoring well established facts and a whole section of the relevant literature is scientifically unacceptable.

Au contraire, my suggestions are quickly becoming the established norm among most of those working in the many subfields (see below).

The main idea of the author is that a double-degenerate merger can produce a diversity of observable supernovae depending on whether it is surrounded by a common envelope or depending on its viewing angle. While it is plausible that this leads to different observational
events, the link of this postulate to observed supernova types is less than convincing. It is generally believed (for good scientific reasons!) that there are at least two different explosion mechanisms, core collapse and thermonuclear explosions (leaving GRBs aside for the moment).

Toward the end of the Wednesday afternoon session of the SN 1987A, 20 Years After and GRB conference in Aspen on Type Ia SNe, the question was asked: “Is there any way of avoiding double-degenerate for these [Type Ia SNe]?” Someone ventured an answer, but Nino reminded him that his suggestion had already been discredited. There was no other reply.

Bob Kirshner was there.
Craig Wheeler was there,
Alex Filippenko was there.
Tom Janka was there.

So the current thinking on Ia’s is that they ARE indeed DD, which means that they are core-collapse objects producing NSs, which likely will indeed be weakly magnetized and rapidly spinning.

What does THAT say about calculations of “gravitationally confined detonation”?
What does that say about calculations of “delayed detonation”.
For that matter, what does it say about “collapsars” being real?
And if Ia’s are DD, why not other SNe of progenitors of modest mass, which share at least early polarization in common?

The author seems to invoke only core collapse even to explain SNe Ia that on average eject 0.6 Msun of Ni and sometimes substantially more. How is this possible in this scenario? The author briefly addresses this issue in the third but last paragraph, but that discussion is little more than uninformed gobbledegook and reveals an astonishing lack of understanding of basic supernova physics. All of a sudden he refers to “efficient combustion/detonation” to produce core collapse, i.e. a ms pulsar, and 1.2 Msun of Ni? These numbers just do not add up. He now introduces the concept that different ways of mixing “TN fuel” produce different types of events. None of this discussion refers to any proper simulation of double-degenerate mergers or makes any suggestion for the cause of the differences from event to event. In this context, it should be noted that there has been substantial progress in understanding thermonuclear explosions from first principles that are generally believed to produce SNe Ia.
While there are still some detailed arguments concerning, in particular, the transition to a detonation, the basic paradigm is very sound and cannot just be ignored.

The single degenerate paradigm for Ia’s is lying on the floor, shattered into pieces (see above), so this criticism is irrelevant.

Again, also, the science is being perverted. A super-Chandrasekhar mass WD is a fundamental violation of known physics, and its existence should require extraordinary evidence, not just so much blather about unburned mass determined from some lines, especially if these were interpreted in the context of a wrong paradigm.

The inference of the spectroscopy as regards to amount of unburned material has never been redone in context of DD, and the resulting bipolar SNe. The high luminosity of the thermonuclear ball is no guarantee because the polar cones/jets can shade/expose it.

There is clearly a lot of confusion, even in the author’s mind, what he means by a double-degenerate merger, but let me now address the various sections more systematically.

The folks at Aspen had no problems with it.

SN 1987A:

The author uses SN 1987A as his prime example for a double-degenerate scenario. He points out correctly that this was an unusual event, specifically referring to the bipolar nebula and the mystery spot, both of which could be indicative of rapid rotation. Indeed, he points out that the probably most promising model for the progenitor invokes the merger of two stars, though not as he claims of two electron-degenerate cores but of a red supergiant with a normal-type star. A double-degenerate merger inside a common envelope, as he proposes, could possibly also explain some of these supernova features, but it cannot explain many others. First, the merger occurred 20000 years before the explosion (based on the dynamical age of the nebula). Why would the star look like a blue supergiant for 20000 years? ... Easy – too much angular momentum. A blue straggler on steroids. Does this reviewer seriously believe that SN 1987A had an Fe core?

Even if the core collapse event could be delayed by 20000 years (this may well be possible), the merged cores surrounded by a large envelope would almost certainly have the appearance of a red supergiant, just like any 10 Msun star with a compact core of about 2-3 Msun, not a blue supergiant (if the author is convinced otherwise, he would have to demonstrate this by a reasonable calculation).

Again, too much angular momentum. ApJL is a journal where one can make reasonable suggestions, without having to take a year each to run calculations (which have been
discredited anyway – see above) on every little detail. In addition, are current calculations even capable of resolving this question? I doubt it. It is not even clear what the criticism is about. The reviewer is just trying to stall this paper, in case he can’t kill it, which is embarrassing because he/she and so many others were so clueless for such a long time. Ia’s have been found to be DD at Aspen, and at Santa Barbara, they were apologetic about it, and the whole house of cards is still collapsing as I write. I don’t OWE them waiting until 20 minutes after THEY decide the paradigm has shifted, before I can write about it.

\[ \text{I have used these particular values, trying to imagine how such a scenario could work, taking a positive constructive view, but this reflects another generic problem with the paper, namely that it is lacking enough details to allow a proper evaluation.} \]

Again, it is easy to suggest that detailed calculations be made every time a suggestion is offered, with the full knowledge that ApJL is not the place where there is room to do this. And again, it’s a stalling tactic.

The second, even more severe problem is that we know from the analysis of the supernova ejecta that the core of the star that exploded had a H-deficient core of at least 5 Msun (see, e.g. the work by Thielemann 1990, ApJ, 349, 222, but also many [!] other people like Arnett, Woosley). This is not compatible with a double-degenerate merger that can produce at most 1.5 Msun of non-H ejecta (assuming that 1.5 Msun went into a neutron star).

A 1990 paper is pretty much out of date. Woosley, Burrows and others at the Aspen conference made no attempt to eliminate, or discredit DD as a possible hypothesis for SN 1987A. Tom Janka paid tribute to Philipp’s and Morris’s work on the SN 1987A rings, and its implications supporting binary merger (and he still went on to use an Fe core, because no one’s ready to calculate DD).

Again, if the author had good scientific reasons to challenge the other studies, it would be up to him to demonstrate why these detailed studies are wrong or at least, at the very minimum, show that his model can produce the basic, observed characteristics of the SN 1987A ejecta. Anything else only qualifies as a fantasy product, not science.

The reviewer is trying to draw the line here about DD for 87A, but with most or all Ia’s DD, it just doesn’t wash. In Iabc’s the dominant mechanisms will be evolution- or collision-induced mergers. In IIs, evolution-induced merger will dominate. So what? With the rings, the bipolar explosion, the mixing of the elements, the blue SG, the Mystery Spot and its coincidence with the early light curve, and, YES, the 2.14 pulsar (see below). If SN 1987A wasn’t a DD SN, I don’t know any more clues about it a SN can possibly have. A full treatment of SN 1987A ejecta under the DD paradigm is a huge task, and will take a few years, and is homework for the modelers.
GRBs:

The author also tries to link SN 1987A to GRBs more generally. He points out, correctly, that the DD process must dominate by a large factor over NS-NS mergers in early-type galaxies and then continues that this “leads to the inescapable conclusion that the DD process produces sGRBs”. I am sorry, but this is a simple non-sequitur, since it would first need to be demonstrated that DD mergers can produce a GRB in the first place. Again the author ignores detailed work on double-degenerate mergers (e.g. by Rosswog, Janka and others) and how this can lead to a truly relativistic event. Nevertheless, in the area of GRBs the uncertainties are large enough that a DD merger can probably not be ruled out (the author may want to look at the work by Todd Thompson and collaborators since that could potentially support some of his suggestions), but the logic as presented is not really tenable.

THIS is the argument that drove Rejean Dupuis out of LIGO and into a banking career! And also where the screaming gets loudest (see the appendix to astro-ph/0608386). See above for 87A being due to a DD merger – it’s credible enough, certainly by the standards of a subfield where collapsars get accepted where there is no compelling evidence for them whatsoever. Events (Lorimer et al. 2007) have proven me correct on this account, as NS-NS mergers have a different signature than GRBs.

This reviewer just says he doesn’t like the logic, but really never says why. Much of this is just a version of name-calling. Tom Janka was in the audience when I gave this talk that Tuesday morning at Aspen. He didn’t challenge it. We were both around all the rest of that long day, looking at each other. Tom never engaged me on these issues. The logic of Horvath et al. 2006 applies BOTH ways – they did not see the need for any other subclass of sGRBs on the low T90 side. So >90% of them are WD-WD mergers, or DD. Who really believes there could be that many NS-NS mergers? Aside from that, the 30-Jy, ∼5 ms, DM=375 radio burst, http://www.sciencemag.org/cgi/content/full/318/5851/777, is an obvious candidate for a NS-NS merger, the ∼100/day/Gpc^3 rate being consistent with Kalogera’s estimate of 3/day/Gpc63 when inspiraling DTN NS-NS pairs are counted.

SNe Ia:

The logic gets even more mangled in the next section, since all of a sudden the same DD mergers are also invoked to account for SNe Ia and normal SNe Ic. The author does not really explain how DD mergers produce this diversity, except to vaguely refer to BJs, PEs, MSs and other abbreviations the author introduces, which mainly serve to obfuscate the logic of the paper. The author postulates further that the difference between a SN Ia and Ic is one of viewing angle. While at early times this might be a possible interpretation, it is not at late times; in the late nebular phase the ejecta are transparent, and it is straightforward
to measure the total amount of Ni produced in the supernova from the nebular Fe lines, irrespective of viewing angle. This has been done extensively for nearby supernovae (e.g. the work by Meikle, also Mazzali et al.), demonstrating that the differences of these supernova types cannot just be due to viewing angle. This example illustrates quite blatantly that the author does not really know much about supernova research.

At late times the spectra of Ia’s and Ic’s are nearly identical. Again, interpretations assuming spherical geometry for bipolar explosions may be seriously in error. Mazzali may have made the claim that Ia’s can’t be Ic’s when viewed from the poles, but he’s also one of the number who stated about Ic’s in abstracts that they “must be massive stars,” a claim that has not held up, and one which Craig Wheeler has also cautioned against. Mazzali should be praised for his observations, and beaten for his abstracts. Aside from that, Ia’s are DD, NOT thermonuclear disruption.

Are all NSs formed as ms pulsars?

The author’s model implies that most neutron stars are born as millisecond pulsars. Indeed, I remember that in an earlier version of this paper (on astro-ph) the author claimed that this was the case. Even though this section has been removed (for good reasons), this implication does not appear consistent with what we know about radio pulsars. Again the author would have to ignore a whole wealth of literature on this topic, in particular from the recent Parkes multi-beam survey. In this context, the author claims that the standard recycling scenario for ms pulsars does not work. Here he ignores recent discoveries of ms pulsars in X-ray binaries, i.e. in the process of being recycled (see in particular the work by Bhattacharya), which has quite impressively confirmed the basic recycling paradigm. The only thing the author could challenge is the recycling + evaporation scenario for the production of *single* ms pulsars for which a DD collapse provides a respectable alternative.

The point X-ray source in Cas A is radio quiet. So much for radio pulsars and the logic of the lamppost. TeraGauss NSs appear to be a rare minority, and this is supported by the lack of hot centers in recent nearby SNe, where SN 1986J is so far the only known exception (and we will know soon enough for the case of SN 2006gy).

The recycling scenario has been on the ropes since I found the 1st MSP in a globular cluster two decades ago, and X-ray MSPs don’t save it. I asked Fred Lamb whether we knew these pulsars were recycled or born fast, and his answer was the we don’t know, and I don’t think his mind has changed since. There’s no way of telling how an MSP was formed, whether born that way, most likely in a binary-binary merger-induced core-collapse, or recycled from a companion acquired after it has died as regular TeraGauss pulsar. However this last possibility requires field decay, or at least effective dipole reduction, because its magnetic
poles have migrated to opposite sides of one of the magnetic poles ala Chen and Ruderman (1993), but it’s not clear whether such a geometry can be as easily recycled as a truly weak magnetic field. The binary-binary collision-produced MSP can inherit a companion from the process, no need to go looking, and field decay is not required. And THESE can be recycled from such companions, and there’s no way to tell the difference.

This paragraph also ignores the implications of two recycled pulsars in binaries in Ter 5, with the fastest at 10 ms and change, both of which weighed in at 1.7 solar (a similar situation holds for M5). Also from Chen, Middleditch, and Ruderman 1993, the pulsars in the core-collapsed GCs and non core-collapsed GCs also indicate that recycling can’t get TeraGauss pulsars into the true ms range. With injection from 2 ms DD PSRs, which already have weak magnetic fields, recycling can reduce their periods below 2 ms, a validation for Ghosh and Lamb 1979, without requiring field decay.

Logic of Presentation:

I have already indicated some serious problems of logic in the paper, but there are many more instances where the author makes claims without any substantiation of the claim (or valid reference). In several cases, the author gives references, but seriously mis-quotes the papers he is referring to. This in itself is scientifically unacceptable.

Here are a few examples:

- Introduction: claim that the beaming factor of GRBs is $10^5$. The only reference given is to a paper by Meszaros who does not claim this. Reasonable estimates are 100 to $10^3$. I believe there is one paper by Don Lamb suggesting a much larger beaming factor but that claim has not survived further scrutiny, as even Don Lamb has implicitly admitted in later papers.

The behavior of the light curve of 87A around days 7-10 indicates that the beam spot and particle jet spot on the polar ejecta must be smaller than 1 lt-day. The distance is $\sim 20$ lt-days, so the beaming factor for 87A was higher than 10,000. The paper was so modified, and there is no need to solicit anyone’s opinion. Anyway, one suspects that the motivation to revise the beaming factor downward, at least for IGRBs, is just to substantiate the claim that they result from exotic, hence rare (or vice-versa), events, thus keeping the hyperbole flowing.

- Footnote 1: claim that the “discovery” of a 2.14 ms pulsar in the SN 87A remnant “is no longer controversial”. I beg to differ. It has not been seen by other groups who should have been able to see it since they were looking at similar times.

The story of high time resolution observations of 87A made by others is a is sorry tale
of incompetence and inadequate effort.

The object was found in data from many telescopes and observatories. Sure, I did the first pass analyses, but reputable collaborators have also verified the signals, and the data have been offered to anyone who requests it. The probabilities in Middleditch et al. 2000 are generous enough. They are not off EIGHT orders of magnitude. The Tassies didn’t hallucinate their data. Who is this reviewer that he thinks he can ignore OUR published observations, while claiming that I can’t ignore flawed and/or inadequate/non-existent observations of others?

We’ve even had a night in common with another group, with an agreement to share the data (I have a slide of the guy with Jerry Kristian on the afternoon of 1992, Nov. 6, in the Las Campanas 2.5-m control room). The promised data was never delivered, even though we did ask for it. (WHY? Written over inside the laptop? Lost? Absolute verification of the signal too damaging to astronomers? Ergo decades of work down the drain? Likely written over)

The guy said “I don’t see much.” Kristian said “That’s not real helpful.”

By those standards, we didn’t see MUCH, but there was something there, and a common observation night, even with a less restrictive filter would have told us a LOT. We used a Wratten 87A (basically the I band, 800-900 nm). The guy used a GG495, basically a 500 nm longpass. There is a factor of 10 difference in count rate on 87A between these two on the same telescope. If the signal were present for the entire GG495 band, he would have seen 20 times the power that we saw. If it were restricted to the Wratten 87, we would have seen a factor of 5 times more power than he saw. We never could convince ourselves that including the 500-800 nm band ever did anyone any good. But isn’t that what simultaneous observations CAN do for us? What an unforgivable waste!

I also pointed the HST/HSP collaboration to candidate frequencies for which we found a signal in their data on June 2, 1992, and March 6, 1993. For whatever reason they did not respond then, wrote a paper claiming an upper limit of 27th magnitude, which Kristian and I refereed. We informed them that it was really 22nd (100 times brighter). The HSP count rate on any object of known magnitude will verify that the instrumental throughput is 1% at best, and from this, limits can be set from the total number of counts in ANY observation. We told ApJ that we’d like to see the paper again before it got published. Next time we saw the paper it was published with a limit of 24.5 (still exaggerated 10 times too dim). A representative of the collaboration showed up at the SN 1987A – 10 Years After conference in La Serena, Chile, and tried to argue for this limit, at least until he showed a power spectrum of his calibration object. When I informed him that the object had 10 times
less background than SN 1987A, and was also integrated over a 50% longer time interval, he could only leave the stage, muttering. Also, a total of 160 minutes of observations of SN 1987A in a couple or YEARS? It’s like they planned to FAIL!

Manchester and Peterson published on not seeing a signal in Dec. of ‘94. I looked at their data, they really didn’t see anything. But they spent only part of the two nights on 87A. In fact, at the Aspen SN 1987 & GRBs Conference during Feb. 19-23, over a dozen YEARS after this observation (with none in between as far and I know), Manchester made a whole contributed talk on the basis of this observation, plus the published times 10 exaggerated faint limit (24.5) from the HSP. As I had done a decade earlier in La Serena, I had to correct the exaggeration on the spot. Mark Phillips remarked to me afterward, “I thought he was there!” So did I! The kindest thing which can be said is that he went off somewhere for that part (he was certainly there prior to that). Also clearly, Manchester had never looked at HSP data, much like a lot of other people who THINK they know what the answer is about SN 1987A. This was not even corrected in Manchester’s proceedings contribution to the Aspen Conference. It quotes the HSP limit as “∼24” – 24 to 22 is quite a stretch even for a ‘∼’! He also quotes his limit on 87A from 4 100-minute segments during his and Peterson’s 2 nights on the AAT in 1994, early Dec., the last time he ever observed 87A, at 24.6. We observed 87A with the CTIO 4-m for 18.6 hours in 1993, late Dec., and achieved a limit of 24.0, detecting the 2.14 ms signal on all three nights at 24.77(0.2), 24.44(0.3), and 24.78(0.2) in the V, R, and I combined bands (using a gold secondary) about 2/3rds of the count rate of an aluminized secondary. So his 24.6 limit is likely exaggerated by at least a magnitude.

Aside from that, it’s not like there aren’t 10 solar masses of starguts moving around (also no guarantee that the pulsar remnant isn’t precessing and potentially changing its beaming). As far as I know, they also had no observations during the interval from Feb. of ‘92 through Sep. of ‘93 (they tried on September 15, 1993, but were clouded out – signals were seen from Tasmania on the 12th and 24th), when we were detecting the signal most consistently. Remember, HST was still nearsighted during that interval.

So THAT was our competition.

Section 2. “PBF - the prime suspect for the r-process”; not necessarily wrong, but a statement without reference or explanation.

OK, fine. “... (PBF – a needed candidate for the r-process, e.g., Arnould et al. 2007) ...” Arnould, Goriely, & Takahashi 2007: “After some fifty years of research on this subject, the identification of a fully convincing r-process astrophysical site remains an elusive dream.”

Footnote 8: Claim that NS-NS mergers occur within a few ms. A reference is given,
but this one is outdated. Recent detailed simulations by Ruffert, Janka, Rosswog et al. have shown that this is not the case.

Hard numbers are remarkably absent from the abstract of their latest paper (III) on NS-NS mergers. Tom Janka was in the audience at Aspen when I gave my talk, including the bit about sGRBs being predominantly DD events. He did not comment then, and has not commented since.

Footnote 12: “What else could they be?” plus “there is not need to invoke exotica...” This sounds like desperation rather than well-founded scientific argumentation. Indeed, what is exotic to one person may not be exotic to another person.

Not desperation, but Occam’s Razor, a principle which, unfortunately, has been absent from much of the garbage that astronomers are promulgating in this subfield, PLUS the fundamental principles of astronomy itself – sources, parents, offspring, etc. Occam’s Razor cuts both ways. What DO Ia’s look like when viewed from their merger poles? If the MS of SN 1987A wasn’t related to GRBs, then what WAS it? It HAS to look pretty impressive when viewed from either pole, likely visible at cosmological distances. Where are THESE events in such samples?

In summary, I do not believe that the author presented a consistent and coherent case that all supernovae are related to DD mergers. Considering how much he has ignored the published literature, I do not think this paper should be published in a respectable journal.

Could the paper be modified to make it publishable? In principle, the author could revise the paper by removing some of its logical inconsistencies and addressing the relevant published literature. However, it would not be enough to just point out that there are numerous unresolved uncertainties in these models (which is definitely true). But since it is this author who is going out on the limb, it would be up to him to demonstrate by performing reasonable model calculations that DD mergers can account for the phenomena he is invoking them for. These would not have to be state-of-the-art multi-dimensional hydrodynamical simulations, but at least have to contain enough reasonable physics to support the author’s claims. Without these, the paper falls more in the category of a phantasy novel than a piece of respectable science. Considering that I judge many of the author’s key claims to be false, I doubt that he would be able to satisfy these requirements.

It is not I who is living in a “phantasy” world, but this reviewer and his/her brethren. Collapsars, SCMWDs, Dark Energy, pair instability SNe, and likely even Dark Matter and will all be found to be garbage (there is a still a chance that collapsars have something to do with black hole formation, but because DD can make, and extreme energetics are no longer required for, GRBs, collapsars are no longer needed to make GRBs), and SN 1987A provides
the leverage through which this will be accomplished. If not now, then when? If not by me, then by whom? I was right about the GRBs, I am being found right about MSPs, and I will be found to be right about SNe, and the pulsars they leave. And all this simply because I am rational, when many are not, and have a sense of what is garbage, and what is not.

Do these people care what the truth is, or have they abandoned that concept so that they can make their lives easier by doing things they are used to doing, instead of those that really need doing, but are much more challenging? At this level, the effects of academia mixed with a defunct grant system is preventing progress from being made in astronomy. Again “What they should be doing is studying the nearby SNe, on the smaller telescopes, but they don’t want to do that.” – a colleague. The revelations of this work show that much of astronomy has to be rebuilt from the ground up, and I can think of no better use for today’s graduate students. These folks have monopolized the big telescopes for the last decade or so, and think that hard work alone is enough to merit a continuance of this state of affairs, NO MATTER how the science breaks.

Like the DD issue for Ia’s, their paradigm(s) have/are crumbled/crumbling beneath their feet. The leaders of this crowd are getting around to admitting it – they wouldn’t have survived the last few conferences if they hadn’t. However, the message apparently has not filtered down to their followers (and this referee is among them), who act as if their audacious, and scientifically unsound assertions haven’t already been seriously challenged. They wouldn’t be so afraid of one dissenting paper if their own case weren’t already toppling around their ears like a house of cards.

When I knew I had a spurious result, I retracted it. After Dark Energy and a lot of other stuff is found to be garbage, these folks will likely just slink back to the halls of academe. Tom Siegfried’s take on the Santa Barbara SN meeting:

[http://www.sciencemag.org/cgi/content/full/316/5822/194](http://www.sciencemag.org/cgi/content/full/316/5822/194) is likely all that will happen. Support to do this bad astronomy amounts to WPA for astronomers. After all of this hullabaloo, astronomers will be lucky if anyone is still willing to give them funding. If they take yet another decade to get around to admitting their problems, then likely no one will give ANY of us funding EVER again.
8. Appendix II: The One Paragraph Diss Rejection

Dear John,

I have received a report from the referee on your revised ApJL paper cited above. A copy of the report is appended below.

The referee finds significant problems with your paper and recommends against publication. In view of the referee’s assessment of your paper, and the negative report of the previous version of the paper, we will not be able to accept this paper for publication in the ApJ.

I am sorry that the revisions did not lead to an acceptable paper. The referee is a very experienced, and also a very objective person (willing to go some distance on topics that are “out of the box” or “non-mainstream”), but the assessment was still negative - with very strong words regarding the impossibility of a further revision leading to an acceptable paper.

I hope you can find another appropriate venue to promote (as in publish) the ideas presented in your manuscript.

With best wishes: Dieter

It is easy to sit there and contend that the assertions are unsupported, the logic is vague and elusive, and that there is little evidence. It is easy to sit there and claim the arguments are too tenuous, knowing full well that this is the most developed possible set which still fits within the space allotted for an ApJ letter.

In fact the review itself is what is vague, elusive, and presents unsupported arguments, a classic case of the pot calling the kettle black. And oh my! How tired one must get when encountering the dread ‘SCMWD’ for the 2nd time! (SCM means something else to me.) We don’t know how this beam/jet from Hell was formed, we only have that picture of 87A, its early light curve, and data on the Mystery Spot. Most of us don’t know yet how pulsars shine. So what? That comes later, in a bigger paper. Requesting an explicit mechanism is just another way of stalling.

As vague and indefinite as it is, it correctly predicted that NS-NS mergers do not make GRBs (see above and Lorimer et al. 2007), the bimodality of the masses of MSPs in globular clusters (Freire et al. arXiv:0712.3826), the offsets of sGRBs from the centers of their host elliptical galaxies, the details of Ia’s, including their two faint subclasses, high velocity features, inverse relation between polarization and luminosity, and also makes a explicit prediction as to the cause and outcome of SN 2006gy. This pile of vague, unsupported
objections, disguised (poorly) as a review, is a symptom of what this branch of astronomy has become – a disingenuous exercise perpetrated on the American taxpayers so that astronomers can pretend their paradigms haven’t crumbled, and BS until the end of the Universe, allowing tiny little dollops of progress only when everyone has covered their behind about having been so utterly clueless about SNe, GRBs, and MSPs. They can’t argue these points in public, outside of the cloak of anonymity provided by the journal, as there really is no rebuttal to them.

I have read this paper three times with good will and a generous approach to lively scientific discussion. I reluctantly conclude that this paper does not meet the standards of the Astrophysical Journal. Although it refers to many interesting astronomical phenomena, the conclusions do not follow from the evidence and there is precious little evidence. If this paper contained a cogent and quantitative physical discussion of the way in which the observed phenomena in SN 1987A shown in Figure 1 are plausibly the result of the mass loss followed by beamed ejections from that object, it might possibly be suitable for the ApJ, but the present discussion is a series of qualitative unsupported assertions, followed by unjustified leaps to unrelated phenomena. The paper is almost impossible to read, due to a propensity to use novel abbreviations (SCMWD) for phrases repeated only a few times. There is a nugget of a scientific idea here, trying to unite a wide variety of phenomena with the notion of double degenerate mergers. But the evidence presented is so fragmentary and allusive that it does not constitute a scientific case for any of the proposals made here. It would be a mistake to publish this paper in the Astrophysical Journal. It would be a mistake to impose further on the editorial processes of the Journal and the goodwill of the scientific community by offering a revised version of this paper. I will not serve again as a referee for this paper.

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Fig. 1.— The very early luminosity history of SN 1987A as observed with the Fine Error Sensor of IUE and the 0.41-m at CTIO. Data points taken at Goddard Space Flight Center by Sonneborn & Kirshner, the Villafranca Station in Madrid, Spain, are marked (see §3).