Induced plant defenses, host–pathogen interactions, and forest insect outbreaks

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Edited by Stephen W. Pacala, Princeton University, Princeton, NJ, and approved July 25, 2013 (received for review January 13, 2013)

Cyclic outbreaks of defoliating insects devastate forests, but their causes are poorly understood. Outbreak cycles are often assumed to be driven by density-dependent mortality due to natural enemies, because pathogens and predators cause high mortality and because natural-enemy models reproduce fluctuations in defoliation data. The role of induced defenses is in contrast often dismissed, because toxic effects of defenses are often weak and because induced-defense models explain defoliation data no better than natural-enemy models. Natural-enemy models, however, fail to explain gypsy moth outbreaks in North America, in which outbreaks in forests with a higher percentage of oaks have alternated between severe and mild, whereas outbreaks in forests with a lower percentage of oaks have been uniformly moderate. Here we show that this pattern can be explained by an interaction between induced defenses and a natural enemy. We experimentally induced hydrolyzable-tannin defenses in red oak, to show that induction reduces variability in a gypsy moth’s risk of baculovirus infection. Because this effect can modulate outbreak severity and because oaks are the only genus of gypsy moth host tree that can be induced, we extended a natural-enemy model to allow for spatial variability in inducibility. Our model shows alternating outbreaks in forests with a high frequency of oaks, and uniform outbreaks in forests with a low frequency of oaks, matching the data. The complexity of this effect suggests that detecting effects of induced defenses on defoliator cycles requires a combination of experiments and models.

host-pathogen model | Lymantria dispar | complex population dynamics | spatial model | hydrolyzable tannins

Periodic outbreaks of forest-defoliating insects severely damage valuable timber and increase atmospheric CO2 levels by converting forests from carbon sinks to carbon sources (1). Decades of research have produced multiple hypotheses to explain defoliator outbreak cycles (2), but a decisive experiment to choose between competing hypotheses faces almost insurmountable logistical difficulties, because outbreaks occur at 5–30 y intervals and typically cover thousands of square kilometers (3). Efforts to support particular hypotheses have therefore instead relied on a mixture of observational field data, small-scale field and laboratory experiments, and mathematical models.

For example, the most widely accepted hypothesis is that defoliator cycles are driven by natural enemies. Support for this hypothesis comes first of all from observational data showing that defoliators experience high rates of infection by specialist pathogens and parasitoids in peak populations (2, 4) and high rates of attack by generalist predators and parasitoids in trough populations (5). Second, experimental data have confirmed key assumptions of defoliator–natural-enemy models, and the models produce long-period, large-amplitude cycles resembling time series of insect densities and defoliation levels (6).

Neither data nor models have provided meaningful support for an important alternative hypothesis, that defoliator cycles are driven by induced defenses. In many trees, antiherbivore defensive compounds increase in response to defoliation (7, 8), and such increases could in principle cause outbreaks to collapse. Direct toxic effects of induced defenses in experiments, however, are often weak, and the mechanisms underlying these defenses are often unknown or poorly understood (9). Moreover, there are no obvious signs of the effects of induced defenses in time series of defoliation or insect densities. Induced-defense models therefore provide no better an explanation for defoliator cycles than do natural-enemy models (10, 11), while additionally providing no explanation for mortality due to natural enemies. Given the successes of the natural-enemy hypothesis, these failures of the induced-defense hypothesis have led to the conclusion that induced defenses play little to no role in defoliator outbreak cycles (3). Here we argue that this conclusion is premature, by presenting evidence showing that induced defenses modulate outbreak cycles of the gypsy moth (Lymantria dispar) in North America.

We suspected that induced defenses play a role in gypsy moth cycles because recent analyses of defoliation data have revealed that gypsy moth cycles differ between forest types (12). In oak–hickory (Quercus–Carya spp.) forests, in which the aboveground tree biomass is 43% oaks, severe outbreaks have alternated with mild outbreaks, leading to a strong subharmonic oscillation in time series of defoliation (Fig. 1A and B). In oak–pine (Quercus–Pinus spp.) forests, in which the aboveground tree biomass is 15% oaks, outbreak severity has instead been roughly uniform, and there has been no subharmonic (Fig. 2A and B). Logistic regression (12) and spatially smoothed autocorrelation (13) analyses have confirmed that these differences are statistically significant.

This difference in outbreak cycles is unlikely to be due to differences in the physical environment, because climatic conditions are effectively identical between forest types and because

Significance

Many forest insects undergo outbreaks, in which their densities rise from undetectable to extremely high. Outbreaks are widely assumed to be driven by specialist natural enemies such as infectious pathogens, but gypsy moth outbreaks show alternating severe and mild outbreaks in forests with a high percentage of oaks, a pattern that cannot be explained by host-pathogen models. We used an experiment to show that induced defenses in red oak reduce heterogeneity among gypsy moth larvae in the risk of virus infection, and extending standard models to allow for this effect produces alternating outbreaks, matching the data. The ability of our model to reproduce this complex pattern suggests that the role of induced defenses in insect outbreaks has been underestimated.


The authors declare no conflict of interest.

This article is a PNAS Direct Submission.

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This article contains supporting information online at www.pnas.org/lookup/suppl/doi:10.1073/pnas.1300759110/-/DCSupplemental.
the soil-moisture differences that determine forest composition have no direct effect on the gypsy moth (12). Meanwhile, simple natural-enemy models that include a specialist baculovirus pathogen (14) and a generalist predator (5) can reproduce qualitative features of gypsy moth cycles (6), but standard models do not produce a subharmonic. Bjornstad et al. (13) therefore extended a natural-enemy model to allow for spatial variability in generalist-predator attack rates. Their work suggests that the gypsy moth is an invader and because invasion dynamics could lead to confounding effects, the data are based only on areas that were completely infested by 1975, which in practice meant mostly the New England and Mid-Atlantic sections of the United States (12).

We therefore considered whether the observed differences in outbreak dynamics between forest types could instead be due to differences in inducibility between genera of gypsy moth host trees. In the range of the gypsy moth in North America, defoliation induces hydrolyzable tannins in most oak species (16), including red oak (Quercus rubra) (17), black oak (Quercus velutina) (17), and chestnut oak (Quercus prinus) (18), whereas the effects of white-oak defoliation (Quercus alba) on gypsy moths are also likely due to increases in hydrolyzable tannins (19). Meanwhile, pines do not contain hydrolyzable tannins at all (20), whereas levels of hydrolyzable tannins in hickories are close to or equal to zero (21). The effects of induced hydrolyzable tannins on baculovirus transmission are therefore likely to be stronger in oak–hickory forests than in oak–pine forests because of the higher fraction of oaks in oak–hickory forests.

Direct toxic effects of induced defenses on gypsy moths are known to be relatively weak (22), but like many baculoviruses (23), the gypsy moth virus is transmitted when host larvae consume virus-contaminated foliage. Induced hydrolyzable tannins in foliage can therefore alter a gypsy moth larva’s risk of infection, but as we will discuss, previous laboratory evidence for such effects (24) was not consistent with field data (14). Induced birch defenses (Betula pubescens ssp. czerepanovii) can similarly alter the responses of autumnal moth (Epirrita autumnata) larvae to artificially implanted plastic filaments in the laboratory (25), but efforts to detect induction effects on autumnal moths in the field were likewise unsuccessful. Also, there is no obvious signature of induced defenses in time series of autumnal moth defoliation (26).

Accordingly, for differences in host-plant inducibility to explain the disparate dynamics of gypsy moth outbreaks in oak–hickory and oak–pine forests, induced hydrolyzable tannins in oaks must first of all affect baculovirus transmission in nature. We therefore carried out an experiment to test whether induced hydrolyzable tannins modulate baculovirus transmission under field conditions. Second, spatial variability in tree-species composition must explain the differences in outbreak dynamics between the two forest types. We therefore used a mathematical model to test whether the mechanism revealed by our field experiment produces alternating severe and mild outbreaks in simulated oak–hickory forests and consistently moderate outbreaks in oak–pine forests, as seen in the data for each forest type.

**Results**

A previous effort to induce hydrolyzable tannins using artificial defoliation was unsuccessful (27). We therefore induced hydrolyzable tannins by spraying foliage with the plant-signaling compound jasmonic acid (JA) (28), which had previously been used to induce hydrolyzable tannins in red-oak seedlings in the greenhouse (29). Hydrolyzable tannin levels in oak foliage in nature increase after budburst in defoliated trees and decline in undefoliated trees (17). We thus expected that induction effects would be manifest through statistically significant effects of week
Table 1. Effects of experimental JA spray in this study, and natural defoliation in a previous study (17), on percent hydrolyzable tannin concentration in red oak foliage

<table>
<thead>
<tr>
<th>Induction method</th>
<th>Treatment</th>
<th>Pretreatment concentration, %</th>
<th>Posttreatment concentration, %</th>
</tr>
</thead>
<tbody>
<tr>
<td>JA spray</td>
<td>JA</td>
<td>19.7 ± 0.82</td>
<td>27.8 ± 0.82</td>
</tr>
<tr>
<td></td>
<td>Control</td>
<td>19.5 ± 0.74</td>
<td>15.9 ± 0.84</td>
</tr>
<tr>
<td>Natural defoliation</td>
<td>Defoliated</td>
<td>23.30 ± 1.0</td>
<td>27.05 ± 1.4</td>
</tr>
<tr>
<td>Ref. 17</td>
<td>Control</td>
<td>23.36 ± 0.9</td>
<td>19.54 ± 0.9</td>
</tr>
</tbody>
</table>

Pretreatment concentrations were significantly different between studies (treatments, t = 2.70, df = 52, P = 0.0093; controls, t = 3.44, df = 73, P = 0.0011), as were posttreatment control concentrations (t = 3.40, df = 73, P = 0.0011), reflecting natural background variability in hydrolyzable tannins. Posttreatment concentrations were nevertheless effectively identical in JA-sprayed and naturally defoliated branches (t = 0.44, df = 52, P = 0.662), and controls in the two studies declined by similar magnitudes.

and through a treatment-by-week interaction. Statistical model parameters associated with these terms were indeed significantly different from zero (week, β = 1.18, t = 3.190, df = 70, P < 0.0001; treatment-by-week interaction, β = 3.75, t = 11.539, df = 70, P < 0.0001; Table 1; mixed-effects repeated-measures model with JA treatment and week as fixed effects, and with tree and branch nested within a tree as random effects, so that separate intercept and slope terms were fitted for each branch). For the same reason, we expected that the effect of induction alone would not be significant, and again this expectation was upheld (β = 0.95, t = 1.545, df = 55, P = 0.1281). Hydrolyzable tannin concentrations in foliage of experimental branches therefore increased relative to controls, reaching levels that were indistinguishable from levels induced by natural defoliation (17). A similar but slightly weaker effect was seen in samples the following year (SI Appendix, section 1.2 and Fig. S1). The JA spray in our experiments was thus highly effective at inducing realistic increases in hydrolyzable tannins.

We then quantified infection rates on induced and noninduced foliage, by first allowing infected larvae to die on both types of foliage and then allowing uninfected larvae to feed on the foliage. The resulting data show that the induction of hydrolyzable tannins had a strong effect on infection rates, but the effect varied across virus densities. At the lower virus density, average infection rates were lower on induced foliage, but at the higher virus density, they were higher on induced foliage (Fig. 3). To confirm that these differences were not simply due to chance effects, we used maximum likelihood and nonlinear optimization routines to fit a range of competing transmission models to the data, such that different models made different assumptions about the effects of induction on transmission. We then used the Akaike Information Criterion (AIC) to choose between the models (30). This model selection procedure provided very strong evidence that transmission was a nonlinear function of virus density on control foliage (Table 2), as in previous experiments (31), and that it was a linear function on induced foliage.

Previous work has shown that nonlinear virus transmission is due to high levels of variability in instantaneous infection risk between individuals (32). Because our AIC analysis demonstrates that induction produced linear virus transmission, it implies that induction strongly reduced variability in infection risk (Table 2). Reflecting the model-selection results, our estimate of variability in infection risk was low on induced foliage and high on noninduced foliage, and the respective confidence intervals did not overlap (SI Appendix, section 1.4 and Table S1, which also shows that there were detectable but weaker effects on average infection risk). Induction of hydrolyzable tannins thus sharply reduced variability in infection risk in the field.

Our field experiment confirmed that induced hydrolyzable tannins alter the transmission of the gypsy moth baculovirus in the field, but the spatial scale of our experiment was much smaller than the scale of naturally occurring outbreaks. Additional support for our hypothesis, however, comes from laboratory experiments and field observations. In simultaneous laboratory experiments, induced defenses affected both physiological susceptibility and risk of exposure (SI Appendix, section 1.5, Tables S2 and S3, and Fig. S3), the two main components of overall infection risk (31). These experiments provide mechanisms by which induced defenses affected transmission in our field experiment, suggesting that the results of that experiment were not simply due to some unknown experimental artifact.

The effects of induction in our experiments also help to resolve a contradiction between a previous laboratory experiment on induced defenses in gypsy moths and field observations of baculovirus epizootics. In a laboratory experiment by Hunter and Schultz (24), average susceptibility declined modestly as a result of induction, as it did in our experiments (note that there was no consideration of changes in variability in susceptibility in Hunter and Schultz’s experiment). The data from their experiment therefore imply that virus mortality should decline with increasing host density, but virus mortality in naturally occurring gypsy moth populations instead increases with increasing host density (14).

Table 2. AIC analysis of the results of the field transmission experiment

<table>
<thead>
<tr>
<th>Model</th>
<th>AICc</th>
<th>ΔAICc</th>
<th>AICc, WT</th>
</tr>
</thead>
<tbody>
<tr>
<td>No treatment effect, linear model</td>
<td>183.9</td>
<td>13.10</td>
<td>0.001</td>
</tr>
<tr>
<td>No treatment effect, nonlinear model</td>
<td>177.9</td>
<td>7.17</td>
<td>0.026</td>
</tr>
<tr>
<td>JA linear, control linear</td>
<td>186.0</td>
<td>15.22</td>
<td>0.001</td>
</tr>
<tr>
<td>JA linear, control nonlinear</td>
<td>170.8</td>
<td>0.00</td>
<td>0.940</td>
</tr>
<tr>
<td>JA nonlinear, control nonlinear</td>
<td>178.8</td>
<td>7.51</td>
<td>0.022</td>
</tr>
<tr>
<td>JA nonlinear, control nonlinear —P different, C different</td>
<td>179.9</td>
<td>9.13</td>
<td>0.001</td>
</tr>
</tbody>
</table>

The best model is in boldface. The model for which transmission was linear on induced branches and nonlinear on control branches provided an overwhelmingly stronger explanation for the data, such that the AIC difference for the next-best model was more than 7 (some more complex models are not included because they converged on the best model; SI Appendix). Moreover, the second-best model that allowed for a treatment effect assumed that the infection rate function was nonlinear on both induced and noninduced branches, again with lower infection risk on JA-treated branches. The probability that induction lowered variability in infection risk was thus greater than 0.96 (0.940 + 0.022 = 0.962).

Fig. 3. Effects of induction on baculovirus transmission. Symbols indicate data, and lines indicate the best fit versions of Eq. 5. Note that on branches without virus-infected cadavers, the fraction infected was very close to 0 (one infected insect out of 1,710), and so the model fit is based on three virus densities. Virus densities are jittered so that error bars can be easily distinguished.
Because we explicitly accounted for variability in infection risk, our results instead imply that virus mortality will increase with host density, for reasons that are explained by epidemiological theory. In our field experiment, the decline in variability in infection risk was much more dramatic than the decline in the average infection risk (SI Appendix, Table S1). Epidemiological theory has shown that declines in variability lead to higher cumulative infectious point in epidemics, because reduced variability means that there are fewer individuals with both a higher than average risk and a lower than average risk, but the reduction in the number of individuals with a lower than average risk has a disproportionate effect on the epidemic (33). The reduction in variability due to induction should therefore cause the cumulative infection rate to rise more rapidly with increasing host density. The results of our field experiment are thus consistent with both Hunter and Schultz’s experiment and with observations of virus mortality at large scales in nature.

Most importantly, the effects seen in our experiments predict both the occurrence of the subharmonic in gypsy moth outbreaks in oak–hickory forests and the disappearance of the subharmonic in oak–pine forests. To show this, we first extended a nonspatial natural-enemy model to allow for an induced defense. The resulting model shows that induction leads to cycles with a longer period and a larger amplitude, demonstrating that the increase in the severity of density-dependent virus mortality due to induction has a destabilizing effect (SI Appendix, section 2.2 and Fig. S4).

As in Bjornstad et al.’s (13) nonspatial model, our nonspatial model showed no subharmonic, and so we extended our model to include spatial variability in inductibility. We also included stochasticity in the insect’s reproductive rate, to reflect the effects of stochastic fluctuations in weather, which are believed to synchronize gypsy moth outbreaks (34). The output of the spatial model then matches the difference in dynamics in gypsy moth outbreaks between forest types. In model forests that are 43% inducible, time series of defoliation show alternating severe and mild outbreaks, as in oak–hickory forests, whereas in model forests that are only 15% inducible, time series of defoliation show only moderate outbreaks, as in oak–pine forests (Figs. 1 and 2). Reflecting these features of the time series, the corresponding model power spectra show peaks at periods that are close to the corresponding peaks in the data power spectra.

An important point is that because the model is stochastic, the model predictions necessarily vary between realizations or “runs.” We took this variability into account first of all by averaging model power spectra across realizations. Different time series, however, may give very similar power spectra (35), and so additional support for our argument is provided by the model’s ability to reproduce not just the data power spectra, but more specifically the alternation of mild and severe outbreaks in oak–hickory forests. Accordingly, it is important to also consider variability in this alternation, and so we examined a large number of additional realizations (SI Appendix, section 3.3 and Figs. S6–S10). In at least 50% of realizations of oak–hickory forests, the model shows at least two alternations of mild and severe outbreaks, meaning two each of severe and mild outbreaks in strict alternation, as seen in the data. Moreover, in almost every realization, there is at least one case in which a mild outbreak is followed by a severe outbreak, or vice versa. Meanwhile, in model oak–pine forests, alternation of severe and mild outbreaks never occurs (SI Appendix, Figs. S11–S15), and continuously increasing the frequency of inducible trees in the model leads to a gradual increase in the dominance of the longer period peak over the shorter period peak (Fig. 4). The model thus predicts that alternation of severe and mild outbreaks should occur frequently in oak–hickory forests, but that the alternation can be disrupted by stochasticity, whereas stochasticity never causes alternation to appear by chance in oak–pine forests (in SI Appendix, section 3.3 and Fig. S16, we discuss why the fit to the oak–hickory data varies across realizations).

Fig. 4. Effects of oak frequency on the power spectrum of the defoliation time series in the spatial model. The colors show the square root of the power of each period, such that dark red indicates the lowest power and white indicates the highest power. The figure thus shows that, at a low frequency of oaks, only short period cycles occur, whereas at a high frequency of oaks, only long period cycles occur. The relative importance of short-period and long-period cycles then gradually shifts as the frequency of oaks increases, so that both short-period and long-period cycles are represented in the power spectrum at intermediate frequencies of oaks.

The lack of clear alternation in oak–hickory forests since 1990 (12) suggests that stochasticity can indeed interrupt the pattern of alternation in such forests, but the introduction of the fungal pathogen Entomophaga maimai (in the late 1980s may also have played a role (6).

Initially, we assumed that net rates of gypsy moth reproduction were as high on pines, hickories, and other nonoak genera as on oaks, because defoliation during outbreaks is often equally severe on trees in all three genera (16). In nature, gypsy moth larval survival is often reduced on nonoaks (36), but in SI Appendix, section 2.3 and Fig. S18, we show that allowing for lower survival on nonoaks, in the form of a lower net reproductive rate, has only a minor effect on our model results. On the other hand, the subharmonic in oak–hickory forests disappears if we assume that differences between host trees affect only net reproduction and not induced defenses (SI Appendix, Fig. S19). Any model of a phenomenon as complex as defoliator population cycles will inevitably provide only a rough approximation of nature. The robustness of our model results nevertheless suggests that differences in gypsy moth cycles between forest types are best explained by the effects of induced defenses on pathogen transmission.

Discussion
As we have described, the hypothesis that induced defenses alter natural enemy attack rates is not new, but previous work relied largely on laboratory data that have been of little use in explaining infection rates in the field (24, 26). Our work is instead directly based on field data, beginning with our field experiment, and our experimental data are consistent with patterns of virus infection in naturally occurring epizootics. Crucial additional support is also provided by differences in defoliation levels between forest types. Meanwhile, the lack of evidence that induced defenses affect other insects may be a consequence of the lack of large-scale variation in tree-species composition within forests attacked by those insects (11, 26), rather than a true lack of effect of induced defenses. Given enough experimental data with which to estimate parameters, however, our models could in principle be used to detect effects of induced defenses in defoliation data even in the absence of variability in forest type.

An important feature of our work is that it combines models and experiments. The spatial and temporal scale of our experiments was nevertheless much smaller than the temporal and
Materials and Methods

Studies of baculovirus transmission have historically relied only on laboratory dose-response experiments, in which larvae are fed moderate doses of a virus solution, and larvae that do not consume the entire dose are discarded. Larvae in nature instead often consume very high doses, and they can sometimes detect and avoid infectious cadavers (38). Because of these differences, laboratory dose-response experiments often cannot be used to predict the effects of plant defenses on baculovirus infection rates in the field (23). We therefore instead carried out a field transmission experiment, in which larvae were allowed to feed freely on foliage contaminated with virus-infected cadavers in the field (we also carried out simultaneous dose-response and feeding experiments, see SI Appendix, section 1.5).

In our field experiment, we first induced hydrolyzable tannin concentrations in eight experimental red oaks, such that on each tree three branches were randomly assigned to a JA spray treatment, and three branches were assigned to the non-JA control spray (additional control branches on unsprayed trees were identical to control branches on sprayed trees, SI Appendix, section 1.1). To quantify changes in hydrolyzable tannin concentrations, we collected samples at budburst, and again 3 weeks later, 24 h before the initially uninfected larvae in the experiment began feeding on the branches (SI Appendix, section 1.1).

Next, we placed virus-infected larvae on each branch (treatments: 0, 10, 40 larvae per 0.2 m² of foliage), and we allowed the infected larvae to die and reproduce only once per year, and only larvae can become infected (23). Transmission thus ends before reproduction begins. Accordingly, to allow for multiple generations, we first use the epizootic model, Eqs. 1–4, to describe the within-generation baculovirus epizootic, and then we account for reproduction and other sources of mortality (6). The model is then:

\[
N_{n+1} = \beta e^{-\lambda e N_{n}}(1 - i(N_{n}, Z_{n}, D_{n})) \times \left(1 - \frac{2abN_{n}}{b^{2}N_{n}}\right) + \sigma Z_{n}.
\]

Here \( T \) is the length of the experiment and \( P_{0} \) is the virus density. We then fit Eq. S to our data using maximum likelihood, and we used AIC to choose the version of the model that explained the data most parsimoniously, such that each version made different assumptions about the effects of induction on transmission (SI Appendix, section 1.4).

To explain our model of gypsy moth outbreaks, we note first that, like most outbreaking forest insects (42), gypsy moths have discrete generations and reproduce only once per year, and only larvae can become infected (23). Therefore, our data using maximum likelihood, and we used AIC to choose the version of the model that explained the data most parsimoniously, such that each version made different assumptions about the effects of induction on transmission (SI Appendix, section 1.4).

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N_{n+1} = \beta e^{-\lambda e N_{n}}(1 - i(N_{n}, Z_{n}, D_{n})) \times \left(1 - \frac{2abN_{n}}{b^{2}N_{n}}\right) + \sigma Z_{n}.
\]
Host density $N_{q+1}$, cadaver density $Z_{q+1}$, and tannin content $D_{q+1}$ at location $q$ in generation $n+1$ are thus dependent upon the post-

\[ Z_{q,n+1} = \alpha N_{q,n} \left( N_{q,n} - Z_{q,n} - D_{q,n} \right) + G_{q,n} \]  

\[ D_{q,n+1} = \alpha N_{q,n} \frac{D_{q,n}}{\beta + D_{q,n}} \]  

ACKNOWLEDGMENTS. J. M. Bergelson, A. F. Hunter, and W. F. Morris made many helpful comments on the manuscript. Discussions between G. Dwyer and K. Haynes began as part of the Forest Insects Working Group at the National Institute for Mathematical and Biological Synthesis. Our work was supported by National Institutes of Health Grant R01GM096655.
Supporting Information for “Induced Plant Defenses, Host-Pathogen Interactions, and Forest Insect Outbreaks”

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August 12, 2013
1 Details of Experimental Methods, and Additional Experimental Results

1.1 JA Application and Chemical Analyses

We randomly selected eight experimental and eight control red oaks, each a mature tree, in an oak-hickory forest in southwest Michigan, at the Lux Arbor site of the Kellogg Biological Station (42.4°N, 85.4°W). On each of the 8 experimental trees, we randomly assigned 3 branches to the JA spray, and 3 to the non-JA control spray. We sprayed branches for three weeks, weather permitting, beginning shortly after bud burst. Experimental branches were treated with 5 mM jasmonic acid (JA) initially solubilized in a solution of 10% EtOH (aq., v/v) that also included 0.125% (v/v) Triton-X detergent to permit JA to penetrate leaf cuticles. Control branches were treated with a solution that lacked JA, but that was otherwise identical. In addition to control branches on experimental trees, control branches included three branches on each of 8 control trees on which there were no branches sprayed with JA. It turned out, however, that hydrolyzable tannin concentrations in control branches on these latter control trees were indistinguishable from concentrations in control branches on trees on which there were JA-sprayed branches (mixed-effects repeated-measures analysis on hydrolyzable tannin concentrations in control branches, with trees, and branches nested within trees, as random effects: $F_{1,46} = 0.81, p = 0.3728$). Control branches on JA-sprayed and non-JA-sprayed trees were therefore effectively identical. To quantify changes in hydrolyzable tannin concentrations, we collected samples at budburst, and again 3 weeks later, 24h before the initially uninfected larvae in the experiment began feeding on the branches.

We flash-froze our single-leaf samples in liquid nitrogen, and we kept them on dry ice for roughly two hours until storage at −80°C. Each sample was then lyophilized, and ground in a mortar and pestle under liquid nitrogen. We next placed 25 mg of each lyophilized, ground sample in a 2 ml microcentrifuge tube, and we extracted it exhaustively by sonication for 2 hours with a total of 2 ml 70% (v/v) acetone at 4°C. We then removed the acetone using rotary evaporation, and we used the aqueous extract in phenolic assays, as follows. We quantified hydrolyzable tannins using the potassium iodate method (Bate-Smith 1975; Schultz and Baldwin 1982), and condensed tannins (proanthocyanidins) using the n-butanol/HCL method (Bate-Smith 1975). Concentrations for each
class of compound were calculated based on standard curves generated with oak tannins purified by a modification of the method of Hagerman and Butler (1980).

1.2 Results from Sample Collections in the Year Following JA Spray

Previous work showed that defoliation of red oak in one year can lead to hydrolyzable tannin induction in the following year (Hunter and Schultz 1993). We therefore collected a final set of samples from each branch in the season following our experiments, again at budburst. In the year following

![Figure S1: Percent hydrolyzable tannin concentrations in experimental branches. Year 1 is the year in which branches were sprayed, while Year 2 is the following year. Error bars indicate 1 standard error of the mean.](image)

JA application, the average difference between JA-sprayed branches and non-JA-sprayed branches was lower (first year: $8\% \pm 1.16$; second year: $3\% \pm 0.90$), but it was again significant (fig. S1, $F_{1,20} = 5.54, p = 0.0289$). The effect of JA spray was thus carried over from one year to the next.
1.3 Insect Rearing Protocols and Variability in Infection Risk

Key features of our protocol reduced extraneous sources of variability, and previous experiments have shown that the protocol therefore allows for surprisingly high statistical power (Dwyer 1991; Fuller et al. 2012). Following this protocol, we used larvae from a USDA colony of low variability, and a plaque-purified virus strain known as “G2” (Dwyer et al. 2005). Also, to match the timing of key transmission rounds in nature (Woods and Elkinton 1987), we used uninfected larvae that were in the third larval stage or “instar”. Finally and most importantly, because susceptibility to the baculovirus changes over a period of hours after molting (Grove and Hoover 2007), we ensured that the uninfected larvae were developmentally synchronized, using the following procedure.

Because we wanted to use third instars, from the larval rearing cups we collected second instars whose head capsules had slipped forward, which indicates that molting to the next instar will occur within 24 hours. As we collected these larvae, we held them at 4°C in a cold room until we had collected enough of them to carry out the experiment, which took roughly 48 hours. We then removed the larvae from the cold room, and held them in cups without diet at 25°C for another 48h, by which time essentially all of them had molted to the third instar. In practice, almost all molting occurred within the first 24h at 25°C, ensuring a high degree of developmental synchrony. Because larvae stop feeding once their head capsules slip forward, using recently molted larvae had the additional advantage that it minimized the effects of previously consumed food items, a crucial issue in studies of the effects of diet on insect infection risk (Keating et al. 1989).

We produced infected larvae by feeding virus-contaminated diet to neonates, using a dose high enough to ensure more than 95% infection. To be certain that the infected larvae were indeed infected, we held them at 26°C for five days, a period long enough to ensure that any uninfected larvae would molt to the second instar, which allowed us to identify and discard the uninfected larvae. We then placed the infected larvae on branches in the field five days before adding uninfected larvae, which is long enough to ensure that the infected larvae would distribute themselves naturally over the branches before dying. Densities of healthy and infected larvae were chosen to match densities in nature (Woods and Elkinton 1987). All larvae were enclosed in mesh bags, which do not affect foliage chemistry or larval behavior, but prevent the loss of insects to dispersal, and the loss of virus to UV degradation, while allowing for natural variability in weather conditions (Fuller
et al. 2012).

The initially uninfected larvae began feeding after leaves were fully expanded, which is when leaf chemistry ceases to change, so defoliation during the field experiment had no effect on hydrolyzable-tannin levels (D’Amico et al. 1998). Meanwhile, 25 uninfected larvae is a large enough number to provide reasonable statistical power, but it prevents starvation because 25 larvae per 40 leaves leads to defoliation levels of less than 50%. We further note that the density of initially uninfected larvae during a field transmission experiment has little to no effect on infection rates or induced defenses (D’Amico et al. 1998).

As in previous experiments (Dwyer et al. 2005), our protocol did not eliminate the effects of variability in infection risk, raising the question, what is the source of the variability? The answer is almost certainly not stochastic fluctuations in infection risk within individuals over time, because such variability is equivalent to demographic stochasticity, and by using hundreds of insects, we greatly reduced the effects of demographic stochasticity. Instead the source appears to be heritable variation at loci that affect infection risk. In previous work, we showed that infection risk in gypsy moth larvae does indeed have a heritable component (Elderd et al. 2008), and because the larvae in the current study were all raised under the same conditions, the variability that we observed here was also probably heritable. In gypsy moths, heritable variation in infection risk is likely maintained by fluctuating natural selection, in combination with a fecundity cost of resistance (Elderd et al. 2008). We did not include selection in our models, however, because we wanted to explain the defoliation data with as simple a model as possible.

1.4 Analysis of Transmission Data

Because virus density was constant in our field experiment, and because the experiment was short enough that none of the initially uninfected larvae became infected and died during transmission, infection was the only process that occurred. This in turn meant that we could analyze our data using what is known in the stochastic processes literature as a “pure-death” model, which allows for the effects of small population size, so-called “demographic stochasticity” (Renshaw 1993). Because we used a total of 200 host insects in each experimental treatment (8 replicates × 25 larvae per replicate), the effects of demographic stochasticity were likely minimal, but it was nevertheless
important to allow for such effects (Dwyer et al. 2005; Elderd et al. 2008). The pure-death process predicts that the number of survivors per unit time follows a binomial distribution with a probability of survival that is a function of time and a rate parameter that can be fit to the data (Renshaw 1993). Also, because individuals vary in their risk of infection, we assume that the pure-death parameter, which is essentially the disease transmission rate, is drawn from a gamma distribution, with mean transmission rate $\bar{\nu}$ and coefficient of variation $C$.

Standard practice in analyzing mortality data is therefore to use a binomial distribution as a likelihood function McCullagh and Nelder (1989), and we thus used a binomial likelihood function in our analyses. Given the likelihood function, the $AIC_c$ is calculated according to:

$$AIC_c = -2L + 2K \left( \frac{n}{n - K - 1} \right).$$

(S1)

Here $L$ is the likelihood of a given model, $K$ is the number of parameters in the model, $n$ is the number of replicates, and the term $n/(n - K - 1)$ is the correction for sample size. The $AIC_c$ thus chooses the best-fit model based on a tradeoff between model complexity, in terms of the number of parameters, and goodness of fit, as measured by the likelihood, such that the best model has the smallest $AIC_c$. In practice, we use $\Delta AIC_c$ scores to measure the difference in $AIC_c$ values between the best-fit model and all other models under consideration. The best-fit model thus has a $\Delta AIC_c$ score of zero. Meanwhile, to calculate the probability that each model is actually the best model, we used $AIC_c$ weights, which are calculated as:

$$w_i = \frac{\exp \left( -\frac{1}{2} \Delta_i \right)}{\sum_{r=1}^{R} \exp \left( -\frac{1}{2} \Delta_r \right)}.$$

(S2)

Here $w_i$ is the weight for the $i$th model, while $\Delta_i$ is the $AIC_c$ difference for the $i$th model. Relative to traditional significance tests, an advantage of the $AIC_c$ is that it does not assume that any model being fit to the data is absolutely correct (Burnham and Anderson 2002). This is a crucial advantage, because our goal was to identify the model that best describes our data, but transmission is so complicated that it is unlikely that any model could capture every mechanism that might affect transmission.

Preliminary analyses showed that the best models did not distinguish between non-JA-treated branches on non-JA-treated trees and non-JA-treated branches on JA-treated trees, and so we only show analyses in which the two types of branches were treated identically. Moreover, because the
variance-inflation factor was very close to the ideal value of 1, our choice of a binomial distribution was clearly appropriate (Burnham and Anderson 2002). The best model thus explained essentially all of the variability in our data that was not simply due to low levels of demographic stochasticity.

An important point is that allowing variability $C$ to approach 0 in a nonlinear transmission model yields a linear transmission model, and so the linear models were nested within the nonlinear models. This in turn meant that, when we fit a nonlinear transmission model to our data, it was possible for the best-fitting value of the variability parameter to be indistinguishable from 0, in which case the conclusion would be that the best-fitting model is instead linear. This happened for the models for which average transmission was the same for induced and control branches but variability was different, and for which both average transmission and variability were different for induced and control branches. In both of these cases, however, the best-fitting model assumed that transmission was linear only on induced branches, and not on control branches, strengthening our basic conclusion. Because the best-fitting versions of these two models reduced to simpler models, we did not include them in the AIC table in the main text.

It is also important to emphasize that when we fit a linear model to the data, the fitting routine cannot estimate the variability parameter, and so in such cases it would not be possible for the routine to conclude that the best model is nonlinear. This is relevant because when we fit a model for which transmission was nonlinear for induced branches and linear for control branches, the fitting routine instead concluded that transmission was linear for both treatments. In the AIC table in the main text, we therefore did not include the model that assumed transmission was nonlinear on induced branches and linear on control branches, because it was effectively the same as the model for which transmission was assumed, from the beginning, to be linear for both treatments. As the table shows, however, the model for which transmission was linear for both treatments provided a very poor explanation for the data, again strengthening our basic conclusion.

An important part of our argument is that induction strongly affects variability $C$ in particular. This conclusion is supported by our parameter estimates for the best model, which allows for differences in both average infection risk $\bar{\nu}$ and variability in infection risk $C$ between induced and control branches. As Table S1 shows, the value of $\bar{\nu}$ on induced branches was moderately lower, reflecting slightly lower average infection risk on induced branches, as in a previous laboratory experiment (Hunter and Schultz 1993), but the value of $C$ was much lower, reflecting much lower variability in
Table S1: Effects of induction on parameters describing infection risk (the protocol for the laboratory experiment is given in a later sub-section). For the laboratory experiment, the mean is the log_{10} of the 50% lethal dose, or “LD_{50}”, the number of occlusion bodies required to cause 50% mortality, which is an inverse measure of infection risk. A lower LD_{50} is thus equivalent to a higher average infection risk. For the field experiment, the mean is the average transmission rate \( \bar{\nu} \), the probability of infection per unit time and per infectious cadaver per square meter, while the C.V. is the parameter \( C \), the variability in infection risk.

Both effects were statistically significant, in that the corresponding 95% confidence intervals for both \( \bar{\nu} \) and \( C \) for induced branches did not overlap with the 95% confidence intervals for non-induced branches. Also, although Table S1 shows that the best estimate of \( C \) on induced foliage was 0, this does not mean that variability in infection was truly 0 on induced foliage. Instead it was simply too low for its effects to be detectable in our experiment.

To see the consequences of these parameter estimates for baculovirus epizootics, we inserted our best-fit values of transmission \( \bar{\nu} \) and variability \( C \) for induced and non-induced foliage into the epizootic model equations in the main text. Fig. S2 then shows that the lower value of transmission \( \bar{\nu} \) on induced foliage modestly raises the lowest density at which virus epizootics occur, but the lower value of variability in transmission \( C \) causes the infection rate to increase much more sharply with host density.

As we mentioned in the main text, this figure resolves an important conundrum associated with Hunter and Schultz (1993)’s previous laboratory experiment, which detected only a reduction in average risk. Induction in nature occurs in response to severe defoliation, which in turn becomes more likely as host densities increase. If induction acted only to reduce average infection risk, then infection rates would fall with increasing host density (Hunter and Schultz 1993), following so-

<table>
<thead>
<tr>
<th>Experiment</th>
<th>Treatment</th>
<th>Mean (95% C.I.)</th>
<th>C.V. (95% C.I.)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Field Control</td>
<td>0.21/day/m² (0.170, 0.276)</td>
<td>0.96 (0.741, 1.177)</td>
<td></td>
</tr>
<tr>
<td>Field Induced</td>
<td>0.12/day/m² (0.111, 0.136)</td>
<td>0 (0, 0.458)</td>
<td></td>
</tr>
<tr>
<td>Laboratory Control</td>
<td>3.19 (2.725, 3.654)</td>
<td>0.17 (0.076, 0.264)</td>
<td></td>
</tr>
<tr>
<td>Laboratory Induced</td>
<td>3.90 (3.773, 4.027)</td>
<td>0.09 (0.013, 0.167)</td>
<td></td>
</tr>
</tbody>
</table>
Figure S2: Effects of induction on epizootic intensity. The vertical axis is the cumulative fraction infected in a single epizootic, as calculated using the epizootic model in the main text. The “Induced” line is based on values of average transmission $\bar{\nu}$ and variability in transmission $C$ estimated from induced foliage, while the “Non-induced” line is based on values estimated from non-induced foliage.
called “negative” density-dependence, but baculovirus infection rates in nature almost invariably rise with increasing host density, following “positive” density-dependence (Moreau et al. 2005; Woods and Elkinton 1987). Meanwhile, fig. S2 shows that a reduction in variability in infection risk causes the infection rate to rise more steeply with host density, which is a well-known result from epidemic theory (Anderson and May 1991). Theory therefore shows that a reduction in variability in infection risk due to induction should cause infection rates to rise very sharply with density, resolving the contradiction between the original experiment and data from naturally occurring virus epizootics in the field.

1.5 Feeding Trials and Dose-Response Bioassays

Once an insect has consumed some virus particles, infection risk depends only on the ability of the insect’s immune response to fight off the infection, but feeding behavior can alter the risk that the virus is consumed in the first place. In dose-response experiments, however, insects that do not consume the entire dose are discarded, and so infection rates depend only on the insect immune response, or on physiological factors that affect the immune response (Watanabe 1987). There are thus no effects of feeding behavior, even though feeding behavior is important in nature (Dwyer et al. 2005). Field transmission experiments thus have an advantage over laboratory dose-response experiments not just because field experiments allow for more natural conditions, but also because they allow for effects of both host behavior and host immune responses on transmission.

Dose-response experiments are nevertheless useful because they allow us to measure the effects of induction of induced defenses on immunological or physiological susceptibility independently of any effects on feeding behavior. Meanwhile, by directly measuring foliage consumption rates, we can measure the effects of induction on feeding behavior in the absence of any effects on immunological susceptibility. Previous work in the fourth author’s lab has therefore shown that such a combination of field transmission experiments, laboratory dose-response experiments, and laboratory feeding-rate experiments can be used to understand how immunological susceptibility and host behavior combine to determine infection risk in the field (Dwyer et al. 2005).

To explain our dose-response and feeding experiments, it is useful to think of the overall risk of infection in terms of simple probability (Dwyer et al. 2005), with $P(I, C)$ as the probability of
consuming the virus and becoming infected, \( P(I|C) \) as the probability of becoming infected given that the virus has been consumed, and \( P(C) \) as the probability of consuming the virus. Following basic probability (Ross 1984), we can then write,

\[
P(I, C) = P(I|C)P(C).
\]

We interpret this equation to mean that the overall probability of infection \( P(I, C) \) depends on both immunological susceptibility, as measured by \( P(I|C) \), and exposure risk, as measured by \( P(C) \).

The goal of our dose-response and feeding experiments was therefore to disentangle the effects of induction on the probability of consumption from the effects of induction on the probability of infection given consumption. Ideally, we would have liked to measure both indices in the field, but such experiments have proved to be impractical. A laboratory dose-response experiment therefore provided a measure of the probability of infection given virus consumption \( P(I|C) \), while a laboratory feeding experiment provided a measure of an important determinant of the probability of consumption \( P(C) \).

To ensure that the composition of foliage in our dose-response and feeding experiments was as close as possible to the composition of the foliage in the field, the two laboratory experiments were carried out within 24 hours of when uninfected larvae first began feeding in our field transmission experiment, and in both experiments we used leaves from the same branches as in the transmission experiment. To further ensure the validity of comparisons between the field and laboratory experiments, we used the same strain of the insects and the same strain of the virus in all experiments.

In the feeding experiment, we fed single leaves to each of 116 larvae in individual cups, such that 56 larvae were fed leaves from experimental branches and 60 were fed leaves from control branches. Before the experiment began, we measured the area of the leaves using a leaf-area meter, and we re-measured the leaves after the larvae had fed on them for 24 hours. The reduction in area after 24 hours is thus a measure of feeding rate. As fig. S3 shows, feeding rates were substantially higher on induced foliage than on non-induced foliage. We analyzed these data using a mixed-effects generalized linear model (Pinheiro and Bates 2004), as in the hydrolyzable-tannin analysis, such that branches were treated as random effects, nested within a tree, and the JA treatment was the fixed effect. This analysis revealed a significant effect of JA treatment on amount of foliage consumed \( (F_{1,118} = 11.48, p = 0.001) \). We thus conclude that induction of hydrolyzable tannins
Figure S3: Effects of induction on gypsy moth feeding rate over 24 hours. Error bars indicate 1 standard error of the mean.
leads to increases in feeding rates, in turn altering infection risk. It therefore seems likely that the effects of induction on overall transmission were partly due to changes in feeding behavior.

In the dose-response experiment, we followed standard protocols by feeding larvae 3 µl of a solution of virus in dH₂O on leaf disks placed on top of agar squares in tightly sealed plastic cups in the laboratory (Dwyer et al. 2005; Hunter and Schultz 1993). Larvae that did not consume the entire leaf disk, and thus the entire dose of virus, were discarded. Control larvae were fed leaf disks with dH₂O alone. The data show that induction led to a lower infection rate at the lower dose, but infection rates were equal at the higher dose (Table S2).

<table>
<thead>
<tr>
<th>Treatment</th>
<th>Dose (occlusion bodies)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>0</td>
</tr>
<tr>
<td>Induced</td>
<td>0.00 ± 0.000</td>
</tr>
<tr>
<td>Non-Induced</td>
<td>0.00 ± 0.000</td>
</tr>
</tbody>
</table>

Table S2: Results of the dose-response bioassay, comparing the effects of induced and non-induced foliage on the probability of becoming infected given consumption of a particular dose of virus. Each value is the mean fraction infected, calculated across 6 replicates, plus or minus 1 standard error of the mean. Within a replicate, leaf disks were all from the same tree. Each replicate included 11-14 larvae. Out of 250 individuals in virus-control treatments, all of which were fed leaf disks in combination with dH₂O, none became infected.

The standard method of analyzing this type of data is to use generalized linear modeling to fit a logistic distribution to the data, using the so-called “logit transform” (Collett 2003). Under this transform, our model for the dose-response data is,

\[
\log \left( \frac{1 - p_i}{p_i} \right) = \beta_0 + \beta_1 \log_{10} D_i. \tag{S4}
\]

Here, \( p_i \) is the fraction infected at dose \( D_i \), and \( \beta_0 \) and \( \beta_1 \) are parameters that are fit to the data. We fit versions of this model to the data under different assumptions about the effects of induction on the model parameters \( \beta_0 \) and \( \beta_1 \). If there were effects of induction, then in the best model either \( \beta_0 \) or \( \beta_1 \) or both would differ between the experimental and control treatments. Our statistical approach was therefore to calculate a likelihood score for each version of the model, which we then used
Table S3: AIC analysis of bioassay data. The best model is in bold-face.

<table>
<thead>
<tr>
<th>Model</th>
<th>AIC&lt;sub&gt;c&lt;/sub&gt;</th>
<th>Δ AIC&lt;sub&gt;c&lt;/sub&gt;</th>
<th>AIC&lt;sub&gt;c&lt;/sub&gt; weights</th>
</tr>
</thead>
<tbody>
<tr>
<td>No effect of dose or induction</td>
<td>283.47</td>
<td>27.92</td>
<td>0.000</td>
</tr>
<tr>
<td>Induction affects only β&lt;sub&gt;0&lt;/sub&gt;</td>
<td>257.46</td>
<td>1.90</td>
<td>0.242</td>
</tr>
<tr>
<td>Induction affects only β&lt;sub&gt;1&lt;/sub&gt;</td>
<td>258.68</td>
<td>3.12</td>
<td>0.132</td>
</tr>
<tr>
<td><strong>Induction affects both β&lt;sub&gt;0&lt;/sub&gt; and β&lt;sub&gt;1&lt;/sub&gt;</strong></td>
<td>255.56</td>
<td><strong>0.00</strong></td>
<td><strong>0.626</strong></td>
</tr>
</tbody>
</table>

To calculate the AIC<sub>c</sub>. To do this, we used a generalized, linear, mixed-model fitting routine for binomial data (Collett 2003) assuming quasi-binomial error, with tree as a random effect.

As Table S3 shows, the best model assumes that induction affects both β<sub>0</sub> and β<sub>1</sub>. Moreover, there was essentially zero probability that the best model was the model that assumed that there was no effect of induction. The dose-response experiment thus showed that part of the reason why induction affected infection risk in the field was because it affected immunological susceptibility.

We note, however, that the data do not clearly distinguish between the model that assumes that induction affects both model parameters, and the models that assume that it affects either β<sub>0</sub> alone or β<sub>1</sub> alone. For our purposes, however, the important point is that there was an effect of induction on immunological susceptibility.

Because we are interested in the effects of induction on variability in infection risk, we also went beyond standard logit analyses to consider what our data tell us about the underlying distribution of susceptibility. In logit analyses, the probability of infection <i>p</i><sub>i</sub> is described by the cumulative distribution function of a logistic distribution. Like most probability distributions, the logistic distribution has both a mean and a C.V., such that the mean is the LD<sub>50</sub>, the dose that causes 50% mortality. In terms of the parameters β<sub>0</sub> and β<sub>1</sub> of the logit equation (S4), the LD<sub>50</sub> can be expressed as (Evans et al. 1993):

\[
\text{LD}_{50} = -\frac{\beta_0}{\beta_1},
\]  

(S5)

while the C.V. can be expressed as:

\[
\text{CV} = \frac{1}{\beta_0} \frac{\pi}{3^{1/2}},
\]  

(S6)

Given estimates of β<sub>0</sub> and β<sub>1</sub>, we used the above expressions to calculate the point estimates of the LD<sub>50</sub> and the C.V. in Table S1. To calculate 95% confidence intervals, we first used the delta method.
to calculate standard errors, and then we multiplied the standard errors by 1.96 (Faraway 2006). As the table shows, both average infection risk, as measured inversely by the LD$_{50}$, and variability in infection risk were lower on induced foliage than on non-induced foliage, matching the field transmission experiment. These differences, however, were only significant for average infection risk, and the difference in C.V.’s was much smaller than in the field transmission experiment. Because field transmission experiments allow for transmission under more natural conditions, however, we do not expect exact agreement between the dose-response experiment and the field-transmission experiment, and in general we place more weight on the results of the field experiment. Our overall conclusion is thus simply that induction affected infection rates in the dose-response experiment, in ways that appear similar to how induction affected infection rates in the field transmission experiment.

2 Details of the Models

2.1 Type III Predation and Long-term Virus Survival

The Type III functional response in our multi-generation models assumes that the generalist predator switches to gypsy moths as densities increase, but that eventually densities are too high for the generalist to maintain the population at the low-density equilibrium. This assumption is useful because it allows the model to easily mimic the multiple generations of low host densities typically seen in inter-outbreak populations (Dwyer et al. 2004; Elkinton et al. 1996). Moreover, in predator-exclusion experiments, gypsy moth populations show sharp rises in population growth (Jones et al. 1998), suggesting that generalist predators can maintain gypsy moth populations at a low, stable equilibrium, as predicted by Type III models (Dwyer et al. 2004). The term $2abN_n/(b^2 + N_n^2)$ in the model is then the fraction of gypsy moth larvae that are killed by the generalist predators, so that $a$ is the maximum fraction of hosts killed, and $b$ is the density at which this fraction is maximized.

Another important feature of the outbreak model is that it allows for long-term virus survival through both contamination of egg masses and environmental reservoirs. The importance of these mechanisms has been confirmed by extensive field experiments using the gypsy moth and its baculovirus (Murray and Elkinton 1989; Podgwaite et al. 1979). For baculoviruses of other insects,
however, similar field experiments have not been carried out, and apparently as a consequence the
literature on baculoviruses often invokes virus survival through covert infections, which are as-
sumed to be activated by stress at some time after infection, due to unidentified stressors (Il’inykh
and Ul’yanova 2005). Although covert infections have in fact been detected in some species of
insects (Burden et al. 2003), whether or not such infections play a role in transmission in the field
is debatable (see Fuller et al. (2012) for a brief review). Moreover, for gypsy moths in particular,
surface-disinfection of egg masses, which we used in all of our experiments, is extremely effective at
eliminating vertical transmission (Doane 1969), as it is in many other insects (Kukan 1999). For this
reason, reports of vertical transmission through the activation of covert infections can often be more
parsimoniously explained by a lack of surface-disinfection (Myers et al. 2000). It therefore seems
likely that covert infections play only a small role in virus survival in the gypsy moth, and possibly
in many other insects as well. In our model, we thus included only environmental reservoirs and
surface contamination of egg masses as mechanisms of virus survival between host generations.

It is nevertheless worth pointing out that in fact our model may be adequate to describe the effects
of covert infections as well. The crucial assumption is simply that some fraction of infections from
previous generations leads to new infections in the current generation. As long as this fraction does
not vary with insect density, and as along as such infections are either rare or occur mostly at the
beginning of the larval season, our model may provide an accurate description of the effects of covert
infections.

2.2 Re-scaling and Estimating Model Parameters

As we mentioned in the main text, we used the temporal model to develop a preliminary understand-
ing of the effects of induction on insect outbreaks, and to generate preliminary estimates of some
parameters. Before fitting model parameters to data, however, it was important to identify parama-
ters that have identical effects on the model’s predictions, as the values of such parameters will be
perfectly correlated and thus statistically non-identifiable. We therefore first re-scaled the model.
Because the infection rate depends on the epidemic model in the main text, we re-scaled both the
year-to-year state variables \( N_n, Z_n, \) and \( D_n \), and the epidemic state variables \( S(t), E_i(t), \) and \( I(t), \)
as follows:

\[ \hat{S}(t) = \nu S(t), \quad (S7) \]
\[ \hat{E}_i(t) = \nu E_i(t), \quad (S8) \]
\[ \hat{P}(t) = \nu P(t), \quad (S9) \]
\[ \hat{N}_n = \nu \hat{N}_n, \quad (S10) \]
\[ \hat{Z}_n = \nu \rho \hat{Z}_n, \quad (S11) \]
\[ \hat{D}_n = \psi \hat{D}_n. \quad (S12) \]

Allowing for this re-scaling, the epizootic model becomes:

\[
\frac{d\hat{S}}{dt} = -\hat{S}\hat{P} \left[ \frac{\hat{S}(t)}{S(0)} \right] C^2_n, \quad (S13)
\]
\[
\frac{d\hat{E}_1}{dt} = \hat{S}\hat{P} \left[ \frac{\hat{S}(t)}{S(0)} \right] C^2_n - m\delta\hat{E}_1, \quad (S14)
\]
\[
\frac{d\hat{E}_i}{dt} = m\delta\hat{E}_{i-1} - m\delta\hat{E}_i, \quad i = 2, \ldots m, \quad (S15)
\]
\[
\frac{d\hat{P}}{dt} = m\delta\hat{E}_m - \mu\hat{P}. \quad (S16)
\]

Note that variability varies from generation to generation because of changes in induced defenses, such that \( C_n = C_0 \exp \left( - (\hat{D}_n + \hat{D}_0) \right) \). The multi-generation model is then:

\[
\hat{N}_{n+1} = \lambda \epsilon_n \hat{N}_n \left( 1 - \hat{N}_n \hat{Z}_n \hat{D}_n \right) \left( 1 - \frac{2ab\hat{N}_n}{b^2 + \hat{N}_n^2} \right), \quad (S17)
\]
\[
\hat{Z}_{n+1} = \phi \hat{N}_n \hat{Z}_n \hat{D}_n + \gamma \hat{Z}_n, \quad (S18)
\]
\[
\hat{D}_{n+1} = \hat{\alpha} \hat{N}_n \frac{\hat{D}_n}{\beta + \hat{D}_n}. \quad (S19)
\]

The model dynamics thus depend on ten parameters, namely \( \lambda, C_0, a, \gamma, \mu \) and the compound parameters \( \phi \equiv \Theta \rho, \hat{\alpha} \equiv \frac{\psi \hat{\beta}}{\beta}, \hat{\beta} \equiv \frac{\psi}{\psi}, \hat{b} \equiv \frac{b}{\beta}, \) and \( \hat{D}_0 \equiv D_0 \psi \). Note that we could also eliminate the baseline variability \( C_0 \), by setting \( \hat{D}_0 \equiv \psi D_0 + \log C_0 \), but as we will explain, this re-scaling would make it difficult to fit the model parameters to data on tannin concentrations.

Previous work provides estimates of the reproductive rate \( \lambda = 74.6 \), the maximum predation rate \( a = 0.967 \), the scaled predation parameter \( \hat{b} = 0.14 \) (Dwyer et al. 2004) and the virus decay rate
\( \mu = 0.39 \text{ /day} \) (Fuller et al. 2012). The parameters \( C_0, \hat{\alpha}, \hat{\beta} \) and \( \hat{D}_0 \) in contrast are related to the dynamics of hydrolyzable tannins, and consequently have not been previously measured, while the pathogen over-wintering parameters \( \phi \) and \( \gamma \) are not well known. To produce preliminary estimates of these six parameters, we therefore followed Kendall et al. (1999) in fitting the period and the amplitude of the model cycles to the corresponding values from a combination of our experimental data and observations of gypsy moth populations in nature. This required that we run the model repeatedly to find sets of parameters for which the model output matched the data.

To understand this fitting procedure, note that hydrolyzable tannins can only be measured as a fraction of a sample’s dry weight, because the total dry weight of leaf material in the forest is unknown. In fitting the model to the data, we therefore calculated the amplitude of change in the log of the hydrolyzable tannin concentration \( \log_{10}(\hat{D}_n + \hat{D}_0) \), so that total dry weight fell out of the calculation as a scaling constant (note that this would not have been true if we had scaled away the baseline variability \( C_0 \)). In our data, the difference in \( \log_{10} \) of hydrolyzable tannin concentrations before and after induction was \( 0.187 \pm 0.028 \). Meanwhile, gypsy moth populations cycle with a period of 5-10 years, and with an amplitude of roughly 3-5 orders of magnitude (Williams et al. 1991). We thus searched for values of \( \hat{\alpha}, \hat{\beta}, \hat{D}_0, \phi, \) and \( \gamma \) for which the amplitude of fluctuations in the host population was at least 3 orders of magnitude, the amplitude of fluctuations in the induced defense was within about 0.06 of 0.187, and the cycle period was at least 5 years. We then ran the model for 200 generations, discarding the first 50 generations to avoid transients, and we calculated the average amplitude and the period for the remaining 150 generations. To reduce the computational burden, we temporarily eliminated stochasticity by setting the standard deviation of the random variate \( \sigma = 0 \).

Fig. S4 shows model output for a set of parameter values that met our fitting criteria. As the figure shows, the host population rises to peaks that are terminated by the pathogen, which has a delayed rise that drives the cycles. The induced defense, however, also contributes to the rise in the pathogen population, because of its effect on variability in infection risk. That is, the rise of the defense, in response to the rise of the host, strengthens the delayed density-dependence of the pathogen, allowing cycles to occur. To demonstrate the importance of the induced defense, in fig. S5 we show model output for a lower value of \( \hat{\alpha} \), the scaled rate of increase of the induced defense. As the figure shows, this lower rate of induction leads to damped cycles, eliminating outbreaks altogether.
Figure S4: Dynamics of the temporal model. Parameter values are: scaled induction response $\hat{\alpha} = 2.5$, scaled induction half-saturation constant $\hat{\beta} = 100$, baseline heterogeneity $C_0 = 0.04$, scaled constitutive defense level $\hat{D}_0 = 3$, long-term virus over-wintering $\gamma = 0.2$, reproductive rate $\lambda = 74.6$, maximum predation rate $a = 0.967$, density at which predation is maximized $b = 0.14$ m$^{-2}$, virus decay rate $\mu = 0.39$ /day. The average amplitude of the cycles in the induced defense is 0.155, and the average amplitude of the cycles in the host population is 6.5 orders of magnitude.
Figure S5: As in fig. S4, except here $\hat{\alpha} = 0.8$, so that increases in the induced defenses are weak. The cycles therefore damp out.
This occurs because, for this value of $\hat{\alpha}$, induction is very weak, so the induced defense never rises high enough to allow for the severe density-dependence that drives outbreaks. The temporal model therefore suggests that induced defenses play a key role in driving gypsy moth outbreaks in nature. The comparison of the spatial model to the defoliation data nevertheless provides a more convincing test of the model, and so the spatial model is the focus of the main text.

In figures (S4) and (S5), and in the figures in the main text, we include panels showing fluctuations in the fraction of trees defoliated. To generate those panels, we translated the model prediction of host population density into a fraction defoliated, using a statistical model and associated parameters from the literature, such that the statistical model parameters had been estimated by correlating defoliation levels with egg densities (Williams et al. 1991). Note that, in the non-dimensionalized version of the model, host density is scaled by the average transmission rate $\bar{\nu}$, which in our experiments is expressed in terms of /day/m$^2$ (in the model equations, the m$^2$ unit balances the units on infectious cadavers, which are in terms of /m$^2$). Once we converted from units of m$^2$ to units of acres, we therefore still had $\bar{\nu}$ as a free parameter. The value of this parameter is ultimately of little interest to our results, but for the sake of completeness we note that we achieved a good fit to the oak-hickory defoliation data for a value of $\bar{\nu} = 0.5$ /day/m$^2$, which is well within the 95% confidence interval on this parameter as estimated both from our own data and from previous experiments (Fuller et al. 2012). For oak-pine forests, the best-fit value was a lower value of $\bar{\nu} = 0.1$ /day/m$^2$, which is not surprising given that we expect lower feeding rates, and thus lower infection risk, in forests with a higher fraction of trees that are not oaks.

### 2.3 Details of The Spatial Model

#### 2.3.1 Dispersal and Spatial Structure

Because the model is based on a discrete spatial grid, it uses discretized dispersal kernels. For example, for dispersal on automobiles, which is by far the most important type of dispersal Liebhold et al. (1992), the model uses a discretized double-exponential or Laplace distribution Kot et al. (1996):

$$k(d_{q,r}) = \kappa \exp(-\omega d_{q,r}) .$$  \hspace{1cm} (S20)
Here $d_{q,r}$ is the distance between patch $q$ and patch $r$, such that $\omega$ controls the degree to which dispersal declines with increasing distance from patch $q$ to patch $r$, while $\kappa$ is a scaling constant that ensures that the fraction of individuals dispersing sums to 1. For dispersal by larval ballooning, we instead used a Gaussian kernel, because it fits the ballooning data better.

Dispersal on automobiles typically occurs when females lay their egg masses on a vehicle. Such egg masses, however, comprise only a small fraction of the total, and so in the model we assumed that a fraction $\zeta$ of each population disperses between patches, after the epizootic, when the egg masses are laid (Liebhold et al. 1992). Based on previous work, we set $\zeta = 10^{-5}$ and $\omega = 0.175$ km$^{-1}$, respectively (Abbott and Dwyer 2008). The transported egg masses then hatch in their new patch the following spring, at which time larvae disperse following a normal dispersal kernel, with an average dispersal distance of 18.9m, as estimated from data on ballooning larvae (Hunter and Elkinton 2000).

We then set the spatial scale of the model forest to be 25 km on a side, for a total of 625 km$^2$ of forest, a typical forest size in the range of the gypsy moth in North America. We assumed that weather stochasticity was the same across grid cells, ensuring the high level of spatial synchrony typical of gypsy moth populations at this scale Peltonen et al. (2002). Because the defoliation data to which we compared the model were collected in areas that had been colonized by the insect decades earlier Johnson et al. (2006), we assumed that the insect had already infested the entire forest.

At the beginning of each realization of the model, the probability that the tree genera at each location were inducible was determined by drawing a $U(0, 1)$ random variate, such that if this random variate was less than the overall fraction inducible $p$, the location was strongly inducible, otherwise it was weakly inducible. Next, following the way in which the defoliation data were summarized (Johnson et al. 2005), we averaged the fraction defoliated across spatial locations, to produce a single time series of defoliation for each realization of the model. To produce power spectra, we used only the last 50 years of each 100-year simulation, roughly matching the time lag between the introduction of the gypsy moth and when the data were collected, and we averaged the spectra across 100 realizations. Power spectra were calculated using the spectrum function in the R programming language (R Development Core Team 2009).

An important point is that power spectra can have minor peaks at integral divisors of the period
associated with a major peak, simply because of the non-sinusoidal character of the data (Chatfield 2003). For our model, however, when the forest is 43% inducible, the peaks of the power spectrum occur at 9 years and about 4.9 years, confirming that the sub-harmonic in the model is not a statistical artifact. The occurrence of non-sinusoidal features in most data sets is also a good reason to combine spectral analysis with visual inspection of the time series being analyzed (Chatfield 2003). In both this document and in the main text, we therefore base our assessment of the fit of the spatial model partly on a visual comparison of model time series to data time series.

As we describe in the main text, individual model realizations vary in the extent to which they reproduce the data, because of stochasticity in forest spatial structure and in the insect’s reproductive rate. In figs. S6-S10, we show that, in at least 50% of realizations in oak-hickory forests, there are at least two alternations of high and low outbreaks, as in the data. Moreover, in almost every realization, there is at least one case in which a severe outbreak follows a mild outbreak, or vice versa. In contrast, when the forest is only 15% inducible, corresponding to oak-pine forests, defoliation peaks are always moderate, and thus never show the alternation of severe and moderate peaks seen in oak-hickory forests (figs. S11-S15).

An additional detail is that, when we used the parameter values estimated by fitting the non-spatial model in the spatial model, they produced amplitudes of fluctuation that were lower than 3 orders of magnitude, suggesting that spatial structure is stabilizing (for the spatial model, we calculated average amplitudes at each location before averaging across locations, thereby matching how the amplitude data were collected (Williams et al. 1991)). To produce more realistic amplitudes, we first set the standard deviation of the stochasticity to $\sigma = 0.3$ to prevent the model trajectories from being captured by a stable attractor (Dwyer et al. 2004). Second, we increased the scaled growth rate of the induced defense $\hat{\alpha}$ on inducible trees from $\hat{\alpha} = 2.5$ to $\hat{\alpha} = 8$.

Changing $\hat{\alpha}$ also changed the frequency with which the alternation of mild and severe outbreaks occurred in the model. In the realizations shown in figs. S6-S11, we set $\hat{\alpha} = 8$ on inducible foliage, and $\hat{\alpha} = 1$ on non-inducible foliage, which as we described produced at least two alternations in 50% of realizations. Reducing $\hat{\alpha}$ on inducible foliage to 4, however, only reduced the frequency of alternation to about 40%, while reducing it to 2.5 reduced the frequency to about 25%. Likewise, reducing the value of $\hat{\alpha}$ on non-inducible foliage to 0.5 only lowered the frequency of alternation to about 40%, while reducing it to 0.25 lowered the frequency to about 20%. Raising either value of
Figure S6: 10 realizations of the spatial model, with 43% of the forest inducible, corresponding to oak-hickory forests. Filled circles identify time periods during which model output provides a near-exact visual match to the defoliation data.
Figure S7: 10 additional realizations of the spatial model, as in fig. S6.
Figure S8: 10 additional realizations of the spatial model, as in fig. S6.
Figure S9: 10 additional realizations of the spatial model, as in fig. S6.
Figure S10: 10 additional realizations of the spatial model, as in fig. S6.
Figure S11: 10 realizations of the spatial model, with 15% of the forest inducible, corresponding to oak-pine forests.
Figure S12: 10 additional realizations of the spatial model, as in fig. S11.
Figure S13: 10 additional realizations of the spatial model, as in fig. S11.
Figure S14: 10 additional realizations of the spatial model, as in fig. S6.
Figure S15: 10 additional realizations of the spatial model, as in fig. S6.
\(\hat{\alpha}\) much above the baseline values instead caused the model to do a poor job of fitting the oak-pine data. In short, the model shows realistic output even if the values of \(\hat{\alpha}\) are somewhat lower than our best-fit values. We therefore argue that our results are not particularly sensitive to changes in the value of \(\hat{\alpha}\).

Increasing \(\hat{\alpha}\) also had the effect of increasing the fraction of realizations that produced at least two alternations of mild and severe outbreaks, as in the defoliation data. The effect is modest, however, such that for \(\hat{\alpha} = 4\) the fraction was 0.4, instead of the 0.5 produced by \(\hat{\alpha} = 8\). The value of \(\hat{\alpha}\) on non-induced foliage also had a modest effect on this fraction, such that \(\hat{\alpha} = 1\) gives 0.5, \(\hat{\alpha} = 0.5\) gives 0.4, and \(\hat{\alpha} = 0.25\) gives 0.2. Substantially higher values of \(\hat{\alpha}\) on either inducible or non-inducible foliage gave a poor fit to the oak-pine data. A more general point is that, because we fit the values of \(\hat{\alpha}\) for the spatial model to the defoliation data, the results of the spatial model are not dependent on the experimental data. This is the basis of our claim in the Discussion that the experimental and observational data provide independent lines of argument.

The reason why individual trajectories in simulated oak-hickory forests do not always match the data has to do with the effects of stochasticity on outbreaks. Weather stochasticity is strongly synchronizing in the model, as it is in nature (Peltonen et al. 2002), but the synchronization is specific to either inducible or non-inducible trees. To illustrate this effect, in fig. S16, we show a realization for which severe and mild outbreaks alternate for the entire time series (upper panel), followed by the associated time series for all spatial locations in the forest plotted on the same axes (lower panel). In this realization of the model, out of the \(10^4\) total locations, there were 4329 inducible locations and 5671 non-inducible locations. The time series for the inducible locations are all identical, however, and so they are plotted on top of each other, as are the time series for the non-inducible locations. The effect is that the graph shows only one line for the inducible locations, and one line for the non-inducible locations. The figure thus shows that, on the inducible trees, there was a synchronized cycle with a large amplitude and a period of 9 years, representing the main peak in the power spectrum in the main text, while on the non-inducible trees there was a synchronized cycle with a small amplitude and a period of roughly 4.9 years, representing the sub-harmonic.

The upper panel of fig. S16 thus shows an alternation of mild and severe outbreaks because every other peak of the cycle on non-inducible trees is in near-perfect synchrony with every peak of the cycle on inducible trees. Because of weather stochasticity or stochastic variability in the spatial
Figure S16: The upper panel shows a single realization of the model, for a case in which the model produces a strict alternation of severe and moderate cycles, as in the defoliation data for oak-hickory forests. The lower panel shows time series for all of the inducible locations and all of the non-inducible locations, such that the data for each location are plotted separately. The lines for the inducible locations are thus all plotted exactly on top of each other, as are the lines for the non-inducible locations. The panel therefore shows that the respective tree types, inducible and non-inducible, have their own synchronized cycles.
locations of inducible and non-inducible trees, in other realizations the peaks of these distinct cycles are not always in synchrony, and in such cases the model trajectory will not show a strict alternation of mild and severe outbreaks. As we mentioned in the main text, the lack of an obvious sub-harmonic in more recent defoliation data may reflect these effects (Johnson et al. 2006), although it may also reflect the effects of the more recently introduced fungal pathogen *Entomophaga maimaiga* (Dwyer et al. 2004). We reiterate, however, that a sub-harmonic never occurs by chance in model trajectories from oak-pine forests because inducible trees represent such a small proportion of oak-pine forests.

### 2.3.2 Modifications of the basic model

In the interests of parsimony, in the main text we present the simplest model that can explain the data, but here we consider alternative model structures, to show that our results are robust to changes in assumptions about the effects of induction. First, in the main text we used a model in which induction affects only variability in transmission, rather than average transmission, because induction had much stronger effects on variability in transmission than it had on average transmission. More generally, however, average transmission is strongly dependent on insect feeding and movement behavior, which are in turn affected by leaf toughness and leaf architecture (Dwyer et al. 2005), whereas variability in transmission is scale-independent and therefore more likely to be robust to differences in leaf characteristics. Because the effects of leaf characteristics on transmission are poorly understood, and because the model that leaves out such effects is more parsimonious, we assumed that differences in host-tree genera only affect variability in transmission. To test whether this assumption affected our conclusions, we also considered a model in which transmission rate declines exponentially with increasing levels of the induced defense, with rate parameter $\eta$, much as variability declines exponentially with defenses in the main model (unlike the corresponding parameter $\psi$ for the change in variability, this parameter cannot be scaled away). We then adjusted $\eta$ to find a value that gave an amplitude of fluctuation of $\log_{10}(\bar{v})$ that included the range seen in our experiments ($\eta = 0.1$ gave a good fit to the experimental amplitude of 0.55). As figure (S17) shows, the results are basically the same as for the main model, and in fact the fraction of time series that resemble the time series in the data is even higher (not shown). We repeat, however, that our main model is likely to be more robust, and it also provides a more parsimonious explanation for the defoliation data.
Figure S17: Effects of tannin-dependent average transmission on power spectra of the model output. As in the main text, induced defenses lower variability in infection risk $C$, but in this case they also lower average infection risk $\bar{\nu}$. 
As we mentioned in the main text, our basic results are robust to a moderate reduction in the insect’s reproductive rate on non-oaks (fig. S18). In this case, the sub-harmonic in the power spec-

![Graph showing the effects of a 25% reduction in host reproductive rate on non-oak locations on the power spectrum of the defoliation time series. Note that there is an increase in the super-harmonic in oak-pine forests. In spite of the superharmonic, however, the time series of defoliation in oak-pine forests do not show obvious alternation of severe and moderate outbreaks (not shown).](image-url)

Figure S18: Effects of a 25% reduction in host reproductive rate on non-oak locations on the power spectrum of the defoliation time series. Note that there is an increase in the super-harmonic in oak-pine forests. In spite of the superharmonic, however, the time series of defoliation in oak-pine forests do not show obvious alternation of severe and moderate outbreaks (not shown).
rum in oak-hickory forests is slightly less pronounced than in the power-spectrum in the data (see main text). We also note that, in the figure in the main text, there is a super-harmonic in the model spectrum for the data from oak-pine forests, but it is small enough that we did not emphasize it. We mention it here because fig. S18 shows that lowered reproduction on pines leads to a slightly more pronounced super-harmonic in the spectrum for the model from oak-pine forests, suggesting that the super-harmonic in the data for oak-pine forests may in fact be meaningful.

As we also mentioned in the main text, lowering the reproductive rate on non-oaks alone is not sufficient to produce a sub-harmonic. To see this, we ran the model with low inducibility at all locations, so that the rate of change of the defense $\dot{\alpha} = 1$ everywhere, while again allowing for a lowered insect reproductive rate on non-oaks. As fig. S19 shows, with weak variability in inducibility throughout the forest, there is no sub-harmonic for a range of values of the reduction of host reproductive rate on non-oaks. Variation in inducibility is therefore apparently required for the sub-harmonic.

A related point is that the model predicts that, in oak-hickory forests, there is a shorter-period, lower-amplitude cycle on non-oaks, which in turn implies that gypsy moth densities in oak-hickory forests should be higher on non-oaks in at least some generations (fig. S16). There are no data that permit a direct test of this prediction, but the prediction at least superficially appears to be contradicted by some literature data. Specifically, Lechowicz and Mauffette (1986) reviewed studies of gypsy moth feeding, using data from both choice tests and defoliation surveys to rank different host tree species as “preferred”, “acceptable”, or “un-acceptable”. Based on a range of studies, they ranked oaks as preferred hosts, and hickories and pines as acceptable hosts, implying that hickories and pines should have lower densities of gypsy moths than oaks.

It is important to remember, however, that relative densities on oaks and non-oaks reflect not just the suitability of different tree species as food sources, but also the dynamics of the virus. Indeed, several studies include data in which larval densities are higher on oaks than on non-oaks, but pupal densities a few weeks later are often lower on oaks (Lechowicz and Jobin 1983; Mauffette and Lechowicz 1984; Rossiter 1987). Whether these higher pupal densities are translated into higher larval densities in the following generation is unclear, but a high fraction of hatchling larvae do not leave the first foliage they encounter, even if that foliage is not that of a preferred host (Hunter and Lechowicz 1992). Higher pupal densities on non-oaks therefore likely lead to higher initial larval
Figure S19: Effects of reductions in host reproductive rate on the power spectrum when there is no variability in inducibility across spatial locations. In all 3 cases, 43% of spatial locations are oaks, corresponding to oak-hickory forests. Irrespective of the reduction in the reproductive rate on non-oaks, there is no meaningful sub-harmonic.
densities on non-oaks in the following generation. The data thus suggest that, at least some of the
time, densities on non-oaks are indeed higher than on oaks, as our model predicts. This effect occurs
in the model because virus epizootics are more severe on oaks, but an alternative explanation for the
pattern in nature is that larvae may move away from oaks near the end of the larval season (Lechow-
icz and Jobin 1983; Mauffette and Lechowicz 1984; Rossiter 1987). Unfortunately, however, there
are no data indicating which explanation is correct.

Moreover, it is not possible to allow for tree-species-specific movement rates in our model,
because of a lack of tree-species-specific movement data. Testing whether such movement would
alter our model results is therefore also not possible. Our overall argument is thus that, given the
available data, our model provides the best explanation for differences in gypsy-moth cycles between
forest types.

A final point is that we also tried different numbers of grid points, ranging from a $5 \times 5$ grid to a
$250 \times 250$ grid, with the model results presented here based on a $100 \times 100$ grid. As the number of
grid points increased beyond $150 \times 150$, realizations that produced exactly alternating high and low
defoliation levels became somewhat less likely, although the power spectra were unchanged. This
effect suggests that at least mild patchiness in inducibility is necessary for alternating peaks, but the
effect is rather weak. Further research is clearly needed to understand such effects.
References


